The Effects of Mandated Maternity Leave on Labor Market Outcomes in India *

Pulak Ghosh Stephanie Hao Lisa Ho Garima Sharma Shreya Tandon

September 15, 2025

Abstract

This paper studies the effects of a 2017 Indian law that increased the duration of paid maternity leave from 12 to 26 weeks on the labor market outcomes of women and men: wages, employment, and career trajectories. Leveraging pre-reform variation in the duration of leave offered across employers (driven by parent company policies), and linking social security records covering the universe of formal workers in India with all LinkedIn profiles, we document four main findings. First, the policy reduced female employment by 6% within six months of implementation and by 10% within four years. These effects were concentrated among young women aged 18 to 35, with no impact on men or older women, indicating the average firm shrank in response to higher costs. Second, employers did not pass costs onto wages: women's wages remained unchanged while men's wages rose slightly, consistent with firms seeking to retain experienced male employees as women became more expensive to employ. Third, men were promoted over women: incumbent male workers were more likely to move into managerial and abstract roles requiring higher firm-specific human capital, while young women were more likely to be placed in manual or routine positions. Fourth, to rationalize the magnitude of the employment decline, employers would have to greatly overestimate the rate at which women take maternity leave (implying employer misperceptions). First estimates suggest the mandate was welfare-neutral: it benefited employed women while raising costs for employers.

*We thank Shrushti Dhoot for exceptional research assistance, and Yasashvi Singh and a team of six interns for fielding the survey. This study was funded by The Weiss Fund for Research in Development Economics and Northwestern University. We received IRB approval from Northwestern (#2106000402A007) and IFMR in Chennai.

1 Introduction

Over 145 countries offer some form of paid maternity leave to young mothers (Hyland et al. 2020). Recent years have seen these policies proliferate across developing countries, with India, Nigeria, and Pakistan all expanding their duration of paid maternity leave for new mothers over just the last decade. While much is known about how such policies impact new mothers—by comparing mothers who gave birth right before a reform to those who gave birth right after (e.g., Lalive and Zweimuller 2009, Dustmann and Schoenberg 2012, Schonberg and Ludsteck 2014, Bailey, Byker, Patel and Ramnath 2019)—virtually no evidence exists on how paid maternity leave impacts women who are not mothers, or men. On the one hand, longer leave could benefit both employers and women by increasing the recruitment and retention of female workers, particularly of high-quality workers. On the other hand, by raising the relative cost of employing women, it could generate unintended consequences by reducing women's wages or employment. Even beyond wages and employment, longer maternity leaves could shift workers' career trajectories—for instance, by pushing women into roles where they develop lower firm-specific human capital if employers expect to lose them for longer. Conversely, if employers expect new mothers to return at higher rates, they may want to place women in roles requiring high firm-specific human capital.

This paper examines the impact of maternity leave expansions on women's and men's labor market outcomes using a 2017 reform in India that increased the duration of paid maternity leave from 12 to 26 weeks, known as the 2017 Maternity Leave Benefits Amendment Act (henceforth MBA). With this expansion India ranked fourth globally in benefit generosity.[1] Our empirical strategy exploits variation in the pre-reform duration of maternity leave offered across employers, driven by differences in parent company policies: we compare firms that previously offered only 12 weeks of leave (treated group) to those that already offered 26 weeks before the reform (comparison group). These two sets of establishments closely resembled each other in baseline characteristics, including two-digit industries, female share, and location. However, comparison group establishments were slightly larger on average, with 164 employees compared to 80 at treated firms. We account for this size difference by (a) including establishment-size-bin x state x industry × time fixed effects, and (b) restricting analysis to establishments with over 75 workers, which collectively employ over 75% of India's formal workforce. Our sample covers 57 million formal workers, or 41% of India's non-agricultural workforce, of which 7% of workers inhabit the control group.

We combine four rich sources of data: (i) pre-period duration of maternity leave from the universe of Glassdoor reviews, newspaper announcements, and a survey of a representative sample of

[1] The Indian maternity leave policy (26 weeks with 100% pay) ranked behind Canada (50 weeks with 55% pay), Norway (49 weeks with 100% pay), and Bulgaria with 410 days at 90% pay.

500 large firms across India[2], (ii) worker outcomes from employer-employee linked social security records covering all formal workers in India, (iii) career trajectories from the universe of LinkedIn profiles[3], and (iv) reform compliance and willingness to pay for maternity leave from a survey of 900 workers on LinkedIn and blue-collar factory workers.

Before turning to results, an important institutional feature of the MBA is worth noting that shapes our analysis. The MBA imposed different costs for two distinct groups of workers, which leads us to report analyses separately for each type. For blue-collar workers earning below Rs. 15,000 per month (767 USD adjusted for PPP), employers only lost mothers for fourteen additional weeks but the social security administration paid their salaries during maternity leave. For white-collar workers earning above this amount, employers both lost mothers for fourteen additional weeks and also paid their salaries during leave. The cost of employing the two types of workers was thus different, and we study outcomes separately for each type. "Blue-collar" and "white-collar" are approximate terms, and Rs.15,000 corresponds to the 67th percentile of the non-agricultural wage distribution.

Our first main result is that the reform led to a large reduction in female employment. Employers may respond to the higher cost of employing young women by reducing their employment or by substituting toward men or older female workers (Summers 1989). Consistent with this, we find that blue-collar women's employment declined by 5.5% within six months of the policy's implementation. White-collar women's employment declined by 6% within the first year and by 10% by four years following implementation. These declines were entirely concentrated among young women aged 18 to 35, who are most likely to become mothers, and we find no effect for older women (above the age of 35). We find no offsetting increase in male or older female employment and can precisely rule out increases that could fully offset the decline in female employment. We further rule out switches into informality or contract work, indicating that the average firm shrank by 1.1% in the face of higher costs. Translated into jobs, the MBA reduced female employment by 250,000 jobs within seven months of implementation, and these women moved into unemployment rather than into informality. Our evidence points to a contraction of labor demand as driving these results rather than labor supply.[4]

Our second result is that employers did not offset the cost of longer leaves by reducing women's

[2]Altogether our study gathers pre-period leave information for nearly 40% of formal workers in India. All results are robust to restricting the treated group to establishments for which we have definitive information on the pre-period duration of leave rather than assuming they offer the legal default.

[3]The sample comprises 57 million workers, with 40.3 million in social security records and 16.7 million on LinkedIn, covering 41% of India's non-agricultural workforce. Social security data cover all workers earning below Rs. 15,000 per month or the 67th percentile of the wage distribution (PLFS 2017).

[4]Standard labor supply models would predict a positive effect on employment following an expansion of maternity leave, as women shift toward or are retained by employers offering better amenities. Labor supply could decline if the reform raises fertility or lowers the rate of returning to work after childbirth, but these changes would have to be implausibly large to explain the observed decline in employment (see Section 3.3).

wages; instead, men's wages increased slightly (by 1.8%). We detect no impact on the earnings of new or incumbent female workers, young or older, and can precisely rule out even small changes. Higher male wages suggest that employers sought to retain experienced male employees as the relative cost of employing women increased and the female workforce shrank.[5]

The finding that the incidence of India's maternity leave expansion fell on employment rather than on wages contrasts with prior work in the U.S., which finds that expanding mandated maternity coverage through health insurance decreased women's wages rather than employment (Gruber 1994). This lack of wage changes in India has two possible explanations. The first is that women did not value longer leaves, limiting the scope for wage reductions. A second explanation is that wage rigidity—for example, due to equity concerns, nominal wage rigidities, or binding minimum wages (e.g., Breza et al. 2018, Kaur 2019, Breza et al. 2023, Sharma 2025)—prevented employers from reducing women's wages, and instead led them to lower employment. Our evidence points against the first explanation and toward the second. Surveys of both white- and blue-collar women show that nearly 100% took the full duration of leave after the reform, even among women who had previously taken only 12 weeks. Women's incentivized willingness-to-pay for the extended leave was between 25-30% of monthly wages. Together, these findings point against women placing little value on leave and instead indicate that employment declined because wages could not fall.

We next turn to studying heterogeneity in the impact of the maternity leave policy by industry. The shift in labor demand induced by the MBA is likely to have been larger for industries where replacing workers for fourteen additional weeks is more costly—i.e., where women are less substitutable in production with men. We therefore examine heterogeneity in the effect on female employment by the baseline female share of an industry: industries with low- and medium- female shares likely exhibit higher substitutability between male and female workers, while high-female-share industries exhibit lower substitutability. Consistent with this, we find that the decline in female employment was concentrated in industries where women are more easily substituted with men (with low- or medium-female-shares, such as financial services, retail trade, and the manufacturing of electrical equipment and chemical products).[6] Importantly, the concentration of declines in low- and medium-female-share industries does not reflect a small aggregate decline in female employment, as we also find large effects at establishments that employ many women. Male employment rose slightly in the most impacted industries, but we can rule out the replacement of more than half of all displaced female workers by men.

Our fourth set of results examines the career trajectories of workers.[7] The risk of losing a

[5]Wage results pertain to the 40.33 million workers observed in social security data.
[6]Table 3 reports examples of low-, medium-, and high-female-share industries.
[7]Career trajectory results pertain to the 17 million white-collar workers observed on LinkedIn.

worker for longer could lead employers to place women in more routine roles and less managerial roles. For incumbent workers, we find that expanding paid maternity leave made young women less likely to be promoted into managerial positions and more likely to be shifted from abstract into manual or routine roles with lower firm-specific human capital (by 1.1%). In contrast, men and older women (above the age of 35) were more likely to be promoted as managers and to move to abstract positions entailing greater firm-specific human capital. Men were 10% more likely to be promoted as managers and 1.1% more likely to be placed in abstract positions. For labor market entrants, we adopt a different design, comparing graduates from the same university–degree program who entered the labor market just after the policy was implemented to those who graduated just before. This approach yields similar results: the policy increased men's probability of promotion within the first two years of entering the labor market by 10%.

In sum, our findings show that expanding the length of paid maternity leave in India from 12 to 26 weeks led to large reductions in female employment among both blue- and white-collar workers. However, a potential silver lining masked by this employment decline is that, to justify the large response, employers would have to greatly overestimate the rate of leave-taking among women (Conlon and Sharma 2024). A simple back-of-the-envelope calculation shows that employers would have to think that 27% of women take leave within the first year of employment compared to a true rate of just 5%. A pilot survey of 41 HR managers at IT firms in India reveals misperceptions at about these levels. These misperceptions suggest the potential policy remedy that correcting beliefs could produce gains for both employers and workers—where workers benefit from lower discrimination and employers benefit from no longer underestimating the returns to hiring women.

Finally, the Indian case offers a valuable opportunity to develop a broader framework to assess the impact of maternity leave policies. We first examine welfare. One rationale for government-mandated maternity leave policies is adverse selection: any firm that individually offers generous paid leave may disproportionately attract women of childbearing age who will take it. Thus, it may be collectively optimal for firms to provide leave but not individually optimal. A second rationale is that women benefit from leave even if some lose jobs. We use incentivized valuations to measure how much women value leave. We measure adverse selection by comparing control firms in markets where many other firms already provided longer leaves (no adverse selection) to those in markets where few other firms did (adverse selection eliminated by the policy). Finally, we use the distribution of employment responses to back out the distribution of costs across firms. First estimates show that the reform increased worker welfare by approximately as much as it raised employer costs, with minimal evidence of adverse selection.

Understanding whether alternative policies—like shorter leave expansions, restricting coverage to low-cost firms, or maternity leave insurance to offset firm losses—could yield some of the

benefits while limiting costs remains a promising avenue for future work, explored in the next version of this paper.

Taken together, the findings of this paper show that the Indian Maternity Benefit Amendment Act of 2017, which increased paid maternity leave from 12 to 26 weeks, benefited mothers who enjoyed longer periods of paid leave and extended job protection, but, at the same time, precipitated unintended consequences for younger women by reducing their employment and slowing their career growth. These findings underscore the need for policymakers to account for both demand- and supply-side responses when designing benefit policies and to consider whether alternative approaches could potentially preserve benefits while reducing costs.

Related Literature This study is most closely linked to a seminal study by Gruber (1994), which finds that US employers passed costs of mandated maternity coverage in health insurance onto women's wages. Gruber and Krueger (1992) similarly find that employers passed costs of mandated workers' compensation insurance onto wages. In our setting, wage rigidity led incidence to fall on employment rather than wages.

Second, while there is an active and growing literature on labor supply reasons for low female labor force participation rates in South Asia, we contribute to the limited research on labor demand factors that depress women's employment (Islam et al. 2021, Gentile et al. 2023, Sharma 2023, Buchmann et al. 2025), complementing the predominant focus on labor supply determinants such as conservative gender norms and safety concerns (e.g., Bernhardt et al. 2018, Borker 2021, Field et al. 2021, Jayachandran 2021, Agte and Bernhardt 2025, Garlick et al. 2025, Ho et al. 2025, McKelway 2025). We find that public policies can shape labor demand in developing countries, especially in good formal jobs, reflecting the need for care when designing policies that disproportionately tax formal employment. In India, female labor force participation is especially low in urban areas and decreases with educational attainment, making it particularly important to understand the drivers of low female workforce engagement in formal employment.

Finally, in labor economics, several papers find mixed effects of the impact of mandated maternity leave on mothers' labor market outcomes by comparing mothers who give birth right before versus right after a policy (Lalive and Zweimuller 2009, Dustmann and Schoenberg 2012, Schonberg and Ludsteck 2014, Bailey, Byker, Patel and Ramnath 2019). We contribute by exploiting pre-reform variation in leave across employers to study outcomes for all workers, including women who are not mothers and men. Rich LinkedIn data enable us to trace workers' career trajectories in addition to wages and employment.

The paper proceeds as follows. Section 2 describes the setting and empirical strategy. Section 3 reports the effects of expanding paid maternity leave on labor market outcomes and provides evidence that effects may be driven by employer misperceptions. Section 4 develops a framework to evaluate the welfare effect of the policy and counterfactual policy alternatives. Section 5

concludes.

2 Reform, Data, and Empirical Strategy

2.1 India's Maternity Benefit Act of 2017

In 2017 India enacted a landmark change to its maternity leave policy by amending the Maternity Benefit Act of 1961 to more than double the duration of paid leave from 12 to 26 weeks. The Maternity Benefit Amendment Act of 2017 (henceforth MBA) made India's policy the fourth most generous in the world[8] (World Bank, 2018). The legislation was passed by the Rajya Sabha (Upper House) in August 2016 and by the Lok Sabha (Lower House) in March 2017, ultimately taking effect in April 2017. The amendment applied to all establishments with 10 or more employees and introduced three key provisions for new mothers, including (a) the extension of mandated paid maternity leave from 12 to 26 weeks for the first two children (with subsequent births entitled to 12 weeks), (b) mandatory crèche facilities in workplaces with more than 50 employees, and (c) work-from-home options during pregnancy when the nature of work permits.

Although the MBA set out three provisions in principle, in practice compliance was entirely concentrated on expanding paid maternity leave to 26 weeks. We assess compliance with various aspects of the law by fielding a survey to the current and former employees of 357 representative firms across India (described in Section 2). Approximately half the respondents reported taking maternity leave at least once during their careers (Figure A.1). Survey responses indicate that while the provision expanding paid maternity leave to 26 weeks was widely implemented, with a notable shift from 42% of pre-2017 maternity leaves lasting 2 to 4 months to nearly 100% of post-2017 leaves lasting the full six months (Figure 2), compliance with other provisions of the law was limited. In particular, compliance with the mandate to provide on-site crèches was strikingly low, with over 75% of respondents at firms with over 50 workers reporting that no crèche facilities were available even after the reform, a share that remained unchanged from the pre-reform period (Figure A.2). Thus, the MBA can be viewed as primarily expanding the duration of paid maternity leave from 12 to 26 weeks.

The MBA imposed different costs for two distinct groups of workers, leading us to report analyses separately for the two types (Figure 1, Panel B). First, for workers earning below Rs. 15,000 per month (767 USD adjusted for PPP)—termed blue-collar workers—employers lost new mothers for an additional fourteen weeks but were not responsible for paying their salaries during leave. The social security administration covered pay through the Employees' State Insurance Corporation (ESIC). In contrast, for white-collar workers earning above Rs.15,000 per month, employers

[8]Behind Bulgaria (410 days at 90% pay), Norway (49 weeks at 100% pay), and Canada (12 months at 55% pay). We define generosity as the number of weeks of paid leave offered multipled by the replacement rate. For example, 26 weeks at full pay is considered equally generous to 52 weeks at half pay.

both lost new mothers for fourteen additional weeks and paid their salaries during leave. The cost of employing the two types of workers was thus different, and we study outcomes separately for each group. The terms blue-collar and white-collar are approximate and Rs.15,000 corresponds to the 67th percentile of the wage distribution in India (Primary Labor Force Survey 2017). Examples of blue-collar workers include tailors, assembly line workers, and security guards, while white-collar workers include software engineers, lab technicians, and HR.

We study the impact of the MBA on formal workers in India—57.3 million workers contained in social security records and the universe of LinkedIn profiles, of which 41.7 million are men and 15.6 million are women. These workers constitute 41% of the non-agricultural workforce in India.

2.2 Formal Labor Market in India

India's formal labor market represents a substantial segment of the economy, accounting for 41% of non-agricultural employment. The formal sector is also growing: the number of EPFO subscriptions approximately doubled between 2019 and 2025. These formal sector positions offer some of the best employment opportunities in the Indian economy, with 2.5x-higher wages on average and better amenities compared to informal work (Citi Research and MoSPI, 2024). Some sectors are also more likely to be formalized than others—for example, financial services, utilities, and manufacturing all have formalization rates of approximately 90% or higher, while construction and retail have much lower rates of formalization. However, formal employment is also subject to additional regulations, such as the MBA, which increases the relative cost of female workers as compared to male alternatives.

Women's overall participation in paid employment in India remains low, and particularly in areas and demographic subgroups that would be particularly drawn to formal work (e.g., more urban or more educated women). In urban areas, for instance, approximately 25% of women engage in paid work compared to 40% in rural areas (PLFS 2023-2024). This urban-rural divide intersects with an education gradient in women's labor force participation: women with below-primary education have the highest participation rates at approximately 45%, while those with above-secondary education have the lowest at 25%.

Although workers displaced from the formal sector could in theory be displaced to the informal sector, we do not find any evidence of this in our analysis. Instead, women are more likely to shift out of paid work entirely, either out of the labor force or into unemployment. While men are marginally more likely than women to be unemployed at the lowest levels of education, the pattern reverses at higher levels of education. For example, among the tertiary-educated (as in our LinkedIn sample), 21% of women are unemployed compared to 10% of men—an 11 percentage point gender gap that suggests educated women in particular face large barriers to employment.

2.3 Data Sources and Sample

Our analysis relies on linking five rich sources of data. First, information on the pre-reform duration of maternity leave offered by employers comes from three datasets: (i) the universe of Glassdoor reviews scraped as of December 2016, (ii) all newspaper announcements of paid maternity leave expansions by individual organizations, and (iii) a survey of female workers at a representative sample of 357 large firms across India conducted via LinkedIn (intended N=500). To extract newspaper announcements in (ii), we used the NexisUni database to identify articles with "maternity leave" in the title between 2011 and 2016, then parsed PDFs using ChatGPT API to scan and extract firm names and leave durations. Each extracted maternity leave duration was manually verified against the original newspaper text. We constructed the survey sample in (iii) by identifying all Indian firms with at least 200 LinkedIn users and drawing a random sample of 500 for which we sought information on the pre-reform duration of leave.[9] For each selected firm, we identified a random sample of 30 women employed in 2016 and invited them to complete an online survey. The survey elicited both pre and post-reform durations of paid maternity leave offered by the respondent's employer.[10] To assess whether women avail the full leave, we also asked respondents to report all episodes and lengths of paid maternity leave they had taken over the past five years.

Second, blue-collar worker outcomes are contained in employer-employee-linked social security records from India's Employees' Provident Fund Organization (EPFO) and span the period between 2015 to 2018. The EPFO collects pension contributions for all workers with monthly earnings below Rs.15,000 (USD 767 adjusted for PPP). For each employment spell, the EPFO data report a worker's name, employer, monthly earnings, tenure, date of birth, and employer characteristics such as five-digit industry code and location. We infer each worker's gender using their name by applying two prediction algorithms. First, we compared workers' first names to those in the Socio Economic and Caste Census (SECC) 2011 and assigned workers to the gender most frequently associated with that name. Any name associated with a woman less than 20% of the time was classified as male, and any name associated with women greater than 50% of the time was classified as female.[11] Second, for any names that remained unclassified (25% of the EPF sample), we applied a Naïve Bayes classifier trained on the SECC data to predict gender based on features of the first name combined with location in India. Manual verification of gender assignment for a random subset of 1000 workers by two native speakers familiar with naming conventions yielded over 95% accuracy.

[9] In total 1600 firms met the size criteria. The 357 firms with responses closely resemble the full sample in firm size and 3-digit industry. The survey will conclude with 500 responses.

[10] Each survey response was cross-verified against an external source reporting paid maternity leave durations identified through a web search of company reports and announcements. We stopped contacting women from firms once two reports were received.

[11] The asymmetric cutoffs are due to unequal representation of men and women in the SECC.

Third, labor market outcomes for white collar workers come from the universe of LinkedIn profiles obtained from Revelio Labs. The data span the period between 2012 and 2023. For each job position held by a worker, the data report her employer, duration of employment, job title, O*NET occupation code, and six-digit NAICS industry code. The data also record a worker's educational background, including university, degree, field, and graduation year (e.g., "Bachelor of Science in Electrical Engineering" from IIT Bombay).

Fourth, in addition to capturing the pre-reform duration of leave, the LinkedIn survey described above captures employer compliance with various provisions of the maternity leave reform and women's experiences accessing paid leave. We merged the four datasets on employer names using a combination of the Jaro-Winkler and Levenshtein distance algorithms: perfect matches were automatically matched and similarity scores ranging from 85% to 99.99% were manually matched by two independent raters.[12]

Fifth, to determine how much women value maternity leave (for welfare calculations), two surveys elicited their willingness to pay for maternity leave. The LinkedIn survey collected valuations for white-collar women and a survey of 312 factory workers in industrial areas near Delhi collected valuations for blue-collar women. Respondents were asked to report the additional pay they would need to accept giving up 10 weeks, 14 weeks, or all 26 weeks of the paid maternity leave to which they are currently entitled. To incentivize truthful reporting, we additionally asked women to guess how much other women like them valued leave; guesses close to the truth were entered into a lottery to receive gift cards worth 50 USD representing 40% of the monthly salary of blue-collar workers. We also collect demographic and employment information of respondents to analyze how their willingness to pay varies by age, number of children, and firm size.

2.4 Empirical Strategy

We use a difference-in-differences (DiD) design to study the impact of the MBA on women and men's labor market outcomes. Treatment is defined as follows. Control establishments are those that already offered more than 16 weeks of paid maternity leave due to parent company policies or local union agreements at baseline in December 2016. Treated establishments are those that offered the previously mandated 12 weeks. Variation in pre-period leave durations predominantly comes from parent company policies. For example, control firms include Johnson & Johnson, which expanded paid maternity leave globally to 17 weeks in April 2015; Nestlé, which expanded maternity

[12] Discrepancies were resolved by an author. Workers may spell the same employer differently on LinkedIn. To harmonize employer names across profiles, we used a clustering-based approach with DBSCAN in Python (Density-Based Spatial Clustering of Applications with Noise). Pairwise name similarities were computed using the Jaro-Winkler algorithm, and a sparse distance matrix was created by retaining only pairs with similarity scores $\geq 96.5\%$. DBSCAN was then applied with an epsilon of 0.035 and a minimum sample size of 1 to form clusters of similar names. Each cluster was assigned a canonical employer name, typically the shortest name, which was used to standardize names across the dataset.

leave to 24 weeks in June 2015; and Indian delivery giant Flipkart, which expanded paid maternity leave to 24 weeks plus four months of flexible work hours with full pay in July 2015.[13] Another example comes from the Indian Banks' Association, which negotiated with several bankers' unions in 2010 to provide 24 months of paid maternity leave at 46 national banks (including Bank of Baroda, Canara Bank, Corporation Bank, and State Bank of India), while several prominent banks outside the association continued to offer 12 weeks (including IDBI, HSBC, and ICICI).[14] Any establishment not identified as offering longer leave at baseline in any dataset—Glassdoor reviews, newspaper announcements, or the LinkedIn survey of female workers—is classified as treated. We measure exact treatment status for nearly 40% of workers, and results are robust to restricting the sample to firms where we measure the pre-period duration of leave.

We also construct a sample of incumbent workers employed at treated or control establishments in 2016 and track their outcomes wherever they go. An incumbent worker is defined as being in the treated group (control group) if employed at a treated (control) establishment in 2016.

Summary Statistics Figure A.3 reports summary statistics from the EPFO data comparing treated and control establishments in April 2017. Control group establishments, which already offered longer durations of paid maternity leave, employed over 7% of workers in social security records. They had the same industry mix as treated establishments but were larger on average, employing 162 workers compared to 84 in the treated group (Panels a and b). The empirical design thus controls for size differences between the two groups. The two sets of establishments had similar female shares of workers: 75% of treated establishments and 80% of control establishments had female employment shares between 1 to 20%, with the average establishment being about 18% female (Panel b). Panel c depicts similarities in the industry mix across the two groups. The top-three most common industries in the treated group were IT and Software, Financial Services, and Basic Metals Products; among control establishments, Wood Products was in third place.

Regression Specification Since establishments in the control group are on average larger than the treated group, the empirical analysis controls for size differences using two methods. First, the DiD specification controls for firm size using establishment-size-bin x state x time fixed effects (λ_{sbt}), with establishments divided into four equally sized bins at baseline. The regression is:

$$Y_{jt} = \sum_{t=-4}^{t=7} \beta_t Treat_j 1_t + \alpha_j + \lambda_{sbt} + \varepsilon_{jt} \tag{1}$$

[13] Sources here, and here

[14] Other examples of multinationals with firmwide 16- to 24- week-long maternity leave policies before 2016 include Accenture, Adobe, Samsung, and Facebook and domestic employers include Godrej, HCL Technologies, Hindustan Unilever, AIIMS, HDFC Bank, and Bank of India.

Y_{jt} denotes the outcome for establishment j in time t around the passage of the reform. Time is denominated in months for social security data and in years for LinkedIn data. $Treat_j$ is an indicator equal to one for establishments offering 12 weeks of paid maternity leave at baseline ($t = -1$) and equal to zero for establishments offering longer leaves (26 weeks). α_j are establishment fixed effects and λ_{sbt} denote state x size-bin x time fixed effects, where b splits establishments into four equally-sized sized bins at baseline. Results are invariant to using fixed effects for two-digit-industry x state x size-bin x time instead, λ_{ksbt}. β_t are the coefficients of interest with β_{-1} omitted. Standard errors are clustered by establishment.

The second strategy—our main empirical strategy—to account for size differences between treated and comparison establishments reports results exclusively for large establishments employing over 75 workers, which collectively employ over 77.5% of formal workers in India. Results remain robust to setting establishment size thresholds at 100, 150, or 200 workers instead.

Worker-level analysis for incumbent workers, defined as those employed at treated or control establishments at baseline, in $t = -1$, uses the following regression:

$$Y_{ijt} = \sum_{t=-4}^{t=7} \beta_t Treat_{ij} 1_t + \eta_i + \lambda_{sbt} + \varepsilon_{ijt} \tag{2}$$

where i denotes a worker and η_i are worker fixed-effects. Standard errors are clustered by establishment.

The identifying assumption is that outcomes would evolve in parallel absent the reform, controlling for time varying shocks at the two-digit-industry x state x firm-size level. Parallel pre-trends demonstrate the plausibility of this assumption. A potential concern is that the reform may have directly affected comparison firms—for instance, by leading female applicants to switch from comparison to treated firms that now also offered longer durations of maternity leave—thereby violating the SUTVA assumption. Two findings mitigate this concern. First, we later show that effects are driven by changes at treated firms rather than shifts in the comparison group. Second, we find no evidence of changes in worker composition at comparison firms, including age groups, share of female hires, share of young female hires, and rates of taking leave, ruling out sorting (Figure A.5). This stability in worker composition reinforces the notion that SUTVA is not violated. The absence of compositional changes likely reflects the appeal of comparison firms as among the most attractive employers for women, which continued after the reform.[15]

[15]Level differences between treated and control firms do not violate the parallel trends assumption.

3 Results

This section reports the effects of the 2017 Maternity Benefits Amendment Act (MBA) on women and men's labor market outcomes. We begin by reporting effects on employment and wages. Next, we report the reform's effect on the career trajectories of incumbent workers and of labor market entrants. We conclude by showing that to justify the magnitude of the reduction in female employment, employers would have to greatly overestimate the rate at which women take maternity leave.

3.1 Employment

Employers may respond to the higher cost of employing women by reducing employment (labor demand). At the same time, longer maternity leaves may enable more mothers to return to the workforce or improve retention among incumbent female workers who value longer leaves, raising labor supply. We first report the cumulative impact of expanding paid maternity leave on female and male employment, followed by a decomposition into effects driven by labor demand and labor supply. All regressions report results at the establishment level.

Figure 3 reports the impact of the reform on female employment at all firms (Panel a) and at large firms with over 75 workers (Panel b), for blue-collar workers captured in social security records. Large firms employed over 75% of formal workers in India as of 2016. Female employment evolved in parallel before the reform, but we find a sharp decline of 5.5% within six months of policy implementation. This decline was concentrated among young women of childbearing age (18-35 years), whose employment fell by 8.5% (Figure 4, panel a). In contrast, we find no effect on the employment of older women above the age of 35 (panel b). We also find no effect on male employment and can rule out increases that would fully offset the decline in female employment at the 95% confidence level (Figure 5). Overall, the decline in female employment due to the MBA meant that the share of women in the workforce declined by 2.4% within six months of policy implementation (Figure 6). Translated into jobs, this represents a loss of 250,000 jobs within six months. Decomposing the total effect into components attributable to incumbent and new workers, we find that 55% of the decline in female employment was driven by the lower retention of incumbent female workers and 45% by the reduced hiring of new women.

A natural question is whether the observed decline in female employment reflects shifts into contract work or informality rather than a true reduction in employment. Our evidence points against both explanations. There is no treatment effect on the share of female workers at the largest thirty contract firms in India, which collectively employ over a third of all contract workers (Figure 7, Panel a). Moreover, female incumbent workers at treated firms were no more likely than

comparison counterparts to switch into contract work at the top-30 or at other contract firms.[16] Finally, ruling out the possibility that the decline in female employment was driven by transitions into informality, we also find a comparable decline in female employment at large firms with over 75 employees, which do not employ informal workers (Figure 7; the Primary Labor Force Survey 2023 shows that over 94% of workers at large firms have formal contracts). The decline in female employment and absence of changes in male employment likely masks important heterogeneity by industry, with different degrees of substitutability between male and female workers. Section 3.3 explores such heterogeneity by industry.

Figure 10 reports the MBA's impact on the employment of white-collar workers in LinkedIn records. There is a 6% decline in employment among white-collar women in the first year following the implementation of the policy, which grew to 10% by year four. In contrast, there is no statistically significant impact on male employment and we rule out employment gains above 1.2% with 95% confidence (a 2.5% increase in male employment is required to fully offset the decline in female employment).

The observed effects on employment reflect the cumulative sum of impacts on labor demand and labor supply. However, several factors point to the declines being driven by declining labor demand. Since the reform led treated firms to offer more female-friendly amenities, one would expect female labor supply to these firms to increase not fall. A supply-side explanation could in theory explain the decline if the reform raised fertility or women were less likely to return to work after leave. Yet these channels are implausible. First, we find a decline in female employment in social security records within just seven months of the reform's implementation, making it unlikely that fertility changes are responsible. Second, a simple back-of-the-envelope calculation shows that the effects cannot be explained by mothers returning to work at lower rates after the reform. Given that the current fertility rate in India is 2 (National Family Health Survey 2019-21) and assuming a uniform probability of pregnancy between ages 18 and 35, the annual likelihood of a female employee of child-bearing age becoming pregnant is approximately 12%. In the LinkedIn sample, we find that female employment at treated establishments declines by 6% to 10% in the four years after the reform. To fully explain this through lower rates of return to work, the reform would have to increase the rate of not returning from maternity leave by 66-83pp, which is implausible for any group and especially for highly-educated women in white-collar positions.

Overall, expanding the duration of paid maternity leave through the MBA precipitated significant reductions in female employment over both the short and long runs, decreasing employment for blue-collar women by 6% within six months of the policy, and white-collar women by 10% within four years. These declines were not offset by increases in male or older female employ-

[16]Contract work is measured as an as an indicator equal to one for employment at one of the thirty largest contract firms in India or at an employer whose name contains the word "contract" or "services".

ment, indicating that the average firm shrank in response to higher costs. While the magnitude of this decline is large, Section 3.4 discusses a potential silver lining and corresponding policy remedy: in order to justify a response of this magnitude, employers would have to greatly overestimate the rate at which women take maternity leaves. This suggests that correcting misperceptions could potentially mitigate the negative employment effects of paid leave policies. Section 3.3 explores heterogeneity in employment effects by industry.

3.2 Wages

Employers may also offset the cost of longer maternity leaves by reducing wages. Compensating differences would predict that women's wages should disproportionately decline to finance the provision of longer leaves (Rosen, 1986). Men's wages could also decline. Nominal rigidities may lead wage adjustments to only manifest for new workers. We therefore separately examine the reform's impact on the mean log wage of incumbent and new workers, separately by gender. Wage data are drawn from social security records and only cover blue-collar workers.

Figure 8 and Table 1 report results. The MBA had no impact on the average wages of women —incumbent or new, young or old. All point estimates are small and precise, and we can rule out wage declines exceeding 1% for both new and established women workers at the 95% confidence level. In contrast, incumbent men's wages increased by 1.8% (Figure 8, panel B), with no effect for new male workers (Table 1). This wage increase for incumbent men may reflect employers' desire to retain experienced male workers amid a shrinking female workforce. Overall, there is no evidence of wage reductions to finance the expansion of paid maternity leave, and a modest increase in men's wages suggests that employers prioritized retaining male employees as women grew more expensive. Figure A.4 shows that results on wages and employment are robust to restricting the sample to establishments for which we have data on pre-period leave durations.[17]

The incidence of paid maternity leave in India on employment rather than wages contrasts with Gruber (1994), who finds that U.S. employers passed the full cost of mandated maternity coverage in health insurance onto women's wages. Gruber and Krueger (1992) similarly show that employers shifted the cost of mandated workers' compensation insurance onto wages. In our context, one explanation for the absence of wage declines is that workers do not value the benefit, and hence employers cannot pass costs onto wages. However, the evidence points against this explanation: we find nearly universal take-up of longer leave in the post-period (Figure 2c); incentivized valuations also show that workers highly value maternity leave, worth between 20-30% of their wage (see Section 4).[18] Instead, incidence here fell on employment rather than wages

[17] Data from Glassdoor, newspaper announcements of leave expansions, the online survey of workers on LinkedIn, and manual sourcing provide pre-period leave durations for nearly 40% of workers in social security records. Each survey-reported duration was verified against an independent source like a company annual report or announcement.

[18] A 30% valuation of leave implies that it would take roughly one year to offset 14 extra weeks of paid leave, not

because relative wages were rigid, potentially due to equity concerns or binding minimum wages.

3.3 Heterogeneity by industry

The shift in labor demand induced by the MBA is likely to have varied by how costly it was for firms to replace workers for fourteen additional weeks—meaning that the average decline in female employment and the absence of changes in male employment likely mask substantial heterogeneity by industry. Replacement costs should be higher for women less substitutable in production.

Table 2 thus examines heterogeneity in employment effects by the baseline female share of an industry, divided into three bins, low (below 25th percentile female share), medium (between the 25th and 75th percentiles), and high (above the 75th percentile). Industries with low- and medium- female shares likely exhibit higher substitutability between male and female workers, while high-female-share industries exhibit lower substitutability. Table 3 reports examples of each type: low-female-share industries include the manufacture of chemical products and wholesale trade, medium-share include financial services and the manufacture of electrical equipment, and the high-share group includes education and textile manufacturing.

We find substantially larger declines in female employment in low- and medium-female-share industries (Table 2, Panel A). Four to seven months after the maternity leave policy was implemented, female employment in low- and medium-female-share industries declined by 8% and 6% respectively, while we find no effect in high-female-share industries. The coefficient for high-female-share industries is a precisely estimated 0.003, statistically indistinguishable from zero. Male employment increased slightly in low- and medium-female-share industries, but the increase was too small to offset the decline in female employment; we can precisely rule out the replacement of more than half the displaced female workers by men.

To assess whether the larger declines in female employment in low-to-medium-female-share industries reflected a small aggregate effect on employment, we examine heterogeneity by the number of female workers at an establishment. Establishments are divided into three bins by their number of female workers employed at baseline: low (below the 25th percentile), medium (between the 25th and 75th percentile), and high (above 75th percentile). Low-number establishments employed on average only 2 women (ranging from 1 to 5), compared to 25 women at medium-number establishments (ranging from 7 to 45 women), and 200 women at high-number establishments (from 45 to 2000 women). We find that the concentration of declines in female employment in low- and medium-female-share industries did not reflect a small numeric effect on employment, as substantial negative effects are found at establishments that employed many female workers (Panel B). The treatment effect on female employment is -8.6% at establishments

including non-monetary benefits such as recovery from childbirth, time with children, and a potentially smoother return to work.

with a medium-number of women and -6.9% at those with a high-number, compared to a precisely estimated zero effect at establishments with few female workers.

Of policy interest, Table 3 shows that the employment effects of the MBA were twice as large in manufacturing industries as in the service sector in social security records (for blue-collar workers earning below Rs.15,000 per month). Larger negative effects in manufacturing are consistent with higher replacement or retraining costs, where firms must train workers to operate machinery, and replacing workers or re-integrating them after longer absences is costly (Adhvaryu et al. 2023, Adhvaryu et al., 2025).

3.4 Careers

Beyond wages and employment, expansions of paid maternity leave could impact the career trajectories of workers. Employers may wish to employ women in roles where they develop less firm-specific human capital since they expect to lose mothers for longer. Conversely, if employers expect mothers to return to work at higher rates, they may wish to employ women in roles involving higher firm-specific human capital.

Figures 11 and 12 report the treatment effect of the MBA on the careers of incumbent workers, employed at treated or control firms in year $t = -1$ and tracked wherever they go. All results focus on white collar employees observed in LinkedIn records (15 million workers in total between 2012 and 2023), since we only observe detailed career trajectories for this group. As before, to account for size differences between treated and comparison groups we limit the sample to establishments with over 75 employees. LinkedIn workers typically earn over Rs. 15000 per month, meaning that the MBA required employers to both extend paid leave to new mothers in this group by fourteen weeks and pay for their salaries during leave.

Figure 11 examines effects on promotions. A worker is defined as being promoted into a managerial role if her job title changes from the previous year and one of the following words appears in the new title: manager, senior, principal, lead, managing, director, head, chief, or supervisor, or the word junior disappears. The reform had no impact on the likelihood of promotion into senior positions for women. However, we find a positive effect on promotions among men, a 10% increase (1pp) relative to a base rate of 10% of workers being managers. This finding suggests that employers were more likely to promote men into senior positions valuable to the firm and with higher firm-specific human capital, an interpretation that is also supported by the results below.

Figure 12 examines effects on a second measure of firm-specific human capital: performing abstract relative to routine or manual roles. Abstract roles correspond to high firm-specific human capital because they involve tasks that rely more heavily on firm-specific knowledge and are less amenable to standardization (examples below; Adda, Dustmann, and Stevens 2017). Employers may be less willing to assign women to such roles when maternity leave durations increase since

these positions are costlier to replace during extended absences. Conversely, if the MBA increased women's retention or raised the likelihood or predictability of returns to work after childbirth, employers may be more likely to assign women to such roles. We follow Adda, Dustmann, and Stevens (2017) to classify occupations into abstract jobs (involving analytical skills and adaptability), routine jobs (stable, repetitive processes), and manual jobs (involving nonroutine physical activity). Examples of abstract roles in LinkedIn data include sales manager, software engineer, graphic designer, finance analyst, consultant, and HR; examples of routine positions include photo editor, optometrist, and accountant; and examples of manual positions include maintenance worker, lab technician, branch operations manager, retail cashier, and customer service. In the original study, abstract roles are associated with higher wage growth and greater firm-specific human capital.

Our evidence points to employers promoting male workers and older women into abstract positions. Both male workers (all ages) and older female incumbent workers (above 35 years) at treated establishments were more likely to take on roles requiring abstract tasks, and less likely to take on roles requiring manual tasks (by about 1.1%). Together these results suggest that employers promoted men and older women into high firm-specific human capital positions, whom they are less likely to lose.

3.5 Career trajectories of labor market entrants

Results for incumbent workers offer only a first glimpse into the impact of maternity leave expansions on the careers of female and male employees. In the analysis below, we assess these effects for labor market entrants—not just workers who were already employed at the time the policy was introduced. Larger effects may arise for new workers if employers have greater flexibility to adjust their hiring practices than to reassign existing workers. We use a "career clone"-matching design to track impacts on career measures for labor market entrants (akin to Sharma ⓡ, Tandon, Ho, Ghosh, and Hao 2025). Specifically, we match workers entering the labor market in years immediately following the reform to similar historical counterparts—same-gender graduates from the same university and degree program (e.g., Bachelor's in Chemistry from the College of Engineering Pune or Bachelor of Science in Electrical Engineering from Amity University)—who graduated in the years immediately prior to the reform. Since undergraduate admissions in India are primarily determined by exam scores on entrance exams or high-school leaving exams, women in the same university-degree program are similarly selected at matriculation and receive identical educations.[19] To estimate the effects of the MBA on labor market entries' trajectories, we estimate

[19]For example, at Delhi University, among India's largest government run universities with 71,000 undergraduates, admissions were based on high school exam scores until 2024, and have since relied on a standardized entrance exam. Similarly, over 2,100 of 3,500 engineering colleges use the Joint Engineering Entrance (JEE) Exam to admit students. In 2021, 940,000 aspirants took the JEE Exam, representing over a quarter of the 3.6 million students

regressions of the following form:

$$y_{it} = \beta_0 + \sum_{\tau=1}^{24} \beta_\tau \mathbb{1}\{month = \tau\} \times treated_i + \lambda_{uft} + \varepsilon_{it} \tag{3}$$

where y_{it} is an individual i's monthly outcome in month t in the two years immediately following their bachelor's degree graduation. The indicator $treated_i$ is equal to one if i graduated in April 2017 or later and equal to zero if i graduated in 2015. $month_t$ is a vector of indicators for months 1-24 defined in reference to i's graduation date. We include university-by-field-by-month fixed effects, λ_{uft}, to compare workers who graduated from the same university-degree program, just after versus just before the reform. The identifying assumption is that the career trajectories of women who graduated from the same university–degree program in years just before versus just after the reform would have evolved in parallel absent the maternity leave reform. Figure 13 examines the effect of the maternity leave reform on promotions. A worker is defined as being promoted into a senior position if her job title changes from the previous year and one of the following words appears in the new title: manager, senior, principal, lead, managing, director, head, chief, or supervisor, or the word junior disappears. In the two years following graduation, we find that the reform led to a modest decline in women's likelihood of being promoted, of 1pp relative to a base rate of 8%. We also find a small increase in men's likelihood of being promoted, of 1pp relative to a base rate of 10%. The results for labor market entrants thus accord with those for incumbent workers, that employers were more likely to place men in managerial positions where they could develop firm-specific human capital and less likely to do so for women.

While our current results for labor market entrants report the effect of the MBA on their promotion into senior roles within the first two years of employment, this far from fully captures the richness of the LinkedIn data. Ongoing analysis therefore examines a wider range of outcomes for entrants: the full trajectory of workers' labor supply, including whether women remain at their first employer and the incidence of career breaks; firm quality; and the quality of the job match, that is, how well-suited a worker is to her position, and whether she is over- or under-qualified. We also study impacts on the time it takes a worker to find their first job and on upward mobility along the firm-quality job ladder.

3.6 Employer Misperceptions

Our results reveal that the MBA led to a large 6% reduction in female employment. Several findings suggest that this response may be driven by employer misperceptions about the rate of maternity leave-taking among women (Conlon and Sharma 2024): in particular, to rationalize the magnitude of the estimated employment decline, employers would have to believe that 27% of women take

pursuing four-year engineering degrees the same year (All India Survey of Higher Education 2021).

maternity leave within their first year of employment, compared to a true rate of just 5% (calculation reproduced below).

A pilot survey of 41 HR managers in the IT industry in India reveals misperceptions at about these levels. The authors asked HR managers at 41 different IT firms (30 to 1000 workers) in Tamil Nadu the following question to elicit their beliefs about the path of women's labor supply: "At IT firms like yours in Tamil Nadu (between a and b employees, and with similar turnover). For every 100 female employees that these firms hired in entry-level roles since 2016, how many women do you think: (i) took maternity leave within the first year? (ii) within the first two years? and (iii) did not take maternity leave within the first two years?" Responses closely match the misperception levels required to explain the employment decline following the maternity leave expansion. Managers believed that 23% of women take maternity leave within the first year of employment and 35% within the first two years, compared to true rates of only 5% and 12% (Figure 9). If such employer misperceptions drive discrimination, then this presents the tantalizing possibility that reducing misperceptions could produce gains for both workers and employers: where workers benefit from lower discrimination and employers benefit from no longer underestimating the returns to hiring women.

Simple calculation We present a simple calculation using the employment decline following the MBA to back out employer beliefs about leave-taking among women. The calculation pertains to blue collar workers where employers lost new mothers for fourteen additional weeks but did not pay their salary during leave (social security paid for salaries). Say an employer hires a female candidate if: $B > p*c$, where B is the benefit of employing the candidate net of her salary, p denotes the probability that the worker takes maternity leave, and c is the cost of maternity leave. Despite not paying worker salaries, say the reform increased c by fourteen weeks worth a worker's salary, i.e., $\Delta c = 14 * salary$. Assume the benefit of employing a female worker is uniformly distributed with mean equal to 18% of salary $B = mrpl - w \sim U(mean = 0.18 * salary)$. This benefit could reflect employers paying women 18% below their marginal product (half the monopsony power estimate in "Monopsony and Gender"). Alternatively, for women workers perfectly substitutable by men, the benefit could reflect employers paying women 18% below equally productive men (the monopsony-induced gender wage gap). Finally, even with no monopsony, differences in male-female quit rates within six months of employment generate over an 18% benefit to hiring women: the quit gap is nearly 18pp. The change in female employment equals: $\Delta log(f) = P[B > pc_{new}] - P[B > pc_{new}]$.

Estimates of employment declines following the MBA then allow us to determine $p_{perceived}$. We estimate a 22% decline in new female hires and 4.2% decline in incumbent workers. Using properties of the uniform distribution, this implies that employers must believe 27% of women take

maternity leave within the first year of employment compared to a true rate of just 5%, and closely matching 23% elicited in a survey (Figure 9).

3.7 Summary

In summary, our findings reveal that while expanding paid maternity leave through the MBA benefited mothers who enjoyed longer leave with full pay and extended job protection, it also generated unintended consequences for young women by reducing their employment. We find significant declines in female employment over both the short and long run: a 6% decline for blue collar women within six months of the policy, and a 10% drop for white collar women within four years. These effects were concentrated among young women aged 18 to 35, who were shifted into unemployment or informal work outside sample firms (Section 3.2 provides evidence against shifts into contract work or informality within formal firms). This decline in female employment was not offset by increases in male or older female employment, implying that the average Indian firm contracted in response to higher costs. We find no effect on women's wages and a modest positive effect for men, consistent with employers seeking to retain experienced male employees. The reform also modestly slowed women's career growth: incumbent male workers were more likely to move into managerial and abstract roles requiring higher firm-specific human capital (a 10% increase), while young women were more likely to be placed in manual or routine positions (a 1.1% increase). However, a potential silver lining masked by the employment decline is that the drop in female employment may be driven, at least in part, by employers overestimating the rate at which women take maternity leave, suggesting that correcting misperceptions could benefit both employers and workers.

4 Welfare and Counterfactuals

The sheer scale of the Indian labor market, with over 14 million women and 41 million men in formal employment, underscores the importance of understanding how the MBA aimed at supporting women's labor force participation and work after childbirth affected their and men's labor market outcomes. We find that expanding paid maternity leave reduced women's employment over both the short and long run (by 6% and 10% respectively) with limited impact on wages, but that mothers valued longer leaves with nearly 100% take-up.

At the same time, the Indian case also offers a valuable opportunity to more broadly develop a blueprint to calculate the benefits and costs of maternity leave policies, assess how such policies are expected to impact wages and employment, and evaluate alternative policy designs that could support mothers while mitigating job losses. This section serves three goals. First, we evaluate the welfare effect of the reform, asking if the benefit to mothers and potential reduction in adverse

selection at firms that already offered longer leaves exceeded the reform's costs. Second, firms may vary in their cost of leave provision such that providing leave is efficient for some but not all firms. We recover this cost distribution to identify which firms can increase leave at net benefit. Finally, we evaluate the impact of several alternative policies—smaller leave expansions, only a subset of firms providing expanded leave, and maternity leave insurance to offset firm losses—that could provide support while potentially limiting job declines. Today we present back-of-the-envelope estimates using averages.

Welfare The mandate may raise welfare by benefiting mothers and potentially curbing adverse selection at firms already providing longer leaves. Adverse selection is the idea that firms may find it collectively optimal to expand maternity leave even if no firm wants to be the only one expanding it because it fears disproportionately attracting soon-to-be-mothers. Both adverse selection or firms' failure to provide maternity leave even if valued by workers—such as due to an inability to pass costs onto wages—furnish a rationale for government intervention. Evaluating welfare requires estimating (i) the value of the benefit to workers, (ii) the distribution of costs across firms, and (iii) the adverse selection penalty. Consider a sum over firms. Net benefit before the mandate is:

$$W_{old} = \sum_{i \in firms} D_i E_i (V_{26} - c_{i,26} - \delta_i) + (1 - D_i) E_i (V_{12} - c_{i,12}) + \pi_i$$

where i denotes a firm, D_i is an indicator equal to 1 for firms providing long leaves in the pre-period, E_i is the number of female workers. V_{26} is the value of the 26-week-long benefit to a female worker. $c_{i,26}$ is i's cost of hiring a female worker in a 26-week regime where everyone provides 26 weeks of leave (i.e., without adverse selection), and δ_i is an adverse selection penalty facing firm i in the pre-period, denoting the extra cost of hiring a female worker with a higher likelihood of becoming pregnant if i is one of the only few firms offering 26 weeks of maternity leave while others offered 12 weeks. While we refer to δ_i as an adverse selection penalty, it could also reflect advantageous selection, for example, if more productive or career-oriented women disproportionately sort into firms offering longer leaves. Our empirical analysis remains agnostic and focuses on estimating the net cost or benefit. $c_{i,12}$ is the cost of providing 12 weeks of leave when most other firms also offer 12 weeks. Note that $c_{i,12}, c_{i,26}, \delta_i$ denote the cost of employing the average female worker at firm i (not just those who become mothers), and incorporate the expected probability of taking leave. π_i is profits net of labor costs.

While the government may share costs with employers, such as in India where the government paid workers' salaries during maternity leave and employer costs c_i only entailed replacement costs, this payment represents a transfer from taxpayers to workers. We abstract away from the dis-

tributional consequences of taxation by assuming that the government finances its benefit through a lump-sum tax, utility is quasi-linear in money, and the planner places equal welfare weight on all individuals—implying that taking $1 from 100 individuals and giving $100 to a single mother does not affect the net benefit calculation. This abstraction allows us to focus in on the key economic forces that govern welfare unique to maternity leave policies: the value of leave to workers, the cost of leave to employers, and potential adverse or advantageous selection when firms are among the few entities offering expanded leaves in the pre-period.[20] Welfare in the post-period is:

$$W_{new} = \sum_{firms} E_{i,new}(V_{26} - c_{i,26})$$

The change is:

$$\Delta W = W_{new} - W_{old} = \underbrace{\sum_{firms} (1 - D_i) E_{i,new}((V_{26} - V_{12}) - (c_{i,26} - c_{i,12}))}_{\text{Expanded coverage}} + \underbrace{\sum D_i \delta_i}_{\text{Eliminate adverse selection}} + \underbrace{\sum (1 - D_i)(E_{i,new} - E_i)(V_{12} - c_i)}_{\text{Job loss}} + \underbrace{\sum \Delta \pi_i}_{Profits}$$

Note that the δ_i term does not disappear from ΔW when all firms switch to offering 26 weeks of leave. This is because $c_{i,26}$ is the cost of employing women in a regime where all firms provide 26 weeks, i.e., in the post-period. By contrast, early adopters face costs of $c_{i,26} + \delta_i$, when few other firms offered long leaves, with δ_i capturing the additional burden from adverse (or advantageous) selection borne only by the early adopters. Thus, δ_i does not disappear if adversely selected women reallocate from early adopters to newly-adopting firms and spread costs, since $c_{i,26}$ already incorporates this re-sorting.

We estimate δ_i by comparing control firms in markets where many firms already offered extended maternity leave (no adverse selection) to control firms in markets where virtually no other firm offered longer leave (previously suffered adverse selection). We estimate the distribution of costs c_i using the distribution of treatment effects on employment, employing the logic described below. Finally, worker valuations V come from an incentivized survey, where workers reveal the wage increase they would require to be willing to switch to shorter, 12-week-long leaves.

What happened A key question is why the maternity leave mandate reduced employment rather than wages. One possibility is that workers did not value the benefit, which prevented wages from declining. A second possibility is that wages were rigid, which led firms to adjust employment rather than wages. Below we show that workers valued the benefit—this is also revealed by the

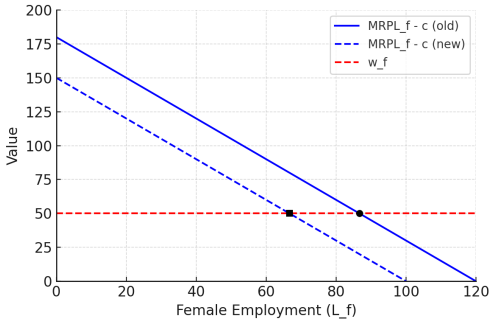
[20] Any distortionary costs of taxation would reduce the welfare gains from the policy.

nearly universal (98%) take-up of longer leaves after the reform, even among women who had taken 12 weeks prior to the reform. These findings suggest that employment declined because wages were rigid, for example, due to wage equity or binding minimum wages.

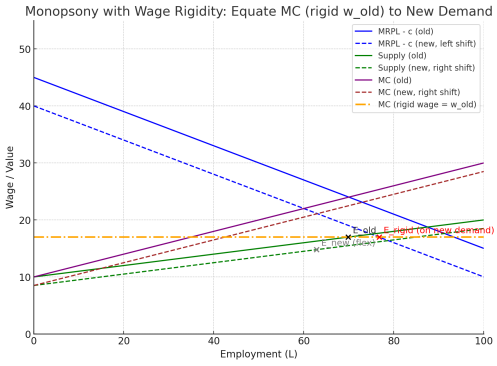
The Figure below graphically depicts the impact of the maternity leave reform on wages and employment. We assume monopsonistic employers face upward-sloping labor supply curves, consistent with growing evidence of labor market power in developing countries, including India (e.g., Amodio et al. 2025, Felix 2022, Morchio and Moser 2024, Sharma 2023, Sharma 2025). The reform shifted labor demand to the left and labor supply to the right, but wages were rigid so employment declined (Panel A). Panel B shows the same intuition under perfect competition. Demand again shifts left, and market-level supply shifts right (because women value the longer leave). However, with rigid wages, individual employers face the same marginal cost (flat wage), and the leftward shift of demand causes employment to decline. Monopsony and perfect competition thus yield the same qualitative conclusion about what happened: demand fell, wages did not adjust, and employment dropped. The distinction between the two only matters for the magnitude of cost changes recovered from the observed changes in employment, not for the basic interpretation of the reform's impact.

Effects under perfect competition and monopsony

Perfect competition



Monopsony



Measuring adverse selection δ_i is the extra cost of hiring a female worker if i is one of only few firms offering 26 weeks in the pre-period while others offered 12 weeks. We measure δ_i as, when AS disappears, (i) $\Delta\bar{quit}_i$, the change in the quit rate of average woman, and (ii) $\Delta\left(\bar{leave} + \bar{return}\right)_i$, change in leave-taking of the average woman. We estimate δ_i using a difference-in-differences design comparing control firms in markets where many firms already offered longer leaves (no adverse selection) to control firms in markets where most other firms offered 12 weeks, so adverse selection was present before but eliminated by the policy. The difference yields $\sum D_i \delta_i / N$. A market is defined as an industry-geography pair. The identifying assumption is that the cost of

hiring female workers did not change systematically at control firms in markets where other firms were also in the control group. Applying this cost estimate to all firms with $D_i = 1$ implies that δ_i would be similar for control firms in markets where all firms offered the longer benefit and markets where only some firms did.

Figure 15 shows that the reform did not increase quit rates nor the female share of the workforce at control firms (Panels a and c). However, we do find a decline in the propensity to take maternity leave among female workers, which falls by 1pp from a base of 2% of women on leave in any given month. Control firms employed 648,900 women at baseline. With 2% of women on leave each month pre-reform and 1% in the post-period, this saves control firms the leave costs of 6489 women every month (quantified below).

Measuring ($V_{26} - V_{12}$) We quantify women's valuation of leave through two surveys: a survey of 312 industrial workers in Delhi for blue-collar workers and the LinkedIn survey for white-collar women. Longer leaves were valuable to women—100% of women in our surveys who took maternity leave after the reform took the full 26 weeks, including those who had previously taken only 12 weeks before the reform (Figure 2c). Two questions elicit workers' valuation of leave. We first ask workers: "What is your monthly salary? (This can be approximate)". We then ask: "In your current job under current labour law, you are entitled to 26 weeks of paid maternity leave. How much more would your monthly salary have to be for you to prefer the higher salary but with 12 weeks of paid maternity leave? My monthly salary would need to be the following to accept only 12 weeks of paid maternity leave:" with options ranging from a monthly increase of Rs.0 to Rs.20000, in increments of Rs.500 (from 0 to 978 USD PPP in increments of 24 USD PPP). We incentivize truthful responses in a separate question by asking workers to report valuations for other workers like them, where answers close to the truth provide entry into a lottery to win gift cards worth 50 USD, which is equal to roughly 40% of their monthly salary.

Elicited valuations are reasonable and reveal that women substantially value maternity leave. The average woman values the additional 14 weeks of maternity leave at Rs. 4399, equivalent to 32.8% of her monthly salary, implying that it would take roughly one year of work to compensate for the monetary cost of leave, with additional non-monetary benefits associated with recovering from childbirth, time with children, and a potentially smoother transition back to the workforce (Figure 14). Three patterns suggest that these valuations are meaningful and grounded in women's experience. First, women with fewer children report higher valuations than women with more children. Among currently employed workers, women with one or two children value fourteen extra weeks of maternity leave at Rs.4878 and Rs.4781 per month on average (37% and 34.4% of salary, respectively), while women with three or more children report lower valuations (Rs.3375 or 25% of monthly salary), and those without children report intermediate values (Rs. 4278.947 or

32%). Higher valuations among women with fewer children are consistent with the expectation that these women anticipate having more children in the future, and slightly lower valuations among women without children are consistent with the idea women learn the value of maternity leave once exposed (e.g., Kuziemko et al. 2020). Second, younger women value maternity leave more than older workers: women aged 20–28 value the additional 14 weeks of leave at Rs. 4690 per month on average, compared to Rs. 3804 among women over age 28—a 10% difference in salary shares. Finally, incentivized willingness to pay for maternity leave for other women like themselves closely matches respondents' valuations for their own selves. Welfare calculations use incentivized valuations by age group, reported in Figure 14.

Cost distribution (c_i) The distribution of treatment effects on employment across firms reveal the distribution of costs $(c_{i,26} - c_{i,12})$: larger cost changes should inspire a larger decline in employment. This is seen by taking the total derivative of the first-order condition for a monopsonist. A monopsonist maximizes profits: $\max_{f_i} R(f_i, m_i) - c_i f_i - w_i f_i$, where R is the revenue function, c_i is the constant marginal cost of the mandate, w_i is the female wage and f_i is employment. The monopsony first order condition is:

$$\underbrace{\left(\frac{e_i}{1+e_i}\right)}_{\mu_i}(mrpl_{fi} - c_i) = w_i$$

Taking the total derivative with respect to the benefit change, and assuming a constant elasticity of labor supply e_i around the time of the policy[21]:

$$\mu_i \left[\frac{\partial mrpl_{fi}}{\partial l_{fi}} dl_{fi} + \frac{\partial mrpl_{fi}}{\partial l_{mi}} dl_{mi} - \frac{dc_i}{db} db\right] = dw_i$$

Rearranging the expression shows that higher mandate costs lead to larger declines in employment:

$$dlnl_{fi} = \frac{dc}{mrpl * \frac{\partial lnmrpl_{fi}}{\partial lnf_i}}$$

The denominator $(\frac{\partial lnmrpl_{fi}}{\partial lnf_i} < 0)$. Employment declines as costs rise, and it declines more when $\frac{\partial lnmrpl_{fi}}{\partial lnf_i}$ is smaller in magnitude, which reflects greater substitutability between women and other factors of production. Intuitively, the marginal female worker affects output less when she is more

[21] Assuming constant elasticity of labor supply with respect to the policy change is equivalent to assuming that firms behave as monopsonists rather than oligopsonists, whose elasticities vary with firm size. This assumption is reasonable in our setting: we do not observe dramatic changes in firm size, as employment at the average firm declines by 1.1%, and the largest change is 2.8% at the most impacted firms. Shocks to firm size would need to be large to elicit oligopsonistic responses.

easily substitutable.[22] Together, the magnitude of the employment response and the slope of the marginal revenue product curve reveal costs:

$$\frac{dc_i}{db} = \frac{dw_i}{db}\frac{1}{\mu_i} + \frac{\partial lnmrpl_{fi}}{\partial lnl_{fi}}\frac{dlnl_{fi}}{db}mrpl_{fi} + \frac{\partial lnmrpl_{mi}}{\partial lnl_{mi}}\frac{dlnl_{mi}}{db}mrpl_{fi}$$

Measuring wage and employment changes $(dw_i, dlnf_i, dlnm_i)$ and imposing a production function which governs $\frac{\partial lnmrpl_{fi}}{\partial lnf_i}$ allows recovering costs. We assume a Cobb-Douglas revenue function in capital and labor $Y_i = z_i K_i^{\alpha_{k1}} l_i^{\alpha_{k2}}$ where labor is a CES aggregation of male and female workers $l_i = [\beta f_i^{\sigma} + m_i^{\sigma}]^{\frac{1}{\sigma}}$. This yields $\frac{\partial lnmrpl_{fi}}{\partial lnf_i} = \frac{s_f - 1}{\sigma} - \alpha s_f$, where s_f is the CES share of females in the labor aggregate. We measure cost changes separately for the three sets of industries represented in Table 2 (low-share, medium-share, and high-share), since we find meaningful heterogeneity in employment responses across industry types. We assume the monopsony first-order condition holds in the pre-period, and common $e_i = 2.3$, implying the average woman takes home 70% of her marginal product (Sharma 2025). $mrpl_{fi}$ can be recovered as a function of the pre-period wage alone, without relying on assumptions about the production function.

For this section we assume that estimated costs equal actual costs, not accounting for potential employer misperceptions. If employers systematically overestimate replacement costs, then we would overestimate cost increases and underestimate the welfare impact of the maternity leave policy. However, perceived costs are the relevant objects for counterfactual analysis, since employer behavior and policy impacts are ultimately determined by what employers believe.

Table 5 reports results: the employment declines imply that costs at low-female-share firms increased by about 33% of a worker's wage per month and at medium-female-share firms increased by 26% of a worker's wage, roughly equal to the stated value of the benefit to workers.

Job loss The average treatment effect on employment directly estimates $\sum(E_{i,new} - E_i)/N$. Because profits decline as employment declines, one approach to estimating the decline in profits net of labor costs is to impose a production function that maps ΔlnE into $\Delta\pi$. We refrain from imposing such structure at the moment. Instead, we focus on estimating cost increases from the provision of expanded maternity leave, which constitutes the first-order impact on profits: the higher cost of employing women after leave.

Results Table 5 reports first estimates of the impact of the maternity leave law on worker and employer surplus respectively. Given the composition of female workers with different valuations of leave, we find that women's welfare went up by approximately 30% of the baseline wage bill.

[22] For example, in a production function where labor is a CES aggregation of male and female workers $l_i = [\alpha f_i^{\sigma} + m_i^{\sigma}]^{\frac{1}{\sigma}}$, $\partial lnmrpl_{fi}/\partial lnf_i$ declines with the elasticity of substitution between male and female workers.

Given the composition of firms with different baseline female shares, employer costs went up by approximately 25.5%. The two would be equal if workers overstate their willingness to pay for maternity leave by 15%. We find small savings from eliminating adverse selection at employers who were already offering longer leaves before (0.1% of the baseline wage bill). Job loss due to the policy reduced welfare by 2% of the wage bill. These first estimates imply that the reform increased worker welfare by roughly as much as it raised employer costs.

Counterfactuals (ongoing) (i) Shorter leave duration, (ii) insurance to pay for leave financed through taxation (which could help especially if employers hold misperceptions), (iii) only low-cost firms provide leave. Counterfactuals require more structure: estimating supply elasticities with respect to leave, estimating demand elasticities ($mrpl$), and mapping the costs of switching from 26 to 12 weeks of leave (what we estimate) to different leave durations.

5 Conclusion

We study the effects of a landmark policy in India that expanded the duration of paid maternity leave from 12 to 26 weeks. While longer leaves benefited new mothers, who enjoyed full pay during leaves and extended job protection, they also generated unintended consequences for younger women by reducing their employment and (to a lesser degree) slowing career growth. We find significant declines in female employment over both the short and long runs: a 6% decline for blue collar women within six months of the policy, and a 10% drop for white collar women within four years. These effects were entirely concentrated among young women aged 18 to 35, who were shifted into unemployment or informal work outside formal firms. This decline in female employment was not offset by increases in male or older female employment, and the average Indian firm contracted in response to higher costs (by 1.1%). We find no effect on women's wages and a modest positive effect for men, suggesting that employers sought to retain experienced male employees in response to the higher cost of employing women. Finally, men were more likely to be promoted to managerial and abstract roles, while young women were more likely to be shifted into manual or routine positions with lower firm-specific human capital. A potential silver lining is that the drop in female employment appears to be driven, at least in part, by employers overestimating the rate of leave-taking among women, suggesting that correcting misperceptions could both reduce discrimination and benefit employers (Conlon and Sharma 2024). First welfare estimates indicate that the reform increased worker welfare by approximately as much as it raised employer costs.

Overall, the findings of this paper highlight how policies ostensibly designed to help women can unintentionally reduce employment and alter the career trajectories of the very group of workers they aim to support. However, they also benefit women who remain employed. Investigating whether alternative policies like shorter leave expansions, restricting coverage to low-cost firms, or

maternity leave insurance to offset firm losses, can yield some of the benefits while limiting costs remains a promising avenue for exploration tackled in the next version of this paper.

References

Adda, Jérôme, Christian Dustmann, and Katrien Stevens. "The career costs of children." Journal of Political Economy 125, no. 2 (2017): 293-337.

Adhvaryu, A., Kala, N., Nyshadham, A. (2023). Returns to on-the-job soft skills training. Journal of Political Economy, 131(8), 2165-2208.

Adhvaryu, Achyuta, Emir Murathanoglu, and Anant Nyshadham. On the allocation and impacts of managerial training. NBER No. w31335. Working Paper, 2025. Agte, Patrick, and Arielle Bernhardt. "The economics of caste norms: Purity, status, and women's work in India." Working Paper (2025).

Antecol, Heather, Kelly Bedard, and Jenna Stearns. "Equal but inequitable: Who benefits from gender-neutral tenure clock stopping policies?." American Economic Review 108, no. 9 (2018): 2420-2441.

Bailey, Martha J., Tanya S. Byker, Elena Patel, and Shanthi Ramnath. "The Long-Run Effects of California's Paid Family Leave Act on Women's Careers and Childbearing: New Evidence from a Regression Discontinuity Design and US Tax Data". No. w26416. National Bureau of Economic Research, 2019.

Breza, Emily, Supreet Kaur, and Yogita Shamdasani. "The morale effects of pay inequality." The Quarterly Journal of Economics 133, no. 2 (2018): 611-663.

Breza, Emily, Supreet Kaur, and Nandita Krishnaswamy. Social norms as a determinant of aggregate labor supply. No. w25880. National Bureau of Economic Research, 2023

Buchmann, Nina, Carl Meyer, and Colin D. Sullivan. Paternalistic discrimination. Stanford Institute for Economic Policy Research (SIEPR), 2024.

Citigroup. "India: Formalisation's Relentless March." Citi Global Insights. 2024. https://www.citigroup.com/global/insights/india-formalisation-s-relentless-march.

Conlon, John, and Garima Sharma. "Employer misperceptions about the path of women's labor supply". Pilot study, 2024.

Dustmann, Christian, and Uta Schönberg. "Expansions in maternity leave coverage and children's long-term outcomes." American Economic Journal: Applied Economics 4, no. 3 (2012): 190-224.

Gruber, Jonathan, and Alan B. Krueger. "The incidence of mandated employer-provided insurance: Lessons from workers' compensation insurance." Tax policy and the economy 5 (1991): 111-143.

Gruber, Jonathan. "The incidence of mandated maternity benefits." The American economic review (1994): 622-641.

Islam, Asad, Debayan Pakrashi, Soubhagya Sahoo, Liang Choon Wang, and Yves Zenou. "Gender inequality and caste: Field experimental evidence from India." Journal of Economic Behavior Organization 190 (2021): 111-124.

Hyland, Marie, Simeon Djankov, and Pinelopi Koujianou Goldberg. "Gendered laws and women in the workforce." American Economic Review: Insights 2, no. 4 (2020): 475-490.

Kaur, Supreet. "Nominal wage rigidity in village labor markets." American Economic Review 109, no. 10 (2019): 3585-3616.

Kuziemko, Ilyana, Jessica Pan, Jenny Shen, and Ebonya Washington. The mommy effect: Do women anticipate the employment effects of motherhood?. No. w24740. National Bureau of Economic Research, 2018.

Lalive, Rafael, and Josef Zweimüller. "How does parental leave affect fertility and return to work? Evidence from two natural experiments." The Quarterly Journal of Economics 124, no. 3 (2009): 1363-1402.

Morchio, Iacopo, and Christian Moser. The gender pay gap: Micro sources and macro consequences. No. w32408. National Bureau of Economic Research, 2024.

Rosen, Sherwin. "The theory of equalizing differences." Handbook of Labor Economics 1 (1986): 641-692.

Rossin-Slater, Maya, Christopher J. Ruhm, and Jane Waldfogel. "The effects of California's paid family leave program on mothers' leave-taking and subsequent labor market outcomes." Journal of

Policy Analysis and Management 32, no. 2 (2013): 224-245.

Schönberg, Uta, and Johannes Ludsteck. "Expansions in maternity leave coverage and mothers' labor market outcomes after childbirth." Journal of Labor Economics 32, no. 3 (2014): 469-505.

Timpe, Brenden. "The labor market impacts of America's first paid maternity leave policy." Journal of Public Economics 231 (2024): 105067.

Sharma, Garima. "Monopsony and gender." Unpublished manuscript (2023).

Sharma, Garima, Shreya Tandon, Lisa Ho, Pulak Ghosh and Stephanie Hao. "Are Some Firms Better for Women's Careers?". AEA Papers and Proceedings 2025.

Summers, Lawrence H. "Some Simple Economics of Mandated Benefits." The American Economic Review 79, no. 2 (1989): 177-183.

Timpe, Brenden. "The labor market impacts of America's first paid maternity leave policy." Journal of Public Economics 231 (2024): 105067.

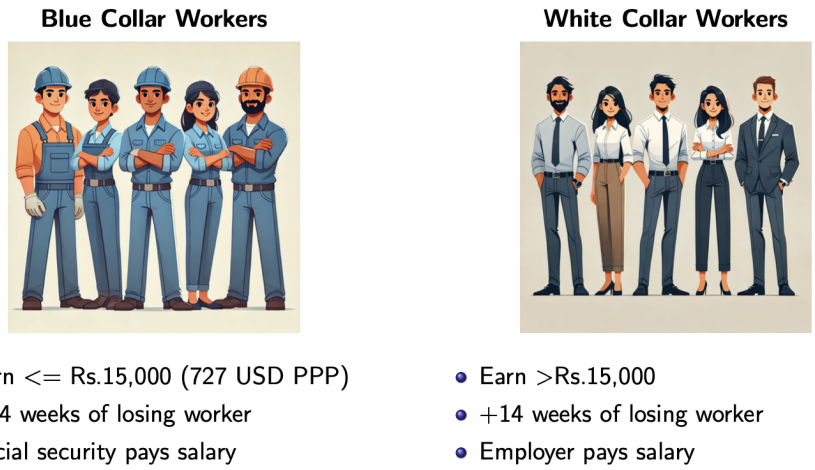
Waldfogel, Jane. "The family gap for young women in the United States and Britain: can maternity leave make a difference?." Journal of labor economics 16, no. 3 (1998): 505-545.

World Bank. Women, Business and the Law Database. Washington, DC: World Bank Group. 2018.

Figures

Figure 1: 2017 Maternity Benefits Amendment Act (MBA) of India

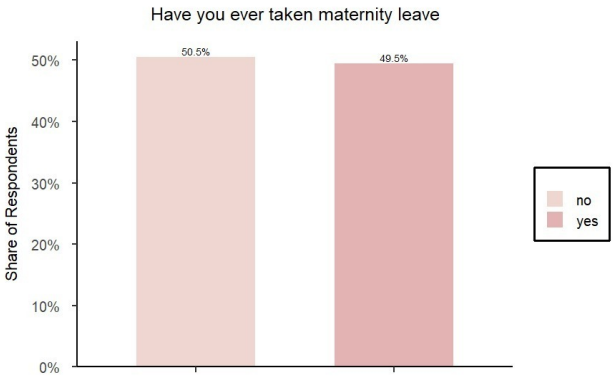
(a) Reform provisions differ by wage threshold



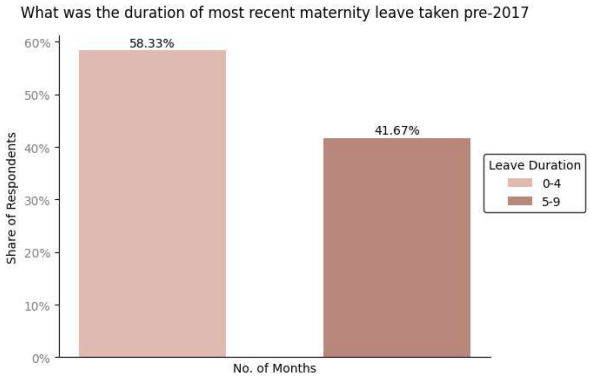
Notes: Figure reveals how the reform differently changed the cost of providing maternity leave for workers with monthly earnings below Rs.15,000 (blue collar workers) and above Rs.15,000 (white collar workers). Blue and white collar are approximate terms. Monthly earnings of Rs.15,000 correspond to the 67th percentile of the wage distribution in India.

Figure 2: First stage effect on duration of leave offered (survey)

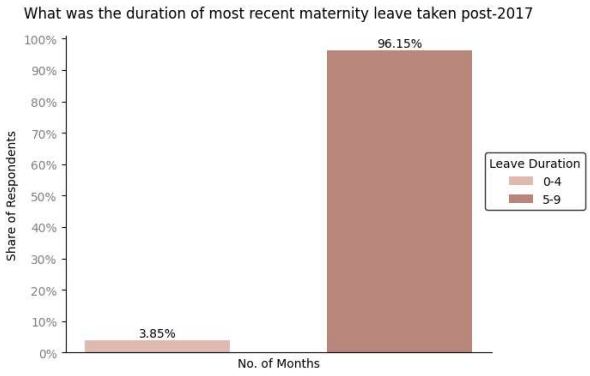
(a) Incidence of leave



(b) Pre-period duration of leave



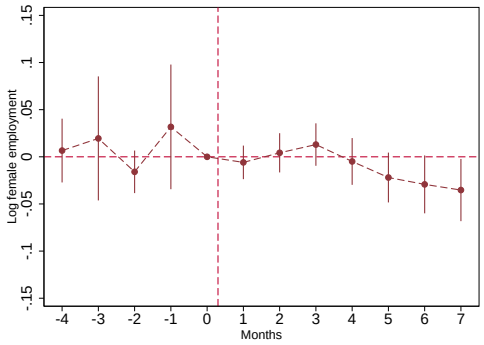
(c) Post-period duration of leave



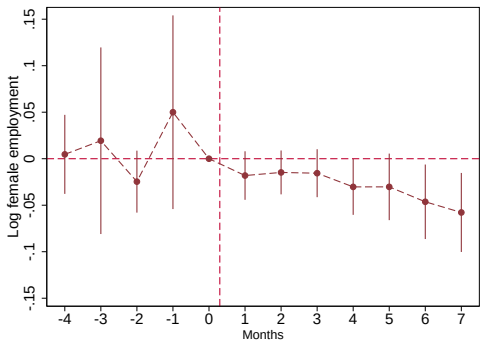
Notes: These figures report responses from a survey of female workers employed at a representative sample of 357 large firms across India (>200 employees), of which 49% of women have taken maternity leave in the last five years. The survey sample is described in Section 2.

Figure 3: Effects on female employment

(a) All firms



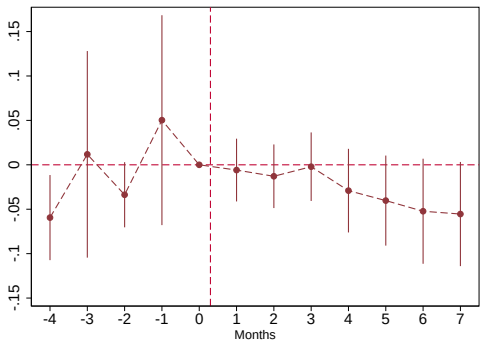
(b) Large firms (> 75 workers)



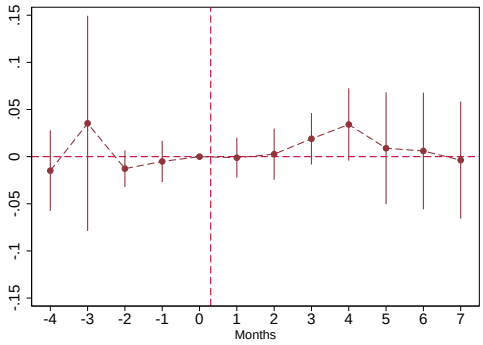
Notes: This figure reports the treatment effect on the log of female employment at all firms (Panel A), and large firms with over 75 workers (Panel B) using the regression specification in equation (1). Results use social security records for workers with monthly earnings below Rs.15,000 (blue collar workers). Regressions control for establishment fixed effects and state-establishment size bin-time fixed effects. Establishment are divided into four equally-sized bins at baseline. Time progresses in months. Standard errors are clustered by establishment. Large firms in Panel B account for over 77.5% of formal employment in India. N workers = 9.27 million.

Figure 4: Effect on female employment – by age

(a) 18 - 35 years

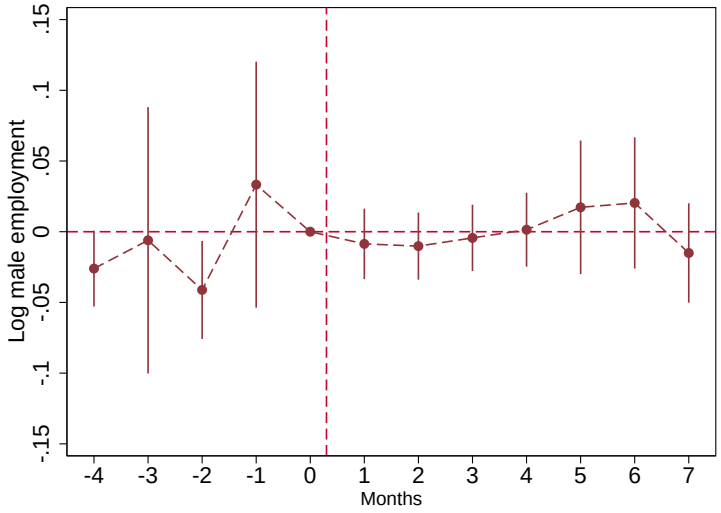


(b) Over 35 years



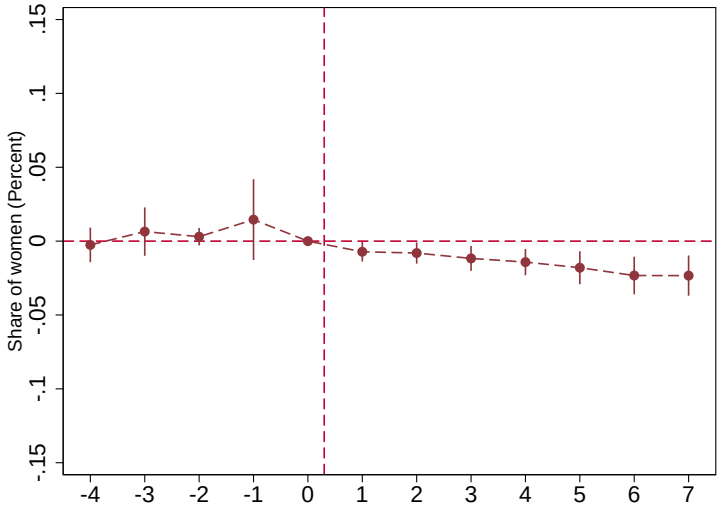
Notes: This figure reports the treatment effect on the log of female employment at firms with over 75 workers using the specification in (1). Panel A reports effects for young women between 18 - 35 years and panel B for older women above 35 years. Regressions control for establishment fixed effects and state-establishment size bin-time fixed effects. Establishment are divided into four equally-sized bins at baseline. Time progresses in months. Standard errors are clustered by establishment.

Figure 5: Effects on male employment



Notes: This figure plots the treatment effect on log male employment using the DiD specification in equation (1). The sample includes establishments with over 75 workers but results are invariant to using all establishments instead. Standard errors are clustered by establishment. N workers at these establishments = 24.05 million men

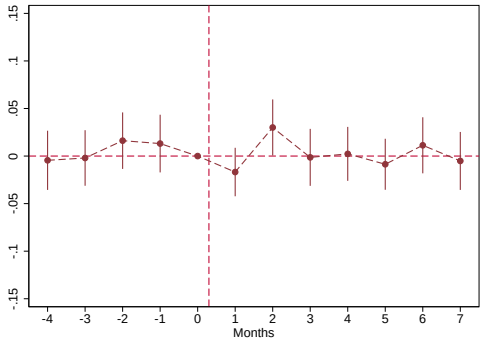
Figure 6: Share of women workers



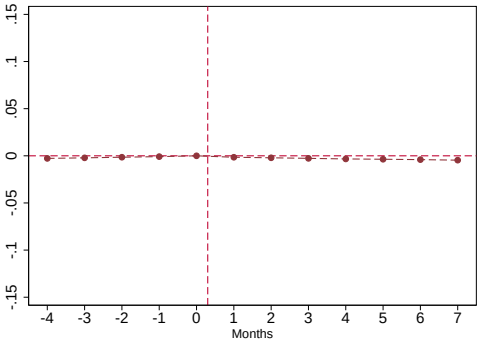
Notes: This figure plots the treatment effect on the share of female workers using DiD the specification in equation (1). The sample includes large establishments with over 75 workers but results are invariant to using all establishments instead. Standard errors are clustered by establishment.

Figure 7: Contract work

(a) Female share, contract firms



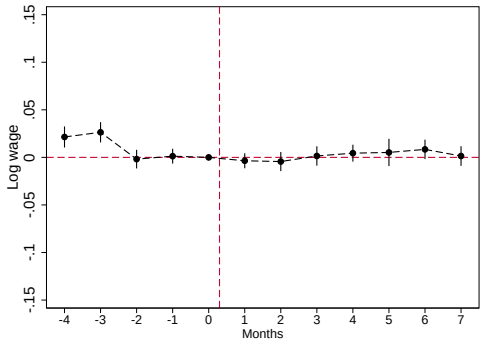
(b) Female incumbent, switch to contract firms



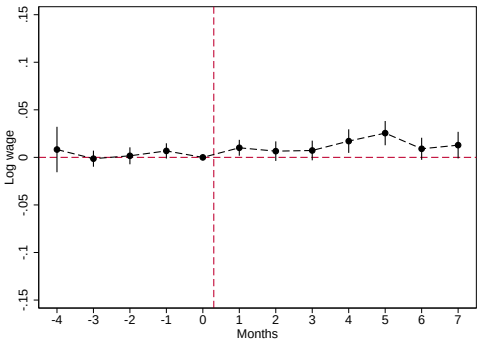
Notes: Panel A shows a simple event study of the female share at the top 30 contract firms in India, which employ nearly a third of contract workers. The regression compares establishments to themselves in $t = -1$ by including establishment fixed effects. The figure reports coefficients on time dummies. Panel B reports the DiD treatment effect on the likelihood of a female incumbent worker switching into contract firms. The worker-level regression uses the specification in equation (2). Standard errors are clustered by establishment.

Figure 8: Effect on wages

(a) Female incumbents



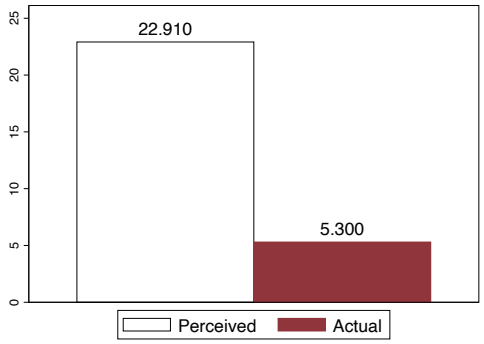
(b) Male incumbents



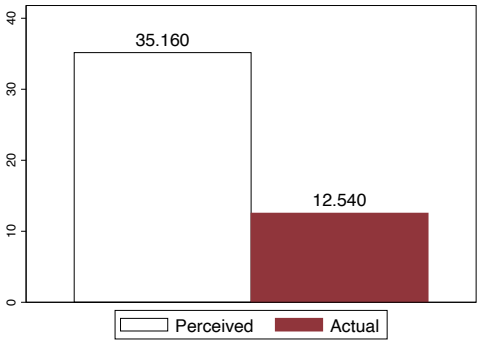
Notes: This figure reports the treatment effect on the log wage of incumbent workers using the specification in equation (2). The sample includes incumbents at firms with over 75 workers but is invariant to using all workers instead. Panel A reports effects for women and panel B for men. Regressions control for worker fixed effects and state-establishment size bin-time fixed effects. Establishment are divided into four equally-sized bins at baseline. Time progresses in months. Standard errors are clustered by establishment. N workers = 40.33 million (9.27 million women, 31.05 million men).

Figure 9: Survey evidence of employer misperceptions

(a) Year 0-1: Perceived vs. Actual

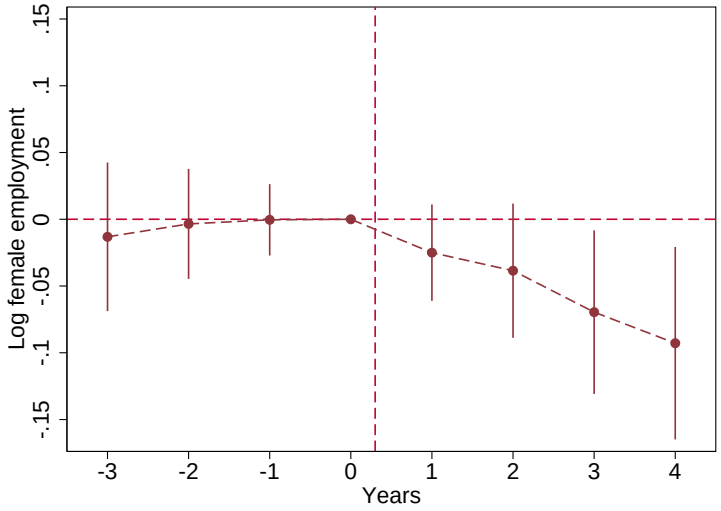


(b) Year 0-2: Perceived vs. Actual



Notes: This figure reports employer perceptions of women's rate of leave taking versus reality. The data come from a pilot survey of 41 HR managers in the IT industry in Tamil Nadu India. The question posed to managers was: "At IT firms like yours in Tamil Nadu (between a and b employees, and with a similar turnover). For every 100 female employees that these firms hired in entry-level roles since 2016, how many women do you think: (i) took maternity leave within the first year? (ii) within the first two years? and (iii) did not take maternity leave within the first two years?

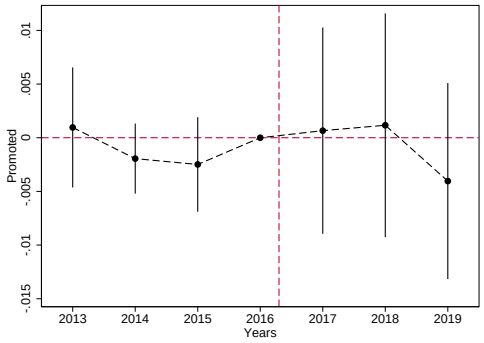
Figure 10: Effect on female employment – LinkedIn records (annual)



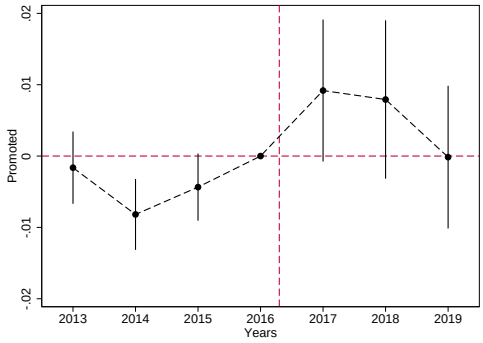
Notes: This figure plots the effect on log female employment using the DiD specification in equation (1). The results pertain to workers o LinkedIn. The sample includes establishments with over 75 workers. The data source is the universe of LinkedIn profiles rather than social security records (which form the basis of Figures 1 through 6).

Figure 11: Career trajectories: promotion into managerial positions

(a) Female incumbents



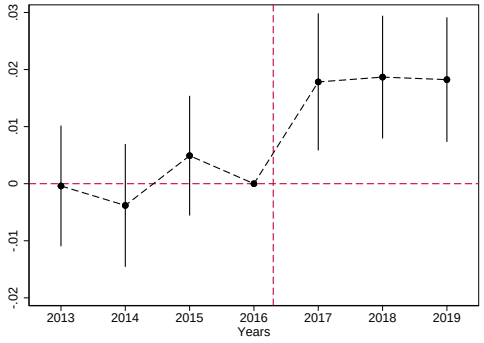
(b) Male incumbents



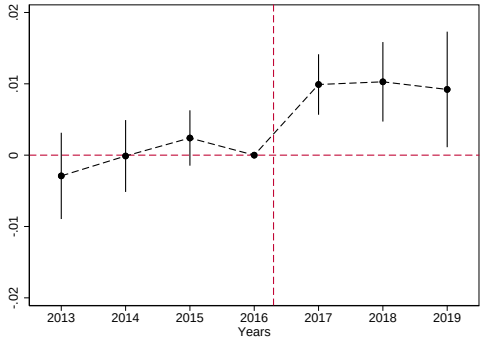
Notes: For incumbent workers, this figure reports the treatment effect on the likelihood of promotion into a managerial position using the specification in equation (2). The results pertain to white collar workers. The sample includes incumbents at establishments with over 75 workers. The data source is LinkedIn records. Standard errors are clustered by establishment.

Figure 12: Career trajectories: Abstract v.s. Manual Role

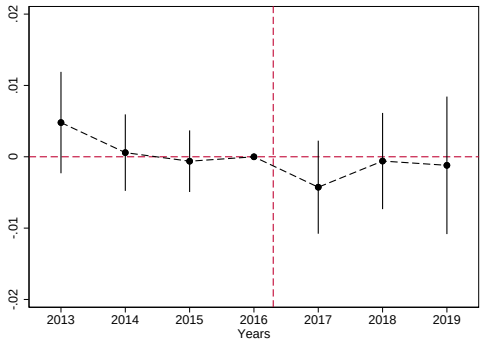
(a) Female incumbents: Abstract Role (women above 35 years)



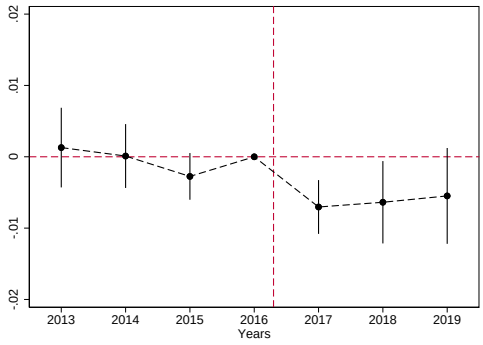
(b) Male incumbents: Abstract Role



(c) Female incumbents: Manual Role



(d) Male incumbents: Manual Role

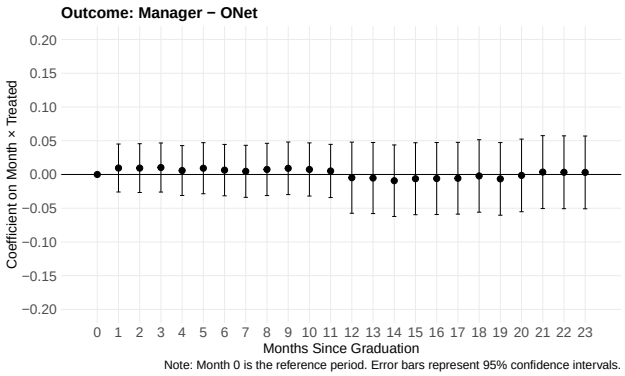


Notes: For incumbent workers, this figure reports the treatment effect on the likelihood of performing an "abstract" role (Panels (a) and (b)) and a "manual role" (Panels (c) and (d)) using the specification in equation (2). Panel a reports effects for incumbent women above 35 years of age at the time of the MBA's implementation. Panels b through d impose no age restrictions. The results pertain to workers observed in LinkedIn records. The sample includes incumbent workers at establishments with over 75 workers. Standard errors are clustered by establishment.

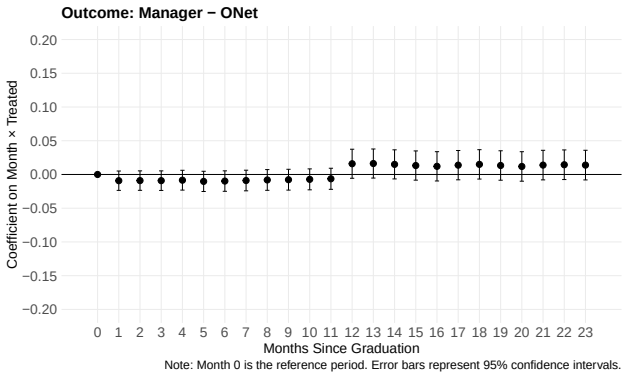
Figure 13: Career trajectories, labor market entrants

Promotion into managerial positions

(a) Female entrants

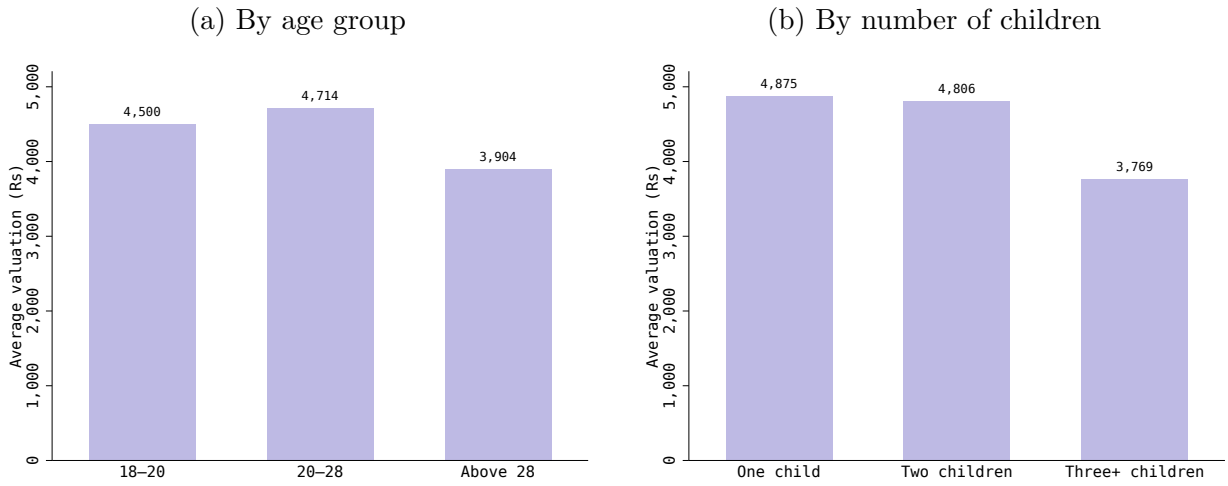


(b) Male entrants



Notes: For labor market entrants, this figure reports the treatment effect on the likelihood of promotion into a managerial position using the "career clone" design in equation (3), which compares labor market entrants entering the labor market in 2017 (post-reform) to graduates from the same university-degree program who graduated in 2015. For example, Bachelor's in Chemistry from the College of Engineering Pune or Bachelor of Science in Electrical Engineering from Amity University. The results pertain to workers present in LinkedIn records. Standard errors are clustered by worker.

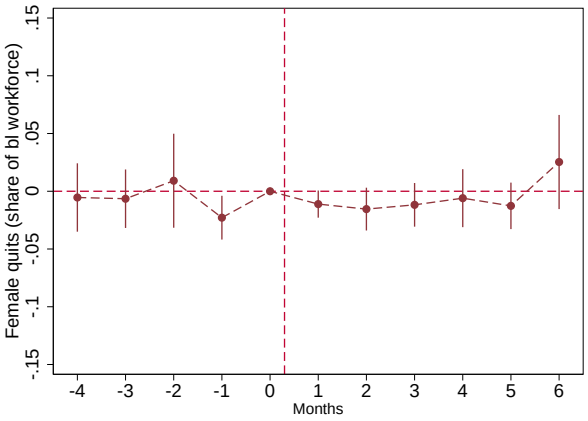
Figure 14: Valuation of maternity leave ($V_{26} - V_{12}$ in Rs./month)



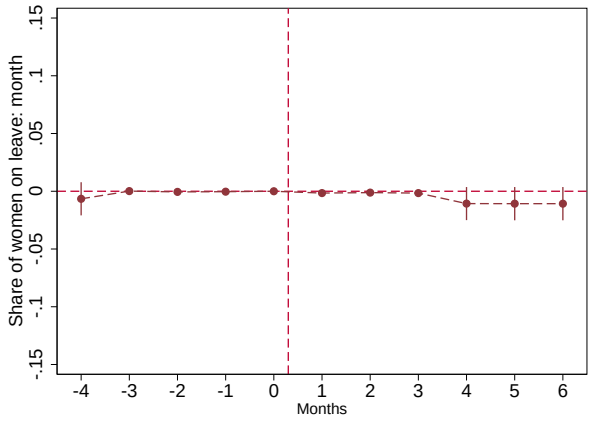
Notes: From the survey of 312 factory workers in industrial areas near Delhi.

Figure 15: Adverse Selection (change in female quits and leave-taking at control firms)

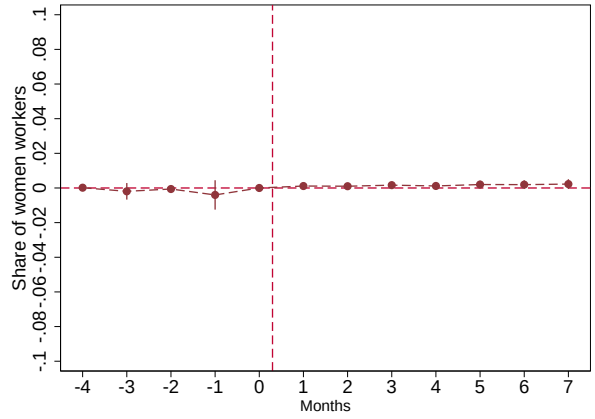
(a) Female quits



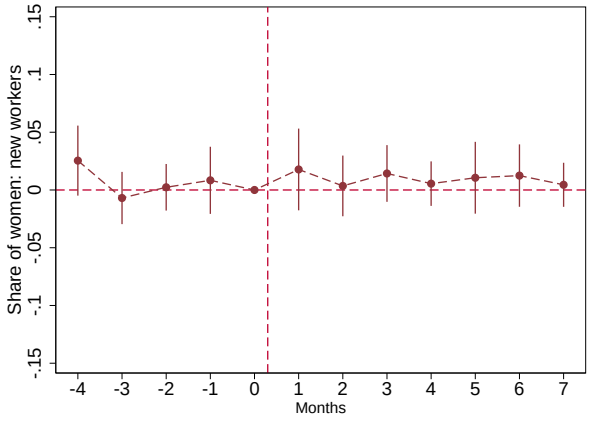
(b) Female leaves



(c) Female workers (Total employees)



(d) Female workers (New employees)



Tables

Table 1: New worker wages

	Women	Men
	(1)	(2)
$T_i \times$post	-0.013	-0.013
	(0.008)	(0.009)
Observations	389,143	389,143

Table 2: Heterogeneity in female employment effects

	(1)	(2)	(3)
	Panel A: Female share of industry		
	Low	Medium	High
$T_i \times$ post1	-0.057**	-0.017	-0.003
	(0.026)	(0.034)	(0.045)
$T_i \times$ post2	-0.080**	-0.061**	0.003
	(0.041)	(0.031)	(0.055)
N	105,559	505,708	208,553
	Panel B: Number of female workers		
	Low	Medium	High
$T_i \times$ post1	0.012	-0.028	-0.026
	(0.022)	(0.024)	(0.018)
$T_i \times$ post2	-0.005	-0.086**	-0.069**
	(0.063)	(0.040)	(0.026)
N	200,073	386,579	204,914

Notes: Table reports heterogeneity in the effect on female employment by the female share of a two-digit industry and number of female workers in an establishment, using social security data (EPFO). Post-period 1, post1, corresponds to the first four months following policy implementation and post-period 2, post2, corresponds to the next three months. Low denotes to the bottom 25th percentile, Medium denotes between the 25th and 75th percentile, and High denotes above 75th percentile. For example, two-digit industries in the bottom 25th percentile by female share fall in the "low" category. Industries with low female shares are below 8.2% female, medium female shares are between 9 and 23% female and high female share are between 23% and 85% female. Establishments with low female workers employ below 5 women, medium employ between 7 and 44 women, and high employ between 45 and 2000 women. The sample resembles Figure 3b.

Table 3: Industry examples by female share

Low	Medium	High
Manufacture of basic metals	Manufacture of electrical equipment	Manufacture of food products
Construction of buildings	Legal and accounting activities	Education
Wholesale trade	Financial service activities, except insurance and pension funding	Crop and animal production, hunting and related services
Civil engineering	Manufacture of chemical products	Manufacture of textiles
Petroleum products	Printing and reproduction of recorded media	Computer programming, consultancy and related activities
Chemical products/wood products	Manufacture of fabricated metal products	Professional activities/Scientific research

Notes: Table gives examples of low, medium, and high female share industries governing heterogeneity in Table2.

Table 4: Effects on female employment in mfg and services

	Manufacturing	Services
	(1)	(2)
$T_i \times$ post1	-0.031	-0.013
	(0.023)	(0.031)
$T_i \times$ post2	-0.095**	-0.043*
	(0.040)	(0.024)
N	247,266	572,554

Notes: Table reports heterogeneity in the effect of India's maternity leave expansion on female employment in manufacturing industries (column 1) and the service sector (column 2). The sample resembles Figure 3b.

Table 5: Welfare (back-of-the-envelope estimates)

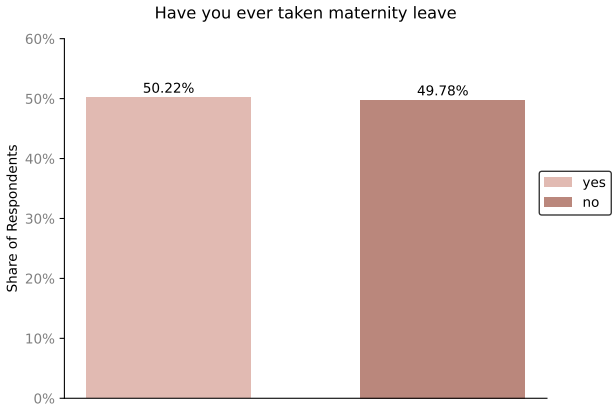
	Workers		Employers		
	Coverage	Job loss	Cost	Adverse selection eliminated	Profits
	(1)	(2)	(3)	(4)	(5)
Million worker salaries per month	2.610	-0.152	-2.196	0.013	NA
As share of wage bill	0.303	-0.018	-0.255	0.002	NA

Notes: Table reports first estimates of India's maternity leave expansion. Values are shown in million worker salaries per month and as a share of the female wage bill at affected firms (8.62 million female workers).

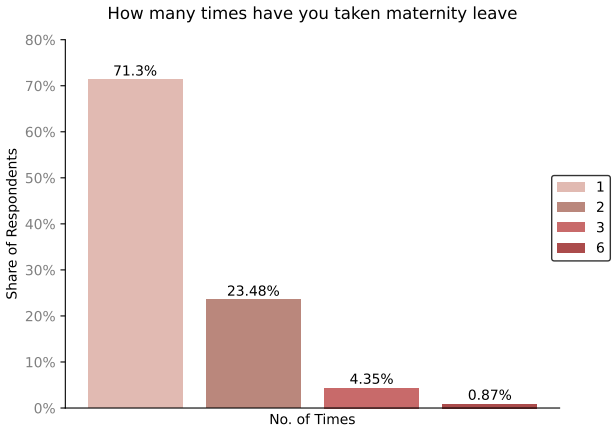
Appendix Figures

Figure A.1: Likelihood of leave (survey respondents)

(a) Incidence of leave



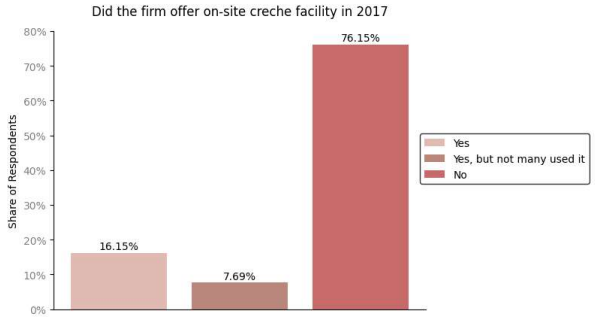
(b) Number of leaves



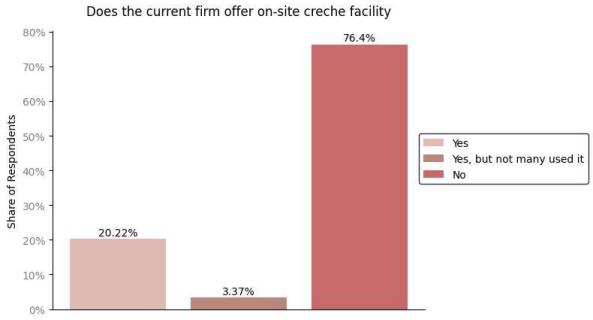
Notes: From the online survey of female workers at 357 firms, the left panel plots the share who took maternity leave in the past five years and right panel plots the number of leaves taken.

Figure A.2: Compliance with MBA 2017 Creche provision

(a) Creche: pre



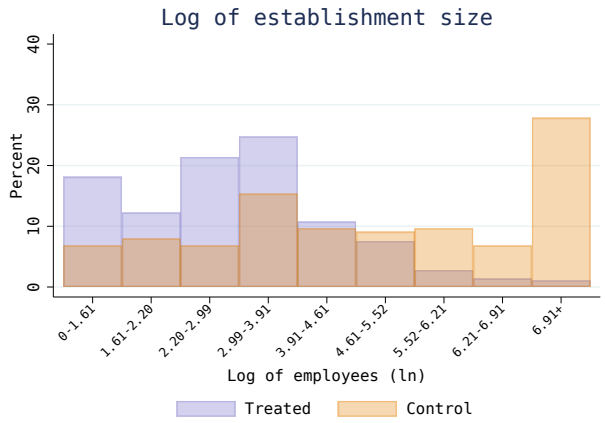
(b) Creche: post



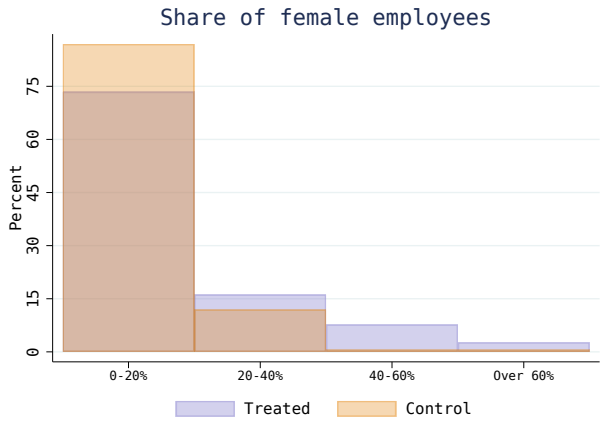
Notes: From the online survey of female workers at a representative sample of 357 large firms (>200 employees, intended N =500), the left panel plots the provision of creche facilities at a respondent's employer before the MBA was passed and right panel plots this provision after the reform's passage.

Figure A.3: Summary Statistics: Treated and Comparison Firms at Baseline

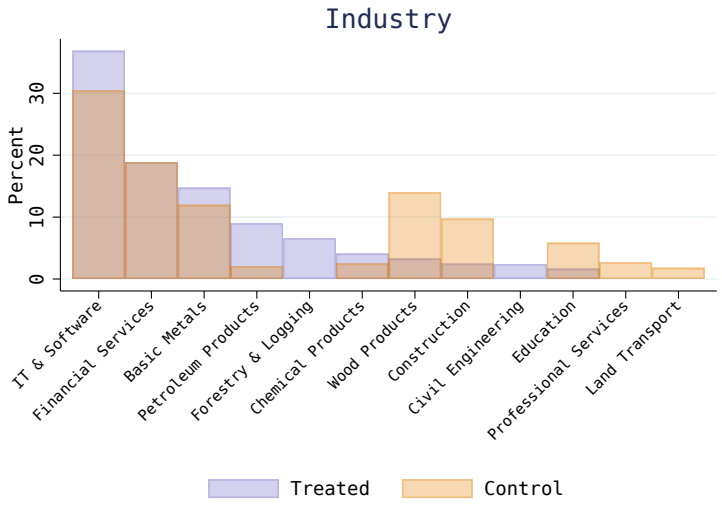
(a) Firm size (log)



(b) Female share

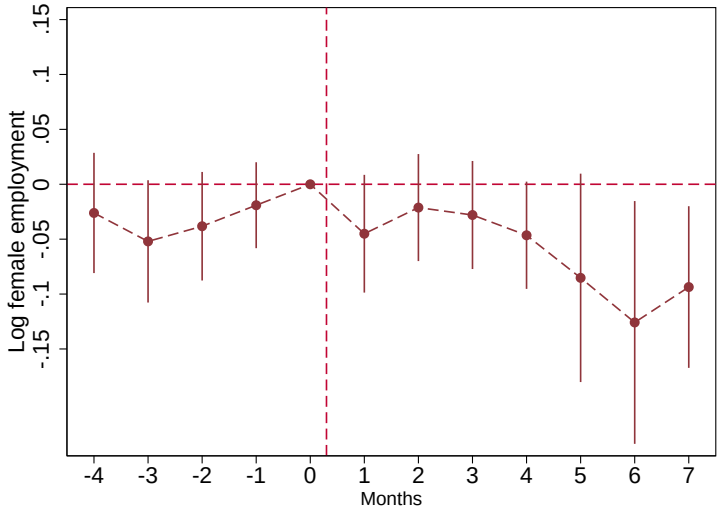


(c) Industry



Notes: The sample includes treated and control establishments represented in the EPFO data at baseline (April 2017). Panel A plots the distribution of the log establishment size for treated and control establishments. Panel B plots the distribution of female employment share for the two sets of employers. Panel C plots the distribution of industry codes, limiting to the top-10 most common industries.

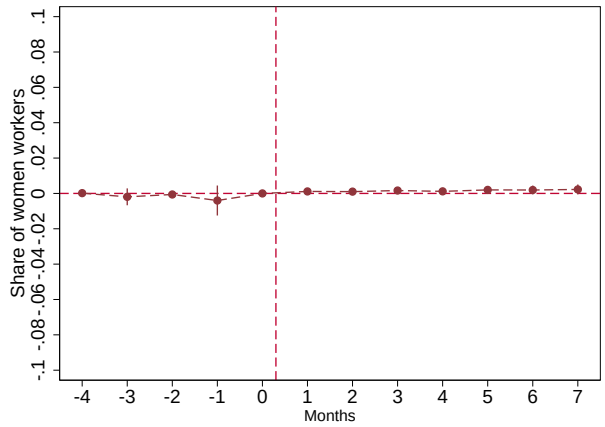
Figure A.4: Effects on female employment (establishments with pre-period leave information)



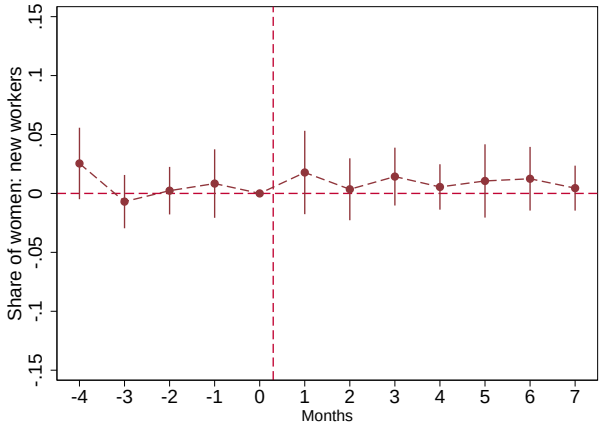
Notes: This figure plots the treatment effect on log female employment using the DiD specification in equation (1). The sample includes establishments with over 75 workers for which we have data on pre-period leave durations, not assuming firms with missing information offer the legal default. Standard errors are clustered by establishment.

Figure A.5: SUTVA test: share of women at control firms

(a) Share of women (total employees)



(b) Share of women (new employees)



Notes: The sample includes control establishments represented in the EPFO data at baseline (April 2017). Panel A plots the share of female employees out of *total* employees at the establishment relative to the month before the reform. Panel B plots the share of female employees out of *new* employees at the establishment relative to the month before the reform. 95% confidence intervals are shown.