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

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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 that 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). Estimates indicate the mandate was benefit-cost neutral: it benefited employed women while raising costs for employers, with minimal impact on adverse selection at firms that already offered long leaves before the reform.

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1 Introduction

Over 145 countries offer some form of paid maternity leave to mothers (Hyland et al. 2020). In recent years, these policies have proliferated across developing countries, with India, Nigeria, and Pakistan all expanding their duration of paid maternity leave for new mothers over just the last decade. However, while much is known about how such policies impact the labor market outcomes of mothers—by comparing mothers who give birth just after a reform to those who give birth just before (e.g., Lalive and Zweimüller 2009, Dustmann and Schönberg 2012, Schönberg and Ludsteck 2014, Bailey et al. 2025)—virtually no evidence exists on how paid maternity leave impacts young women who are not mothers, older women, or men. On the one hand, longer leave could benefit both employers and women by increasing the recruitment and retention of female workers, particularly 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 by leading women into less complex or less senior roles if employers expect to lose them for longer. Conversely, if employers expect new mothers to return to work at higher rates, they may *want* to place women in more senior or complex positions.

This paper examines the impact of maternity leave expansions on labor market outcomes using a landmark 2017 reform in India that expanded paid maternity leave from 12 to 26 weeks, known as the 2017 Maternity Leave Benefits Amendment Act (henceforth the MBA). With this expansion, India ranked fourth globally in benefit generosity.[1] Our empirical strategy exploits variation in the pre-period duration of maternity leave offered across employers, driven by parent company policies, to compare firms that previously offered only 12 weeks of paid leave (the treated group) to those that already offered 26 weeks of leave before the reform (the comparison group). The two sets of establishments closely resembled each other in baseline characteristics, including two-digit industries, female share of the workforce, 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 × state × 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.

Our analysis relies on linking four rich datasets: (i) pre-period duration of maternity leave from the universe of Glassdoor reviews, newspaper announcements, and a survey of a representative sample of 500 large firms across India[2], (ii) worker outcomes from employer-employee linked

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

[2] Altogether our study gathers pre-period leave information for 40% of formal workers in India. Results are robust to

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 912 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 law imposed different costs for two distinct groups of workers, leading 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 while they were on 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 is thus different, and we report 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 women by reducing their employment or by substituting toward men or older female workers. 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 fell by 6% within the first year and by 10% 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 average 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 its implementation, and these women moved into unemployment rather than into informality. The MBA-induced decline increased aggregate female unemployment or exiting the labor force among workers educated beyond primary school by 9-11% between 2017 and 2019 (Periodic Labor Force Survey). Our evidence points to a contraction of labor demand as driving these results rather than changes in labor supply.[4]

restricting the treated group to establishments with definitive information on the pre-period duration of leave rather than assuming that 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 (Periodic Labor Force Survey 2017).

[4]Standard labor supply models would predict that maternity leave expansions should lead labor supply and hence employment to rise, as women shift toward or are retained by employers offering better amenities. Labor supply could decrease if the reform raises fertility or lowers the rate of returning to work after childbirth, but these effects would have to be implausibly large to explain the observed decline in female employment (see Section 3.3).

Our second result is that employers did not offset the cost of longer leaves by reducing women's 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 declines, below -1%, at the 95% confidence level. 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 wages contrasts with the simple economics of mandates, which predicts that mandating a valuable amenity should lead wages to decline (Summers 1989). Valuable amenities should shift labor supply to the right, as workers are willing to supply more labor at the same wage, and shift labor demand left, as workers grow expensive. Wages decline, and employment may not fall if workers sufficiently value the benefit, i.e., if the right shift of labor supply fully offsets the left shift of labor demand. The absence of wage changes in India then has two possible explanations. The first is that women did not value longer leaves, which limited the scope for wage reductions.[6] The second explanation is that wage rigidity prevented employers from reducing women's wages—for instance, due to equity considerations, nominal rigidities, or binding minimum wages (e.g., Breza et al. 2018, Kaur 2019, Breza et al. forthcoming, Sharma 2025)—and led to lower employment instead. Our evidence points against women not valuing leave and toward rigid wages instead: surveys of both white-collar and blue-collar women show that nearly 100% took the full duration of maternity leave after the reform, even among women who had previously taken only 12 weeks. Women's incentivized willingness-to-pay for extended leave also lay between 25-30% of their monthly wage. Together, these findings point against women placing little value on longer leaves and indicate that employment declined because wages could not fall.

We next turn to studying heterogeneity in the impact of the MBA by industry. The shift in labor demand induced by the MBA is likely to have been larger for industries where replacing female workers is harder—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 greater substitutability between male and female workers, while high-female-share industries exhibit lower substitutability.[7] Consistent with theory, we find that the decline in female employment was concentrated in

[5]Wage results pertain to the 40.33 million workers observed in social security data.

[6]If women are paid their reservation wage and labor supply is flat, then a leftward shift of labor demand will leave wages unchanged if women do not value the amenity (i.e., amenity provision does not lower the reservation wage). Labor supply remains flat and lower demand reduces employment.

[7]Theoretically, when the value-added production function is Cobb-Douglas in capital and labor and labor is CES aggregation of male and female workers, $Y = ZK^{\alpha_1}L^{\alpha_2}$ and $L = (\beta_{industry} l_f^{\rho} + l_m^{\rho})^{1/\rho}$, low female CES shares correspond with women's lower relative productivity in the industry, low $\beta_{industry}$, embellished by a high elasticity of substitution between male and female workers. Table 3 reports examples of low-, medium-, and high-CES-female-share

industries where women are more easily substituted in production with men, i.e., industries with low- or medium-female-shares, including financial services, retail trade, and the manufacturing of electrical equipment or chemical products. High female-share industries such as textile manufacturing or education saw no decline in female employment. 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 employed many female workers. Male employment increased 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.[8] The risk of losing a worker for longer could lead employers to place women in more routine and less managerial roles, with lower replacement costs. 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 to manual or routine positions with lower firm-specific human capital (by 1.1%). In contrast, men and older women (above age 35) were more likely to be promoted as managers and to move from routine to abstract positions involving greater firm-specific human capital. Men were 10% more likely to be promoted into managerial positions and 1.1% more likely to be placed in abstract positions. For labor market entrants, we adopt a different design, comparing same-gender graduates from the same university and degree program who entered the labor market immediately after the policy to those who graduated just before the reform. Since university admissions in India rely on exam scores on entrance exams or high-school leaving exams, individuals in the same university-and-degree program are similarly selected at matriculation and receive identical educations. The policy increased the gender gap in job seniority in the first two years after graduation by 12%.

In sum, our findings show that expanding paid maternity leave in India from 12 to 26 weeks led to large reductions in female employment among both blue- and white-collar workers. A potential silver lining masked by this decline is that, to justify the large magnitude of the response, employers would have to overestimate the rate of leave-taking among women (Conlon and Sharma 2024). A simple back-of-the-envelope calculation reveals that employers would have to believe that 27% of women take leave within the first year of employment compared to a true rate of just 5%. A pilot survey of HR managers at IT firms in India reveals misperceptions at about these levels. 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, we develop a broader framework to assess the impact of maternity leave policies. We

industries.

[8]Career trajectory results pertain to the 17 million white-collar workers observed on LinkedIn.

begin by examining benefits versus costs. One rationale for government-mandated maternity leave policies is adverse selection: firms may not wish to be the only entities offering generous maternity leave for fear of disproportionately attracting women of childbearing age who will take it. It may therefore be individually suboptimal for firms to provide leave but collectively optimal. A second rationale is that women benefit from leave even as some lose jobs.

Evaluating net benefits from maternity leave policies requires four key ingredients: the value of leave to women, the adverse selection penalty, the distribution of costs across employers, and profits net of labor costs. We use incentivized valuations to measure women's valuation of leave, asking 912 female workers across the income distribution what monthly pay increase they would need to switch from 26 to 12 weeks of paid leave. An incentivized version asked women to predict the pay increase that other women like them would require, with accurate answers entered into a lottery to win $50 (worth 30% of the average respondent's monthly wage). We measure adverse selection by comparing control firms in markets where many firms already provided longer leaves (no adverse selection) to those in markets where few other firms did (adverse selection was present before but eliminated by the policy). Finally, we use the distribution of employment responses to back out the distribution of costs across firms.

The reform increased worker benefits by approximately as much as it raised employer costs, with minimal evidence of adverse selection. Benefits equaled 30% of the female wage bill at baseline, costs equaled 25%, the cost of job loss equaled 2%, and alleviating the adverse selection penalty less than 1%. Ongoing work investigates how other policies that the government could implement—including shorter leave expansions, restricting coverage to low-cost firms, or maternity leave insurance to offset firm losses—could preserve some fraction of the benefits to women while limiting costs to employers and women who lost jobs. Quantifying the impact of alternative policies requires estimating labor demand, labor supply, and mapping different durations of leave to worker replacement costs, ongoing.

Taken together, the findings of this paper show that the Maternity Benefit Amendment Act of 2017, which expanded paid maternity leave in India from 12 to 26 weeks, benefited mothers who enjoyed longer periods of paid leave and extended job protection, but simultaneously led to unintended consequences for younger women by reducing their employment and slowing their career growth. Our findings highlight the imperative for policymakers to anticipate both demand- and supply-side responses when designing benefit mandates, and to assess how alternative policy designs could preserve some benefits of maternity leave policies while limiting costs and job loss.

Related Literature This study is most closely linked to a seminal study by Gruber (1994), which finds that the incidence of mandated coverage of maternity health benefits in U.S. insurance plans fell entirely on women's wages. Gruber and Krueger (1991) similarly find incidence of mandated workers' compensation insurance falling on wages rather than employment. In our setting, wage

rigidity led incidence to fall on employment rather than wages.

Second, in labor economics, several papers document mixed effects of mandated maternity leave on the labor market outcomes of mothers, by comparing mothers who give birth immediately after a policy to those who give birth just before. Studies find positive long-run labor force attachment for mothers in Europe (Lalive and Zweimüller 2009, Dustmann and Schönberg 2012, Schönberg and Ludsteck 2014), negative long-run effects on employment and earnings for mothers in California (Bailey et al. 2025), and positive effects on mothers' working hours and wages across U.S. states (Rossin-Slater et al. (2013), Waldfogel (1998)). More recently, Timpe (2024) uses the staggered expansion of paid maternity leave through disability insurance across U.S. states in the 1960s and 1970s to show declines in the wages and employment of women of childbearing age. We contribute by studying outcomes for all workers, including young women who are not mothers, older women, and men. Social security data enable us to track granular wage and employment trajectories, and LinkedIn data enable us to track workers' career trajectories.

Finally, while there is an active and growing literature on the 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. 2024), complementing the predominant focus on labor supply determinants such as conservative gender norms and safety concerns (e.g., Bernhardt et al. 2018, Borker et al. 2021, Field et al. 2021, Jayachandran 2021, Agte and Bernhardt 2025, Garlick et al. 2025, Ho et al. 2024, Jalota and Ho 2024, 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 determinants of low female engagement in formal employment (the sample in this study covers 41% of the non-agricultural workforce in India).

The paper proceeds as follows. Section 2 describes the setting and empirical strategy. Section 3 reports effects on labor market outcomes and provides evidence that effects on female employment may be driven by employer misperceptions. Section 4 develops a framework to conduct cost-benefit analysis and evaluate counterfactual policy alternatives. Section 5 concludes.

2 Reform, Data, and Empirical Strategy

2.1 India's Maternity Benefit Amendment Act of 2017

In 2017 India enacted a major 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 termed the MBA) made India's policy

the fifth most generous in the world in pay-equivalent weeks[9] (World Bank Group 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 the law predominantly served to expand 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 (Section 2 describes the sample). 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 3), compliance with other provisions of the law was more 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 employees reporting no provision of crèche facilities even after the reform, a share that remained unchanged relative to 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 2). First, for workers who earned below Rs. 15,000 per month (767 USD adjusted for PPP)—termed blue-collar workers—employers lost new mothers for fourteen additional 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 who earned above Rs.15,000 per month, employers both lost new 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 group. The terms "blue-collar" and "white-collar" are approximate and Rs.15,000 corresponds to the 67th percentile of the salaried wage distribution in India (Periodic 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 personnel.

[9]Behind Bulgaria (410 days at 90% pay), Norway (49 weeks at 100% pay), Canada (12 months at 55% pay), and Albania (6 months at 80% pay followed by 6 months at 50% 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.

We study the effect of the MBA on formal workers in India. Our sample includes 57.3 million workers present in social security records and the universe of LinkedIn profiles, of which 41.7 million workers are men and 15.6 million are women. These workers constitute 41% of the non-agricultural, non-self-employed workforce in India (PLFS 2017-18).

2.2 Women and the Formal Labor Market in India

India has a total labor force of 330 million workers, of which nearly half or 151 million workers are non-agricultural and non-self-employed, including 125 million men and 26 million women (Periodic Labor Force Survey, PLFS 2017-2018). Over 45% of non-agricultural and non-self-employed workers are formally employed and maked social security contributions. Formality rates are higher among women, at 62%, compared to 41.5% of men. The formal sector in India is expanding: the number of EPFO subscriptions approximately doubled between 2019 and 2025.

Formal sector positions offer some of the best employment opportunities in the Indian economy, with wages about 2.5 times higher on average and better amenities than 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, 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%. Women's prospects in the labor force also decline upon marriage. Recent estimates suggest that the female labor force participation declines by 12 pp upon marriage, with an additional but smaller penalty at childbirth (Bussolo et al., 2024).

Although workers displaced from the formal sector could in theory be shifted to the informal sector, we do not find any evidence of this in our analysis. Instead, women are more likely to leave 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 workers with education above secondary school (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 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) 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. 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.[10] 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.[11] 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.[12] 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.

[10] 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.

[11] 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.

[12] 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.[13]

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 factory workers in industrial areas near Delhi collected valuations for blue-collar women (N=412). Respondents reported 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 over 30% of the monthly salary of blue-collar workers. The survey collected demographic and employment information to analyze how willingness to pay for leave 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

[13] Discrepancies between raters 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.[14] 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 the State Bank of India), while several prominent banks outside the association continued to offer 12 weeks (including IDBI, HSBC, and ICICI).[15] 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 (Figure A.6).

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

Summary Statistics Figure A.3 reports summary statistics from the EPFO data comparing treated and comparison establishments in April 2017. Comparison 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 comparison 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:

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

[14] Sources here and here.

[15] 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 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 (calculated from social security records). Results remain robust to setting establishment size thresholds at 100, 150, or 200 workers instead, with results available upon request.

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

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

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 at firms offering short and long durations of maternity leave in the pre-period, controlling for time-varying shocks at the two-digit-industry x state x firm-size level. The essence of this identifying assumption is that treated and comparison firms are "similar" enough that they would experience common shocks to labor demand and labor supply absent the policy, thereby exhibiting parallel trends in counterfacutal employment—differencing out shocks at comparison firms then isolates the treatment effect of the policy. Three patterns support the identifying assumption: (i) treated and comparison firms closely resembled each other in distributions of two-digit industries and female shares at baseline, (ii) restricting the sample to similar large firms (with >75 workers) while controlling for industry-specific time-varying shocks produces identical results (Figure 4 Panel B and Appendix Figure A.4), and (iii) parallel pre-trends in core outcomes (employment, female shares, and wages).

A potential threat to identification is a violation of the SUTVA assumption, if the reform directly impacted comparison firms by leading female applicants to switch from comparison to treated firms that now also offered expanded maternity leave, or if it raised labor supply to comparison firms from the women turned away by treated firms. Three results mitigate concerns about a SUTVA violation. First, estimated treatment effects are driven by changes at treated firms rather

than the comparison group. Second, worker composition at comparison firms remains stable, including the share of female hires, share of young female hires, age groups, and leave-taking rates among new hires, ruling out sorting (Figures A.7 and 15). Finally, although the hypothesized mechanisms predict that leave-loving women would exit comparison firms and women released by treated employers would flock to comparison firms, there is no change in worker "churn" at control firms around the passage of the MBA. The stability in worker composition reinforces the absence of a SUTVA violation. The lack of compositional changes likely reflects the appeal of comparison firms as being among the most attractive employers for women, which continued after the reform.[16]

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 substantially overestimate the rate at which women take maternity leave.

Predicted effect Figure A.5 depicts the predicted effect of a mandate. A mandate that provides a valuable amenity should shift labor supply to the right, as workers are willing to supply more labor at the same wage. It should shift labor demand leftward, as workers grow more expensive. Wages will fall, both due to the decline in labor demand and because employers can pass some costs onto workers' wages, who value the amenity. Finally, employment will fall if the leftward shift of labor demand outweighs the rightward shift of labor supply. In sum, the simple economics of mandates predicts that expanding paid maternity leave should lead women's wages to fall and employment to fall if the decline in labor demand exceeds the increase in labor supply (Summers 1989).

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

[16]Level differences between treated and control firms do not violate the parallel trends assumption. The identifying assumption of parallel trends holds as long as SUTVA holds. Why spillovers did not occur ultimately reflects how the labor market functions—comparison firms were among the best employers for women in the pre-period and thus rationed employment. Rationing is evidenced by the fact that these firms did not need to raise wages to expand employment following positive firm-specific demand shocks in the pre-period (available upon request). Although comprison firms were rationing employment and operating on their labor-demand curve, parallel trends hold as long as they experienced common labor-demand shocks with similarly sized firms in the same industry and location. Results remain robust to restricting both comparison and treated firms to observably similar sets (Figure A.6).

workforce or improve retention among incumbent female workers who value longer leaves, thereby 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 with establishment fe.

Figure 4 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 in social security records. Large firms employed over 75% of formal workers in India as of 2016 (calculated from social security records). Female employment evolved in parallel before the reform, but we find a sharp decline of 5.5% within six months of the policy's implementation. This decline was concentrated among young women of childbearing age (18-35 years), whose employment fell by 8.5% (Figure 5, Panel A). By contrast, we find no effect on employment among older women above the age of 35 (Figure 5, 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 6). 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 its passage (Figure 7). 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 thirty largest contract firms in India, which collectively employ over one-third of all contract workers (Figure 8, 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.[17] 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 4 Panel B); the Periodic 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, which exhibit different degrees of substitutability between male and female workers and is explored in Section 3.3.

Figure 10 reports the MBA's impact on the employment of white-collar workers in LinkedIn records. White-collar women's employment declines by 6% within the first year and by 10%

[17]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".

by four years after the policy. 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). This lack of full substitution to male workers is consistent with imperfect substitutability between men and women—policies that raise the relative cost of an imperfectly substitutable input should reduce firm size if employers cannot pass costs onto wages or prices.

The observed effect on employment reflects the cumulative sum of impacts on labor demand and labor supply. However, several factors point to declines being driven by labor demand rather than labor supply. Supply-side factors could in theory explain the decline in employment if the reform increased fertility or if women were less likely to return to work after leave. Yet the evidence points against these channels. 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, there is no effect on the rate of leave-taking among women at treated firms. Finally, we estimate a 9.6% increase in return-to-work from leave due to the policy which would increase rather than decrease labor supply (Figure A.8).[18].

The expansion of paid maternity leave may nonetheless have increased labor supply, such that the decline in female employment reflects the net effect on labor demand. Labor supply could change in three ways. First, on the intensive margin, improved amenities may draw existing female workers to treated firms from the control group since treated firms now also offer 26-week-long leaves. We find no evidence of moves away from control to treated firms, measured as the DiD effect on the monthly share of female workers transitioning to a treated establishment (Figures 15 and A.7). No intensive-margin change is expected within the treated group since all firms simultaneously expanded paid maternity leave, thereby maintaining their relative attractiveness. Several standard models of labor supply predict no change in the relative attractiveness of firms that simultaneously improve amenities: for example, consider a standard utility function where worker i's utility from working at employer j is $u_{ij} = log(w_j) + \beta log(a_j) + \varepsilon_{ij}$, where ε_{ij} epsilon is an idiosyncratic worker-employer match shock with a Type I extreme value distribution, and amenity a_j is log-linear in weeks of leave $log(a_j) = \psi^T X_j + \alpha log(L_j)$.[19] A reform expanding maternity leave

[18]A simple back-of-the-envelope calculation also shows that changes in the rates of returning to work could not possibly explain the decline in female employment. Given the current fertility rate in India of 2 children (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%. A survey of Indian women employed in the corporate, media, and development sectors in 2017 suggests that 73% of mothers do not return to work after having a child (Ashoka University 2018). Even if the reform reduced the rate of returning to work among the remaining 27% of new mothers to 0, this could only contribute to a 3.2% decline in female employment (0.12×0.27). In the LinkedIn sample, we find that female employment at treated establishments declines by 6% to 10% in the four years after the reform relative to control establishments. These results cannot be fully explained by lower rates of return, and we estimate a 9% increase in return-to-work following maternity leave

[19]Relative labor supply shares also remain constant if utility is concave in leave duration, $log(a_j) = \psi^T X_j + \alpha g(L_j)$.

at all firms leaves the relative appeal of each firm unchanged, and any extensive margin increase in women's labor supply affects firms in proportion to their baseline utility. Second, longer maternity leaves may change the extensive margin of labor supply through return-to-work following childbirth. Figure A.8 shows a positive effect, a 9% increase, in women returning-to-work following childbirth. Finally, also on the extensive margin, expanded maternity leave may increase women's entry into the labor force. For white-collar workers, we compare the participation rate of labor market entrants graduating from the same university-degree program (e.g., Bachelor of Science in Electrical Engineering from IIT Bombay), who entered the labor force immediately after the policy versus in the two years prior to implementation. Because cohort sizes are determined pre-policy and workers similarly select into and receive identical educations within the same program, any change in participation across cohorts is attributable to the policy. We find a 2pp increase in female labor force participation in the year following the policy: in 2015, 40% of female graduates reported a position in the first year following graduation, which increased to 42% for the 2017 cohort. In sum, the reform had a modest impact on women's labor supply—on the extensive margin (small positive effect), in return-to-work behavior (positive effect), and on the intensive margin of reallocation across firms (no effect).

Overall, expanding the duration of paid maternity leave through the MBA led to 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 employment, indicating that the average firm shrank in response to higher costs. While the magnitude of the employment decline is large, Section 3.6 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 workers and new workers, separately by gender. Wage data come from social security records and only cover blue-collar workers.

Figure 9 and Table 1 report results. The MBA had no impact on the average wages of women:

Both specifications imply diminishing marginal utility in the weeks of maternity leave.

incumbent or new, young or old. All point estimates are small and precise and we 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 9, 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.6 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.[20]

The incidence of paid maternity leave in India on employment rather than wages contrasts with the simple economics of mandates, which predicts that mandating a valuable amenity should lead wages to decline and employment to decline only if the left shift of labor demand outweighs the right shift of labor supply. Consistent with this framework, Gruber (1994) finds that U.S. employers passed the full cost of mandated coverage of maternity expenses in health insurance onto women's wages. Gruber and Krueger (1991) similarly show that U.S. employers shifted the cost of mandated workers' compensation insurance onto wages.

Two possible explanations drive the absence of wage declines in India. The first is that women did not value the benefit and were already paid their reservation wage, such that labor supply was flat. A left shift of labor demand will then leave wages unchanged. Several pieces of evidence point against women not valuing leave: we find nearly universal take-up of longer maternity leave in the post-period (3 Panel C); incentivized willingness to pay valuations also reveal that female workers highly valued the expansion of maternity leave, worth between 20-30% of their monthly wage (see Section 4). The pure monetary benefit of 14 additional weeks of pay corresponds with a 10% monthly wage increase, and we can rule out wage declines of much smaller magnitude (the predicted tenure of young women in social security data is 3.5 years). [21]. Instead, the incidence of this mandate fell on employment rather than wages because wages were rigid, potentially due to equity considerations, nominal wage rigidities, or binding minimum wages. Binding minimum wages drove wage rigidity for 17% of establishments, which paid the minimum wage both before and after the mandate. Factories in India also regularly post wages outside their premises, making

[20]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.

[21]Given young 18-28-year-old women's predicted tenure of 3.5 years (from observed quit rates in social security data), a 30% valuation of leave implies that it would take roughly one year to offset the salary benefit of 14 weeks of leave, with non-monetary benefits such as recovery from childbirth, additional time with children, and a potentially smoother return to work adding 7.2 months of pay beyond the monetary benefit. By way of benchmark, Indian women valued 14 additional weeks of paid maternity leave roughly as much as 20 days of paid time off in the U.S. (23% in Maestas et al. 2023) and women's willingness to pay for flexible work arrangements that grant them control over their work schedules in Colombia (26% in Bustelo et al. 2023)

wage discrimination by gender difficult (Sharma 2025, Appendix Figure A1).

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.

A simple conceptual framework identifies industries with larger expected declines in employment. Consider a Cobb-Douglas value-added revenue function in capital and labor with labor a CES aggregation of male and female workers: $Y = ZK^{\alpha_1}L^{\alpha_2}$ and $L = (\beta_k l_f^{\rho} + l_m^{\rho})^{1/\rho}$, with elasticity of substitution between male and female workers σ , $\rho = \frac{\sigma-1}{\sigma}$, and β_k denoting women's relative productivity in industry k. 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 hiring a female worker given the mandate, w_i is the female wage, and f_i is employment. The monopsony first order condition is: $\left(\frac{e_i}{1+e_i}\right)(mrpl_{fi} - c_i) = w_i$. To uncover the predicted change in employment, take the total derivative of the first order condition with respect to the benefit change, assume a constant elasticity of labor supply e_i around the policy change, and re-arrange[22]:

$$dlnl_{fi} = \frac{dc_i + dw_i}{mrpl_i * \frac{\partial lnmrpl_{fi}}{\partial lnf_i}} \tag{3}$$

The denominator reflects diminishing marginal revenue product of labor, $\frac{\partial lnmrpl_{fi}}{\partial lnf_i} < 0$. Declines in female employment increase with the change in replacement costs (dc_i), with larger declines when $\frac{\partial lnmrpl_{fi}}{\partial lnf_i}$ is smaller in magnitude, indicating greater substitutability between women and men. Intuitively, the marginal female worker affects output less when more easily substitutable in production with men. Appendix A shows the magnitude $\left|\frac{\partial lnmrpl_{fi}}{\partial lnf_i}\right|$ increases with the CES female share of an industry, which itself increases with women's relative productivity β_k and lower substitutability between men and women (σ). In sum, larger declines in female employment are expected in industries where women are less productive and more easily substitutable in production, reflected in smaller female shares.

Table 2 heterogeneity in employment effects by the baseline female share of an industry, divided into three bins, low (below the 25th percentile), medium (between the 25th and 75th per-

[22] 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.

centiles), and high (above the 75th percentile). Industries with low- and medium- female shares exhibit greater 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 high-female-share industries includes education and textile manufacturing. On average, the female share of workers in low-female-share industries is 8% , medium-female-share is 15% and high-female-share is 37%.

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 of all 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 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 4 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. 2023a, Adhvaryu et al. 2023b).

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 described next.

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 et al. 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 et al. (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. The analysis below assesses 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[ⓡ] et al. 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's in Economics from Delhi 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.[23] Because our analysis consists of those who were already in their undergraduate degree when the reform is passed, and there is limited scope to change majors, our results do not reflect a change in selection of students into university-degree programs. To estimate the effects of the MBA on labor market entrants' trajectories, we estimate regressions of the following form:

$$y_{it} = \beta_0 + \sum_{\tau=1}^{24} \beta_\tau \mathbb{1}\{month = \tau\} \times treated_i \times female_i + female_i + \lambda_{uft} + \varepsilon_{it} \tag{4}$$

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

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

2017 or later and equal to zero if i graduated in 2015 or 2016. $month_t$ 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 students 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 seniority level. In the two years following graduation, we find that the reform increased the gender gap in job position seniority by approximately 12%. 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 the present results for labor market entrants report the MBA's effect on promotion into senior positions in the first two years of employment, this outcome does not fully capture the richness of the LinkedIn data. Ongoing analysis examines a wider range of outcomes for entrants, 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. Parallel work by Conlon and Sharma (2024) shows that this response is at least partly driven by employer misperceptions about the rate of maternity-leave-taking among women: in particular, to rationalize the magnitude of the estimated employment decline, employers would have to believe that 27% of women take 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 2017 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% (Appendix Figure A.9). If

employer misperceptions drive discrimination, this presents the tantalizing possibility that reducing misperceptions can 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 Conlon and Sharma (2024) 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%, closely matching the 23% perceived rate of leave-taking elicited in an employer survey of HR managers (Appendix Figure A.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 3.1 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 Cost-benefit 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 offers a valuable opportunity to develop a broader 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 loss. This section serves three goals. First, we conduct a cost-benefit analysis to quantify how the benefit to mothers and potential reduction in adverse selection at firms that already offered long leaves before the reform compare against its costs. Second, firms may vary in their cost of leave provision such that providing leave is efficient for some but not for all firms. We recover the cost distribution across firms to identify which firms can increase leave at net benefit. Finally, we quantify how several alternative policies—smaller leave expansions, only a subset of low-cost firms providing expanded leave, and maternity leave insurance to offset firm losses—affect both the benefits to mothers and costs of job loss. All results pertain to blue-collar workers.

Costs and benefits The mandate may raise net-benefit by benefiting mothers and potentially curbing adverse selection at firms that already provided long 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 leave 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 net benefits requires estimating (i) the value of the benefit to workers, (ii) the distribution of costs across firms, (iii) the adverse selection penalty, and (iv) loss in profits net of labor costs. Consider a sum over firms. We assume equal weights on each individual and employer such that \$1 in benefits to workers offsets a \$1 increase in costs to firms. Net benefit before the mandate is:

$$W_{old} = \sum_{i \in firms} D_i E_i (V_{26} - c_{i,26} - \delta_i) + (1 - D_i) E_i (V_{12} - c_{i,12}) + \pi_i \tag{5}$$

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 beyond wages of hiring a female worker in a 26-week regime where everyone provides 26 weeks of leave (i.e., without adverse selection). δ_i is an adverse selection penalty facing firm i in the pre-period and denotes the extra cost of hiring a female worker with greater likelihood of becoming pregnant if i is one of the only few firms offering 26 weeks of maternity leave in the pre-period while others offered 12 weeks. While we describe δ_i as an adverse selection penalty, it could also reflect advantageous selection, such as if more productive or career-oriented women disproportionately sort into firms offering generous maternity leave. Our empirical analysis remains agnostic regarding the adverse or advantageous nature of δ_i and instead simply estimates it. $c_{i,12}$ is the cost of providing 12 weeks of leave when most other firms offer 12 weeks. Note that $c_{i,12}$, $c_{i,26}$, and δ_i denote the costs of employing the *average* female worker at firm i not just women who become mothers. $c_{i,12}$ then includes the probability of taking maternity leave times the firm's cost of covering leave in the 12-week regime, plus the probability of a permanent quit times the replacement cost of the worker. π_i denotes profits net of labor costs.

The expression for W_{old} excludes both costs incurred by the government such as salary payments during leave, as these represent transfers from taxpayers to workers, and any change to consumer surplus from shrinking firms. We abstract away from the distributional and efficiency consequences of taxation by assuming that (i) the government finances its benefit through a lump-sum tax, (ii) utility is quasi-linear in money, and (iii) equal welfare weights on all individuals such that redistributing \$100 from 100 individuals to 1 single mother does not affect net benefits. Losses in consumer surplus due to shrinking firms are likely minimal given the only 4.5% shrinkage among the most impacted firms. These abstractions help focus 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.[24] Net benefits in the post-period are:

$$W_{new} = \sum_{firms} E_{i,new}(V_{26} - c_{i,26}) \tag{6}$$

The change has four components:

$$\Delta W = W_{new} - W_{old} = \underbrace{\sum_{firms} (1 - D_i) E_{i,new}((V_{26} - V_{12}) - (c_{i,26} - c_{i,12}))}_{\text{Expanded coverage}} + \underbrace{\sum D_i \delta_i}_{\text{Eliminate adverse selection}} + \underbrace{\sum (1 - D_i)(E_{i,new} - E_i)(V_{12} - c_i)}_{\text{Job loss}} + \underbrace{\sum \Delta \pi_i}_{Profits} \tag{7}$$

Note that δ_i does not disappear from ΔW even if adversely selected women reallocate across firms to spread costs. This is because $c_{i,26}$ denotes 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 $c_{i,26} + \delta_i$, 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 that was eliminated by the policy). 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 for different age groups come from an incentivized survey where workers reveal the wage increase they would require to switch to shorter 12-week-long leaves.

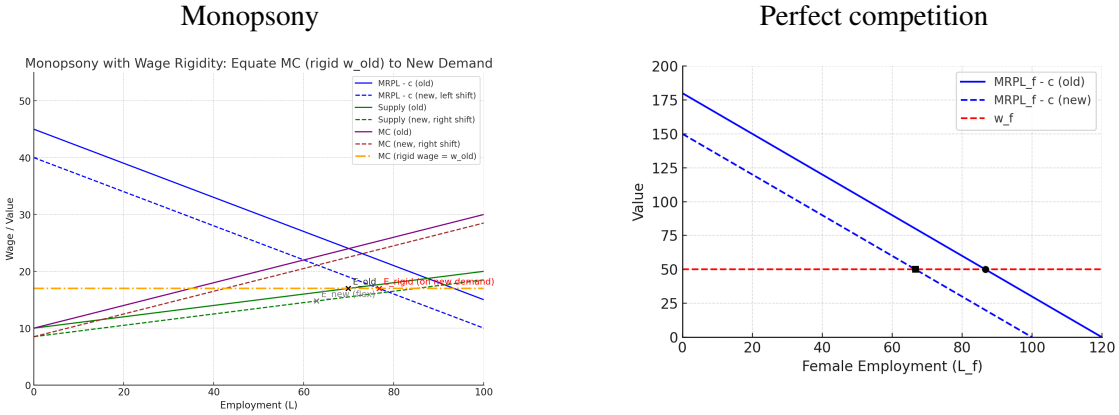
What happened A key question is why the maternity leave mandate reduced employment rather than wages. One possibility is that workers did not value the benefit, which prevented wages from declining. A second possibility is that wages were rigid, leading firms to adjust employment rather than wages. Below we show that workers valued the benefit—this is also revealed by the nearly universal (96%) 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. Binding minimum wages drove wage rigidity for 17% of establishments, which paid the minimum wage both before and

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

after the mandate. Factories in India regularly post wages outside their premises, making wage discrimination by gender difficult.

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 in India (e.g., Amodio et al. 2025, Felix 2021, Morchio and Moser 2024, Sharma 2023, Sharma 2025). The reform shifted labor demand to the left and labor supply to the right, but rigid wages led employment to decline (Panel A). Panel B demonstrates the same intuition under perfect competition. Demand again shifts left, and market-level supply shifts right (because women value longer leave). However, rigid wages lead individual employers to face the same marginal cost (flat wage), and the leftward shift of demand leads employment to decline. Monopsony and perfect competition thus yield identical qualitative conclusions about the impact of the MBA: demand fell, wages did not adjust, and employment dropped. The distinction between the two only influences the magnitude of cost changes recovered from the observed decline in employment (recovered below), not the basic interpretation of the reform's impact.

Figure 1: Effects under monopsony and perfect competition



Notes: The reform led demand to shift left (since female workers became more expensive); supply shifted right as workers valued the benefit. Wages were rigid and female employment declined.

Measuring adverse selection δ_i is the extra cost of hiring a female worker if i is one of the few firms offering 26 weeks of leave in the pre-period while others offered 12 weeks. As previously noted δ_i may be positive (a cost) if providing longer leaves adversely selects in women with greater proclivity to take it or negative (a benefit) if longer leaves attract more productive workers. We measure δ_i as, when adverse selection disappears, (i) $\Delta\bar{quit}_i$, the change in the quit rate of the average woman, (ii) $\Delta\bar{(leave + return)}_i$, the change in leave-taking of the average woman, and (iii) Δ worker quality of new hires. We estimate δ_i using two methods with identical results. First,

a simple event study of control establishments with establishment fixed effects. Second, a DiD design comparing control firms in markets where many firms already offered longer leaves (over 25% of employment) to control firms in markets where other firms offered 12 weeks, implying 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-city pair. The identifying assumption is that the cost of hiring female workers did not systematically change at control firms in markets where other firms also inhabited the control group.

Figure 15 shows the MBA had no effect on quit rates or the female share of the workforce at control firms (Panels A and C). However, women at control firms were less likely to take maternity leave, declining from an average of 2% of women on leave in a month at baseline to 1%. Control firms employed 648,900 women at baseline. The treatment effect on leave-taking implies savings of 6489 leave costs per month (quantified below). The provision of generous leaves may also change worker composition by attracting more productive female workers. However, we detect no change in standard measures of new worker productivity, including whether workers are poached, prior wage, and experience.

Measuring ($V_{26} - V_{12}$) We quantify women's valuation of leave through two surveys: a survey of 412 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 3 Panel C). 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 Rs.0 to Rs.20000 per month in increments of Rs.500 (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 or roughly 30% of the monthly salary. The incentivized response distribution looks similar to the unincentivized responses (see Appendix Figure A.10; $p = 0.20$ for the difference in means). In addition, several studies find that elicited valuations for hypothetical amenities closely resemble incentivized valuations for real jobs (e.g., Mas and Pallais 2017, He et al. 2021).

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 compen-

sate for the monetary cost of leave, with additional benefits like time to recover 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. 2018). 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.

By way of benchmark, elicited valuations for 14 extra weeks of paid maternity leave (32.8%) resemble the value of 20 days of paid time off in the U.S. (23% in Maestas et al. 2023) and women's willingness to pay for flexible work arrangements that grant them control over their schedules in Colombia (26% in Bustelo et al. 2023).

Cost distribution (c_i) The distribution of treatment effects on employment across firms reveals 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[25]:

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

Rearranging the expression shows that higher mandate costs lead to larger declines in employment (substituting in $dw_i = 0$ and $dlnl_{mi} = 0$):

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

The denominator ($\frac{\partial lnmrpl_{fi}}{\partial lnf_i} < 0$) captures diminishing marginal revenue product of labor. Employment declines as costs rise, with greater declines when $\frac{\partial lnmrpl_{fi}}{\partial lnf_i}$ is smaller in magnitude signifying greater substitutability between women and other factors of production. Intuitively, the marginal female worker affects output less when more easily substitutable in production with men.[26] Together, the magnitude of the employment response and the slope of the marginal revenue product curve reveal costs:

$$\frac{dc_i}{db} = \frac{dw_i}{db}\frac{1}{\mu_i} + \frac{\partial lnmrpl_{fi}}{\partial lnl_{fi}}\frac{dlnl_{fi}}{db} mrpl_{fi} + \frac{\partial lnmrpl_{mi}}{\partial lnl_{mi}}\frac{dlnl_{mi}}{db} mrpl_{fi} \tag{8}$$

Measuring wage and employment changes $(dw_i, dlnf_i, dlnm_i)$ and imposing a production function to estimate $\frac{\partial lnmrpl_{fi}}{\partial lnf_i}$ 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 effects across industry. We impose monopsony first-order condition in the pre-period and common elasticity of labor supply, $e_i = 2.3$, implying the average woman takes home 70% of her marginal revenue product (Sharma 2023). The monopsony FOC allows recovering $mrpl_{fi}$ as a function of the pre-period wage alone given markdowns.

[25] 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.

[26] For example, in a production function where labor is a CES aggregation of male and female workers $l_i = [\alpha f_i^{\sigma} + m_i^{\rho}]^{\frac{1}{\rho}}$, the magnitude of $\partial lnmrpl_{fi}/\partial lnf_i$ declines with the CES-female share of an industry. Low CES shares correspond to women's lower relative productivity in an industry, embellished by a higher elasticity of substitution between male and female workers.

We assume that estimated costs equal actual costs without accounting for potential employer misperceptions. Employers systematically overestimating replacement costs would lead to overestimating cost increases, since employment changes reflect misperceptions rather than costs, and, consequently, underestimating the welfare impact of the maternity leave policy. Nonetheless 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% worth 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 $\sum(E_{i,new} - E_i)/N$. Declines in profits net of labor costs are bounded by the fact that the most impacted firms only shrank by 4.5%.

Results Table 5 reports the impact of the maternity leave law on worker and employer surplus respectively. Given the composition of female workers with different leave valuations, women's welfare increased by approximately 30% of the baseline wage bill. Given the composition of firms with different baseline female shares, employer costs increased by 25.5%. Benefits equal costs if workers overstate their willingness to pay for maternity leave by 15%. We find small savings from eliminating adverse selection at employers that already offered long leaves prior to the reform (0.1% of the baseline wage bill). Job loss reduced benefits by 2% of the wage bill. Estimates imply that the reform benefited employed women by approximately 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 labor supply elasticities with respect to leave, 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 major policy change in India that expanded paid maternity leave from 12 to 26 weeks. While longer leaves benefited new mothers, who enjoyed full pay during leave and extended job protection, they also generated unintended consequences for younger women by reducing their employment and (to a lesser degree) slowing their career growth. 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 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 as female workers became more expensive. Finally, men were more likely to be promoted into 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. A cost-benefit analysis shows that the reform benefited employed women who valued longer leaves 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 the extent to which different policy designs—such as shorter leave expansions, restricting coverage to low-cost firms, or maternity leave insurance to offset firm losses—can preserve some benefits of maternity leave policies while limiting costs and job loss remains a promising avenue for exploration tackled in the next version of this paper.

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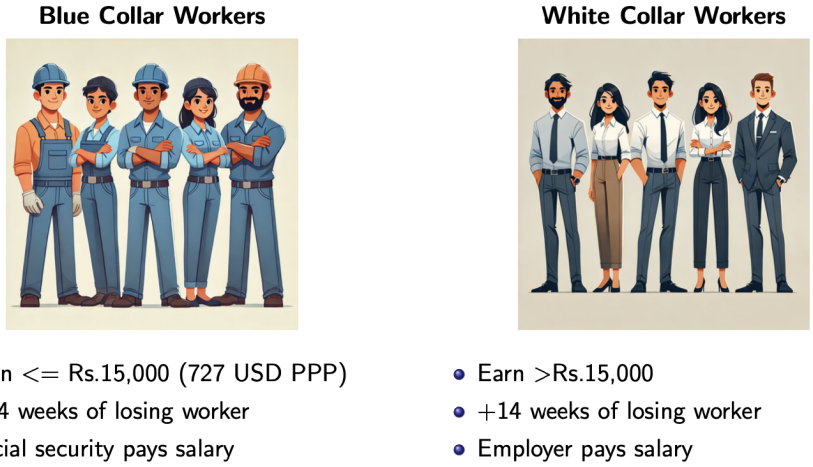
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Figures

Figure 2: 2017 Maternity Benefits Amendment Act (MBA) of India

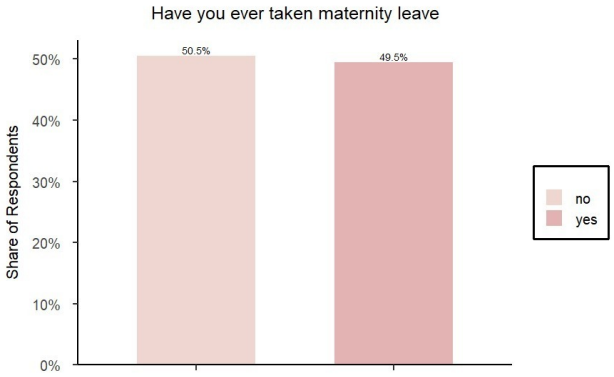
Panel A: Reform provisions differ by wage threshold



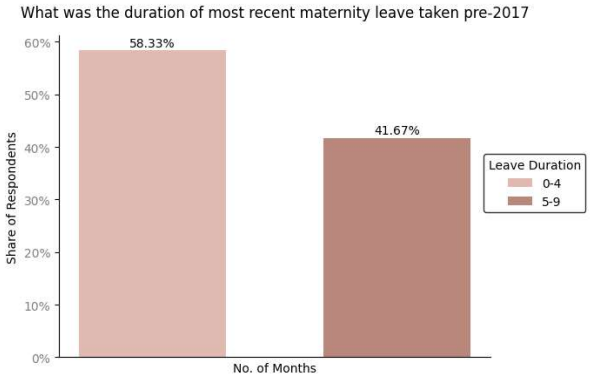
Notes: The figure reveals how the reform differently changed the cost of providing maternity leave for workers with monthly earnings below Rs.15,000 (blue collar workers) and above Rs.15,000 (white collar workers). Blue and white collar are approximate terms. Monthly earnings of Rs.15,000 correspond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

Figure 3: First stage effect on duration of leave offered (survey)

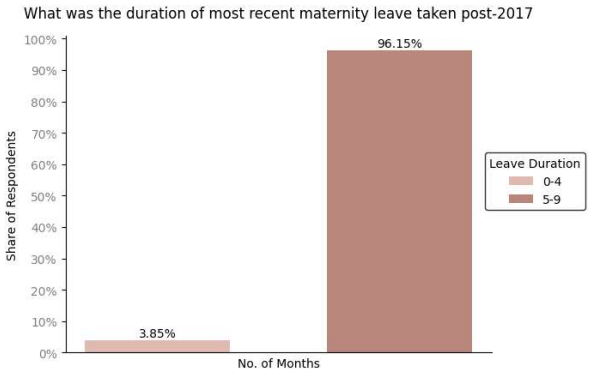
Panel A: Incidence of leave



Panel B: Pre-period duration of leave



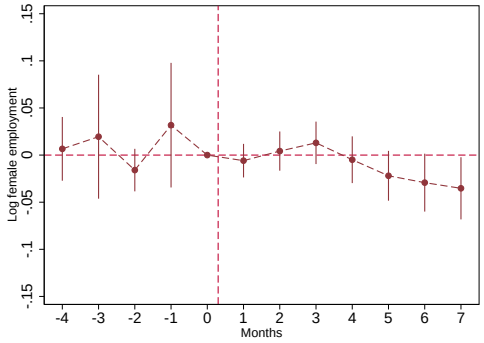
Panel C: Post-period duration of leave



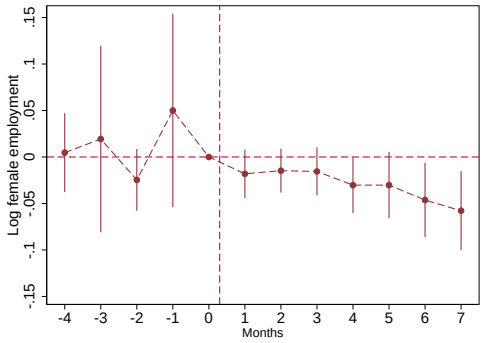
Notes: These figures report responses from a survey of female workers employed at a representative sample of 357 large firms across India (>200 employees), of which 49% of women have taken maternity leave in the last five years. The survey sample is described in Section 2

Figure 4: Effects on female employment

Panel A: All firms



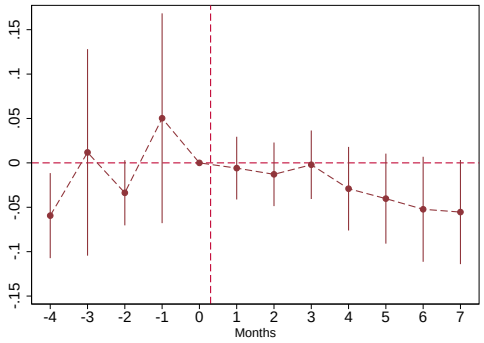
Panel B: Large firms (> 75 workers)



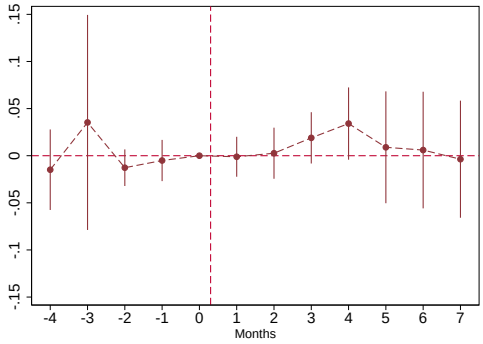
Notes: This figure reports the treatment effect on the log of female employment at all firms (Panel A), and large firms with over 75 workers (Panel B) using the regression specification in equation (1). Results use social security records for workers with monthly earnings below Rs.15,000 (blue collar workers). Regressions control for establishment fixed effects and state-establishment size bin-time fixed effects. Establishments 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 5: Effect on female employment – by age

Panel A: 18 - 35 years

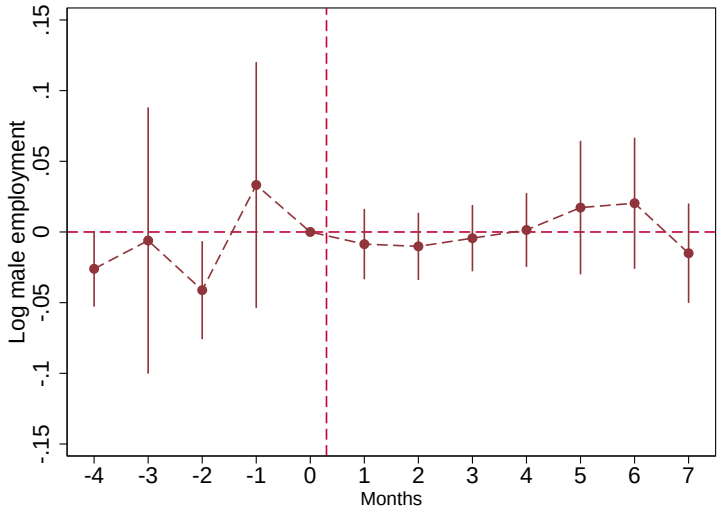


Panel B: Over 35 years



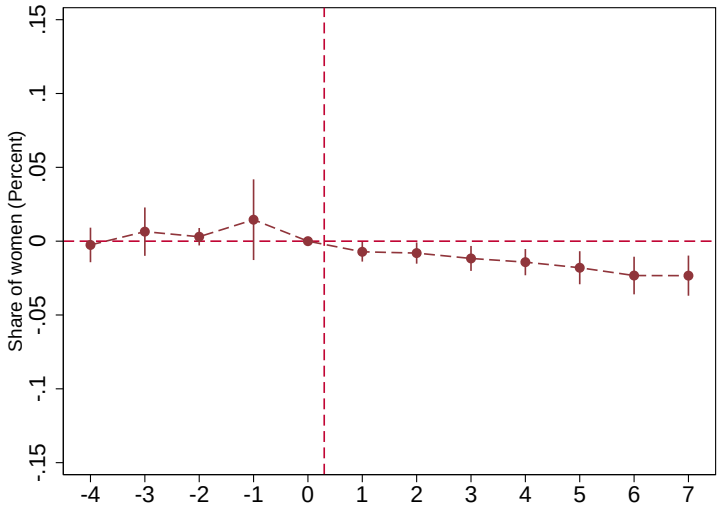
Notes: This figure reports the treatment effect on the log of female employment at firms with over 75 workers using the specification in (1). Panel A reports effects for young women between 18 - 35 years and Panel B for older women above 35 years. Regressions control for establishment fixed effects and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

Figure 6: 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

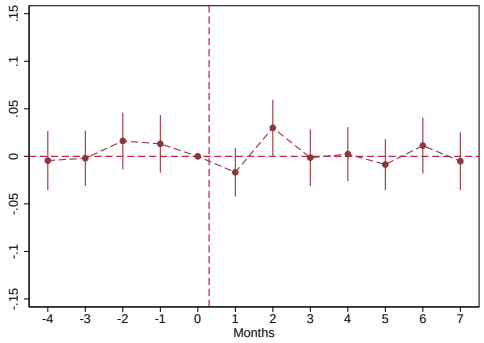
Figure 7: Share of women workers



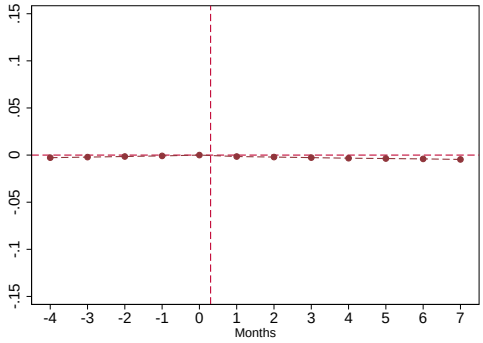
Notes: This figure plots the treatment effect on the share of female workers using DiD the specification in equation (1). The sample includes large establishments with over 75 workers but results are invariant to using all establishments instead. Standard errors are clustered by establishment.

Figure 8: Contract work

Panel A: Female share, contract firms



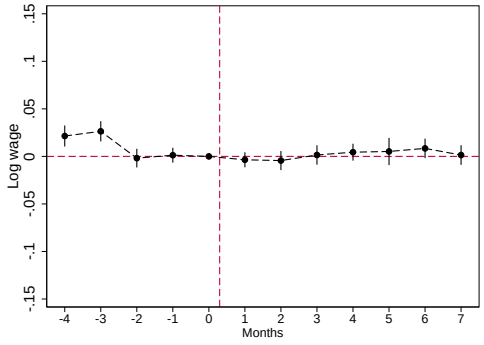
Panel B: Female incumbent, switch to contract firms



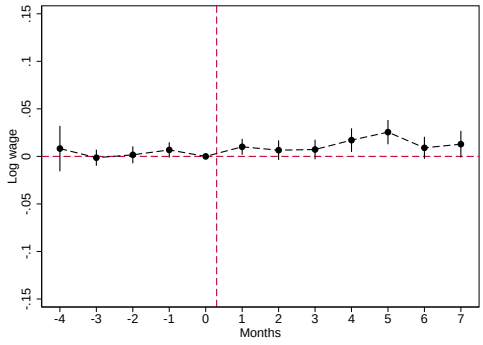
Notes: Panel A shows a simple event study of the female share at the top 30 contract firms in India, which employ nearly a third of contract workers. The regression compares establishments to themselves in $t = -1$ by including establishment fixed effects. The figure reports coefficients on time dummies. 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 9: Effect on wages

Panel A: Female incumbents

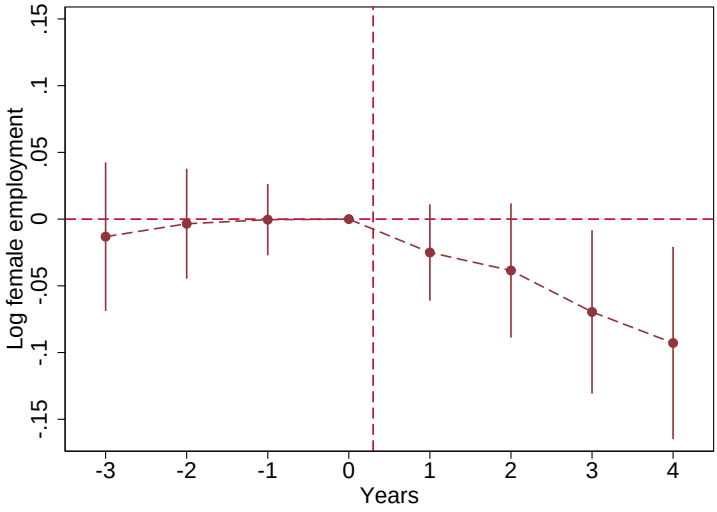


Panel B: Male incumbents



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).

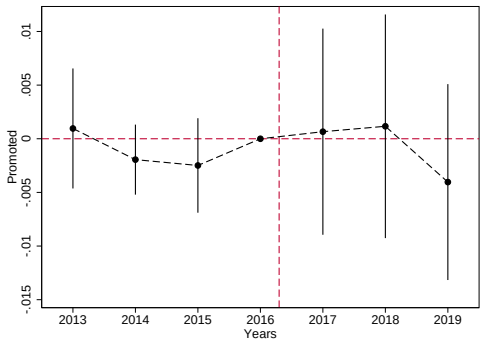
Figure 10: Effect on female employment – LinkedIn records (annual)



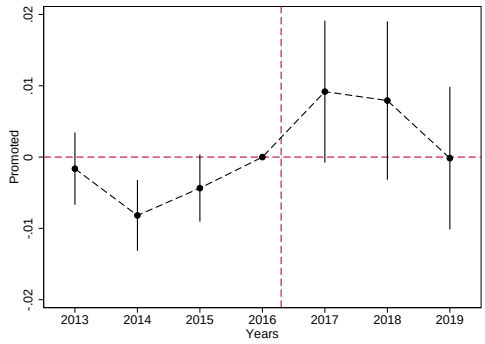
Notes: This figure plots the effect on log female employment using the DiD specification in equation (1). The results pertain to workers on LinkedIn. The sample includes establishments with over 75 workers. The data source is the universe of LinkedIn profiles rather than social security records (which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

Figure 11: Career trajectories: promotion into managerial positions

Panel A: Female incumbents



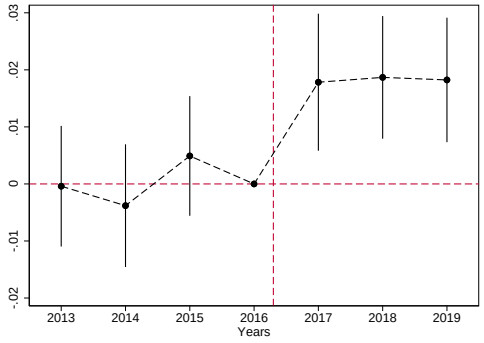
Panel B: Male incumbents



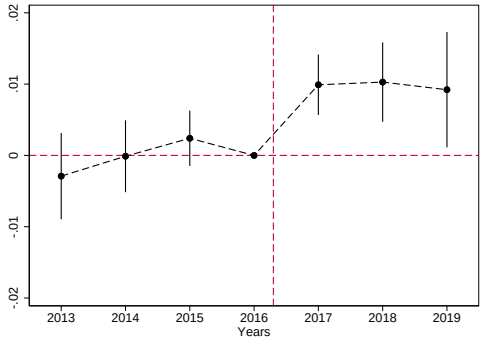
Notes: For incumbent workers, this figure reports the treatment effect on the likelihood of promotion into a managerial position using the specification in equation (2). The results pertain to white collar workers. The sample includes incumbents at establishments with over 75 workers. The data source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

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

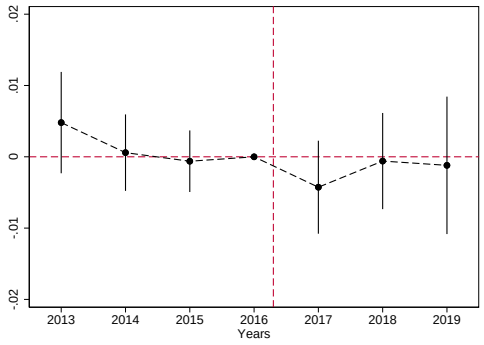
Panel A: Female incumbents: Abstract Role (women above 35 years)



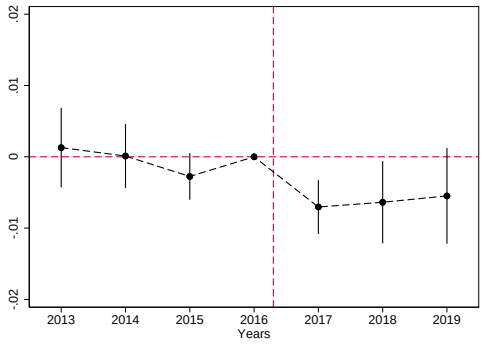
Panel B: Male incumbents: Abstract Role



Panel C: Female incumbents: Manual Role

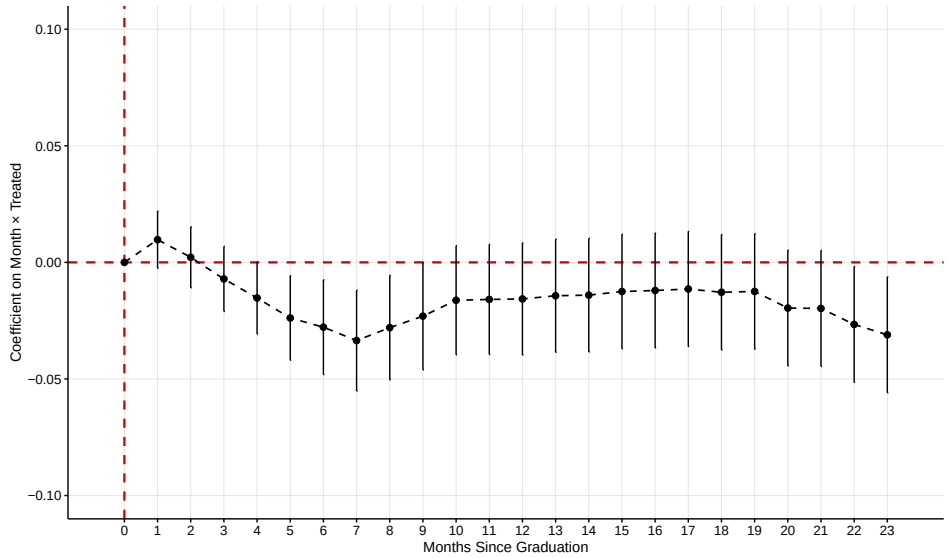


Panel D: Male incumbents: Manual Role



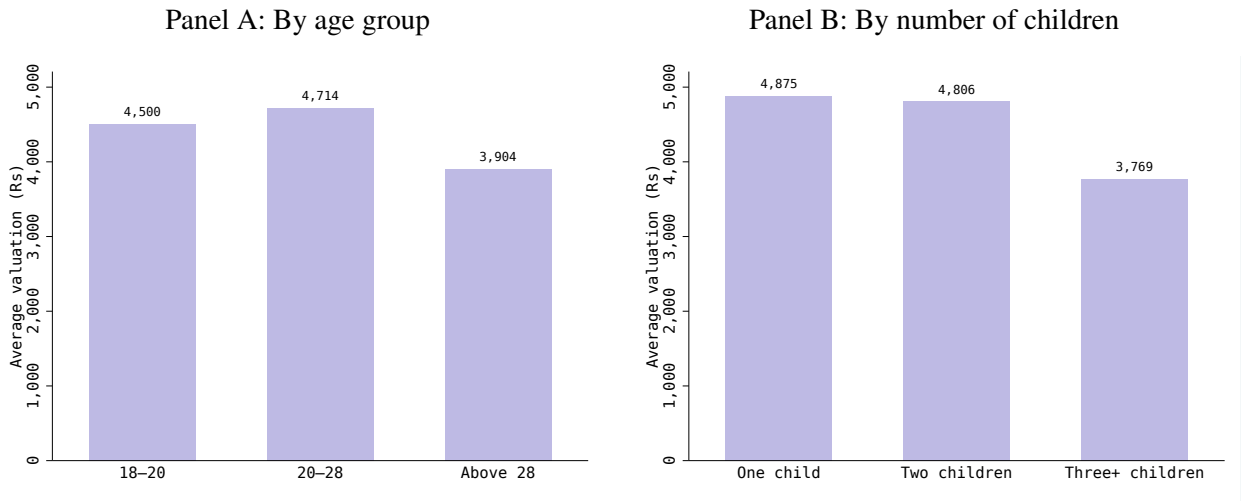
Notes: For incumbent workers, this figure reports the treatment effect on the likelihood of performing an "abstract" role (Panels (A) and (B)) and a "manual role" (Panels (C) and (D)) using the specification in equation (2). Panel A reports effects for incumbent women above 35 years of age at the time 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: Gender gap in seniority level



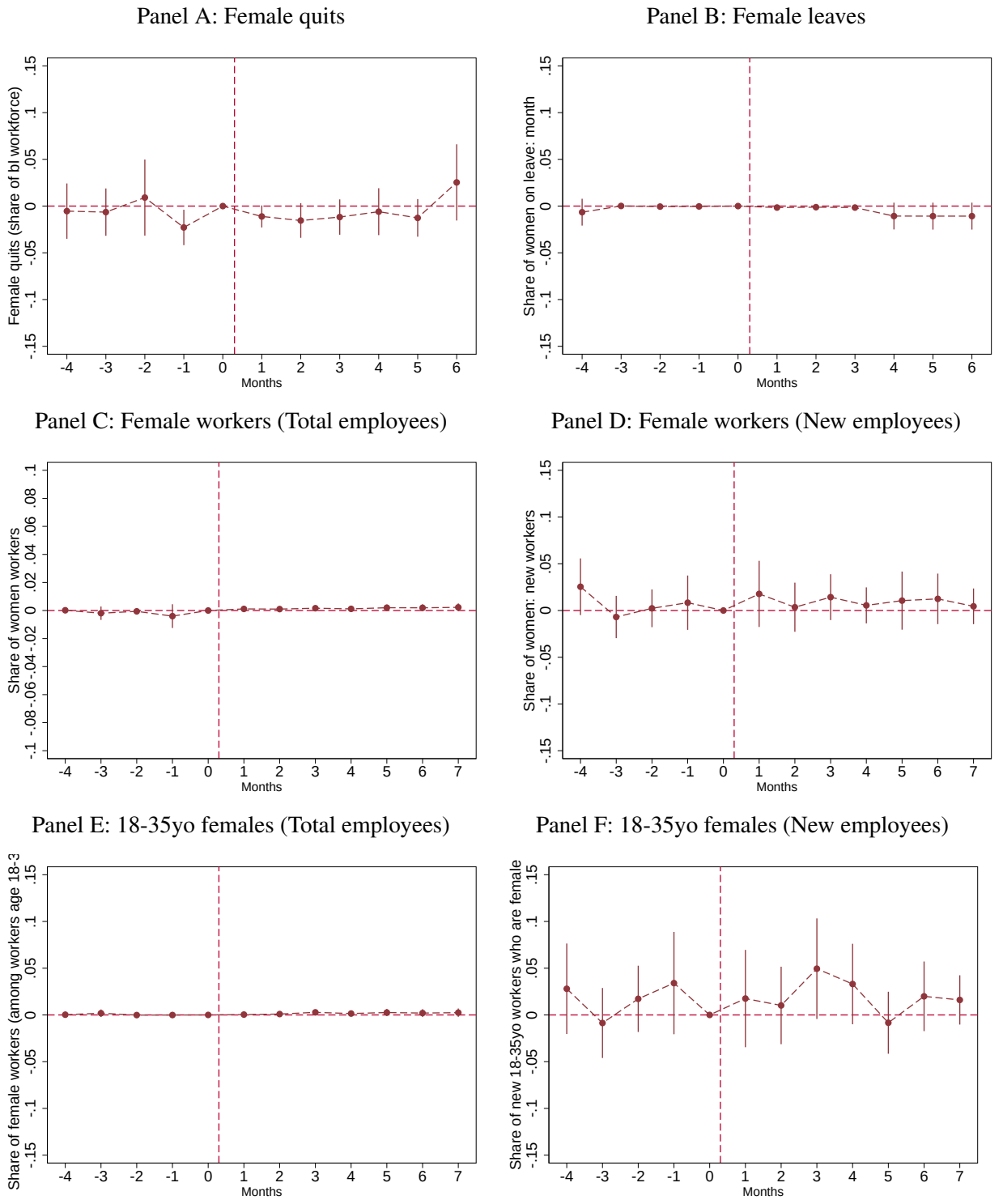
Notes: This figure reports the difference in the seniority level of men versus women during the first two years after graduation from a bachelor's degree program. We plot coefficients from equation (3), which compares labor market entrants entering the labor market in 2015-2016 (pre-reform) to graduates from the same university-degree program who graduated in 2017-2018 (post-reform). For example, we compare within groups such as those with a Bachelor's in Chemistry from the College of Engineering Pune or those with a Bachelor of Economics from Delhi 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: Estimated valuation of maternity leave from the survey of 412 factory workers in industrial areas near Delhi.

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



Notes: This figures plots quits and leave-taking among female employees, share of female employees, share of women among young employees (aged 18-35), share of female hires, and the share of women among young hires (aged 18-35) at control firms. Results use social security records for workers with monthly earnings below Rs. 15,000 (blue collar workers). Time progresses in months. Standard errors are clustered by establishment.

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

Notes: The table reports treatment effects on the log wage of new workers by gender. Regressions control for worker fixed effects and state-establishment size bin-time fixed effects. Establishments are divided into four equally-sized bins at baseline. Standard errors are clustered by establishment.

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 Table 2.

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 4.

Table 5: Costs and benefits

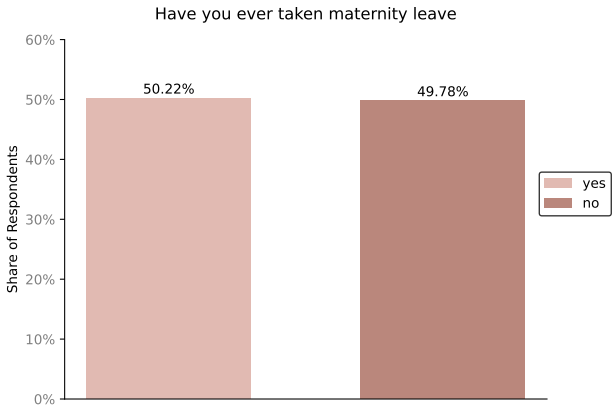
	Workers			Employers	
	Coverage	Job loss	Cost	Adverse selection eliminated	Profits
	(1)	(2)	(3)	(4)	(5)
Million worker salaries per month	2.610	-0.152	-2.196	0.013	NA
As share of wage bill	0.303	-0.018	-0.255	0.002	NA

Notes: Table reports estimates of the benefits and costs 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).

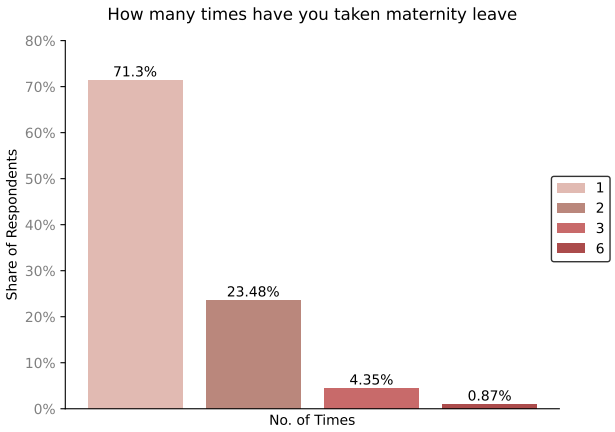
Appendix Figures

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

Panel A: Incidence of leave



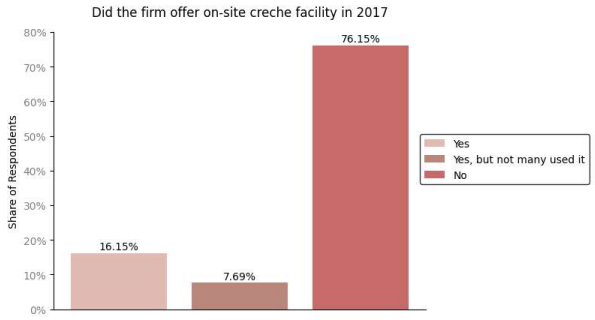
Panel B: Number of leaves



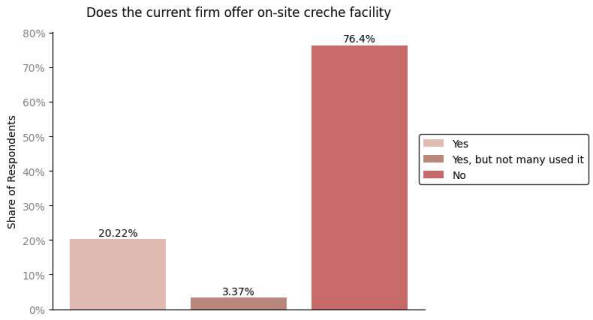
Notes: From the online survey of female workers at 357 firms, the left panel plots the share who took maternity leave in the past five years and right panel plots the number of leaves taken.

Figure A.2: Compliance with MBA 2017 Creche provision

Panel A: Creche: pre



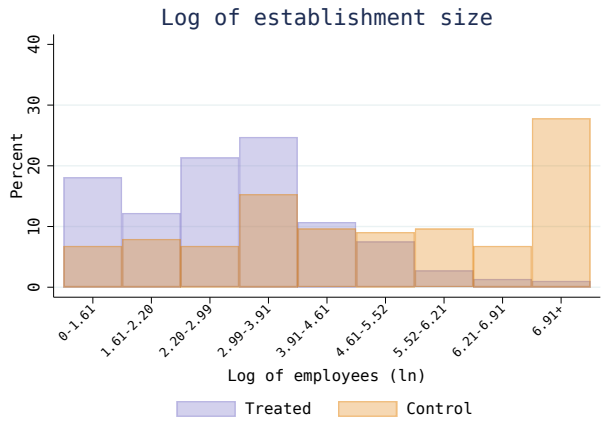
Panel B: Creche: post



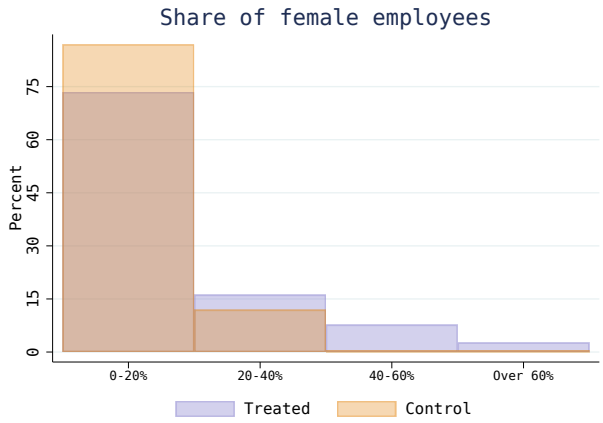
Notes: From the online survey of female workers at a representative sample of 357 large firms (>200 employees, intended N =500), the left panel plots the provision of creche facilities at a respondent's employer before the MBA was passed and right panel plots this provision after the reform's passage.

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

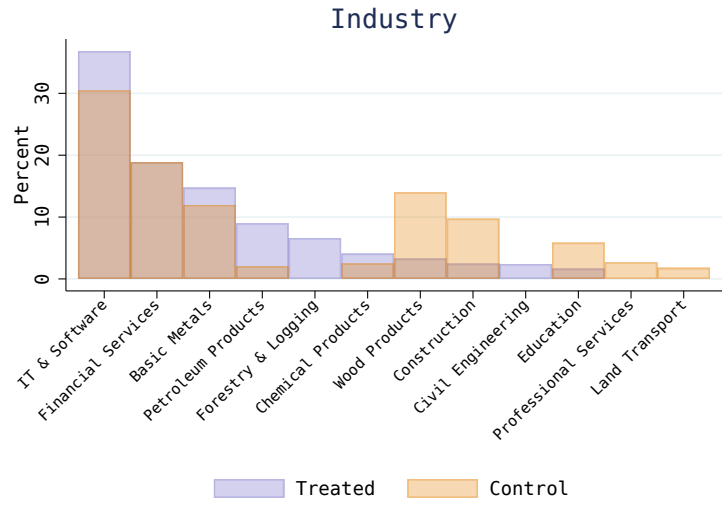
Panel A: Firm size (log)



Panel B: Female share

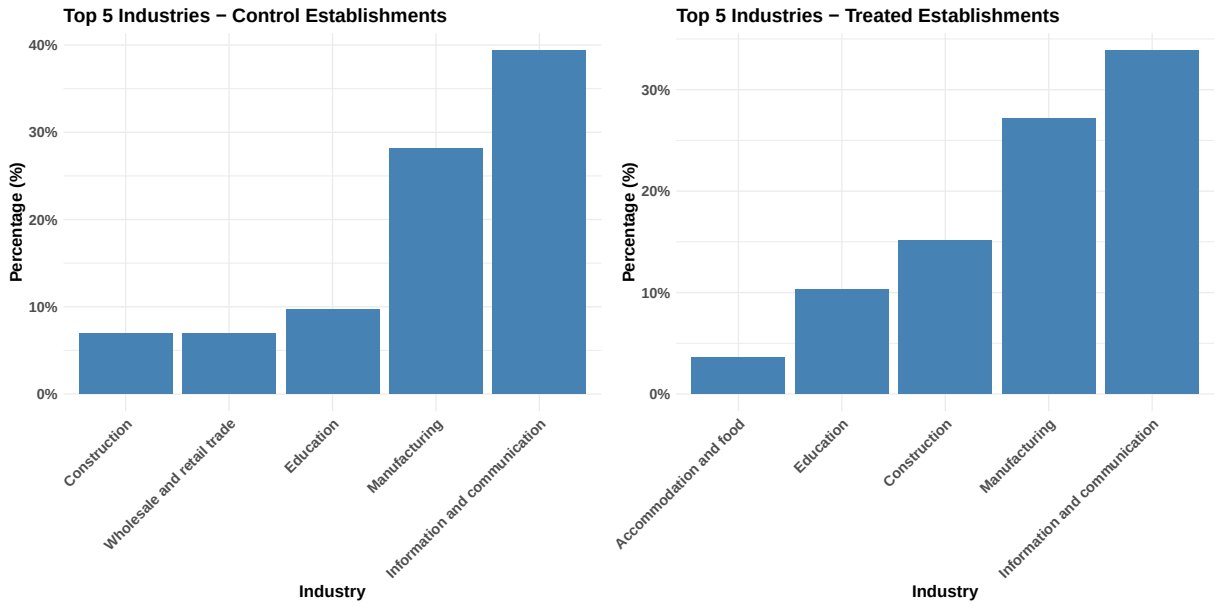


Panel C: Industry



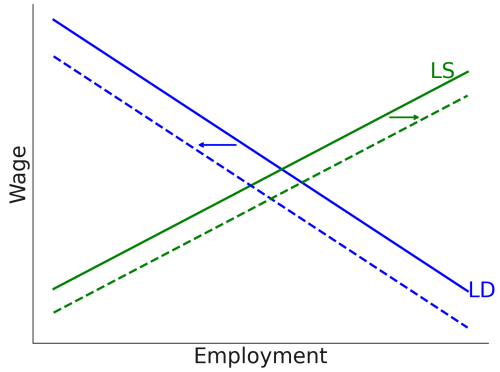
Notes: The sample includes treated and control establishments represented in the EPFO data at baseline (April 2017). Panel A plots the distribution of the log establishment size for treated and control establishments. Panel B plots the distribution of female employment share for the two sets of employers.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

Figure A.4: Establishment Summary Statistics: Industry



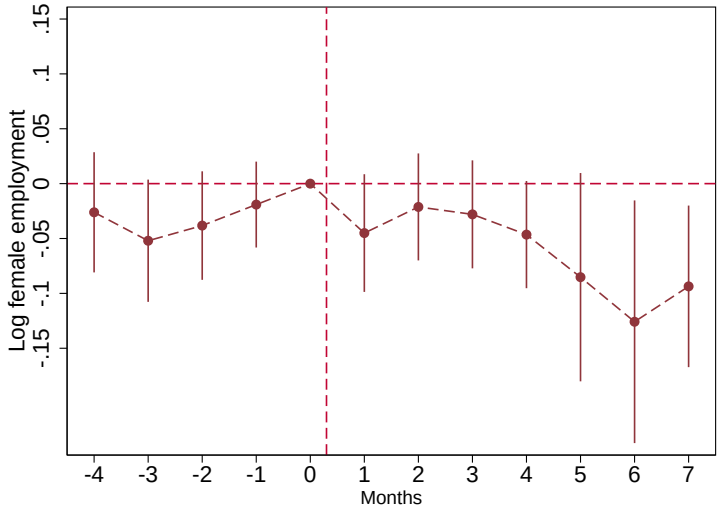
Notes: This figure plots the share of establishments in the top five most common industries by treatment status. The EPF dataset provides industry codes for every establishment but does not provide the mapping of the codes to industry names. To label the EPF-provided industry codes, we randomly selected forty firms from each of the 192 EPF-provided industry codes and classified those 7,680 firms according to their 2-digit NIC 2008 code. The corresponding 2-digit NIC codes for firms within each EPF-provided industry code sometimes vary, and so we select the modal 2-digit NIC 2008 code within each EPF-provided industry code to apply to each of the 192 EPF-provided industry codes. For this figure, we then aggregate those subindustries into the following broader categories: (1) Agriculture, forestry and fishing, (2) Mining and quarrying, (3) Manufacturing, (4) Repair and installation of equipment, (5) Electricity, gas, steam and air conditioning supply, (6) Water supply, (7) Construction, (8) Wholesale and retail trade, (9) Transportation and Storage, (10) Accommodation and food, (11) Information and communication, (12) Financial and insurance, (13) Real estate, (14) Professional, scientific and technical activities, (15) Rental and leasing activities, (16) Employment contractors, (17) Travel/tour operators, (18) Public Administration, (19) Education, (20) Human health and social work, (21) Arts, entertainment and recreation, (22) Other service activities, (23) Activities of households as employers, and (24) Activities of extraterritorial organizations and bodies.

Figure A.5: Predicted effect of a mandate



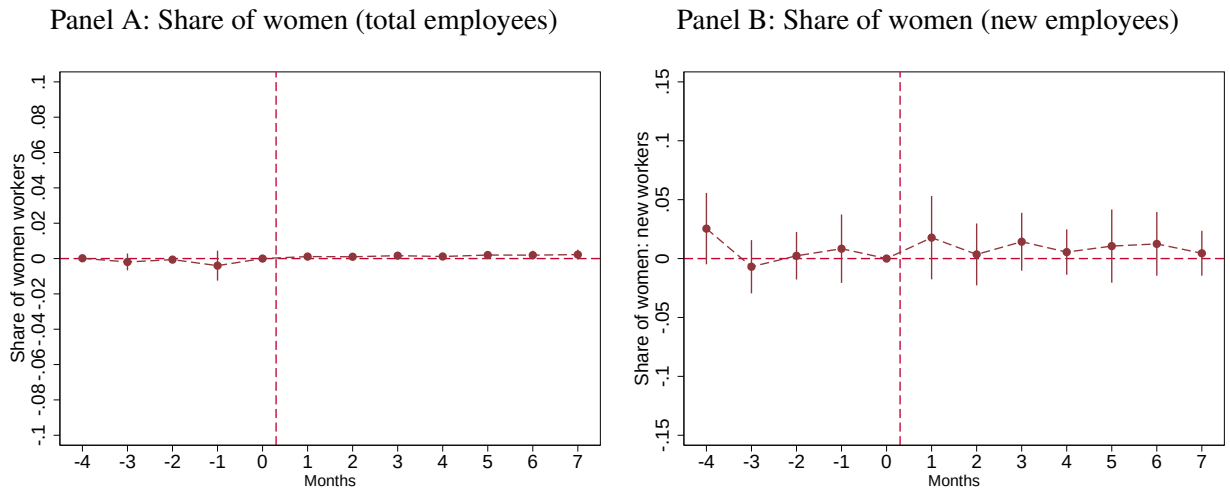
Notes: Figure depicts the predicted effects for the mandate of a valuable amenity. Labor supply shifts right as workers are willing to supply more labor at the same wage. Labor demand shifts left as workers grow expensive. Wages fall, and employment falls if the left shift of labor demand outweighs the right shift of labor supply.

Figure A.6: Effects on female employment (establishments with pre-period leave information)



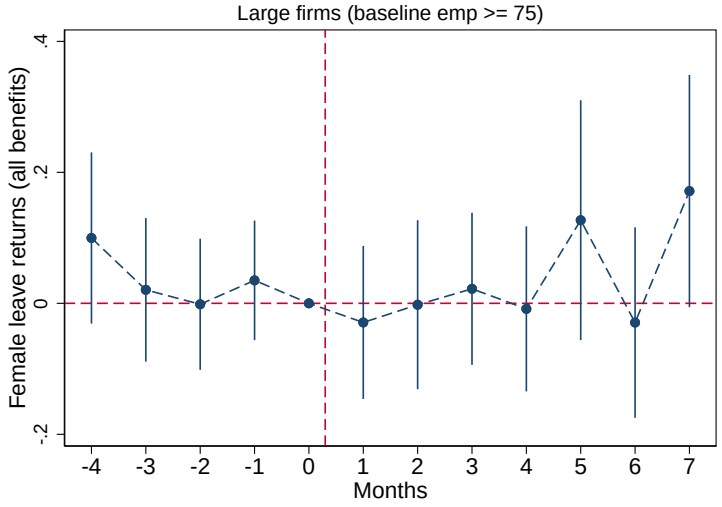
Notes: This figure plots the treatment effect on log female employment using the DiD specification in equation (1). The sample includes establishments with over 75 workers for which we have data on pre-period leave durations, not assuming firms with missing information offer the legal default. Standard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

Figure A.7: SUTVA test: share of women at control firms



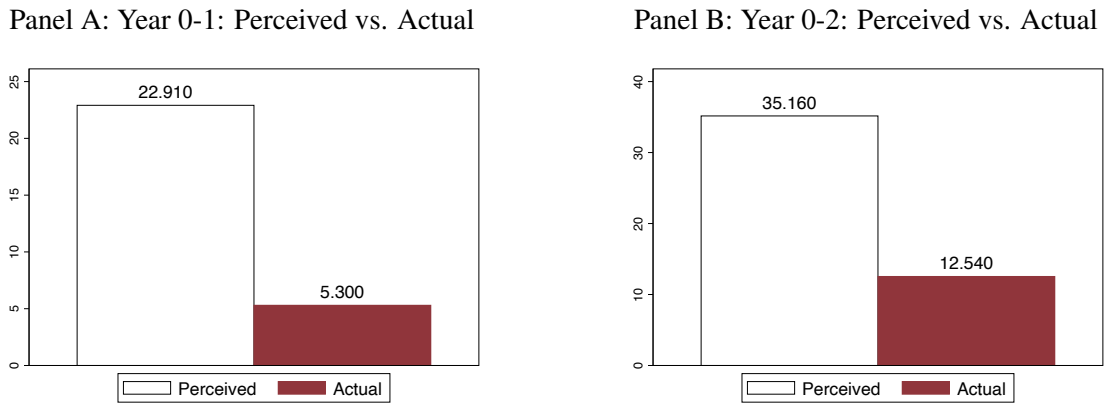
Notes: The sample includes control establishments represented in the EPFO data at baseline (April 2017). Panel A plots the share of female employees out of *total* employees at the establishment relative to the month before the reform. Panel B plots the share of female employees out of *new* employees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

Figure A.8: Effects on return-to-work after leave (number)



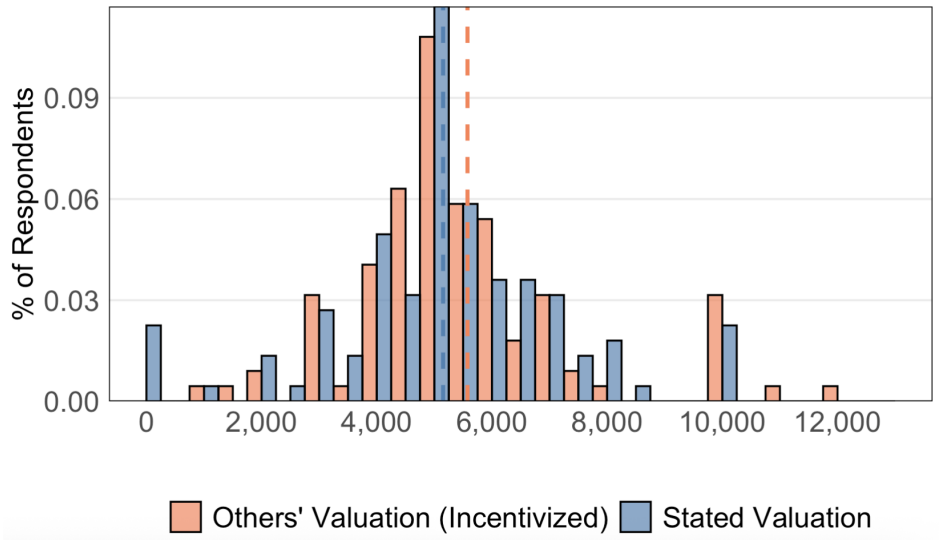
Notes: This figure plots the treatment effect on number of women who return from leave in a given month (where leave is defined as reduced employee contributions commensurate with maternity leave for 3+ months). The sample includes establishments with over 75 workers. Standard errors are clustered by establishment. The baseline number of women who began leave in the three pre-reform months is 0.96.

Figure A.9: Survey evidence of employer misperceptions



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 conducted by Conlon and Sharma 2024. 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 A.10: Comparing incentivized and unincentivized valuations of maternity leave



Notes: This figure compares women's (unincentivized) valuations of maternity leave with other women's (incentivized) guesses of how much other women value maternity leave ($N = 190$). The unit is additional Rupees per month of salary that women would need to be paid in order to be willing to accept only 12 weeks of maternity leave rather than the 26 weeks guaranteed by the MBA.

A Appendix: Predicted changes in female employment by industry

A simple conceptual framework identifies industries with larger expected declines in employment. Consider a Cobb-Douglas value-added revenue function in capital and labor with labor a CES aggregation of male and female workers: $Y = ZK^{\alpha_1}L^{\alpha_2}$ and $L = (\beta_k f^{\rho} + m^{\rho})^{1/\rho}$, with elasticity of substitution between male and female workers σ, $\rho = \frac{\sigma-1}{\sigma}$, and β_k denoting women's relative productivity in industry k. 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 hiring a female worker given the mandate, w_i is the female wage, and f_i is employment. The monopsony first order condition is: $\left(\frac{e_i}{1+e_i}\right)(mrpl_{fi} - c_i) = w_i$. To uncover the predicted change in employment, take the total derivative of the first order condition with respect to the benefit change, assume a constant elasticity of labor supply e_i around the policy change, and re-arrange:

$$dlnl_{fi} = \frac{dc_i + dw_i}{mrpl_i * \frac{\partial lnmrpl_{fi}}{\partial lnf_i}}$$

The denominator reflects diminishing marginal revenue product of labor, $\frac{\partial lnmrpl_{fi}}{\partial lnf_i} < 0$. Declines in female employment increase with the change in replacement costs (dc_i), with larger declines when $\frac{\partial lnmrpl_{fi}}{\partial lnf_i}$ is smaller in magnitude, indicating greater substitutability between women and men. Intuitively, the marginal female worker affects output less when more easily substitutable in production with men.

Derivative of marginal product across industries Given the revenue function, the marginal revenue product of labor for female workers is:

$$mrpl_{fi} = z_i K_i^{\alpha_1} \alpha_2 L_i^{\alpha_2 - 1} \frac{1}{\rho} [\beta_k f_i^{\rho} + m_i^{\rho}]^{\frac{1}{\rho}-1} \beta_k \rho f_i^{\rho-1}$$

Taking the log:

$$lnmrpl_{fi} = lnz_i + \alpha_1 lnK_i + ln(\beta_k \alpha_2) + (\alpha_2 - 1) lnL_i + \left(\frac{1}{\rho} - 1\right) ln[\beta_k f_i^{\rho} + m_i^{\rho}] + (\rho - 1) lnf_i$$

Taking the derivative with respect to lnf_i:

$$\frac{\partial lnmrpl_{fi}}{\partial lnf_i} = (\rho - 1) + (1 - \rho) s_{fi} + (\alpha_2 - 1) s_{fi} \tag{9}$$

This is because:

$$\begin{aligned}\frac{\partial ln(\beta f_i^{\rho} + m_i^{\rho})}{\partial ln f_i} &= \frac{f_i}{\beta f_i^{\rho} + m_i^{\rho}} \frac{\partial (\beta_k f_i^{\rho} + m_i^{\rho})}{\partial f_i} \\ &= \frac{\rho \beta_k f_i^{\rho}}{\beta f_i^{\rho} + m_i^{\rho}} \\ &= \rho s_{fi} \end{aligned} \tag{10}$$

where s_{fi} is the CES female share. Re-writing in terms of the elasticity of substitution between male and female workers, $\rho = \frac{\sigma - 1}{\sigma}$:

$$\frac{\partial lnmrpl_{fi}}{\partial ln f_i} = \underbrace{(\frac{\sigma - 1}{\sigma} - 1)}_{<0}(1 - s_{fi}) + \underbrace{(\alpha_2 - 1)}_{<0} s_{fi} \tag{11}$$

Equation 11 reveals, first, the presence of diminishing marginal revenue product of labor $\frac{\partial lnmrpl_{fi}}{\partial ln f_i} < 0$. Second, the magnitude $\left|\frac{\partial lnmrpl_{fi}}{\partial ln f_i}\right|$ increases with the CES female share of an industry for the range of σ (elasticities of substitution) and α_2 (labor share) in the literature. E.g., for any $\sigma > 1.7$ and $\alpha_2 = 0.4$ or $\sigma > 1.45$ and $\alpha_2 = 0.3$. Gallen (2018) estimates $\sigma = 5.94$. The CES female share itself increases with women's relative productivity β_k and lower substitutability between men and women (high σ). Small female share industries have lower productivity and higher substitutability. In sum, larger declines in female employment are expected in industries where women are less productive and more easily substitutable in production, reflected in smaller female shares.