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## Mandated Maternity Leave and Labour Substitution: Firm-Level Evidence from India

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# Mandated Maternity Leave and Labour Substitution: Firm-Level Evidence from India

## Abstract

Using nationally representative data from the Annual Survey of Industries, we examine the effects of India's Maternity Benefit (Amendment) Act, 2017, which extends the paid maternity leave from 12 weeks to 26 weeks on firms' hiring decisions and gender-specific employment outcomes at both the extensive and intensive margins, using a difference-in-differences framework. We use the institutional variation of the policy mandate, which applies only to firms with 10 or more employees, to define treatment and control firms. Our results reveal that firms respond to the mandate primarily through labour substitution, i.e., replacing female workers with male workers rather than bearing the increased cost of female employment. We find a decline of 8.4% in female workers and 9.5% in female mandays worked, accompanied by a symmetric increase of 2.6% in male workers and 2.1% in male mandays. Critically, this substitution is concentrated in industries with high female employment shares, while firms in low female-share industries show no female employment effects but exhibit positive spillovers to male wages. Firm output and profit are unaffected, indicating that male workers serve as effective production substitutes. These findings directly implicate demand-side statistical discrimination as the primary mechanism through which well-intentioned maternity leave policies can inadvertently reinforce gender labour market disparities.

## JEL classification

J13, J33, J31

## Keywords

maternity benefit, female labour, employment

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# 1 Introduction

How do firms adjust their employment practices when faced with gender-specific regulatory mandates? This question is central to understanding whether well-intentioned labour market policies achieve their intended goals or generate unintended consequences that undermine them. We study this question in the context of India’s Maternity Benefit (Amendment) Act 2017 (MBAA), which extended paid maternity leave from 12 to 26 weeks, with costs entirely borne by employers — creating a sharp, exogenous increase in the expected cost of employing women of childbearing age. Unlike OECD countries (with the exception of the U.S.), where maternity leave entitlements are entirely government-funded, India’s employer-liability financing model places the full cost burden on firms, creating strong incentives for firms to adjust their workforce composition in response. In recent years, there has been a noticeable shift away from such employer-liability systems towards arrangements funded through social insurance or a combination of public funds and employer liability, to lessen the burden falling solely on the employer (ILO, 2014). India, thus, provides a rare and policy-relevant setting in which to study how firms respond when the full cost of a gender-specific mandate falls on them alone. The ‘motherhood penalty’, which arises from the disproportionate burden of childcare borne by mothers, has been widely documented in both developed and developing economies (Kleven et al., 2019; Cortés and Pan, 2023; Angelov et al., 2016; Zhang et al., 2024; Casarico and Lattanzio, 2023; Heath, 2017; Koopmans et al., 2024; Aaronson et al., 2021). To mitigate these negative economic consequences and safeguard women’s and child’s health, there have been provisions made by the International Labour Organization (ILO), enabling women to balance both their reproductive and workplace responsibilities together. The ILO’s Maternity Protection Convention, 2000 (No. 183) requires all the member countries to provide a maternity leave of at least 14 weeks to women with a payment of at least two-third of their previous earnings, made through social insurance or public funds.<sup>1</sup> Later, it also suggested to increase the duration of leave to not less than 18 weeks.<sup>2</sup> Over time, majority of the countries have instituted maternity leave policies that meet or even exceed the ILO’s standard of maternity leave duration of 14 weeks. However, the effect of paid maternity leave on women’s labour market outcomes depends critically on the policy’s design and funding mechanism.

There exists a vast literature examining the effect of maternity or parental leave policies on women’s labour market outcomes in the developed countries, with a growing number of studies focusing on developing countries as well; however the evidence remains mixed (Vu and Glewwe, 2022; Ruhm, 1998; Uribe et al., 2019; Liu et al., 2024; Albagli and Rau, 2019; Amin and Islam, 2022). On the demand side, a mandated extension of paid maternity leave can negatively affect employers’ demand for female workers by increasing the cost of hiring them. These higher costs arise not only from wage compensation during leave periods, but also from the anticipated expenses of searching for, hiring, and training replacement workers in the worker’s absence (Liu et al., 2024; Rossin-Slater, 2017; Gruber,

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<sup>1</sup>See here [https://normlex.ilo.org/dyn/nrmlx\\_en/f?p=NORMLEXPUB:12100:0::NO::P12100\\_ILO\\_CODE:C183](https://normlex.ilo.org/dyn/nrmlx_en/f?p=NORMLEXPUB:12100:0::NO::P12100_ILO_CODE:C183)

<sup>2</sup>ILO suggested to increase the maternity leave period to at least 18 weeks in its Maternity Protection Convention (No. 191).

1994). Moreover, statistical discrimination<sup>3</sup> may further intensify these effects, leading employers to reduce female hiring, wages, and promotion prospects due to uncertainty surrounding women’s future productivity, working hours, and fertility decisions (Thomas, 2020). In contrast, employers may prefer to retain female workers with firm-specific human capital rather than bearing the costs associated with searching for and hiring equally productive replacements (Banerjee et al., 2026; Huebener et al., 2025). On the supply side, maternity leave policies may strengthen women’s attachment to their pre-birth employer by reducing the need to search for a new job after leave and by encouraging them to pursue higher-level positions through maintained career continuity (Baker and Milligan, 2008; Bennett et al., 2025). By lowering re-entry costs around childbirth, such policies enable women to accumulate greater work experience. Since current participation enhances future wages through experience accumulation, this mechanism can further increase women’s labour force participation in subsequent periods (Mukhopadhyay, 2012). It may also increase women’s labour supply by incentivising them to remain employed to qualify for availing the benefits when they have children (Ruhm, 1998). However, in the absence of complementary paternity leave policies, such provisions may also weaken women’s overall attachment to the labour market by reinforcing traditional gender roles and shifting a disproportionate share of unpaid care responsibilities onto women (Gupta, 2023). Moreover, longer leave durations may reduce the likelihood of returning to work due to depreciation of human capital during the leave period. Another possibility could be that spending more time with the child raises women’s desire to spend even more time, leading to a decline in her labour supply (Schönberg and Ludsteck, 2014). Maternity leave policies may further reduce women’s labour supply by positively influencing fertility decisions (Ruhm, 1998; Liu et al., 2024; Schönberg and Ludsteck, 2014; Aaronson et al., 2021).

Vu and Glewwe (2022) find that extending paid maternity leave by two months, financed through social insurance, has a positive effect on women’s formal employment in Vietnam. Ruhm (1998) observed an increase in the employment to population ratios of women of childbearing age in both short and long term leave in nine European countries, but a decline in wages when there is a lengthier leave period. The study attributed the positive effect to the tendency of women to increase their labour supply in order to qualify for availing the paid maternity leave and the decline in wages to result from the non-wage cost incurred by the firms like replacement of workers and the plausible loss of human capital as the leave lengthens. Additionally, Rossin-Slater (2017) also highlights that leave of less than one year positively affects women’s likelihood of employment while longer leaves can become detrimental for women’s employment and wages in the long run. Albagli and Rau (2019) confirmed a positive effect of an extension of the paid leave from 12 to 24 weeks on maternal employment in Chile as a result of women’s job continuity, induced by the incentive to continue accumulating firm-specific human capital and returning to pre-childbirth employer. In contrast, Uribe et al. (2019) report that a two-week extension of paid maternity leave in Colombia, where employers bear a significant share of the cost, led to higher informality, unemployment, and self-employment among women of childbearing

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<sup>3</sup>Statistical discrimination refers to a situation in which a decision-maker (such as an employer or lender) treats an individual based on the average characteristics of the group to which they belong rather than on their individual attributes.

age. [Liu et al. \(2024\)](#) also reported a reduction of 3.6% in the likelihood of women’s employment and a corresponding decline in wages, resulting from an extension in maternity leave benefit of 36 days in China. The study attributed the unintended effect of the policy to the greater dependence on the employers for the payment.

Several studies have examined the effect of maternity or parental leave policies on women’s labour market outcomes; however, less is known about how these policies affect firms’ outcomes and their hiring decisions ([Rossin-Slater, 2017](#); [Huebener et al., 2025](#); [Ginja et al., 2023](#); [Brenøe et al., 2024](#); [Bennett et al., 2025](#)). [Ginja et al. \(2023\)](#) analysed the effect of an unanticipated expansion in parental leave in Sweden on firms’ outcomes such as hiring and replacement costs, wage bills, and firms’ performance. The study finds an increase in the hiring of primary and temporary workers, an increase in the work hours of the co-workers, and the new hires, along with having a positive effect on firms’ total wage bills, suggesting that the adjustments are not perfectly frictionless. Further, they find that the adjustment costs differ across firms based on the availability of internal substitutes, without having any strong effects on firms’ sales and productivity. [Gallen \(2019\)](#) also reports a negative effect of an unanticipated extension of parental leave by 22 weeks on firm survival and retention of mothers in Denmark. In contrast, [Brenøe et al. \(2024\)](#) studied the effect of an anticipated parental leave taken by a female employee in a non-reform setting on small firms and co-workers in Denmark. The study finds that firms are able to compensate for the lost female labour supply by increasing hiring and work hours among substitute workers, without having any adverse effects on firms’ wage bills, output, and gross profit. [Huebener et al. \(2025\)](#) also find no long-term effect of an extended parental leave reform in Germany on firms’ employment, wage bills or likelihood to shut down.

India amended its Maternity Benefit Act in 2017, which increased the paid maternity leave from 12 weeks to 26 weeks for women working in firms employing 10 or more employees and the cost of providing this maternity benefit is completely borne by the employers. Using Consumer Pyramids Household Survey (CPHS), [Banerjee et al. \(2026\)](#) examine the effect of the extension on women’s employment and wages, comparing women in the high-fertility age group with women in the low-fertility age group and with men, and find a negative impact on women’s employment as well as wages. Building on [Banerjee et al. \(2026\)](#), we shift the unit of analysis from the individual to the firm, using the Annual Survey of Industries (ASI) to directly observe firms’ hiring decisions, workforce composition, and production outcomes — margins that are invisible in household survey data. Crucially, firm-level data allows us to detect whether female job losses are offset by male hiring gains, a substitution margin that individual-level data cannot identify. We show that they are: treated firms do not simply employ fewer women — they replace them with men, maintaining output and profit in the process. [Bose and Chatterjee \(2024\)](#) utilize the variation that the policy is applicable to only firms employing 10 or more workers to identify the effect of the extension on the nature of employment contracts of employed women using Employment and Unemployment Survey (EUS) and Periodic Labour Force Survey (PLFS) of India. The study observed a decline in the probability of women employed as regular salaried workers and an increase in their likelihood of employment as unpaid and wage labourers.

These unintended consequences were suggestively attributed to firms shifting towards temporary or contractual hiring to restrict women’s eligibility for availing maternity benefits, since the mandate requires women to work for at least 160 days continuously with the employer prior to the delivery date. However, there are only two studies, to the best of our knowledge - [Gupta \(2023\)](#) and [Ghosh et al. \(2025\)](#) - that have looked at the effect of the policy on firms’ gendered labour market outcomes. Using data from the CPHS and the Annual Survey of Industries (ASI), [Gupta \(2023\)](#) compares firms with more than ten employees to those with fewer than ten employees and reports a decline in the share of female workers, accompanied by a reduction in the female share of firms’ wage bills. The study also finds that the likelihood of women’s employment declines more sharply in industries that are more heavily exposed to the policy compared to the industries with lower exposure.<sup>4</sup> On the other hand, [Ghosh et al. \(2025\)](#) exploited the pre-treatment duration of maternity leave offered by the employers, comparing firms that previously offered 12 weeks of leave (treatment group) with the firms that already offered 26 weeks of leave before the amendment (control group), using the employer-employee-linked social security records from India’s Employees’ Provident Fund Organization (EPFO) along with other primary surveys and newspaper announcements. They find a decline of 6% in female employment belonging to 18-35 age-group, with no effect on men or older women. Additionally, the study reveals that the existing male workers are more likely to get promoted to positions requiring high skills while women are more likely to be shifted to manual positions.

Our study aims to estimate the effect of the extension in India’s Maternity Benefit Act on firms’ likelihood of employing any female (male) worker, the number of female (male) workers hired, female (male) wage bills, female (male) mandays worked, female (male) wage rate, and firms’ output and profit. Utilizing data from the ASI for the period 2012–2019, we exploit the institutional variation of the policy mandates, which applies only to firms with 10 or more employees, within a difference-in-differences framework. We also examine the heterogeneous effect of the policy mandates across industries, classified by their female employment share. This study is most closely related to [Gupta \(2023\)](#) but departs from it in several important ways. First, unlike [Gupta \(2023\)](#), which focuses primarily on outcomes at the intensive margins such as female employment and wage shares, we examine outcomes at both the extensive and intensive margins, thereby capturing entry into and exit from employment rather than only within-firm adjustments. We utilize observations on all firms, instead of restricting the sample to firms with at least one male and one female worker. This approach avoids potential sample selection bias that may arise if firms that completely exclude female (male) workers are systematically omitted from the analysis. Second, we estimate the effects separately for male and female outcomes. Doing so enables us to distinguish whether the observed changes arise from adjustments in female employment, male employment, or both. Such distinctions cannot be identified when outcomes are expressed only as gender shares. Third, our heterogeneity analysis extends beyond female employment to examine the effects on both the extensive and intensive margins, thereby, providing more comprehensive understanding of the policy’s differential effects across labour market

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<sup>4</sup>The exposure to the industry is defined as the ratio of number of female workers employed in the treated firms in industry ‘k’ and the total number of female workers employed in industry ‘k’.

conditions. Fourth, beyond employment outcomes, we examine the effects of the policy on firms’ output and profit, which has not been studied so far.

This paper makes three contributions to the literature. First, we provide the firm-level evidence that India’s MBAA induced systematic male-for-female labour substitution — complementing the individual-level evidence in [Banerjee et al. \(2026\)](#). However, our results are different from [Ghosh et al. \(2025\)](#), who report no significant impact on male employment. Second, by estimating outcomes in absolute levels for both male and female workers across all firms, we show how firms change hiring patterns to avoid additional costs associated with the mandate. Third, we are the first to examine firm output and profit in the Indian MBAA context, finding that firms absorb the employment restructuring without sacrificing performance, which implies effective substitutability between male and female workers in Indian manufacturing and has direct implications for the welfare evaluation of employer-financed maternity mandates.

Our main findings indicate that the maternity benefit extension has a negative effect of almost 8.4% on the number of female workers hired by firms and leads to a reduction in female mandays worked by 9.5%, while having no significant impact on firms’ female wage bills. In contrast, we find a positive effect on male outcomes, including an increase in the number of male workers hired, by approximately 2.6%, and a 2.1% increase in the male mandays worked. Additionally, we observe a positive effect of approximately 29.3% and 12.5% on female and male wage rates, respectively, but no significant effects on firms’ output and profit. These patterns are consistent with a labour substitution mechanism: firms exposed to the mandate respond by reallocating labour demand from female to male workers, retaining a smaller but higher-wage pool of female employees while hiring additional male workers to maintain production. Moreover, we find that the adverse effects on female outcomes and the corresponding positive effects on male outcomes are concentrated in firms operating in industries with relatively high female employment shares. This industry-level heterogeneity suggests that the observed effects are driven by demand-side responses to the increased cost of female employment, rather than supply-side changes in women’s labour force participation — since supply-side responses would not generate the differential patterns across industries that we document.

The remainder of the paper is structured as follows. Section 2 offers the policy background. Section 3 provides the data and empirical strategy used in the study. Section 4 reports the results obtained from the difference-in-differences specification. Section 5 presents the results on firm-level outcomes. Section 6 reports the robustness analysis. Section 7 concludes with discussion and conclusion.

## 2 Policy Background

In 2017, India amended its Maternity Benefit Act 1961, thereby fulfilling ILO’s standard of providing atleast 14 weeks of maternity leave. The MBAA (2017) was passed in the Rajya Sabha on August 11, 2016, and in the Lok Sabha on March 9, 2017. The Act received the assent of the President

of India on March 27, 2017 and came into force on April 1, 2017. The MBAA increased the paid leave for women from 12 weeks to 26 weeks for their first two children, of which a woman can avail up to a maximum of eight weeks of leave prior to the expected delivery date (instead of six weeks prior to the amendment) and the remaining 18 weeks after childbirth. However, a woman with two or more than two surviving children could avail a maximum benefit of 12 weeks only, of which not more than six weeks before the expected delivery date can be availed by a woman. The amendment is only applicable to women working in an establishment which is a factory, mine or plantation, or in government establishments in which ten or more persons are employed, or were employed, on any day of the preceding twelve months. Additionally, the amendment extended the leave of 12 weeks to adopting and surrogate mothers, from the date the child is handed over to them. The MBAA requires all establishments having 50 or more employees to provide creche facilities to mothers along with provision for four visits a day to the creche. The employer may allow a woman to work from home after the maternity leave, based on the nature of work and mutual agreement between the employer and the woman. Every establishment must inform every woman, in writing, of her entitlement to maternity benefits at the time of her appointment.<sup>5</sup> To avail of the paid maternity leave, a woman is required to have worked for at least one hundred sixty days with the employer in the 12 months preceding the date of her delivery. During the leave, she is entitled to the average daily wages paid to her in the preceding three months prior to the leave.<sup>6</sup>

Unlike other countries like Japan, Canada, France or United Kingdom, where the cost of maternity benefit is primarily borne by the government or social securities companies, in India, the burden of the paid leave is entirely borne by the employers. Thus, India provides a unique setting within the developing country context to study how maternity leave policies affect firms' employment practices as these policies may either help narrow existing gender gaps or, conversely, reinforce them through adjustments in hiring, and workplace adjustments.

### 3 Data Source and Empirical Strategy

#### 3.1 Data Source

We utilize data from the repeated cross-sectional version of the ASI, conducted annually by the National Sample Survey Organization (NSSO) for the financial year 2012-2013 to 2019-2020. It is a nationally representative survey that covers the organized sector manufacturing establishments, including manufacturing processes, repair services, gas and water supply, and cold storage activities, registered under the Factories Act of 1948. We use establishment and firm interchangeably. The schedule for ASI has two parts. Part-I of ASI schedule provides information on firm's characteristics, assets and liabilities, employment and labour cost, bonuses, expenses on staff welfare, contribution to provident funds, receipts, expenses, input items: indigenous and imported, products and by-products

<sup>5</sup>See [https://www.indiacode.nic.in/repealedfileopen?rfilename=A2017-6.pdf&utm\\_source=](https://www.indiacode.nic.in/repealedfileopen?rfilename=A2017-6.pdf&utm_source=)

<sup>6</sup>See [https://www.indiacode.nic.in/bitstream/123456789/19024/1/the\\_maternity\\_benefit\\_act\\_1961.pdf?utm\\_source=](https://www.indiacode.nic.in/bitstream/123456789/19024/1/the_maternity_benefit_act_1961.pdf?utm_source=)

manufactured, distributive expenses, etc. These information are available in different blocks. Part-II reports monthly data on different information of labour statistics, namely, working days, man-days worked, absenteeism, labour turnover, man-hours worked, etc.

We use part-I of the ASI schedule for this analysis which provides information on firm's characteristics, such as, the industry of occupation, type of organization, type of ownership, region, year of initial production, cost of production, input items, expenses, sales, etc. along with the employment details of all the workers working in and for the establishment, such as, the average number of male and female workers directly employed, workers employed through contractors, unpaid family members, other managerial and supervisory staff, mandays worked on manufacturing and non-manufacturing days, wages and salaries, number of mandays paid for, etc. However, the gender distribution of employees is only available for regular as well as casual workers who are directly employed by the firms. We do not have any information on gender of the other employees such as, those employed through contractors, unpaid family workers, supervisors and managerial staff, and other employees. Hence the present analysis is restricted to a set of workers for whom the gender information is available. Although the MBAA is explicitly targeted at female workers, it may also generate spillover effects on male workers. Firms' responses to the amendment may involve adjustments in their workforce composition, such as substitution between female and male workers or reorganization of job roles to manage the increased cost associated with maternity benefits. These adjustments can indirectly affect male workers' employment and wages, even though they are not directly covered by the policy.

The study outcomes comprise the following: (i) a binary indicator for whether a firm employs any female (male) worker, which takes value 1 if the firm reports to have any positive number of female (male) workers and 0, otherwise; (ii) number of female (male) workers directly hired by the firm; (iii) wages and salaries paid to female (male) workers, hereafter referred to as the female (male) wage bill, which includes remuneration paid in respect of workers' employment along with payments for leave periods, overtime, dearness, bonus, etc.; (iv) female (male) mandays worked on manufacturing and non-manufacturing days. The ASI defines the mandays worked on manufacturing days as the total number of workers attending in each shift across all shifts worked on all working days during the accounting year and mandays worked on non-manufacturing days includes mandays worked on repair and maintenance and/or construction works and also non-working days; (v) female (male) wage rate, defined as the ratio of female (male) wage bills and female (male) mandays worked. Additionally, we also examine the effect of the policy on firms' output and profit - available only for a subset of firms. Following the ASI definition, output is measured as gross output, which includes the ex-factory value of products and by-products manufactured (exclusive of taxes and duties, but inclusive of subsidies), changes in work-in-progress, receipts from services rendered, and other operating receipts of the establishment. Profit is defined as the difference between firms' net income and labour costs.

The initial sample comprises 98,083 operational firms, which have reported having a positive number of total employees. We exclude 12 states from the analysis, as they amended other labour market reforms during the study period, such as amendment to night-shift regulations for female workers,

which removes the restriction on females rendering night shift, and changes to the Industrial Disputes Act (IDA) which increases the threshold from 100 to 300 workers for firms to layoff workers before prior permission.<sup>7</sup> Since these policies may independently affect women’s labour market outcomes, as highlighted by [Chakraborty and Mahajan \(2025\)](#) and [Gupta et al. \(2025\)](#), their inclusion could confound the identification of the effect of the maternity benefit mandate. This restriction leads to a sample of 45,723 firms. Finally, we drop firms which have zero male and female workers. The final analytical sample comprises 41,670 firms.<sup>8</sup> We use the STATA 19.5 SE—Standard Edition software for the analysis.

### 3.2 Empirical Strategy

We employ a Difference-in-Differences (DiD) approach to estimate the effect of the MBAA on firms’ hiring decisions and gender-specific employment outcomes at both the extensive and intensive margins. Specifically, we examine the effect on whether a firm employs any female (male) workers, the total number of female (male) workers hired by the firms, female (male) wage bill, female (male) mandays worked, female (male) wage rate, firms’ output, and profit. We use the annual Wholesale Price Index (WPI) with base year 2011-2012 to adjust nominal female (male) wage bill, firms’ output, and profit to real terms, along with taking the natural logarithm (of female (male) wage bill and firms’ output) to account for the skewed nature of these variables. Moreover, we also winsorized all the variables at 99.9<sup>th</sup> percentile to ensure that the estimates are not unduly influenced by extreme observations. Since the provisions of the MBAA apply only to firms employing ten or more workers, we define firms with fewer than ten employees as the control group and firms with 20–29 employees as the treatment group. We exclude firms employing 10–19 workers from the baseline specification because they are in close proximity to the statutory threshold and are therefore most likely to adjust their employment levels strategically in response to the regulation. In particular, firms near the cutoff may have incentives to postpone hiring, or reduce employment to remain below the threshold and avoid the additional compliance costs associated with the mandate. Such behavioral responses would make treatment status endogenous. Excluding firms around the threshold helps ensure a clearer separation between the treatment and control groups, mitigates concerns regarding manipulation of firm size, and strengthens the identification of the effects of the policy. We also exclude larger firms from the analysis because they differ substantially from smaller firms in terms of output, assets and liabilities, production costs, and other expenditures. Focusing on firms with 20 to 29 workers thus allows us to construct a more comparable and internally valid treatment group. The empirical specification of the

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<sup>7</sup>The excluded states include Jharkhand, Rajasthan, Madhya Pradesh, Uttar Pradesh, Maharashtra, Haryana, Andhra Pradesh, Assam, Himachal Pradesh, Punjab, Goa, and Karnataka.

<sup>8</sup>There is a slight change in the reporting method of variables in ASI after 2016, which makes it incomparable with the previous years’ data. It seems that after 2016, ASI started reporting zero values for variables for which information is not available. However, we can not differentiate between the true zero values from those that are reported as zero because of unavailability of data. We account for this change in the best possible way to make the data across all the years comparable - we replace the missing values for the number of female (male) workers, female (male) wage bills, and female (male) mandays worked with zero, if the total number of employees is non-zero and the firms are operational.

model is given by equation (1):

$$Y_{ijst} = \beta_0 + \beta_1 Treat_i + \beta_2 Amendment_t + \beta_3 Treat_i \times Amendment_t + X'_{it}\alpha + \delta_t + \mu_s + \gamma_j + \epsilon_{ijst} \quad (1)$$

where,  $Y_{ijst}$  is the outcome variable for firm  $i$  belonging to industry  $j$  within state  $s$  in year  $t$ .  $Treat_i$  is an indicator for firms which equals one if a firm has 20-29 employees and zero, if it has less than 10 employees.  $Amendment_t$  takes value one for years after the amendment (2017) and zero, otherwise.  $\beta_3$  is the coefficient of interest which gives the effect of the MBAA on the outcome variable for firms belonging to treatment group compared to control group.  $X'_{it}$  is a vector of covariates, including region, firms' initial year of production, type of organization, number of units (in case of joint production), and number of months in operation.<sup>9</sup>  $\delta_t$  denotes the year fixed effects,  $(\mu_s)$  the state fixed effects,  $(\gamma_j)$  the industry fixed effects, and  $\epsilon_{ijst}$  is the error term. The standard errors are robust to heteroskedasticity. We use sampling weights provided by the ASI to make the estimates nationally representative. The identification of the causal effect of the MBAA relies on the parallel trends assumption, which requires that in the absence of the MBAA, firms in the treatment and control groups would have exhibited similar trends in the outcomes over time. Since the counterfactual outcomes for the treatment group are unobserved, we assess the validity of the parallel trend assumption by jointly testing the significance of the pre-treatment interaction coefficients for each of the outcome variables, using the event-study specification of equation (1) as follows:

$$Y_{ijst} = \beta_0 + \beta_1 Treat_i + \beta_2 Amendment_t + \sum_{\substack{l=-4 \\ l \neq -1}}^2 \lambda_l D_{it}^l + X'_{it}\alpha + \delta_t + \mu_s + \gamma_j + \epsilon_{ijst} \quad (2)$$

where,  $l$  is the event time representing the number of years relative to the treatment. It ranges from four years ( $l=-4$ ) before to two years ( $l=2$ ) after the treatment. The event time  $l=0$  denotes the amendment year (2017). We omit the year prior to the amendment ( $l=-1$ ) as the reference year and all the coefficients are interpreted with respect to this.  $D_{it}$  is a set of dummy variables for each event time  $l$  for firm  $i$  in year  $t$ . These dummies will take the value zero for the firms belonging to the control group. Here, the coefficient of interest is  $\lambda_l$ ; for  $\lambda_l > -1$ , it gives the effect of the MBAA on the outcomes variable for firms belonging to the treatment group compared to the control group relative to the year before the treatment and for  $\lambda_l < -1$ , it exhibits the pre-trend, i.e., the difference in average outcome between the treatment and control group prior to the treatment.

Our identification strategy is not without potential concerns. One potential concern is that firms may have anticipated the policy change and adjusted their hiring and firing decisions in advance. Nevertheless, the amendment process was completed within a relatively short period, which reduces the scope for meaningful anticipation effects and supports the validity of our identification strategy. Moreover, it is not very convenient to drastically reduce firms' size because of various other labour laws such as IDA (1947) which requires prior permission from government to retrench a worker.<sup>10</sup>

<sup>9</sup>The definition of each of these variables based on ASI are available in Appendix I.

<sup>10</sup>See here <https://www.indiacode.nic.in/bitstream/123456789/11102/1/industrial-disputes-act-1947.pdf>

Second, there are concerns related to compliance with the amendment. While the policy legally applies to firms employing ten or more employees, we do not observe whether eligible firms actually complied with the maternity benefit provisions. Consequently, treatment in our analysis is defined based on policy eligibility rather than actual take-up. As a result, our estimates should be interpreted as intent-to-treat (ITT) effects, capturing the average effect of exposure to the amendment among eligible firms, rather than the average treatment effect on the treated. To the extent that compliance is imperfect, our estimates likely represent a lower bound of the true treatment effect.

We conduct several robustness checks. First, we perform the analysis considering firms having 10-19 employees as another treatment group. Second, we examine the credibility of the main findings by changing the control group to firms with employees below 8. Third, we assess the robustness of our findings by implementing placebo tests based on both alternative treatment timings and alternative treatment group assignments. Fourth, we also account for both state by year and industry by year fixed effects to validate the results.<sup>11</sup> Our findings are robust to these additional checks.

## 4 Results

### 4.1 Graphical Evidence

Figure 1 shows the distribution of firm size, measured by the total number of employees, which includes workers directly employed by the firms, managerial and supervisory staff, unpaid family workers, and other employees during the study period. We restrict the firm size to be 100 or less. The distribution is highly right-skewed in all years, indicating that most firms are small, with employment concentrated at lower firm sizes, while only a few firms employ a large number of workers. The peak of the distribution lies roughly between 5 and 15 employees, after which the density declines steadily as firm size increases. Importantly, the overall shape of the distribution remains remarkably stable over time, suggesting that the firm size structure does not change substantially across years.

### 4.2 Descriptive Statistics

Table 1 presents summary statistics of the study variables for firms in the treatment and control group in the pre- (2012-13 to 2016-17) and post- (2017-18 to 2019-20) treatment years. It highlights that the key variables remain comparable between the treatment and control groups in both the pre- and post-treatment periods, supporting the notion that the sample composition of the two groups has remained fairly stable over time. The numbers show that, on average, the proportion of firms employing female (male) workers in the treatment group is significantly higher than those in the control group in both the pre- (38.7% vs. 21.1% (98.5% vs 97.2%)) and post-treatment (38.7% vs. 21.5% (98.6% vs 97.7%)) years, respectively. Similarly, other outcomes - such as the number of female (male) workers, female (male) wage bills, and female (male) mandays worked are significantly higher for the treatment group relative to the control group in both periods. We also observe a pronounced

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<sup>11</sup>These results are available upon request.

gender disparity across all labour market outcomes, with levels substantially higher for males than for females. In addition, 35.6% of treated firms are privately owned, compared to only 11.3% of control firms in the pre-treatment period, while the corresponding numbers are 34.4% and 12.1% in the post-treatment years. Finally, firms' output and profit are significantly higher for treated firms compared to control firms in both the pre- and post-treatment years. Table 1 reveals that treatment and control firms differ substantially in observable characteristics — notably in organization type, output levels, and wage bills. This is an inherent feature of using firm size as the source of treatment variation, since larger firms are structurally different from smaller ones along many dimensions. Three considerations support the validity of our identification strategy despite these level differences. First, our baseline specification controls for a rich set of firm characteristics — region, year of initial production, type of organization, number of production units, and months in operation — along with year, state, and industry fixed effects, absorbing time-invariant heterogeneity at the industry-state level and common time trends. Second, the DiD estimator does not require treatment and control firms to be at the same level — it requires only that they would have followed parallel trends in the absence of the amendment. Third, and most importantly, our event study results (Figure 2) confirm that the treatment and control groups followed statistically indistinguishable trends across all outcomes during the four pre-treatment years (2012–2016), the period during which structural differences would generate differential trends if they were confounding the estimates. The joint insignificance of pre-treatment coefficients across all outcome panels supports the validity of the parallel trends assumption despite the level differences documented in Table 1.

### 4.3 Effect of MBAA on Female Labour Market Outcomes

Table 2 shows the effect of MBAA on the likelihood of a firm employing any female worker (Panel A), number of female workers hired (Panel B), female wage bill (Panel C), female mandays worked (Panel D), and female wage rate (Panel E), respectively, for the treatment group compared to the control group, estimated using equation (1). We consider five different specifications as presented in columns (1) to (5). The first specification (column (1)) is the most parsimonious, where we do not account for any of the covariates and fixed effects. Column (2) shows the coefficients after controlling for the covariates, namely, region, firms' initial year of production, type of organisation, number of units (in case of joint production), and the number of months in operation. Column (3) adds year fixed effects to the previous specification. Column (4) further includes state fixed effects, while retaining the year fixed effects and other control variables. Column (5) additionally includes industry fixed effects. We include industry fixed effects based on two-digit National Industrial Classification (NIC)-2008 codes<sup>12</sup>. It is evident from Panel A that we do not see any effect on firms' likelihood of employing any female worker as a result of the MBAA. However, we do see a negative and significant reduction of 0.235 female workers in the treated firms compared to those of the control firms (Panel B, column (5)). This corresponds to a decline of about 8.4% over the average number of female workers. We do not observe any significant effect on female wage bill in the treated firms compared to those of

<sup>12</sup>The description of these industries is available here [https://www.mospi.gov.in/sites/default/files/6ec\\_dirEst/ec6\\_nic\\_2008\\_code.html](https://www.mospi.gov.in/sites/default/files/6ec_dirEst/ec6_nic_2008_code.html)

control firms (Panel C). Panel D exhibits the effect of MBAA on female mandays worked, where we find a negative effect across all the specification (column(1) to (5)). After the MBAA, treated firms experienced, on average, a reduction of about 76 female mandays worked relative to control firms (Panel D, column(5)). This corresponds to a decline of approximately 9.5% over the mean value. We also observe a marginally positive effect at 10% level of significance on female wage rate (Panel E, column(5)). This implies that, after the implementation of the MBAA, female wage rate increased by about Rs. 67.75 in treated firms relative to control firms, indicating an increase of almost 29.3% over the mean value.

#### 4.4 Effect of MBAA on Male Labour Market Outcomes

Table 3 shows the effect of MBAA on the likelihood of a treated firm employing any male worker (Panel A), number of male workers hired (Panel B), male wage bill (Panel C), male mandays worked (Panel D), and male wage rate (Panel E), respectively, compared to the firms belonging to the control group, estimated using equation (1). We consider five different specifications as presented in columns (1) to (5). In the first specification (column (1)) we do not control for any covariates or fixed effects. Column (2) controls for the covariates, such as, region, firms' initial year of production, type of organisation, number of units (in case of joint production), and the number of months in operation. Column (3) adds year fixed effects to the previous specification. Column (4) further includes state fixed effects, while retaining the year fixed effects and other control variables. Column (5) additionally includes industry fixed effects. We include industry fixed effects based on two-digit NIC- 2008 codes. Panel A of Table 3 shows that there is no significant effect of the MBAA on the firms' likelihood of employing any male worker. But, we do observe a positive and significant increase of 0.348 male workers (Panel B, column (5)) in the treated firms compared to the control firms. This corresponds to almost 2.6% increase over the mean value of male workers. We do not find any significant effect on the male wage bill once we control for covariates and fixed effects (Panel C, column (5)). We observe an increase of about 81.91 male mandays worked in the treated firms relative to the control firms. This number represents an increase of almost 2.1% over the mean number of male mandays worked. Panel E shows the effect of MBAA on male wage rate, where we see a positive and significant increase of about Rs. 37.57 in the treated firms relative to control firms (column(5)). This represents an increase of approximately 12.5% over the mean value.

#### 4.5 Event-Study Analysis

In this section, we present the results estimated using equation (2). The key identifying assumption underlying the DiD framework is the parallel trends assumption, which states that in the absence of the treatment, the outcomes for both the treatment and the control groups would have followed similar trends. This assumption is not testable since it requires observing the counterfactual. As an alternative, to assess its validity, we tested the joint significance of the pre-treatment interaction coefficients for each of the outcome variable. Figure 2 shows the event study results for all the outcomes which we have considered earlier (see Panel A to Panel J). The event-time  $l=0$  denotes the amendment

year (2017) and the dashed line at  $l=-1$  indicates the omitted base (reference) year. Each coefficient shows the effect of the MBAA on the treated firms' female (male) labour market outcomes compared to those of control firms, relative to a year before the amendment. We control for year fixed effects, state fixed effects, industry fixed effects (based on two-digit NIC- 2008 codes), region, firms' initial year of production, type of organisation number of units (in case of joint production), and number of months in operation in all the panels. The bars indicate 95% confidence intervals obtained from heteroskedasticity robust standard errors. Sampling weights have been used in all the specifications.

Figure 2 provides evidence that on average, outcomes of firms in the treatment and control groups do not differ significantly prior to the policy amendment, as the pre-treatment coefficients are jointly insignificant across all the outcomes considered (Panels A to J) at 5% level of significance. This supports the validity of the parallel trends assumption. Notably, the negative effects on the number of female workers and female mandays worked (Panel C and G) emerge in the first year after the amendment, suggesting that firms adjusted to the policy with a lag. In contrast, the positive effects on the number of male workers and male mandays worked (Panel D and H) become apparent immediately following the implementation of the amendment. Moreover, the positive effects previously observed for female and male wage rates are not reflected in the event-study estimates, indicating that wage adjustments are less persistent or less precisely estimated in a dynamic framework.

## 4.6 Heterogeneity Analysis

To uncover the mechanisms underlying our baseline results, we conduct a heterogeneity analysis based on firms' exposure to female workers. The effect of the MBAA is likely to vary with the existing share of female workers in a firm, as firms employing a larger proportion of women are mechanically more exposed to the policy. In the Indian manufacturing sector, female employment levels are generally low, and there are no strictly female-intensive industries. However, certain industries are known to employ a relatively higher share of female workers, including food processing, tobacco, wearing apparel, textiles, and leather and related products.<sup>13</sup> We categorize the firms into relatively high and low female share industries based on the average share of female workers in the industries in the pre-treatment years (2012-2013 to 2016-2017) using two-digit NIC- 2008 codes. Relatively high female share industries include industries with more than 15% share of female workers, for example, manufacturing of food, tobacco, wearing apparel, textiles, and leather and related products, manufacturing of chemicals and chemical products, manufacturing of paper, manufacturing of pharmaceuticals, agriculture, hunting and related service activities, other personal service activities, office administrative and support activities, manufacturing of beverages, other mining and quarrying, manufacturing of computer, electronic and optical products, other manufacturing, scientific research and development, services to buildings and landscape. The other remaining industries are categorised as relatively low female share industries, using the two-digit NIC- 2008 codes. A graphical representation of the share of female and male workers across various industries is available in Appendix I (Figure A1). We then

<sup>13</sup>See <https://ceda.ashoka.edu.in/how-many-women-work-in-indias-factories/>.

estimate the policy effects separately for each group. The results are reported in Tables 4 and 5, respectively, for all the outcomes considered earlier. All specifications control for year fixed effects, state fixed effects, and industry fixed effects (based on two-digit NIC- 2008 codes), region, firms’ initial year of production, type of organisation, number of production units (in the case of joint production), and number of months in operation. The standard errors are robust to heteroskedasticity. We use the sampling weights provided by the ASI to make the estimates nationally representative.

Table 4 presents the results on both female and male labour market outcomes (Panels A and B) for firms operating in relatively high female-share industries. We do not find any significant effect on treated firms’ likelihood of employing any female worker (Panel A, column (1)), but we do find a reduction of 0.526 female workers (Panel A, column (2)) and 168.4 female mandays worked (Panel A, column(4)), which corresponds to a 12.0% and 13.3% decline over the mean values, respectively. We also observe an increase of Rs. 35.17 (Panel A, column (5)) in female wage rate (16.0% increase over the average value). Interestingly, these firms also exhibit spillover effects on male labour outcomes (Panel B). It shows an increase of 0.489 male workers and almost 105.4 increase in male mandays worked (Panel B, columns (2) and (4)), respectively. These figures correspond to an increase of 4.1% and 3.0% over the mean values of the number of male workers hired and the number of male mandays worked, respectively. Additionally, we also find an increase of Rs. 33.27 (almost an 12.0% increase over the mean value) in male wage rate (Panel B, column (5)).

In contrast, Table 5 reports the results for firms belonging to relatively low female-share industries. For these firms, we find no statistically significant effects of the MBAA on any of the female labour market outcomes (Panel A, columns (1) to (5)). However, we do see a positive and significant effect of 7.5% on male wage bill (Panel B, column (3)) and an increase of Rs. 42.07 (13.3% increase over the mean value) in male wage rate (Panel B, column (5)).

## 5 Effect of MBAA on Firms’ Output and Profit

We also examine the effect of the policy mandates on firms’ overall performance, measured using firms’ total output and profits. Examining these outcomes is important because any reorganisation of labour inputs may have direct implications for firms’ performance ([Lerner and Appelbaum, 2014](#); [Bennett et al., 2025](#)). Adjustments along the employment margins do not necessarily imply that the remaining or newly hired workers are as productive as those displaced. If the new or incumbent workers are less productive, changes in employment and wages could adversely affect firms’ output and profit ([Brenøe et al., 2024](#)).

We report the estimated effects of the MBAA on firms’ total output and profit, expressed in real terms using the WPI with base year 2011-12 in Table 6 (Panel A and B), respectively. We consider five different specifications as presented in columns (1) to (5). In the first specification (column (1)), we do not control for any covariates or fixed effects. Column (2) shows the coefficients after controlling for

the covariates, such as region, firms’ initial year of production, type of organisation, number of units (in case of joint production), and the number of months in operation. Column (3) adds year fixed effects to the previous specification. Column (4) further includes state fixed effects, while retaining the year fixed effects and other control variables. Column (5) additionally includes industry fixed effects. We include industry fixed effects based on two-digit NIC- 2008 codes. The standard errors are robust to heteroskedasticity. We use the sampling weights provided by the ASI to make the estimates nationally representative. We find no statistically significant effect of the MBAA on any of these performance measures.

## 6 Robustness

### 6.1 Firm Size: 10-19 (Treatment group)

Tables A1 and A2 present the results using firms with 10–19 employees as an alternative treatment group. We do not find any significant effect of the MBAA on female labour market outcomes for this group. However, male labour market outcomes for this group broadly align with the main results.

The absence of female employment effects for firms with 10–19 employees, alongside clear negative effects for firms with 20–29 employees, may be explained by differential compliance costs. Firms with 10–19 employees sit just above the 10-employee statutory threshold and therefore face a qualitatively different strategic choice than firms with 20–29 employees. For the former group, the cheapest response to the mandate is to exit the treatment entirely — by reducing total employment below 10 workers, a relatively modest operational adjustment requiring the departure of at most nine employees. For firms with 20–29 employees, exiting the mandate would require shedding more than half the workforce — a costly and legally constrained option under India’s Industrial Disputes Act (1947), which requires prior government permission to retrench workers. Unable to exit the mandate at reasonable cost, firms with 20–29 employees instead substitute male for female labour within the firm, the response that drives our main results.

A second and complementary explanation relates to the informalization margin. [Bose and Chatterjee \(2024\)](#) document that a key firm response to the MBAA is the conversion of female workers from regular salaried contracts to informal, casual, or contractual arrangements — which disqualify women from maternity benefits by preventing them from accumulating the 160 days of continuous employment required for eligibility. Firms with 10–19 employees, being smaller and less formally organized, are likely to have greater operational flexibility to pursue this margin without triggering regulatory scrutiny. The ASI, which covers only formally employed workers, would record such informalization as a null effect on measured female employment — precisely the pattern we observe for this group. Firms with 20–29 employees, which tend to be more formally organized (Table 1 shows a substantially higher share of private limited companies in this group), may face higher costs of informalization at scale, making within-firm male-for-female substitution the preferred margin.

A third explanation concerns compliance incentives. Firms barely above the 10-employee threshold may calculate that the probability of regulatory inspection and penalty is sufficiently low that non-compliance is the rational response — particularly given the well-documented enforcement gaps in India’s formal manufacturing sector. If firms with 10–19 employees simply do not comply with the extended leave provision, they bear no additional cost and have no incentive to adjust their hiring. Firms with 20–29 employees, which are larger, more formally organized, and more likely to be subject to regulatory scrutiny, may face a higher probability of compliance enforcement, making the cost real and the employment response observable.

Taken together, these three mechanisms — threshold exit, informalization, and differential compliance suggest that the observed treatment effects should be concentrated in firms sufficiently far from the threshold to be unable to exit or informalize cheaply, and sufficiently formal to face credible compliance pressure. This is consistent with what we find. The null finding for firms with 10–19 employees also strengthens the demand-side interpretation of the policy’s effects: firms are sophisticated regulators of their own compliance costs, choosing the cheapest available adjustment margin conditional on their distance from the statutory threshold and their degree of organisational formality. These results differ from [Gupta \(2023\)](#), which reports significant female employment effects for the 10–19 treatment group. This discrepancy likely reflects differences in sample construction and outcome measurement: [Gupta \(2023\)](#) uses share-based outcomes restricted to firms with both male and female workers, whereas we use absolute employment levels for all firms. If firms with 10–19 employees respond primarily by converting female workers to informal arrangements, i.e., reducing their measured formal female employment share, the two approaches could generate different findings even with identical underlying data.

## 6.2 Firm Size: 0-7 (Control group)

We also estimate the effect of MBAA by changing the definition of our control group to firms with employees below 8, keeping unchanged the treatment group which comprises of firms with 20-29 employees. These results are presented in Appendix I (Tables A3 and A4) and are broadly align with the main findings.

## 6.3 Placebo Test

To verify that our estimated effects are driven by the MBAA and not by other contemporaneous factors, we conduct a placebo test by introducing a fake treatment and examining its effect on the outcomes that were found to be significant in the baseline analysis - namely, the number of female (male) workers hired, female (male) mandays worked, and female (male) wage rates. If the baseline results indeed capture the causal effect of the MBAA, we would expect the placebo treatment to have no statistically significant effect on these outcomes. For this purpose, we restricted the analysis to the pre-policy years and chose the fake treatment year to be 2015. Thus, 2015 and 2016 are considered to be the post-treatment years and 2012-2014 are the pre-treatment years. These results have been

shown in Appendix I (Table A5 and A6). Additionally, we also perform a placebo test by replacing the original treatment group (firms having 20-29 employees) with a fake treatment group which never receives the treatment. We consider the firms with 5-9 employees as the alternative treatment group and firms having below 5 employees as another control group. These results are available in Appendix I (A7 and A8). We find no significant effect of the placebo tests, strengthening the credibility of our identification and findings.

## 7 Discussion and Conclusion

We provide firm-level evidence that India’s Maternity Benefit (Amendment) Act 2017 — which extended paid maternity leave from 12 to 26 weeks with costs entirely borne by employers — induced systematic male-for-female labour substitution in the formal manufacturing sector. Treated firms reduced female workers by 8.4% and female mandays by 9.5%, while simultaneously increasing male workers by 2.6% and male mandays by 2.1%. These symmetric adjustments, combined with null effects on female and male wage bills, indicate that firms achieved cost neutrality through workforce reorganization rather than wage compression — reallocating labour demand from female to male workers in response to the increased expected cost and uncertainty of female employment.

The positive effects on wage rates for both female and male workers suggest substitution towards fewer but higher-productivity employees — consistent with quality upgrading documented by [Bennett et al. \(2025\)](#). We note that the female wage rate result is partly mechanical, since it is the ratio of an insignificant wage bill to a declining mandays measure, and interpret it with caution. The male wage rate increase more straightforwardly reflects upward wage pressure as demand for male labour rises. These findings are consistent with [Ghosh et al. \(2025\)](#) on female employment effects but differ in documenting significant male employment gains, which [Ghosh et al. \(2025\)](#) do not find — a difference we attribute to our firm-level data and ASI-based identification strategy.

The heterogeneity results are central to identifying the mechanism. In high female-share industries — those most exposed to the mandate — we find both a significant decline in female employment and a significant increase in male employment, providing direct within-industry evidence of labour substitution. In low female-share industries, female employment effects are absent but male wage rates rise, suggesting economy-wide spillovers as the relative cost of female labour increases. Critically, this two-tier pattern — substitution in exposed industries, wage spillovers in unexposed industries — is inconsistent with a supply-side explanation: if women were voluntarily reducing labour supply to take advantage of extended leave, we would not observe differential effects across industries with varying female employment intensity. The heterogeneity thus provides the closest available evidence for a demand-side causal interpretation within our DiD framework.

The null effects on output and profit are consistent with [Ginja et al. \(2023\)](#) and [Brenøe et al. \(2024\)](#) but goes beyond prior literature. Our results imply that the cost of the mandate is borne not by

firms in the form of lost productivity, but by female workers in the form of lost employment. Male workers serve as effective production substitutes in Indian manufacturing, allowing firms to maintain performance while shifting the burden of the policy onto the group it was designed to protect.

This study has several limitations. First, we are unable to examine the impact of the policy on other categories of workers employed by firms, including contract workers, unpaid family workers, supervisory and managerial staff, and other employees, as gender-disaggregated information is not available for these groups. Second, our analysis cannot assess whether the policy altered the nature or quality of employment. For instance, firms may have substituted regular female employees with contractual or part-time workers in response to the policy. However, the absence of gender-specific information for these employment categories prevents us from examining such effects. Finally, information on firms' output and profits is available only for a relatively small subset of firms, possibly because many firms did not report these variables. Consequently, the findings related to firm performance should be interpreted with caution and cannot be generalized to the full sample.

These findings do not argue for rolling back maternity leave entitlements — the health and developmental benefits for mothers and children are well-documented ([Albagli and Rau, 2019](#); [Rossin, 2011](#); [Ahmed and Fielding, 2019](#)). Rather, they point to three specific policy directions. First, shifting financing from employer liability to social insurance — standard in OECD countries — would preserve the leave entitlement while eliminating the firm-level cost that drives labour substitution. Second, non-transferable, government-financed paternity leave would equalize the expected hiring cost of male and female workers, reducing the statistical discrimination implied in our findings. Third, extending maternity protections to informal sector workers is essential, as the formal sector adjustments we document likely displace women into informal employment where protections are absent. Together, these measures would preserve the social benefits of extended maternity leave while closing the channels through which employer-financed mandates inadvertently reinforce the gender disparities they are designed to address.

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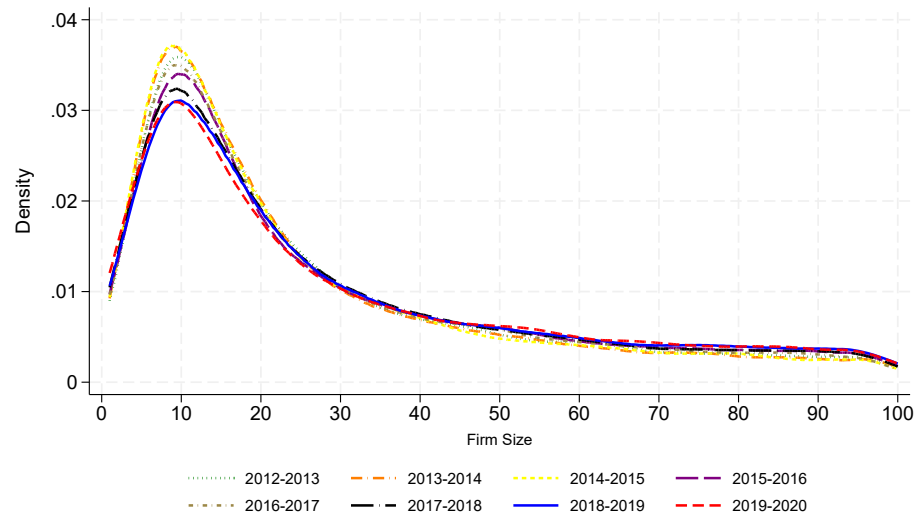
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## Results



**Figure 1.** Distribution of firm size (number of employees) over time

**Table 1.** Descriptive Statistics of the study variables for Treatment and Control firms

| Variable                         | 2012–2013 to 2016–2017      |                             |                      | 2017–2018 to 2019–2020      |                             |                      |
|----------------------------------|-----------------------------|-----------------------------|----------------------|-----------------------------|-----------------------------|----------------------|
|                                  | Treatment                   | Control                     | p-value <sup>§</sup> | Treatment                   | Control                     | p-value <sup>§</sup> |
| Observations                     | N=9,817                     | N=16,505                    |                      | N=5,766                     | N=9,582                     |                      |
| Firms employed any Female Worker | 0.387<br>(0.487)            | 0.211<br>(0.408)            | <0.001               | 0.387<br>(0.487)            | 0.215<br>(0.411)            | <0.001               |
| Firms employed any Male Worker   | 0.985<br>(0.123)            | 0.972<br>(0.166)            | <0.001               | 0.986<br>(0.117)            | 0.977<br>(0.150)            | <0.001               |
| Number of Female Workers         | 3.163<br>(5.330)            | 0.537<br>(1.236)            | <0.001               | 2.879<br>(4.959)            | 0.519<br>(1.189)            | <0.001               |
| Number of Male Workers           | 13.083<br>(5.893)           | 3.685<br>(1.860)            | <0.001               | 13.348<br>(5.820)           | 3.556<br>(1.816)            | <0.001               |
| Female Wage Bills                | 180830.300<br>(323268.300)  | 24859.880<br>(64161.100)    | <0.001               | 211218.500<br>(370657.800)  | 30353.520<br>(77755.810)    | <0.001               |
| Male Wage Bills                  | 1136237.000<br>(837673.500) | 258795.500<br>(229785.800)  | <0.001               | 1447123.000<br>(981596.000) | 301774.400<br>(251703.700)  | <0.001               |
| Female Mandays Worked            | 910.720<br>(1541.067)       | 139.643<br>(333.330)        | <0.001               | 822.787<br>(1416.176)       | 134.478<br>(319.545)        | <0.001               |
| Male Mandays Worked              | 3869.057<br>(1832.143)      | 1019.652<br>(577.424)       | <0.001               | 3938.065<br>(1791.887)      | 979.310<br>(569.485)        | <0.001               |
| Female Wage Rate                 | 219.608<br>(134.645)        | 244.448<br>(2486.808)       | 0.186                | 287.892<br>(186.238)        | 232.712<br>(112.884)        | <0.001               |
| Male Wage Rate                   | 293.412<br>(172.439)        | 266.272<br>(1287.194)       | 0.421                | 372.438<br>(304.356)        | 306.056<br>(218.964)        | <0.001               |
| Urban                            | 0.578<br>(0.494)            | 0.618<br>(0.486)            | <0.001               | 0.553<br>(0.497)            | 0.575<br>(0.494)            | <0.001               |
| <b>Type of Organization</b>      |                             |                             |                      |                             |                             |                      |
| Individual Proprietorship        | 0.237<br>(0.425)            | 0.492<br>(0.500)            | <0.001               | 0.234<br>(0.424)            | 0.485<br>(0.500)            | <0.001               |
| Partnership                      | 0.372<br>(0.483)            | 0.376<br>(0.484)            | <0.001               | 0.404<br>(0.491)            | 0.379<br>(0.485)            | 0.038                |
| Public Limited Company           | 0.016<br>(0.126)            | 0.005<br>(0.070)            | <0.001               | 0.005<br>(0.069)            | 0.002<br>(0.047)            | 0.038                |
| Private Limited Company          | 0.356<br>(0.479)            | 0.113<br>(0.316)            | <0.001               | 0.344<br>(0.475)            | 0.121<br>(0.326)            | <0.001               |
| Cooperative Society              | 0.011<br>(0.102)            | 0.007<br>(0.086)            | 0.224                | 0.010<br>(0.101)            | 0.009<br>(0.095)            | 0.006                |
| Others <sup>†</sup>              | 0.008<br>(0.088)            | 0.008<br>(0.087)            | 0.002                | 0.003<br>(0.055)            | 0.003<br>(0.057)            | 0.242                |
| No. of Units                     | 1.009<br>(0.105)            | 1.002<br>(0.049)            | <0.001               | 1.015<br>(0.136)            | 1.004<br>(0.073)            | <0.001               |
| Year of Initial Production       | 1999.155<br>(13.629)        | 1995.520<br>(26.367)        | <0.001               | 2001.664<br>(15.121)        | 1999.311<br>(14.883)        | <0.001               |
| Months in Operation              | 11.761<br>(1.182)           | 11.490<br>(1.760)           | <0.001               | 11.796<br>(1.076)           | 11.413<br>(1.892)           | <0.001               |
| Total Output                     | 85,800,000<br>(412,000,000) | 16,100,000<br>(125,000,000) | <0.001               | 95,800,000<br>(159,000,000) | 18,800,000<br>(47,100,000)  | <0.001               |
| Total Profit                     | 1,649,384<br>(109,000,000)  | 72,887.770<br>(9,390,314)   | 0.442                | 2,395,386<br>(66,500,000)   | 431,732.200<br>(11,200,000) | 0.574                |

*Notes:* Weighted mean and standard deviation (in parentheses) of the study variables in the pre- and post-treatment years for firms belonging to the treatment and control group. Except for firms that employed female (male) workers, region, and type of organisation, all the other variables are continuous.

<sup>§</sup> p-values are reported using the t-test for continuous variables and the proportion test for categorical variables.

<sup>†</sup> 'Others' includes Joint Family, Khadi and Village Industries Commission, Handlooms, Trusts, Wakf Boards, etc.

**Table 2.** Effect of the MBAA on Female Labour Market Outcomes

|                                            | (1)                    | (2)                    | (3)                    | (4)                    | (5)                    |
|--------------------------------------------|------------------------|------------------------|------------------------|------------------------|------------------------|
| <b>Panel A: Employed any Female Worker</b> |                        |                        |                        |                        |                        |
| Treated $\times$ Amendment                 | -0.004<br>(0.013)      | -0.007<br>(0.013)      | -0.006<br>(0.013)      | 0.002<br>(0.012)       | 0.001<br>(0.011)       |
| Amendment                                  | 0.004<br>(0.007)       | 0.004<br>(0.007)       |                        |                        |                        |
| Treated                                    | 0.176***<br>(0.009)    | 0.213***<br>(0.009)    | 0.213***<br>(0.009)    | 0.183***<br>(0.008)    | 0.166***<br>(0.008)    |
| Mean                                       | 0.347                  | 0.347                  | 0.347                  | 0.347                  | 0.347                  |
| <b>Panel B: Number of Female Workers</b>   |                        |                        |                        |                        |                        |
| Treated $\times$ Amendment                 | -0.266**<br>(0.124)    | -0.287**<br>(0.121)    | -0.286**<br>(0.121)    | -0.226**<br>(0.111)    | -0.235**<br>(0.105)    |
| Amendment                                  | -0.018<br>(0.021)      | -0.018<br>(0.022)      |                        |                        |                        |
| Treated                                    | 2.627***<br>(0.084)    | 2.969***<br>(0.090)    | 2.968***<br>(0.089)    | 2.752***<br>(0.079)    | 2.649***<br>(0.075)    |
| Mean                                       | 2.797                  | 2.797                  | 2.797                  | 2.797                  | 2.795                  |
| <b>Panel C: Female Wage Bill (Logs)</b>    |                        |                        |                        |                        |                        |
| Treated $\times$ Amendment                 | -0.016<br>(0.168)      | -0.058<br>(0.166)      | -0.048<br>(0.165)      | 0.054<br>(0.148)       | 0.044<br>(0.140)       |
| Amendment                                  | 0.086<br>(0.084)       | 0.087<br>(0.083)       |                        |                        |                        |
| Treated                                    | 2.529***<br>(0.107)    | 2.981***<br>(0.110)    | 2.982***<br>(0.110)    | 2.616***<br>(0.097)    | 2.399***<br>(0.092)    |
| Mean                                       | 4.420                  | 4.420                  | 4.420                  | 4.420                  | 4.417                  |
| <b>Panel D: Female Mandays Worked</b>      |                        |                        |                        |                        |                        |
| Treated $\times$ Amendment                 | -82.769**<br>(35.595)  | -91.297***<br>(34.888) | -91.146***<br>(34.782) | -74.026**<br>(32.028)  | -76.479**<br>(30.242)  |
| Amendment                                  | -5.165<br>(5.755)      | -3.122<br>(6.163)      |                        |                        |                        |
| Treated                                    | 771.078***<br>(24.370) | 858.447***<br>(25.726) | 858.198***<br>(25.589) | 797.073***<br>(22.812) | 764.248***<br>(21.411) |
| Mean                                       | 804.292                | 804.292                | 804.292                | 804.292                | 803.706                |
| <b>Panel E: Female Wage Rate</b>           |                        |                        |                        |                        |                        |
| Treated $\times$ Amendment                 | 80.020*<br>(45.390)    | 74.591*<br>(40.039)    | 76.083*<br>(40.995)    | 72.902*<br>(40.159)    | 67.748*<br>(36.176)    |
| Amendment                                  | -11.736<br>(45.025)    | -8.128<br>(39.754)     |                        |                        |                        |
| Treated                                    | -24.840<br>(44.998)    | -54.407<br>(55.182)    | -55.281<br>(56.221)    | -41.995<br>(48.978)    | -45.864<br>(48.621)    |
| Mean                                       | 231.658                | 231.658                | 231.658                | 231.658                | 231.317                |
| Observations                               | 10,396                 | 10,395                 | 10,395                 | 10,395                 | 10,392                 |
| Covariates                                 | No                     | Yes                    | Yes                    | Yes                    | Yes                    |
| Year FE                                    | No                     | No                     | Yes                    | Yes                    | Yes                    |
| State FE                                   | No                     | No                     | No                     | Yes                    | Yes                    |
| Industry FE                                | No                     | No                     | No                     | No                     | Yes                    |
| Observations                               | 41,670                 | 41,669                 | 41,669                 | 41,669                 | 41,665                 |

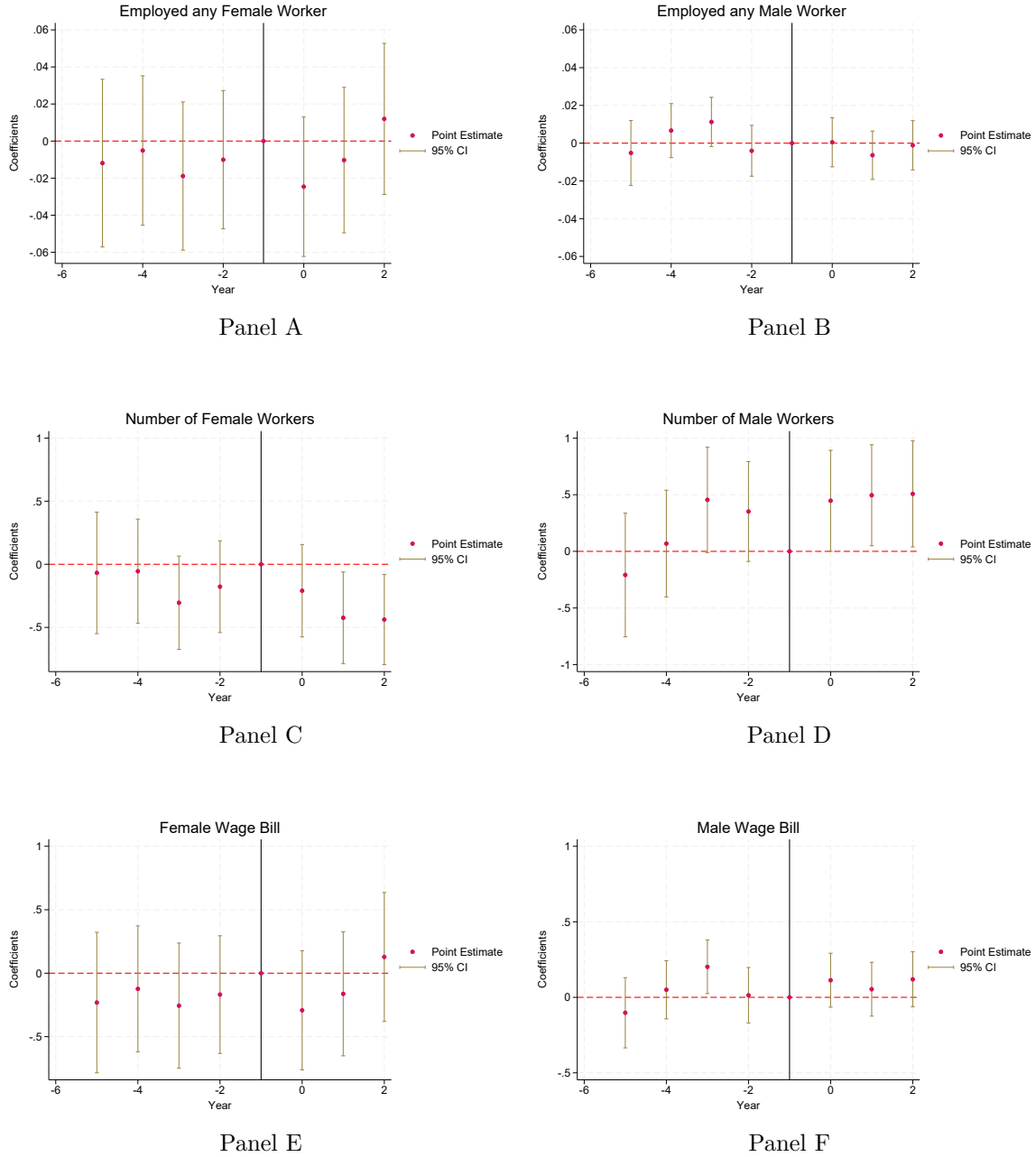
*Notes:* The table shows the effect of MBAA on the treated firms' (firms having 20-29 employees) likelihood of employing any female worker, number of female workers hired, female wage bill, female mandays worked, and female wage rate compared to those of firms belonging to control group (firms with below 10 employees) in Panel A-E, respectively, estimated using equation (1). Column (1) does not control for any covariates or fixed effects. Column (2) shows the coefficients after controlling for covariates such as regions, firms' initial year of production, type of organisation, number of units (in case of joint production), and number of months in operation. Column (3) adds year fixed effects to the previous specification. Column (4) further includes state fixed effects, while retaining the year fixed effects and other control variables. Column (5) additionally includes industry fixed effects. We include industry fixed effects based on two-digit National Industrial Classification (NIC)- 2008 codes. Mean represents the mean of the dependent variable in the pre-treatment years for the treated firms. Standard errors, shown in parentheses, are robust to heteroskedasticity. Sampling weights are also applied. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

**Table 3.** Effect of the MBAA on Male Labour Market Outcomes

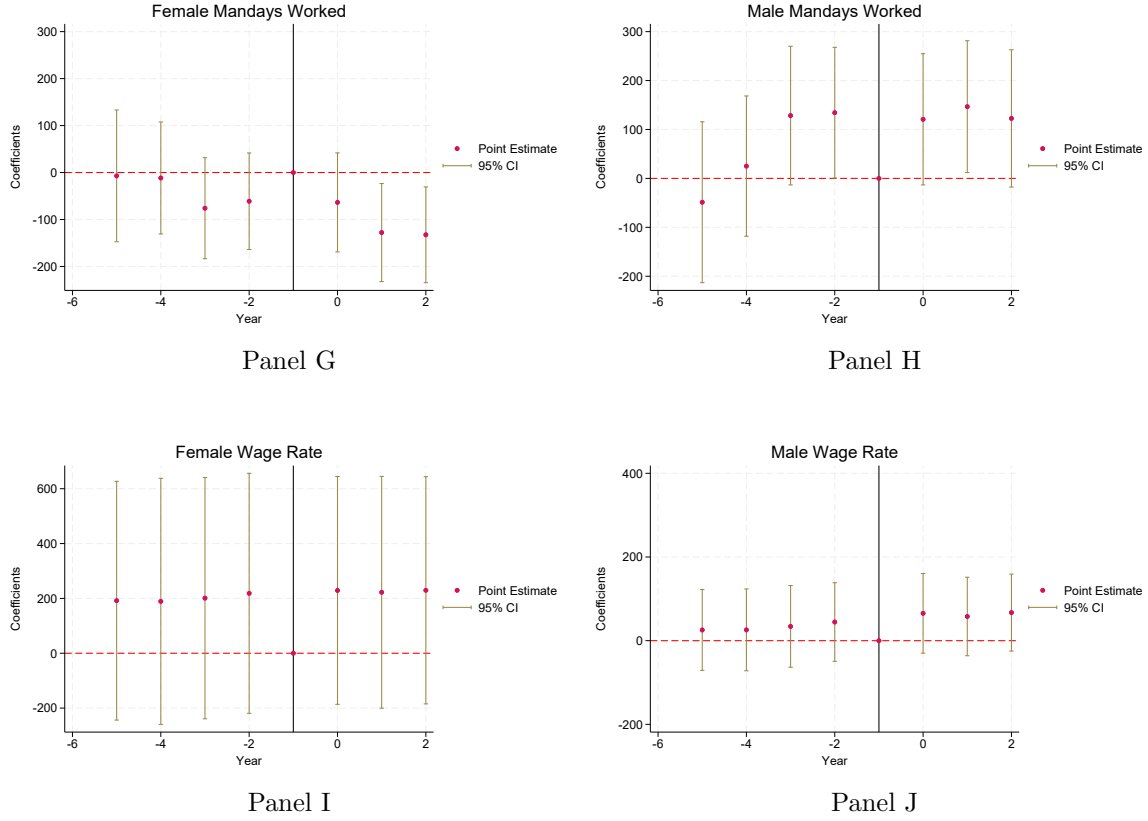
|                                          | (1)                     | (2)                     | (3)                     | (4)                     | (5)                     |
|------------------------------------------|-------------------------|-------------------------|-------------------------|-------------------------|-------------------------|
| <b>Panel A: Employed any Male Worker</b> |                         |                         |                         |                         |                         |
| Treated × Amendment                      | -0.004<br>(0.004)       | -0.004<br>(0.004)       | -0.004<br>(0.004)       | -0.005<br>(0.004)       | -0.004<br>(0.004)       |
| Amendment                                | 0.005**<br>(0.003)      | 0.006**<br>(0.003)      |                         |                         |                         |
| Treated                                  | 0.013***<br>(0.003)     | 0.011***<br>(0.003)     | 0.011***<br>(0.003)     | 0.013***<br>(0.003)     | 0.015***<br>(0.003)     |
| Mean                                     | 0.981                   | 0.981                   | 0.981                   | 0.981                   | 0.981                   |
| <b>Panel B: Number of Male Workers</b>   |                         |                         |                         |                         |                         |
| Treated × Amendment                      | 0.394***<br>(0.138)     | 0.391***<br>(0.138)     | 0.388***<br>(0.138)     | 0.349***<br>(0.135)     | 0.348***<br>(0.131)     |
| Amendment                                | -0.129***<br>(0.032)    | -0.131***<br>(0.032)    |                         |                         |                         |
| Treated                                  | 9.398***<br>(0.089)     | 9.200***<br>(0.094)     | 9.202***<br>(0.094)     | 9.277***<br>(0.091)     | 9.326***<br>(0.088)     |
| Mean                                     | 13.228                  | 13.228                  | 13.228                  | 13.228                  | 13.229                  |
| <b>Panel C: Male Wage Bill (Logs)</b>    |                         |                         |                         |                         |                         |
| Treated × Amendment                      | 0.098*<br>(0.055)       | 0.084<br>(0.054)        | 0.082<br>(0.054)        | 0.054<br>(0.053)        | 0.062<br>(0.051)        |
| Amendment                                | 0.199***<br>(0.036)     | 0.210***<br>(0.036)     |                         |                         |                         |
| Treated                                  | 1.625***<br>(0.037)     | 1.479***<br>(0.039)     | 1.482***<br>(0.038)     | 1.536***<br>(0.038)     | 1.558***<br>(0.037)     |
| Mean                                     | 13.457                  | 13.457                  | 13.457                  | 13.457                  | 13.457                  |
| <b>Panel D: Male Mandays Worked</b>      |                         |                         |                         |                         |                         |
| Treated × Amendment                      | 109.351**<br>(42.623)   | 95.459**<br>(41.685)    | 93.916**<br>(41.661)    | 82.525**<br>(40.916)    | 81.911**<br>(39.585)    |
| Amendment                                | -40.343***<br>(9.922)   | -31.528***<br>(9.689)   |                         |                         |                         |
| Treated                                  | 2849.404***<br>(27.703) | 2744.926***<br>(28.566) | 2745.532***<br>(28.462) | 2769.210***<br>(27.671) | 2780.010***<br>(26.703) |
| Mean                                     | 3913.118                | 3913.118                | 3913.118                | 3913.118                | 3913.444                |
| <b>Panel E: Male Wage Rate</b>           |                         |                         |                         |                         |                         |
| Treated × Amendment                      | 39.242***<br>(11.814)   | 42.143***<br>(11.759)   | 42.547***<br>(11.870)   | 37.567***<br>(11.182)   | 37.565***<br>(10.641)   |
| Amendment                                | 39.784***<br>(10.286)   | 40.125***<br>(9.257)    |                         |                         |                         |
| Treated                                  | 27.140***<br>(10.294)   | 4.559<br>(14.648)       | 5.308<br>(14.664)       | 12.315<br>(13.671)      | 9.827<br>(13.988)       |
| Mean                                     | 300.040                 | 300.040                 | 300.040                 | 300.040                 | 299.891                 |
| Observations                             | 40,698                  | 40,697                  | 40,697                  | 40,697                  | 40,693                  |
| Covariates                               | No                      | Yes                     | Yes                     | Yes                     | Yes                     |
| Year FE                                  | No                      | No                      | Yes                     | Yes                     | Yes                     |
| State FE                                 | No                      | No                      | No                      | Yes                     | Yes                     |
| Industry FE                              | No                      | No                      | No                      | No                      | Yes                     |
| Observations                             | 41,670                  | 41,669                  | 41,669                  | 41,669                  | 41,665                  |

*Notes:* The table shows the effect of MBAA on the treated firms' (firms having 20-29 employees) likelihood of employing any male worker, number of male workers hired, male wage bill, male mandays worked, and male wage rate compared to those of firms belonging to control group (firms with below 10 employees) in Panel A-E, respectively, estimated using equation (1). Column (1) does not control for any covariates or fixed effects. Column (2) shows the coefficients after controlling for covariates such as regions, firms' initial year of production, type of organisation, number of units (in case of joint production), and number of months in operation. Column (3) adds year fixed effects to the previous specification. Column (4) further includes state fixed effects, while retaining the year fixed effects and other control variables. Column (5) additionally includes industry fixed effects. We include industry fixed effects based on two-digit National Industrial Classification (NIC)- 2008 codes. Mean represents the mean of the dependent variable in the pre-treatment years for the treated firms. Standard errors, shown in parentheses, are robust to heteroskedasticity. Sampling weights are also applied. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

# Event Study Plots



**Figure 2.** Effect of MBAA on females' and males' labour market outcomes



**Figure 2.** Effect of MBAA on females' and males' labour market outcomes (contd.)

*Notes:* The figure shows the event-time coefficients estimated using equation (2). The outcomes variables are - firms' likelihood of employing any female (male) worker (Panels A and Panel B), number of female (male) workers hired (Panel C and Panel D), female (male) wage bills (Panel E and Panel F), female (male) mandays worked (Panel G and Panel H), and female (male) wage rates (Panel I and Panel J). The event-time  $l=0$  denotes the amendment year (2017). The dashed line at  $l=-1$  indicates the omitted base (reference) year. Each coefficient shows the effect of MBAA on treated firms' female (male) labour market outcomes compared to control firms, relative to a year before the amendment. The bars indicate 95% confidence intervals obtained from heteroskedasticity robust standard errors. Sampling weights are also applied. We control for year fixed effects, state by year fixed effects, industry by year fixed effects (based on two-digit National Industrial Classification (NIC)- 2008 codes), region, firms' initial year of production, type of organisation, number of units (in case of joint production), and number of months in operation in all the panels.

# Heterogeneity Analysis

**Table 4.** Effects of the MBAA in High Female Share Industries

|                          | (1)                           | (2)                         | (3)                       | (4)                      | (5)                   |
|--------------------------|-------------------------------|-----------------------------|---------------------------|--------------------------|-----------------------|
| <b>Panel A</b>           | Employed<br>any Female Worker | Number of<br>Female Workers | Log (Female<br>Wage Bill) | Female<br>Mandays Worked | Female<br>Wage Rate   |
| Treat $\times$ Amendment | -0.019<br>(0.018)             | -0.526***<br>(0.184)        | -0.207<br>(0.217)         | -168.355***<br>(52.974)  | 35.171***<br>(11.943) |
| Mean                     | 0.480                         | 4.387                       | 6.171                     | 1263.654                 | 219.375               |
| Observations             | 17,150                        | 17,150                      | 17,150                    | 17,150                   | 6,237                 |
| <b>Panel B</b>           | Employed<br>any Male Worker   | Number of<br>Male Workers   | Log (Male<br>Wage Bill)   | Male<br>Mandays Worked   | Male<br>Wage Rate     |
| Treat $\times$ Amendment | -0.005<br>(0.007)             | 0.489**<br>(0.201)          | 0.058<br>(0.099)          | 105.378*<br>(59.934)     | 33.274***<br>(8.790)  |
| Mean                     | 0.967                         | 11.964                      | 13.083                    | 3513.651                 | 277.899               |
| Observations             | 17,150                        | 17,150                      | 17,150                    | 17,150                   | 16,440                |

*Notes:* The table shows the effect of MBAA on the likelihood of firms' employing any female (male) worker, number of female (male) workers hired, female (male) wage bills, female (male) mandays worked, and female (male) wage rate in column (1) to column (5), respectively, for treatment group (firms having 20-29 employees) compared to control group (firms with below 10 employees), estimated using equation (1) for a sub-sample of firms belonging to industries with relatively high female share. Mean represents the mean of the dependent variable in the pre-treatment years for the treated firms. We control for year fixed effects, state fixed effects, industry fixed effects (based on two-digit National Industrial Classification (NIC)- 2008 codes), region, firms' initial year of production, type of organisation, number of units (in case of joint production), and number of months in operation in each column. Standard errors, shown in parentheses, are robust to heteroskedasticity. Sampling weights are also applied. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

# Heterogeneity Analysis

**Table 5.** Effects of the MBAA in Low Female Share Industries

| <b>Panel A</b>           | (1)<br>Employed<br>any Female Worker | (2)<br>Number of<br>Female Workers | (3)<br>Log (Female<br>Wage Bill) | (4)<br>Female<br>Mandays Worked | (5)<br>Female<br>Wage Rate |
|--------------------------|--------------------------------------|------------------------------------|----------------------------------|---------------------------------|----------------------------|
| Treat $\times$ Amendment | 0.012<br>(0.014)                     | -0.011<br>(0.090)                  | 0.175<br>(0.177)                 | -8.921<br>(26.440)              | 126.379<br>(99.823)        |
| Mean                     | 0.242                                | 1.538                              | 3.032                            | 440.244                         | 250.086                    |
| Observations             | 24,515                               | 24,515                             | 24,515                           | 24,515                          | 4,153                      |
| <b>Panel B</b>           | (1)<br>Employed<br>any Male Worker   | (2)<br>Number of<br>Male Workers   | (3)<br>Log (Male<br>Wage Bill)   | (4)<br>Male<br>Mandays Worked   | (5)<br>Male<br>Wage Rate   |
| Treat $\times$ Amendment | -0.003<br>(0.003)                    | 0.239<br>(0.166)                   | 0.075*<br>(0.042)                | 62.541<br>(50.588)              | 42.066**<br>(17.270)       |
| Mean                     | 0.991                                | 14.229                             | 13.752                           | 4229.369                        | 316.842                    |
| Observations             | 24,515                               | 24,515                             | 24,515                           | 24,515                          | 24,253                     |

*Notes:* The table shows the effect of MBAA on the likelihood of firms' employing any female (male) worker, number of female(male) workers hired, female(male) wage bills, female(male) mandays worked, and female(male) wages paid per mandays worked in column (1) to column (5), respectively, for treatment group (firms having 20-29 employees) compared to control group (firms with below 10 employees), estimated using equation (1) for a sub-sample of firms belonging to industries with relatively low female share. Mean represents the mean of the dependent variable in the pre-treatment years for the treated firms. We control for year fixed effects, state fixed effects, industry fixed effects (based on two-digit National Industrial Classification (NIC)- 2008 codes), region, firms' initial year of production, type of organisation, number of units (in case of joint production), and number of months in operation in each column. Standard errors, shown in parentheses, are robust to heteroskedasticity. Sampling weights are also applied. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

# Firm Level Outcomes

**Table 6.** Effect of the MBAA on Firm - Level Outcomes

|                               | (1)                      | (2)                    | (3)                    | (4)                      | (5)                      |
|-------------------------------|--------------------------|------------------------|------------------------|--------------------------|--------------------------|
| <b>Panel A: Output (Logs)</b> |                          |                        |                        |                          |                          |
| Treated $\times$ Amendment    | 0.016<br>(0.040)         | 0.030<br>(0.038)       | 0.031<br>(0.038)       | 0.008<br>(0.037)         | 0.002<br>(0.034)         |
| Amendment                     | 0.224***<br>(0.027)      | 0.203***<br>(0.026)    |                        |                          |                          |
| Treated                       | 1.884***<br>(0.025)      | 1.661***<br>(0.026)    | 1.661***<br>(0.026)    | 1.692***<br>(0.026)      | 1.616***<br>(0.023)      |
| Mean                          | 16.275                   | 16.275                 | 16.275                 | 16.275                   | 16.275                   |
| <b>Panel B: Profit</b>        |                          |                        |                        |                          |                          |
| Treated $\times$ Amendment    | 387,157<br>(1.621e+06)   | 796,920<br>(1.274e+06) | 773,887<br>(1.286e+06) | 794,837<br>(1.302e+06)   | 785,163<br>(1.306e+06)   |
| Amendment                     | 358,844**<br>(144,019)   | 602,486**<br>(253,939) |                        |                          |                          |
| Treated                       | 1.576e+06<br>(1.435e+06) | 1.417e+06<br>(965,383) | 1.439e+06<br>(989,938) | 1.630e+06<br>(1.100e+06) | 1.790e+06<br>(1.143e+06) |
| Mean                          | 467701.1                 | 467683.5               | 467683.5               | 467683.5                 | 467762.2                 |
| Covariates                    | No                       | Yes                    | Yes                    | Yes                      | Yes                      |
| Year FE                       | No                       | No                     | Yes                    | Yes                      | Yes                      |
| State FE                      | No                       | No                     | No                     | Yes                      | Yes                      |
| Industry FE                   | No                       | No                     | No                     | No                       | Yes                      |
| Observations                  | 33,077                   | 33,076                 | 33,076                 | 33,076                   | 33,072                   |

*Notes:* The table shows the effect of MBAA on the treated firms' (firms having 20-29 employees) profit and output compared to those of firms belonging to the control group (firms with below 10 employees) in Panels A and B, respectively, estimated using equation (1). Column (1) does not control for any covariates or fixed effects. Column (2) shows the coefficients after controlling for covariates such as regions, firms' initial year of production, type of organisation, number of units (in case of joint production), and number of months in operation. Column (3) adds year fixed effects to the previous specification. Column (4) further includes state fixed effects, while retaining the year fixed effects and other control variables. Column (5) additionally includes industry fixed effects. We include industry fixed effects based on two-digit National Industrial Classification (NIC)- 2008 codes. Mean represents the mean of the dependent variable. Standard errors, shown in parentheses, are robust to heteroskedasticity. Sampling weights are also applied. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

## A Appendix I

### Definition of Covariates used in the study

#### 1. Type of organization:

- (a) Individual Proprietorship: An individual is the sole owner of the enterprise.
- (b) Partnership: It means relation between persons who have agreed to share the profits of a business carried on by all or any one of them acting for all.
- (c) Public Limited Company: It comprises of two categories, namely, Government Company-Public and Government Company-Private. Government Company- Public is defined as a company where paid-up share capital of the appropriate Government (Central/ State/ Local) is not less than 51% and number of shareholders is at least 7 and no upper limit for number of shareholders. Government Company-Private is defined as a company where paid-up share capital of the appropriate Government (Central/ State/ Local) is not less than 51% and number of shareholders (including the Government) is at least 1 and a maximum number of shareholders is 200.
- (d) Private Limited Company: It comprises of two categories, namely, Non-Government Company-Public and Non-Government Company-Private. Non-Government Company-Public is defined as a company where paid-up share capital of the appropriate Government (Central/ State/ Local) is less than 51% and number of shareholders is at least 7 and no upper limit for number of shareholders. Non-Government Company-Private is defined as a company where paid-up share capital of the appropriate Government (Central/ State/ Local) is less than 51% and number of shareholders is at least 1 and maximum number of shareholders is 200.
- (e) Cooperative Society: It is a society formed through the co-operation of a number of persons to benefit the members. The funds are raised by members' contributions/ investments, and the members share the profits. The government or government agency can also be a member or shareholder of a registered co-operative society, but this fact cannot render the society into a public sector enterprise for the purpose of the survey.
- (f) Others: These are the enterprises not falling under any of the above categories. It

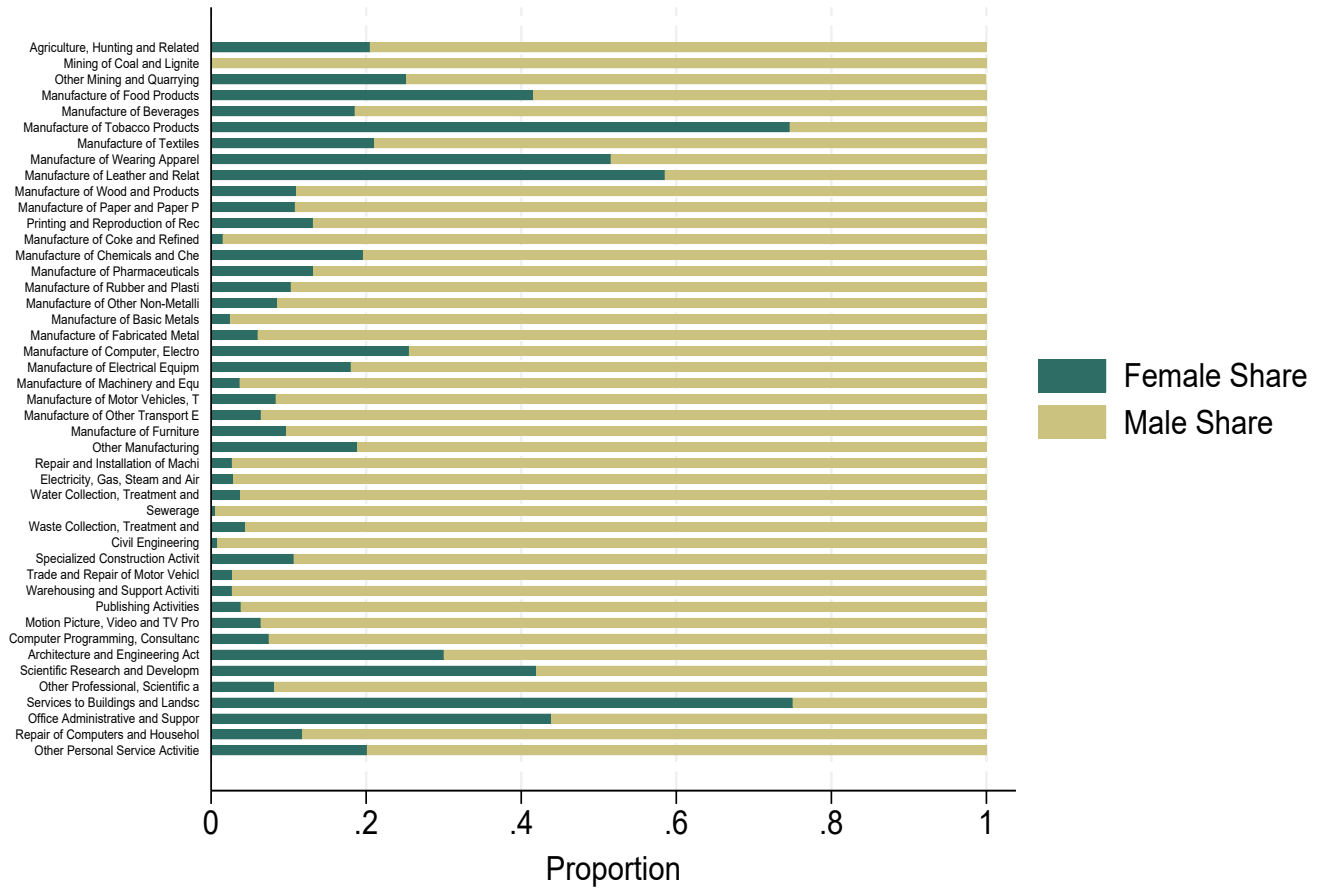
includes Joint Family, Khadi and Village Industries Commission, Handlooms, Trusts, Wakf Boards, etc.

2. **Number of Units:** It is the number of units for which the return is compiled. Here the number of units will be greater than 1 in the case of joint returns only.

3. **Year of Initial Production:** The year of initial production for the factory. It is to be decided irrespective of a change in site or ownership or new registration.

4. **Months in Operation:** The total number of months in which the factory has operated during the accounting year.

# Share of Female and Male Workers



**Figure A1.** Share of female and male workers across different industries

**Table A1.** Effect of the MBAA on Female Labour Market Outcomes (10-19 Treatment group)

|                                            | (1)                   | (2)                   | (3)                   | (4)                   | (5)                   |
|--------------------------------------------|-----------------------|-----------------------|-----------------------|-----------------------|-----------------------|
| <b>Panel A: Employed any Female Worker</b> |                       |                       |                       |                       |                       |
| Treated × Amendment                        | -0.012<br>(0.010)     | -0.015<br>(0.010)     | -0.015<br>(0.010)     | -0.008<br>(0.009)     | -0.011<br>(0.009)     |
| Amendment                                  | 0.004<br>(0.007)      | 0.004<br>(0.007)      |                       |                       |                       |
| Treated                                    | 0.112***<br>(0.007)   | 0.129***<br>(0.007)   | 0.130***<br>(0.007)   | 0.121***<br>(0.006)   | 0.111***<br>(0.006)   |
| Mean                                       | 0.292                 | 0.292                 | 0.292                 | 0.292                 | 0.292                 |
| <b>Panel B: Number of Female Workers</b>   |                       |                       |                       |                       |                       |
| Treated × Amendment                        | -0.038<br>(0.050)     | -0.052<br>(0.049)     | -0.053<br>(0.049)     | -0.027<br>(0.046)     | -0.038<br>(0.044)     |
| Amendment                                  | -0.018<br>(0.021)     | -0.023<br>(0.022)     |                       |                       |                       |
| Treated                                    | 1.000***<br>(0.031)   | 1.098***<br>(0.032)   | 1.100***<br>(0.032)   | 1.052***<br>(0.029)   | 1.011***<br>(0.029)   |
| Mean                                       | 1.404                 | 1.404                 | 1.404                 | 1.404                 | 1.405                 |
| <b>Panel C: Female Wage Bills (Logs)</b>   |                       |                       |                       |                       |                       |
| Treated × Amendment                        | -0.112<br>(0.124)     | -0.154<br>(0.123)     | -0.154<br>(0.123)     | -0.078<br>(0.112)     | -0.104<br>(0.107)     |
| Amendment                                  | 0.086<br>(0.084)      | 0.082<br>(0.083)      |                       |                       |                       |
| Treated                                    | 1.526***<br>(0.077)   | 1.731***<br>(0.078)   | 1.735***<br>(0.078)   | 1.631***<br>(0.071)   | 1.509***<br>(0.068)   |
| Mean                                       | 3.564                 | 3.564                 | 3.564                 | 3.564                 | 3.565                 |
| <b>Panel D: Female Mandays Worked</b>      |                       |                       |                       |                       |                       |
| Treated × Amendment                        | -15.127<br>(13.896)   | -21.139<br>(13.723)   | -21.639<br>(13.727)   | -14.133<br>(12.745)   | -17.317<br>(12.309)   |
| Amendment                                  | -5.165<br>(5.755)     | -4.990<br>(5.800)     |                       |                       |                       |
| Treated                                    | 288.017***<br>(8.822) | 308.944***<br>(8.930) | 309.464***<br>(8.933) | 296.472***<br>(8.215) | 283.714***<br>(7.992) |
| Mean                                       | 390.667               | 390.687               | 390.687               | 390.687               | 390.783               |
| <b>Panel E: Female Wage Rate</b>           |                       |                       |                       |                       |                       |
| Treated × Amendment                        | 61.231<br>(45.368)    | 63.395<br>(46.791)    | 65.714<br>(47.766)    | 64.665<br>(47.384)    | 62.687<br>(45.249)    |
| Amendment                                  | -11.736<br>(45.023)   | -11.992<br>(44.130)   |                       |                       |                       |
| Treated                                    | -32.365<br>(45.154)   | -48.897<br>(51.545)   | -50.500<br>(52.557)   | -43.775<br>(48.554)   | -46.242<br>(47.553)   |
| Mean                                       | 213.239               | 213.239               | 213.239               | 213.239               | 213.228               |
| Observations                               | 14,021                | 14,020                | 14,020                | 14,020                | 14,019                |
| Covariates                                 | No                    | Yes                   | Yes                   | Yes                   | Yes                   |
| Year FE                                    | No                    | No                    | Yes                   | Yes                   | Yes                   |
| State FE                                   | No                    | No                    | No                    | Yes                   | Yes                   |
| Industry FE                                | No                    | No                    | No                    | No                    | Yes                   |
| Observations                               | 57,221                | 57,219                | 57,219                | 57,219                | 57,213                |

*Notes:* The table shows the effect of MBAA on the treated firms' (firms having 10-19 employees) likelihood of employing any female worker, number of female workers hired, female wage bill, female mandays worked, and female wage rate compared to those of firms belonging to control group (firms with below 10 employees) in Panels A to E, estimated using equation (1). Column (1) does not control for any covariates or fixed effects. Column (2) shows the coefficients after controlling for covariates such as regions, firms' initial year of production, type of organisation, number of units (in case of joint production), and number of months in operation. Column (3) adds year fixed effects to the previous specification. Column (4) further includes state fixed effects, while retaining the year fixed effects and other control variables. Column (5) additionally includes industry fixed effects. We include industry fixed effects based on two-digit National Industrial Classification (NIC)- 2008 codes. Mean represents the mean of the dependent variable in the pre-treatment years for the treated firms. Standard errors, shown in parentheses, are robust to heteroskedasticity. Sampling weights are also applied. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

**Table A2.** Effect of the MBAA on Male Labour Market Outcomes (10-19 Treatment group)

|                                          | (1)                     | (2)                     | (3)                     | (4)                     | (5)                     |
|------------------------------------------|-------------------------|-------------------------|-------------------------|-------------------------|-------------------------|
| <b>Panel A: Employed any Male Worker</b> |                         |                         |                         |                         |                         |
| Treated × Amendment                      | -0.007**<br>(0.003)     | -0.007**<br>(0.003)     | -0.007**<br>(0.003)     | -0.008**<br>(0.003)     | -0.007**<br>(0.003)     |
| Amendment                                | 0.005**<br>(0.003)      | 0.006**<br>(0.003)      |                         |                         |                         |
| Treated                                  | 0.013***<br>(0.002)     | 0.012***<br>(0.002)     | 0.012***<br>(0.002)     | 0.013***<br>(0.002)     | 0.013***<br>(0.002)     |
| Mean                                     | 0.984                   | 0.984                   | 0.984                   | 0.984                   | 0.984                   |
| <b>Panel B: Number of Male Workers</b>   |                         |                         |                         |                         |                         |
| Treated × Amendment                      | 0.209***<br>(0.064)     | 0.206***<br>(0.064)     | 0.208***<br>(0.064)     | 0.178***<br>(0.063)     | 0.192***<br>(0.062)     |
| Amendment                                | -0.129***<br>(0.032)    | -0.129***<br>(0.032)    |                         |                         |                         |
| Treated                                  | 4.288***<br>(0.040)     | 4.209***<br>(0.041)     | 4.208***<br>(0.041)     | 4.215***<br>(0.040)     | 4.224***<br>(0.039)     |
| Mean                                     | 8.052                   | 8.052                   | 8.052                   | 8.052                   | 8.053                   |
| <b>Panel C: Male Wage Bills (Logs)</b>   |                         |                         |                         |                         |                         |
| Treated × Amendment                      | 0.025<br>(0.046)        | 0.011<br>(0.045)        | 0.013<br>(0.045)        | -0.011<br>(0.044)       | -0.002<br>(0.043)       |
| Amendment                                | 0.199***<br>(0.036)     | 0.212***<br>(0.036)     |                         |                         |                         |
| Treated                                  | 1.075***<br>(0.030)     | 0.993***<br>(0.030)     | 0.993***<br>(0.030)     | 1.014***<br>(0.029)     | 1.016***<br>(0.029)     |
| Mean                                     | 12.936                  | 12.936                  | 12.936                  | 12.936                  | 12.936                  |
| <b>Panel D: Male Mandays Worked</b>      |                         |                         |                         |                         |                         |
| Treated × Amendment                      | 69.522***<br>(20.104)   | 56.968***<br>(19.315)   | 57.321***<br>(19.317)   | 48.643**<br>(18.892)    | 51.371***<br>(18.509)   |
| Amendment                                | -40.343***<br>(9.922)   | -30.842***<br>(9.665)   |                         |                         |                         |
| Treated                                  | 1289.707***<br>(12.442) | 1236.327***<br>(12.242) | 1235.993***<br>(12.235) | 1238.222***<br>(11.969) | 1240.326***<br>(11.759) |
| Mean                                     | 2342.513                | 2342.528                | 2342.528                | 2342.528                | 2342.526                |
| <b>Panel E: Male Wage Rate</b>           |                         |                         |                         |                         |                         |
| Treated × Amendment                      | 26.321**<br>(10.610)    | 28.410***<br>(10.683)   | 28.926***<br>(10.727)   | 27.015**<br>(10.610)    | 27.313***<br>(10.473)   |
| Amendment                                | 39.784***<br>(10.286)   | 39.984***<br>(9.695)    |                         |                         |                         |
| Treated                                  | 5.739<br>(10.122)       | -3.920<br>(11.673)      | -3.871<br>(11.695)      | 0.750<br>(11.236)       | -0.686<br>(11.550)      |
| Mean                                     | 274.863                 | 274.865                 | 274.865                 | 274.865                 | 274.759                 |
| Observations                             | 56,044                  | 56,042                  | 56,042                  | 56,042                  | 56,036                  |
| Covariates                               | No                      | Yes                     | Yes                     | Yes                     | Yes                     |
| Year FE                                  | No                      | No                      | Yes                     | Yes                     | Yes                     |
| State FE                                 | No                      | No                      | No                      | Yes                     | Yes                     |
| Industry FE                              | No                      | No                      | No                      | No                      | Yes                     |
| Observations                             | 57,221                  | 57,219                  | 57,219                  | 57,219                  | 57,213                  |

*Notes:* The table shows the effect of MBAA on the treated firms' (firms having 10-19 employees) likelihood of employing any male worker, number of male workers hired, male wage bill, male mandays worked, and male wage rate compared to those of firms belonging to control group (firms with below 10 employees) in Panels A to E, estimated using equation (1). Column (1) does not control for any covariates or fixed effects. Column (2) shows the coefficients after controlling for covariates such as regions, firms' initial year of production, type of organisation, number of units (in case of joint production), and number of months in operation. Column (3) adds year fixed effects to the previous specification. Column (4) further includes state fixed effects, while retaining the year fixed effects and other control variables. Column (5) additionally includes industry fixed effects. We include industry fixed effects based on two-digit National Industrial Classification (NIC)- 2008 codes. Mean represents the mean of the dependent variable in the pre-treatment years for the treated firms. Standard errors, shown in parentheses, are robust to heteroskedasticity. Sampling weights are also applied. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

**Table A3.** Effect of the MBAA on Female Labour Market Outcomes (0-7 Control group)

|                                            | (1)                    | (2)                    | (3)                    | (4)                    | (5)                              |
|--------------------------------------------|------------------------|------------------------|------------------------|------------------------|----------------------------------|
| <b>Panel A: Employed any Female Worker</b> |                        |                        |                        |                        |                                  |
| Treated $\times$ Amendment                 | 0.001<br>(0.014)       | -0.002<br>(0.014)      | -0.001<br>(0.014)      | 0.009<br>(0.013)       | 0.006<br>(0.012)                 |
| Amendment                                  | -0.001<br>(0.008)      | -0.001<br>(0.008)      |                        |                        |                                  |
| Treated                                    | 0.199***<br>(0.009)    | 0.240***<br>(0.010)    | 0.239***<br>(0.010)    | 0.208***<br>(0.009)    | 0.190***<br>(0.008)              |
| Mean                                       | 0.347                  | 0.347                  | 0.347                  | 0.347                  | 0.347                            |
| <b>Panel B: Number of Female Workers</b>   |                        |                        |                        |                        |                                  |
| Treated $\times$ Amendment                 | -0.248**<br>(0.124)    | -0.277**<br>(0.121)    | -0.274**<br>(0.121)    | -0.191*<br>(0.111)     | -0.222**<br>(0.105)              |
| Amendment                                  | -0.036*<br>(0.021)     | -0.029<br>(0.024)      |                        |                        |                                  |
| Treated                                    | 2.751***<br>(0.084)    | 3.179***<br>(0.093)    | 3.177***<br>(0.092)    | 2.918***<br>(0.081)    | 2.808***<br>(0.077)              |
| Mean                                       | 2.797                  | 2.797                  | 2.797                  | 2.797                  | 2.795                            |
| <b>Panel C: Female Wage Bills (Logs)</b>   |                        |                        |                        |                        |                                  |
| Treated $\times$ Amendment                 | 0.060<br>(0.174)       | 0.006<br>(0.172)       | 0.020<br>(0.172)       | 0.143<br>(0.155)       | 0.106<br>(0.147)                 |
| Amendment                                  | 0.010<br>(0.096)       | 0.019<br>(0.096)       |                        |                        |                                  |
| Treated                                    | 2.819***<br>(0.111)    | 3.327***<br>(0.117)    | 3.326***<br>(0.117)    | 2.945***<br>(0.105)    | 2.711***<br>(0.100)              |
| Mean                                       | 4.420                  | 4.420                  | 4.420                  | 4.420                  | 4.417                            |
| <b>Panel D: Female Mandays Worked</b>      |                        |                        |                        |                        |                                  |
| Treated $\times$ Amendment                 | -77.064**<br>(35.560)  | -90.392***<br>(34.915) | -89.641***<br>(34.747) | -65.934**<br>(32.050)  | -74.579**<br>(30.331)            |
| Amendment                                  | -10.870**<br>(5.527)   | -4.707<br>(6.581)      |                        |                        |                                  |
| Treated                                    | 805.417***<br>(24.378) | 914.872***<br>(26.483) | 914.329***<br>(26.268) | 840.864***<br>(23.290) | 805.494***<br>(22.047)           |
| Mean                                       | 804.292                | 804.292                | 804.292                | 804.292                | 803.706                          |
| <b>Panel E: Female Wage Rate</b>           |                        |                        |                        |                        |                                  |
| Treated $\times$ Amendment                 | 121.315<br>(77.088)    | 114.766<br>(70.760)    | 117.768<br>(72.675)    | 114.828<br>(72.467)    | 106.858 <sup>†</sup><br>(66.412) |
| Amendment                                  | -53.031<br>(76.873)    | -47.234<br>(69.076)    |                        |                        |                                  |
| Treated                                    | -62.277<br>(76.812)    | -92.443<br>(85.305)    | -94.966<br>(87.343)    | -79.080<br>(77.581)    | -84.948<br>(78.514)              |
| Mean                                       | 231.658                | 231.658                | 231.658                | 231.658                | 231.317                          |
| Observations                               | 8,415                  | 8,415                  | 8,415                  | 8,415                  | 8,412                            |
| Covariates                                 | No                     | Yes                    | Yes                    | Yes                    | Yes                              |
| Year FE                                    | No                     | No                     | Yes                    | Yes                    | Yes                              |
| State FE                                   | No                     | No                     | No                     | Yes                    | Yes                              |
| Industry FE                                | No                     | No                     | No                     | No                     | Yes                              |
| Observations                               | 33,198                 | 33,198                 | 33,198                 | 33,198                 | 33,195                           |

*Notes:* The table shows the effect of MBAA on the treated firms' (firms having 20–29 employees) likelihood of employing any female worker, number of female workers hired, female wage bill, female mandays worked, and female wage rate compared to those of firms belonging to the control group (firms with below 8 employees) in Panels A to E, estimated using equation (1). Column (1) does not control for any covariates or fixed effects. Column (2) shows the coefficients after controlling for covariates such as regions, firms' initial year of production, type of organisation, number of units (in case of joint production), and number of months in operation. Column (3) adds year fixed effects to the previous specification. Column (4) further includes state fixed effects, while retaining the year fixed effects and other control variables. Column (5) additionally includes industry fixed effects. We include industry fixed effects based on two-digit National Industrial Classification (NIC)- 2008 codes. Mean represents the mean of the dependent variable in the pre-treatment years for the treated firms. Standard errors, shown in parentheses, are robust to heteroskedasticity. Sampling weights are also applied. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ , <sup>†</sup> $p < 0.11$ .

**Table A4.** Effect of the MBAA on Male Labour Market Outcomes (0-7 Control group)

|                                          | (1)                      | (2)                      | (3)                      | (4)                      | (5)                      |
|------------------------------------------|--------------------------|--------------------------|--------------------------|--------------------------|--------------------------|
| <b>Panel A: Employed any Male Worker</b> |                          |                          |                          |                          |                          |
| Treated × Amendment                      | -0.004<br>(0.005)        | -0.004<br>(0.005)        | -0.004<br>(0.005)        | -0.005<br>(0.004)        | -0.004<br>(0.004)        |
| Amendment                                | 0.005<br>(0.004)         | 0.005<br>(0.004)         |                          |                          |                          |
| Treated                                  | 0.018***<br>(0.003)      | 0.017***<br>(0.003)      | 0.017***<br>(0.003)      | 0.019***<br>(0.003)      | 0.021***<br>(0.003)      |
| Mean                                     | 0.981                    | 0.981                    | 0.981                    | 0.981                    | 0.981                    |
| <b>Panel B: Number of Male Workers</b>   |                          |                          |                          |                          |                          |
| Treated × Amendment                      | 0.347**<br>(0.138)       | 0.346**<br>(0.138)       | 0.340**<br>(0.137)       | 0.293**<br>(0.135)       | 0.317**<br>(0.131)       |
| Amendment                                | -0.082***<br>(0.031)     | -0.086***<br>(0.032)     |                          |                          |                          |
| Treated                                  | 10.073***<br>(0.089)     | 9.858***<br>(0.097)      | 9.863***<br>(0.096)      | 9.945***<br>(0.094)      | 10.010***<br>(0.091)     |
| Mean                                     | 13.229                   | 13.228                   | 13.228                   | 13.228                   | 13.229                   |
| <b>Panel C: Male Wage Bills (Logs)</b>   |                          |                          |                          |                          |                          |
| Treated × Amendment                      | 0.103<br>(0.063)         | 0.081<br>(0.062)         | 0.077<br>(0.062)         | 0.041<br>(0.061)         | 0.065<br>(0.059)         |
| Amendment                                | 0.195***<br>(0.048)      | 0.211***<br>(0.047)      |                          |                          |                          |
| Treated                                  | 1.895***<br>(0.042)      | 1.748***<br>(0.045)      | 1.753***<br>(0.045)      | 1.818***<br>(0.045)      | 1.836***<br>(0.044)      |
| Mean                                     | 13.457                   | 13.457                   | 13.457                   | 13.457                   | 13.457                   |
| <b>Panel D: Male Mandays Worked</b>      |                          |                          |                          |                          |                          |
| Treated × Amendment                      | 102.566**<br>(42.557)    | 79.618*<br>(41.732)      | 77.275*<br>(41.692)      | 62.747<br>(41.035)       | 69.785*<br>(39.729)      |
| Amendment                                | -33.56***<br>(9.632)     | -15.784<br>(9.849)       |                          |                          |                          |
| Treated                                  | 3,047.455***<br>(27.676) | 2,932.815***<br>(29.265) | 2,933.292***<br>(29.095) | 2,959.445***<br>(28.385) | 2,973.151***<br>(27.484) |
| Mean                                     | 3913.118                 | 3913.118                 | 3913.118                 | 3913.118                 | 3913.444                 |
| <b>Panel E: Male Wage Rate</b>           |                          |                          |                          |                          |                          |
| Treated × Amendment                      | 48.546***<br>(16.692)    | 51.229***<br>(16.390)    | 51.643***<br>(16.524)    | 45.793***<br>(15.599)    | 45.970***<br>(14.778)    |
| Amendment                                | 30.481*<br>(15.648)      | 31.321**<br>(13.722)     |                          |                          |                          |
| Treated                                  | 20.475<br>(15.542)       | -4.080<br>(20.519)       | -3.077<br>(20.511)       | 5.596<br>(19.292)        | 2.062<br>(20.055)        |
| Mean                                     | 300.040                  | 300.040                  | 300.040                  | 300.040                  | 299.891                  |
| Observations                             | 32,381                   | 32,381                   | 32,381                   | 32,381                   | 32,378                   |
| Covariates                               | No                       | Yes                      | Yes                      | Yes                      | Yes                      |
| Year FE                                  | No                       | No                       | Yes                      | Yes                      | Yes                      |
| State FE                                 | No                       | No                       | No                       | Yes                      | Yes                      |
| Industry FE                              | No                       | No                       | No                       | No                       | Yes                      |
| Observations                             | 33,198                   | 33,198                   | 33,198                   | 33,198                   | 33,195                   |

*Notes:* The table shows the effect of the MBAA on the treated firms' (firms having 20–29 employees) likelihood of employing any male worker, number of male workers hired, male wage bill, male mandays worked, and male wage rate compared to those of firms belonging to the control group (firms with below 8 employees) in Panels A to E, estimated using equation (1). Column (1) does not control for any covariates or fixed effects. Column (2) shows the coefficients after controlling for covariates such as regions, firms' initial year of production, type of organisation, number of units (in case of joint production), and number of months in operation. Column (3) adds year fixed effects to the previous specification. Column (4) further includes state fixed effects, while retaining the year fixed effects and other control variables. Column (5) additionally includes industry fixed effects. We include industry fixed effects based on two-digit National Industrial Classification (NIC)- 2008 codes. Mean represents the mean of the dependent variable in the pre-treatment years for the treated firms. Standard errors, shown in parentheses, are robust to heteroskedasticity. Sampling weights are also applied. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

**Table A5.** Effect of the MBAA on Female Labour Market Outcomes (Fake Treatment)

|                                          | (1)                    | (2)                    | (3)                    | (4)                    | (5)                    |
|------------------------------------------|------------------------|------------------------|------------------------|------------------------|------------------------|
| <b>Panel A: Number of Female Workers</b> |                        |                        |                        |                        |                        |
| Treated $\times$ Amendment               | 0.038<br>(0.162)       | 0.030<br>(0.158)       | 0.030<br>(0.158)       | 0.091<br>(0.144)       | 0.058<br>(0.136)       |
| Amendment                                | -0.022<br>(0.027)      | -0.006<br>(0.029)      |                        |                        |                        |
| Treated                                  | 2.611***<br>(0.120)    | 2.977***<br>(0.127)    | 2.976***<br>(0.126)    | 2.721***<br>(0.111)    | 2.630***<br>(0.104)    |
| Mean                                     | 2.649                  | 2.649                  | 2.649                  | 2.649                  | 2.645                  |
| <b>Panel B: Female Mandays Worked</b>    |                        |                        |                        |                        |                        |
| Treated $\times$ Amendment               | -2.073<br>(46.648)     | -6.483<br>(45.382)     | -6.346<br>(45.280)     | 11.209<br>(41.494)     | 1.966<br>(39.072)      |
| Amendment                                | -5.194<br>(7.291)      | 0.478<br>(7.792)       |                        |                        |                        |
| Treated                                  | 771.884***<br>(34.906) | 866.663***<br>(36.723) | 866.298***<br>(36.556) | 793.880***<br>(32.301) | 764.177***<br>(30.285) |
| Mean                                     | 766.168                | 766.168                | 766.168                | 766.168                | 765.152                |
| <b>Panel C: Female Wage Rate</b>         |                        |                        |                        |                        |                        |
| Treated $\times$ Amendment               | -85.370<br>(112.824)   | -78.535<br>(109.469)   | -82.636<br>(112.426)   | -87.132<br>(113.761)   | -90.292<br>(115.609)   |
| Amendment                                | 133.820<br>(112.696)   | 140.976<br>(122.743)   |                        |                        |                        |
| Treated                                  | 8.663<br>(15.815)      | -32.977<br>(29.285)    | -32.455<br>(29.220)    | -13.834<br>(24.638)    | -15.266<br>(25.613)    |
| Mean                                     | 211.567                | 211.567                | 211.567                | 211.567                | 210.930                |
| Observations                             | 6,478                  | 6,477                  | 6,477                  | 6,477                  | 6,474                  |
| Covariates                               | No                     | Yes                    | Yes                    | Yes                    | Yes                    |
| Year FE                                  | No                     | No                     | Yes                    | Yes                    | Yes                    |
| State FE                                 | No                     | No                     | No                     | Yes                    | Yes                    |
| Industry FE                              | No                     | No                     | No                     | No                     | Yes                    |
| Observations                             | 26,322                 | 26,321                 | 26,321                 | 26,321                 | 26,317                 |

*Notes:* The table shows the effect of fake MBAA amendment on the number of female workers hired, female mandays worked, and female wage rate in Panels A to C, respectively, for the firms belonging to the treatment group (firms having 20-29 employees) compared to the control group (firms with below 10 employees), estimated using equation (1). We restricted the analysis to the pre-policy years and chose the fake treatment year to be 2015. Thus, 2015 and 2016 are considered to be the post-treatment years and 2012-2014 are the pre-treatment years. Column (1) does not control for any covariates or fixed effects. Column (2) shows the coefficients after controlling for covariates such as regions, firms' initial year of production, type of organisation, number of units (in case of joint production), and number of months in operation. Column (3) adds year fixed effects to the previous specification. Column (4) further includes state fixed effects, while retaining the year fixed effects and other control variables. Column (5) additionally includes industry fixed effects. We include industry fixed effects based on two-digit National Industrial Classification (NIC)- 2008 codes. Mean represents the mean of the dependent variable in the pre-treatment years for the treated firms. Standard errors, shown in parentheses, are robust to heteroskedasticity. Sampling weights are also applied. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

**Table A6.** Effect of the MBAA on Male Labour Market Outcomes (Fake Treatment)

|                                        | (1)                     | (2)                     | (3)                     | (4)                     | (5)                     |
|----------------------------------------|-------------------------|-------------------------|-------------------------|-------------------------|-------------------------|
| <b>Panel A: Number of Male Workers</b> |                         |                         |                         |                         |                         |
| Treated $\times$ Amendment             | 0.077<br>(0.173)        | 0.071<br>(0.172)        | 0.071<br>(0.172)        | 0.025<br>(0.167)        | 0.052<br>(0.161)        |
| Amendment                              | -0.041<br>(0.040)       | -0.050<br>(0.040)       |                         |                         |                         |
| Treated                                | 9.367***<br>(0.125)     | 9.153***<br>(0.132)     | 9.156***<br>(0.131)     | 9.274***<br>(0.127)     | 9.320***<br>(0.123)     |
| Mean                                   | 13.278                  | 13.278                  | 13.278                  | 13.278                  | 13.279                  |
| <b>Panel B: Male Mandays Worked</b>    |                         |                         |                         |                         |                         |
| Treated $\times$ Amendment             | 43.050<br>(53.807)      | 31.247<br>(52.337)      | 31.233<br>(52.279)      | 17.411<br>(50.899)      | 26.075<br>(49.034)      |
| Amendment                              | -12.173<br>(12.325)     | -9.128<br>(12.082)      |                         |                         |                         |
| Treated                                | 2832.261***<br>(38.852) | 2726.061***<br>(39.942) | 2726.817***<br>(39.804) | 2761.837***<br>(38.620) | 2770.915***<br>(37.120) |
| Mean                                   | 3925.606                | 3925.606                | 3925.606                | 3925.606                | 3926.167                |
| <b>Panel C: Male Wage Rate</b>         |                         |                         |                         |                         |                         |
| Treated $\times$ Amendment             | -5.336<br>(24.773)      | -4.844<br>(25.274)      | -6.086<br>(25.881)      | -7.129<br>(25.969)      | -7.210<br>(26.450)      |
| Amendment                              | 70.517***<br>(24.349)   | 70.437***<br>(25.392)   |                         |                         |                         |
| Treated                                | 29.637***<br>(5.845)    | 5.646<br>(10.604)       | 6.021<br>(10.567)       | 11.776<br>(10.043)      | 10.638<br>(10.096)      |
| Mean                                   | 274.492                 | 274.492                 | 274.492                 | 274.492                 | 274.226                 |
| Observations                           | 25,697                  | 25,696                  | 25,696                  | 25,696                  | 25,692                  |
| Covariates                             | No                      | Yes                     | Yes                     | Yes                     | Yes                     |
| Year FE                                | No                      | No                      | Yes                     | Yes                     | Yes                     |
| State FE                               | No                      | No                      | No                      | Yes                     | Yes                     |
| Industry FE                            | No                      | No                      | No                      | No                      | Yes                     |
| Observations                           | 26,322                  | 26,321                  | 26,321                  | 26,321                  | 26,317                  |

*Notes:* The table shows the effect of fake MBAA amendment on the number of male workers hired, male mandays worked, and male wage rate in Panels A to C, respectively, for the firms belonging to the treatment group (firms having 20-29 employees) compared to the control group (firms with below 10 employees), estimated using equation (1). We restricted the analysis to the pre-policy years and chose the fake treatment year to be 2015. Thus, 2015 and 2016 are considered to be the post-treatment years and 2012-2014 are the pre-treatment years. Column (1) does not control for any covariates or fixed effects. Column (2) shows the coefficients after controlling for covariates such as regions, firms' initial year of production, type of organisation, number of units (in case of joint production), and number of months in operation. Column (3) adds year fixed effects to the previous specification. Column (4) further includes state fixed effects, while retaining the year fixed effects and other control variables. Column (5) additionally includes industry fixed effects. We include industry fixed effects based on two-digit National Industrial Classification (NIC)- 2008 codes. Mean represents the mean of the dependent variable in the pre-treatment years for the treated firms. Standard errors, shown in parentheses, are robust to heteroskedasticity. Sampling weights are also applied. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

**Table A7.** Effect of the MBAA on Female Labour Market Outcomes (Fake Treatment group)

|                                          | (1)                   | (2)                   | (3)                   | (4)                   | (5)                   |
|------------------------------------------|-----------------------|-----------------------|-----------------------|-----------------------|-----------------------|
| <b>Panel A: Number of Female Workers</b> |                       |                       |                       |                       |                       |
| Treated $\times$ Amendment               | 0.014<br>(0.035)      | 0.009<br>(0.035)      | 0.008<br>(0.034)      | 0.024<br>(0.034)      | 0.015<br>(0.033)      |
| Amendment                                | -0.018<br>(0.023)     | -0.019<br>(0.023)     |                       |                       |                       |
| Treated                                  | 0.408***<br>(0.024)   | 0.424***<br>(0.024)   | 0.424***<br>(0.024)   | 0.393***<br>(0.023)   | 0.387***<br>(0.022)   |
| Mean                                     | 0.557                 | 0.557                 | 0.557                 | 0.557                 | 0.557                 |
| <b>Panel B: Female Mandays Worked</b>    |                       |                       |                       |                       |                       |
| Treated $\times$ Amendment               | 3.769<br>(9.149)      | 1.232<br>(9.082)      | 0.958<br>(9.052)      | 4.717<br>(8.894)      | 2.362<br>(8.721)      |
| Amendment                                | -5.205<br>(5.802)     | -3.859<br>(5.816)     |                       |                       |                       |
| Treated                                  | 111.929***<br>(6.230) | 112.457***<br>(6.262) | 112.400***<br>(6.242) | 104.471***<br>(6.016) | 102.984***<br>(5.868) |
| Mean                                     | 147.583               | 147.572               | 147.572               | 147.572               | 147.583               |
| <b>Panel C: Female Wage Rate</b>         |                       |                       |                       |                       |                       |
| Treated $\times$ Amendment               | 475.791<br>(358.596)  | 434.642<br>(324.571)  | 448.259<br>(334.162)  | 468.271<br>(345.812)  | 465.658<br>(344.163)  |
| Amendment                                | -427.886<br>(358.571) | -388.566<br>(322.422) |                       |                       |                       |
| Treated                                  | -465.465<br>(358.499) | -466.393<br>(354.167) | -479.744<br>(364.557) | -490.925<br>(369.192) | -505.685<br>(381.430) |
| Mean                                     | 189.096               | 189.075               | 189.075               | 189.075               | 189.010               |
| Observations                             | 4,880                 | 4,879                 | 4,879                 | 4,878                 | 4,877                 |
| Covariates                               | No                    | Yes                   | Yes                   | Yes                   | Yes                   |
| Year FE                                  | No                    | No                    | Yes                   | Yes                   | Yes                   |
| State FE                                 | No                    | No                    | No                    | Yes                   | Yes                   |
| Industry FE                              | No                    | No                    | No                    | No                    | Yes                   |
| Observations                             | 26,087                | 26,086                | 26,086                | 26,086                | 26,085                |

*Notes:* The table shows the effect of MBAA on the number of female workers, female mandays worked, and female wage rate in Panels A to C, respectively, for the fake treatment group (firms having 5-9 employees) compared to the control group (firms with below 5 employees), estimated using equation (1). Column (1) does not control for any covariates or fixed effects. Column (2) shows the coefficients after controlling for covariates such as regions, firms' initial year of production, type of organisation, number of units (in case of joint production), and number of months in operation. Column (3) adds year fixed effects to the previous specification. Column (4) further includes state fixed effects, while retaining the year fixed effects and other control variables. Column (5) additionally includes industry fixed effects. We include industry fixed effects based on two-digit National Industrial Classification (NIC)- 2008 codes. Mean represents the mean of the dependent variable in the pre-treatment years for the treated firms. Standard errors, shown in parentheses, are robust to heteroskedasticity. Sampling weights are also applied. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

**Table A8.** Effect of the MBAA on Male Labour Market Outcomes (Fake Treatment group)

|                                        | (1)                   | (2)                   | (3)                   | (4)                   | (5)                   |
|----------------------------------------|-----------------------|-----------------------|-----------------------|-----------------------|-----------------------|
| <b>Panel A: Number of Male Workers</b> |                       |                       |                       |                       |                       |
| Treated $\times$ Amendment             | -0.056<br>(0.047)     | -0.051<br>(0.047)     | -0.048<br>(0.047)     | -0.053<br>(0.046)     | -0.031<br>(0.046)     |
| Amendment                              | -0.027<br>(0.032)     | -0.030<br>(0.032)     |                       |                       |                       |
| Treated                                | 2.260***<br>(0.030)   | 2.248***<br>(0.031)   | 2.248***<br>(0.031)   | 2.254***<br>(0.030)   | 2.238***<br>(0.030)   |
| Mean                                   | 4.136                 | 4.136                 | 4.136                 | 4.136                 | 4.136                 |
| <b>Panel B: Male Mandays Worked</b>    |                       |                       |                       |                       |                       |
| Treated $\times$ Amendment             | -10.503<br>(14.854)   | -18.250<br>(14.351)   | -17.808<br>(14.364)   | -21.255<br>(14.160)   | -14.432<br>(14.030)   |
| Amendment                              | -15.046<br>(10.149)   | -3.694<br>(10.150)    |                       |                       |                       |
| Treated                                | 665.340***<br>(9.587) | 633.516***<br>(9.312) | 633.345***<br>(9.289) | 636.752***<br>(9.071) | 632.027***<br>(9.003) |
| Mean                                   | 1161.248              | 1161.201              | 1161.201              | 1161.201              | 1161.202              |
| <b>Panel C: Male Wage Rate</b>         |                       |                       |                       |                       |                       |
| Treated $\times$ Amendment             | 74.999<br>(51.687)    | 73.704<br>(51.253)    | 72.949<br>(51.137)    | 73.617<br>(51.958)    | 74.792<br>(52.357)    |
| Amendment                              | -20.580<br>(51.622)   | -19.933<br>(50.436)   |                       |                       |                       |
| Treated                                | -73.996<br>(51.163)   | -76.358<br>(52.132)   | -74.914<br>(51.907)   | -73.258<br>(52.126)   | -76.081<br>(54.353)   |
| Mean                                   | 253.187               | 253.188               | 253.188               | 253.188               | 253.186               |
| Observations                           | 25,396                | 25,395                | 25,395                | 25,395                | 25,394                |
| Covariates                             | No                    | Yes                   | Yes                   | Yes                   | Yes                   |
| Year FE                                | No                    | No                    | Yes                   | Yes                   | Yes                   |
| State FE                               | No                    | No                    | No                    | Yes                   | Yes                   |
| Industry FE                            | No                    | No                    | No                    | No                    | Yes                   |
| Observations                           | 26,087                | 26,086                | 26,086                | 26,086                | 26,085                |

*Notes:* The table shows the effect of MBAA on the number of male workers, male mandays worked, and male wage rate in Panels A to C, respectively, for the fake treatment group (firms having 5-9 employees) compared to the control group (firms with below 5 employees), estimated using equation (1). Column (1) does not control for any covariates or fixed effects. Column (2) shows the coefficients after controlling for covariates such as regions, firms' initial year of production, type of organisation, number of units (in case of joint production), and number of months in operation. Column (3) adds year fixed effects to the previous specification. Column (4) further includes state fixed effects, while retaining the year fixed effects and other control variables. Column (5) additionally includes industry fixed effects. We include industry fixed effects based on two-digit National Industrial Classification (NIC)- 2008 codes. Mean represents the mean of the dependent variable in the pre-treatment years for the treated firms. Standard errors, shown in parentheses, are robust to heteroskedasticity. Sampling weights are also applied. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .