Doing More Harm Than Good?
Labour Market Consequences to Paid Maternity Leave Extension of India[*]

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Abstract

Can maternity leaves decrease female employment? Amid a recent spark in the global debate, India decided to amend its existing policy on paid maternity leaves and doubled the leaves for mothers. But in this seemingly progressive and generous leave extension, are there unintended harmful consequences on female labour market? Previous research finds widely contrasting impacts of maternal leave policies on gendered labor outcomes, primarily on account of varying policy structure, underlying labor market dynamics, firm's ability to transfer additional costs of the paid leaves, social norms defining gender roles in a country. In this study, I examine the impact of India's 2017 maternity leave legislation on gendered labor market outcomes. The empirical methodology utilizes two-fold analysis to determine the effect of the policy, first by using a firm-level employment data to measure substitution of employment between genders when comparing treated-untreated firms. Then, I use an individual-level panel data to measure the impact to female employment across varying degree of exposure to the policy. I find that the policy leads to substitution of female employees by male counterparts by 1-2 percent points in treated firms, and large declines of female employed in highly exposed industries. This indicates that the policy worsened labour outcomes for the female gender, and provides evidence of a maternity policy fuelling gender differences, when implemented in the absence of an equivalent leave for fathers.

Keywords: Labor Market, Maternity Leave, Public Policy, Gender

JEL: J13; J16; J21

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1 Introduction

The motherhood penalty in the labor market has been a topic of research for the past few decades. To minimize the negative effect of this natural gendered filial shock of motherhood, policymakers have often resorted to well-intended maternity leave policies, birthing a growing volume of literature on the implications of parental leaves on the labor market. Despite the existing empirical evidences on the effects of a paid maternity leave on female employment, the evidence is mixed and inconclusive. Studies have found contradicting evidences on the impact of paid maternity leaves on female labour force participation, especially in the contexts of developing countries. Uribe et al. [2019] find a negative impact of maternity leave extension on female employment in the case of Columbia, while Vu and Glewwe [2022] reports improvements to female labor in Vietnam. Del Rey et al. [2021] study this relation using a panel of 159 countries, and find ambiguous results from increasing leave duration on female labour force participation.

Erosa et al. [2010] discuss how gendered paid parental leave policies might have dynamic affects on the two genders depending on who directly benefits from the policy and the mechanisms involved behind how the firms are able to transfer the additional cost of the leaves. In case of leaves being financed by the government, the redistribution of taxpayers money to the leave-takers will take place and we may see welfare gains for parent who get paid leaves. On the other hand, in the context where the additional cost to the firms is not subsidized, and they are unable to transfer the costs directly, and they might resort to minimize their costs by readjusting labour demand and wages of employees who impose additional costs from the policy. Del Rey et al. [2021] & Kleven [2023] highlight that social norms, among other factors can also play a significant role in the effect of maternity leave policies on female labor outcomes. Some other cross-countries studies find that the length of the paid leaves is crucial in determining the effect on female labour outcomes (Bates et al. [2023] ; Del Rey et al. [2021]; Schönberg and Ludsteck [2014]). These complex mechanisms that derive how paid maternity leave policies might affect female labour outcomes, highlighted in the existing literature, call for context specific analysis of a policy in order to predict and measure the effect of such policy on the female employment in that economy, since the outcomes are an amalgamation of country-specific channels at play.

In this paper, I analyze the gendered impact of increasing paid maternity leaves on the labor market outcomes for both genders in India. India's fast-growing economy provides a unique set-up for our policy analysis, due to its alarmingly low and declining female labor force participation rate, which has been a mystery for economists and policymakers (Afridi et al. [2022], Klasen and Pieters [2015]). One of the primary attribute to this low female labor force participation is the persistent motherhood penalty and absence of child care options in India (Bedi et al. [2021]; Das and Zumbyte [2017]). Men in India are considered the primary breadwinners of the family and women often times are placed under sole unpaid care-work responsibilities with expectations to regard their family care needs above their paid labour market work. Sarkar et al. [2019] report evidence of increase in females exiting the labour market in India, after childbirth. These conservative socio-cultural norms that influence gender roles and female labor demand-supply in India are very distinctive, yet relevant in the context of this analysis, as we need to understand channels through which the policy can potentially affect the female labour market outcomes unintended by the policymakers.

The Government of India intentions from the Maternity Benefits (Amendment) Act of 2017 was to provide better health outcomes for mother and child, while ensuring them financial support in times of maternity. However, in this seemingly progressive step of providing longer leaves for mothers, little is known about its implications on the employment and overall household decision-making dynamics. In the absence of paternal leave policy in place, the policy has a potential to have negative repercussions on the female labor outcomes. Besides the societal barriers to mothers in India, the policy design of India's maternity leave legislation poses additional organizational barriers for working women. The 2017 amendment of the policy extended the fully-paid leaves for working women from 12 weeks to 26 weeks, but no leaves are provisioned for the fathers, alienating fatherhood responsibilities in the policy design itself.

Additionally, the policy also put the entire cost burden of the paid leaves solely upon the employers, with no provision of government/social security subsidy, as is usually in the case of parental leave policies in most developed countries. This added cost burden for employers who hired and retained female employees, likely posing a threat for firms to engage in gender-based discrimination. This unique setting makes a similar studies done in the context of other countries, externally invalid for the Indian maternity leave policy.

Literature on the effects of the paid maternity leave extension policy of India is scant. The only research paper, in my knowledge, empirically studying the labour market effects on the policy in India is Banerjee et al. [2022]. This working paper measures the impact of the policy using an individual level panel, find that high-fertility age-group women in their sample are less likely to be employed after the policy. They likely do give us a good prediction of the direction of the effect, but they are unable to measure the size of the impact on the economy. Their data also makes is impossible to identify the treatment effects of the policy, as individual panel does not distinguish women employed in treated organized sector from untreated informal-sector employment.

My paper empirically measures the impact the paid maternity leave extension policy in Indian setting, estimating the causal impact on the female and male labor market. I also find direct estimates of the treatment effect on the treated firms, and substitution of employment between genders. The data also allows me to explore potential mechanisms that derive these labor market outcomes for women, and capture the variation in the impact across industries by the degree of exposure to the policy. For this, I use two-fold estimation, first I look at firm level data to measure employment substitution and wage changes across genders due to the policy. I compare the larger treated firms with the smaller untreated firms, and estimate the changes in gendered employment and wage shares. I later explore a large individual level data to capture the variation in the impact on females, employed across industries with different treatment exposure. I find a similar negative impact on the female employment as a result of the policy from both the estimation strategies, indicating substitution of female employees by males. I do not find evidence of any significant changes in female wages of those employed, compared to their male counterparts. These results hint towards a potential channel of firms optimizing their added costs due the policy by replacing female employees with lower opportunity-cost male workers and not engaging in gendered wage discrimination.

My study contributes to the existing literature in three ways: Firstly, it provides causal evidence on the effects of paid maternity leave policy of India and add to the scant empirical evidence on implications to the labor market. Secondly, the paper attempts to disentangle the labour demand changes from labour supply factors affected due the policy. The findings hint towards a discriminatory gender-biased employment patterns, by the employer due to disproportional opportunity costs. Lastly, the paper also provides greater insight for policymakers into how some well-intentioned public policies can have unintended consequences, if the policy design is not suited for the context of the specific economy like due to rigid norms, etc.

2 Policy Background

India's landmark Maternity Benefits Act, 1961 ensured women who worked atleast 80 days in the past year, 12 weeks of fully paid maternity leave benefit, employed in any organized establishment (firms who employ 10 or more workers) in India. Recently, the Government of India formally amended this under the Maternity Benefits (Amendment) Act intending to promote workplace gender equality by providing longer time-off for mothers of newborns without loosing their jobs or incomes along with better health for mother and child. The policy increased the paid maternity leaves from previous 12 weeks to 26 weeks for each mother's first two living children, which came into effect in April 2017. The mothers are entitled to 12 weeks of paid leaves for their third child onwards. Furthermore, the amendment also made a provision to extend the benefits to adopting and commissioning mothers. A key feature of this policy is that it only applies to women employed in organized sector firms with at least 10 or more employees, and does not encompass those employed in the organized small firms or the informal sector of India. The firm size with 9 persons or less cutoff, allows us to utilize these untreated firms as a comparison group, and see the effect of the policy on the larger treated firms. The policy also provisioned for firms with more than 50 employees to provide mandatory childcare crèche facilities near work-location and allowing for atleast 4 nursing breaks/day to new mothers. I also exploit this 50 firm size cutoff as a robustness check to see the effect of the additional burden on larger firms. As of now, there is no paternity leave mandated federally in India, except the 15 days optional leaves provisioned for male employed under certain government sectors, under the Central Civil Services (Leave) Rules, 1972.

Another key distinguishing feature of the policy, is that it provides no financial support from the government, insurance or social security to the employers for the leave costs. The direct costs of the paid leaves to the mothers, along with the indirect cost of hiring and training the temporary replacement for the work during the 26 week leave period is all expected to be borne by the employer. This provision is unlike the paid parental leave policies of most other nations like- Ireland, Japan, Canada, UK, Germany, Norway, USA, etc, where atleast part of the cost burden is shared by the government or social security benefits.

Furthermore, the act entitles all eligible women to be given a 100% of the women's wage, averaged over the last three calendar months from the date which she starts her maternity leave. It is also illegal for employers to employ a pregnant woman in work six week immediately preceding her expected delivery date, and the employer must provide information on the benefits entitled to her, in writing at the time of appointment. Failure of the employers to not adhering to the act, can have severe repercussions, and may even lead to imprisonment up to a year or fines, making them liable in court.

3 Conceptual Framework

Under the labour demand factors, the opportunity cost of a female employee rises by the direct cost from fully paid leaves, & indirect cost of finding, and training a temporary replacement worker. If the firms are unable to transfer these costs, there is a possibility of employers resorting to readjusting labour demand of high cost workers, in order to minimize their costs. The added cost burden upon the employer is disproportional across genders, so they might resort to optimizing their cost of production by decreasing hiring & retention, decreasing wages, withholding promotions, hiring at lower pay grade, worsened contract terms, etc for the employees. These are more probable for the gender with higher opportunity cost, thus potentially disproportionately worsening female labour demand.

On the other hand, labour supply channels from the policy can be both negative or positive. The longer leaves might reduce the financial burden for working mothers and allow them to spend more time with their newborns for better health and economic outcomes, as they take longer leaves on maternity, and return to their job later than before, with economic stability. Another potential reason for improved labour supply could be better childcare options, due to the policy design, mandating crèche facilities in larger firms, making them attractive for working women.

But there are also some potential negative implications of the policy on female labour supply. The extended leaves of 6 months now, might make the mother detached from the labour market, and not wanting to return back to her job post maternity. Another negative impact to labour supply channel could be from intra-household substitution of unpaid care work and bargaining power within households due to longer leaves. As the policy design discourages the role of fathers in child-rearing, and alienates fatherhood by making no provision for paternity leaves, male's contribution in unpaid care work might decrease.

These labour demand and supply factors at play will in turn determine how the gendered employment and wages respond to the policy, although I will not be able to directly distinguish between labour demand and labour supply changes. I explore this aspect of the policy, by comparing the gendered labour outcomes on employment and wage ratios across treated and untreated establishments, before and after the policy.

4 Empirical Methodology

Identifying the causal impact of the maternity leave extension policy itself is not an easy task. In a ideal world, we would like to compare two identical firms, one of which is randomly selected for treatment, and then compare their labour outcomes before and after the policy. But as the India's maternity leave extension policy was federally implemented in 2017, I am unable to exploit experimental design or geographical variation in treatment when finding similar comparable entities, and identifying an appropriate counterfactual group for this comparison is challenging. In this paper, I exploit two separate empirical strategies to best estimate the causal impact of the maternity leave extension on the gendered labour market outcomes in India. One uses a firm-level data to capture the impact of the policy on gendered employment and wages, while the second methodology uses an individual level panel of women, to measure impact of the policy on based on the treatment intensity. I explain both methods in detail below-

4.1 Estimation 1: Firm-level Comparison

For the first estimation strategy, I use a difference -in-difference design to compare labour market outcomes, before and after the policy was implemented between the treated-untreated firms. I compare the treated firms of more than 10 persons employed and define the untreated firms in the organized sector with 1-9 persons as control.

Formally I estimate the following Difference-in-Differences model-

$$Y_{it} = \beta_0 + \beta_1(Treat_i * Post_t) + \beta_2 Treat_i + \beta_3 Post_t + \lambda_{k(i)} + \gamma_{s(i)} + \alpha_t + \chi_{s(i)t} + \epsilon_{it} \quad (1)$$

where Y_{it} represents outcomes variables, such as female employment and wage shares of a firm i in year t. $Post_t$ is defined as 1 for all financial years beginning April 2017 and 0 otherwise while $Treat_i$ is the treatment dummy for the firms with 10+ employees. The parameter of interest is β_1, the coefficient of the interaction term for treatment and post, which identifies the effect of the policy. I control for $\lambda_{k(i)}$ industry fixed effects, $\gamma_{s(i)}$ state fixed effects, α_t time fixed effects, & $\chi_{s(i)t}$ state by year fixed effects. These fixed effects control for time-invariant unobserved heterogeneity across industries and states. I cannot control for firm fixed effects directly due to cross-sectional nature of the data. The state by time fixed effects $\chi_{s(i)t}$ ensure that the results are not driven by unobserved state-year shocks. I cluster all standard errors at the state level.

One of the threats to validity of this identification strategy is that these smaller and larger firms might not behave similarly in the absence of the policy. In order to check for parallel pre-treatment trends in my comparison groups, I estimate the event-study model design as follows-

$$Y_{it} = \beta_0 + \beta_{1t} \sum_{t=1}^{T} (Treat_i * Year_t) + \beta_{2t} \sum_{t=1}^{T} Year_t + \beta_3 Treat_i \lambda_{k(i)} + \gamma_{s(i)} + \chi_{s(i)t} + \epsilon_{it} \quad (2)$$

where $Year_t$ is a dummy for each year between 2014 to 2019 except FY 2016, and $Treat_i * Year_t$ represents the treatment-time interaction terms, and β_{1t} are my coefficients of interest. In order to rule out any differences between my treatment-control firms pre-treatment for the validity of my estimation, I should expect my β_{1t} coefficients to be significantly close to zero before 2017 policy. If this does not hold, there is a potential threat of factors unrelated to the maternity leave policy itself to be causing labour market outcomes to be different across the two groups over time.

Among possible confounders to my estimation might be other policy shocks such as Demonetization in India or structural transformations, that happened around the time of the policy, that could affect the local labour demand in smaller and larger firms differently. For instance, if smaller firms are more likely to be cash dependent, and adversely respond with labour demand than compared to larger firms. My estimation methodology does control for this to some extent, by adding industry & state by time fixed effects. Additionally, even if some factors were likely to affect these firms differently at all, it should not affect the gender compositions of the employees within a firm, hence comparing the female employment shares should be valid.

4.2 Estimation 2: Individual-Level Comparison

For my second estimation design, I use a difference-in difference model, to compare the treatment effect on women directly by the intensity of treatment. The treatment indicator is a function of industry level exposure to the policy. The women employed in highly exposed industry before the policy was implemented are my treatment group, which I compare to women employed in industries with low exposure to the policy. The exposure of an industry to the policy is defined as the baseline (2012) ratios of women employed in treated firms in industry $k(i)$ to total women employment:

$$Exposure_k = \left[\frac{(\text{No. of Females Employed in Treated firms})_{k(i)}}{(\text{Total Women Employed})_{\ k(i)}}\right] \tag{3}$$

There is a large variation in the treatment exposure, fairly uniformly distributed between 0 and 1. I then calculate a threshold cutoff exposure, dividing my sample into almost equal size of treatment and control groups, with a cutoff of 0.5, based on the rule below:

$$Treat_{ik} = \begin{cases} 1, & \text{if } Exposure_k \geq \text{Cutoff Exposure (=0.5)} \\ 0, & \text{if } Exposure_k < \text{Cutoff Exposure (=0.5)} \end{cases} \tag{4}$$

I thus divide my individual panel of employed women into treated and control groups, based on their industry of employment in the pre-period (2014-2016). I then estimate the following Fuzzy Difference-in-Differences regression, to compare the labour market outcomes of more treated vs control (less treated) women, before and after the policy in 2017. The estimated model is as follows:

$$Y_{it} = \beta_0 + \beta_1(Treat_i * Post_t) + \beta_2 Treat_i + \beta_3 Post_t + \alpha_t + \gamma_i + \chi_{s(i)t} + \epsilon_{it} \tag{5}$$

where Y_{it} represents outcomes variables, such as employment for woman i in year t. $Post_t$ is defined as 1 for all years beginning 2017 and 0 otherwise. $Treat_i$ is the treatment dummy for the woman treatment status calculated based on industry of employment at baseline as described above. The main parameter of interest is β_1 the coefficient of the interaction term for treatment and post, which identifies the effect of the policy on the females employment. I cluster all standard errors at the state level. α_t represents the year effects, γ_i represents individual fixed effects, and χ_{st} are state*year fixed effects. These state by time fixed effects ensures that these results are not driven by just state-year shocks.

One of the threats to validity of this identification strategy is that the women employed in high exposure industries might not have similar behaviour as the ones in low exposure industries, due to the differences across industry types, in the absence of the policy. In order to check for parallel pre-treatment trends in my comparison groups, I also extent the estimation using an event-study model design as follows-

$$Y_{it} = \beta_0 + \beta_{1t}\sum_{t=1}^{T}(Treat_i * Year_t) + \beta_{2t}\sum_{t=1}^{T} Year_t + \beta_3 Treat_i + \gamma_i + \chi_{st} + \epsilon_{it} \tag{6}$$

where $Year_t$ is a dummy for each year between 2014 to 2019 excluding 2017, and $Treat_i * Year_t$ represents the treatment-time interaction terms, and β_{1t} are my coefficients of interest. In order to rule out any differences between my treatment-control female outcomes pre-treatment, for the validity of my estimation, I should expect my β_{1t} coefficients to be significantly close to zero before 2017 policy. Controlling for individual level fixed effects in my regressions makes sure that the individual-level time invariant heterogeneity is controlled for.

5 Data

This section comprises of description to the main sources of data that I use for my two empirical analyses, a firm level data and a individual level data.

5.1 Firm Level Data

To understand the labor market effects to the policy, I use a large annual firm-level cross-sectional data - Annual Survey of Industries(ASI) which surveys the manufacturing and mining sector in India. The data includes a yearly repeated cross-section sample of about 16k nationally representative firms of the organized manufacturing industries in India. I use the annual surveys for financial years 2013-2019 for the analysis. Each firm is surveyed on their employee compositions and firm-industry performance measures. I utilize outcomes like gendered shares of employment and wage paid per firm from this dataset to compare across treated and untreated firms. Treated firms are defined as those firms with atleast 10 or more workers engaged, who were covered under the Maternity Amendment Policy (2017) and had to provide mandatory maternity benefits to the female workers. The untreated firms with less that 10 workers in my sample are used as the control group, as they should not face any direct impact of the policy amendment.

Figure 1 below shows us distribution of firm size in my sample both pre and post policy (excluding firms size larger than 100). We see some spike of firm size distribution, at size of 10. This is because of a lot of other policies are discontinuous at 10, as the factories act of India is only applicable for firm with size 10 or above. But we see that the firm size is distributed very similarly in the pre-2017 period (red line) when compared to the post period (blue line), especially across the policy threshold of firm size 10.

Table 5.1 and 5.2 below gives us baseline summary statistics of the Treated vs control group firms with their standard deviations. We see that two comparison groups are different from each other, and have significant differences across most variables except wages, as we would have expected due to size difference in firms. Even though, they don't match on baseline characteristics, differences in levels do not pose a threat to identification of my empirical strategy. I will argue the parallel trend assumption still holds, for which I will present event study results later.

5.2 Individual Level Data

I utilize an individual level survey- Consumer Pyramids Household Survey(CPHS) conducted by the Center for Monitoring Indian Economy (CMIE) which is a nationally representative large panel surveying individuals on demographics and labour market outcomes, etc. I use this to evaluate the patterns of female employment in India as a result of the policy for my second estimation design. The data contains a sample of about 60k females per year, in over 30 industry categories, in all sectors. I use the panel for years from 2014, which is when the data was first collected, upto 2019, to exclude any effects from the pandemic.

Figure 2 below, shows us raw trends in employment shares by treatment groups, for all adult employed females in my panel data. We see a declines in the employment shares across both the high and low exposure treated groups in the raw data. These are just raw data and do not show any causal impact.

Figure 1: Firm Size Distribution

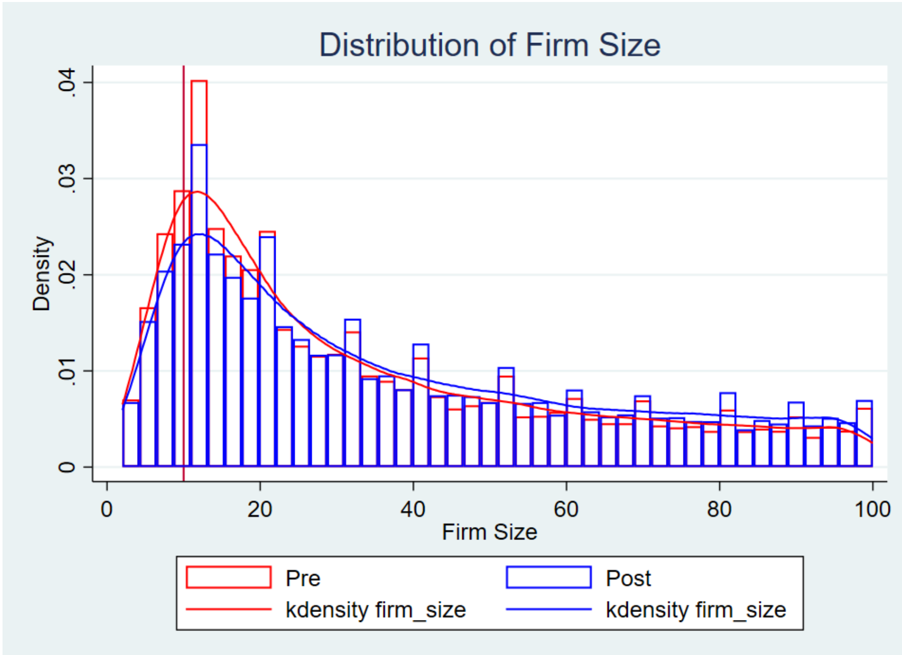

Figure 2: Raw Employment Shares by Treatment Exposure

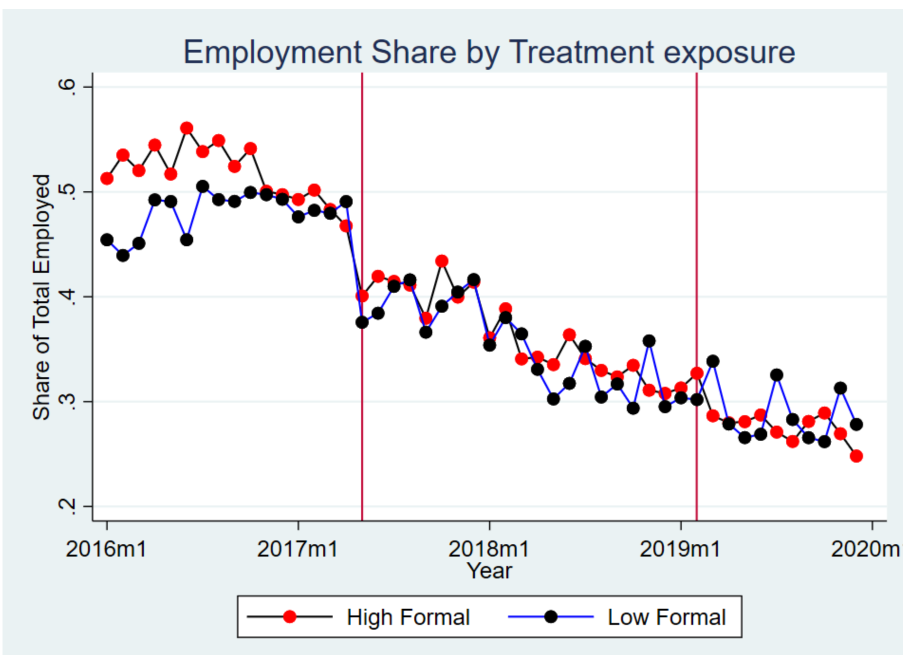

Table 5.1: Firm Characteristics - Baseline

Variable	(1) Full Sample Mean/(SD)	(2) Control Firms Mean/(SD)	(3) Treated Firms Mean/(SD)
Total Regular Employment	188.386	4.927	200.929
	(758.836)	(1.617)	(782.781)
No. Female Employed	68.458	2.092	72.995
	(481.573)	(1.192)	(497.441)
No. Male Employed	119.929	2.835	127.934
	(408.877)	(1.444)	(421.438)
Female FTEs/Day	68.413	2.010	72.953
	(481.575)	(1.203)	(497.443)
Male FTEs/Day	119.901	2.775	127.909
	(408.886)	(1.446)	(421.446)
Shifts Paid/Female	1.156	1.065	1.163
	(1.155)	(0.348)	(1.190)
Shifts Paid/Male	1.798	1.133	1.843
	(72.308)	(0.683)	(74.739)
Female Hourly Wage Rate	351.911	231.189	360.165
	(432.102)	(122.709)	(444.281)
Male Hourly Wage Rate	398.531	279.273	406.685
	(296.514)	(135.027)	(302.734)
Bonusavg	62.460	17.358	65.543
	(303.560)	(33.699)	(313.406)
Welfareavg	87.364	13.337	92.425
	(628.740)	(31.220)	(649.520)
ProvidentFundavg	103.546	24.534	108.948
	(363.360)	(52.058)	(374.723)
urban	0.560	0.580	0.559
	(0.496)	(0.494)	(0.497)
Total no. of Working days	299.347	277.982	300.808
	(39.395)	(55.690)	(37.588)
Number of observations	16048	1027	15021

Table 5.2: Firm Characteristics(continued)

Variable	(1) Full Sample Mean/(SD)	(2) Control Firms Mean/(SD)	(3) Treated Firms Mean/(SD)
Total Regular Employment	188.386	4.927	200.929
	(758.836)	(1.617)	(782.781)
Share of Female Employees	0.350	0.432	0.345
	(0.268)	(0.185)	(0.272)
Share of Male Employees	0.650	0.568	0.655
	(0.268)	(0.185)	(0.272)
Share of Female FTEs/Day	0.349	0.426	0.344
	(0.269)	(0.193)	(0.273)
Share of Male FTEs/Day	0.651	0.574	0.656
	(0.269)	(0.193)	(0.273)
Share of Shifts/day/Female	0.990	0.978	0.991
	(0.077)	(0.138)	(0.071)
Share of Shifts/day/Male	1.000	1.010	0.999
	(0.036)	(0.095)	(0.028)
Share of Shifts Paid- Female	0.997	0.971	0.999
	(0.288)	(0.149)	(0.295)
Share of Shifts Paid- Male	1.003	1.018	1.002
	(0.116)	(0.115)	(0.115)
Share of Female Wage Bill	0.325	0.385	0.321
	(0.262)	(0.192)	(0.266)
Share of Male Wage Bill	0.675	0.615	0.679
	(0.262)	(0.192)	(0.266)
Share of Female Hourly Wage Rate	1.473	0.912	1.511
	(64.126)	(0.275)	(66.282)
Share of Male Hourly Wage Rate	1.081	1.089	1.080
	(0.343)	(0.244)	(0.349)
Number of observations	16048	1027	15021

Table 5.3 below shows the summary statistics of the females at baseline. We do not see large significant differences in age and employment, health characteristics across my treated vs control sample. But some slight differences in possession of a mobile phone and urban rural divide. But we should not be concerned, as I include individual level and geographic fixed effects in my regressions.

Table 5.3: Summary statistics - Baseline

	(1) Full Sample		(2) Control Indust		(3) Treated Indust	
Variable	N	Mean/(SD)	N	Mean/(SD)	N	Mean/(SD)
Female	691379	1.000 (0.000)	429973	1.000 (0.000)	261406	1.000 (0.000)
Age	691379	40.778 (10.981)	429973	41.563 (10.909)	261406	39.488 (10.976)
Employed	691379	0.369 (0.483)	429973	0.360 (0.480)	261406	0.385 (0.487)
Empoyed since	392161	23.109 (19.758)	246216	23.784 (19.416)	145945	21.969 (20.271)
Married	108255	0.752 (0.432)	68187	0.771 (0.420)	40068	0.719 (0.449)
Has Mobile Phone	691379	0.557 (0.497)	429973	0.487 (0.500)	261406	0.672 (0.469)
Is Healthy	691251	0.975 (0.157)	429923	0.972 (0.165)	261328	0.979 (0.143)
Urban	691379	0.628 (0.483)	429973	0.508 (0.500)	261406	0.825 (0.380)

I also utilize the 2012 Employment-Unemployment Survey of India, to measure the exposure indicator for each industry k, based on the percentage of women employed in treated firms over total women employed for each industry, to calculate the Exposure index explained in section 4.2. I use 2012 as my baseline year, as it is the last labour force survey published by the government prior to 2017.

6 Results

This section discusses the results from the regressions described in the estimation strategy.

6.1 Results from Firm Data

Figure 3 below show results from regression 2 , for the outcome share of female full time employment equivalent per day (henceforth FTE/day). I do not see any signs of pre-trends, as there are no significant differences in the pre-period estimates, which provides evidence towards in the validity of my design.

Figure 3: Share of Female FTE/Day

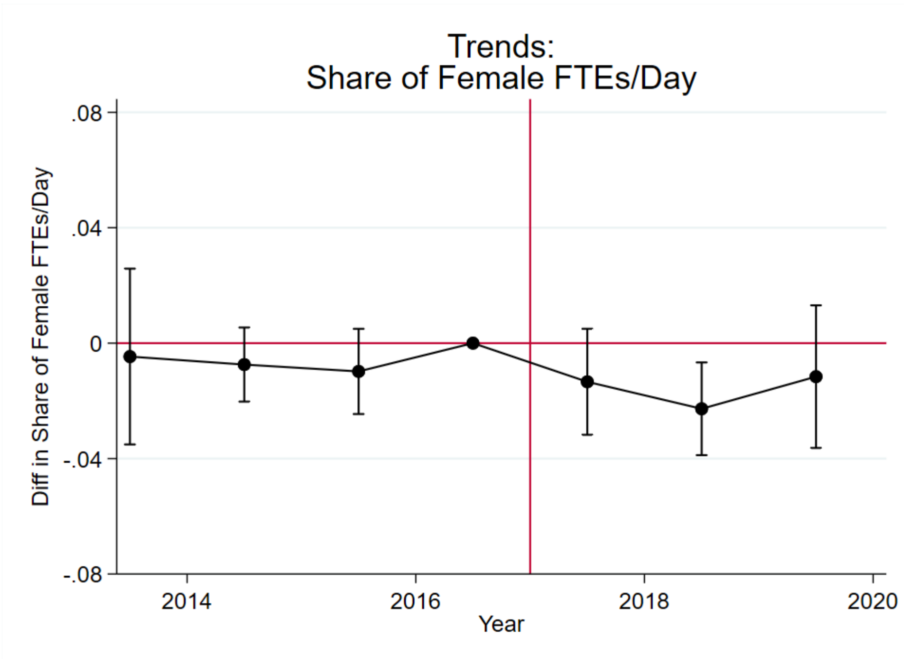

Table 6.1 gives us results from regression 1, and I see there is about 1-2 percentage point differential negative impact on the female FTE/day share, as a result of the policy. The coefficient remains significant, as I add more and more controls, across column 1-3. This shows that the policy had a negative impact on female FTE. Since, this FTE/day is a combination of both number of women employed, as well as the shifts worker per women in a day on average, this does not tell us where the decline can be attributed to. Thus, next I run my regression solely on the number of females employed to total employment share in figure 4 and table 6.2.

Again figure 4 shows no sign of pre-trends, confirming the validity of comparison groups. Table 6.2 gives me coefficient estimates very similar to the female FTE, indicating that the primary decline in female FTE is due to a reduction in number of female employees in the treated firms.

Table 6.1: Share of Female FTE/Day

VARIABLES	(1) Share of Female FTEs/Day	(2) Share of Female FTEs/Day	(3) Share of Female FTEs/Day
Treat10*Post	-0.021***	-0.012**	-0.011**
	[0.005]	[0.005]	[0.005]
Post	-0.002	-0.016**	-0.034***
	[0.004]	[0.006]	[0.009]
Treated 10	-0.087***	-0.067***	-0.068***
	[0.028]	[0.016]	[0.016]
Constant	0.436***	0.421***	0.479***
	[0.015]	[0.024]	[0.027]
Observations	124,719	124,719	124,719
Industry FE	No	Yes	Yes
State FE	No	Yes	Yes
Year FE	No	Yes	Yes
State*Year FE	No	No	Yes

Robust standard errors in brackets
Standard errors clustered at State Level
*** p<0.01, ** p<0.05, * p<0.1

Figure 4: Share of Female Employees

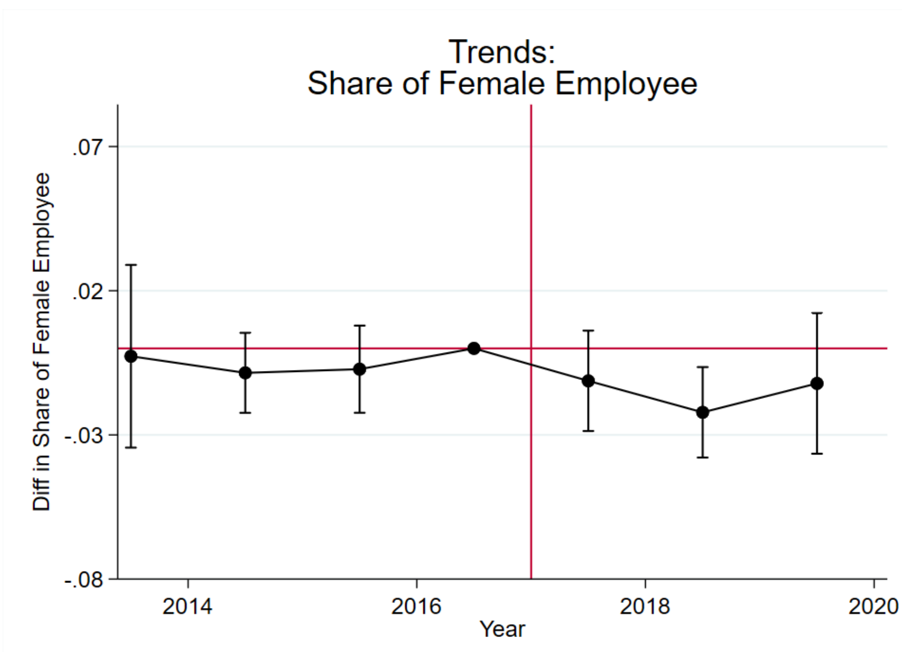

Table 6.2: Share of Female Employees

	(1)	(2)	(3)
VARIABLES	Share of Fem Employees	Share of Fem Employees	Share of Fem Employees
Treat10*Post	-0.021***	-0.012**	-0.011**
	[0.005]	[0.005]	[0.005]
Post	-0.002	-0.015***	-0.029
	[0.004]	[0.006]	[0.054]
Treated 10	-0.092***	-0.072***	-0.072***
	[0.028]	[0.004]	[0.004]
Constant	0.442***	0.427***	0.484***
	[0.014]	[0.013]	[0.041]
Observations	124,720	124,720	124,720
Industry FE	No	Yes	Yes
State FE	No	Yes	Yes
Year FE	No	Yes	Yes
State*Year FE	No	No	Yes

Robust standard errors in brackets
Standard errors clustered at State Level
*** p<0.01, ** p<0.05, * p<0.1

Since, most of the variation in the female FTE is already explained by the changes in number of female employees, we should not expect large changes in the number of shifts undertaken on average by each women. We see these results in Figure 5 and table 6.3. As expected there are no significant difference in the number of female shift shares due to the policy, confirming our intuition. I conclude that the policy resulted in decline in number of females employed, but given the employment shares post policy, there was no changes in work hours of women employed.

I then estimate the outcomes as number of female employees instead of shares relative to men to be able to understand the mechanism. The decline in the number of female shares could be because of the decline in female employees, or an increase in male employees/ total employees or a combination. In order to disentangle this, I estimate the Difference-in-Difference estimates for the number of female and total employees in tables 6.4 and 6.5 respectively. We see a negative coefficient on number of female employees, but they are not significant due to large standard errors.Similarly, we see insignificant, but mostly negative coefficient on the number of total employee outcome. This again, is suggestive of that the decline in female shares can be attributed to the firms experiencing decline in number of female employee, rather than any increase in total employees.

Now we move on to look at wage outcomes. Figure 6 and Table 6.6 look at the share of shifts paid to females. This includes the shifts for which the female might have worked for or was on a paid leave, but she received a payment. We not see any significant difference in the share of shifts females on average are paid for. We would had expected to see an increase in this outcome, due to the increase in paid leaves length. A potential

Figure 5: Share of Shifts/day/Female

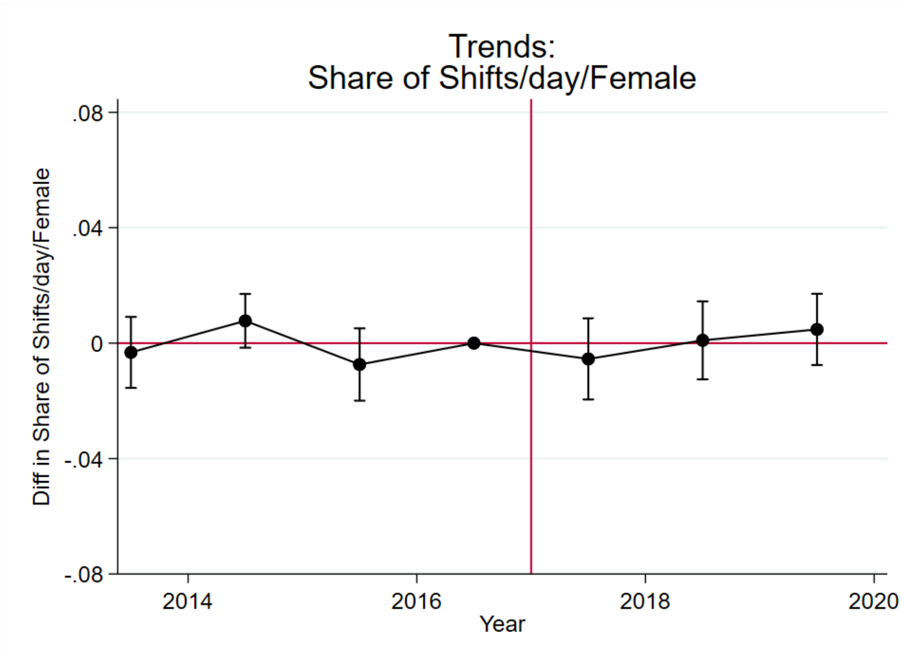

Table 6.3: Share of Shifts/day/Female

VARIABLES	(1) Share of Shifts/day/Female	(2) Share of Shifts/day/Female	(3) Share of Shifts/day/Female
Treat10*Post	0.000	0.001	-0.000
	[0.003]	[0.003]	[0.003]
Post	-0.001	-0.000	-0.015***
	[0.003]	[0.003]	[0.003]
Treated 10	0.012***	0.012**	0.013***
	[0.004]	[0.005]	[0.005]
Constant	0.979***	0.982***	0.981***
	[0.005]	[0.005]	[0.005]
Observations	124,719	124,719	124,719
Industry FE	No	Yes	Yes
State FE	No	Yes	Yes
Year FE	No	Yes	Yes
State*Year FE	No	No	Yes

Robust standard errors in brackets
Standard errors clustered at State Level
*** p<0.01, ** p<0.05, * p<0.1

Table 6.4: Number of Female Employees

VARIABLES	(1) # Female Employed	(2) # Female Employed	(3) # Female Employed
Treat10*Post	-4.734	-3.932	-4.987
	[6.583]	[5.626]	[8.128]
Post	-0.029	-15.034	19.630**
	[0.037]	[13.808]	[8.207]
Treated 10	75.888***	58.455***	58.997***
	[12.150]	[5.974]	[6.420]
Constant	2.138***	-15.188	-35.116**
	[0.096]	[20.752]	[15.602]
Observations	124,720	124,720	124,720
Industry FE	No	Yes	Yes
State FE	No	Yes	Yes
Year FE	No	Yes	Yes
State*Year FE	No	No	Yes

Robust standard errors in brackets
Standard errors clustered at State Level
*** p<0.01, ** p<0.05, * p<0.1

explanation for this is that, due to the simultaneous decline in female employees due to the policy, it could have been negated the impact of longer paid leaves, giving us an insignificant estimate.

In Figure 7 and Table 6.7, we now look at the female wage bill share, to capture differences in changes in payments and wages rates due to the policy. Figure 7 shows no signs of pre-trends. Table 6.7 shows a significant reduction in the female wage bill share from the policy. The wage bill is a combination of both the number of female employees and the wage rates paid to them, hence, next I break them down to measure the wage rate share per shift, as we already know that the number of female employed is falling due to the policy.

In table 6.8 I see no significant reduction in the wage rate shares of females, and if anything they are positive coefficients. Figure 8 also shows no signs of pre-trends. Hence, I can conclude that the firms do not reduce the wage rates paid to the female employed due to the policy, even though they do employ a fewer female share, which significantly reduces their wage bill. The firms likely use this saved wage bill to pay for the additional burden of paid maternity leaves for the remaining women workers they continue to employ.

Table 6.5: Number of Total Employees

VARIABLES	(1) Total Regular Employment	(2) Total Regular Employment	(3) Total Regular Employment
Treat10*Post	2.663	-2.929	-3.259
	[7.743]	[6.382]	[9.346]
Post	-0.026	-11.875	54.955***
	[0.065]	[14.695]	[10.325]
Treated 10	201.341***	149.760***	149.962***
	[11.489]	[7.755]	[7.427]
Constant	4.888***	43.737	36.020
	[0.112]	[26.499]	[26.009]
Observations	124,720	124,720	124,720
Industry FE	No	Yes	Yes
State FE	No	Yes	Yes
Year FE	No	Yes	Yes
State*Year FE	No	No	Yes

Robust standard errors in brackets
Standard errors clustered at State Level
*** p<0.01, ** p<0.05, * p<0.1

Figure 6: Share of Shifts Paid- Female

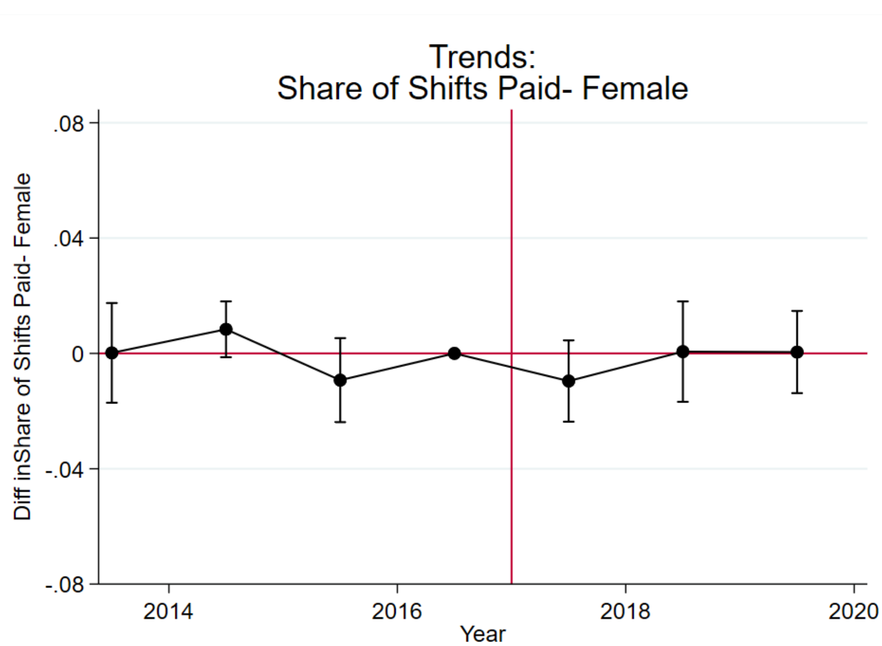

Table 6.6: Share of Shifts Paid- Female

VARIABLES	(1) Share of Shifts Paid- Female	(2) Share of Shifts Paid- Female	(3) Share of Shifts Paid- Female
Treat10*Post	-0.003	-0.002	-0.001
	[0.005]	[0.005]	[0.005]
Post	0.002	0.002	-0.004
	[0.004]	[0.004]	[0.004]
Treated 10	0.028***	0.025***	0.024***
	[0.007]	[0.007]	[0.006]
Constant	0.970***	0.972***	0.978***
	[0.008]	[0.008]	[0.007]
Observations	124,719	124,719	124,719
Industry FE	No	Yes	Yes
State FE	No	Yes	Yes
Year FE	No	Yes	Yes
State*Year FE	No	No	Yes

Robust standard errors in brackets
Standard errors clustered at State Level
*** p<0.01, ** p<0.05, * p<0.1

Figure 7: Share of Female Wage Bill

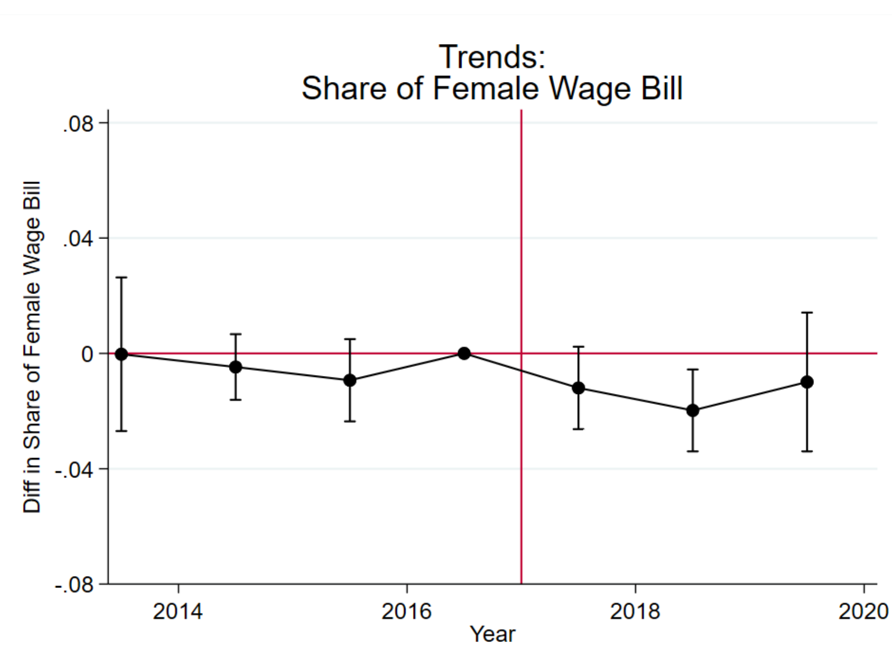

Table 6.7: Share of Female Wage Bill

VARIABLES	(1) Share of Female Wage Bill	(2) Share of Female Wage Bill	(3) Share of Female Wage Bill
Treat10*Post	-0.020***	-0.012**	-0.011**
	[0.006]	[0.005]	[0.005]
Post	0.001	-0.011*	-0.034***
	[0.004]	[0.006]	[0.008]
Treated 10	-0.068**	-0.053***	-0.054***
	[0.027]	[0.017]	[0.017]
Constant	0.393***	0.390***	0.455***
	[0.012]	[0.026]	[0.027]
Observations	124,720	124,720	124,720
Industry FE	No	Yes	Yes
State FE	No	Yes	Yes
Year FE	No	Yes	Yes
State*Year FE	No	No	Yes

Robust standard errors in brackets
Standard errors clustered at State Level
*** p<0.01, ** p<0.05, * p<0.1

Figure 8: Share of Female Hourly Wage Rate

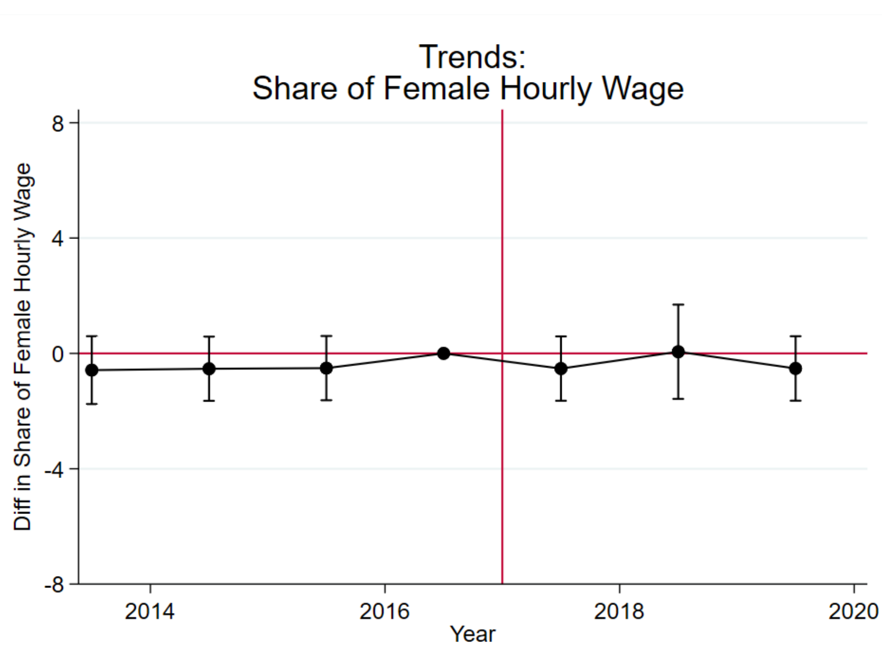

Table 6.8: Share of Female Hourly Wage Rate

	(1)	(2)	(3)
VARIABLES	Share of Female Wage Rate	Share of Female Wage Rate	Share of Female Wage Rate
Treat10*Post	0.193	0.157	0.045
	[0.321]	[0.306]	[0.198]
Post	-0.026	-0.252	-0.343
	[0.029]	[0.259]	[0.317]
Treated 10	0.173	-0.043	0.011
	[0.148]	[0.156]	[0.108]
Constant	0.939***	0.542*	0.680***
	[0.035]	[0.282]	[0.175]
Observations	124,720	124,720	124,720
Industry FE	No	Yes	Yes
State FE	No	Yes	Yes
Year FE	No	Yes	Yes
State*Year FE	No	No	Yes

Robust standard errors in brackets
Standard errors clustered at State Level
*** p<0.01, ** p<0.05, * p<0.1

6.1.1 Heterogeneity

Given these results, I further break down the mechanism, by comparing my results across two heterogeneous groups- urban vs rural. I use an indicator for urban, and interact with my coefficient of interest from regression 1. Table 6.9 gives me results from this regression. I also include indicators for urban and urban interacted with treat in the regression. I see a positive significant coefficient on the triple interaction term, shows us that the primary reduction in the female employee shares is happening more in rural firms by about 1 pp, when compared to their urban counterparts, due to the policy.

Table 6.9: Heterogeneity by Urban Vs Rural

VARIABLES	(1) Share of Female Employees	(2) Share of Female Employees	(3) Share of Female Employees
Treat10*Post*Urban	0.013***	0.007	0.008*
	[0.003]	[0.004]	[0.004]
Treat10*Post	-0.027***	-0.016***	-0.016***
	[0.007]	[0.005]	[0.005]
Post	-0.003	-0.015**	-0.032***
	[0.006]	[0.006]	[0.009]
Treated 10	-0.082***	-0.054**	-0.055***
	[0.006]	[0.020]	[0.020]
Constant	0.460***	0.428***	0.487***
	[0.005]	[0.020]	[0.023]
Observations	124,720	124,720	124,720
Industry FE	No	Yes	Yes
State FE	No	Yes	Yes
Year FE	No	Yes	Yes
State*Year FE	No	No	Yes

Includes Urban and Urban*Treat as Controls
Standard errors clustered at State Level
Standard errors in brackets
*** p<0.01, ** p<0.05, * p<0.1

6.2 Results from Individual-level Data

This section gives the results from the second estimation technique that uses the individual level analysis. The event study results shown in figure 9, do not show significant pre-trends in the data, making my treated control groups of women comparable. We also see significant reduction in the employment happening right after the policy is implemented in 2017, which remains persistent over the years. This also holds true in the Difference-in-Differences estimate in Table 6.10, which gives us an estimate of about 3 pp reduction in employment in the treated high exposed women, when compared to women who experience low exposure.

This is a fuzzy Difference-in-Differences, as we are comparing women, who are all exposed to the policy, but vary by the intensity of the policy exposure they may face based on their baseline industry employment. As expected, we do see a large reduction in female employment by 2-3 pp, due to higher exposure to the policy.

Figure 9: Event Study

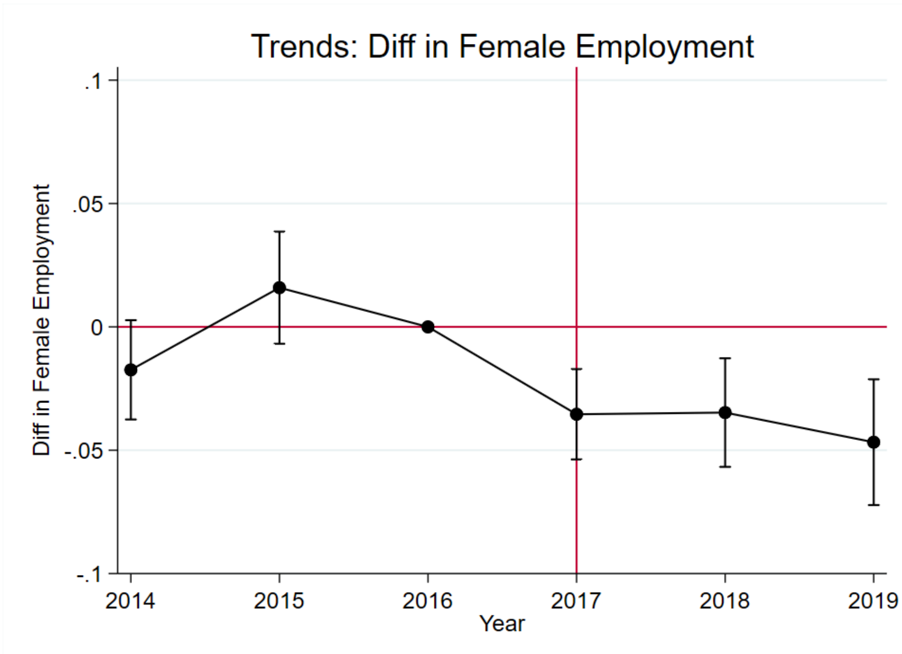

Table 6.10: DID regression results

VARIABLES	(1) Employed	(2) Employed	(3) Employed
interact	-0.035***	-0.033***	-0.018**
	[0.010]	[0.009]	[0.007]
post	-0.047***	-0.069***	-0.075***
	[0.011]	[0.008]	[0.007]
treat	0.039***		
	[0.007]		
Observations	691,379	691,379	691,379
Time FE	No	Yes	Yes
Ind FE	No	Yes	Yes
State*Time FE	No	No	Yes

Robust standard errors in brackets
Standard errors clustered at District Level
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

7 Robustness Checks

In order to check the robustness of the estimation methodology 1, especially to ensure that the results are not being driven by any differences in firm size causing selection, I look at the main outcome by restricting my sample to upto firm size of 20. This allows me to compare firms with size 1-9 with firms size 10-19. This gives me an equally distributed comparison group on both side of the threshold, which will be more comparable. Table 7.1 gives us these results, and I see that the female employee shares does decline significantly even then in my treated firms, confirming the robustness of my previous results. These results also hold, across even a smaller sample size, very narrow width around the threshold. This eliminates our identification concern of treatment -control groups being incomparable due to big size differences.

Table 7.1: Robustness across Small Sample- Firm size less than 20

	(1)	(2)	(3)
VARIABLES	Share of Female Employees	Share of Female Employees	Share of Female Employees
Treat10*Post	-0.014**	-0.012**	-0.009*
	[0.005]	[0.005]	[0.005]
Post	-0.002	-0.010*	0.060
	[0.004]	[0.006]	[0.095]
Treated 10	-0.061***	-0.046***	-0.048***
	[0.011]	[0.004]	[0.004]
Constant	0.442***	0.406***	0.443***
	[0.014]	[0.019]	[0.074]
Observations	33,458	33,458	33,458
Industry FE	No	Yes	Yes
State FE	No	Yes	Yes
Year FE	No	Yes	Yes
State*Year FE	No	No	Yes

Robust standard errors in brackets
*** p<0.01, ** p<0.05, * p<0.1

Furthermore, I also check for treatment effects at the threshold of firm size 50, which under the policy design, also need to providing crèche facilities and nursing breaks to the mothers, along with the longer leaves. In Table 6.2, gives the treatment effects, when comparing these big firms with 50+ persons to our control group of firms with less than 10 persons. We see that the results are not significant anymore. The coefficients are still negative, though small. This is potentially due to the fact that large firms, have a lot more resources, and might not be credit constrained in order to support women employee's longer maternity leaves. These results show us the the major part of reduction in female employees is coming from relatively smaller treated firms, who now have an added burden of longer paid leaves for their female employees and are unable to finance the added costs. This hints towards the labour demand factors at play, due to the policy, and firms might be resorting to discriminate based on gender in employment, because of the policy, as our negative impacts on female employment is mostly focused in relatively smaller treated firms, who are unable to finance themselves or transfer the additional leave cost burden.

Table 7.2: Comparison for Treatment at Firm Size larger than 50

	(1)	(2)	(3)
VARIABLES	Share of Female Employees	Share of Female Employees	Share of Female Employees
Treat50*Post	-0.021***	-0.008	-0.008
	[0.007]	[0.007]	[0.007]
Post	-0.001	-0.014	-0.071***
	[0.004]	[0.009]	[0.010]
Treated 50	-0.115***	-0.093***	-0.094***
	[0.037]	[0.021]	[0.020]
Constant	0.446***	0.439***	0.514***
	[0.012]	[0.029]	[0.028]
Observations	88,303	88,303	88,303
Industry FE	No	Yes	Yes
State FE	No	Yes	Yes
Year FE	No	Yes	Yes
State*Year FE	No	No	Yes

Robust standard errors in brackets
Standard errors clustered at State Level
*** p<0.01, ** p<0.05, * p<0.1

8 Conclusion

The well-intentioned policy of the government to increase female benefits during maternity, likely has negative labour market impact on the female labour. The policy causes about 1-2 percentage points decline in female employee shares, and the impact is higher for higher treatment exposure. There is no causal impact of the policy on wage rates, or work hours of women. These 2 pp decline might not seem to be a big negative impact, but when taken in the context of India, where the female labour force participation rate has been low and declining in the past few decades, this 0.5% decline in female employee share should be of concern.

This empirical evidence is suggestive of firm's apprehension in recruiting women, because of the policy, or female labour supply reduction leading to an overall fall in female employment compared to men. This policy in effect is changing the gender composition of employment structure in the formal sector, likely either driving women out of the formal economy into informal employment or completely towards unemployment or out-of-labor force.

In the absence of a complimentary paternal leave policy, the policy by design disincentives female formal employment, doing more harm than good to the female labor force of India.

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