

Doing More Harm Than Good?

Labor Market Consequences to Paid Maternity Leave Extension of India*

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Abstract

Can generous and seemingly 'progressive' paid maternity leaves lead to decrease in female employment? In this paper, I examine the effects from the extension of paid maternity leaves on the gendered labor market outcomes, in the context of India. The empirical analysis utilizes a two-fold estimation, first using firm-level data to measure difference in policy effects across genders. Second estimation measures reallocation of female-employment between industries, by varying degrees of policy-exposure, using a fuzzy difference-in-difference design. The results indicate that policy causes firms to substitute women out by their male-counterparts and they get reallocated to less-formal industries, effectively changing the gender composition of formal-employment. The analysis suggests that the policy worsened labor outcomes for women by deriving women into unpaid household-chores/informal jobs and unemployment, thereby fueling gender labor differences.

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

JEL classifications: J16, J18, J21, J23, J31, J71

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1 Introduction

Motherhood penalty in the labor market has been a topic of research for the past few decades. To minimize the negative impacts females have to face from this natural gendered filial shock of motherhood, policymakers have often resorted to well-intended maternity leave policies. Amid a recent spark in the global debate on parental leaves, more countries have recently adopted or amended their parental leave policies, birthing a growing volume of literature on the implications of parental leaves on the labor market. But are paid maternity leaves always good for the mothers in the labor market? Can generous and seemingly 'progressive' paid maternity leaves lead to unintended harmful consequences for the female employment? These questions are yet to be answered and the existing empirical studies on effects of paid maternity leave on female employment, provide mixed evidence across countries (Uribe et al. [2019]; Vu and Glewwe [2022]; Baum and Ruhm [2016]; Rossin-Slater et al. [2013]). Previous research finds contrasting impacts, primarily on account of varying policy structure, length of leaves per gender, underlying labor market dynamics, firm's ability to transfer additional costs from paid leaves, social norms defining gender roles in the country, etc. Del Rey et al. [2021] study this relation using a panel of 159 countries, and find ambiguous results from increasing leave duration on female labor 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 minimizing their costs by readjusting labor 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. These complex mechanisms that derive the effects of paid maternity leave policies on female labor outcomes, as highlighted in the existing literature, calls for context specific analysis of a policy in order to predict and measure the effect of such policies on the gendered employment in that economy, as the policy outcomes are an amalgamation of country-specific channels.

In this paper, I analyze the impact from generously increasing the paid maternity leaves on the labor market outcomes like employment and wages for all females. 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 labor market work. Sarkar et al. [2019] report evidence of increase in females exiting the labor 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 labor market outcomes unintended by the policymakers.

The Maternity Benefits (Amendment) Act of 2017 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. The Government of India intentions from the 2017 amendment of the policy was ensuring job security and greater financial support for mothers. However, in this seemingly progressive step of providing longer leaves of maternity, little is known about its implications on the employment and overall household decision-making dynamics. In the absence of any paternal leave, the policy has a potential to have negative repercussions on the female labor outcomes. Additionally, the policy also puts 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. Besides the societal barriers to mothers in India, the policy design of India's maternity leave legislation poses additional organizational barriers for working women. This unique setting makes a similar studies done in the context of other countries, externally invalid for the Indian maternity leave policy.

This paper empirically evaluates the impact of the paid maternity leave extension policy in Indian setting, estimating the causal impact on the female labor market outcomes like participation and wages. Taking advantage of a detailed, administrative annual survey of industries and a rich panel of individual survey data between 2014- 2019, I employ a comprehensive two-fold estimation to capture the direct treatment effect estimates on the treated firms, and measure substitution of employment between genders. I then 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.

I first analyze the 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 using a difference-in-difference design and estimate the changes in gendered employment and wage shares due to the policy. I perform an event study design to ensure absence of differential pre-trends and also conduct numerous validity and robustness checks to further strengthen the credibility of my research design. I find significant negative impact of the policy on the share of females employed of about 1-2 percentage points declines, but no impact on the share of hours worked. I also do not find any significant impact on the wage shares of females, compared to their male counterparts.

To disentangle the impacts from the policy across industries with different treatment exposure, I then utilize a large individual panel data to capture the variation in the impact on females based on their employment industry. I formally measure the policy-exposure for each industry, using a national labor-force survey data. I then employ a fuzzy difference-in-difference estimation design to compare treatment effects across industries by exposure to the policy. I find significantly larger decreases in employment for females, who were employed in highly treated industries, when compared to less-exposed industries. I conduct further decomposition exercise to measure redistribution of females across sectors and occupation categories. I find evidence of large significant industry reallocation for females who remain employed from high-exposed industries to low-exposed industries. The policy also significantly increases the likelihood to become home-makers or unoccupied for the unemployed females, with no changes in their willingness to work.

These findings hint towards a potential channel of firms optimizing their added costs from the policy, by replacing female employees with lower opportunity-cost male workers, but not engaging in gendered wage-discrimination. I do find some evidence for this channel as the reduction in female employee only takes place in relatively smaller firms, which face a larger share of cost burden, but disappears for firms with firm

size larger than 50 employees, as they are better able to absorb the added cost from maternity and are not credit-constrained. The results can also be potentially explained by a decline in female labor supply from the policy for instance due to wage replacement effects or changes in fertility decisions, intra-household unpaid-work redistribution among partners due to long paid leaves only for mothers and no leaves for fathers that can persist after the length of the leaves. The policy can also lead to detachment of the female employees from labor market due to longer leaves. We cannot determine which of these labor demand or labor supply channel is the primary driver of these impacts, as we can only observe the overall equilibrium effects of the policy. But the large increases in the likelihood of females exiting the labor market, redistribution of female work force towards informality with a greater probability of becoming unemployed or transitioning to homemaker roles, indicate the unintended negative consequence of the policy.

My study contributes to three strands of literature: First, it adds to the large evidence on motherhood penalty, and impact of parental leave policies on labor force participation reviewed by [Kleven et al. \[2024\]](#); [Olivetti and Petrongolo \[2017\]](#) among others. My paper makes several significant contributions to this existing literature. First, it adds to the very limited quantitative evidence on impact from expansion of family-leaves. My setting in context of India's recent policy amendment allows me to capture the pure effects from extending female-only leaves, in the absence of paternal leaves. My study closely relates to some cross-countries studies that discuss the length of the paid maternity leave is crucial in determining the effect on female labor outcomes ([Bates et al. \[2023\]](#) ; [Del Rey et al. \[2021\]](#); [Schönberg and Ludsteck \[2014\]](#)), but the documented evidence does not provide conclusive answer to the effectiveness of long maternity leaves for mothers' employment. Second, my paper provides causal evidence for effects from paid maternity leave policy for all females measuring the intention-to-treat (ITT) effects on the labor outcomes. The existing literature on parental leaves primarily focuses on the fertility and labor impacts only for the parents, but do not measure the spillover effects from mandatory family leave policies on all labor force. My estimation explores the potential general equilibrium outcomes including the spillover effects on the non-targeted groups by measuring female-male employment substitution, and not just the labor changes for mothers who actually take the maternity leave. This is crucial as the employers are likely to anticipate additional costs and respond by adjusting labor demand for all employees, as they cannot ex-ante identify which female employees will take the leaves. This comparison has not been previously studied and existing studies only look at the impacts on mothers or women of child-bearing age. Lastly, my paper adds specifically to the scant empirical evidence on implications for developing countries. 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. I complement to the within-country studies such as [Schönberg and Ludsteck \[2007\]](#); [Uribe et al. \[2019\]](#); [Vu and Glewwe \[2022\]](#). The contrasting evidence from these country-specific studies highlight the importance of country-specific analysis. My paper is one of the first few empirical estimations providing causal evidence in the context of India's 2017 maternity leave policy. The other research papers, in my knowledge, empirically studying the labor market effects of the policy in India include [Bose and Chatterjee \[2024\]](#) and [Banerjee et al. \[2022\]](#). My paper complements these studies, and by identifying the total economic cost of the policy, and not just the within firm response to the policy change. While [Bose and Chatterjee \[2024\]](#)'s paper examines how firms adjusted the nature of the contractual employment among working women, this paper focuses on the broader labor market consequences of the 2017 maternity leave expansion and measure the effect on employment probabilities and earnings. The latter 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 their estimates use low-fertility age women or men as their control group, who are not unaffected by this policy. The comparison makes it difficult to identify the treatment effects of the policy, as we cannot confirm the direction of the spillover effects on the control groups. My paper credibly identifies the magnitude of the policy’s impact, or at the very least provides a lower bound estimate, by comparing treated and untreated firms as well as conducting an across-industry comparison based on the intensity of treatment.

Second, from the empirical perspective, my paper adds to the growing methodological work on difference-in-difference designs (Goodman-Bacon [2021]; Borusyak et al. [2024]; Roth et al. [2023]) including fuzzy differences (De Chaisemartin and d’Haultfoeuille [2018]). The combined two-fold identification strategy along with numerous validity and robustness checks provides credible estimation for policies implemented nation-wide that do not have a clearly defined control group for comparison.

Lastly, my paper adds to the literature on India’s female labor force participation and provides insight on the mystery behind its low and declining female labor force participation rate. The literature uses various demographic and socioeconomic factors (Andres et al. [2017], Mehrotra and Parida [2017], Klasen and Pieters [2015]); structural changes in economic growth (Lahoti and Swaminathan [2016]); social and organizational constraints (Posso et al. [2019]) social norms (Costagliola [2021]); motherhood penalty (Deshpande and Singh [2021]) as reasons for explaining the low labor force participation of Indian women. My paper adds to this literature by providing evidence from the India’s maternity leave policy as the deriving force behind declines to female labor force participation and pushing women out towards informality and out-of-labor force and attempts to disentangle the labor demand changes from labor supply factors affected due the policy. I further add to the discussion on unintended consequences from gendered policies, implications of mandatory leaves and discriminatory labor reforms.

The rest of the paper is organized as follows- Section 2 describes the policy and institutional setting, section 3 provides an brief conceptual framework. Section 4 presents the data used followed by Section 5 develops the two empirical strategies used. Section 6 list the results and robustness checks. Section 7 provide some additional results, with final conclusion in section 8.

2 Policy Background

Following global trend, India too decided to amend its existing policy on paid maternity leaves and doubled the length of paid leaves for mothers in 2017. India’s landmark Maternity Benefits Act, 1961 ensured all women who worked atleast 80 days in the past year, employed in any organized establishment (firms who employ 10 or more workers), were entitled to 12 weeks of fully paid maternity leave benefit, in India. Recently, the Government of India formally amended this under the Maternity Benefits (Amendment) Act in 2017, 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 only 12 weeks of paid leaves for her third child onward, as before. 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 effects 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. This ensured widespread implementation of the policy across the country, and reports of increase in redressal filed in courts.

3 Conceptual Framework

3.1 Expected labor demand responses to the reform

Under the labor demand factors, the opportunity cost of a female employee rises by the direct additional cost from fully paid leaves, & indirect cost of finding, and training a temporary replacement worker. If the firms are unable to transfer these costs directly, there is a possibility of employers resorting to readjusting their labor demand for the high cost workers, in order to minimize their costs. The maternity leaves disproportionately increases the opportunity cost of an equally skilled female employee, if she is expected to take maternity leaves at some point. The added cost burden upon the employer is disproportional across genders, so the employer might resort to optimizing their cost of production from labor costs by decreasing hiring & retention, decreasing wages, withholding promotions, hiring at lower pay grade, worsened contract terms, etc. These are more probable for the gender with higher opportunity cost, thus potentially disproportionately worsening female labor demand by firms. Furthermore, this cost burden is more prominent among the smaller credit constrained firms, compared to larger firms who might be better able to absorb the added cost burden from the policy.

3.2 Expected labor supply responses to the reform

With the institutional changes from the maternity leave policy, can both increase or decrease labor demand. The longer leaves might help 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. This higher income and job security channel could potentially increase female labor supply ex-post. Another potential reason for improved labor supply could be better childcare options, as the policy design mandates crèche facilities in larger firms, making them attractive for working women.

On the other hand, there can also be some potential negative implications of the policy on female labor supply. The extended leaves of 6 months now, might make the mother detached from the labor market, and experience skills degradation, not able to return back to her job post maternity. Another negative impact to labor supply channel could be from intra-household substitution of unpaid care work and shift of bargaining power within members of the households from long leaves, that could persist after the leave duration. 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 within a household might decrease, further pushing females into unpaid homemakers roles.

These labor 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 labor demand and labor supply channels from the policy, I explore this aspect, by comparing the gendered labor outcomes on employment and wage ratios across treated and untreated establishments, before and after the policy. I then decompose the effects, by looking at changes in likelihood of becoming homemaker, and unemployed roles by the unemployed females due to the policy. My results hint towards a negative labor supply effect.

4 Data

This section describes the main sources of data used for the empirical analyses.

4.1 Firm Level Data

My main source of data for the firm level empirical analysis is a large annual cross-sectional firm survey data - **Annual Survey of Industries (ASI)** which is the primary source of industrial statistics for the government. The data includes a yearly sample of about 16,000 nationally representative firms from the organized manufacturing and mining industries in India. The data is surveyed taking an annual repeated cross-section sample of firms, and does not provide matchable firm-id across years for confidentiality reasons. This administrative data contains details on firm indicators on their employee, wages, capital, inputs used, output and financial statistics. Each firm is surveyed on their employee compositions and firm-industry performance measures. It collects data on different aspects of labor statistics, namely, working days, shifts worked, absenteeism, labor turnover, hours worked etc. My sample includes about 16,000 firms annually, surveyed over financial years 2013-2019 for over a total of over 100,000 observations.

4.2 Individual Level Data

My other primary source of data is 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 of individuals on demographics and labor market outcomes, etc. The survey conducted comprehensively thrice every year, at the level of individual households. My estimation sample contains a panel of all working age women, for years from 2014, which is when the data was first collected, and stops at financial year 2019-2020, to exclude any effects from the pandemic, for a total of over 600k observations.

I complement this individual panel with the **Employment-Unemployment Survey of India** dataset of 2012, statistics on formality and informality shares of female employment in order to create the industry exposure to the policy. This National sample Survey (NSS) data is a primary sources on various indicators of labor force at National and State levels. This government survey dataset on an all-India household survey on the subject of employment and unemployment in India, is a well suited source of statistics for capturing level of informality in each industry in India.

5 Empirical Methodology

Estimating the causal impact of the nationally implemented paid maternity leave extension policy on labor market outcomes, involves constructing an appropriate counterfactual for treatment. In this section, I will explain how I will evaluate this policy using two different empirical strategies to best estimate the causal impact of the maternity leave extension on the gendered labor market outcomes in India. First method uses a difference-in-difference strategy comparing treated-untreated formal firms. I use a firm-level data to capture the impact of the policy on gendered employment outcomes and wages. Since, this firm level data only captures the organized manufacturing industries of India, the design does not account for any the spillover effects from the policy across other sectors and industries. In order to understand the impact across all industries, I employ a second estimation methodology using a nationally represented individual-panel of women, to measure impact of the policy on based on industry exposure to the policy in a fuzzy difference-in-difference design. I explain both methods in detail below-

5.1 Estimation 1: Firm-level Comparison

For the first estimation strategy, I use a difference-in-difference design to compare labor 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-

$$Y_{it} = \beta_0 + \beta_1(Treat_{it} * Post_t) + \beta_2Treat_{it} + \lambda_{k(i)} + \gamma_{s(i)} + \alpha_t + \epsilon_{it} \quad (1)$$

where Y_{it} represents outcomes variables, such as female employment and wage shares for a firm i in year t .

$Post_t$ is defined as 1 for all financial years beginning April 2017 (policy implementation) and 0 otherwise while $Treat_{it}$ 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. 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. I also check robustness using $\chi_{s(i)t}$ state by year fixed effects. The state by time fixed effects $\chi_{s(i)t}$ ensures that the results are not driven by unobserved state-year shocks. I present robust standard errors for all regressions. The results are also robust to clustering at the state level.

5.1.1 Design validity

First, I check for parallel pre-treatment trends in my comparison groups, I estimate the event-study model design as follows-

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

where $Year_t$ is a dummy for each year between 2014 to 2019 excluding FY 2016 as base year, and $Treat_{it} * Year_t$ 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 labor market outcomes to be different across the two groups over time.

Among possible confounders to my estimation could 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 labor demand in smaller and larger firms differently. For instance, if smaller firms are more likely to be cash dependent, and adversely respond with labor demand than compared to larger firms. My estimation methodology does control for this, 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.

Unlike standard difference in difference model, where treatment is defined at baseline, before the policy, to compare outcomes across treated and control groups, my cross-sectional firm data does not allow for this. The firm size in any given year contemporaneously defines if the firm is within the treated or control group in each period. Therefore the policy provides the firms an opportunity to change the firm-size, causing the treatment manipulation to be a concern for the causal estimates. I employ specification validity tests using time-invarying firm characteristics, and numerous robustness and placebo tests around the cutoff, described in section 6.1.2 to ensure the two groups are not observationally different and are comparable for unbiased causal estimates.

5.1.2 Summary Statistics

I use the annual surveys for financial years 2013-2019 for the analysis. I utilize outcomes like gendered shares of employment and wage paid per firm from this dataset to compare across treated and untreated firms.

Table 1 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 Estimation 2: Individual-Level Comparison

For my second estimation design, I use a difference-in difference model, to compare the treatment effect on women, by the intensity of treatment, since there is no such control women whose exposure to the treatment does not change over time. 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. Using the 2012 Employment-Unemployment survey data, to measure the exposure indicator for each industry, based on the percentage of women employed in treated firms over total women employed for each industry, I calculate the Exposure index according to the firm distribution. I use 2012 as my baseline year, as it is the last labor force survey published by the government prior to 2017. 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:

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

Figure ?? illustrates this distribution of exposure across industries. There is a large variation in the treatment exposure, fairly uniformly distributed between 0 and 1. I 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:

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

I thus divide my individual panel of employed women into treated and control groups, based on their most-frequent (mode) industry of employment in the pre-period (2014-2016). Women who were not employed at all during the pre-period, are not included in my sample, as their treatment/control industry cannot be defined. I then estimate the following Fuzzy Difference-in-Differences regression based on [De Chaisemartin and d'Haultfoeuille \[2018\]](#), to compare the labor market outcomes of more treated vs control (less treated)

women, before and after the policy in 2017. The estimated model is as follows:

$$Y_{it} = \beta_0 + \beta_1(Treat_i * Post_t) + \beta_2Treat_i + \alpha_t + \gamma_i + \epsilon_{it} \quad (5)$$

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, interpreted as the percent point change in likelihood of being employed due to the policy, when comparing women who were employed in high- vs low-exposed industry at baseline. α_t represents the year effects, γ_i represents individual fixed effects, and also show robustness using state*year fixed effects. These state by time fixed effects ensures that these results are not driven by just state-year shocks. I cluster all standard errors at the industry level. I then, employ a decomposition exercises by evaluating the outcomes like industry reallocation for women who remain employed, and separately for differential occupation category outcomes, and willingness to work for women who become unemployed. This estimation helps identifying the mechanism and provide evidence for channels that potentially derive the results.

5.2.1 Design validity

One of the threats to validity of my difference-in-differences identification strategy is that the women employed in high exposure industries might not have the same outcomes as the ones in low exposure industries and would have followed parallel trends due to the differences across industry types, in the absence of the policy. As I cannot directly check for this identifying assumption, I look for parallel pre-treatment trend violations in my comparison groups, by evaluating the pre-period coefficients of estimation using an event-study model design as follows-

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

where $Year_t$ is a dummy for each year between 2014 to 2019 excluding FY 2016-2017 as base year, and $Treat_i * Year_t$ represents the treatment-time interaction terms, and β_{1t} are my coefficients of interest, which identifies the changes in outcome between treated and counterfactual women, relative to the same difference at base year prior to the policy. 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 any individual-level time invariant heterogeneity is controlled for in my analysis.

5.2.2 Summary Statistics

I use this to evaluate the patterns of female employment in India as a result of the policy for my second estimation design. My sample contains about 60,000 working-age females per year, in over 30 industry

categories, across all sectors. I use an yearly panel from 2014- 2019.

Table 2 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 this should not be concern, as the level differences does not pose a threat to my difference in difference empirical estimation. I also include individual level and geographic fixed effects in my regressions, to control for time and location-invarying heterogeneity.

6 Results

This section discusses the results from the regressions described by the two estimation strategies. First, 6.1 will discuss the results from Estimation 1 on firm data, followed by heterogeneity in 6.1.1 and robustness checks in 6.1.2. I will then discuss the results from Estimation Strategy 2 in 6.2.

6.1 Results from Firm Data

Female Employment. Figure 2 below reports the event-study coefficients from Equation 2, for Share of Female Full-Time Equivalent (FTEs) per day¹. I do not see any indication of pre-trends, as there are no significant differences in the pre-period estimates, which provides evidence towards in the validity of my research design. Table 3 reports the main regression results for the difference in difference estimation equation 1. Column 1 on Share of female FTE, significantly declines by about 1 percentage point, indicates a negative impact on the female FTE share, as a result of the policy. This shows that the policy led to a decline in female to total employee share of employment time. Since, this FTE per day is a combination of both number of workers employed multiplied by the shifts worked per worker in a day on average, I can further decompose my results into the impact of the policy on the number of employees and number of shifts worked on average.

Figure 3 shows regression results solely on the share of females employed, indicating no sign of pre-trends, confirming the validity of design. Column 2 in Table 3 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.

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 worker. We see the event study results in Figure 4 on Share of female shifts worked² and regression results in Column 3 of Table 3. As expected there are no significant difference in the share of female shifts worked 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.

¹Share of Female FTE per day is calculated as the ratio of average employment hours worked by all females in the firm to the average employment hours worked by all employees in the firm

²Defined as the ratio of shifts worked by females on average to the shifts worked by all workers on average

Figure 2 on Share of paid female shifts³ and regression results in Column 4 of Table 3. These shifts for which the female might have worked for or was on a paid leave, but she received a payment. We do not see any significant difference in the share of shifts females on average are paid for. We would have expected to see an increase in this outcome, due to the increase in paid leaves length. A potential explanation for this is that, due to the simultaneous decline in female employees due to the policy, could have negated the impact of longer paid leaves, giving us an insignificant estimate.

I also estimate the outcomes as number of female employees instead of shares relative to total employees to be able to understand the mechanism. The decline in the number of female shares could indicate either decline in female employees only, or increase in male employees or both causing substitution of female employees by male counterparts. In order to disentangle this, I estimate the difference-in-differences estimates for the number of female and total employees respectively. Appendix table ?? and table ?? shows that the results are driven by a decline in female employees larger than a decline in total employees, suggestive of female to male substitution of employment by firms.

Wages. Now we move on to look at wage outcomes. Figure 6 and column 1 in Table 4 look at the share of shifts paid to females. I now report results on the female wage bill share⁴, to capture differences in changes in payments and wages rates due to the policy. Figure 6 shows no signs of pre-trends. Table 4 Column 1 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.

Column 2 in Table 4 shows no significant reduction in the wage rate shares of females, and if anything they are positive coefficients. Figure 7 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. All of the results above are robust to adding state*time fixed effects as control to account for state*year shocks.

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 5 gives me results from this regression. I also include indicators for urban and urban interacted with treat and post 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. A potential explanation for larger decline in rural firms could be attributed to greater credit constraints for rural firms, and hence they are more likely to engage in gender-discriminatory employment to minimize their additional labor cost from the policy.

³Defined as the ratio of paid shifts worked by females on average to the paid shifts worked by all workers on average

⁴Defined as amount of wage bill paid to female employees as a ratio of total wage bill paid to all employees

6.1.2 Robustness

Small Sample. In order to check the robustness of the estimation strategy 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 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, making the comparison groups comparable in size. Table 6 Column 2 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 (4-15), as shown in Column 2 Table 7. This eliminates our identification concern of treatment-control groups being incomparable due to large size differences in firm.

Placebo Test. Furthermore, we could be concerned that the firms could engage in treatment manipulation by changing the contemporaneously defined firm-size. I conduct robustness checks by placing placebo cutoffs at firm size of 4, 6, 14 & 16. Absence of any significant difference across all 4 placebo treatment cutoffs verify the validity of my results. Table 8 gives us these results robustness, and we do not see any significance across any of the four alternate placebo cutoffs.

Specification Validity. I also perform specification validity tests across outcomes variables that are expected to be time-invariant characteristics of the firms, which should not have any significant difference across my two comparison groups. This is done to ensure that the outcomes are not driven by the cross-sectional nature of my data, rather the effect is specific to the firms response due to the policy. I use firm location- urban dummy and the total number of working days, shown in Table 9 as proxy of time-invariant firm characteristics. I do not find any significant difference, strengthening the validity of my results. All regression results shown are robust to clustering at the state level, and adding other controls such as state*year fixed effects.

6.2 Results from Individual-level Data

All the above results from estimation strategy confirm that the policy did result in a decline in female employment in the manufacturing sector. But there can be potential spillovers across sectors outside manufacturing sector, resulting in shift of female employment across other industries and into informal economy due to the policy. These effects cannot be captured in the previous estimation due to data restrictions. Hence, my second estimation using a nationally represented individual data on all employed females, perfectly complements my previous estimation, and captures the effects across all sectors and industries, including the informal economy. This section gives the results from the second estimation technique that uses the individual level data from regression 5 & 6. This fuzzy difference-in-differences design, compares outcomes for 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.

Women Employed. Column 1 & 2 Table 10 report the regression results from equation 5. In column 1, we see a negative coefficient on difference-in-differences estimate for the number of women employed outcome, which gives us an insignificant but negative 4 percentage point estimate of employment in the treated high exposed women, when compared to women who experience low exposure. The event study results shown in

figure 8, also show the downward trend in employment, but the estimates are insignificant. This shows that there is no overall significant reduction in the number of female employees due to the policy.

I then compare the likelihood of being employed within the high-treated industries⁵, regression results (Column 2 Table 10) report a large significant reduction of about 36 percentage points. The event study results in Figure 9 reflect no-pretends, providing evidence of women being driven out of employment in high-formality industries (high-exposed treated group) towards more the low-formality industries (less-exposed control group). These results can be interpreted as industry-reallocation of women’s employment, towards informality as a result of the policy.

6.2.1 Decomposition

These results look at the outcomes only for those unemployed for various outcomes to help us understand what channels do women take, when they become unemployed due to the policy.

Willingness to Work. The survey asked respondents, questions about their willingness to work if unemployed in year 2016. I measure the effect on this outcome due to the policy using 2016 as baseline. Results reported Column 3 of Table 10 have small but positive and significant coefficients, which tells us that the unemployed women, are not out-of-labor force yet, have increased willingness to work, but are unable to find jobs. Further in Figure 10, we see that there is an initial drop the willingness right after the policy is implemented, but gradually increases. This hints towards evidence that the results are being driven mostly by negative labor demand shocks as employers employ less women, and not a result of negative labor supply.

Home Maker. Column 4 of Table 10 shows large increases in likelihood of women becoming home-makers, again corroborating suggestive evidence of women being crowded out of paid employment towards unpaid care work and increase informality, resulting in worsened labor market outcomes for the women. Note that Figure 11 shows some indication of pre-trends in this data, so the estimates are only suggestive and should not be interpreted as causal evidence.

Unoccupied/Student. Column 5 & 6 Table 10 also show evidence of increase in women becoming unoccupied and slight reduction in likelihood of becoming students, as a result of the policy. Again, the event study results in Figure 13 & Figure 12 show existence of pre-trends, so we cannot necessarily interpret the results as a result of the policy, but they do provide us some evidence of potential negative impacts from the policy. We also see very small standard errors due to smaller sample size.

7 Additional Results

As discussed in section 2, the paid maternity leave policy, not only required firms to provide longer leaves, but also mandated larger firms with 50 or more employees under the policy design, to provide crèche facilities and nursing breaks to the mothers, along with the longer leaves. I check for the treatment effects at the threshold of firm size 50 using my first estimation strategy, to capture the impact from these additional clauses. Table

⁵Employment in high treatment industry is and mutually exclusive dummy defined as 1 if the female is employed in the industries with high exposure, and 0 if employed in low-exposure industry

11, 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 labor 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 12 compares firms with 10-49 employees with firms with 50 or more employees. We do not see any significant difference across the two groups in the treatment effects. The added benefits of the creche facilities did not lead to any significant changes to the female employment in the manufacturing firms

8 Conclusion

The well-intentioned policy of the government to increase female benefits during maternity, likely has negative labor market impact on the female labor. The policy causes about 1-2 percentage points decline in female employee shares, and along with industry- reallocation among the employment for women, deriving them away from formal jobs (high treatment exposure industries). There is no causal impact of the policy on wage rates, or work hours of women. These 1 pp decline might not seem to be a big negative impact, but when taken in the context of India, where the female labor 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 labor 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 driving women out of the formal economy into informal employment and also increasing unemployed and out-of-labor force women to share larger burden of unpaid-house work.

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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Tables

Table 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 (758.836)	4.927 (1.617)	200.929 (782.781)
Share of female Employees	0.350 (0.268)	0.432 (0.185)	0.345 (0.272)
Share of male Employees	0.650 (0.268)	0.568 (0.185)	0.655 (0.272)
Share of female FTEs per day	0.349 (0.269)	0.426 (0.193)	0.344 (0.273)
Share of male FTEs per day	0.651 (0.269)	0.574 (0.193)	0.656 (0.273)
Share of female workshifts per day	0.990 (0.077)	0.978 (0.138)	0.991 (0.071)
Share of male workshifts per day	1.000 (0.036)	1.010 (0.095)	0.999 (0.028)
Share of female workshifts(paid)	0.997 (0.288)	0.971 (0.149)	0.999 (0.295)
Share of male workshifts(paid)	1.003 (0.116)	1.018 (0.115)	1.002 (0.115)
Share of female Wage Bill	0.325 (0.262)	0.385 (0.192)	0.321 (0.266)
Share of male Wage Bill	0.675 (0.262)	0.615 (0.192)	0.679 (0.266)
Share of female WageRate	1.473 (64.126)	0.912 (0.275)	1.511 (66.282)
Share of male WageRate	1.081 (0.343)	1.089 (0.244)	1.080 (0.349)
Number of observations	16048	1027	15021

Notes: Firm-level data, Annual Survey of Industries data, FY 2016-2017(baseline). All statistics are calculated using sample of all firms who reported atleast 1 male and atleast 1 female employed as a regular worker. Control group includes firm size 1-9 and Treatment group includes firm size greater than 10. Column 1 reports statistics on the full sample, and columns 2 and 3 limit the sample to control and treated firms, respectively. Share refers as the ratio to total. FTE refers to full-time equivalent of work.

Table 2: Individual Characteristics

Variable	(1)		(2)		(3)	
	N	Mean/(SD)	N	Mean/(SD)	N	Mean/(SD)
Female	619176	1.000 (0.000)	411345	1.000 (0.000)	207831	1.000 (0.000)
Age	619176	41.008 (10.950)	411345	41.729 (10.875)	207831	39.581 (10.956)
Employed	619176	0.409 (0.492)	411345	0.399 (0.490)	207831	0.430 (0.495)
Employed in Treat Industry	253403	0.339 (0.473)	164079	0.037 (0.188)	89324	0.894 (0.308)
Home Maker	365773	0.908 (0.289)	247266	0.925 (0.263)	118507	0.873 (0.333)
Student	365773	0.039 (0.194)	247266	0.022 (0.147)	118507	0.075 (0.263)
Unoccupied	365773	0.008 (0.087)	247266	0.007 (0.081)	118507	0.010 (0.099)
Unemployed(Willing to work)	287036	0.062 (0.241)	192801	0.054 (0.227)	94235	0.078 (0.268)
Retired/Aged	365773	0.040 (0.197)	247266	0.042 (0.200)	118507	0.037 (0.189)
Married	107668	0.751 (0.432)	72092	0.764 (0.424)	35576	0.726 (0.446)
Has Mobile Phone	619176	0.566 (0.496)	411345	0.504 (0.500)	207831	0.688 (0.463)
Is Healthy	619049	0.973 (0.161)	411292	0.971 (0.167)	207757	0.978 (0.147)
Urban	619176	0.627 (0.484)	411345	0.523 (0.499)	207831	0.834 (0.372)

Notes: Individual-level data, Consumer Pyramids Household Survey, 2014-2019. All statistics are calculated using sample of working-age women. Control group includes women most-frequently employed in industries with low exposure to the policy before 2017, and treatment group includes women employed in high-exposure industries. Treat industry refers to high-policy exposed industries. Column 1 reports statistics on the full matched sample, and columns 2 and 3 limit the sample to control and treated groups respectively.

Table 3: Empirical Strategy I: Employment Outcomes

VARIABLES	(1) Share Fem FTE	(2) Share Fem Employee	(3) Share Fem shifts	(4) Share Fem shifts(paid)
Treat*Post	-0.010** [0.005]	-0.010** [0.004]	0.000 [0.003]	-0.003 [0.005]
Treated	-0.067*** [0.016]	-0.072*** [0.017]	0.012** [0.005]	0.024*** [0.007]
Constant	0.411*** [0.013]	0.417*** [0.014]	0.979*** [0.004]	0.975*** [0.006]
Observations	108,253	108,254	108,253	108,253
Industry FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
State*Year FE	No	No	No	No
Control Mean	0.345	0.346	0.991	0.996

*** p<0.01, ** p<0.05, * p<0.1

Notes: Firm-level data, Annual Survey of Industries data, FY 2013-2019. All statistics are calculated using sample of all firms who reported atleast 1 male and atleast 1 female employed as a regular worker. Control group includes firm size 1-9 and Treatment group includes firm size greater than 10. This table presents coefficients β_1 from Equation 1 for different outcomes Y_{it} . I define Share as female to total ratios for each outcome in column 1-4. The results in graph format are reported in Figure 2-5. Robust standard errors in brackets. Results are robust to State*Year controls.

Table 4: Empirical Strategy I: Wages

VARIABLES	(1) Share of female Wage Bill	(2) Share of female WageRate
Treat*Post	-0.010** [0.005]	0.067 [0.237]
Treated	-0.053*** [0.017]	0.003 [0.114]
Constant	0.375*** [0.014]	1.095*** [0.034]
Observations	108,254	108,254
Industry FE	Yes	Yes
State FE	Yes	Yes
Year FE	Yes	Yes
State*Year FE	No	No
Control Mean	0.321	1.127

*** p<0.01, ** p<0.05, * p<0.1

Notes: Firm-level data, Annual Survey of Industries data, FY 2013-2019. All statistics are calculated using sample of all firms who reported atleast 1 male and atleast 1 female employed as a regular worker. Control group includes firm size 1-9 and Treatment group includes firm size greater than 10. This table presents coefficients β_1 from Equation 1 for different outcomes Y_{it} . I define Share as female to total ratios for each outcome in column 1-2. The results in graph format are reported in Figure 6-7. Robust standard errors in brackets. Results are robust to State*Year Controls.

Table 5: Heterogeneity by Urban Vs Rural

VARIABLES	(1) Share of female Employees	(2) Share of female Employees	(3) Share of female Employees
Treat*Post*Urban	0.013* [0.007]	0.007* [0.004]	0.008* [0.004]
Treat*Post	-0.026*** [0.008]	-0.014*** [0.005]	-0.014*** [0.005]
Post	-0.004 [0.003]		
Treated	-0.082** [0.036]	-0.054** [0.020]	-0.055*** [0.020]
Constant	0.460*** [0.013]	0.430*** [0.021]	0.415*** [0.012]
Observations	108,259	108,259	108,254
Industry FE	No	Yes	Yes
State FE	No	Yes	No
Year FE	No	Yes	No
State*Year FE	No	No	Yes
Control Mean	0.346	0.346	0.346

*** p<0.01, ** p<0.05, * p<0.1

Notes: Firm-level data, Annual Survey of Industries data, FY 2013-2019. All statistics are calculated using sample of all firms who reported at least 1 male and at least 1 female employed as a regular worker. Control group includes firm size 1-9 and Treatment group includes firm size greater than 10. This table presents triple difference coefficients for outcome Y_{it} - Share of female to total employment, for robustness across different sets of controls in column 1-3. Urban is a location dummy. I also include Urban*post and urban*treat coefficients in the regression. Robust standard errors in brackets.

Table 6: Robustness across Small Sample: Firm size less than 20

VARIABLES	(1) Share of female Employees	(2) Share of female Employees	(3) Share of female Employees
Treat*Post	-0.013** [0.006]	-0.010*** [0.004]	-0.008* [0.004]
Post	-0.003 [0.003]		
Treated	-0.061*** [0.011]	-0.047*** [0.008]	-0.048*** [0.009]
Constant	0.442*** [0.014]	0.408*** [0.014]	0.442*** [0.019]
Observations	29,483	29,483	29,483
Industry FE	No	Yes	Yes
State FE	No	Yes	No
Year FE	No	Yes	No
State*Year FE	No	No	Yes
Control Mean	0.390	0.390	0.390

*** p<0.01, ** p<0.05, * p<0.1

Notes: Firm-level data, Annual Survey of Industries data, FY 2013-2019. All statistics are calculated using sample of all firms who reported atleast 1 male and atleast 1 female employed as a regular worker. This table presents coefficients β_1 from Equation 1 on outcome Y_{it} - Share of female to total employment, for robustness across different sets of controls in column 1-3. Treatment is defined as 0 if firm size is between 1-9 workers, and 1 if firm between 10 -20 workers. Robust standard errors in brackets.

Table 7: Robustness across Small Sample: Firm size between 4-9 vs 10-15

VARIABLES	(1) Share female Employees	(2) Share female Employees	(3) Share female Employees
Treat*Post	-0.013* [0.007]	-0.010* [0.005]	-0.009 [0.006]
Post	-0.002 [0.003]		
Treated	-0.048*** [0.009]	-0.035*** [0.007]	-0.036*** [0.007]
Constant	0.433*** [0.015]	0.403*** [0.015]	0.540*** [0.020]
Observations	21,894	21,894	21,894
Industry FE	No	Yes	Yes
State FE	No	Yes	No
Year FE	No	Yes	No
State*Year FE	No	No	Yes
Control Mean	0.392	0.392	0.392

Robust standard errors in brackets

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Notes: Firm-level data, Annual Survey of Industries data, FY 2013-2019. All statistics are calculated using sample of all firms who reported atleast 1 male and atleast 1 female employed as a regular worker. This table presents coefficients β_1 from Equation 1 on outcome Y_{it} - Share of female to total employment, for robustness across different sets of controls in column 1-3. Treatment is defined as 0 if firm size is between 4-9 workers, and 1 if firm between 10 -15 workers. Robust standard errors in brackets.

Table 8: Placebo Cutoffs

VARIABLES	(1) Share F Emp	(2) Share F Emp	(3) Share F Emp	(4) Share F Emp
Placebocutoff_4*Post	0.004 [0.009]			
Placebocutoff_6*Post		0.000 [0.009]		
Placebocutoff_14*Post			-0.003 [0.004]	
Placebocutoff_16*Post				0.000 [0.005]
Constant	0.484*** [0.013]	0.468*** [0.006]	0.400*** [0.010]	0.398*** [0.010]
Observations	6,693	6,693	101,536	101,536
Industry FE	Yes	Yes	Yes	Yes
State FE	No	No	No	No
Year FE	No	No	No	No
State*Year FE	Yes	Yes	Yes	Yes
Control Mean	0.441	0.441	0.339	0.339

*** p<0.01, ** p<0.05, * p<0.1

Notes: Firm-level data, Annual Survey of Industries data, FY 2013-2019. All statistics are calculated using sample of all firms who reported at least 1 male and at least 1 female employed as a regular worker. This table presents coefficients β_1 for outcome Y_{it} - Share of female to total employment, using placebo cutoff firm-size to define treatment. Placebo cutoffs (#) define treatment as 0 if firm-size is below (#) and 1 if firm-size is bigger than equal to (#). Robust standard errors in brackets.

Table 9: Specification Validity

VARIABLES	(1) No. of Working days	(2) Urban
Treat*Post	0.069 [1.473]	0.016 [0.012]
Treated	21.222*** [3.055]	-0.114*** [0.019]
Constant	278.757*** [3.096]	0.657*** [0.018]
Observations	108,254	108,254
Industry FE	Yes	Yes
State FE	Yes	Yes
Year FE	Yes	Yes
State*Year FE	No	No
Control Mean	298.7	0.557

*** p<0.01, ** p<0.05, * p<0.1

Notes: Firm-level data, Annual Survey of Industries data, FY 2013-2019. All statistics are calculated using sample of all firms who reported atleast 1 male and atleast 1 female employed as a regular worker. Control group includes firm size 1-9 and Treatment group includes firm size greater than 10. This table presents coefficients β_1 from Equation 1 for potentially time-invarying outcomes Y_{it} . Urban is defined as a 1 if the firm location is an urban area, and 0 if rural. No. of working days refers to the days per year on which the firm was manufacturing. Robust standard errors in brackets.

Table 10: Empirical Strategy II: Employment Outcomes

VARIABLES	(1) Employed	(2) Employed(Treat Ind)	(3) Willing work	(4) HomeMaker	(5) Unoccupied	(6) Student
Treat*Post	-0.049 [0.055]	-0.367*** [0.089]	0.009*** [0.002]	0.022*** [0.002]	0.005*** [0.000]	-0.017*** [0.000]
Post 2017	-0.064* [0.033]	0.097** [0.046]	-0.023*** [0.002]	-0.011*** [0.003]	-0.001*** [0.000]	0.007*** [0.000]
Constant	0.448** [0.179]	0.349*** [0.016]	0.075*** [0.002]	0.911*** [0.004]	0.007*** [0.000]	0.038*** [0.000]
Observations	619,176	243,792	284,113	363,636	363,636	363,636
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Ind FE	Yes	Yes	Yes	Yes	Yes	Yes
State*Time FE	No	No	No	No	No	No
Y Mean	0.409	0.339	0.0613	0.909	0.00762	0.0391

*** p<0.01, ** p<0.05, * p<0.1

Notes: Individual-level data, Consumer Pyramids Household Survey, 2014-2019. Control group includes women who are most-frequently employed in industries with low-exposure to the policy before March 2017, and treatment group includes women frequently employed in high-exposure industries pre-policy. This table presents coefficients β_1 from Equation 5 for different outcomes Y_{it} . Column 1 includes my full sample of working-age women, column 2 uses sample of only employed women, while column 3-6 uses sample of only unemployed women. Column 3 on willingness to work uses data for years starting 2016 when the survey question was added. The results in graph format are reported in Figure 8-13. Standard errors are reported in parentheses and are clustered at the industry level.

Table 11: Comparison for Treatment at Firm Size larger than 50

VARIABLES	(1) Share female Employees	(2) Share female Employees	(3) Share female Employees
Treat*Post(50+)	-0.018*** [0.006]	-0.006 [0.006]	-0.007 [0.006]
Post	-0.003 [0.003]		
Treated(50+)	-0.110*** [0.035]	-0.093*** [0.020]	-0.093*** [0.020]
Observations	77,735	77,735	77,728
Industry FE	No	Yes	Yes
State FE	No	Yes	No
Year FE	No	Yes	No
State*Year FE	No	No	Yes
Control Mean	0.332	0.332	0.332

*** p<0.01, ** p<0.05, * p<0.1

Notes: Firm-level data, Annual Survey of Industries data, FY 2013-2019. All statistics are calculated using sample of all firms who reported atleast 1 male and atleast 1 female employed as a regular worker. This table presents coefficients β_1 for outcome Y_{it} - Share of female to total employment, for robustness to different set of controls across columns 1-3. Treatment is defined as 1 if firm size is bigger than 50 workers and 0 if firm employs less than 10 workers. Robust standard errors in brackets.

Table 12: Comparison for Treatment at Firm Size larger than 50 with Treatment between 10-49

VARIABLES	(1) Share female Employees	(2) Share female Employees	(3) Share female Employees
Treated*Post(50+vs(10-49))	-0.002 [0.007]	0.003 [0.004]	0.002 [0.004]
Post	-0.019*** [0.004]		
Treated(50+vs(10-49))	-0.056** [0.023]	-0.051*** [0.014]	-0.051*** [0.014]
Observations	101,541	101,541	101,536
Industry FE	No	Yes	Yes
State FE	No	Yes	No
Year FE	No	Yes	No
State*Year FE	No	No	Yes
Control Mean	0.339	0.339	0.339

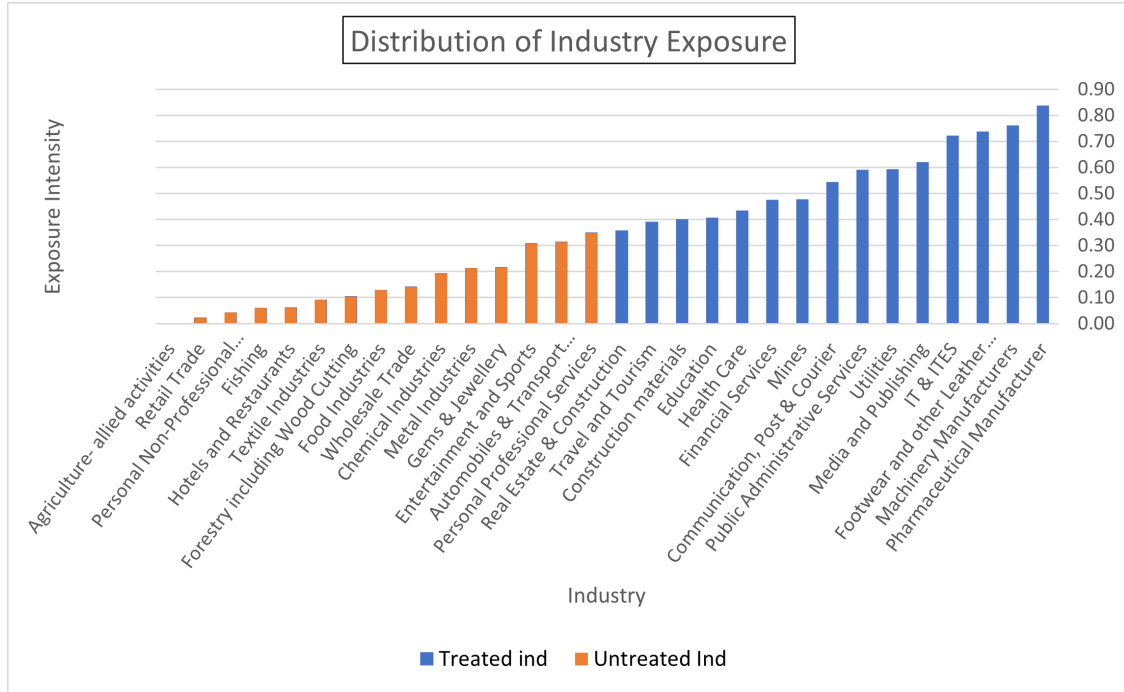
Robust standard errors in brackets

*** p<0.01, ** p<0.05, * p<0.1

Notes: Firm-level data, Annual Survey of Industries data, FY 2013-2019. All statistics are calculated using sample of all firms who reported atleast 1 male and atleast 1 female employed as a regular worker. This table presents coefficients β_1 for outcome Y_{it} - Share of female to total employment, for robustness to different set of controls across columns 1-3. Treatment is defined as 1 if firm size is bigger than 50 workers and 0 if firm employs between 10-49 workers. Robust standard errors in brackets.

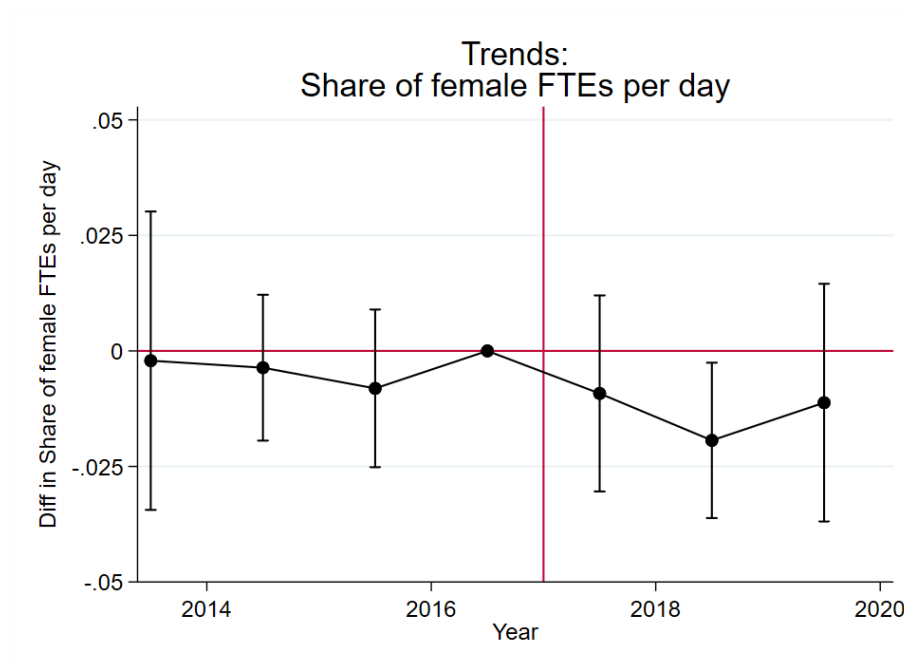
Figures

Figure 1: Exposure Index by Industry



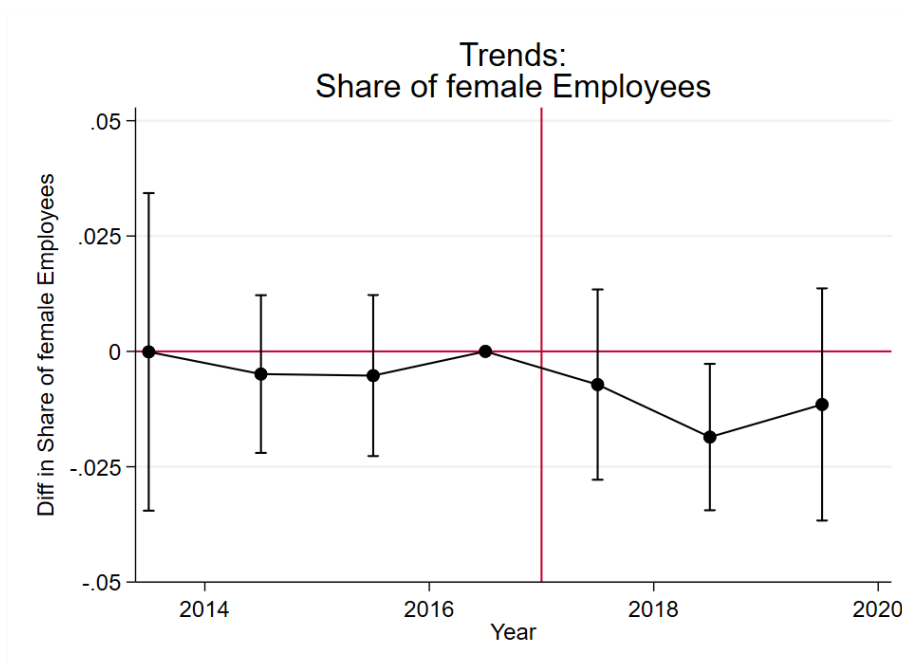
Employment-Unemployment Data 2012; Consumer Pyramids Household Survey, 2014-2019. Industry exposure to the policy calculated as the ratio of Number of females employed in treated firms to the total number of women employed for each industry, merged with the concurrent industry code in the CPHS data.

Figure 2: Empirical Strategy I: Share of Female FTEs per Day



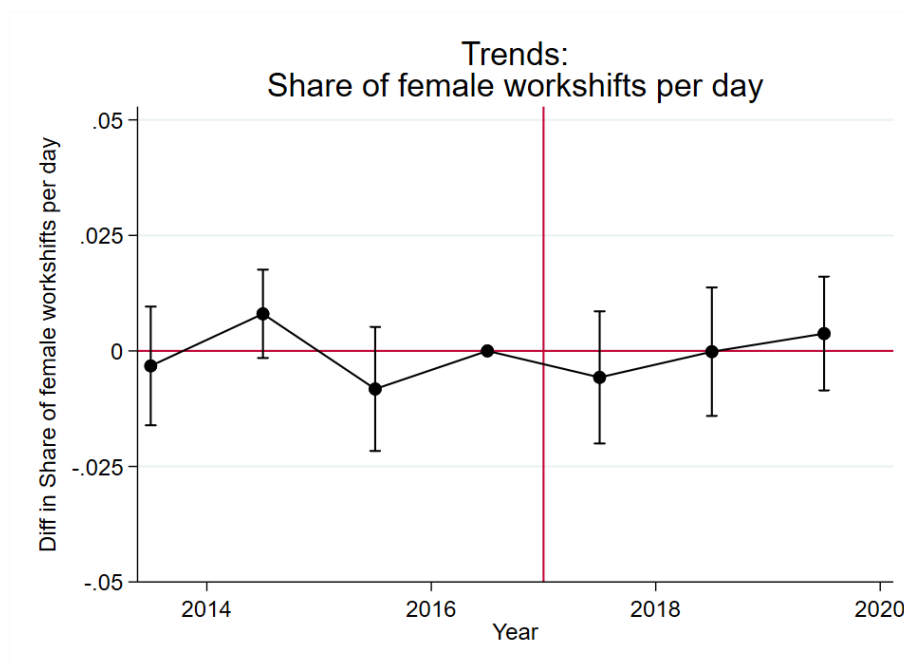
Firm-level data, Annual Survey of Industries data, FY 2013-2019. All statistics are calculated using sample of all firms who reported atleast 1 male and atleast 1 female employed as a regular worker. Control group includes firm size 1-9 and Treatment group includes firm size greater than 10. This figure presents coefficients β_{1t} from Equation 2 for outcomes Y_{it} - Share of Female FTE per day is calculated as the ratio of average employment hours worked by all females in the firm to the average employment hours worked by all employees in the firm.FY 2016-2017 is used as base year. Robust standard errors.

Figure 3: Empirical Strategy I: Share of Female Employees



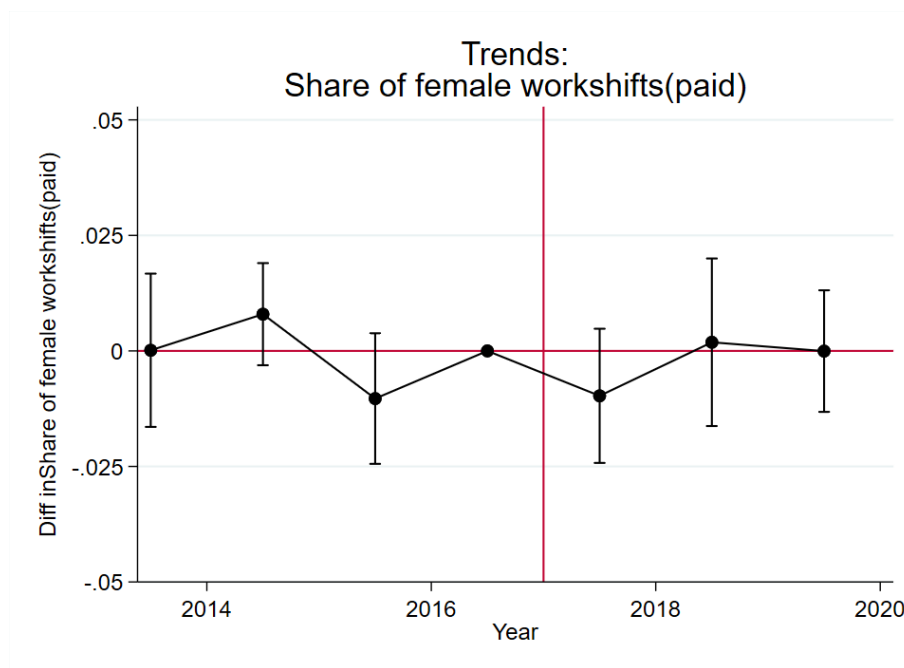
Firm-level data, Annual Survey of Industries data, FY 2013-2019. All statistics are calculated using sample of all firms who reported atleast 1 male and atleast 1 female employed as a regular worker. Control group includes firm size 1-9 and Treatment group includes firm size greater than 10. This figure presents coefficients β_{1t} from Equation 2 for outcomes Y_{it} - Share of Females Employees is calculated as the ratio of average number of women employed in the firm to the average number of employees in the firm.FY 2016-2017 is used as base year. Robust standard errors.

Figure 4: Empirical Strategy I: Share of Female workshifts per day



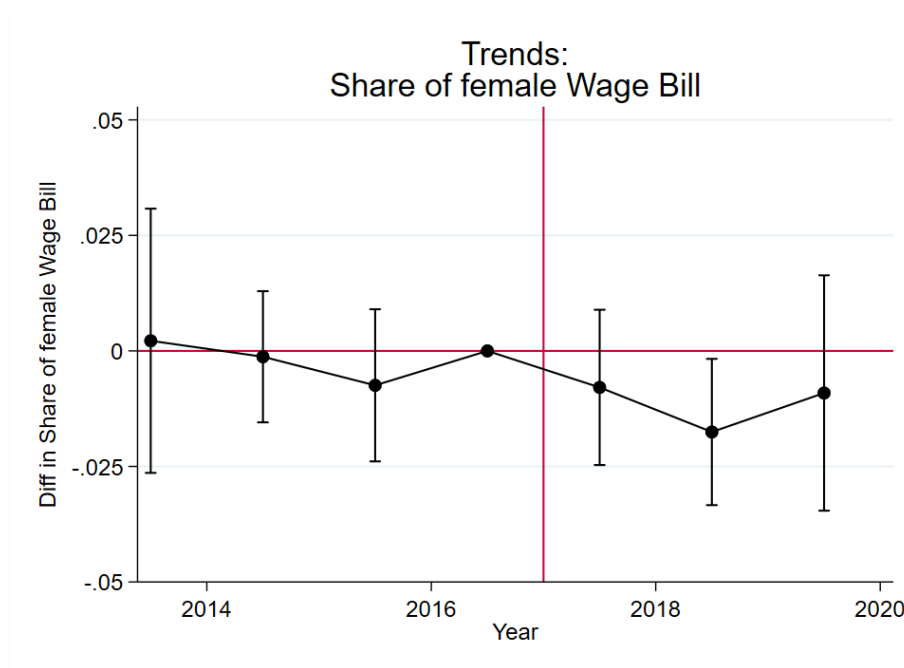
Firm-level data, Annual Survey of Industries data, FY 2013-2019. All statistics are calculated using sample of all firms who reported atleast 1 male and atleast 1 female employed as a regular worker. Control group includes firm size 1-9 and Treatment group includes firm size greater than 10. This figure presents coefficients β_{1t} from Equation 2 for outcomes Y_{it} - Share of Females work shifts is calculated as the ratio of average number of women work shifts in the firm to the average work shifts for all employees in the firm.FY 2016-2017 is used as base year. Robust standard errors.

Figure 5: Empirical Strategy I: Share of Female workshifts per day(paid)



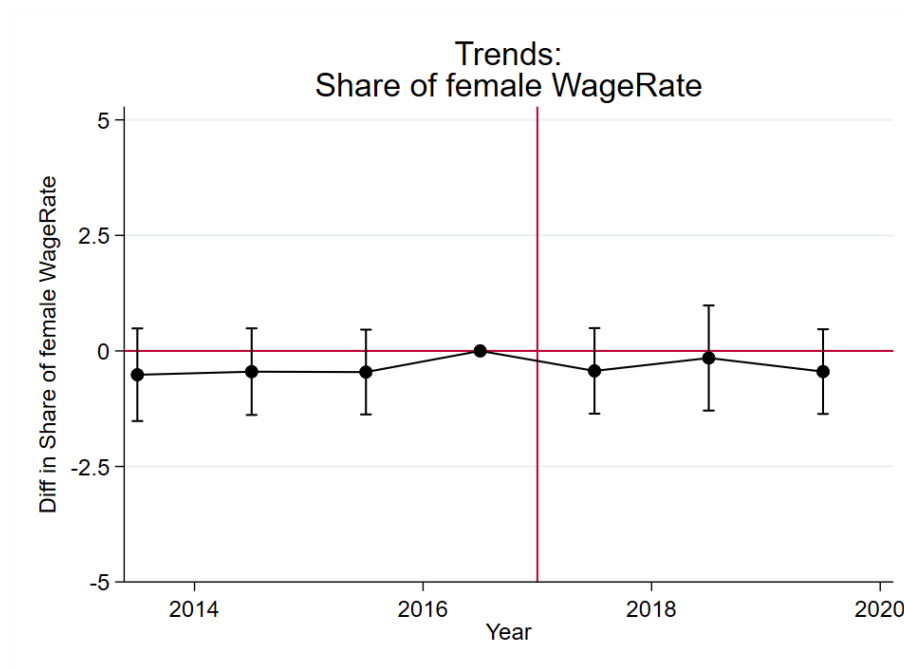
Firm-level data, Annual Survey of Industries data, FY 2013-2019. All statistics are calculated using sample of all firms who reported atleast 1 male and atleast 1 female employed as a regular worker. Control group includes firm size 1-9 and Treatment group includes firm size greater than 10. This figure presents coefficients β_{1t} from Equation 2 for outcomes Y_{it} - Share of Females work shifts(paid) is calculated as the ratio of average number of women paid work shifts in the firm to the average paid work shifts for all employees in the firm.FY 2016-2017 is used as base year. Robust standard errors.

Figure 6: Empirical Strategy I: Share of Female Wage Bill



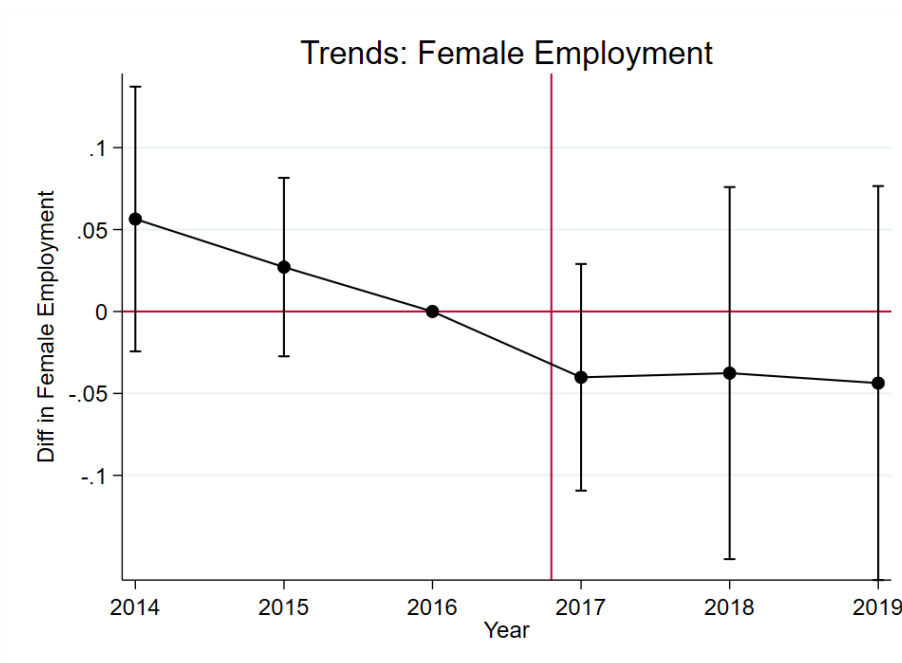
Firm-level data, Annual Survey of Industries data, FY 2013-2019. All statistics are calculated using sample of all firms who reported atleast 1 male and atleast 1 female employed as a regular worker. Control group includes firm size 1-9 and Treatment group includes firm size greater than 10. This figure presents coefficients β_{1t} from Equation 2 for outcomes Y_{it} - Share of Females Wage Bill is calculated as the ratio of average wage bill paid to women employees in the firm to the wage bill paid for all employees in the firm.FY 2016-2017 is used as base year. Robust standard errors.

Figure 7: Empirical Strategy I: Share of Female Wage Rate



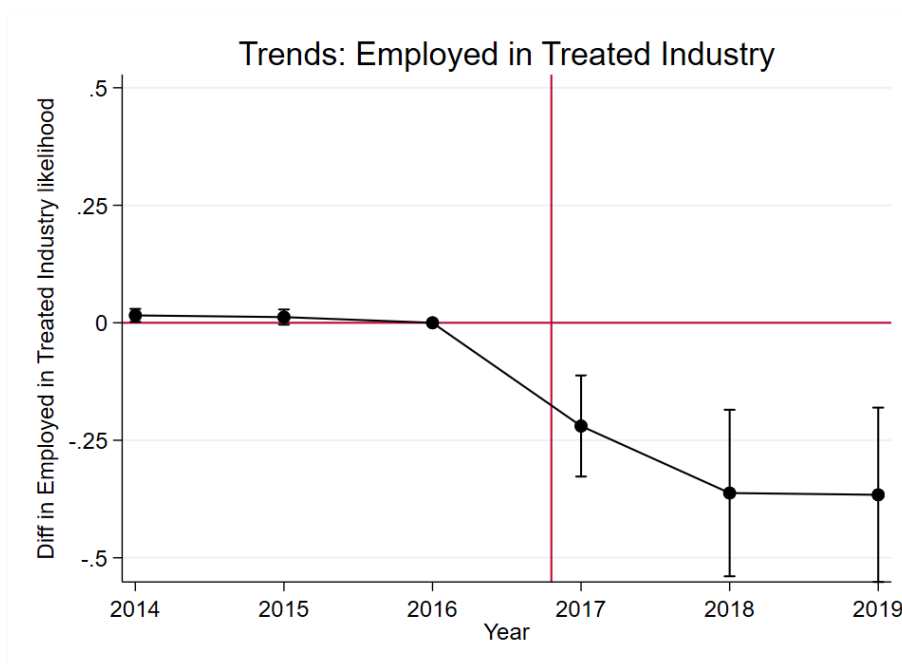
Firm-level data, Annual Survey of Industries data, FY 2013-2019. All statistics are calculated using sample of all firms who reported atleast 1 male and atleast 1 female employed as a regular worker. Control group includes firm size 1-9 and Treatment group includes firm size greater than 10. This figure presents coefficients β_{1t} from Equation 2 for outcomes Y_{it} - Share of Females Wage Rate is calculated as the ratio of average wage rate paid to women employees in the firm to the wage rate paid for all employees in the firm.FY 2016-2017 is used as base year. Robust standard errors.

Figure 8: Empirical Strategy II: Female Employment



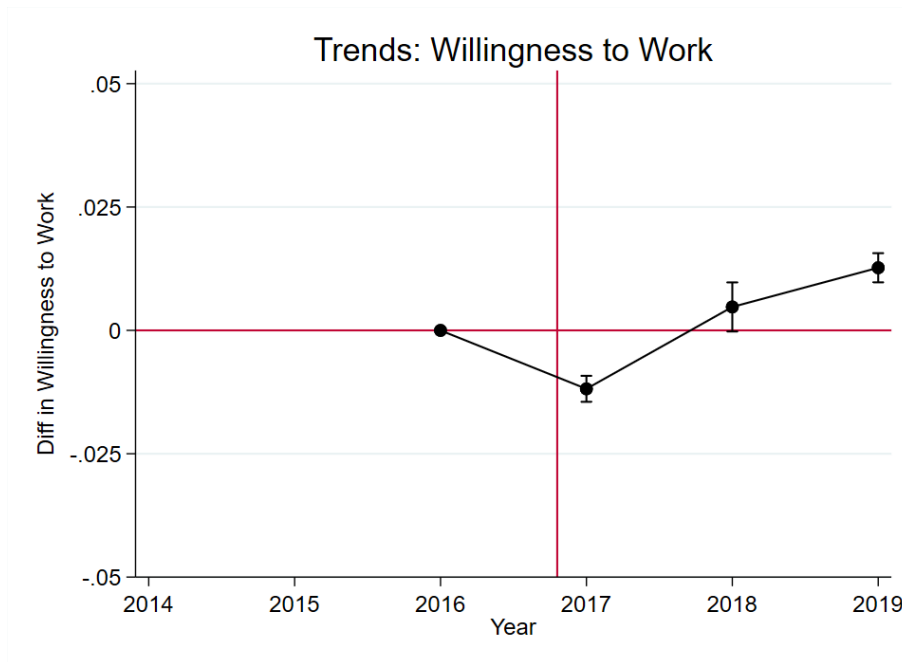
Individual-level data, Consumer Pyramids Household Survey, 2014-2019. Control group includes women who are most-frequently employed in industries with low-exposure to the policy before March 2017, and treatment group includes women frequently employed in high-exposure industries pre-policy. This table presents coefficients β_{1t} from Equation 6 for different outcomes Y_{it} . Female employment includes my full sample of working-age women. FY 2016-2017 is used as base year. Standard errors are reported are clustered at the industry level.

Figure 9: Empirical Strategy II: Employment in Treated Industries



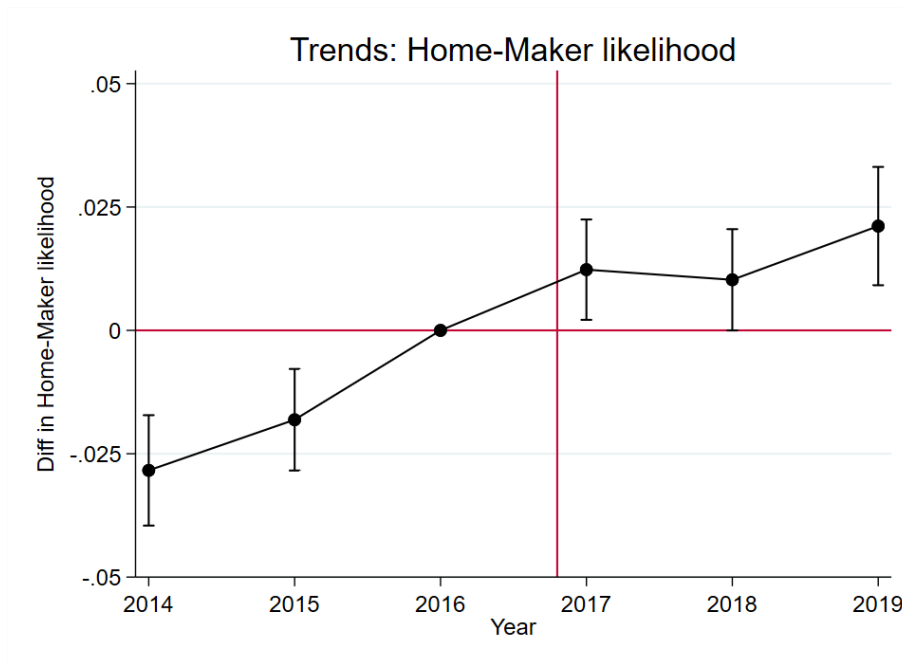
Individual-level data, Consumer Pyramids Household Survey, 2014-2019. Control group includes women who are most-frequently employed in industries with low-exposure to the policy before March 2017, and treatment group includes women frequently employed in high-exposure industries pre-policy. This table presents coefficients β_{1t} from Equation 6 for different outcomes Y_{it} . Female employed in treated industry uses sample of women employed in treated (high-exposure) industries. FY 2016-2017 is used as base year. Standard errors are reported are clustered at the industry level.

Figure 10: Empirical Strategy II: Willingness to Work



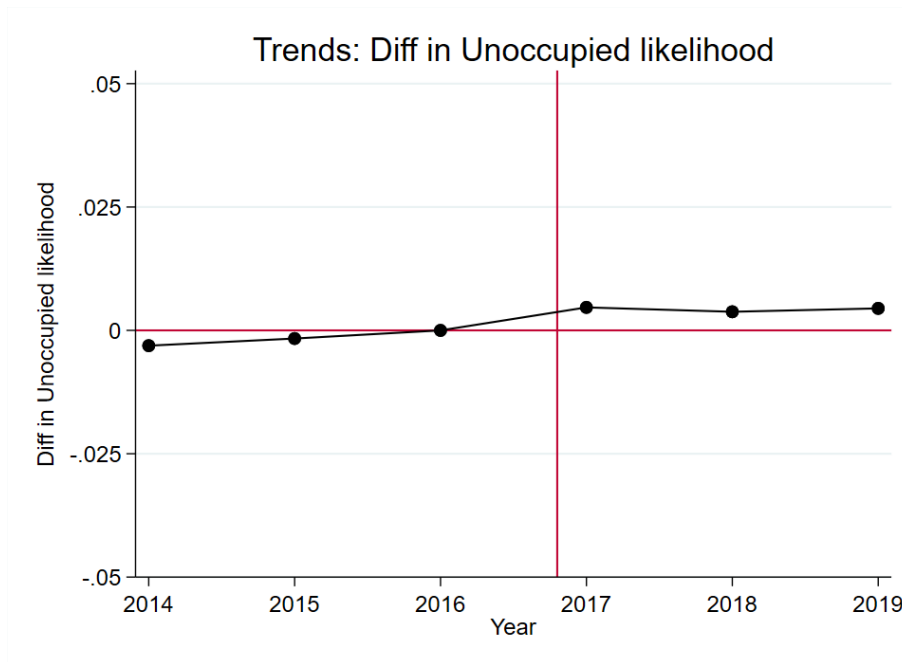
Individual-level data, Consumer Pyramids Household Survey, 2014-2019. Control group includes women who are most-frequently employed in industries with low-exposure to the policy before March 2017, and treatment group includes women frequently employed in high-exposure industries pre-policy. This table presents coefficients β_{1t} from Equation 6 for different outcomes Y_{it} . Willingness to work uses data for years starting 2016 when the survey question was added. FY 2016-2017 is used as base year. Standard errors are reported are clustered at the industry level.

Figure 11: Empirical Strategy II: Home Maker Likelihood



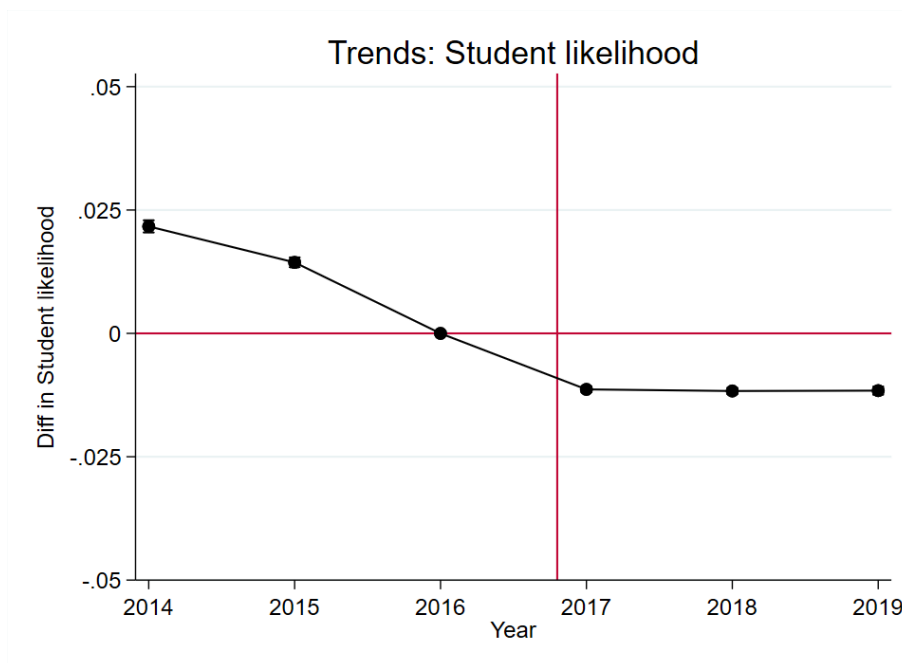
Individual-level data, Consumer Pyramids Household Survey, 2014-2019. Control group includes women who are most-frequently employed in industries with low-exposure to the policy before March 2017, and treatment group includes women frequently employed in high-exposure industries pre-policy. This table presents coefficients β_{1t} from Equation 6 for different outcomes Y_{it} using only women who reported to be unemployed and working as home-maker. FY 2016-2017 is used as base year. Standard errors are reported are clustered at the industry level.

Figure 12: Empirical Strategy II: Unemployed



Individual-level data, Consumer Pyramids Household Survey, 2014-2019. Control group includes women who are most-frequently employed in industries with low-exposure to the policy before March 2017, and treatment group includes women frequently employed in high-exposure industries pre-policy. This table presents coefficients β_{1t} from Equation 6 for different outcomes Y_{it} for who reported as unemployed and unoccupied, not looking for work. FY 2016-2017 is used as base year. Standard errors are reported are clustered at the industry level.

Figure 13: Empirical Strategy II: Student



Individual-level data, Consumer Pyramids Household Survey, 2014-2019. Control group includes women who are most-frequently employed in industries with low-exposure to the policy before March 2017, and treatment group includes women frequently employed in high-exposure industries pre-policy. This table presents coefficients β_{1t} from Equation 6 for different outcomes Y_{it} for women who reported as unemployed and students. FY 2016-2017 is used as base year. Standard errors are reported are clustered at the industry level.