Essays on Gender Inequality and Human Capital Formation

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Abstract

Chapter 1 analyzes the effect of intergenerational coresidence on women's labor supply decisions. Women's labor force participation is stymied by childcare and housework duties, as well as long-held social norms that restrict their autonomy and mobility in developing countries. A coresiding mother-in-law may restrict women's labor force participation as the custodian of gender-specific social norms, but may also help by taking on housework responsibilities. Using a nationally representative panel dataset from India, my coauthor and I use the exogenous variation in the mother-in-law's death to empirically investigate which effect dominates. We show that a mother-in-law's death reduces her daughter-in-law's labor force participation by 10 percent in an individual fixed-effects model. A placebo test reveals no effect of a coresiding father-in-law's death on his daughter-in-law's labor force participation, which alleviates concerns about demographic changes as the drivers of our results. Also, women with four or more children drive the effects of the mother-in-law's death. We provide suggestive evidence to show that by sharing the burden of household production tasks, coresiding mothers-in-law free up their daughter-in-law's time, which allows them to participate in the labor market. Overall, our results suggest that long-established gender roles that limit women's role as homemakers and caregivers play a critical role in shaping women's labor supply decisions in India.

Chapter 2 examines the unintended effects of making in-utero (prenatal) sex detection illegal. Ultrasound technology gives parents control over fertility and enables them to influence their children's gender composition through prenatal sex detection. To address declining female-to-male ratios, the Indian government put a legal ban on prenatal sex detection. A successful ban can increase the probability of a female

birth. However, in the absence of prenatal sex-detection techniques and the presence of strong son preferences, parents can respond by investing fewer resources in 'unwanted' girls they would have otherwise aborted. Using a difference-in-differences strategy, individual-level survey data, and the World Health Organization's z-score based measures for child health, I find that girls born after the ban are more likely to be stunted/malnourished compared to boys as a result of the ban. Also, the probability of stunting is significantly increasing in girls' age after the ban, suggesting that parents respond by investing fewer resources in girls. While helpful, existing papers only focus on the effect of prenatal sex detection on sex ratios and child mortality. This paper adds to the literature by examining the impact of prenatal sex detection on female child health and gender discrimination.

Chapter 3 examines the effects of communal violence on women's marital outcomes. Using individual-level survey data from India and a difference-in-differences approach, the study shows that the Hindu-Muslim riots in Gujarat in the year 2002 led to a decrease in women's age of marriage and an increase in the probability of getting married before the age of 18. However, event-study and synthetic controls methods suggest that the effects were not immediate and are prominent two years after the riots. Women married after the riots also had fewer years of education, and poorer social and economic status.

JEL Classifications: I15, J12, J16, J21, J22

Keywords: Female labor force participation, Family structure, Housework burden, Home production, Gender, Prenatal sex-detection, Gender discrimination, Ultrasound, Child health, Female labor, India

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Chapter 1

Reinforcing Gender Norms or Easing Housework Burdens? The Role of Mothers-in-Law in Determining Women's Labor Force Participation

Madhulika Khanna and Divya Pandey

1.1 Introduction

Gender disparities in labor market outcomes can be explained by long-established gender roles that put a disproportionately higher burden of household production tasks and childcare on women (Angrist and Evans 1998; Bertrand, Goldin, and Katz

2010; Field, Jayachandran, and Pande 2010; Alesina, Giuliano, and Nunn 2013; Adda, Dustmann, and Katrien 2017; Kleven, Landais, and Søgaard 2019). In South Asia, women's labor force participation is further inhibited by gender-specific social norms that restrict women's bargaining power and mobility (Jayachandran 2019; Anukriti et al. 2020). A coresiding mother-in-law (MIL) can affect the two forces in opposite directions by either helping her daughter-in-law (DIL) with housework or reinforcing restrictive social norms. Thus, it is essential that we understand the effect of a coresiding MIL on her DIL's employment decisions to inform any policy that challenges restrictive norms or lessens women's housework burden.

In a South Asian patriarchal-patrilocal joint family in which intergenerational coresidence is typical, the MIL heads the power hierarchy within the domain of the tasks and responsibilities allotted to women. MILs are assigned the role of guardians of social norms and family honor. Restrictive social norms are such that the family's honor and social status depend on women's demeanor, and their movement outside the home is stigmatized (Jayachandran 2015; Bernhardt et al. 2018). To ensure that DILs do not sully the family's image and adhere to their predefined roles in household chores, MILs restrict their bargaining power and mobility, and thereby their employment decisions (Eswaran, Ramaswami, and Wadhwa 2013; Gram et al. 2018; Anukriti et al. 2020). The massive audience for Indian soap operas that portray a complicated MIL/DIL love-hate relationship testifies to this relationship's relevance.[1]

In this paper, we first propose a conceptual framework that contains two countervailing channels through which a MIL's presence affects her DIL's decision to participate in the labor market. On the one hand, the MIL imposes constraints on

1. One of the most popular TV shows in this genre ran for close to a decade with almost 2000 episodes.

the DIL's autonomy and mobility and restricts her labor force participation. On the other hand, the MIL boosts the DIL's labor force participation by sharing her housework burden and loosening constraints on the DIL's time. Within this framework, the ultimate effect of the MIL's presence (or absence) is ambiguous and contingent on the relative sizes of the two countervailing effects. We base our empirical examination of the effect of the MIL's death on her DIL's labor force participation on the conceptual framework described above.

The primary challenge in identifying the MIL's effect on women's labor force participation is that the decision to live with the MIL is endogenous. For example, a woman who views a MIL as a potential barrier to her decision to participate in the labor market may choose not to live with her MIL. Unobserved omitted variables may jointly determine cohabitation between a woman and her MIL and her decision to participate in the labor market. To address this concern, we use a sample from a nationally representative household panel dataset in India in which the MIL coresides with the respondent DIL at baseline.

To isolate the causal effect of coresident MILs on women's labor force participation, we exploit the exogenous death of the MIL. We compare women's labor force participation over time across households in which the MIL dies versus lives, employing individual fixed effects. Thus, any time-invariant unobservable characteristics of women that are correlated with their decision to participate in the labor force and the MIL's death are absorbed by the fixed effects. To alleviate further concerns about the household's time-variant socioeconomic conditions that affect both the death of the MIL and the DIL's labor force participation, we conduct a placebo test to investigate the effects of the death of the coresident father-in-law (FIL).

This paper's key result is that the MIL's death reduces her DIL's labor force par-

ticipation by 4.6 percentage points, which is 10 percent of the labor force participation rate of women whose MILs did not die. The effect is primarily driven by women who lost their MILs most recently, which suggests that they adjust to the initial shock of the MIL's death over time. We do not find any effect of the MIL's death on her son's employment status or of the FIL's death on the DIL's employment status. These results demonstrate that compared with men, women's labor supply decisions are more responsive to adverse shocks that potentially increase the housework burden.

Having established that the MIL's death reduces women's labor force participation, we provide suggestive evidence that the increased housework burden described by our conceptual framework drives this result. First, we show that women whose MILs are dead are less likely to visit their natal homes often, which suggests that they have less residual time. Second, we show that the MIL's death increases the time women spend on household production tasks, such as collecting water and fuel. Consistent with the argument that it is the increased housework burden that reduces women's labor force participation, women with four or more children—who conceivably have more housework to attend to—drive the effect of the MIL's death.

Next, we show that the negative effect of the MIL's death on women's labor force participation is driven by relatively older (over age 30) DILs. This result is explained by the nature of social norms that are weaker for older women, and these women are more likely to be employed outside the home in the first place (Rahman and Rao 2004; Sarkar, Sahoo, and Klasen 2019). Even though our overall results suggest that the housework burden is a crucial factor in women's employment decisions, heterogeneous effects by age provide evidence on the role of restrictive social norms. Finally, we present and argue against several other mechanisms that could have driven the decrease in the DIL's labor force participation due to her MIL's death.

This paper contributes to several strands of the literature. First, it contributes to studies on the factors that influence gender disparities in the labor market as reviewed, for instance, by Bertrand (2011); Olivetti and Petrongolo (2016); and Blau and Kahn (2017). The literature has presented many explanations for the determinants of female labor force participation; for example, discrimination and human capital accumulation (Altonji and Blank 1999; Deshpande, Goel, and Khanna 2018); fertility and contraception (Goldin and Katz 2002; Bailey 2006; Bailey, Hershbein, and Miller 2012; Goldin 2014); technology (Greenwood, Seshadri, and Yorukoglu 2005; Dinkelman 2011); labor market opportunities and peer effects (Beaman et al. 2009; Jensen 2012; De Mel, McKenzie, and Woodruff 2014; Ghani, Kerr, and OConnell 2014; Field et al. 2016a); property rights and divorce laws (Voena 2015); systematic differences in psychological factors, such as risk preferences and competitiveness (Gneezy, Niederle, and Rustichini 2003; Niederle and Vesterlund 2007); family structure (Landmann, Seitz, and Steiner 2018); and social norms (Akerlof and Kranton 2000; Fernández and Fogli 2009). There is also a sizeable amount of work on the effect of fertility and motherhood on women's labor market performance (Angrist and Evans 1998; Agüero and Marks 2008; Bertrand, Goldin, and Katz 2010; Fernández-Kranz, Lacuesta, and Rodríguez-Planas 2013; Angelov, Johansson, and Lindahl 2016; Adda, Dustmann, and Katrien 2017; Kleven and Landais 2017; Kleven, Landais, and Søgaard 2019; Talamas 2020; Delecourt and Fitzpatrick 2020). We complement and advance this literature by showing that the interaction of social norms and housework burden can also impede womens labor force participation.

The study most closely related to ours is that of Talamas (2020), who shows that the death of a co-inhabiting MIL reduces mothers' employment rate by 25 percent due to its impact on childcare. We examine detailed mechanisms and shed light on the

relevance of household production chores, in addition to childcare, as a driving force that impedes women's employment. We also provide some suggestive evidence on the role of restrictive social norms by showing that older women with weaker mobility constraints experience a more considerable decline in their labor force participation rates after the MIL's death.

Second, this paper is related to the literature on the relevance of family structure and intrahousehold dynamics. The literature has examined the effects of family structure on schooling, gender gaps in education, child health, intimate-partner violence, non-farm employment, female autonomy, and fertility (Evans and Miguel 2007; Bertocchi and Bozzano 2016; Duflo 2003; Kaur 2017; Tur-Prats 2019; Dhanaraj and Mahambare 2019; Debnath 2015; Anukriti et al. 2020). Anukriti et al. (2020) show that in India, coresidence with the MIL is negatively correlated with her DIL's mobility and ability to form social connections outside the household. Our results suggest that even though the MIL's restrictive role is important in other contexts, it is not a binding constraint in women's decision to work. Consistent with our results, other studies also show that geographical proximity to or coresidence with parents or in-laws has a significant positive effect on married women's labor force participation in the U.S (Compton and Pollak 2014); Japan (Sasaki 2002); both rural and urban China (Guo et al. 2018; Maurer-Fazio et al. 2011; Ang, Chuanchuan, and Xiangting 2019); Austria (Frimmel et al. 2020) and Mexico (Talamas 2020). Our paper complements these studies by providing external validity for their results by showing that the MIL's death negatively affects her DIL's labor force participation in the Indian context. Additionally, we show that the increased burden of housework drives this finding.

Third, this paper contributes to the literature on the importance of cultural norms

or beliefs regarding appropriate gender-specific roles in determining women's status. Less equal gender norms, measured using reported gender-role attitudes and female employment, have their roots in the historical division of labor within the household (Alesina, Giuliano, and Nunn 2013; Hansen et al. 2015). Even today, enduring norms penalize women in the marriage market for making career-enhancing decisions that signal ambition and assertiveness (Fisman, Iyengar, and Simonson 2006; Bertrand, Kamenica, and Pan 2015; Bursztyn, Fujiwara, and Pallais 2017; Folke and Rickne 2020). Our findings reinforce the importance of gender roles defined by social norms in determining female employment: The MIL's death decreases her DIL's labor force participation, not her son's. Also, the patriarch's or the FIL's death does not affect his DIL's labor force participation. Since gendered norms cast women as more suited to or productive in domestic chores as caregivers and caretakers, they predominantly bear the household work burden. This increases disproportionately compared with men's burden in response to an adverse shock (death of the MIL in this case).

Finally, this paper's findings are especially relevant to India, where female employment rates remain low and have displayed a secular decline despite economic growth, educational gains, and a decline in fertility. The reduction in female labor force participation reflects both demand- and supply-side factors and has attracted substantial attention from researchers and policymakers (Bhalotra 1998; Klasen and Pieters 2015; Gupta 2015; Mehrotra and Parida 2017; Afridi, Dinkelman, and Mahajan 2018; Fletcher, Pande, and Moore 2018; Afridi, Monisankar, and Mahajan 2019; Deshpande and Kabeer 2019; Sarkar, Sahoo, and Klasen 2019). From a policy perspective, removing supply-side constraints that impede women from getting help for domestic work beyond childcare can improve women's labor force participation. Encouraging women's labor force participation can have additional benefits, because

paid employment and control over one's income improves the survival rates of girls, increases women's bargaining power, and relaxes gender norms against women's employment outside the home (Qian 2008; Carranza 2014; Field et al. 2016b).

The rest of the paper is organized as follows: Section 1.2 presents the conceptual framework that underlies the relationship between the death of the MIL and her DIL's labor force participation. Section 1.3 describes the data followed by a summary of the empirical strategy in Section 1.4. Section 1.5 presents the main results and discusses the placebo tests. Section 1.6 presents evidence that the main results are driven by an increase in workload for the DIL and discusses and rejects other mechanisms that could drive this result. Section 1.7 discusses the results and concludes.

1.2 Conceptual Framework

This section describes a simple static framework of women's labor supply decisions and their interaction with MIL's presence. Conceptually, there are two potential ways in which the MIL's presence affects the DIL's labor force participation: (1) by sharing her housework burden, and (2) by imposing restrictive gender norms. With this in mind, this framework explores how the MIL's presence (or absence) can affect her DIL's labor force participation. Next, we use this framework to delineate testable implications for the heterogeneous consequences of the MIL's death.

In this setup, DIL i divides her time endowment of one unit across three uses: leisure (R_i), housework (H_i), and labor (L_i). She has aggregate $\bar{H}_i$ units of housework to attend, and her MIL shares some given units of those responsibilities by contributing H_i^{MIL} units of work. Therefore, $H_i = \bar{H}_i - H_i^{MIL}$. We also assume that the DIL has to consume some minimum leisure, $\underline{R}$.

Her non-labor income is z. One can think of z as her share of household wealth or fixed income she receives from other family members. She earns a wage rate of w for each unit of labor work. We assume that the DIL does not save, and therefore, her private consumption, $C_i = wL_i + z$.

Finally, when the DIL works, she faces a norms-based utility cost, γ (Field et al. 2016b). Within this framework, γ represents the norms costs imposed on the DIL by her family, including her MIL. In the Indian context, social norms are such that a family's honor depends on women's behavior (Eswaran, Ramaswami, and Wadhwa 2013), and unconstrained movement of women outside the home is considered socially inappropriate. In this framework, norms costs reflect the social stigma of allowing women to work outside the home. And MILs play an especially critical role in guarding their families' honor by imposing gender norms in this context. We draw from studies that document a negative correlation between MIL's presence and the DIL's autonomy to argue that norms costs are more restrictive if the MIL is present in the household (see for instance: Jejeebhoy and Sathar 2001; Gram et al. 2018).

We posit that γ will also depend on the DIL's age and caste. While all women living with their MIL and working outside the home incur norms costs in this framework, this cost is likely to be much higher for younger women compared to older women. Social norms are less restrictive for older women, who face fewer mobility constraints, and thus, γ associated with their outside work would be lower (Rahman and Rao 2004). Norms costs are also likely to be lower for lower-caste (lower social-status) women, and these women are more likely to leave home for employment in the first place (Srinivas 1956; Eswaran, Ramaswami, and Wadhwa 2013).

Putting all this together, therefore, the DIL solves the following maximization problem in deciding whether to work:

$$\max_{L_i C_i} U(1 - \bar{H}_i + H_i^{Mil} - L_i, C_i) - \gamma 1(L_i > 0)$$

subject to

$$(1) wL_i + z = C \geq 0$$

$$(2) R_i \geq \underline{R}$$

Some useful conclusions can be gleaned from this simple framework. To choose whether to work, DILs will compare their utility when they pay the norms costs and choose an optimal amount of labor to their utility when they do not work and don't incur norms costs. Therefore, when the MIL dies, then the DIL's labor force participation will be affected through two key channels:

(a) *Increased Housework*: The DIL's labor force participation would decline as she has to allocate more time to household work. Since $H_i = \bar{H}_i - H_i^{MIL}$, and H_i^{MIL} becomes zero after the MIL's death, H_i must increase, and thus, L_i could decrease. We call this the *workload effect*.

(b) *Decreased Norms Costs*: The MIL's death also reduces γ, and increases her DIL's autonomy. The DIL's labor force participation could increase if γ decreases. We call this the *autonomy effect*.

In this setup, since there are two opposing effects, the overall effect of the MIL's death on the DIL's labor force participation would be determined by which effect is dominant.

Observation 1: *The overall effect of the MIL's death on the DIL's labor force participation is ambiguous and is a matter of empirical investigation.*

Next, women with more children would arguably have more housework responsibilities or a higher $\bar{H}_i$. After the MIL's death, these women would have to allocate more time to household work, and the *workload effect* is more likely to be larger than the *autonomy effect*.

Observation 2: *The decrease in the DIL's labor force participation after the death of the MIL, if present, would be more pronounced among women who have more children.*

As discussed above, older and lower caste women are likely to have lower norms costs (γ) to begin with. Therefore, the positive *autonomy effect* will likely be smaller for these women relative to the negative *workload effect*.

Observation 3: *The decrease in the DIL's labor force participation due to the death of the MIL, if present, would be driven by older women and lower caste women.*

To summarize, this framework shows while the net effect of the death of the MIL may be ambiguous, if it is negative, it should be more pronounced among women with more children, older women, and women from lower caste households. Moreover, suppose the *workload effect* is the predominant channel through which the death of the MIL affects her DIL's labor supply. In that case, we should expect to see an increase in DIL's housework responsibilities. We take observations one through three to data to understand the drivers of the DIL's labor supply and confirm the validity of our conceptual framework. While not tested formally, the framework outlined in this section provides an intuitive and parsimonious set up to interpret this paper's primary results.

1.3 Data

We use panel data from the Indian Human Development Survey (IHDS), a two-wave survey conducted in 2005 and 2012. The IHDS is a nationally-representative panel survey of 41,554 households across 1503 villages and 971 urban neighborhoods in India. The IHDS interviewed ever-married women between ages 15 and 49, randomly selected from each sampled household. For the bulk of the analysis, we build the analysis sample from data based on these interviews.

There are several advantages to using this nationally representative survey. First, it collects longitudinal data that allows us to track the same households and individuals across two survey rounds spanning seven years. Second, while we can identify the MIL/DIL pairs using the household roster, along with the two rounds of longitudinal data, tracking data across all members is publicly available, which allows us to identify in which cases the MIL died and not just moved away.[2] Finally, in addition to demographics and employment, this collected extensive data on themes relevant for this paper, including gender relations and time-use by gender.

To estimate the causal effect of living with the MIL on female labor force participation, we restrict the sample to 7,541 women living with their MIL in 2004-05. This restriction allows us to address the issue that the choice to live with in-laws is endogenous. More specifically, a woman's decision to live with her MIL will depend on her prior beliefs about the role a MIL would play in the household. For example, if a woman believes that her MIL could restrict her autonomy and ability to work, she may not want to live with her MIL. However, if she sees the MIL as a potential help with household work and childcare, she may prefer to live with her MIL. Table

2. Appendix 1.A describes the rules we followed to identify MIL/DIL pairs in detail.

1.7 describes the characteristics of the full sample of ever-married women in 2004-05 across two subgroups of women–those who lived with their MIL and those who did not live with their MIL. It suggests that women living with their MIL are different from those who do not live with their MIL in almost every regard. We try to address this concern by restricting our analysis sample to women living with their MILs at baseline.

The primary outcome that this paper focuses on is the labor force participation of the DIL. This is a binary outcome variable equal to one if the individual worked for more than 240 hours in the year preceding the interview and zero otherwise. There is no time criterion required for an individual to be considered a part of the labor force in the IHDS. Each respondent is asked about her contribution to the family business as well as any other income-generating activity.[3]

Table 1.1 describes the baseline characteristics of our restricted sample across two sub-groups: (1) women whose MILs are still in the household and (2) women whose MILs passed away between the two rounds of the IHDS, i.e., between 2004-05 and 2011-12. In this sample, 6,045 women were still living with their MILs, whereas 1,496 had lost their MILs by the second round. There are some expected differences between the two groups of women. Women whose MILs passed away were older, had more siblings, had greater economic autonomy, and had higher labor force participation at the baseline. The baseline difference in the labor force participation between the two groups could be because women whose MILs died are likely to be older and because older women face fewer mobility constraints, they are more likely to be employed, to begin with.

3. Klasen and Pieters (2015) show that estimates of female labor force participation from the IHDS are comparable to those from the National Sample Survey, data that have been typically used to study labor markets in India.

1.4 Empirical Strategy

This paper aims to isolate the effect of the MIL's presence on her DIL's labor force participation. As discussed in Section 1.3, the key empirical challenge in identifying this effect is that the decision to live with a MIL is endogenous. For instance, a woman who strongly prefers to work may choose to live with her MIL. We address this challenge by restricting the analysis sample to those women who were living with their MILs and could be linked to their MILs. This means that our results should be interpreted as internally valid for the sample of women who lived with their MILs at baseline.

Next, we use the variation in the coresidence due to the MIL's death during the 7-8 years between the two rounds of the IHDS. In the restricted sample, 20% of the women lost their MIL between the two survey rounds. Recall that the two groups of women differ along several characteristics (Table 1.1). These descriptive statistics suggest that the death of the MIL itself is a nonrandom event correlated with her DIL's labor force participation. Therefore, a difference-in-differences strategy, without additional controls, may not identify an unbiased effect of the MIL's death. In this case, one would want to control carefully for these baseline characteristics to make inferences about the impact of the MIL's death. Still, unobservables correlated with MIL's death, like the household's attitude towards women, may bias results since even after observables are controlled for, her death might not be random.

Our identification strategy addresses these concerns by combining the variation in the MIL's death with individual fixed effects. Individual fixed effects de facto account for anything that may differ about those women, their households, and their communities, whose MILs passed away between two survey rounds. This approach is

very similar to Evans and Miguel (2007), who use child fixed effects to estimate the effect of a parent's death on schooling in Kenya.

To summarize, our identification strategy uses a combination of difference-in-differences design with individual fixed effects. The first difference comes from comparing women's employment status before and after the MIL's death. The second difference comes from comparing women's employment status across households where the MIL passed away with those where she did not. The key identifying assumption is that any relative shift in the employment status of women living with MILs is attributable to the MIL's death.

Our main specification is:

$$y_{it} = \beta_0 + \beta_1 MILdied_i * Post_{it} + \beta_2 Post_{it} + \delta_i + X'_{it}\gamma + \epsilon_{it} \tag{4.1}$$

where y_{it} is the employment status, time use, or a measure of woman's autonomy i in survey round t. $MILdied_i$ is an indicator equal to one if woman i's MIL died between two rounds of the IHDS, and $Post_{it}$ is an indicator equal to one when we consider data from the second survey round. X_{it} is a vector of time-varying woman-specific characteristics, including the total number of comparable assets and number of children. Finally, δ_i denote individual fixed effects. ϵ_{it} is a conditionally-mean-zero error term. Standard errors are clustered at the level of the primary sampling unit. β_1 is the difference-in-differences estimator that captures the effect of MIL's death on her DIL's labor supply.

In theory, it is possible that the effect of the MIL's death accumulates over time or decreases if coping mechanisms materialize over time. To examine such effects, we also estimate a version of specification 4.1 by replacing the $Post_{it}$ with years since MIL's death dummies at the time of the second round of interviews. This specification

allows us to assess the effect of the MIL's death on the DIL's employment status by years passed since death.

Finally, for further exploratory analysis, we estimate the effect of the death of the MIL for different sub-groups of women. The specification for such analysis is:

$$y_{it} = \beta_0 + \sum_h \beta_{1h} MILdied_i * Post_{it} * H_i^h + \beta_2 Post_{it} + \delta_i + X_{it}'\gamma + \epsilon_{it} \tag{4.2}$$

where all variables are defined as in equation 4.1 and H_i^h is an indicator equal to one if individual i belongs to the sub-group h. Finally, β_{1h} is the effect of the MIL's death on group h's labor supply.

1.5 Results

This section starts by documenting the key result of this paper: MIL's death decreases her DIL's labor supply. We then discuss results by sectors and placebo tests and provide relevant robustness checks. We conclude this section by addressing the different levels of labor force participation at the baseline.

1.5.1 Effect on Labor Force Participation

Figure 1.1 plots the labor force participation rate for those women who lost their MILs between the rounds of the IHDS and those who did not. Three striking patterns are clear from this figure. First, women's labor force participation consistently declined over seven years between the rounds of the IHDS survey. Second, the labor force participation for those who eventually lost their MILs was always higher than those who did not. Last, the decline in labor force participation for women who lost

their MILs was steeper, which is essentially the paper's main result.

Table 1.2 describes our main set of results: Column (1) presents the results using equation 4.1, and Column (2) presents results that vary by the length of time that has elapsed since the MIL's death using equation 4.2. Table 1.2 demonstrates that the death of the MIL decreases her DIL's labor supply by 4.6 percentage points. Compared to the labor supply rate among women whose MILs did not pass away between the two survey rounds, this effect denotes a 10 percent reduction in labor supply. Our results are statistically significant at the 5 percent level. Further, this effect is driven by women who lost their MIL in the one year before the second round of the interview, which shows that the effect documented in Column (1) is the strongest right after the MIL's death (Figure 1.2).

A critical caveat is that while this result is internally valid for the sample of women who lived with their MILs in 2004-05, we cannot comment on the results' validity for the rest of the sample. In 2004-05, 30 percent of the interviewed women lived with their MILs (1.7). It is worth mentioning that the labor force participation of women who lived with their MILs at the baseline was lower than others. If we expect this pattern to persist, we should ex-ante expect that the MIL's death would increase the female labor force participation, and any negative effect is underestimated.

To better understand the economic significance of our results, we compare the magnitude of our estimates to other studies analyzing the determinants of female labor force participation. Bailey (2006) showed that the introduction of birth control pills increased female labor force participation by 3 percentage points in the US. According to Dinkelman (2011), electrification raised female employment rates by 9 percentage points in South Africa. In the Indian context, providing recruiting services and daycare programs increased women's employment rates by 2.4 and 2.6 percentage

points (Jensen 2012; Nandi et al. 2020). It is worth noting that our estimated effect of a 4.6 percentage point decline in women's employment rate due to the MIL's death lies within the range of estimates found by other studies.

1.5.2 Effect on Labor Supply by Sector

Next, we explore which sector is driving the effect documented in Table 1.2. To do so, we use an indicator of participation in a particular sector as the dependent variable. The decrease in farm work participation almost entirely drove the decline in DIL's labor supply (Table 1.3). This result is in line with the already documented evidence on the recent decrease in female labor force participation in India, where the largest decline is driven by the agricultural sector (Afridi, Dinkelman, and Mahajan 2018).

1.5.3 Placebo Tests

We present the following placebo tests to support the validity of the effect of the MIL's death on her DIL's employment rate described in Table 1.2.

Father-in-law and Spouse. The first test uses a placebo *treatment*–the death of the FIL. Column (1) in Table 1.8 shows that unlike the case of the MIL, FIL's death between the two survey rounds decreases women's labor supply only by 1.1 percentage points and is statistically insignificant.[4] The second test uses the spouses' labor supply as the outcome to show that the death of the MIL does not significantly

4. As was the case while estimating the effect of the MIL's death, the sample is restricted to those households where the FIL was a coresident at the baseline.

affect the labor supply of her son (Column (2), Table 1.8).[5] The first test result suggests that the FIL does not share the daughter-in-law's household work burden, and hence, his death does not affect her labor force participation. The second test explains that negative shocks that increase the burden of household chores affect women's labor force participation but not their spouses' labor force participation. These patterns highlight the importance of gender roles in impeding female labor force participation.

Permutation Test. Next, we use a placebo treatment assignment as a falsification exercise based on a simulation exercise that confirms that the key results are not due to pure chance (Athey and Imbens 2017). We falsely assigned the MIL's death status to a randomly selected set of observations many times to re-estimate the effect of the placebo treatment on the DIL's labor force participation.[6] This exercise creates a reference distribution of the treatment estimate and the associated t-statistic that would arise if the null hypothesis of no treatment effect was true. Reassuringly, according to Figure 1.6, the placebo estimates are centered around zero, and only 2.35 percent of the replicated estimates reach the estimate of -4.6 percentage point. The p-value recovered from randomization inference is 0.043, which is very similar to what we describe in Table 1.2 (0.041).

5. We use the labor supply status of spouses of women in the restricted sample. However, all women in this sample could not be matched to their spouses in the roster, giving us a smaller sample for this analysis.

6. We repeat this exercise 2,000 times. Young (2019) shows that typically 2,000 replications suffice to recover the non-parametric distribution of estimated effect size under placebo treatment.

1.5.4 Difference in Labour Force Participation at the Baseline

Recall that women who lost their MILs were more likely to be working at the baseline (Table 1.1). While we account for the difference in the proclivity to work at the baseline by including individual fixed effects, this strategy does not address the concern that the two groups might have had different trends in the labor force participation had one group not lost their MILs. To confirm that this is unlikely to be confounding our key results, we look at the differences in the characteristics of those women who lost their coresident FIL between the two survey rounds with those who did not. As was the case for MIL's death, women who lost their FILs had higher labor force participation at the baseline (Table 1.9). However, we do not see any effect of FIL's death on female labor force participation (Table 1.8).

1.6 Mechanisms

In this section, we study whether the mechanisms underlying the main results are consistent with the conceptual framework described in Section 1.2, and are evident because of a (relatively) large *workload effect*.

1.6.1 Heterogeneous Effects

We start by examining heterogeneity in the effect of the MIL's death across multiple margins and confirming whether the patterns of heterogeneous impacts are consistent with predictions from the framework. This exercise is also relevant to highlight that the average effects mask a substantial heterogeneity across sub-groups.

Observation 2 argues that the workload effect would dominate for women with more children. To test this argument empirically, we estimate equation 4.2 by the number of children in the second survey round.[7] In line with *Observation 2*, we find that the effect is monotonically increasing in the number of children in the second survey round and only statistically significant in case the woman had four or more children (Column (1), Table 1.4; Figure 1.3). Next, we focus on *Observation 3*, which noted that since the positive autonomy effect would be lower for older women and lower caste women, the key result should be more pronounced among these groups. Column (1) in Table 1.5 describes the key result by age categories.[8] The negative effect of the death of the MIL is monotonically increasing in DIL's age, and statistically significant only if the DIL is over 30 (Figure 1.4). Note that this pattern clarifies that the key result of this paper is not contrary to Anukriti et al. (2020)'s result that MILs restrict their DILs mobility because their analysis sample was restricted to 18-30-year-women. In line with *Observation 3*, the decrease in the DIL's labor force participation is marginally higher for lower caste women compared to higher caste women in the analysis sample. However, the difference in the effects on the two groups is not statistically significant (Column 2, Table 1.5; Figure 1.5).

Finally, we estimate equation 4.2 by baseline estimates of wealth on an asset-based index. The effect of the MIL's death, by wealth categories, could go in either

7. The following categories are used: (1) less than or equal to two children, (2) three children, (3) four children, and (4) five or more children. These cutoffs represent 49.5%, 27.6%, 13.5%, and 9.4% of the analysis sample. Since the number of children is discrete, we chose a categorization of the number of children such that the sample is the most evenly split while still capturing the intensity of housework. While these results are robust to including the number of children as a continuous dimension of heterogeneity, we use this specification for the ease of interpretation.

8. Following categories based on the baseline age are used: below 25, between 26 and 30, between 31 and 35, between 36 and 40, and between 41 and 49. These cutoffs represent 37.3%, 24.9%, 17.6%, 12.4%, and 7.7% of the analysis sample. While these results are robust to including the age as a continuous dimension of heterogeneity, we use this specification for the ease of interpretation.

direction. In comparison to poorer women, richer women may absorb adverse shocks of increased housework burden due to the MIL's death by hiring domestic help and thus maintaining their employment status. However, due to less financial constraints, richer women may be able to substitute outside employment for housework after the MIL's death, and their labor supply would be more responsive to this shock. We find that the difference in the effects on the top 50% in terms of wealth compared to the bottom 50% is not statistically different from zero (Column 2, Table 1.16; Figure 1.9).

1.6.2 Other Outcomes

If the negative *workload effect* is indeed the driving mechanism, we should see a higher work burden, and consequently lower free time for the DIL. We find that DILs are 5.1 percentage points (or 6.7 percent) less likely to visit their natal home often after their MIL's death, indicating that they have lesser free time (Column (3), Table 1.10).

In addition, we use the information on the time spent on collecting water and fuel by different household members. An important caveat for these results is that these data were collected during the household interview and not the interview with the eligible women. Table 1.6 describes the effect of the MIL's death on women's time-use patterns: women in the household spent more time collecting water (4.4 more minutes over the control average of 25.0 minutes) and fuel (17.9 more minutes over the control average of 52.4 minutes). These patterns persist if we scale the total time spent on those activities by the number of women in the household (Table 1.11). Reassuringly, we do not find any effect of the FIL's death on women's time use patterns (Table

1.12) or of the MIL's death on time use patterns of men and children (Table 1.13 & Table 1.14).

1.6.3 Robustness to Alternate Mechanisms

We have shown several patterns in data consistent with the narrative that the MIL's death decreases her DIL's labor supply due to increased work burden within the household. However, other frameworks would also causally link the MIL's death to her DIL's labor supply. In this sub-section, we discuss some of these other frameworks and argue that they are unlikely to be driving our key results.

Chronically ill mother-in-law. The first alternative mechanism relates to how the MIL passed away. If the MIL passed away because of prolonged sickness, the DIL might have had to leave work to care for her. Indeed, if they died between the two survey rounds, MILs were 10.7% more likely to be chronically ill (Table 1.1). In that case, MIL's death would have freed up the DIL's time and allowed her to work. This effect should go in a direction opposite to that of the documented effect. Moreover, individual fixed effects control for differences in the MIL's health at baseline.

Financial burden. Next, it is possible that the DIL started working because of the financial burden of her MIL's illness and stopped working after the latter's death because this financial need waned. In such a scenario, we should expect a reduction in the labor force participation in response to the FIL's death. However, the estimated impact of the FIL's death on the DIL's labor supply is statistically insignificant and much smaller in magnitude (Table 1.8).

Increased Mental Stress. In the context of losing a MIL, mental stress may arise due to two reasons. First, stress could be associated with the emotional trauma

of losing a family member. If there is a mental health effect due to the MIL's illness and death, it could reduce the DIL's labor market participation (Banerjee, Chatterji, and Lahiri 2017). Placebo tests described in Table 1.8 help discard this mechanism. If this was the case, we should see a similar decline in her husband's labor force participation and a decline in her participation if her FIL passes away. Next, stress could be associated with the increased household work burden on the DIL after the MIL passed away. We cannot completely rule out this mechanism. The DIL's mental health might take a toll due to the increased household work pressure, affecting her labor supply.

Bargaining Power. Consider another narrative that would negatively link the MIL's death with her DIL's labor supply. We also know that there is a positive association between women's labor force participation and autonomy. (Dharmalingam and Morgan 1996; Rahman and Rao 2004). Anderson and Eswaran (2009) present a theoretical argument that earned income would be more effective in increasing a woman's autonomy than an increase in unearned income (from work on own farm). They also provide empirical evidence that establishes a causal link between a woman's employment outside her husband's farm and her autonomy; working on her husband's farm does not improve her autonomy. Now, if a MIL's presence reduces the DIL's autonomy, she may choose to work to improve her bargaining position within the household. Once the MIL passes away, the DIL does not need to assert her autonomy, so one would expect to see a decrease in her labor supply. Recall that the negative effect on the DIL's labor supply was due to a decrease in farm work (Table 1.3), which would not have increased her autonomy before the MIL's death. Also, while we do find some, albeit statistically insignificant improvements in markers of performance of gender (Table 1.10), the likelihood that daughters-in-law take permission to go out

does not decrease with her MIL's death (Table 1.15).[9]

Wealth Shock. Finally, the MIL's death could also be a positive wealth shock due to inheritance. The labor-leisure trade-off would suggest that an increase in wealth would increase leisure and induce women to drop out of the labor force. We do not think that a financial windfall is driving our result because the property is typically passed on to the son and not the DIL in India (Agarwal 1995). We confirm this pattern with more recent data by exploring female property ownership patterns in the National Family Health Survey conducted in 2015-16. In only 9.84 percent of the cases, a woman was listed as one of the owners of agricultural land (individually or jointly); in only 12.63 percent cases, a woman was listed as one of the dwelling owners. Of course, if the impact between the MIL's death was due to the income effect, it should have been evident for the male labor supply or when the FIL passed away, which is not the case (Table 1.8).

1.7 Conclusion

Women's labor force participation is not only thwarted by gender roles that cast women as better suited to housework and childcare duties, but also by restrictive social norms that impede women's employment outside the home in developing countries. A coresident MIL, who plays the role of gatekeeper of restrictive social norms, may affect both frictions in opposite directions. The coresident MIL can encourage the DIL's labor force participation by sharing the housework burden. However, the MIL

9. It is worth mentioning that the proportion of women who report that they have to seek permission to go out massively increased between two survey rounds, indicating a change in the data collection methodology. While the inclusion of $Post_{it}$ would control for the trend increase in these outcomes, the estimates should be interpreted with caution.

may instead inhibit the DIL's labor force participation by restricting her autonomy and mobility. The popular media in South Asia has also portrayed the MIL/DIL relationship as complex. In this paper, we explore the role a MIL plays in the DIL's decision to participate in the labor market in India.

Against the backdrop of a conceptual framework that describes the two countervailing effects of a MIL's presence on the DIL's labor force participation, we show that the MIL's death decreases her DIL's labor force participation. Our empirical approach relies on a difference-in-differences strategy combined with individual fixed effects and uses longitudinal data from two rounds of a nationally representative survey. Although the MIL's death decreases her DIL's labor force participation, we do not observe any effect due to a coresiding FIL's death or any impact on the son's labor force participation. These patterns suggest the role that older women play in the household, by sharing the housework burden and consequently allowing younger women to work, is critical in shaping the latter's labor force participation.

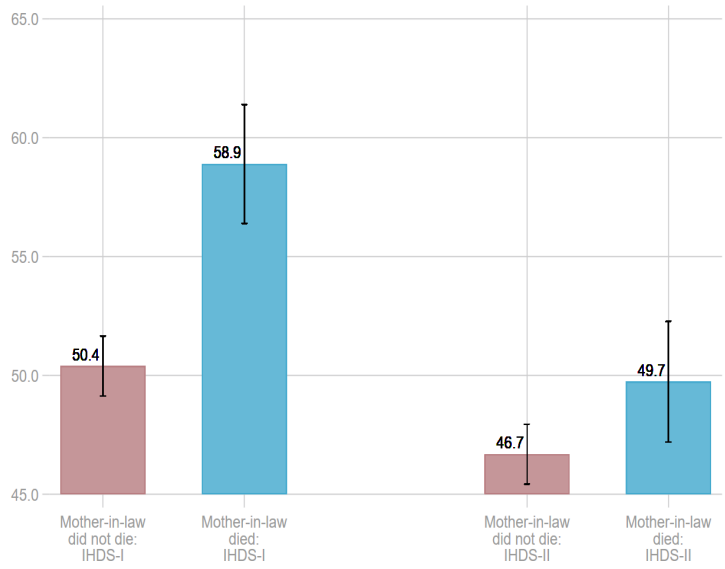
Together, our results suggest that the restrictive role of MILs, while essential in other contexts such as the DIL's bargaining power, social networks, and fertility decisions, is not a binding constraint for their DIL's decision to work. This finding is further confirmed by the increased time the DIL spends on household production tasks or fewer visits to her natal home after her MIL's death. Also, we show that the labor force participation of women with four or more children, who are likely to have a more substantial household work burden, is more strongly affected by their MIL's death. Nevertheless, these results should be interpreted with caution and are only valid for our sample of women living with their MIL in 2004-05.

The results of this paper provide insights into the critical importance of long-established gender roles that put the burden of household chores and childcare on

women in shaping women's decision to work. More immediately, providing institutional childcare support or creches across industries can encourage female labor force participation; mothers' labor force participation responds to greater availability of childcare across contexts, from Germany and France to Kenya and India (Bick 2016; Givord and Marbot 2015; Glinskayai, Lokshin, and Garcia 2000; Nandi et al. 2020). Policies such as neutral tax treatment, which increases the return on married women's market work and the availability of part-time jobs, childcare facilities, and paid parental leave boost female labor force participation (Jaumotte 2004; Colonna and Marcassa 2015; Byker 2016). These policy responses designed to encourage women's work are particularly important in the Indian context, in which the female labor force participation is low and has even declined. Ultimately, challenging the norms that put housework and childcare responsibilities solely on women must be at the center of any policy response to low female labor force participation. Fortunately, restrictive gender norms are not immutable. For instance, in India, exposure to female leaders and attempts to reshape gender attitudes through discussion and persuasion not only challenges stereotypical gender roles but also closes adolescents' gender aspiration gap and makes attitudes more supportive of gender equality (Beaman et al. 2012; Dhar, Jain, and Jayachandran 2018).

1.8 Figures

Figure 1.1. Average labour force participation for the estimation sample across IHDS-I and IHDS-II



Note: This figure plots the average labour force participation rate for women who lost their MILs and those who didn't across IHDS-I and IHDS-II. The estimation sample is restricted to the women who were living with their MILs at the baseline.

1.9 Tables

Table 1.1. Sample Characteristics in 2004-05

	(1) Sub-group: Mother-in-law did not pass away	(2) Sub-group: Mother-in-law passed away	(2) - (1)
Urban residence	0.26	0.27	0.00
SC/ST/OBC	0.86	0.85	-0.01
Own land	0.54	0.54	0.01
Assets (number)	13.08	12.41	-0.67***
Age (in years)	28.31	32.96	4.65**
Age at marriage (in years)	17.61	17.31	-0.30**
Number of brothers	1.96	2.04	0.07**
Number of sisters	1.87	1.92	0.04**
Purdah	0.59	0.59	0.00
Men eat first	0.39	0.34	-0.05**
Cash in hand	0.76	0.80	0.03**
Name in house papers	0.10	0.13	0.03**
Mother-in-law ill in 2004-05	0.35	0.39	0.04**
Labor force participation: >240 hours	0.50	0.59	0.09***
Number of observations	6,045	1,496	

Note: This table reports simple differences in means using data from the IHDS-I for DILs who live with their MILs in 2004-05 across two groups of women: whose MILs didn't die and those whose MILs died between the IHDS-I and IHDS-II.
* Significant at 10%; ** Significant at 5%; *** Significant at 1%.

Table 1.2. Effect of Mother-in-law's Death on Labor Force Participation

	(1) Past Year Labor Force Participation: < 240 Hours	(2) Past Year Labor Force Participation: < 240 Hours
Post	-0.032**	-0.031*
	(0.016)	(0.016)
Post X Mother-in-law Died	-0.046**	
	(0.023)	
Post X Mother-in-law died last year		-0.218**
		(0.096)
Post X Mother-in-law died one years ago		0.000
		(0.056)
Post X Mother-in-law died two years ago		-0.047
		(0.049)
Post X Mother-in-law died three years ago		-0.121***
		(0.045)
Post X Mother-in-law died four years ago		-0.057
		(0.052)
Post X Mother-in-law died five years ago		-0.005
		(0.059)
Post X Mother-in-law died six years ago		-0.038
		(0.065)
Post X Mother-in-law died more than seven years ago		-0.025
		(0.106)
Observations	15048	15048
R-squared	0.689	0.689
Control Average	0.467	0.467
Individual Fixed Effects	Yes	Yes
Time Varying Controls	Yes	Yes

Note: This table reports the results from a difference-in-differences specification 4.1 in Column (1) and from specification 4.2 in Column (2). The outcome is an indicator for if the woman was active in the labor market in the past one year. Time-varying controls include assets and number of children. We also include individual fixed effects. Robust standard errors clustered at the level of the PSU are reported.
* Significant at 10%; ** Significant at 5%; *** Significant at 1%.

Table 1.3. Effect of Mother-in-law's Death on Labour Force Participation: By Sector

	(1)	(2)	(3)	(4)	(5)	(6)
	Past Year Labour Force Participation: < 240 Hours Farm Job	Past Year Labour Force Participation: < 240 Hours Animal Husbandry Job	Past Year Labour Force Participation: < 240 Hours Agricultural Wage Job	Past Year Labour Force Participation: < 240 Hours Non-agricultural Wage Job	Past Year Labour Force Participation: < 240 Hours Salaried Job	Past Year Labour Force Participation: < 240 Hours Business Job
Post	0.007	-0.035**	0.038***	0.049***	-0.033***	-0.035***
	(0.013)	(0.014)	(0.010)	(0.008)	(0.005)	(0.004)
Post X Mother-in-law Died	-0.034*	-0.016	0.006	-0.006	0.003	-0.002
	(0.019)	(0.021)	(0.016)	(0.013)	(0.007)	(0.007)
Observations	15048	15048	15048	15048	15048	15048
R-squared	0.724	0.686	0.700	0.581	0.508	0.509
Control Average	0.252	0.280	0.137	0.0768	0	0
Individual Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Time Varying Controls	Yes	Yes	Yes	Yes	Yes	Yes

Note: This table reports the results from a difference-in-differences specification 4.1. The outcome is an indicator for if the woman was active in the labor market for different sectors in the past one year. Time-varying controls include assets and number of children. We also include individual fixed effects. Robust standard errors clustered at the level of the PSU are reported.
* Significant at 10%; ** Significant at 5%; *** Significant at 1%.

Table 1.4. Effect of Mother-in-law's Death on Labour Force Participation: Heterogeneous Effects, by Number of Children in Round 2

	(1) Past Year Labour Force Participation: < 240 Hours, by Number of Kids
Post X Mother-in-law died X 0-2 children	-0.005
	(0.034)
Post X Mother-in-law died X 3 children	-0.039
	(0.037)
Post X Mother-in-law died X 4 children	-0.107**
	(0.047)
Post X Mother-in-law died X >=5 children	-0.123*
	(0.063)
Observations	15048
R-squared	0.689
Control Average	0.467
Individual Fixed Effects	Yes
Time Varying Controls	Yes

Note: This table reports the results from heterogeneity by the number of kids in the difference-in-differences specification 4.2. The outcome is an indicator for if the woman was active in the labor market in the past one year. Column (1) shows that the effect is the strongest for women with more than three children. The differences in estimates between women with 0-2 children and women with 4 or more children are statistically significant. However, the differences in estimates for women with 3 children and those with 4 or more children is not statistically different from zero. Time-varying controls include assets and number of children. We also include individual fixed effects. Robust standard errors clustered at the level of the PSU are reported.
* Significant at 10%; ** Significant at 5%; *** Significant at 1%.

Table 1.5. Effect of Mother-in-law's Death on Labour Force Participation: Heterogenous Effects, by Age and Caste

	(1) Past Year Labour Force Participation: < 240 Hours, by Age	(2) Past Year Labour Force Participation: < 240 Hours, by Caste
Post X Mother-in-law died X <=25 years	0.049	
	(0.049)	
Post X Mother-in-law died X 26-30 years	0.018	
	(0.046)	
Post X Mother-in-law died X 31-35 years	-0.094**	
	(0.045)	
Post X Mother-in-law died X 36-40 years	-0.103**	
	(0.046)	
Post X Mother-in-law died X >40 years	-0.120***	
	(0.045)	
Post X Mother-in-law died X Non-SC/ST/OBC		-0.039
		(0.050)
Post X Mother-in-law died X SC/ST/OBC		-0.048*
		(0.024)
Observations	15048	15048
R-squared	0.690	0.689
Control Average	0.467	0.467
Individual Fixed Effects	Yes	Yes
Time Varying Controls	Yes	Yes

Note: This table reports the results from heterogeneity by age and caste in the difference-in-differences specification 4.2. The outcome is an indicator for if the woman was active in the labor market in the past one year. The differences in effects for women who are of age 25 or less and those who are over 30 are statistically significant. The differences in the coefficients reported in Column (2) for the two caste categories are not significantly different from zero. Time-varying controls include assets and number of children. We also include individual fixed effects. Robust standard errors clustered at the level of the PSU are reported.
* Significant at 10%; ** Significant at 5%; *** Significant at 1%.

Table 1.6. Effect of Mother-in-law's Death on Women's Aggregate Time Use

	(1) Total Time Spent in Getting Water	(2) Total Time Spent in Getting Fuel
Post	-12.646***	-87.503***
	(1.649)	(5.840)
Post X Mother-in-law Died	4.338*	17.894**
	(2.228)	(7.685)
Observations	28720	28650
R-squared	0.616	0.608
Control Average	25.01	52.39
Individual Fixed Effects	Yes	Yes
Time Varying Controls	Yes	Yes

Note: This table reports the results from a difference-in-differences specification 4.1. The outcome in column (1) is the total time women spend in getting water and the outcome in column (2) is the total time women spend in getting fuel. Time-varying controls include assets and number of children. We also include individual fixed effects. Robust standard errors clustered at the level of the PSU are reported.
* Significant at 10%; ** Significant at 5%; *** Significant at 1%.

Appendix

1.A Identifying Mother-in-law/Daughter-in-law Pairs

In this section, we describe how we used the household roster at the baseline to identify co-residing MIL/DIL pairs. The household roster listed demographic information on all the individuals who lived in this dwelling and shared the same kitchen for over the past six months. These individuals constitute household members.

The key ingredients for identifying identify MIL/DIL pairs were the sex of the head of household, sex of their children, their children's marital status, and relationship to the head of the household. We used four rules to identify MIL/DIL pairs using the household roster:

Rule 1: If the household had a male head and a married son, then the wife of the household head is the MIL and the female child-in-law of the household head is the DIL.

Rule 2: If the household had a female head and a married son, then the household head is the MIL, and the female child-in-law of the household head is the DIL.

Rule 3: If the household had a married male head and his mother was a household member, then his mother is the MIL, and his wife is the DIL.

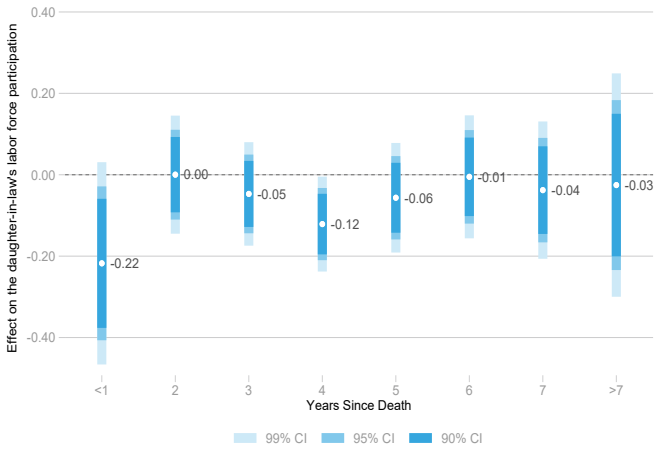
Rule 4: If the household had a married female head and her parent-in-law was a

household member, then her female parent-in-law is the MIL, and she is the DIL.

We used analogous rules to identify the FIL of the DIL we thus identified. Finally, we used information on the spouse identification number within the same roster to identify the spouses of the DIL.

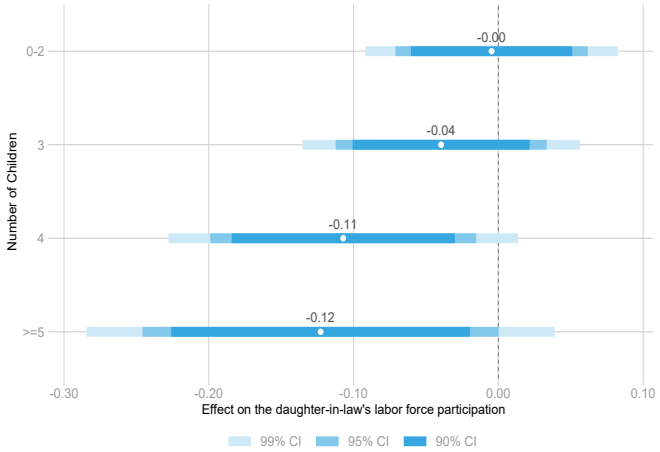
1.B Additional Figures

Figure 1.2. Effect of the Death of the Mother-in-Law, by years of death



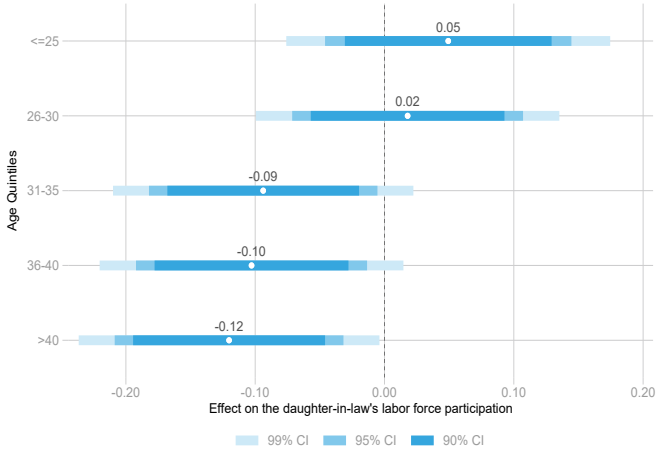
Note: Each circle plots the point estimate for the coefficient on $MILdied_i * Post_{it} * H_i$ where H_i is the number of years since the MIL's death (see equation 4.2). The regressions include individual fixed effects and time-varying controls (assets and number of children). All standard errors are clustered at the level of the primary sampling unit.

Figure 1.3. Effect of the Death of the Mother-in-law, by the number of children



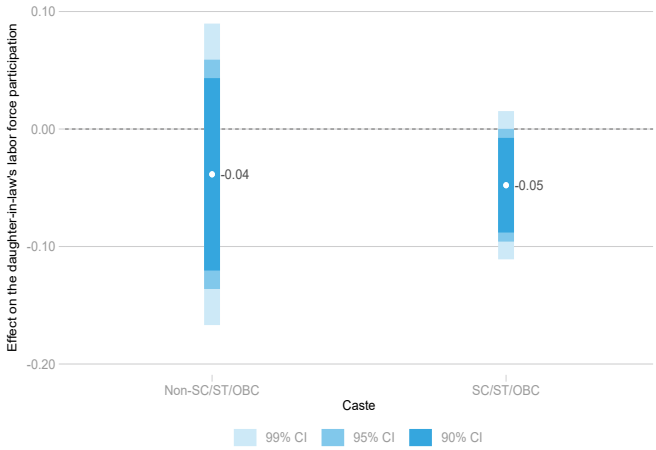
Note: Each circle plots the point estimate for the coefficient on $MILdied_i * Post_{it} * H_i$ where H_i is the number of children the DIL had in 2011-12 (see equation 4.2). The regressions include individual fixed effects and time-varying controls (assets and number of children). All standard errors are clustered at the level of the primary sampling unit.

Figure 1.4. Effect of the Death of the Mother-in-Law, by age



Note: Each circle plots the point estimate for the coefficient on $MILdied_i * Post_{it} * H_i$ where H_i is the DIL's age (see equation 4.2). The regressions include individual fixed effects and time-varying controls (assets and number of children). All standard errors are clustered at the level of the primary sampling unit.

Figure 1.5. Effect of the Death of the Mother-in-Law, by caste



Note: Each circle plots the point estimate for the coefficient on $MILdied_i * Post_{it} * H_i$ where H_i is caste (see equation 4.2). The regressions include individual fixed effects and time-varying controls (assets and number of children). All standard errors are clustered at the level of the primary sampling unit.

Figure 1.6. Permutation Tests for the Effect of Mother-in-law's Death on Daughter-in-law's Labor Force Participation

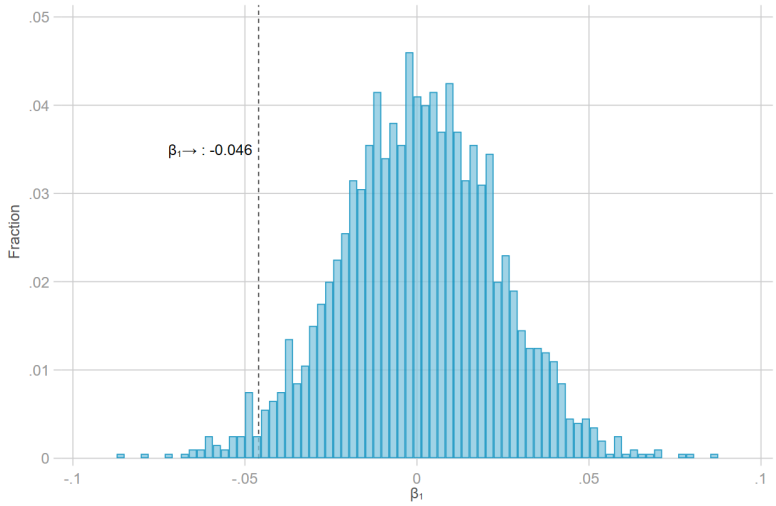


Figure 1.7. Distribution of β_1

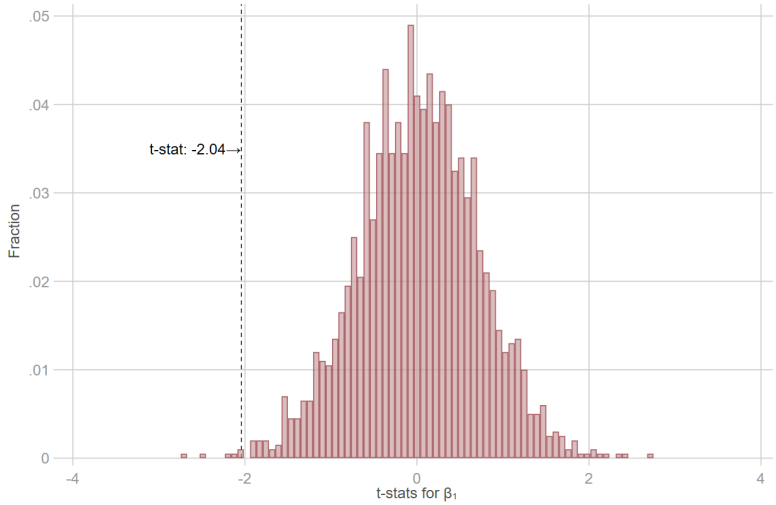


Figure 1.8. Distribution of t-statistic

Note: Panel (a) shows the distribution of estimates of β_1 from a permutation test for equation 4.1. Panel (b) shows the distribution of estimates of t-statistic from a permutation test for equation 4.1. For each observation, treatment status was randomly generated and these assignments were used to compute β_1 and associated t-statistic 2000 times.

1.C Additional Tables

Table 1.7. Full Sample Characteristics of Eligible Women in 2004-05

	(1) Sub-group: Did not live with mother-in-law	(2) Sub-group: Lived with mother-in-law	(2) - (1)
Urban residence	0.33	0.26	-0.06***
SC/ST/OBC	0.85	0.86	0.00
Own land	0.44	0.54	0.09***
Assets (number)	11.50	12.95	1.45***
Age (in years)	34.88	29.23	-5.64***
Age at marriage (in years)	16.99	17.55	0.56***
Number of brothers	2.05	1.98	-0.07***
Number of sisters	1.96	1.88	-0.08***
Purdah	0.53	0.59	0.06***
Men eat first	0.30	0.38	0.09***
Cash in hand	0.84	0.77	-0.07***
Name in house papers	0.17	0.11	-0.06***
Labor force participation: >240 hours	0.58	0.52	-0.06***
Number of observations	17,935	7,541	

Note: This table reports simple differences in means using data from the IHDS-I for all eligible women in 2004-05 across two groups of women: who live with their MILs and those who did not live with their MILs.
* Significant at 10%; ** Significant at 5%; *** Significant at 1%.

Table 1.8. Effect of Mother-in-law's Death on Labor Force Participation: Placebo Tests

	(1)	(2)
	Past Year Labor Force Participation: < 240 Hours	Past Year Labor Force Participation: < 240 Hours
	Treatment	Outcome
	Father-in-law's Death	Spouse's Labor Force Participation
Post	-0.025	-0.049**
	(0.020)	(0.019)
Post X Father-in-law died	-0.011	
	(0.027)	
Post X Mother-in-law Died		-0.034
		(0.027)
Observations	9949	13095
R-squared	0.678	0.737
Control Average	0.467	0.458
Individual Fixed Effects	Yes	Yes
Time Varying Controls	Yes	Yes

Note: This table reports the results from a difference-in-differences specification 4.1. The outcome for column (1) is an indicator for if the woman was active in the labor market in the past one year and for column (2) is an indicator if her spouse was active in the labor market in the past one year. In the first column, the sample is restricted to those women who lived with their FILs, and in the second column it is restricted to those men whose wives lived with their mothers-in-law. Time-varying controls include assets and number of children. We also include individual fixed effects. Robust standard errors clustered at the level of the PSU are reported.
* Significant at 10%; ** Significant at 5%; *** Significant at 1%.

Table 1.9. Sample Characteristics in 2004-05

	(1) Sub-group: Father-in-law did not pass away	(2) Sub-group: Father-in-law passed away	(2) - (1)
Urban residence	0.24	0.25	0.01
SC/ST/OBC	0.86	0.85	-0.02
Own land	0.57	0.56	-0.00
Assets (number)	13.29	12.98	-0.31
Age (in years)	26.81	29.86	3.05***
Age at marriage (in years)	17.54	17.67	0.13
Number of brothers	1.94	1.97	0.03
Number of sisters	1.86	1.94	0.07
Purdah	0.64	0.59	-0.05**
Men eat first	0.47	0.41	-0.05**
Cash in hand	0.75	0.76	0.01
Name in house papers	0.08	0.11	0.03**
Labor force participation: >240 hours	0.47	0.55	0.08***

Note: This table reports simple differences in means using data from the IHDS-I for DILs who live with their FILs in 2004-05 across two groups of women: whose FILs didn't die and those whose FILs died between the IHDS-I and IHDS-II.
* Significant at 10%; ** Significant at 5%; *** Significant at 1%.

Table 1.10. Effect of Mother-in-law's Death on Women's Other Outcomes

	(1) Men Eat First	(2) Woman Keeps Purdah	(3) Woman Visits Natal Home Often
Post	-0.096***	0.002	0.024
	(0.018)	(0.015)	(0.016)
Post X Mother-in-law Died	0.026	-0.031	-0.051**
	(0.024)	(0.020)	(0.023)
Observations	15048	15048	15048
R-squared	0.612	0.775	0.583
Control Average	0.278	0.600	0.760
Individual Fixed Effects	Yes	Yes	Yes
Time Varying Controls	Yes	Yes	Yes

Note: This table reports the results from a difference-in-differences specification 4.1. In column (1), the outcome is if men eat first, in column (2), the outcome is if woman keeps purdah, and in column (3), the outcome is if woman visits natal home often. Time-varying controls include assets and number of children. We also include individual fixed effects. Robust standard errors clustered at the level of the PSU are reported.

* Significant at 10%; ** Significant at 5%; *** Significant at 1%.

Table 1.11. Effect of Mother-in-law's Death on Women's Per Capita Time Use

	(1) Average Time Spent in Getting Water	(2) Average Time Spent in Getting Fuel
Post	-8.040***	-43.787***
	(0.755)	(2.584)
Post X Mother-in-law Died	2.748***	12.245***
	(0.957)	(3.180)
Observations	28720	28650
R-squared	0.614	0.607
Control Average	9.453	19.90
Individual Fixed Effects	Yes	Yes
Time Varying Controls	Yes	Yes

Note: This table reports the results from a difference-in-differences specification 4.1. The outcome in column (1) is the average time women spend in getting water and the outcome in column (2) is the average time women spend in getting fuel. Time-varying controls include assets and number of children. We also include individual fixed effects. Robust standard errors clustered at the level of the PSU are reported.
* Significant at 10%; ** Significant at 5%; *** Significant at 1%.

Table 1.12. Effect of Father-in-law's Death on Women's Time Use

	(1) Total Time Spent in Getting Water	(2) Total Time Spent in Getting Fuel	(3) Average Time Spent in Getting Water	(4) Average Time Spent in Getting Fuel
Post	-11.991***	-89.828***	-7.873***	-46.927***
	(1.822)	(6.469)	(0.848)	(3.115)
Post X Fother-in-law died	-1.400	-2.711	-1.288	-5.186
	(2.847)	(8.739)	(1.459)	(4.712)
Observations	17302	17252	17284	17234
R-squared	0.616	0.607	0.613	0.608
Control Average	24.44	51.12	9.766	20.40
Individual Fixed Effects	Yes	Yes	Yes	Yes
Time Varying Controls	Yes	Yes	Yes	Yes

Note: This table reports the results from a difference-in-differences specification 4.1. The outcome in column (1) is the total time women spend in getting water, the outcome in column (2) is the total time women spend in getting fuel, the outcome in column (3) is the average time women spend in getting water, and the outcome in column (4) is the average time women spend in getting fuel. Time-varying controls include assets and number of children. We also include individual fixed effects. Robust standard errors clustered at the level of the PSU are reported.
* Significant at 10%; ** Significant at 5%; *** Significant at 1%.

Table 1.13. Effect of Mother-in-law's Death on Men's Time Use

	(1) Total Time Spent in Getting Water	(2) Total Time Spent in Getting Fuel	(3) Average Time Spent in Getting Water	(4) Average Time Spent in Getting Fuel
Post	-3.779***	-39.456***	-3.042***	-21.002***
	(0.996)	(3.933)	(0.509)	(1.845)
Post X Mother-in-law Died	1.128	9.215	0.458	0.497
	(1.526)	(5.854)	(0.763)	(3.072)
Observations	28720	28709	28597	28586
R-squared	0.562	0.563	0.566	0.573
Control Average	11.06	31.36	4.480	13.44
Individual Fixed Effects	Yes	Yes	Yes	Yes
Time Varying Controls	Yes	Yes	Yes	Yes

Note: This table reports the results from a difference-in-differences specification 4.1. The outcome in column (1) is the total time men spend in getting water, the outcome in column (2) is the total time men spend in getting fuel, the outcome in column (3) is the average time men spend in getting water, and the outcome in column (4) is the average time men spend in getting fuel. Time-varying controls include assets and number of children. We also include individual fixed effects. Robust standard errors clustered at the level of the PSU are reported.
* Significant at 10%; ** Significant at 5%; *** Significant at 1%.

Table 1.14. Effect of Mother-in-law's Death on Children's Time Use

	(1) Girls' Total Time Spent in Getting Water	(2) Girls' Total Time Spent in Getting Fuel	(3) Boys' Total Time Spent in Getting Water	(4) Boys' Total Time Spent in Getting Fuel
Post	-1.143*	-2.166**	1.640***	-1.501*
	(0.659)	(0.942)	(0.437)	(0.853)
Post X Mother-in-law died	0.526	-0.501	-0.979	-0.039
	(1.139)	(2.218)	(1.309)	(1.411)
Observations	28720	28720	25305	28720
R-squared	0.507	0.492	0.575	0.479
Control Average	4.512	5.245	4.160	3.738
Individual Fixed Effects	Yes	Yes	Yes	Yes
Time Varying Controls	Yes	Yes	Yes	Yes

Note: This table reports the results from a difference-in-differences specification 4.1. The outcomes in column (1) & (2) are the total time girls spend in getting water and fuel. The outcomes in Column (3) & (4) are the total time boys spend in getting water & fuel. Time-varying controls include assets and number of children. We also include individual fixed effects. Robust standard errors clustered at the level of the PSU are reported.
* Significant at 10%; ** Significant at 5%; *** Significant at 1%.

Table 1.15. Effect of Mother-in-law's Death on Measures of Autonomy

	(1) Need Permission to Visit Local Grocery Store	(2) Need Permission to Visit Friend's Home	(3) Need Permission to Visit Health Center
Post	0.197***	0.086***	0.105***
	(0.022)	(0.016)	(0.013)
Post X Mother-in-law died	-0.007	-0.029	-0.007
	(0.029)	(0.022)	(0.020)
Observations	15048	15048	15048
R-squared	0.544	0.522	0.534
Control Average	0.485	0.878	0.922
Individual Fixed Effects	Yes	Yes	Yes
Time Varying Controls	Yes	Yes	Yes

Note: This table reports the results from a difference-in-differences specification 4.1. In column (1), the outcome is if the woman needs permission to visit a local grocery store, in column (2), the outcome is if she needs permission to visit her friend's home, and in column (3), the outcome is if woman she needs permission to visit a health center. Time-varying controls include assets and number of children. We also include individual fixed effects. Robust standard errors clustered at the level of the PSU are reported.
* Significant at 10%; ** Significant at 5%; *** Significant at 1%.

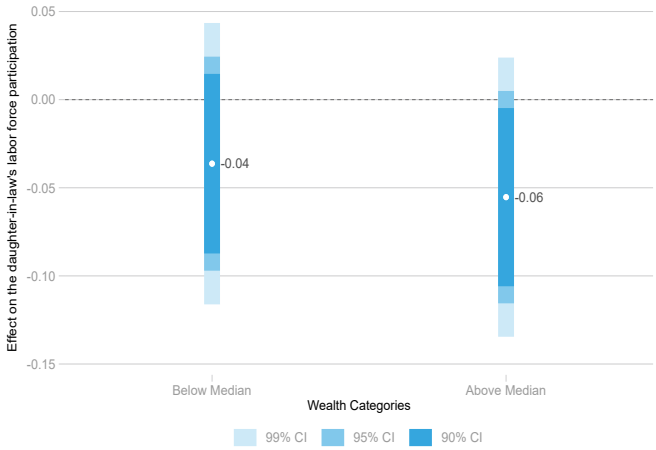
1.D Additional Analysis

Table 1.16. Effect of Mother-in-law's Death on Labour Force Participation: Heterogenous Effects, by Wealth

	(1) Past Year Labour Force Participation: < 240 Hours, by Wealth
Post X Mother-in-law died X Bottom 50%	-0.036
	(0.031)
Post X Mother-in-law died X Top 50%	-0.055*
	(0.031)
Observations	15048
R-squared	0.689
Control Average	0.467
Individual Fixed Effects	Yes
Time Varying Controls	Yes

Note: This table reports the results from heterogeneity by wealth in the difference-in-differences specification 4.2. The outcome is an indicator for if the woman was active in the labor market in the past one year. Column (1) shows that the effect is marginally stronger for richer women. However, the differences in the estimates between the two wealth categories are not statistically different from zero. Time-varying controls include assets and number of children. We also include individual fixed effects. Robust standard errors clustered at the level of the PSU are reported.
* Significant at 10%; ** Significant at 5%; *** Significant at 1%.

Figure 1.9. Effect of the Death of the Mother-in-Law, by wealth



Note: Each circle plots the point estimate for the coefficient on $MILdied_i * Post_{it} * H_i$ where H_i is wealth categories (see equation 4.2). The regressions include individual fixed effects and time-varying controls (assets and number of children). All standard errors are clustered at the level of the primary sampling unit.

Chapter 2

The Effects of Making Prenatal Sex Detection Illegal

Divya Pandey

2.1 Introduction

Discrimination against females can manifest itself in the form of sex selection. Sex selection can be defined as the deliberate elimination of girls through feticide or infanticide. Excess female mortality caused by gender discrimination, sex-selective abortions, and neglect has led to missing women in several developing countries with strong son preferences (Sen 1990).[1] He finds that there are more than 100 million missing women in South Asia, West Asia, and North Africa as a result of inequality and neglect. Among the number of women that could have been potentially alive today, India alone has 25 million "missing women" (Anderson and Ray 2012).

1. According to Sen 1990, the female to male ratio in developing countries can be as low as 0.94 in comparison to 1.05-1.06 in the developed countries.

One of the reasons for declining female to male ratios in India is sex selection- the deliberate elimination of girls through feticide or infanticide. The introduction of prenatal sex determination along with the legalization of abortion led to an increase in sex-selective abortions. In an attempt to remedy this problem, the Indian Parliament enacted the Pre-Natal Diagnostic Techniques (PNDT) Act in 1996 which made the use of prenatal sex determination techniques illegal. The act aimed to improve the distorted sex ratios by reducing sex-selective abortions. A similar law was passed by the Indian state of Maharashtra before 1996. Studies suggest that the policy improved the female to male ratio in the 0-6 age group (Nandi and Deolalikar 2013).

This paper examines the unintended consequences of placing a legal ban on prenatal sex-selection techniques. While the intended effect of the PNDT Act was to increase the probability of female birth, the policy may have substituted prenatal sex selection for postnatal sex discrimination. For example, parents who would have otherwise aborted a female may respond to the PNDT Act by investing fewer resources in unwanted girls. Fewer health investments can eventually lead to worse health outcomes for girls in the long run.

To study gender discrimination and child health, this paper studies measures of child health and parental behavior from the National Family Health Surveys (NFHS). To estimate causal impacts, the identification strategy exploits two sources of variation. Specifically, I rely on variation over time (birth before and after the ban) and spatial variation due to regional differences. For the spatial variation, the Indian state of Maharashtra was the first to impose a ban on prenatal sex determination techniques in the year 1988, while the rest of India imposed the same policy in the year 1996. Therefore, the national level PNDT Act only affected children born in non-Maharashtrian states during or after 1996.

Consistent with previous literature, preliminary findings suggest a positive effect on the probability of female birth. However, positive effects are only restricted to first-born girls. In terms of health, using World Health Organization's z-score based measures for weight and height-for-age, I find those female children born after the ban are 13.8 and 12 percent more likely to be malnourished or stunted in comparison to boys. Further, at birth, there is no difference between the health outcomes of boys and girls. Nevertheless, the probability of malnutrition or stunting is significantly higher for older girls as a result of the ban.

The findings of this paper in terms of female child health outcomes provide support to the hypothesis that as a result of the ban on prenatal sex determination parents are responding by investing fewer resources in their female children. Existing studies lend support to this hypothesis by showing that mothers breastfeed boys more than girls (Jayachandran and Kuziemko 2011). A ban on prenatal sex determination can widen gender gaps in breastfeeding. In terms of prenatal investments, mothers receive more tetanus shots and make more antenatal visits when pregnant with a male child (Bharadwaj and Lakdawala 2013). In the Indian context, with high son preferences, another effect of a successful legal ban on prenatal sex determination could imply the reduction of gender gaps in prenatal investments. At this point, I restrict my analysis to examine the effect of the policy on female outcomes.

Based on existing literature, two potential mechanisms could explain my findings. First, prenatal sex determination not only gives parents control over fertility but also enables them to influence the gender composition of children. In the absence of prenatal sex determination techniques and prevalence of son preferences, parents can respond by investing more in boys relative to girls because they can no longer abort 'unwanted' girls. Second, even when parents do not actively discriminate against

children and they only have the desired sex composition of children with at least one son, then they may exhibit fertility-stopping behavior. This means that when sex selection is not allowed parents continue to have children until they achieve their desired number of sons or at least one son. Consequently, girls will be born in families with more children on average and hence get fewer resources (Jayachandran and Pande 2017; Rosenblum 2013).

Some papers examining the effects of sex-selective abortions that closely complement my findings are discussed as follows. First, a study exploiting exogenous variation in the availability of sex-selective abortion caused by the legalization of abortion in Taiwan indicates that sex selection increases male-to-female ratios and reduces excess female mortality (Lin, Liu, and Qian 2014). Second, Anukriti, Bhalotra, and Tam, n.d.) show that the availability of sex-detection technologies in India has caused a reduction in fertility and the erosion of gender differentials in breastfeeding, immunization, and post-neonatal mortality rates. The findings of these papers are in line with my findings. If sex determination technologies reduce postnatal discrimination, then the absence of these technologies can lead to worse female health outcomes.

There are opposing views on the effects of sex selection in the long run. Sex selection can cause an increase in crime against women due to a surplus of males at marriageable and crime-prone ages (Amaral and Bhalotra 2016). However, high male-to-female ratios can improve female bargaining power in the marriage market and hence the likelihood of getting married (Angrist 2002).

Studies using Indian Census data and Indian District Level Household Survey indicate that a legal ban on prenatal sex-determination increased female to male ratios for children in the age group 0-6 (Nandi and Deolalikar 2013) and did not affect

the relative mortality of infant girls (Nandi 2014). Empirical evidence also suggests that a legal ban on prenatal sex determination did not affect sex ratios among Asian newborn children in Pennsylvania and Illinois (Nandi, Kalantry, and Ciltro 2014). While there are several papers studying the short and long-run effects of sex selection, studies on the effects of making sex-detection technologies inaccessible are limited. From a policy perspective, it is an important question as it has implications for "missing women". Also, the policy may backfire if a ban on prenatal sex discrimination increases postnatal sex discrimination. Although these studies suggest that such a ban does not affect female child mortality, it is possible that it affects female child health and eventually long run mortality.[2] While helpful, existing papers only focus on the effect of a ban on prenatal sex detection on sex ratios and child mortality. This paper seeks to add to the literature by looking at the effects of the ban on female child health and prenatal and postnatal investments made by parents.

2.2 Background

Cultural norms such as patriarchal and patrilineal societies are crucial reasons for son preferences (Jayachandran 2015). Name and property are passed on to the next generation through male descendants in patrilineal societies and daughters are married off to other families. Parents prefer sons because they view them as insurance for old age. The tradition of dowry at the time of daughters' wedding in South Asian countries also makes having daughters more expensive (Bhalotra, Chakrvarty, and Gulesci 2016; Anukriti, Kwon, and Prakash 2016). The legalization of abor-

2. In fact Anderson and Ray 2012) find that even though sex selection at birth and poor treatment of female children are the key explanations for "missing women", they find that there are more missing women at older ages in the Indian context.

tion and the introduction of sex determination technologies allowed parents to abort 'unwanted' girls.

Abortion was first legalized in India through the passage of the Medical Termination of Pregnancy Act (MTP) 1971, effective in most states since 1972. The stated objective of the MTP ACT was to improve maternal health by preventing a large number of unsafe abortions leading to maternal mortality.[3] The MTP Act lays out the valid reasons for a legal abortion, who can terminate pregnancies legally, and the type of facility in which abortions can be performed. Under the Act, abortion is legal if the pregnancy endangers the pregnant woman's life, causes injury to her physical or mental health, is a result of rape or contraceptive failure (applies only to married women), or indicates mental or physical abnormalities in the fetus. [4]

Sex selection became an important cause of abortion after the introduction of amniocentesis in the 1970s. Amniocentesis was originally introduced to detect genetic abnormalities, but it was soon being used to determine the sex of the fetus for sex-selective abortions. Fetal sex determination became even more feasible after ultrasound became accessible in the mid-1980s. The imports of ultrasound machines spiked up after the gradual liberalization of the Indian economy since the 1980s (Anukriti et al. 2017). [5] Ultrasound techniques were advertised as good investments with slogans that said that the cost of ultrasound is much lower than the future costs of dowry (Sudha and Rajan 1999). The ubiquity and affordability (roughly $10-$20

3. Some believe that population control was an important motive of the Act (Phadke 1998).

4. In most cases approval of the pregnant woman is enough for a legal abortion, however, written approval is required from a guardian if the woman seeking an abortion is under 18 years of age or mentally ill. Further, approval from one registered practitioner is required for abortions taking place at less than 12 weeks of pregnancy and two registered medical practitioners must approve abortions between 12 and 20 of pregnancy (Arnold et al. 2002). Pregnancy cannot be terminated beyond 20 weeks of gestation.

5. The production of ultrasound machines increased around 15 times in the period 1988-2003 as a result of relaxation of industrial licensing policies (George 2006, Grover and Vijayvergia 2006).

according to Arnold et al. 2002) of sex-detection technologies contributed to the cascading female-to-male ratios in India (Bhalotra and Cochrane 2010).

The increasing number of sex-selective abortions led to several protests and feminist movements (Gangoli 1998). The Indian government banned sex detection in government facilities as early as 1976. Nevertheless, private health centers remained unregulated for almost 20 years and the use of sex detection continued conspicuously. In 1988, the Indian state of Maharashtra became the first state to put a legal ban on prenatal sex determination driven by the health and feminist movements. The rest of India enacted a law called the Pre-Natal Diagnostic Techniques (Regulation and Prevention of Misuse) Act, 1994 which became fully effective on January, 1st 1996. The law made it illegal to use amniocentesis or ultrasound to determine the sex of the child, however, the techniques could be used to determine maternal and fetal health. The PNDT law was further amended in 2003, by bringing within the bounds of the law, techniques of preconception sex selection, where doctors try to influence the gender of the child through techniques like sperm sorting.

The efficacy of the PNDT Act has often been questioned. There are concerns that the act has been poorly enforced in India because female to male ratios has continued to decline from 945 girls per 1000 boys in 1991 to 927 in 2001 to 910 in 2011. According to estimates by Bhalotra and Cochrane 2010) around 0.48 million girls were aborted in India per annum during 1995-2005. The Indian Child Development Minister, Menaka Gandhi suggested in 2016 that sex determination should be made compulsory so that the government can keep track of how many girls were conceived and how many were born. There are opposing views on a legal ban on prenatal sex determination. While some believe that the ban can improve sex ratio at birth (Nandi and Deolalikar 2013), others believe that prenatal sex discrimination can lead to a decrease in postnatal sex

discrimination (Anukriti et al. 2017, Goodkind 1996, 1999).

2.3 Data

This study uses data from the National Family Health Surveys (NFHS) that provides information on population, health, and nutrition across Indian states.[6] The NFHS is a large-scale, multi-round survey conducted in a sample of households throughout India. These surveys were designed to provide estimates of important indicators on family welfare, maternal and child health, anthropometrics, and nutrition. Women between the ages of 15 and 49 were interviewed. So far, there have been four rounds of these surveys in India. To study the effects of putting a legal ban on prenatal sex determination, I use data from the first two rounds of the surveys, NFHS-1 (1992-93) and NFHS-2 (1998-99).

The effect of a ban on prenatal sex determination on the survival probability of girls is estimated using NFHS-2 (1998-99). The surveys include the entire birth history of women including the date of birth of the child, the child's gender and birth order, and mother's age at first birth. The sample consists of 80,539 births, out of which 4,489 are in Maharashtra. The advantage of using this sample is that it is not affected by gender-specific mortality rates because, unlike census data, it reports the entire birth history of women which includes living as well as deceased children. The sample used for this analysis consists of all births between 1990 and 1999. I do not include earlier births because the empirical strategy exploits the fact that Maharashtra had imposed a legal ban on prenatal sex determination in 1988 and was

6. NFHS data is available through the Demographic and Health Surveys (DHS) Program's data distribution system.

unaffected by the national level ban in 1996.[7]

To estimate the effect on health outcomes and investments, I merged the first two rounds of the NFHS (NFHS-1 and NFHS-2). NFHS-2 (1998-99) collected anthropometric and investment data on children who were less than 3 years of age, as a result, there was no information on children born before the ban. Merging the second round with the first round provides data on children born in the pre-ban period. Child malnutrition and stunting are measured using weight-for-age (WFA) and height-for-age (HFA) which are World Health Organization's universally applicable, z-score based growth standards for children aged 0 to 5 years.[8] A z-score of 0 represents the median of the gender and age-specific reference population and a z-score of -2 implies that the child is 2 standard deviations below the median of the reference population. A z-score of -2 is the threshold for being considered malnourished or stunted.[9] The sample for estimating the effect on child health outcomes consists of 38,611 children, out of which 2,678 were born in Maharashtra.

A potential problem with using survey data is selection bias. The surveys automatically exclude women who died, for instance, from maternal mortality. If abortions increase the risk of maternal mortality, then the sample would not include women who died before the survey due to unsafe sex-selective abortions. This could lead to the underestimation of the effect of the ban on the survival probability of females. The effect of the selection bias on the estimates for child outcomes is ambiguous.

7. Years 1988 and 1989 are dropped because of a limited number of observations.

8. The WHO constructs the height distribution using a sample of children from six affluent populations across five continents (Brazil, Ghana, India, Norway, Oman, and the United States) with no known health or environmental constraints to growth and who received recommended nutrition and health inputs (WHO Multicentre Growth Reference Study Group 2006b, Jayachandran and Pande 2017)

9. As per WHO guidelines, I exclude outliers when: (i) WFA z-score>5 or <-6, (ii) (i) HFA z-score>6 or <-6.

The estimates would be downwards biased if we assume that children born to unhealthy mothers who died due to abortion are also unhealthy. However, they could be biased upwards if women who used sex-selective abortions gave birth to 'wanted' girls. In that case, the sample will not include healthy girls who did not face any discrimination.

2.4 Empirical Strategy

The hypothesis I test in this paper is whether a ban on prenatal sex determination causes an increase in postnatal sex discrimination. Prenatal sex determination was first banned in the Indian state of Maharashtra in the year 1988 and the rest of India followed in the year 1996. An individual's exposure to the national level policy is determined by the year of birth and the state of birth. I use a difference-in-differences strategy, similar to Nandi and Deolalikar 2013, to compare individuals born in Indian states other than Maharashtra (treatment group) to individuals born in Maharashtra (control group) before and after the national-level ban in 1996. The DD specification used to examine the effects of a legal ban on prenatal sex determination is described as follows,

$$Y_{ist} = \alpha_0 + \gamma Post96_t Treat_s + \alpha Post96_t + \beta Treat_s + \Theta X_{ist} + \epsilon_{ist} \tag{2.1}$$

where i is an individual, s is the individual's state of birth, and t is the birth year. X_{ist} includes other control variables. $Post96_t$ is equal to one for 1996 and following years and $Treat_s$ is equal to one for non-Maharashtrian states. The dependent variable, Y_{ist} in equation (1) is an indicator for a female birth. The DD estimate in

equation (1), γ suggests the effect of the law on the probability of a female birth. All standard errors are clustered by state or state and birth-year to attenuate serial correlation in the error term.

$$Y_{ist} = \alpha_0 + \gamma Post96_t Treat_s Female_i + \alpha_1 Post96_t + \alpha_2 Treat_s + \alpha_3 Female_i \\ + \beta_1 Post96_t Treat_s + \beta_2 Post96_t Female_i + \beta_3 Treat_s Female_i + \Theta X_{ist} + \epsilon_{ist} \tag{2.2}$$

The dependent variables in equation (2) are z-scores for WFA or HFA or indicators for under-three malnutrition or stunting for child i. As the main outcomes of interest are the gender differentials in health, I use an indicator for the gender of the child ($Female_i$) as the third interaction term in the triple difference-in-differences specification in equation (2). The triple difference estimate, γ captures the effect of the legal ban on prenatal sex determination on female child health.

One of the challenges to interpreting γ (equation (1)) as the causal effect of the ban on female health is that it does not take into account that there could be heterogeneous effects across age groups. When the sex of the child is unknown before birth then parents are unable to selectively invest less when pregnant with a female child. As a result, one should not expect gender gaps in health outcomes biased against females at birth. However, if girls receive fewer resources after birth, then malnutrition should be increasing with age among girls. To test for heterogeneous effects by gender and

age, I estimate the following equation:

$$Y_{ist} = \alpha_0 + \gamma_0 Treat_t Post_{96} + \gamma_1 Treat_s Post96_t Age1_i + \gamma_2 Treat_s Post96_t Age2_i$$
$$+\alpha_1 Age1_i + \alpha_2 Age2_i + \alpha_3 Treat_s + \alpha_4 Post96_t + \beta_1 Treat_s Age1_i + \beta_2 Treat_s Age2_i$$
$$+\beta_3 Post96_t Age1_i + \beta_4 Post96_t Age2_i + \Theta X_{ist} + \epsilon_{ist} \tag{2.3}$$

In equation (3), $Age1_i$ and $Age2_i$ are age-specific dummy variables equal to one when the age of child i is equal to 1 and 2. The omitted age group includes all children who are 0 years of age.[10] γ_0, γ_1 and γ_2 are the estimates of interest determining the effect of banning prenatal sex determination on malnutrition among children in the age group 0, 1 and 2. Including $Female$ specific interactions in equation (3) allows observing differences in health outcomes by age and gender.

Soil quality, cultural differences, and other unobservables like beliefs can also affect the degree of son preferences. To address this, I include district fixed effects in my regression equations. Including district fixed effects allows controlling for unobservable time-invariant characteristics (soil quality, cultural or linguistic characteristics) that can affect female health. I also include birth order fixed effects which can also affect child health (Jayachandran and Pande 2017).

10. $Age0$ implies that the child is less than 12 months old. $Age1$ implies that the kid is greater than or equal to 12 months old, but less than 24months. $Age2$ implies that the child's age is greater than equal to 24 months, but less than 36 months.

2.5 Results

2.5.1 Female Birth

Table 2.2 presents estimates of the effect of putting a legal ban on prenatal sex determination on the probability of female birth with different sets of controls. Column (1) reports coefficients from a specification similar to Nandi and Deolalkiar (2004). Specifications in columns (2)-(4) include birth year fixed effects with and without state-specific trends and birth-order fixed effects. Across all the different specifications my results are consistent with the previous literature. The coefficient on $Treat \times Post96$ confirms that as a result of the ban there was a 2.3 percentage point increase in the probability of female birth. This corresponds to a 4.8 percent increase in the probability of female births.[11]

2.5.2 Health

Table 2.3 reports estimates of the effect of legally banning prenatal sex determination on z-scores for WFA and HFA. The coefficients on $Treat \times Post96 \times Female$ demonstrate that there was a 0.20 and 0.21 standard deviation decrease in the z-scores of under-three female WFA and HFA, respectively. With reference to the pre-period, this accounts for an 11.5 percent and 10.7 percent decrease in the z-scores of female WFA and HFA.

Table 2.4 presents results when the outcome variable is a binary outcome variable equal to one of the child is malnourished or stunted. The positive coefficients indicate that the probability of under-three female malnutrition and stunting increased by 5.8

11. The fraction of female births in the baseline year (1995) in the treated units was 0.48.

and 6 percentage points, respectively. This implies that female malnutrition and stunting increased by 13.8 and 12 percent as a result of the ban.

The above results show that there was an overall increase in the probability of female malnutrition and stunting as a result of the ban. The heterogeneous effects by age are reported in Table 2.5. Coefficients in the first row of Table 4 demonstrate that the ban had no negative effect on the health of girls who were less than a year old at the time of the survey. However, at older ages, the coefficients indicate an adverse effect on female health. One-year-old girls were 8 percentage points more likely to be stunted and two-year-old girls were 24.3 percentage points more likely to be stunted after the ban. Boys are not likely to be stunted at age 1, but the probability of stunting is 1.3 percentage points for them at age 2. Nevertheless, the probability of male stunting is still lower at age 2 compared to females. These results indicate that parents responded by investing fewer resources in the 'unwanted' girls.

2.5.3 Robustness Checks

Pre-trends

A major threat to identification in a difference in differences strategy are trends different from zero in the outcome variable for treatment and control groups. To test for differential pre-trends in under-three malnutrition/stunting for treatment and control groups, I regress an indicator for stunting on the full set of interactions between indicators for treatment ($Treat_t$), female child ($Female_i$) and birth year, with birth order and district fixed effects.[12] The coefficients on the triple-interactions are re-

12. The baseline year is 1992. The pre-ban data comes from a survey conducted in 1992-93. Therefore, I can only conduct this analysis for 1991 and 1992 in the pre-period. I omit 1993 because it only has 13 observations in the control group.

ported in Figure 1. There is no significant divergence between stunting in treatment and control groups in the pre-ban period. However, after the ban, there is an increase in the probability of female stunting.

Clustering Issues

Standard errors which are cluster-robust allow for heteroskedasticity and errors to be correlated within clusters. However, cluster-robust standard errors presume a large number of clusters. If the number of clusters is very few then the standard asymptotic tests tend to over-reject the null hypothesis. I restrict my sample to 20 states to estimate the effect of the ban on female health by age.[13] To correct for asymptotic inconsistencies generated by a small number of clusters, I use the wild cluster bootstrap-t method proposed by Cameron, Galbech, and Miller (2008). The p-values from using the wild cluster bootstrapping method are reported in [] of Tables 2.3, 2.4, and 2.5. The results do not change significantly after using this method.

2.5.4 Falsification Tests

In Table 5, I randomly assign four major Indian states as control states. The outcome variable used is an indicator for stunting. The coefficients are either statistically insignificant or in the direction opposite to what I find in this paper.

2.6 Discussion

This paper examines the effects of putting a legal ban on prenatal sex determination on female child health. My findings suggest that while the probability of female

13. I restrict my sample because of missing pre or post-data.

birth increased as a result of the ban, it adversely affected female health. The reason for this could be that parents responded by investing fewer resources in girls they could have aborted earlier. I tested this hypothesis using the NFHS and find no effects of the policy on the vaccinations received by boys and girls. A caveat of using vaccines as an outcome variable is that these were self-reported outcomes which could bias my results. It is also likely that girls received fewer resources in terms of other investments like food and care that lead to their poor health.

The results of this paper suggest that while sex-selective abortions distort sex-ratios, the inability of parents to choose the gender composition of their children can lead to worse outcomes for girls in the presence of strong son preferences. My findings suggest that a simple ban on prenatal sex detection may not be enough to improve sex ratios in the long run. Poor health during childhood can adversely affect the human capital formation and mortality of women at older ages. Policies that challenge social norms may be more effective in improving sex ratios and reducing gender biases (Gupta et al. 2003; Dhar, Jain, and Jayachandran 2018).

Nevertheless, this paper is not without limitations. First, the causal results should be interpreted with caution because the tariff reductions in India in the year 1993 could be biasing my estimates if they affected boys and girls differently. Second, I only rely on two years of pre-treatment data, which does not help me completely rule out differential trends in male and female health in the treatment and control states before the policy was enacted.

2.7 Figures

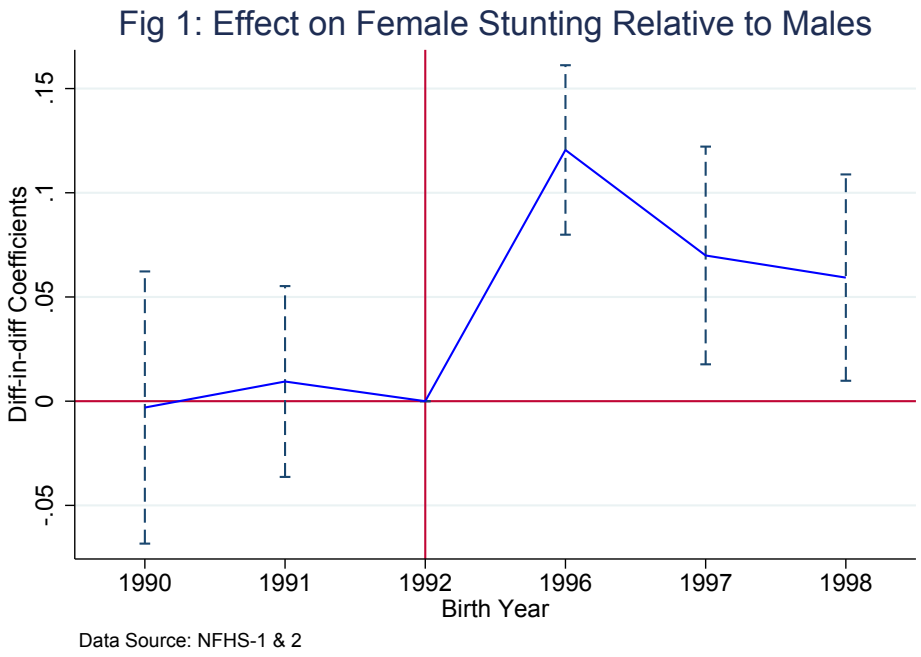


Fig 1: Effect on Female Stunting Relative to Males
Diff-in-diff Coefficients
-.05
0
.05
.1
.15
1990
1991
1992
1996
1997
1998
Birth Year
Data Source: NFHS-1 & 2

2.8 Tables

Table 2.1. Summary Statistics

Variables	Pre-1996		
	Control	Treatment	Difference
WFA_z	-1.92	-1.74	-0.18***
	(1.26)	(1.40)	
HFA_z	-1.81	-1.95	0.14**
	(1.73)	(1.84)	
Malnourished	0.47	0.42	0.05***
	(0.50)	0.50)	
Stunted	0.47	0.50	-0.03**
	(0.50)	(0.50)	
Child's Age (months)	16.83	17.03	-0.2
	(10.06)	(9.98)	
Birth-Order	2.54	2.93	-0.39***
	(1.62)	(1.98)	
Age at first birth	18.4	19.53	-1.13***
	(3.26)	(3.43)	
Mother's years of schooling	2.80	3.00	-0.200*
	(1.90)	(2.05)	
Urban	0.405	0.285	0.12***
	(0.49)	(0.45)	
Hindus	0.74	0.736	0.004
	(0.44)	(0.44)	
Muslims	0.156	0.132	0.024**
	0.36	0.34	
Schedule Caste	0.06	0.131	-0.071***
	(0.24)	(0.34)	
Schedule Tribe	0.107	0.114	-0.007
	(0.31)	(0.32)	

Table 2.2. Effect on Probability of Female Birth

	(1)	(2)	(3)	(4)
VARIABLES	Female	Female	Female	Female
$Treat \times Post96$	0.019***	0.018***	0.023*	0.023*
	(0.005)	(0.0054)	(0.0121)	(0.0123)
District FE	Yes	Yes	Yes	Yes
Birth Year FE	No	Yes	Yes	Yes
State-Specific Trends	No	No	Yes	Yes
Birth-Order FE	No	No	No	Yes
Observations	80,539	80,539	80,539	80,539

This table presents results using specification 2.1 and NFHS-II

Standard errors clustered by state are reported in ().

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

Table 2.3. Effect on z-scores for Height and weight

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	WFA_z	HFA_z	WFA_z	HFA_z	WFA_z	HFA_z	WFA_z	HFA_z
$Treat \times Post96$	0.200**	0.163*	0.196**	0.169*	0.241**	0.262**	0.243**	0.273**
	(0.081)	(0.086)	(0.080)	(0.082)	(0.0938)	(0.108)	(0.100)	(0.114)
	[0.06]	[0.16]	[0.08]	[0.08]	[0.04]	[<0.01]	[0.08]	[<0.01]
$Treat \times Post96 \times Female$	-0.263***	-0.349***	-0.223***	-0.309***	-0.229***	-0.236***	-0.198***	-0.206***
	(0.045)	(0.033)	(0.050)	(0.037)	(0.043)	(0.028)	(0.048)	(0.031)
	[0.02]	[0.02]	[0.02]	[0.02]	[0.02]	[0.02]	[0.02]	[0.02]
Pre-Treat Mean	-1.74	-1.95	-1.74	-1.95	-1.74	-1.95	-1.74	-1.95
State FE	No	No	No	No	Yes	Yes	Yes	Yes
Birth Order FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Birth Year FE	No	No	No	No	Yes	Yes	Yes	Yes
Controls	No	No	Yes	Yes	No	No	Yes	Yes
Observations	38,611	38,611	38,611	38,611	38,611	38,611	38,611	38,611

This table presents results using specification 2.2 and NFHS-I and II

Robust standard errors clustered by state are in ().

p-values from wildboot cluster reported in []

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

Table 2.4. Effect on Probability of Being Stunted and Malnourished

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	Mal	Stunting	Mal	Stunting	Mal	Stunting	Mal	Stunting
$Treat \times Post96$	-0.059***	-0.054***	-0.058***	-0.057***	-0.073***	-0.080***	-0.073***	-0.084***
	(0.016)	(0.014)	(0.017)	(0.014)	(0.019)	(0.021)	(0.022)	(0.023)
	[0.06]	[0.02]	[0.06]	[0.02]	[0.02]	[0.02]	[0.02]	[0.02]
$Treat \times Post96 \times Female$	0.081***	0.096***	0.066***	0.085***	0.069***	0.067***	0.058***	0.060***
	(0.013)	(0.006)	(0.015)	(0.007)	(0.012)	(0.007)	(0.014)	(0.008)
	[<0.01]	[<0.01]	[<0.01]	[<0.01]	[<0.01]	[<0.01]	[<0.01]	[<0.01]
Pre-Treat Mean	0.42	0.50	0.42	0.50	0.42	0.50	0.42	0.50
State FE	No	No	No	No	Yes	Yes	Yes	Yes
Birth Order FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Birth Year FE	No	No	No	No	Yes	Yes	Yes	Yes
Controls	No	No	Yes	Yes	No	No	Yes	Yes
Observations	38,611	38,611	38,611	38,611	38,611	38,611	38,611	38,611

This table presents results using specification 2.2 and NFHS-I and II

Robust standard errors clustered by state are in ().

p-values from wildboot cluster reported in []

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

Table 2.5. Effect on Stunting by Age and Gender

VARIABLES	Female Stunting	Female Stunting	Male Stunting	Male Stunting
$Treat \times Post96$	-0.110***	-0.080***	-0.108***	-0.106***
	(0.021)	(0.025)	(0.026)	(0.015)
	[0.02]	[0.02]	[0.02]	[0.02]
$Treat \times Post96 \times Age1$	0.083***	0.080***	0.017	-0.005
	(0.026)	(0.023)	(0.020)	(0.017)
	[0.04]	[0.06]	[0.4]	[0.84]
$Treat \times Post96 \times Age2$	0.290***	0.243***	0.149***	0.137***
	(0.026)	(0.021)	(0.038)	(0.032)
	[<0.01]	[<0.01]	[<0.01]	[<0.01]
Birth Order FE	Yes	Yes	Yes	Yes
State FE	Yes	No	Yes	No
District FE	No	Yes	No	Yes
Birth-Year FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	18,821	18,821	19,790	19,790

This table presents results using specification 2.3 and NFHS-I and II

Robust standard errors clustered by state in ().

p-values from wildboot cluster reported in []

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

Table 2.6. Falsification Test-Stunting

	(1)	(2)	(3)	(4)
VARIABLES	Gujarat	Karnataka	Rajasthan	Uttar Padesh
$Treat \times Post96 \times Female$	0.008	0.015	-0.042***	-0.005
	(0.011)	(0.010)	(0.010)	(0.011)
District FE	Yes	Yes	Yes	Yes
Birth Order FE	Yes	Yes	Yes	Yes
Birth Year FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	38,611	38,611	38,611	38,611

This table presents results using specification 2.3 and NFHS-I and II

Robust standard errors clustered by state in ().

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

Chapter 3

The Effects of Communal Violence on Women's Marital Outcomes: Evidence from Hindu-Muslim Riots in India

Devaki Ghose and Divya Pandey

3.1 Introduction

The disruptive impacts of violent conflicts are seldom gender-neutral. Women suffer in very specific ways during times of conflict as existing law and order break down, making them more susceptible to sexual violence and abuse. The Hindu-Muslim riots of 2002 in the Indian state of Gujarat were one such event in which not only were several killed, injured, and displaced, sexual violence against women

had become its key feature (Jha and Farasat 2016). While the direct consequences of violent conflicts on women include rapes and other forms of abuse, its indirect consequences could involve changes in their marital outcomes and agency (La Mattina 2017; Gutierrez and Gallegos 2016). In this paper, we examine the effects of the intercommunal violence in Gujarat in the year 2002 on women's age at marriage and their postmarital socioeconomic status.

To examine the effects of the Gujarat riots of 2002 on women's marital outcomes, we use a retrospective state-year level dataset from the fourth round of the National Family Health Surveys (NFHS-IV) and a difference-in-differences strategy. Our empirical strategy relies on variation over time in marriages that took place before and after 2002, and the state-wise variation in the occurrence of riots. We also use the event study approach to observe differential trends in women's age at marriage between Gujarat and the other Indian states.

The difference-in-differences estimates suggest that age at marriage declined for women in Gujarat relative to other Indian states after the riots of 2002. The probability of women getting married before the age of 18 increased by 4.4 percentage points or by 14 percent after the riots in Gujarat compared with the control states. Also, the probability of getting married before age 18 is relatively higher among Muslim women who were the main victims of sexual violence during the riots.

Our event study estimates suggest that the effects are prominent two years after the intercommunal violence and the gap in terms of women's age at marriage between Gujarat and the other Indian states has been increasing over time. Our results are robust to using synthetic control methods or restricting the control group to states that share their borders with Gujarat. We do not find conclusive evidence on the effect of the riots on men's age at marriage.

Next, we examine the effects of communal violence in Gujarat on women's post-marital socioeconomic status. Existing literature has established that marrying young or exposure to conflict can decrease women's bargaining power within the marriage (Field and Ambrus 2008; Jensen and Thornton 2003). We find that women married after the riots had fewer years of education, were more likely to experience domestic violence, had less control over their income, and had lower autonomy in terms of making decisions related to household purchases and visits.

Several mechanisms could explain our results. First, in the Indian culture, rape victims face the risks of being socially ostracised, which could worsen their position in the marriage market. Therefore, the fear inflicted by sexual violence on victims and their families could lead to early marriages of women. Second, distortions in sex ratios could also affect women's position in the marriage market (Becker 1981; Teso 2018; Abramitzky, Delavande, and Vasconcelos 2011). Third, negative income shocks caused by the riots can also alter women's marriage age due to the payment of dowry or bride prices at the time of marriages (Corno, Hildebrandt, and Voena 2020; Khanna and Kochhar 2020). We are in the process of exploring mechanisms that drive our results and will include them in future versions of the paper.

This study fits into the broad literature analyzing the effects of violent conflicts. Several studies have analyzed the effects of conflicts on economic growth (Abadie and Gardeazabal 2003); human capital formation (Camacho 2008; Duque 2017); and women's employment rates and agency (Goldin and Olivetti 2013; Zárate-Barrera and Rogall 2020). Other papers show that ethnic violence affects electoral votes (Iyer and Shrivastava 2018); and long-term biases in terms of lending behavior and judicial results (Hjort 2014); Shayo and Zussman 2011; Shayo and Zussman 2017; Fisman et al. 2020). This paper adds to the literature by examining the effects of intercommunal

Hindu-Muslim riots on women's marital outcomes in India.

Our paper also interacts with the literature on the determinants of early marriages of women. Most studies examine the effects of income shock associated with natural disasters on women's age at marriage (Corno and Voena 2016; Corno, Hildebrandt, and Voena 2020; Khanna and Kochhar 2020). Corno, Hildebrandt, and Voena (2020) show that the interaction of social norms such as bride price and dowry, and local income shocks caused by droughts increase the incidence of child marriage in Sub-Saharan African and reduce the same in India. While income shocks can be crucial drivers of marital outcomes of women in the case of riots, fears associated with sexual violence can also determine the incidence of child marriages. Our paper seeks to contribute to this literature by examining the effects of communal riots on women's age at marriage, and also explore mechanisms that are beyond income shocks, for example, sexual violence against women.

The rest of this paper is structured as follows. Section 3.2 discusses the context. Sections 3.3 and 3.4 discusses the data and the empirical strategy. Sections 3.5 and 3.6 discuss our main results and their robustness. Section 3.7 concludes.

3.2 Background: Gujarat Riots of 2002

India has had a history of communal riots whereby violence was perpetrated by both Hindus and Muslims. The Hindu-Muslim violence reached its peak during the partition of India based on religion in 1947 and then settled down to periodic clashes centered on religious festivals and places of worship. These clashes are often determined by the state and nature of intercommunal civic engagements in the case of India (Varshney 2008; Varshney and Gubler 2012; Wilkinson 2009).

The Gujarat riots of 2002 erupted after a carriage carrying Hindu activists was set on fire in Godhra on February 27, 2002. While there are several theories that circulate on who set the train on fire, the dominant one is that a Muslim mob set the train on fire due to an altercation between a Muslim tea seller and the Hindu activists, and possibly the attempted molestation of the tea seller's daughter (Khanna 2008; Simpson 2017). Out of the 59 people killed on the train, several of them were women and children.

Followed by this incident there was three-day-long violence perpetrated by Hindu mobs against the minority Muslim population. According to official figures, the riots killed 790 Muslims and 254 Hindus. However, unofficial statistics suggest that close to 2000 Muslims were killed over the next three months, and many others were injured (Report 2005). More than 200,000 Muslims were displaced because of the looting and burning of their homes and businesses (Srivastava 2011). There were further outbreaks of statewide violence for the next year.

Sexual violence against women was a key feature of the Gujarat riots of 2002. Several women, mostly Muslim, were raped and killed. Survivors reported horrific instances of sexual violence such as rapes, gang-rapes, forced nudity, torching, mutilation of the stomach and sexual organs, and insertion of objects into bodies (Report 2002). According to Jha and Farasat (2016), Muslim women were not the only victims of sexual violence in the Gujarat carnage of 2002, Hindus were also targeted in some cases. Many of these cases were not reported to the police because of the fear of further attacks, stigma, and ostracism.

During episodes of communal violence, women are often targeted because they are viewed as transmitters of the culture of a particular community. Exposure to such brutalities can have deep physical and emotional effects on the victims, their families,

and their communities, and thereby their marital decisions. The fear of sexual violence and stigmatization can lead to early marriages of women. In this paper, we study the effects of the Gujarat riots of 2002 on women's marital outcomes.

3.3 Data

This study uses data from the fourth round of the National Family Health Surveys (NFHS-IV), based on the Demographic and Health Surveys (DHS), conducted in India during 2014-15. These are nationwide repeated cross-sectional surveys that are representative at the state level. The survey reports information on the year of marriage, age at marriage, age at cohabitation, age at consummation of marriage, and age at first birth of ever-married women between ages 15 and 49. Using this information, we construct a retrospective state-year level dataset of marriages that took place between 1990 and 2014 across Indian states. This allows us to compare outcomes for women across Indian states married before and after the Gujarat riots of 2002. For a small subsample of women, the survey also reports information on their socioeconomic status, which we use to analyze the effect of the Gujarat riots on women's post-marital outcomes.

The variables used for analyzing the effects of the riots on women's socioeconomic outcomes include women's years of education, indicators for whether they experienced physical or emotional violence by husband. We also include indicators for women's employment status, autonomy over their income, whether they participate in decisions related to household purchase decisions, and visiting relatives and friends.

Table 3.1 reports the sample means of key variables for women married before 2002 across Gujarat and the other Indian states. There are some differences between

the characteristics of women married before 2002 in Gujarat and the other Indian states. For example, in terms of religion, Gujarat has a higher fraction of the Hindu population and a lower fraction of Muslims. Gujarat is also wealthier on average than the other states. The average age at marriage for women belonging to Gujarat or other Indian states is approximately 18 years. While the fraction of women married before age 18 is lower in Gujarat compared with other Indian states, the difference between the two groups is not statistically significant.

3.4 Empirical Strategy

We use a difference-in-differences strategy to isolate the effect of the Hindu-Muslim riots on women's marital outcomes. In particular, we rely on spatial variation due to the riots that erupted in the Indian state of Gujarat, and the temporal variation in marriages before and after the riots of 2002. We first estimate the following equation:

$$Y_{ist} = \alpha + \beta Post_t * Riot_s + X_{ist}^{'}\theta + Year_{FE} + District_{FE} + \epsilon_{ist} \tag{3.1}$$

where Y_{ist} is the outcome of interest such as age at marriage, an indicator for marriage before age 18, age at cohabitation, years of education, a measure of autonomy or domestic violence for woman i in state s married in the year t; $Post_t$ equals 1 if the woman was married after the year 2002 and 0 otherwise; $Riot_s$ is an indicator for residence in the state of Gujarat;[1] and X_{ist} is a vector of controls that includes indicators for wealth, religion, caste, residence in a rural area, the gender of the household

1. Since the survey only provides information on the state of residence, we are unable to account for women who migrated to another state due to marriage. However, this should not significantly affect our results because interstate migration in India is very low (Biswas 2017).

head, and household's wealth. We also include year fixed effects and district fixed effects. The coefficient of interest, β, measures the effect of the Hindu-Muslim riots of 2002 in Gujarat on women's marital outcomes.

The difference-in-differences estimate relies on the assumption of parallel trends. In equation 3.1, β is the causal effect of the communal violence under the assumption that women's outcomes in the Indian states other than Gujarat followed similar trends to those in Gujarat before 2002. We check for differential trends in women's age at marriage and rate of child marriages by estimating the following event-study equation:

$$Y_{ist} = \alpha + \sum_{t=1991,\neq 2002}^{2014} \beta_t Year_d * Riot_s + X_{ist}^{'}\theta + Year_{FE} + District_{FE} + \epsilon_{ist} \quad (3.2)$$

where $Year_d$ are year-specific dummies and the omitted year is the year 2002; β_t represent the coefficients on interactions between the year dummies and the $Riot_s$ dummy. Another advantage of estimating the difference-in-differences coefficients by year is that it enables us to investigate if the effect of the riots on women's outcomes are temporary or persist over time.

Next, we examine the effects of the riots by religion. We expect our results to be more pronounced for Muslim women because during the Gujarat riots of 2002 as violence was mostly perpetrated against the Muslim community. The following equation identifies the effect of the riots by religion:

$$\begin{aligned} Y_{ist} = \alpha + \gamma Post_t * Treat_s * Muslim_i + \beta Post_t * Riot_s + \delta Post_t * Muslim_i \\ + \phi Riot_s * Muslim_i + X_i^{'}\theta + Year_{FE} + District_{FE} + \epsilon_{ist} \end{aligned} \quad (3.3)$$

where $Muslim_i$ is equal to 1 if woman i belongs to the Muslim community. The coefficient of interest, γ identifies the additional effect of the riots on Muslim women's marital outcomes, relative to the outcomes of women of all other regions. We cluster all our our standard errors at the state level or at the state-year level.

3.5 Results

In this section, we discuss the effects of the Hindu-Muslim riots of Gujarat in 2002 on women's age at marriage, and their post-marital social and economic status.

3.5.1 Effects on Women's Marriage Age

Using data from NFHS-IV and specification 3.1, Table 3.2 presents the estimates of the Gujarat riots on women's age at marriage, probability of child marriage, age at cohabitation, age at consummation, and age at first birth. Columns (1) and (2) suggest that the age at marriage of women declined after the riots, and the probability of marriage before the age of 18 increased by 4.4 percentage points or by 14 percent compared with the rest of India. Columns (3) and (4) indicate that the age at cohabitation and age at first sex also declined after the riots. The estimates in the first four columns of Table 3.2 are significant at the 1 percent level. According to column (5) of Table 3.2, while statistically insignificant, the effect of the violence on women's age at first birth is negative.

Figures 3.1 and 3.2 present coefficients using specification 3.2. Figure 3.1 suggests that the age at marriage decreased for women married after the Gujarat riots of 2002. Figure (2) suggests the probability of child marriage increased for women married after 2002. Nevertheless, these effects are not immediate and they are prominent

after the year 2004. The coefficients on the years before the riots suggest that there were some differences between women's marriage age between Gujarat and the other Indian states even before 2002. In figure 3.3, we examine the effects of the riots on men's age at marriage and find inconclusive results.

3.5.2 Effects on Marriage Age by Religion

In Table 3.3 we present results by religion using specification 3.3. We expect the effect of the riots to be larger on Muslim women because violence was mostly perpetrated against the Muslim community during the Hindu-Muslim riots of 2002. Column (1) of Table 3.3 shows that the effect of the riots on Muslim women's age at marriage is negative and statistically insignificant. According to column (2) Muslim women are 3.1 percentage points more likely to get married before the age of 18 as a result of the riots.

3.5.3 Effects on Women's Education

In this section, we examine the effects of communal violence in Gujarat on women's education outcomes. Women who are married at a younger age are more likely to enter marriages with fewer years of education (Field and Ambrus 2008). Table 3.4 presents estimates of the effect of the riots on women's education using specification 3.1. According to column (1) of Table 3.4, women who were married after the riots in Gujarat have 5 percent fewer years of education than women in the rest of the country. In terms of years, women have 0.34 fewer years of education in Gujarat. Columns (3) and (4) show that women married after the riots in Gujarat are 1.8 and 1.5 percentage points less likely to complete secondary and higher education.

3.5.4 Effects on Post-Marital Social and Economic Outcomes

Next, we estimate the effects of the Gujarat riots on women's post-marital social and economic outcomes using specification 3.1. It has been established in the literature that early marriage of women is associated with lower autonomy and bargaining power within the marriage (Jensen and Thornton 2003). Columns (2) and (3) of Table 3.5 suggest that women married after the riots in Gujarat were more likely to suffer emotional and physical violence from the husband. Column (5) suggests that women are less likely to make decisions about visits to friends and relatives. In Table 3.6 we present the effects of the riots on women's economic status after marriage. Columns (1) and (2) of Table 3.6 suggest that women who were married after 2002 were less likely to be employed, and were also less likely to control their income. Column (4) suggests that these women also had lower decision-making power in big household purchases. Nevertheless, the coefficient on women's control over some part of income is positive and significant in column (3) of Table 3.6.

3.6 Robustness Checks

3.6.1 Gujarat and the Bordering States

As evident from Table 3.1, Gujarat is significantly different from other Indian states in terms of cultural, social, and economic factors, which are important determinants of women's age at marriage and agency. Many of these factors are unobserved or lack credible proxies. In this section, we restrict our control group to the states that share their borders with the riots that affected Gujarat. This approach has also been adopted in the previous literature to restrict the control group to regions that

are most similar to the treatment region (Anukriti 2018).

Figures 3.4 and 3.5 show the event-study coefficients using specification 3.2 after restricting the control group to the bordering states of Gujarat, which are Madhya Pradesh, Maharashtra, and Rajasthan. Figure 3.4 shows that there are no significant differences in age at marriage between Gujarat and its neighboring states before the riots of 2002. However, women's age at marriage is consistently declining in Gujarat relative to its neighboring states. According to figure 3.5, the probability of women getting married before the age of 18 increased after the Gujarat riots.

3.6.2 Synthetic Controls Approach

The results from the event study approach (Figures 3.2 and 3.2) suggest that there were differential trends between Gujarat and the control states even before the riots of 2002. This could cause our difference-in-differences estimator to be biased. To address this concern, we examine the effects of the Gujarat riots by employing the synthetic controls approach suggested by Abadie and Gardeazabal (2003). Using a combination of other Indian states, we first construct a synthetic-Gujarat that best approximates the characteristics of women in Gujarat before the riots of 2002. We then use the post-riot outcomes for the synthetic-Gujarat to estimate the outcomes that would have been observed in Gujarat in the absence of the riots of 2002. Next, we compare the outcomes of synthetic-Gujarat to actual Gujarat after 2002.

For the synthetic controls method, we collapse the individual-level data in a state-year panel. To approximate the outcomes (age at marriage and proportion of child marriages) at the state level, we use the proportion of women who reside in rural areas, reside in households with male heads, belong to rich or poor households, are Hindus,

and are scheduled castes/scheduled tribes/other backward classes. The results from the synthetic control methods are presented in figures 3.6 and 3.7. Figure 3.6 suggests that the age at marriage in Gujarat is identical to that in synthetic-Gujarat before the riots of 2002, and begins to decrease two years after the riots. These findings are similar to what we find using the event study method in figure 3.1. The probability of child marriage also increased after 2002 (Figure 3.7).

3.7 Conclusion

In this paper, using the 2002 Gujarat Hindu-Muslim riots, we show that communal violence can have long-term socioeconomic effects on women, much beyond the immediate effects of sexual violence. We show that years after the riots, women in Gujarat continue to be married off early. Early marriages are often associated with worse socioeconomic outcomes for women such as fewer years of education and lower bargaining power within the household. We show that in post riots Gujarat, women have a lower probability of finishing secondary and higher education. They enter marriage with fewer years of education and have a lower probability of continuing their education after marriage. Lower education is often associated with worse employment outcomes and bargaining power within households. We also find that in post communal riots Gujarat women not only have a lower probability of employment, but also have lower autonomy in social and economic decisions within the household.

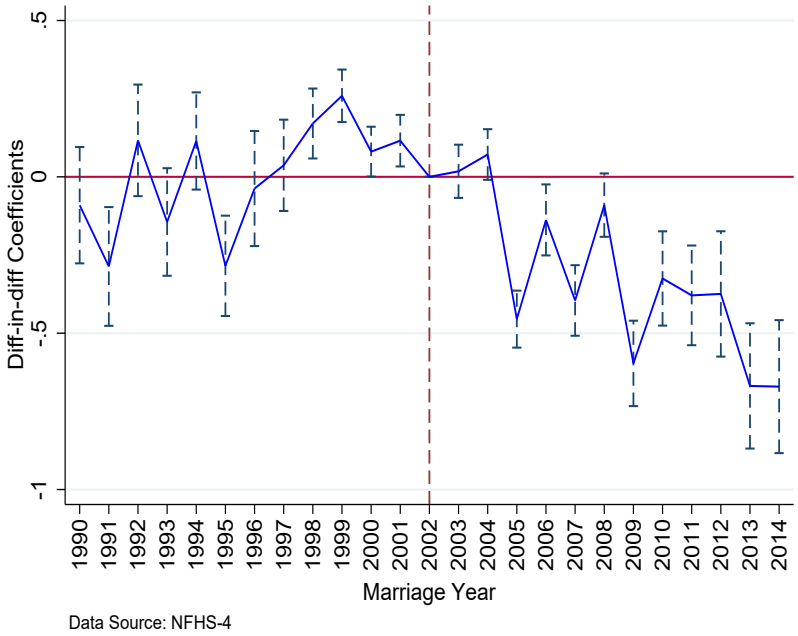
A priori, one could expect these effects to be driven by worse socioeconomic outcomes for Muslim women, as the Muslim community was the worst affected group. However, we find that these effects are not only driven by worse socioeconomic outcomes for Muslim women but for the overall community. This points to the fact

that communal violence is a threat to the larger community who live in riot exposed regions, even though in most instances they are targeted against a particular religion or group. Our results suggest that policy makers should be mindful of the numerous ways communal violence affect women, and should provide incentives and resources to women to continue their education and delay marriage in riot affected regions.

In future versions of the paper, we plan to analyze the mechanisms through which communal violence affects women's age at marriage. In particular, to test whether negative income shocks caused by riots affect women's marriage age, we will look at the amount and instances of dowry and bride-prices paid at the time of marriages before and after after the riots. It is harder to pinpoint if social stigma associated with sexual violence precipitated early marriage for women. Nevertheless, we plan to collect detailed data on reported crimes ranging from theft, murderer, and sexual violence. Analyzing how reported rape and sexual violence changed in post riots Gujarat relative to other states and other types of crimes will give us an idea whether social stigma, as manifested by less reporting of rape relative to other crimes, played a role in early marriages of women in Gujarat.

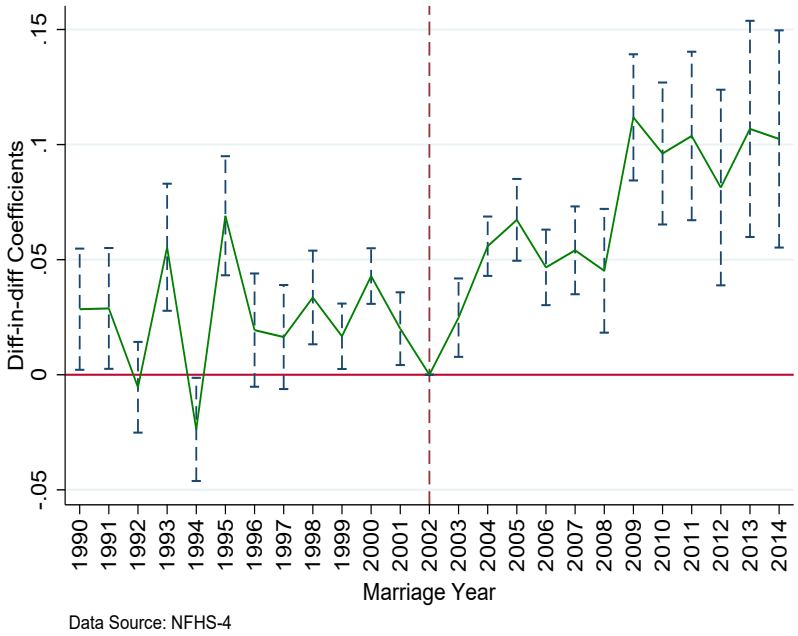
3.8 Figures

Figure 3.1. Effect of Gujarat Riots on Women's Marriage Age



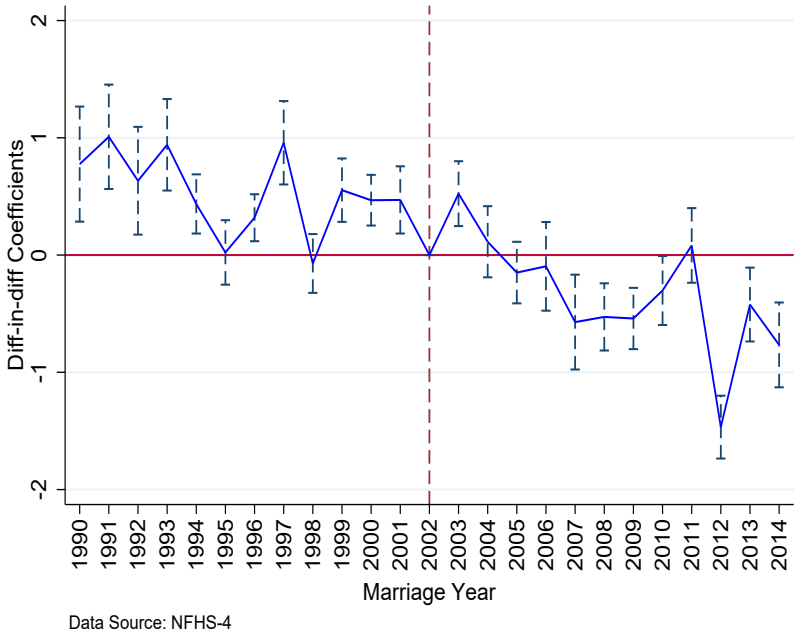
Note: This figure plots the difference-in-differences estimates from specification 3.3 using NFHS-IV. The outcome variable is women's age at marriage. Standard errors were clustered at the state level.

Figure 3.2. Effect of Gujarat Riots on Women's Marriage Before Age 18



Note: This figure plots the difference-in-differences estimates from specification 3.3 using NFHS-IV. The outcome variable is an indicator for women's marriage before age 18. Standard errors were clustered at the state level.

Figure 3.3. Effect of Gujarat Riots on Men's Marriage Age



Note: This figure plots the difference-in-differences estimates from specification 3.3 using NFHS-IV. The control states only include the bordering states of Gujarat, which are Maharashtra, Rajasthan and Madhya Pradesh. The outcome variable is men's age at marriage. Standard errors were clustered at the state level.

3.9 Tables

Table 3.1. Sample Characteristics of Women Married Before 2002

	(1) Other States	(2) Gujarat	(2) - (1)
Control Variables:			
Hindu	0.75	0.92	0.16***
Muslim	0.12	0.07	-0.05**
SC/ST/OBC	0.75	0.74	-0.01
Male Household Head	0.87	0.89	0.02*
Wealth	2.90	3.35	0.45**
Rural Resident	0.71	0.64	-0.07***
Outcome Variables:			
Women's Age at Marriage	17.72	17.98	0.26
Women' Child Marriage	0.49	0.43	-0.05
Women' Age at Cohabitation	17.97	18.24	0.27
Women' Age at First Intercourse	18.03	18.13	0.09
Women's Age at First Birth	20.37	20.59	0.22*
Women's Years of Education	4.71	5.17	0.46
Husband Controls	0.48	0.47	-0.02
Husband is Emotionally Violent	0.14	0.11	-0.03**
Husband is Physically Violent	0.33	0.21	-0.12***
Makes Health Decisions	0.78	0.78	-0.01
Employed	0.90	0.86	-0.04***
Controls Own Income	0.84	0.79	-0.05***
Makes Household Purchase Decisions	0.78	0.79	0.02*
Makes Decisions About Visits	0.78	0.81	0.02*
Men's Age at Marriage	22.35	21.40	-0.95**

Note: This table reports simple differences in means of the characteristics of women living in Gujarat and other Indian states using data from the NFHS-IV
* Significant at 10%; ** Significant at 5%; *** Significant at 1%.

Table 3.2. Effect of Gujarat Riots on Women's Marriage Outcomes

	(1) Age at Marriage	(2) Married Before Age 18	(3) Age at Cohabitation	(4) Age at First Sex	(5) Age at First Birth
Post X Riot	-0.330***	0.044***	-0.271***	-0.398***	-0.030
	(0.085)	(0.016)	(0.074)	(0.062)	(0.083)
Observations	415441	425254	405697	418219	389744
R-squared	0.248	0.164	0.216	0.228	0.128
Control Average	19.57	0.312	19.68	19.52	21.34
District Fixed Effects	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes

Note: This table reports coefficients from specification 3.1. Standard errors are clustered at the state level. Control Variables include indicators for religion, sc/sct/obc, rural residence, wealth quintiles, and male houshold head.
* Significant at 10%; ** Significant at 5%; *** Significant at 1%.

Table 3.3. Effect of Gujarat Riots on Women's Marriage Outcomes, by Religion

	(1) Age at Marriage	(2) Married Before Age 18	(3) Age at Cohabitation	(4) Age at First Sex	(5) Age at First Birth
Post X Riot	-0.326***	0.043***	-0.261***	-0.395***	-0.021
	(0.081)	(0.015)	(0.070)	(0.056)	(0.080)
Post X Riot X Muslim	-0.155	0.031*	-0.149	-0.065	-0.160
	(0.234)	(0.018)	(0.243)	(0.205)	(0.253)
Observations	415441	425254	405697	418219	389744
R-squared	0.247	0.164	0.216	0.228	0.128
Control Average	19.47	0.323	19.49	19.41	21.04
District Fixed Effects	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes

Note: This table reports coefficients from specification 3.3. Standard errors are clustered at the state level. Control Variables include indicators for religion, sc/sct/obc, rural residence, wealth quintiles, and male houshold head.
* Significant at 10%; ** Significant at 5%; *** Significant at 1%.

Table 3.4. Effect of Gujarat Riots on Women's Educational Outcomes

	(1) Years of Education	(2) At least Completed Primary School	(3) At Least Completed Secondary School	(4) Completed Higher Education
Post X Riot	-0.388***	-0.016	-0.018**	-0.016*
	(0.096)	(0.010)	(0.008)	(0.008)
Observations	423957	423957	423957	423957
R-squared	0.483	0.330	0.369	0.211
Control Average	7.405	0.781	0.648	0.130
District Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Note: This table reports coefficients from specification 3.1. Standard errors are clustered at the state level. Control Variables include indicators for religion, sc/sct/obc, rural residence, wealth quintiles, and male houshold head.
* Significant at 10%; ** Significant at 5%; *** Significant at 1%.

Table 3.5. Effect of Gujarat Riots on Women's Social Status Post-Marriage

	(1) Controlled by Husband	(2) Emotional Violence	(3) Physical Violence	(4) Makes Health Decisions	(5) Makes Decisions about Visits
Post X Riot	0.003	0.028***	0.045***	-0.001	-0.019**
	(0.004)	(0.003)	(0.005)	(0.006)	(0.007)
Observations	55133	55162	55162	70700	70700
R-squared	0.135	0.0557	0.124	0.0643	0.0760
Control Average	0.474	0.118	0.280	0.740	0.730
District Fixed Effects	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes

Note: This table reports coefficients from specification 3.1. Standard errors are clustered at the state level. Control Variables include indicators for religion, sc/sct/obc, rural residence, wealth quintiles, and male houshold head.
* Significant at 10%; ** Significant at 5%; *** Significant at 1%.

Table 3.6. Effect of Gujarat Riots on Women's Economic Status Post-Marriage

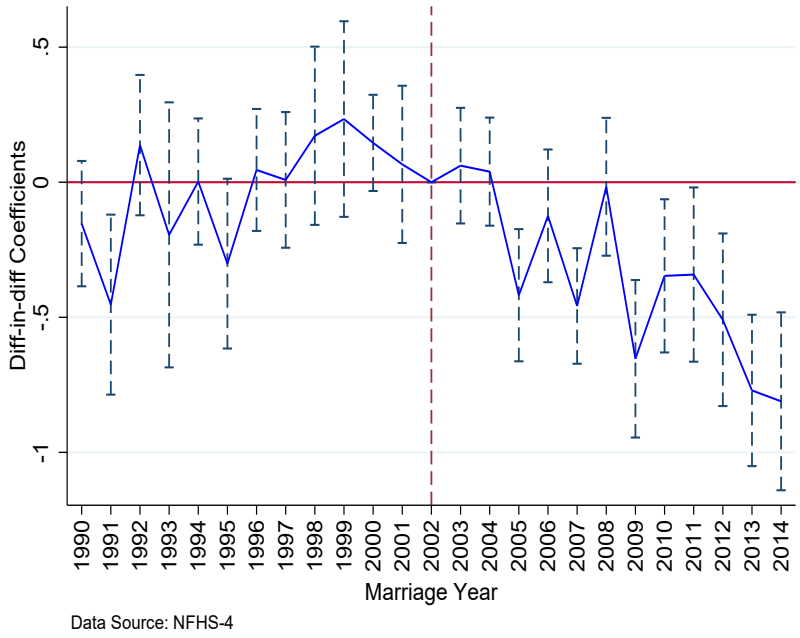
	(1) Employed in Last 12 months	(2) Controls own Income	(3) Solely Controls Part of Income	(4) Makes Household Purchase Decisions
Post X Riot	-0.017***	-0.022***	0.022***	-0.050***
	(0.002)	(0.007)	(0.007)	(0.004)
Observations	423957	16043	74251	70700
R-squared	0.0208	0.108	0.103	0.0687
Control Average	0.871	0.817	0.401	0.717
District Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Note: This table reports coefficients from specification 3.1. Standard errors are clustered at the state level. Control Variables include indicators for religion, sc/sct/obc, rural residence, wealth quintiles, and male houshold head.
* Significant at 10%; ** Significant at 5%; *** Significant at 1%.

Appendix

3.A Event Study: Gujarat and Its Bordering States

Figure 3.4. Effect of Gujarat Riots on Women's Marriage Age



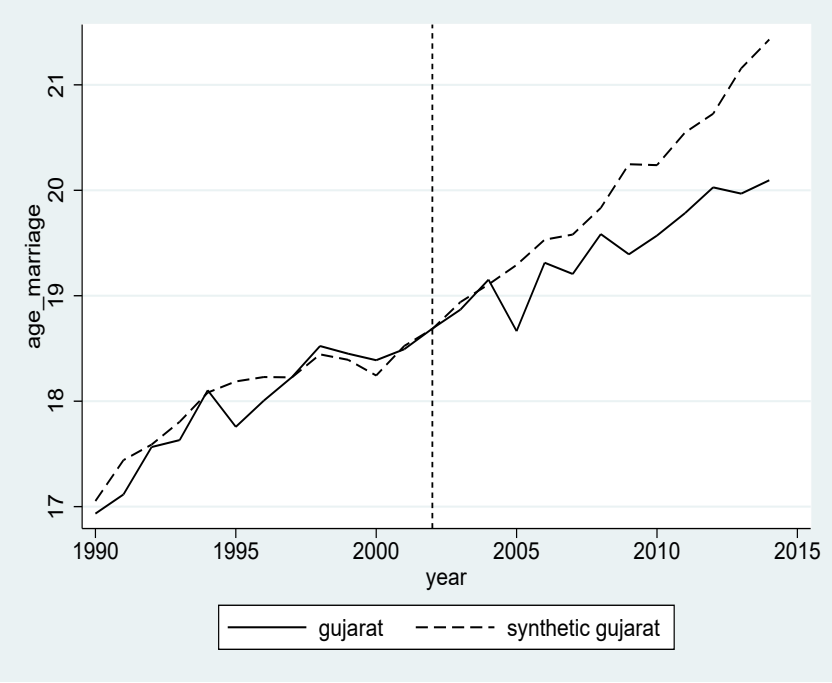
Note: This figure plots the difference-in-differences estimates from specification 3.3 using NFHS-IV. The outcome variable is women's age at marriage. The control states only include the bordering states of Gujarat, which are Maharashtra, Rajasthan and Madhya Pradesh. Standard errors were clustered at the state-year level.

Figure 3.5. Effect of Gujarat Riots on Women's Marriage Before Age 18

Note: This figure plots the difference-in-differences estimates from specification 3.3 using NFHS-IV. The outcome variable is an indicator of women's marriage before age 18. The control states only include the bordering states of Gujarat, which are Maharashtra, Rajasthan and Madhya Pradesh. Standard errors were clustered at the state-year level

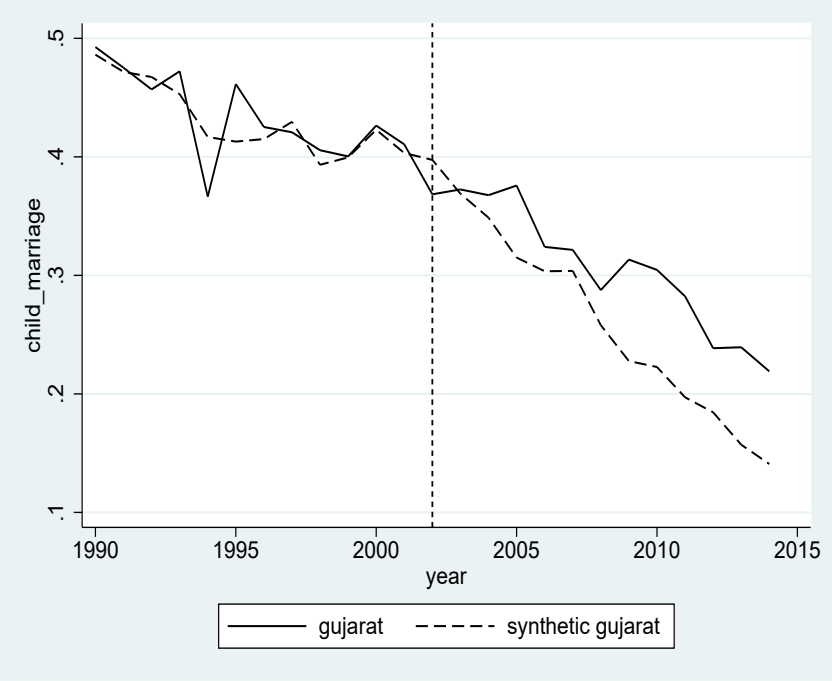
3.B Synthetic Controls

Figure 3.6. Synthetic Controls: Effect of Gujarat Riots on Women's Age at Marriage



Note: This figure plots the estimates using synthetic controls approach and NFHS-IV. The outcome variable is women's average age at marriage.

Figure 3.7. Synthetic Controls: Effect of Gujarat Riots on Women's Marriage Before Age 18



Note: This figure plots the estimates using synthetic controls approach and NFHS-IV. The outcome variable is the fraction of women who got married before age 18.

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