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Is Child Marriage an Unintended Consequence of a Ban on Sex-Selective Abortion?

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Abstract

The introduction of prenatal sex-detection technologies has led to a surge in the abortion of female fetuses in South and East Asia. Bans on sex-selection are typically introduced to combat this practice, but they may have unintended consequences for girls due to persistent norms of son preference. This paper identifies an increase in child marriage as one such consequence in the context of India. I exploit a natural experiment created by Maharashtra's implementation of a ban on prenatal sex-determination while it remained legal in neighboring states. I find that treated girls born after the policy's implementation were significantly more likely to be married as children. The effects are driven by poorer households, who face greater barriers to evading the ban. The findings are robust to restricting the sample only to districts either side of the Maharashtra border. I provide empirical evidence that dowry burdens tend to be lower for younger brides, suggesting that child marriage may constitute an alternative coping strategy to abortion against the costliness of raising daughters.

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1 Introduction

While over a third of women worldwide live in countries where sex-selective abortion is prohibited, the broad implications of these policies remain poorly understood (Greenhalgh (2023); Government of India (1994); World Bank (2024)). Concentrated in South and East Asia, bans on sex-selection typically aim to curb the growing use of prenatal sex-detection technologies like ultrasound to terminate the pregnancies of female over male foetuses. Female-specific abortion is often seen as an infringement of gender equality, with policy-makers citing these concerns when implementing such bans (Government of India 1994). However, a growing body of research, focused on the inability of policy to change persistent gender norms, suggests that such policies may unintentionally worsen outcomes for girls and women (Bhalotra, Brulé, and Roy (2020), Khalifa (2022)). Previously documented unintended consequences of bans on sex-selection include increased female infant mortality rates (Dasgupta and Sharma 2024) and worsened educational attainment for girls (Rastogi and Sharma 2022).

A plausible but under-explored consequence of prohibiting sex-selective abortion is a rise in child marriage rates. Like sex-selective abortion, child marriage can serve as a strategy for parents to mitigate the financial burden of having a daughter when girls become the responsibility of the groom and his family upon marriage (Corno, Hildebrandt, and Voena 2020). Further, in dowry-paying societies, marrying off daughters early can reduce dowry payments, which are generally lower for younger brides (Chowdhury 2010). Child marriage is profoundly dis-empowering, causally linked to higher rates of intimate partner violence (Roychowdhury and Dhamija 2021), school dropout (Field and Ambrus 2008), and maternal mortality (Nour 2006). Given the expansive coverage of sex-selection bans, it is important to examine their broader social consequences, including potential effects on child marriage.

This paper investigates whether a law prohibiting sex-selective abortion causes an increase in child marriage among surviving girls. I focus on India which, like several countries, has both widespread sex-selective abortion usage as well as high child marriage rates (UNFPA (2020a); UNICEF (2019)). I exploit a natural experiment created by the plausibly exogenous timing of Maharashtra's state-level ban on sex-determination in 1988, eight years before a national-level ban took effect. The bordering states Gujarat, Madhya Pradesh, Chhattisgarh, Telangana, Andhra Pradesh, Goa, and Karnataka, having received no such ban during the study period, serve as the control. To explore the impact on births, I use an event-study design to compare the probability of a female birth in the treated state before and after the introduction of the ban to that of neighboring states using district-level census data. I then compare the marriage outcomes of women who were born before and after the passing of the ban in the treated state to that of women in the control states. I use a sample of over 238,000 women born between 1975 and 1995 who were over the age of 18 at the time of interview from the 2021 and 2016 waves of the Indian National Family and Health Survey.

I find that the ban on sex-determination led to both the additional births of girls and higher child marriage rates. Analysis of the sex-ratio suggests that the ban caused the additional births of 36-59 additional girls per 1,000 boys in the treated state, in line with Nandi and Deolalikar (2013), who find similar effects due to the nationwide ban in 1996. Difference-in-differences specifications show that the ban on sex-selection led to a 4.8 percentage point (14.8%) rise in child marriage before 18 on average. Event study analysis shows that statistically significant effects are detected for girls born two or more years after implementation. The ban caused a 7.2 percentage point (22.2%) increase in marriage before age 18 among women born seven years after the ban, the final year of the study window. The effect is robust to changing the age threshold to define child marriage. Among the ever-married sample, the ban caused the age at first marriage to decrease by 0.25 years on average. Consistent with marriage and school dropout often coinciding, the ban caused secondary school completion

to decline by 16.8% for women in the final cohort of the study period. The effect on child marriage is concentrated among Hindu women, consistent with the practice of dowry being most strongly practiced among the Hindu community in India (Bhalotra, Chakravarty, and Gulesci (2020); Bloch, Vijay Rao, and Desai (2004); Botticini and Siow (2003)). Using data on wealth quintiles, I find that the effect is most pronounced among poorer households in the bottom 60% of India's wealth distribution. Analyzing heterogeneous effects by caste reveals that the effect is driven by lower caste respondents. In a harsher test of these results, I restrict the sample to women residing in districts either side of the Maharashtra state border, and find that the effect is robust within this set-up. I also show that the effect is robust to restricting the study period to only those women born before the economic liberalization policies rolled out from 1991.

Two mechanisms could explain these results. First, child marriage may serve as an alternative coping strategy to sex-selective abortion in response to the financial burden of raising daughters. Anecdotal evidence supports this interpretation: Desai (1994) notes that some Mumbai clinics displayed posters reading, "Better pay 500 Rs now than 50,000 Rs later," where 500 Rs was the price of an abortion and 50,000 Rs the anticipated dowry. Prior research shows that female-specific abortion is used to avoid dowry (Bhalotra, Chakravarty, and Gulesci 2020). When abortion is restricted, a possible alternative coping strategy to reduce dowry is the earlier marriage of daughters, since dowry burdens tend to be lower for younger brides (Chowdhury (2010); Maertens and Chari (2020); Loaiza and Wong (2012)). Supporting this mechanism, I provide empirical evidence using Rural Economic Development Survey data from 1999 that dowry burdens are increasing in the age of the bride in India, even when holding educational attainment, sibling composition and groom characteristics constant. The second potential mechanism is a shift in assortative matching patterns in the marriage market: perhaps child marriage is increased by girls becoming less scarce as a result of sex-selective abortion being prohibited. This potential alternative mechanism runs

counter to the existing theoretical and empirical evidence which predicts that the balancing of sex-ratios causes child marriage to decrease (Vijayendra Rao (1993), Edlund (1999), Anderson (2007b), Chiplunkar and Weaver (2023)). To investigate the marriage market implications of the ban, I restrict the sample to women who can be matched with their spouses and examine whether the ban affects spousal age and educational gaps — key indicators of assortative matching. I find no significant effects on either marker. Therefore, child marriage as an alternative coping strategy to abortion against the cost of raising daughters emerges as the plausible interpretation of the results.

The contribution of this paper is threefold. Firstly, this paper adds to a growing literature which shows that policies intending to eradicate harmful practices can cause unintended consequences if they are ineffective at changing underlying social norms (Ashraf, Bau, and Nunn (2017); Erten and Keskin (2018); Khalifa (2022)). Bhalotra, Brulé, and Roy (2020) show that a reform to strengthen women's inheritance rights in India unintentionally caused an exacerbation of son preference behaviors (measured as sex-selective abortion prevalence) by increasing the cost of having daughters, suggesting that policy changes alone are insufficient without accompanying shifts in sticky social norms. I contribute to a subset of this literature on the unintended consequences of legally prohibiting sex-selective abortion specifically. While such bans are shown to balance sex ratios (Nandi and Deolalikar 2013), previously studied unintended consequences for girls include increased infant mortality (Dasgupta and Sharma 2024) and reduced educational attainment (Rastogi and Sharma 2022). In their exploration of the marriage market as a potential channel linking sex-selective abortion restriction at the national level to worsened educational attainment, Rastogi and Sharma (2022) interestingly find an opposite effect to the results I present here: that the ban decreased child marriage before age 15. The differing findings may be attributable to differences in the research design - while their identification strategy relies on a partially pre-treated control group, this paper exploits a setting with an untreated control. This paper provides

the first evidence that a restriction on access to sex-selective abortion causally increases child marriage.

Secondly, I contribute to the literature on the relationship between access to birth control technologies and economic outcomes for girls and women. It is well established that the rise of birth control technologies, from contraceptives to abortion, has had far-reaching implications not only for women (Goldin and Katz (2002); Bailey (2006)) but also for children (Gruber, Levine, and Staiger (1999); Donohue and Levitt (2001); Donohue, Grogger, and Levitt (2009)). Focusing specifically on sex-selective abortion, Anukriti, Bhalotra, and Tam (2022) find that increased access to prenatal sex-detection technologies in India led to the narrowing of the gender gap in under-five mortality, driven by moderated son-biased fertility stopping and reduced gender gaps in parental investments into children. Similarly, Kalsi (2015) finds that abortion legalization, by facilitating sex selection, led to an increase in university enrollment for second and higher birth order girls. While the existing literature focuses on how access to birth control technologies shapes health and educational outcomes for surviving girls, this paper speaks to its impact on marriage.

Lastly, I contribute to a broader literature on son preference (Sen (1992); Sen (2003); Bhalotra and Cochrane (2010); Anderson and Ray (2010); Bhalotra, Chakravarty, and Gulesci (2020)) and its link to child marriage and virilocality (Corno, Hildebrandt, and Voena (2020) Khanna and Kochhar (2023)). While Corno, Hildebrandt, and Voena (2020) find that delayed marriage can be a cost-saving strategy in dowry-paying settings, this reflects responses to short-term aggregate shocks, rather than forward-looking decisions about the costs of raising an additional daughter. Closely related to this, Bhalotra, Chakravarty, and Gulesci (2020) provide evidence that exogenous hikes in perceived dowry burden motivate increased usage of sex-selective abortion, meaning the abortion of females can be seen as a strategy to avoid dowry. This paper looks at the opposite direction of that causal relationship -

if sex-selective abortion is restricted, do households alleviate dowry burdens by alternative means such as by marrying off daughters at a younger age, when dowry tends to be lower? This paper contributes new evidence consistent with a conceptualization of child marriage as a long-term cost saving strategy for the household.

The remainder of the paper is organized as follows: Section 2 provides a conceptual framework, Section 3 discusses the background, Section 4 describes the data, Section 5 outlines the empirical approach, Section 6 presents the results, Section 7 presents robustness checks and Section 8 concludes.

2 Conceptual Framework

In this section I propose a simple conceptual framework of the relationship between sex-selective abortion and child marriage. Fertility patterns in India are consistent with a son-specific stopping rule (Baland, Cassan, and Woitrin 2023). Since the gender of a fetus is determined randomly, female fetuses are conceived in the attempt for sons. If pre-natal sex-determination and abortion are accessible, women may terminate such pregnancies. Alternatively, she may carry the pregnancy to term, resulting in an *instrumental birth*, and a larger number of daughters than initially desired. In line with this, I conceptualize a ban on sex-determination as inducing the births of instrumental daughters. This is verified empirically: the prohibition of sex selective abortion has been shown to cause the birth of more girls, measured as the number of daughters per mother (Rastogi and Sharma 2022), and the girl-boy birth ratio at district level (Nandi and Deolalikar 2013). I provide empirical evidence that Maharashtra's ban on sex-selection caused the additional births of girls in Section 6.1.

How might the birth of instrumental girls, caused by a ban on sex-selection, affect daughters' likelihood of being married in childhood? Two plausible mechanisms are discussed here.

2.1 Child Marriage as an Alternative Coping Strategy against Costliness of Raising Daughters?

The burden of dowry has been identified as a key driver of sex-selective abortion usage (Bhalotra, Chakravarty, and Gulesci 2020). When sex-selective abortion is restricted by a legal ban, households are likely to adopt an alternative coping strategy against dowry costs. I hypothesize that one such alternative coping strategy is child marriage, since younger brides typically require a lower dowry (Chowdhury (2010); Maertens and Chari (2020); Loaiza and Wong (2012)). I provide further evidence of the positive relationship between the woman's age at marriage and dowry in Section 5.1. Dowry can be impoverishing for households, often amounting to values greater than a household's annual income (Chiplunkar and Weaver (2023); Anderson (2007a)). Resource-constrained families raising a greater number of daughters than initially desired, due to a ban on sex-determination, may have strong incentives to marry an instrumental daughter off in childhood in order to reduce the dowry burden. Moreover, child marriage transfers the day-to-day costs of raising a child to the family of the groom in virilocal societies like India, strengthening this incentive to marry instrumental daughters early. This conceptualization predicts that a ban on sex-selective abortion causally increases child marriage, via an alternative coping strategy channel.

2.2 Child Marriage as a Result of Balancing Sex-Ratios on the Marriage Market?

If the restriction of sex-selective abortion causes the birth of more girls, this changes the landscape of the marriage market. Brides become less scarce relative to grooms, weakening each girl's level of bargaining power and therefore potentially affecting her age at marriage (Becker (1981); Angrist (2002); Anderson (2007b); Abramitzky, Delavande, and Vasconcelos (2011)). According to marriage squeeze theories, child marriage is predicted to decrease as a result of reduced female scarcity. Previous work has provided theoretical motivation for

why more balanced sex-ratios lead to later marriage for girls (Vijayendra Rao (1993), Edlund (1999), Anderson (2007b)). When brides are less scarce due to the demographic effect of rising populations in marriage markets where men tend to marry younger women, the resolving equalizing mechanism is that women delay their marriage age. Since a restriction of sex-selective abortion similarly reduces female scarcity, it is predicted to cause a decrease in child marriage.

Chiplunkar and Weaver (2023) provide empirical evidence in support of marriage squeeze theory in the context of India, finding that an increase in the number of women relative to men is associated with a rise in the female age of marriage. Adding to this empirical evidence, Lafortune (2013) finds that tighter marriage market conditions for girls causes investment into their "attractiveness" to potential matches. For instance, parents may invest in the educational attainment of their daughter, delaying her entrance to the marriage market, and causing child marriage to decrease (Rastogi and Sharma 2022).

Therefore, existing evidence predicts that a marriage market channel works against the effect which operates via a dowry coping strategy. In Section 7.1, I empirically test whether the ban affected spousal age and educational gaps and find no significant effect. This suggests that the observed increase in child marriage is inconsistent with changes in assortative matching within the marriage market driven by a balancing of the sex-ratio.

3 Background

3.1 Abortion and Sex-Determination in India

Medical techniques such as amniocentesis, chorionic villus sampling, and ultrasound, originally intended for detecting fetal abnormalities, were introduced in India in the 1970s (Luthra 1999). At this time, abortion before 12 weeks gestation had already been legalized at national

level (Government of India 1971) allowing for these new technologies to be quickly repurposed for sex-determination (Luthra 1999). This resulted in the widespread abortion of female fetuses, spurring concern among members of the public and policymakers, who viewed the intentional termination of a pregnancy due to the fetal sex as unacceptable (UNFPA 2001). Further, the impact of sex-selective abortion became evident in official demographic data as sex-ratios became increasingly male-skewed. For example, the evolution of India's sex-ratio at birth from 1960 to 2024 is shown in Figure 1.

Figure 1: Female births per hundred male births, India, 1960-2024



Note: Data comes from the World Bank.

Maharashtra became the first state to ban prenatal sex determination with the Maharashtra Regulation of Pre-natal Diagnostic Techniques Act in 1988 (Government of Maharashtra (1988); Government of India (1994)). An activist group in Mumbai, the *Forum Against Sex Determination and Sex Pre-selection*, led the campaign for this policy change (UNFPA 2001). The Act sought to regulate obstetric diagnostic technologies and explicitly aimed to "prevent... pre-natal sex determination leading to female feticide" (Government of Maharashtra 1988). It prohibited sex-selective abortions by banning the disclosure of fetal sex to

expectant mothers. It remains illegal for expectant mothers in Maharashtra, and India as whole, to learn the sex of their baby before birth today.

The implementation of the ban involved a State Vigilance Committee which oversaw a set of Local Vigilance Committees operating at the district level (Government of Maharashtra 1988). These committees were tasked with visiting medical facilities to check compliance, either at random or in response to reports from members of the public or media outlets. They were further responsible for preventing the operation of unauthorized clinics. The ban mandated that any medical provider involved in fetal sex determination could face up to three years in prison and a fine of five thousand rupees. Women seeking sex-selection services faced similar penalties, including up to three years in prison or a fine of three thousand rupees. The law also held husbands and family members liable by default for abetting the offense (Government of Maharashtra 1988).

Bans on sex-determination are typically associated with imperfect compliance, and prior research suggests that compliance varies along religious and socio-economic lines. First, Hindu women are expected to be more strongly affected by these bans if the underlying mechanism is linked to dowry practices. Dowry has historically been practiced more extensively among Hindu households than among other religious communities in India, as the institution of dowry originates in Hindu tradition. (Bloch, Vijay Rao, and Desai (2004); Botticini and Siow (2003)). Second, less wealthy families have been shown to exhibit stronger responses to bans on sex selective abortion (Rastogi and Sharma 2022), consistent with resource constraints limiting households' ability to circumvent the ban by accessing costly private illegal services or traveling outside of the state. I provide evidence of stronger heterogeneous effects among Hindu and less wealthy households in Section 6.2.

3.2 Cross-country Correlation between Sex-selective Abortion and Child Marriage

In terms of the global picture, female-specific abortion and the early marriage of girls are often viewed as co-occurring symptoms of harmful gender norms (UNFPA 2020b). This view might lend itself to the prediction that female birth rates and child marriage rates are inversely correlated: as social norms surrounding the value of girls change, more females are born (as sex-selective abortion becomes scarcer) and child marriage rates decline. Appendix Figure 1 shows that, in fact, the correlation is positive: countries with high child marriage rates are likely to have more balanced sex-ratios at birth, and countries with male-skewed birth ratios tend to suffer less with the problem of child marriage. This correlation raises the possibility that lower sex-selective abortion usage may be associated with higher rates of early marriage for girls.

4 Data

4.1 Births Data

To explore the effect of the ban on sex-selective abortion on the additional births of girls, I use total population counts at district level by gender and single-year of age from the Indian 2001 Census. I create a retrospective panel meaning those registered as age ten in 2001 are coded in the data as born in 1991, and so on. While I use aggregate district-level data, a frequency-weighted regression makes use of an effective sample size of 100.52 million births from 1981 to 1994. These data are also broken down by urban/rural residence, allowing for the inclusion of a binary variable to control for this dimension.

Census data suggests that while Maharashtra's sex ratio at birth was male skewed at the time of the ban on sex-determination, it was not an outlier in this regard. For India as a

whole, 91.2 baby girls were born for every 100 baby boys born in 1977, while for Maharashtra this figure stood at 89.0. Among control states, this figure ranged from 87.1 in Madhya Pradesh to 95.7 in Chhattisgarh (See Appendix Figure 3a)

Differential state trends in terms of incomes per capita could be a source of bias, as son preference intensity could be shaped by economic conditions. To control for this, I use official data on net state-level domestic product at constant (1980-1981) prices per capita from the Indian Ministry of Statistics as a time-varying control. This data spans 1981 to 1994 and for this reason, I restrict the study window for the regression analysis of births to these years.

4.2 Marriage Data

Individual-level data on marriage outcomes come from the 2016 and 2021 waves of the Indian National Family and Health Surveys (NFHS). The NFHS is a household survey which is representative at state and country level.

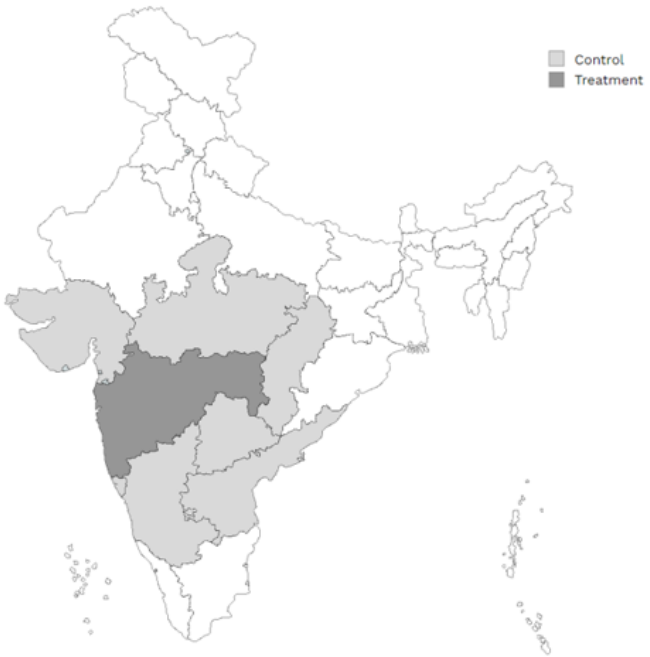
All girls and women in the households between the ages of 15 and 49 are interviewed, regardless of marital status. Information on marriage is collected retrospectively. Women are asked to recall the month and year when they first got married. I include only respondents who are over the age of 18 at the time of being interviewed.

I define child marriage for girls as marriage before the age of 18, in line with Indian legislation on minimum legal marriage ages (Tandon 2010), international consensus (e.g., UNICEF 2019) and previous economic research on child marriage (e.g., Nour 2006; Field and Ambrus 2008; Raj 2010; Tsaneva 2020). In supplementary analyses, I consider child marriage before age 15, the approximate half-way point along the child marriage risk window in India (Corno, Hildebrandt, and Voena 2020), as well as several alternative age cut-offs to ensure the effect

is robust to the choice of the age threshold.

The full sample comprises 238,452 women. Appendix Table 1 provides descriptive statistics of the sample. Approximately 16% of the sample, or 38,160 women, are from Maharashtra, the treated state. Of the entire sample, just under 40% of women had been married before the age of 18, and the mean marriage age among the ever-married sample is 18.3 years.

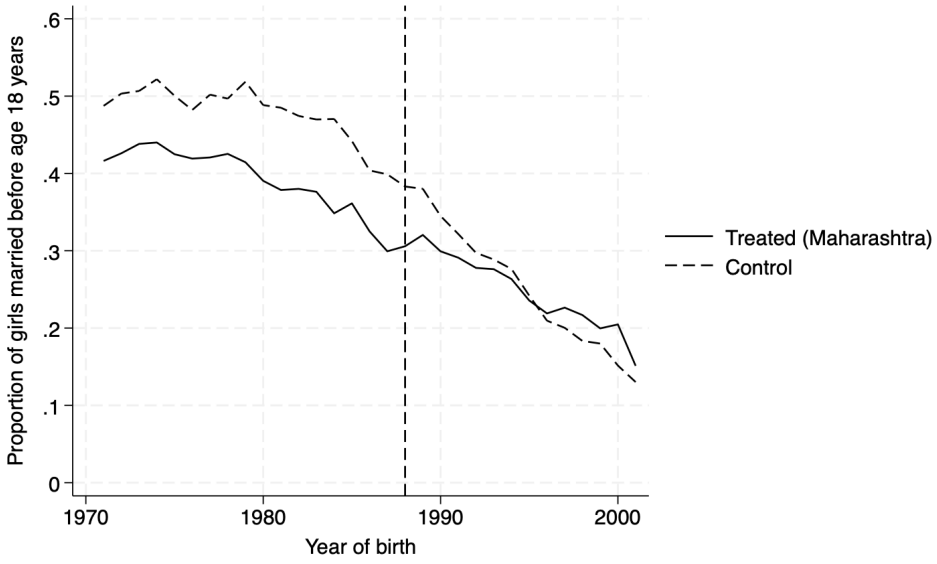
Figure 2: Map of India indicating treated and control states



Note: State boundaries as of 2025.

Raw trends in the proportion of women who reported being married before the age of 18 are presented in Figure 3. This plot shows an apparent parallel evolution in child marriage rates in the raw data from 1970 until the time of the treatment, 1988. For the duration of the pre-ban period, Maharashtra reports a child marriage rate before age 18 approximately 8 percentage points lower than the bordering untreated states. Similar trends are observed when considering child marriage before age 15, as shown in Appendix Figure 2.

Figure 3: Child Marriage before 18 by Treatment Status 1970-2002



Note: 1988 is the year of implementation of a ban on sex-selection in Maharashtra.

5 Empirical Approach

5.1 Women's age at marriage and dowry values

A central hypothesis in this paper hinges on dowry burdens increasing in the age of the bride, so that families raising an instrumental daughter have an incentive to arrange her marriage at a younger age. I provide empirical evidence on this by modeling the net real value of dowry payments made as a function of the bride's age at marriage. I use the 1999 wave of the Rural Economic and Demographic Survey (REDS) to obtain information on the monetary value of dowry payments for 3,846 marriages where a dowry was paid from 1975 to 1995. These include all sisters and daughters of surveyed household heads. The estimated equation is

$$\mathrm{d}_{ist} = \beta age_{it} + \gamma X_i + \alpha_s + t + t^2 + \epsilon_{ist} \tag{1}$$

where d_{ist} is the logarithm of the net real dowry value (deflated using the Indian consumer

price index) paid by woman i's family in marriage year t in state s, age_{it} is the age of woman i at marriage in year t. This represents the value of dowry payments from the bride's side to the groom's side net of the value of dowry payments paid from the groom's side to the bride's side. I control for relevant couple characteristics X_i including caste and education of the bride and the groom, number and gender composition of the bride's siblings, state fixed effects α_s and time trends.

I show that a robust positive relationship exists between a woman's age at marriage and the value of dowry payments in Appendix Table 2. The median and mean net dowry values in 2010 real terms for the sample are 34,441 and 66,287 rupees respectively. Each one year increase in a woman's age at marriage is a associated with a rise in dowry of approximately 6.4%, controlling for educational attainment. These results are in line with Chowdhury (2010) who also documents a strong association between the bride's age at marriage and dowry payments for marriages of Hindu couples only. I show that net dowries are lower for child marriages, suggesting that the observed increase in dowry with age is not driven by extreme payments for the oldest brides. This pattern supports the hypothesis that parents face an economic incentive to expedite their daughters' marriages.

5.2 The impact of prohibiting sex-selective abortion on births and child marriage

To assess the impact of prohibiting sex-selective abortion on births and child marriage, I exploit the natural experiment created by Maharashtra's ban on sex-determination in 1988. I compare child marriage outcomes in Maharashtra before and after the ban, using neighboring states - Gujarat, Madhya Pradesh, Chhattisgarh, Telangana, Andhra Pradesh, Goa, and Karnataka - as a control group[1]. These states did not experience any changes in

[1]Although Andhra Pradesh has not shared a border with Maharashtra since the creation of Telangana in 2014 (Vaddiraju 2020), it bordered Maharashtra throughout the study period and is therefore included in the control group. As a robustness check, I omit Andhra Pradesh from the control group and show that the

policies affecting sex-selective abortion accessibility during the study period 1975-1995, with a similar nationwide ban on the practice introduced in 1996 (Government of India 1994). I leverage this natural experiment using an event study approach. The estimated equation is

$$\mathrm{Y}_{idt} = \sum_{j=1}^{J} \beta_j(\mathrm{Event}_j \times \mathrm{Treated}_d) + \alpha_d + \delta_t + \gamma X + \epsilon_{idt} \tag{2}$$

where Y_{idt} is an outcome of interest for women i, in district d, born in year t. The outcome variable in the standard specification is a binary variable coded as one if a woman retrospectively reports to have been married before age 18 and zero otherwise. When analyzing births data to explore the effect of the ban on sex-determination on the birth of girls, I use the gender of each baby born as the dependent variable, coding the birth of a girl as one and the birth of a boy as zero. $Treated_d$, which is coded as one for districts within Maharashtra and zero otherwise, is interacted with a set of event-year dummies $Event_j$, where j denotes the event-year. This is defined so that the dummy variable $Event_{i-1}$ takes a value of one if the woman i was born one year before the ban was passed and zero otherwise, $Event_{i1}$ takes a value of one if woman i was born one year after the ban was passed and zero otherwise, and so on, with 1988 constituting the base year. Each estimate of β_j provides the change in outcomes for treated women relative to non-treated women for each event-year. α_d and δ_t capture district and year of birth fixed effects, respectively. X include individuals' characteristics, such as caste identification, religion. Standard errors ϵ_{idt} are clustered at the district level to account for correlated shocks within districts. In the treated state, Maharashtra, local district-level committees were responsible for the enforcement of the ban, meaning policy implementation and household responses are likely correlated within districts.

I run this event study on a range of alternative dependent variables. These include a continuous dependent variable which captures the women's age at first marriage as well as a

results hold in Section 7.4.

binary variable coded one if the respondent has completed secondary education. Women and men in the sample who self-report to be married are matched, and their spousal age gap and educational attainment gap is used as a further dependent variable.

In places, I collapse this event study into a difference-in-differences set-up. In these specifications, girls born in January 1989 or later are classified as being in the post-treatment period, while those born before January 1989 are classified as being in the pre-treatment period. This is based on the evidence that by the late 1980s in India, prenatal sex determination was technologically feasible from approximately four months gestation (Patel 1989), and the ban on sex-selection was enacted in Maharashtra in June 1988 (Patel 2011). Babies born in January 1989 would have reached four months gestation by the time the ban was enacted, assuming a pregnancy length of roughly ten months from conception (Jukic et al. 2013). This implies that mothers of babies born in January 1989 or later would not have been able to legally access sex-selective abortion, as they would not have been far enough along in their pregnancies to know the sex of the fetus before the implementation of the ban. In contrast, mothers of babies born in January 1989 or earlier would have had at least a brief window to pursue legal sex-selective abortion before the ban took effect.

The key identifying assumption underlying this strategy is that in Maharashtra, where the ban on sex-determination was implemented, women's timing of marriage (and other dependent variables of interest) would have exhibited parallel trends with that of women in the control states if the ban had not been introduced. The post-ban period analyzed in this study extends up to 1995 because the nationwide ban on sex-selective abortion, the Prenatal Diagnostics Techniques Act (PNDT), was implemented in 1996.

Including only bordering states as the control group, rather than all remaining states also helps to ensure the credibility of the identifying assumption, because it is likely that un-

observable co-founders are more likely to evolve in parallel between Maharashtra and its neighboring states than between Maharashtra and more distant regions in India. In further analysis, I restrict the sample to only districts on either side of the Maharashtra state border and show that the effect holds when controlling for border-segment fixed effects. As a robustness check, I use all remaining states of India as a control group and find that the effect on child marriage in Maharashtra remains significant (see Section 7.3).

Existing research mainly exploits the 1996 nationwide ban, the PNDT Act as the treatment (Nandi and Deolalikar (2013); Nandi (2015); Dasgupta and Sharma (2024); Rastogi and Sharma (2022)) and use Maharashtra, where the PNDT act was non-binding due to Maharashtra having its own ban, as the control. By exploiting the 1988 ban, I can leverage a larger sample of women who are adults at the time of the interview compared to using the 1996 national ban, for which data coverage of adult women is more limited. Moreover, it allows for the use of a wholly untreated, rather than a pre-treated, control.

6 Results

6.1 The impact of prohibiting sex-selective abortion on female births

Figure 4a illustrates the impact of the ban on sex-selective abortion on the birth sex ratio, as specified by equation (1). The ban increased female births by 1.0–1.6 per 100 births or 36–59 points in the sex ratio (females per 1,000 males). This is comparable to the 14–26 point increase in the child sex ratio reported by Nandi and Deolalikar (2013) for the nationwide PNDT Act of 1996. The smaller effect of the later nationwide ban is consistent with widespread criticism regarding the weak enforcement of the national-level policy. Corresponding regression output is presented in column (3) of Appendix Table 3.

6.2 The impact of prohibiting sex-selective abortion on child marriage

Table 1 shows that the ban caused a 4.8 percentage point increase in child marriage before age 18 on average across post-treatment period, corresponding to an increase of 14.8% relative to the counterfactual. Marriage before each of the five age cutoffs from 14 - 18 exhibited significant treatment effects, with the largest effect in percentage terms seen for marriage before the age of 15, which saw a rise of 24.5%. Among the ever-married sample, the ban on sex-selective abortion caused the age of marriage to decrease on average by 0.25 years.

The event study results corresponding to equation (2) presented in Figure 4b reveal the dynamic impact of the ban on sex-selective abortion on the rate of child marriage before age 18. The ban had a positive and statistically significant effect on child marriage before age 18 for women born two or more years after implementation. The magnitude of the effect reaches 7.2 percentage points among girls born in 1995, seven years after the implementation of the ban, the last year of the study window. The parallel trends assumption is supported, with differences in the pre-treatment period minimal and point estimates fluctuating around zero. Corresponding regression output is presented in Appendix Table 5. Using four alternative dependent variables which capture marriage before age 14 - 17, I find that the event study results are not sensitive to the choice of the age threshold. These results are presented in Appendix Table 4 and Appendix Figure 4.

Table 1: Effect of prohibiting sex-selective abortion on marriage outcomes, difference-in-difference estimates

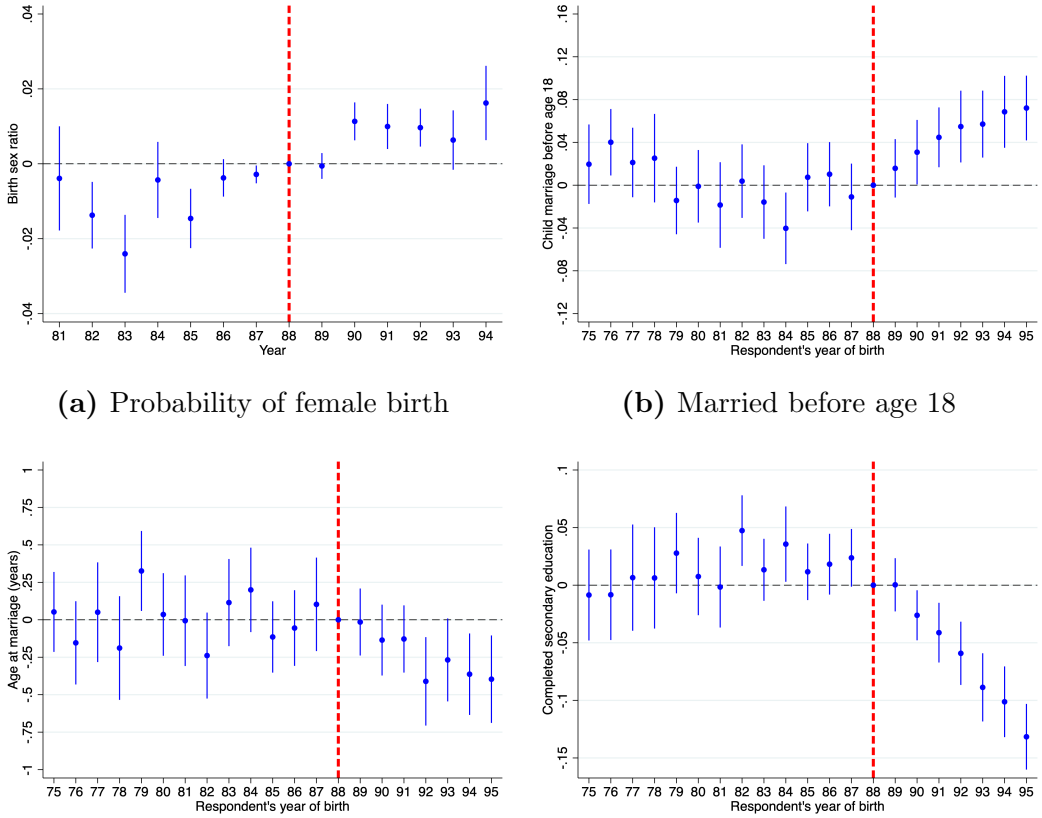
	(1)	(2)	(3)	(4)	(5)	(6)
	Child marriage					Age at marriage
	By 18	By 17	By 16	By 15	By 14	
Treated x Post	0.048***	0.043***	0.037***	0.024***	0.010**	-0.245***
	(0.007)	(0.007)	(0.007)	(0.006)	(0.005)	(0.063)
N	238,452	238,452	238,452	238,452	238,452	213,893
adj. R^2	0.088	0.081	0.069	0.050	0.035	0.091
Pre-ban mean of DV	0.325	0.239	0.154	0.098	0.059	18.296

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Treated=1 if the state is Maharashtra, where a ban on sex-selective abortion is implemented in 1988. Post=1 if woman's date of birth if after 1988 and zero otherwise. "Pre-ban mean of DV" captures the mean of the dependent variable among women born in 1987, one year before the implementation of the ban. All regressions control for year of birth and district fixed effects, a dummy variable=1 if the woman reports to be lower caste and a dummy variable=1 if the woman is Hindu. Robust standard errors are clustered at the district level. The data comes from the 2016 and 2021 waves of the India National Family and Health Survey.

Figure 4: Event study plots of effects of prohibiting sex-selective abortion on female births, marriage outcomes and education



(a) Probability of female birth

(b) Married before age 18

(c) Age at first marriage

(d) Completed secondary education

Note: Figures 4a-4d show the results from estimating equation 2. In 4a, the dependent variable is a dummy=1 if the birth is to a girl and zero if it is to a boy. The data comes from India Census 2001 table PC01. The regression is frequency-weighted by number of babies born per district per year and includes year of birth and district fixed effects, a dummy=1 if the household is situated in a rural area, and a time-varying control for state-level real output per capita. Robust standard errors are clustered at the district level. The dependent variable in Figures 4b - 4d are a dummy=1 if the woman was married by the age of 18 and zero otherwise, a continuous variable capturing the woman's age at marriage in years, and a dummy=1 if the woman reports to have completed secondary level education. All regressions control for year of birth and district fixed effects, a dummy variable=1 if the woman reports to be lower caste and a dummy variable=1 if the woman is Hindu. Robust standard errors are clustered at the district level. The data comes from the 2016 and 2021 waves of the India National Family and Health Survey. The red dashed line represents 1988, the year of implementation of a ban on sex-determination for Maharashtra, the treated state.

Figure 4c illustrates that the ban caused a statistically significant downward effect on age at first marriage for women among the ever-married sample, born four or more years after the ban's roll-out. Among the final cohort of the study period, the ban caused a decrease in age at first marriage of 0.40 years. The corresponding regression output is shown in Appendix Table 5.

Prior research shows that women attain less schooling as a result of early marriage (Field and Ambrus 2008). Figure 4d shows that the ban on sex-selective abortion had a strongly significant and negative effect on secondary educational attainment, dropping by 13.2 percentage points, or 16.8% relative to the counterfactual, among the final cohort. These results are in line with Rastogi and Sharma (2022), who find that the 1996 nationwide ban on sex-determination in India causally reduced the educational attainment of girls relative to that of boys. Related to this, Appendix Table 8 shows that the effect on child marriage is robust to controlling for various measures of educational attainment, indicating that the effect on marriage is not solely driven by an educational attainment channel.

As discussed in Section 3, previous research has suggested that bans on sex-determination may exhibit heterogeneous effects by religion and socio-economic status. For testing heterogeneity by socio-economic status, I use data on the wealth quintile, coding the binary variable as one for respondents within the lower three quintiles of the wealth distribution. This measure of wealth reflects post-marital levels, but avoids threats of endogeneity on the assumption that upward marriage mobility along the wealth distribution will most commonly be within-quintile. I also use caste identification as an exogenous proxy for the socio-economic status of household of origin, based on the fact that inter-caste marriage in India is low. I code a binary variable as one if a respondent self-identifies as belonging to a scheduled caste, scheduled tribe, or other backwards caste (SC/ST/OBC) and zero for identifying as part of an upper caste, regardless of religious affiliation.

For marriage before 18, Table 2 column (2) shows that the effect is driven by Hindu women, in line with an understanding that dowry is most strongly practiced within Hindu communities compared to other major religions in India such as Christianity and Islam. Column (3) shows that the effect is stronger among women from the bottom 60% of the wealth distribution, consistent with evasion of the ban being costly. Similarly, column (4) shows that the effect is driven by those who report to belong to a lower caste. While non-Hindu women frequently self-identify as belonging to a caste, caste groupings historically emanate from Hinduism. As such, I restrict the sample to Hindu women in column (5) and find that while the direction of effect is positive, the coefficient of the differential effect is insignificant. The heterogeneous effect for lower caste Hindu women relative to upper caste Hindu women is significant for child marriage before age 15, as shown in Appendix Table 6.

Figure 5a and 5b show the results from estimating equation 2 on the sample split by wealth and caste identification separately, illustrating that the effect on child marriage is driven most strongly by the low-wealth and lower caste sub-samples.

Table 2: Effect of prohibiting sex-selective abortion on child marriage before 18, heterogeneity analysis

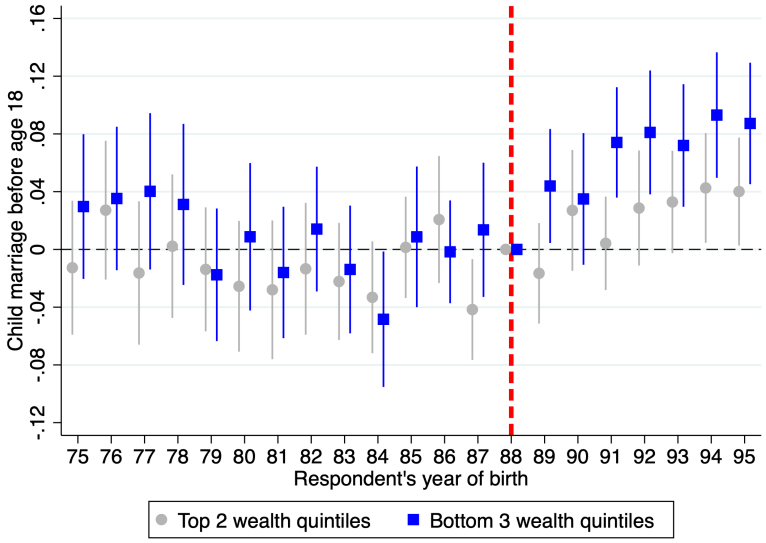
	(1)	(2)	(3)	(4)	(5)
Treated x Post	0.048***	0.017	0.035***	0.018	0.034**
	(0.007)	(0.012)	(0.009)	(0.011)	(0.014)
Treated x Post x Hindu		0.035**			
		(0.015)			
Treated x Post x Low Wealth			0.029**		
			(0.013)		
Treated x Post x Lower Caste				0.033**	0.018
				(0.013)	(0.015)
Observations	238,452	238,452	238,452	238,452	212,673
Adjusted R^2	0.088	0.088	0.107	0.089	0.089
Linear Combination		0.052	0.064	0.051	0.052
		(0.008)	(0.010)	(0.008)	(0.009)
Sample restriction?	No	No	No	No	Hindu-only

Standard errors in parentheses

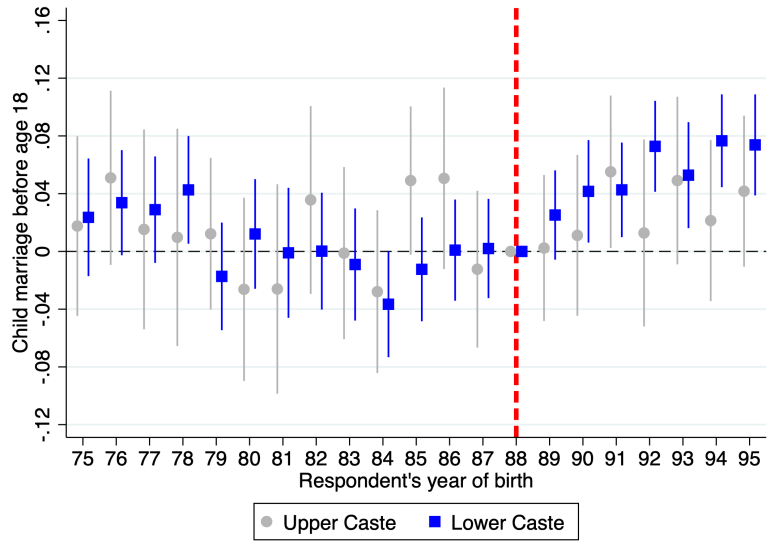
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The table shows results of estimating specification 2 where the dependent variable is child marriage which is equal to 0 if the woman was married by the age of 18 and 1 otherwise. All regressions control for year of birth and district fixed effects, a dummy variable=1 if the woman reports to be lower caste and a dummy variable=1 if the woman is Hindu. Robust standard errors are clustered at the district level. The data comes from the 2016 and 2021 waves of the India National Family and Health Survey.

Figure 5: Event study plots of effect of prohibiting sex-selective abortion on child marriage, by wealth and caste



(a) Sample split by wealth level



(b) Sample split by caste

Note: Figure 5a and 5b show the results from estimating equation 2 on split samples. In Figure 5a, the effect is plotted by grey circles for women in the top two wealth quintiles and by blue squares for women in the bottom three quintiles. In Figure 5b, the effect is plotted by grey circles for upper caste respondents and by blue squares for scheduled caste, scheduled tribe and other backward caste members. All regressions control for year of birth and district fixed effects, a dummy variable=1 if the woman reports to be lower caste and a dummy variable=1 if the woman is Hindu. Robust standard errors are clustered at the district level. The data comes from the 2016 and 2021 waves of the India National Family and Health Survey.

6.2.1 The impact of prohibiting sex-selection on child marriage in border districts

In a harsher test of the results on child marriage, I restrict the sample to districts of Maharashtra which share a border with a control state and control state districts which share a border with Maharashtra. The estimating equation is:

$$Y_{idbt} = \beta \left(\text{Treated}_d \times \text{Post}_t\right) + \alpha_d + \lambda_t + \phi_b + \gamma X_i + \epsilon_{idbt} \tag{3}$$

where Y_{idbt} is a binary indicator equal to one if the girl i in district d along border segment b with year of birth t was married by the age threshold. $\text{Treated}_d \times \text{Post}_t$, captures the difference-in-differences effect of the ban on sex-determination in treated districts. α_d is district fixed effects and λ_t is year of birth fixed effects. ϕ_b is border-segment fixed effects, which captures the control state boundary which the district adjoins to or sits within. If a treated district adjoins to multiple control states, border segment takes the value of the control state with which it shares the longest boundary. X_i is a vector of control variables including binary variables to capture Hinduism and belonging to a lower caste. This identification strategy, similar to that used by Naidu (2012), controls for a variety of unobservable variables that may bias sweeping cross-state analyses.

Table 3 presents the results of this analysis. Panel (A) contains the results of interest, which are the difference-in-differences estimates using women born from border districts, including border segment fixed effects. For comparison, panel (B) presents results of estimating equation (2) omitting districts which share a border with another state. Panel (C) reproduces the results from estimating equation (2) on the unrestricted sample for comparison.

These results show that the impact of the ban is robust to comparing outcomes for border districts within border segments. While point estimate magnitudes are smaller, the effect of the ban was not statistically significantly different among women closer to the border.

This is consistent with Rastogi and Sharma (2022), who similarly find a lack of evidence for women temporarily migrating to access abortion.

7 Robustness Checks

7.1 Alternative mechanism via the marriage market

While it is unlikely for changes in assortative matching on the marriage market to be driving the effect on child marriage based on existing research predicting the opposite direction of effect, I test for this empirically. I restrict the sample to only women who can be matched to their husbands and run an event study on this sub-sample as specified by equation (2). This provides a sample size of 33,432 ever-married women over the age of 18 at the time of their interview. I examine the impact of the ban on key indicators of assortative matching in the marriage market: the spousal age gap and the spousal educational attainment gap. The spousal age gap is defined as the difference between a woman's husband's age and her own, while the educational gap refers to the difference in years of education between husband and wife. The effect of the ban on the spousal age gap and educational gap are presented in Appendix Figure 5a and 5b respectively. Corresponding regression output is shown in Appendix Table 9. The ban appears to have no effect on either of these markers in the post-treatment period. The lack of effect on these markers of marriage market conditions fails to provide evidence for a sex-ratio channel, making a mechanism tied to dowry as an alternative coping strategy most plausible.

Table 3: The effect of prohibiting sex-selection on child marriage in border districts

	By 18	By 17	By 16	By 15	By 14
Panel A: Sub-sample of border districts					
Treated x Post	0.039***	0.032**	0.027**	0.014	0.001
	(0.014)	(0.013)	(0.012)	(0.010)	(0.007)
N	55,372	55,372	55,372	55,372	55,372
adj. R^2	0.076	0.068	0.057	0.041	0.029
Border Segment FE	Y	Y	Y	Y	Y
Panel B: Sub-sample excluding border districts					
Treated x Post	0.053***	0.047***	0.035***	0.022***	0.012*
	(0.009)	(0.007)	(0.009)	(0.008)	(0.007)
N	182,071	182,071	182,071	182,071	182,071
adj. R^2	0.090	0.082	0.070	0.051	0.035
Border Segment FE	N	N	N	N	N
Panel C: Unrestricted sample					
Treated x Post	0.048***	0.043***	0.037***	0.024***	0.010**
	(0.007)	(0.007)	(0.007)	(0.006)	(0.005)
N	238,452	238,452	238,452	238,452	238,452
adj. R^2	0.087	0.080	0.069	0.050	0.034
Border Segment FE	N	N	N	N	N

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The table shows the results from estimating equation (3) where the dependent variable is a binary variable coded as one if the girl was married by the age indicated. Border segment is defined based on the control state boundary which the district adjoins to. If a treated district borders more than one control state, the border segment is assigned to the control state with which it shares the longest boundary. All regressions control for year of birth and district fixed effects, a dummy variable=1 if the woman reports to be lower caste and a dummy variable=1 if the woman is Hindu. Robust standard errors are clustered at the district level. The data comes from the 2016 and 2021 waves of the India National Family and Health Survey.

7.2 The "Emergency"

In the 1980s and early 1990s, India faced a severe balance of payments crisis. This crisis triggered a wave of economic liberalization and deregulation from 1991 onwards, the effects of which were felt unevenly across different states (Ahluwalia 2002). A key threat to the identifying assumption is that Maharashtra and the control states may have experienced divergent trends in economic growth in the post-ban period, affecting child marriage trajectories differently and undermining the identification assumption.

If the detected effect is driven by asymmetric impacts of economic liberalization between the treatment and control groups, rather than the ban on sex-determination, it should not be observable prior to liberalization, which began with the election of a reform-oriented government in 1991 (Ahluwalia 2002). To test this, I restrict the sample to women born between 1987 and 1990—two years before and two years after the ban's implementation, and importantly, before liberalization. Additionally, I include wealth quintile fixed effects in the basic difference-in-differences specification to make comparisons only between women of similar levels of wealth. In Table 4, I show that the effect remains robust to this set up, a result that is inconsistent with a liberalization-driven alternative mechanism.

Maharashtra exhibited stronger growth during the 1990s in connection to liberalization, as shown in Appendix Figure 7. If economic growth leads to a reduction in child marriage,

Table 4: Robustness check: Restricted sample of women born before "The Emergency"

	(1)	(2)	(3)	(4)	(5)
	By 18	By 17	By 16	By 15	By 14
Treated x Post	0.031***	0.021**	0.017*	0.016*	0.007
	(0.010)	(0.009)	(0.009)	(0.009)	(0.006)
N	49,436	49,436	49,436	49,436	49,436
adj. R^2	0.107	0.092	0.071	0.051	0.034
Mean of Dep. Var.	0.367	0.267	0.181	0.114	0.067

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The dependent variable is a binary variable coded as one if the girl was married by the age specified. The sample contains women born between 1987 and 1990. All regressions control for year of birth, district and wealth quintile fixed effects, a dummy variable=1 if the woman reports to be lower caste and a dummy variable=1 if the woman is Hindu. Robust standard errors are clustered at the district level. The data comes from the 2016 and 2021 waves of the India National Family and Health Survey.

then Maharashtra's stronger growth relative to the control works against the impact of restriction of sex-selective abortion. However, if economic growth increases child marriage, this could be co-founding the detected effect caused by the ban. To address this concern, I also include wealth quintile fixed effects in the main specification. The results remain robust to this adjustment, as shown in Appendix Figure 6a and Appendix Table 10. By estimating the effect within wealth quintiles, I mitigate concerns that the results are being driven by households in Maharashtra becoming wealthier over time due to liberalization. Post-marital wealth quintiles are more appropriate as a control variable compared to a continuous measure of household income or wealth, on the assumption that most women marry into households within the same wealth quintile as that of their family of origin prior to marriage.

7.3 Using all remaining states as control

As an additional robustness check, Appendix Figure 6b presents the results from equation (2) using a control group that includes all remaining states in India, rather than only those bordering the treated state of Maharashtra. While supporting evidence for the validity of the parallel trends assumption is somewhat weaker in this specification, the treatment effect remains detectable. This alleviates concerns that the observed effect is driven by an anomalous decline of child marriage in Maharashtra's border states.

7.4 Omitting Andhra Pradesh from the control group

Andhra Pradesh ceased to share a border with Maharashtra in 2014 with the creation of Telangana as a separate state (Vaddiraju 2020). I omit Andhra Pradesh from the control group, and find that the detected effect is robust to this change as shown in Appendix Figure 6c. The corresponding regression output is included in Appendix Table 10.

7.5 Omitting Mumbai from the treated group

The 1988 Maharashtra Regulation of Pre-natal Diagnostic Techniques Act was a result of a campaign led by an activist group in Mumbai, called the *Forum Against Sex Determination and Sex Pre-Selection* (UNFPA 2001). It is plausible that this activism was a result of an unobserved variable such as a change in gender norms, creating bias due to endogeneity of the treatment. The activism against sex-selective abortion was concentrated within Mumbai. If it is driven by endogenous omitted variables, the removal of Mumbai from the treatment group would reasonably result in a reduced detected effect. I omit Mumbai from the treatment group, and find that the detected effect is robust to this change as shown in Appendix Figure 6d. The corresponding regression output is included in Appendix Table 10.

7.6 Testing the effect on remaining unmarried

It is possible that a restriction on sex-selective abortion would affect the proportion of women who remain unmarried. Remaining unmarried constitutes the outside alternative option to marriage, and an alternative strategy to avoid dowry. To test this, I restrict the sample to only women who are over the age of 25 at the time of interview, and use a dummy variable coded as one for women who remain unmarried at this age and zero otherwise as the dependent variable. This yields a sample size of 180,920 women. Using a higher age threshold is prohibited due to a lack of data for unmarried women older than 25. Table 5 shows that the ban had no significant impact on the outside option of remaining unmarried, consistent with the hypothesis that norms which drive high marriage rates in India persisted in the post-treatment period.

8 Conclusion

Exploiting the natural experiment provided by Maharashtra's 1988 ban on pre-natal sex-determination, I present the first evidence that legal restrictions on sex-selective abortion may causally increase the prevalence of child marriage.

The analysis indicates that the legal restriction of sex-selective abortion in Maharashtra affected both fertility and marriage outcomes. The policy increased the number of girls born, raising the sex ratio (girls per 1,000 boys) by approximately 36-59 points. This is credible in the context of Nandi and Deolalikar (2013) reporting an effect size of 14-26 points in the sex-ratio due to the national ban on the practice in 1996, a policy widely criticized as poorly enforced. The difference-in-differences estimates show that the ban increased marriage before age 18 by 4.8 percentage points (14.8%). Event-study results indicate that effects appear for cohorts born two or more years after implementation and reach 7.2 percentage points (22.2%) for the cohort born seven years after the ban, the final year of the study period.

The results are robust to changing the age threshold to define child marriage. The ban reduced the age at first marriage by 0.25 years among ever-married women and reduced girls' secondary school completion by 16.8% among the final cohort in the sample. The effects are concentrated among Hindu, poorer, and lower-caste households. The estimates remain similar when restricting the sample to pre-liberalization cohorts and when limiting the analysis to border districts.

These patterns are consistent with financially constrained households being less able to circumvent the ban through high-cost illegal abortion services, in line with evidence from Rastogi and Sharma (2022). The evidence presented points to a mechanism whereby, when access to sex-selective abortion is restricted, child marriage becomes an alternative coping strategy for households to mitigate the financial burden associated with raising daughters, including dowry. To support this mechanism I use survey data on real net dowry values to show empirically that there is a significant and positive relationship between dowry and women's age at marriage, even when controlling for educational attainment. Until now, the direct relationship between access to sex-selective abortion and child marriage has been under-explored, with the only existing paper connecting sex-selective abortion to child marriage finding an opposite direction of effect (Rastogi and Sharma 2022).

This paper speaks to broader discussions about how sex-selection relates to gender discrimination and the persistence of son preference as a social norm. Son preference, which since the 1980s has often manifested in the use of sex-selective abortion, is frequently characterized as discrimination against girls. This view risks overlooking the structural conditions under which such norms are created and sustained. These results suggest that policies which increase the number of girls born, without addressing the reasons why parents avoid raising daughters, may inadvertently contribute to harmful outcomes. Efforts to eradicate son preference that rely solely on restricting abortion are unlikely to improve outcomes for girls

broadly unless the material constraints that make raising daughters difficult are similarly addressed.

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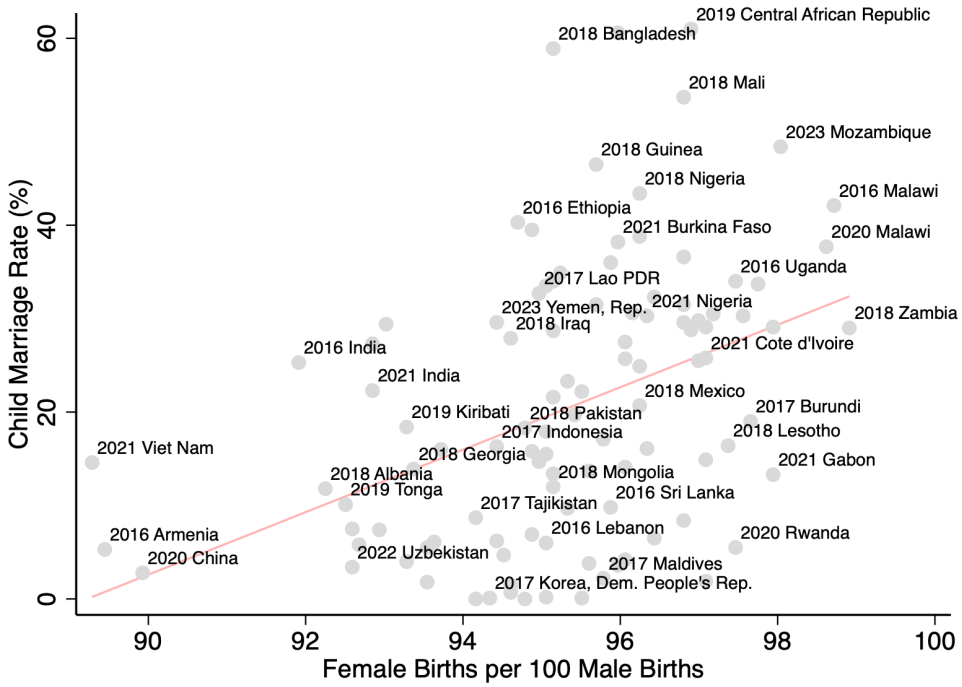
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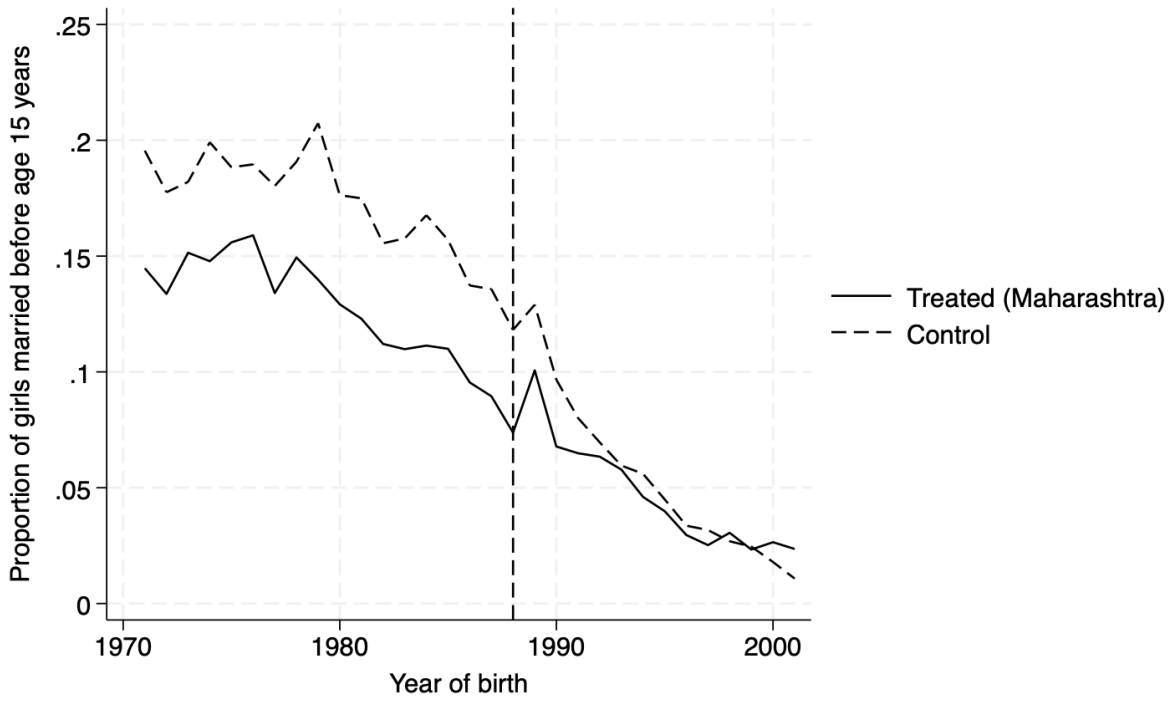
9 Appendix

Appendix Figure 1: Cross-country scatter plot of pre-18 child marriage rates and birth sex-ratios (World Bank)



Note: This figure presents all country–year observations from 2015 to 2024 World Bank Data that have both child-marriage and sex-ratio available.

Appendix Figure 2: Raw trends in child marriage before 15 by treatment status 1970–2002



Note: 1988, the year of implementation of a ban on sex-determination in Maharashtra, is denoted by the dashed vertical line.

Appendix Table 1: Descriptive Statistics

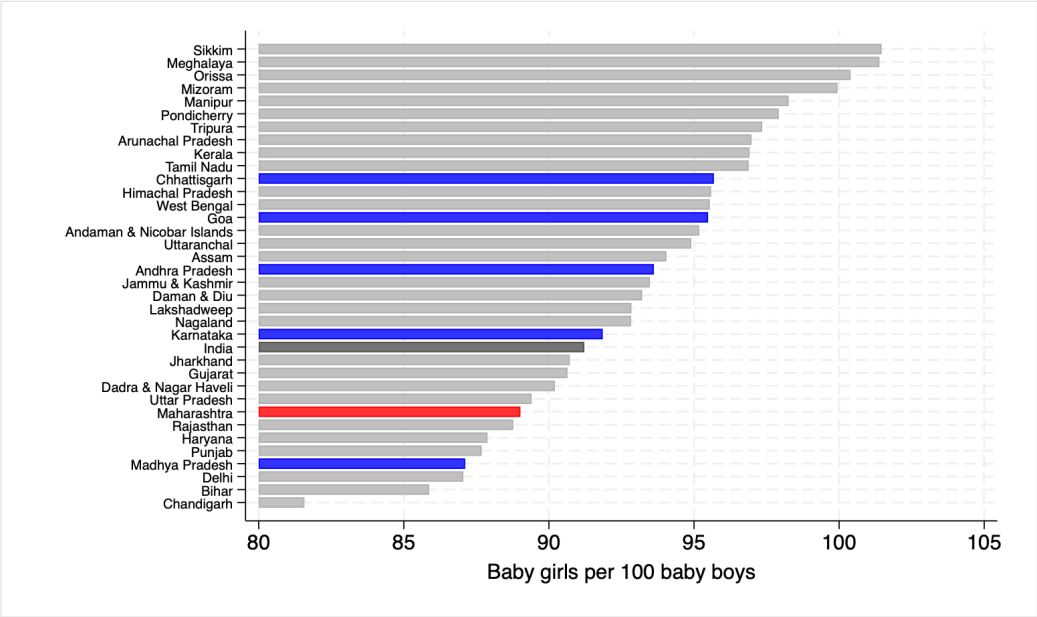
Panel A: Summary Statistics for Full Sample

	Mean	Std. Dev.	Min	Max
Child marriage before age 18	0.39	0.48	0	1
Child marriage before age 15	0.13	0.33	0	1
Age at First Marriage	18.30	3.86	0	44
Hindu	0.89	0.31	0	1
Lower Caste	0.83	0.37	0	1
Rural	0.70	0.46	0	1
Completed primary	0.72	0.44	0	1
Completed secondary	0.58	0.49	0	1
Years of education	6.65	5.24	0	20
Treated	0.16	0.36	0	1
Observations (Full Sample)	238,452			
Observations (Ever-Married Sample)	213,893			

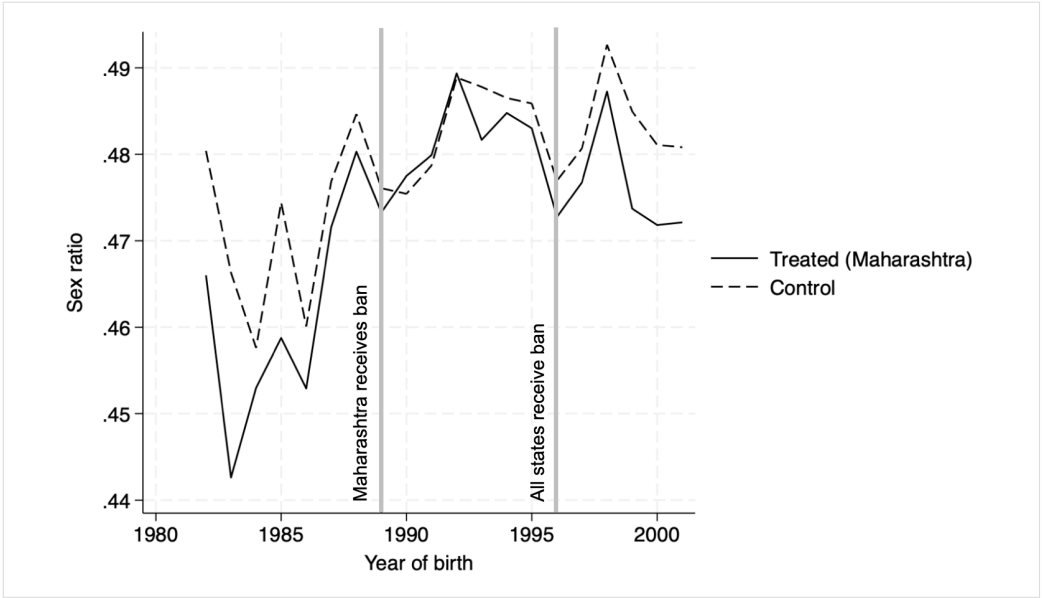
Panel B: Summary Statistics by Treatment Group, Pre-Treatment

	Treated = 0		Treated = 1		All	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Child marriage before age 18	0.46	0.50	0.38	0.49	0.45	0.50
Child marriage before age 15	0.17	0.37	0.12	0.33	0.16	0.37
Age at First Marriage	18.08	4.16	18.66	4.11	18.18	4.15
Hindu	0.91	0.29	0.82	0.38	0.89	0.31
Lower Caste	0.86	0.34	0.64	0.48	0.83	0.38
Rural	0.70	0.46	0.65	0.48	0.70	0.46
Completed primary	0.61	0.49	0.82	0.38	0.65	0.48
Completed secondary	0.45	0.50	0.68	0.47	0.49	0.50
Years of education	5.25	5.15	7.47	4.78	5.61	5.16
Observations	122,022		23,857		145,879	
Observations (Ever-Married Sample)	116,217		22,622		138,839	

Appendix Figure 3: Sex Ratio at Birth, India: Descriptive Graphs



(a) Female births per 100 male births by state, India 1987 (2001 Census)



(b) Sex ratio at birth by treatment status 1983-2002 (2001 Census)

In Appendix Figure 3 the sex-ratios shown are estimated and retrospective based on single-year of age census returns in 2001, and therefore conditional on survival to 2001 with minimal cross-state migration. In panel (a) The red bar represents the treated state Maharashtra, while blue bars represent control states.

Appendix Table 2: Effect of woman's age at marriage on real net dowry payment

	(1)	(2)	(3)	(4)	(5)	(6)
Age at marriage (years)	0.079***	0.081***	0.064***			
	(0.027)	(0.027)	(0.024)			
Child marriage				-0.262***	-0.266***	-0.198**
				(0.087)	(0.088)	(0.080)
N	3,846	3,846	3846	3,846	3,846	3,846
adj. R^2	0.315	0.317	0.390	0.313	0.315	0.388
Mean of Dep. Var.	10.290	10.290	10.290	10.290	10.290	10.290
Sibling composition	N	Y	Y	N	Y	Y
Schooling	N	N	Y	N	N	Y

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The table shows the results from estimating specification 1 where the dependent variable is the log real value of dowry payment made by the bride's side to the groom's side, net of that paid by the groom's side to the bride's side, at the time of marriage, including women for whom a dowry payment was reported. Columns (1) - (3) include the continuous variable for age at marriage in years columns (4) - (6) include a dummy variable=1 if the girl was married before the age of 18. Data is from the 1999 wave of the Rural Economic and Demographic Survey. Dowry is deflated using Indian CPI from World Bank Development Indicators. All regressions control for state fixed effects, caste of the bride's family, linear and quadratic trends in the year of marriage. Columns (1) - (3) include the square of age of marriage. The regression in columns (2) and (5) additionally control for the bride's number of sisters and brothers; the regressions in columns (3) and (6) control for the number of years of schooling of the bride and groom; Standard errors are clustered at the district level.

Appendix Table 3: Event study of the effect of prohibiting sex-selective abortion on birth sex-ratio

	(1)	(2)	(3)
1981 * Treated	-0.001	-0.004	-0.004
	(0.007)	(0.007)	(0.007)
1982 * Treated	-0.010**	-0.014***	-0.014***
	(0.004)	(0.005)	(0.005)
1983 * Treated	-0.021***	-0.024***	-0.024***
	(0.005)	(0.005)	(0.005)
1984 * Treated	-0.001	-0.004	-0.004
	(0.005)	(0.005)	(0.005)
1985 * Treated	-0.011***	-0.015***	-0.015***
	(0.004)	(0.004)	(0.004)
1986 * Treated	-0.003	-0.004	-0.004
	(0.002)	(0.003)	(0.003)
1987 * Treated	-0.002	-0.003**	-0.003**
	(0.001)	(0.001)	(0.001)
1989 * Treated	0.000	-0.001	-0.001
	(0.002)	(0.002)	(0.002)
1990 * Treated	0.006***	0.011***	0.011***
	(0.002)	(0.003)	(0.003)
1991 * Treated	0.004*	0.010***	0.010***
	(0.002)	(0.003)	(0.003)
1992 * Treated	0.005**	0.010***	0.010***
	(0.002)	(0.003)	(0.003)
1993 * Treated	-0.004	0.006	0.006
	(0.002)	(0.004)	(0.004)
1994 * Treated	0.002	0.016***	0.016***
	(0.002)	(0.005)	(0.005)
GDP		-0.000***	-0.000***
		(0.000)	(0.000)
Rural			-0.001
			(0.001)
N	100,518,590	100,518,590	100,518,590
adj. R^2	0.001	0.001	0.001
Mean of Dep. Var.	0.500	0.500	0.500

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The dependent variable is coded one if birth is a girl and zero if a boy. Based on India Census 2001 table PC01. The regression is frequency-weighted by number of babies born per district per year. All regressions include year of birth and district fixed effects.

Appendix Table 4: Event study of the effect of prohibiting sex-selective abortion on child marriage using alternative age cut-offs

	(1)	(2)	(3)	(4)
	By 17	By 16	By 15	By 14
1975 * Treated	0.019	-0.006	0.023	0.021*
	(0.017)	(0.015)	(0.013)	(0.010)
1976 * Treated	0.034*	0.025	0.026	0.021
	(0.016)	(0.014)	(0.016)	(0.012)
1977 * Treated	0.004	-0.004	0.008	0.002
	(0.018)	(0.015)	(0.016)	(0.013)
1978 * Treated	0.026	0.011	0.018	0.023
	(0.019)	(0.019)	(0.017)	(0.015)
1979 * Treated	-0.018	-0.034*	-0.018	-0.008
	(0.017)	(0.016)	(0.015)	(0.010)
1980 * Treated	0.002	-0.006	0.013	0.014
	(0.016)	(0.015)	(0.014)	(0.009)
1981 * Treated	-0.017	-0.016	0.002	0.004
	(0.016)	(0.015)	(0.014)	(0.010)
1982 * Treated	0.018	0.010	0.015	0.017
	(0.016)	(0.014)	(0.015)	(0.011)
1983 * Treated	0.005	-0.017	-0.001	0.003
	(0.017)	(0.016)	(0.015)	(0.010)
1984 * Treated	-0.030*	-0.031*	-0.008	0.003
	(0.015)	(0.013)	(0.011)	(0.008)
1985 * Treated	0.003	-0.007	0.005	0.008
	(0.013)	(0.012)	(0.011)	(0.008)
1986 * Treated	0.005	-0.018	0.004	-0.004
	(0.015)	(0.012)	(0.010)	(0.007)

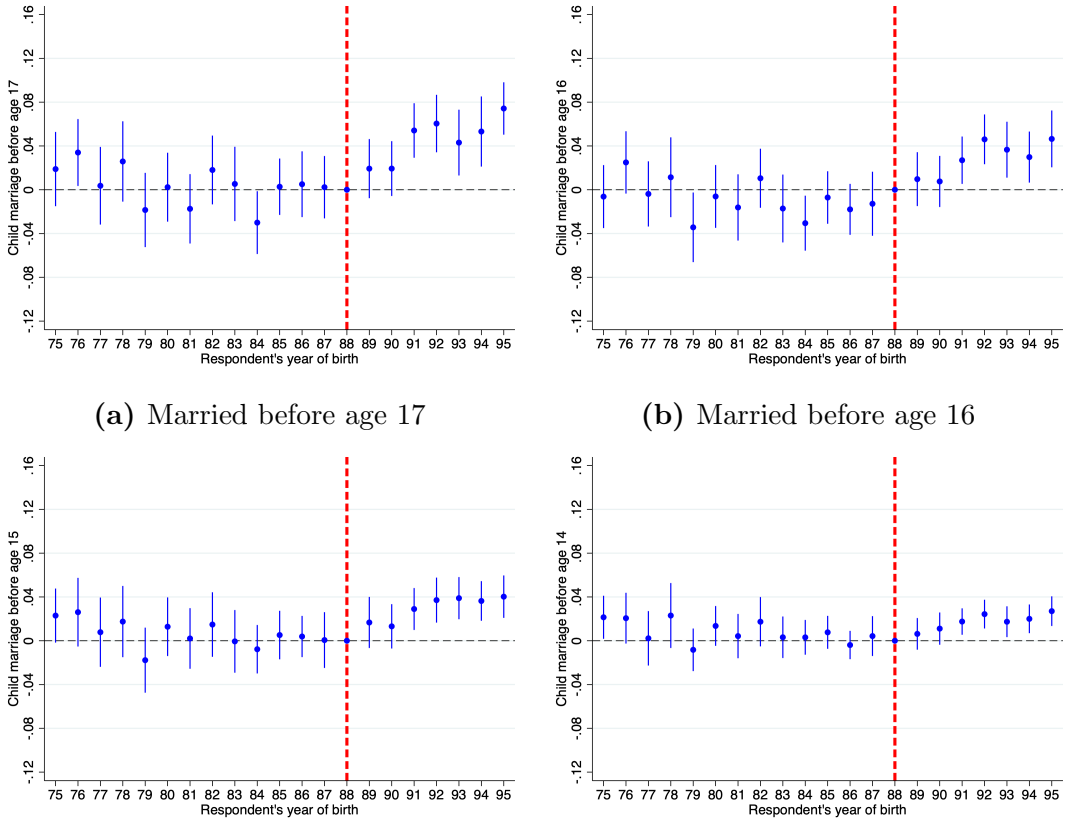
1987 * Treated	0.002	-0.013	0.001	0.004
	(0.014)	(0.015)	(0.013)	(0.009)
1989 * Treated	0.019	0.010	0.017	0.006
	(0.014)	(0.012)	(0.012)	(0.007)
1990 * Treated	0.019	0.008	0.013	0.011
	(0.013)	(0.012)	(0.010)	(0.007)
1991 * Treated	0.054***	0.027*	0.029**	0.018**
	(0.013)	(0.011)	(0.010)	(0.006)
1992 * Treated	0.060***	0.046***	0.037***	0.024***
	(0.013)	(0.012)	(0.010)	(0.007)
1993 * Treated	0.043**	0.037**	0.039***	0.017*
	(0.015)	(0.013)	(0.010)	(0.007)
1994 * Treated	0.053**	0.030*	0.036***	0.020**
	(0.016)	(0.012)	(0.009)	(0.007)
1995 * Treated	0.074***	0.046***	0.040***	0.027***
	(0.012)	(0.013)	(0.010)	(0.007)
N	238452	238452	238452	238452
adj. R^2	0.081	0.069	0.051	0.035
Mean of Dep. Var.	0.289	0.199	0.126	0.074

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: The dependent variable is a binary variable coded as one if the woman reports to have been married by the age specified. All regressions control for year of birth and district fixed effects, a dummy variable=1 if the woman reports to be lower caste and a dummy variable=1 if the woman is Hindu. Robust standard errors are clustered at the district level. The data comes from the 2016 and 2021 waves of the India National Family and Health Survey.

Appendix Figure 4: Event study of the effect of prohibiting sex-selective abortion on child marriage using alternative age cut-offs



(a) Married before age 17

(b) Married before age 16

(c) Married before age 15

(d) Married before age 14

Note: The dependent variable is a binary variable coded as one if the woman reports to have been married by the age specified. All regressions control for year of birth and district fixed effects, a dummy variable=1 if the woman reports to be lower caste and a dummy variable=1 if the woman is Hindu. Robust standard errors are clustered at the district level. The data comes from the 2016 and 2021 waves of the India National Family and Health Survey. The red dashed line represents 1988, the year of implementation of a ban on sex-determination for Maharashtra, the treated state.

Appendix Table 5: Event study of the effect of prohibiting sex-selective abortion on marriage outcomes

	(1)	(2)	(3)	(4)
	Married by 18	Age married	Secondary Edu.	Unmarried at 25
1975 * Treated	0.020	0.053	-0.009	0.008
	(0.019)	(0.135)	(0.020)	(0.012)
1976 * Treated	0.040*	-0.154	-0.008	-0.002
	(0.016)	(0.141)	(0.020)	(0.013)
1977 * Treated	0.021	0.051	0.007	0.002
	(0.016)	(0.169)	(0.023)	(0.012)
1978 * Treated	0.025	-0.189	0.006	-0.011
	(0.021)	(0.176)	(0.022)	(0.012)
1979 * Treated	-0.014	0.326*	0.028	0.002
	(0.016)	(0.135)	(0.018)	(0.013)
1980 * Treated	-0.001	0.035	0.008	0.005
	(0.017)	(0.140)	(0.017)	(0.012)
1981 * Treated	-0.019	-0.006	-0.002	0.009
	(0.020)	(0.153)	(0.018)	(0.012)
1982 * Treated	0.004	-0.239	0.047**	-0.021
	(0.017)	(0.146)	(0.016)	(0.012)
1983 * Treated	-0.016	0.115	0.013	0.001
	(0.017)	(0.148)	(0.014)	(0.013)
1984 * Treated	-0.040*	0.200	0.036*	-0.001
	(0.017)	(0.143)	(0.017)	(0.012)
1985 * Treated	0.007	-0.115	0.012	0.009
	(0.016)	(0.121)	(0.012)	(0.012)
1986 * Treated	0.010	-0.055	0.018	-0.014
	(0.015)	(0.128)	(0.013)	(0.011)

1987 * Treated	-0.011	0.103	0.024	0.013
	(0.016)	(0.158)	(0.013)	(0.013)
1989 * Treated	0.016	-0.015	0.000	0.000
	(0.014)	(0.114)	(0.012)	(0.013)
1990 * Treated	0.031*	-0.135	-0.026*	-0.002
	(0.015)	(0.120)	(0.011)	(0.014)
1991 * Treated	0.045**	-0.128	-0.041**	-0.002
	(0.014)	(0.114)	(0.013)	(0.013)
1992 * Treated	0.055**	-0.411**	-0.059***	-0.016
	(0.017)	(0.150)	(0.014)	(0.016)
1993 * Treated	0.057***	-0.268	-0.089***	0.011
	(0.016)	(0.141)	(0.015)	(0.015)
1994 * Treated	0.069***	-0.363**	-0.101***	
	(0.017)	(0.138)	(0.016)	
1995 * Treated	0.072***	-0.396**	-0.132***	
	(0.015)	(0.148)	(0.014)	
N	238,452	213,893	238,452	180,920
adj. R^2	0.088	0.091	0.194	0.037
Mean of Dep. Var.	0.393	18.296	0.576	0.105

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: The dependent variable is a binary variable coded as one if the woman retrospectively reports to have been married by the age of 18 in column (1) and by 15 in column (2). The dependent variable captures the age at marriage among the ever-married sample in column (3). In column (4) the dependent variable is a binary variable coded as one if the woman reports to have completed secondary education or higher, and includes the full (ever married and unmarried) sample. In column (5) the dependent variable is a binary variable coded as one if the woman remains unmarried at the age of 25, and the sample includes only women aged over 25 at the time of interview. All specifications control for district and year of birth fixed effects and for binary indicators for belonging to a lower caste and Hinduism. Robust standard errors clustered at the district level in parentheses. Based on India NFHS waves 2016 and 2021.

Appendix Table 6: The effect of prohibiting sex-selective abortion on child marriage before 15, heterogeneity analysis

	(1)	(2)	(3)	(4)	(5)
Treated x Post	0.024***	0.004	0.017***	0.001	0.008
	(0.006)	(0.010)	(0.007)	(0.007)	(0.008)
Treated x Post x Hindu		0.023**			
		(0.009)			
Treated x Post x Low Wealth			0.010		
			(0.007)		
Treated x Post x Lower Caste				0.027***	0.021*
				(0.010)	(0.011)
Observations	238,452	238,452	238,452	238,452	212,673
Adjusted R^2	0.050	0.051	0.059	0.051	0.051
Linear Combination		0.027	0.027	0.027	0.029
		(0.006)	(0.007)	(0.008)	(0.008)
Mean of Dep. Var.	0.126	0.126	0.126	0.126	0.128
Sample restriction?	No	No	No	No	Hindu-only

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The dependent variable is a binary variable coded as one if the girl was married by age 15. All regressions control for year of birth and district fixed effects, a dummy variable=1 if the woman reports to be lower caste and a dummy variable=1 if the woman is Hindu. Robust standard errors are clustered at the district level. The data comes from the 2016 and 2021 waves of the India National Family and Health Survey.

Appendix Table 7: Event study of the effect of prohibiting sex-selective abortion on child marriage, splitting sample by caste and wealth

	(1)	(2)	(3)	(4)
	High Wealth	Low Wealth	Upper Caste	Lower Caste
1975 * Treated	-0.013	0.030	0.018	0.024
	(0.024)	(0.025)	(0.032)	(0.021)
1976 * Treated	0.027	0.035	0.051	0.034
	(0.024)	(0.025)	(0.031)	(0.018)
1977 * Treated	-0.016	0.040	0.015	0.029
	(0.025)	(0.027)	(0.035)	(0.019)
1978 * Treated	0.002	0.031	0.010	0.043*
	(0.025)	(0.028)	(0.038)	(0.019)
1979 * Treated	-0.014	-0.018	0.012	-0.017
	(0.022)	(0.023)	(0.027)	(0.019)
1980 * Treated	-0.026	0.009	-0.026	0.012
	(0.023)	(0.026)	(0.032)	(0.019)
1981 * Treated	-0.028	-0.016	-0.026	-0.001
	(0.024)	(0.023)	(0.037)	(0.023)
1982 * Treated	-0.013	0.014	0.036	0.000
	(0.023)	(0.022)	(0.033)	(0.021)
1983 * Treated	-0.022	-0.014	-0.001	-0.009
	(0.021)	(0.022)	(0.030)	(0.020)
1984 * Treated	-0.033	-0.048*	-0.028	-0.037
	(0.020)	(0.024)	(0.029)	(0.019)
1985 * Treated	0.001	0.009	0.049	-0.012
	(0.018)	(0.025)	(0.026)	(0.018)
1986 * Treated	0.021	-0.002	0.051	0.001
	(0.022)	(0.018)	(0.032)	(0.018)
1987 * Treated	-0.042*	0.014	-0.012	0.002
	(0.018)	(0.024)	(0.028)	(0.017)
1989 * Treated	-0.017	0.044*	0.002	0.025
	(0.018)	(0.020)	(0.026)	(0.016)

1990 * Treated	0.027	0.035	0.011	0.042*
	(0.021)	(0.023)	(0.028)	(0.018)
1991 * Treated	0.004	0.074***	0.055*	0.043*
	(0.016)	(0.019)	(0.027)	(0.017)
1992 * Treated	0.029	0.081***	0.013	0.073***
	(0.020)	(0.022)	(0.033)	(0.016)
1993 * Treated	0.033	0.072***	0.049	0.053**
	(0.018)	(0.022)	(0.029)	(0.019)
1994 * Treated	0.043*	0.093***	0.021	0.077***
	(0.019)	(0.022)	(0.028)	(0.016)
1995 * Treated	0.040*	0.087***	0.042	0.074***
	(0.019)	(0.021)	(0.027)	(0.018)
N	92751	145701	39854	198598
adj. R^2	0.096	0.074	0.084	0.087
Mean of Dep. Var.	0.283	0.463	0.312	0.410

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: The table shows results of estimating specification 2 where the dependent variable is child marriage which is equal to 0 if the woman was married by the age of 18 and 1 otherwise. All regressions control for year of birth and district fixed effects, a dummy variable=1 if the woman reports to be lower caste and a dummy variable=1 if the woman is Hindu. Robust standard errors are clustered at the district level. The data comes from the 2016 and 2021 waves of the India National Family and Health Survey.The regressions in columns (1) and (2) restrict the sample to respondents within the top two quintiles and bottom three quintiles of the wealth distribution respectively. The regressions in column (3) and (4) restrict the sample to respondents reporting to belong to an upper caste and a lower caste respectively.

Appendix Table 8: Effect on child marriage before 18, controlling for education

	(1)	(2)	(3)	(4)
	No Edu. Control	Primary	Secondary	No. Years
1975 * Treated	0.020	0.039*	0.017	0.020
	(0.019)	(0.019)	(0.018)	(0.018)
1976 * Treated	0.040*	0.056***	0.038*	0.037*
	(0.016)	(0.016)	(0.016)	(0.016)
1977 * Treated	0.021	0.041*	0.023	0.025
	(0.016)	(0.016)	(0.015)	(0.014)
1978 * Treated	0.025	0.040*	0.027	0.022
	(0.021)	(0.021)	(0.021)	(0.020)
1979 * Treated	-0.014	0.005	-0.007	-0.002
	(0.016)	(0.015)	(0.015)	(0.015)
1980 * Treated	-0.001	0.012	0.001	0.005
	(0.017)	(0.017)	(0.018)	(0.017)
1981 * Treated	-0.019	-0.001	-0.019	-0.013
	(0.020)	(0.020)	(0.019)	(0.018)
1982 * Treated	0.004	0.020	0.016	0.013
	(0.017)	(0.017)	(0.018)	(0.017)
1983 * Treated	-0.016	-0.007	-0.012	-0.015
	(0.017)	(0.017)	(0.017)	(0.017)
1984 * Treated	-0.040*	-0.030	-0.031	-0.031*
	(0.017)	(0.016)	(0.016)	(0.016)
1985 * Treated	0.007	0.012	0.010	0.005
	(0.016)	(0.015)	(0.015)	(0.015)
1986 * Treated	0.010	0.019	0.015	0.013
	(0.015)	(0.014)	(0.015)	(0.014)
1987 * Treated	-0.011	-0.004	-0.005	-0.004

	(0.016)	(0.016)	(0.016)	(0.015)
1989 * Treated	0.016	0.018	0.016	0.019
	(0.014)	(0.014)	(0.014)	(0.013)
1990 * Treated	0.031*	0.028	0.024	0.020
	(0.015)	(0.015)	(0.015)	(0.015)
1991 * Treated	0.045**	0.040**	0.034*	0.038**
	(0.014)	(0.014)	(0.015)	(0.014)
1992 * Treated	0.055**	0.045**	0.040*	0.044**
	(0.017)	(0.017)	(0.016)	(0.015)
1993 * Treated	0.057***	0.044**	0.035*	0.035*
	(0.016)	(0.015)	(0.015)	(0.014)
1994 * Treated	0.069***	0.052**	0.043*	0.044**
	(0.017)	(0.017)	(0.017)	(0.016)
1995 * Treated	0.072***	0.054***	0.039**	0.042**
	(0.015)	(0.015)	(0.015)	(0.014)
N	238,452	238,452	238,452	238,452
adj. R^2	0.088	0.117	0.141	0.178
Mean of Dep. Var.	0.393	0.393	0.393	0.393

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

The dependent variable is a binary variable coded as one if the woman was married by the age of 18. Columns (2) and (3) control for whether the respondent has completed primary and secondary school education, respectively. Column (4) includes a control which captures the self-reported number of years of education completed. Column (1) shows the original specification on observations for which education data is available, where education can vary as part of the treatment effect. All regressions control for year of birth and district fixed effects, a dummy variable=1 if the woman reports to be lower caste and a dummy variable=1 if the woman is Hindu. Robust standard errors are clustered at the district level. The data comes from the 2016 and 2021 waves of the India National Family and Health Survey.

Appendix Table 9: Event study of spousal age and education gap

	(1)	(2)
	Age Gap	Edu. Gap
1975 * Treated	-0.114	0.620
	(0.439)	(0.343)
1976 * Treated	-0.037	0.365
	(0.351)	(0.405)
1977 * Treated	-0.327	0.242
	(0.372)	(0.375)
1978 * Treated	0.640	0.544
	(0.344)	(0.453)
1979 * Treated	-0.500	0.560
	(0.356)	(0.424)
1980 * Treated	0.101	0.064
	(0.346)	(0.352)
1981 * Treated	0.167	0.104
	(0.363)	(0.443)
1982 * Treated	-0.235	0.256
	(0.390)	(0.378)
1983 * Treated	-0.322	0.427
	(0.309)	(0.392)
1984 * Treated	0.212	0.805*
	(0.316)	(0.318)
1985 * Treated	-0.268	0.373
	(0.250)	(0.371)
1986 * Treated	-0.169	0.029
	(0.346)	(0.329)
1987 * Treated	-0.322	0.310

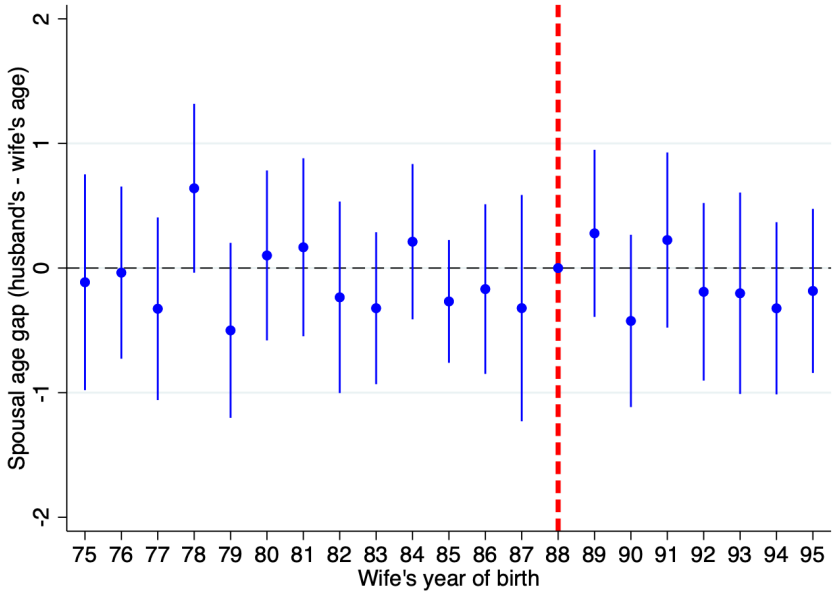
	(0.461)	(0.370)
1989 * Treated	0.279	0.499
	(0.340)	(0.315)
1990 * Treated	-0.424	0.315
	(0.351)	(0.349)
1991 * Treated	0.225	0.479
	(0.357)	(0.313)
1992 * Treated	-0.191	0.334
	(0.362)	(0.315)
1993 * Treated	-0.202	0.422
	(0.410)	(0.353)
1994 * Treated	-0.323	0.313
	(0.351)	(0.364)
1995 * Treated	-0.184	0.490
	(0.334)	(0.421)
N	33,432	33,432
adj. R^2	0.157	0.076
Mean of Dep. Var.	4.751	1.424

Standard errors in parentheses

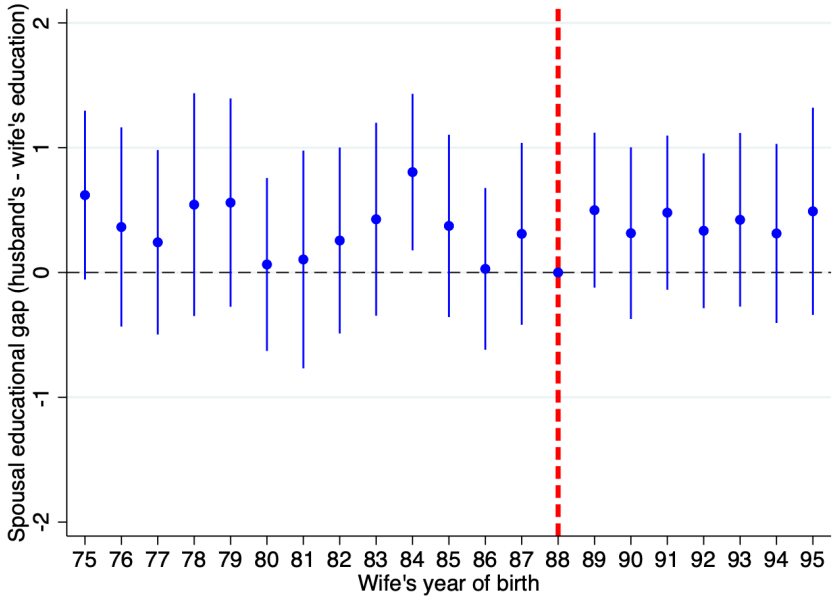
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: Appendix table 9 includes a sub-sample of women for whom data on her husband's age and education attainment is available. In column (1), the dependent variable captures the spousal age gap defined as each woman's husband's age minus her own age. In column (2), the dependent variable captures the spousal gap in years spent in education. This is calculated as the number of years each woman's husband spent in education, minus the number of years the respondent spent in education. All regressions control for year of birth and district fixed effects, a dummy variable=1 if the woman reports to be lower caste and a dummy variable=1 if the woman is Hindu. Robust standard errors are clustered at the district level. The data comes from the 2016 and 2021 waves of the India National Family and Health Survey.

Appendix Figure 5: Event study plots of spousal age gaps and educational gaps



(a) Spousal age gap



(b) Spousal educational attainment gap

Note: In 5a the dependent variable is a woman's husband's age in years minus her own age in years. In 5b the dependent variable is a woman's husband's years spent in education minus her own years spent in education. All regressions control for year of birth and district fixed effects, a dummy variable=1 if the woman reports to be lower caste and a dummy variable=1 if the woman is Hindu. Robust standard errors are clustered at the district level. The data comes from the 2016 and 2021 waves of the India National Family and Health Survey.

Appendix Table 10: Robustness checks on the effect of prohibiting sex-selective abortion on child marriage

	(1)	(2)	(3)	(4)
	All India sample	Wealth quintile FE	Omit Andhra Pr., by 18	Omit Mumbai, by 18
1975 * Treated	0.045**	0.019	0.024	0.023
	(0.017)	(0.018)	(0.019)	(0.019)
1976 * Treated	0.058***	0.041**	0.044**	0.044**
	(0.014)	(0.015)	(0.016)	(0.016)
1977 * Treated	0.060***	0.022	0.025	0.025
	(0.015)	(0.016)	(0.017)	(0.016)
1978 * Treated	0.046*	0.025	0.029	0.028
	(0.020)	(0.020)	(0.021)	(0.021)
1979 * Treated	0.022	-0.009	-0.010	-0.012
	(0.014)	(0.015)	(0.016)	(0.016)
1980 * Treated	0.027	-0.000	-0.000	0.002
	(0.016)	(0.018)	(0.017)	(0.017)
1981 * Treated	0.015	-0.018	-0.015	-0.014
	(0.019)	(0.019)	(0.020)	(0.020)
1982 * Treated	0.025	0.004	0.007	0.007
	(0.016)	(0.017)	(0.018)	(0.017)
1983 * Treated	0.005	-0.017	-0.013	-0.013
	(0.016)	(0.017)	(0.018)	(0.017)
1984 * Treated	-0.019	-0.040*	-0.038*	-0.041*
	(0.016)	(0.016)	(0.017)	(0.017)
1985 * Treated	0.020	0.006	0.010	0.008
	(0.015)	(0.016)	(0.016)	(0.016)
1986 * Treated	0.006	0.010	0.012	0.013
	(0.014)	(0.015)	(0.015)	(0.015)
1987 * Treated	0.002	-0.009	-0.008	-0.009
	(0.015)	(0.016)	(0.016)	(0.016)
1989 * Treated	0.019	0.020	0.019	0.019
	(0.013)	(0.014)	(0.014)	(0.014)

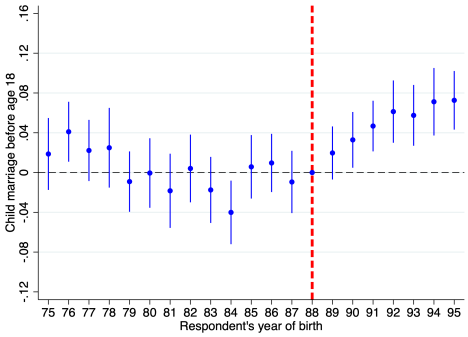
1990 * Treated	0.023	0.033*	0.033*	0.034*
	(0.014)	(0.014)	(0.015)	(0.015)
1991 * Treated	0.038**	0.047***	0.046**	0.048***
	(0.013)	(0.013)	(0.014)	(0.014)
1992 * Treated	0.039*	0.061***	0.057**	0.057**
	(0.016)	(0.016)	(0.017)	(0.017)
1993 * Treated	0.043**	0.058***	0.058***	0.058***
	(0.014)	(0.016)	(0.016)	(0.016)
1994 * Treated	0.051**	0.071***	0.072***	0.071***
	(0.015)	(0.017)	(0.017)	(0.017)
1995 * Treated	0.046***	0.073***	0.075***	0.075***
	(0.014)	(0.015)	(0.016)	(0.015)
N	826287	238452	224828	237643
adj. R^2	0.104	0.115	0.089	0.088
Mean of Dep. Var.	0.346	0.393	0.388	0.394

Standard errors in parentheses

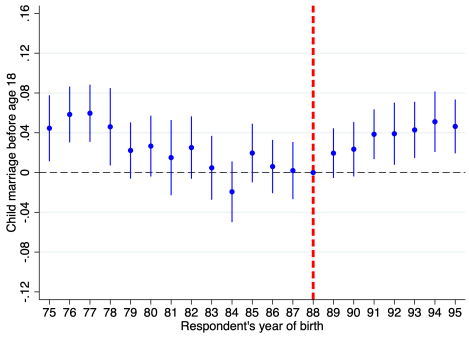
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: The table shows results of estimating specification 2 where the dependent variable is child marriage which is equal to 0 if the woman was married by the age of 18 and 1 otherwise. All regressions control for year of birth and district fixed effects, a dummy variable=1 if the woman reports to be lower caste and a dummy variable=1 if the woman is Hindu. Robust standard errors are clustered at the district level. The data comes from the 2016 and 2021 waves of the India National Family and Health Survey.The regression in column (1) uses all remaining states, rather than only states which border Maharashtra as the control group. The control group in the regressions in column (2)-(4) comprise only bordering states. The regression in column (2) adds wealth quintile fixed effects to specification 1. Andhra Pradesh is omitted from the control group in column (3), and Mumbai is ommitted from the treatment group in column (4).

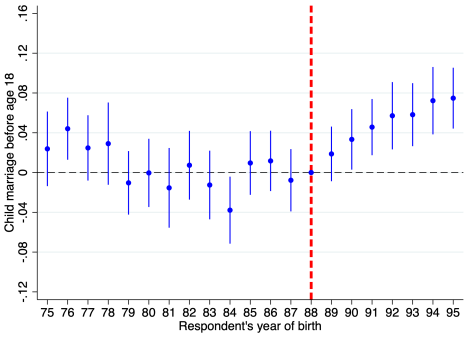
Appendix Figure 6: Robustness Checks



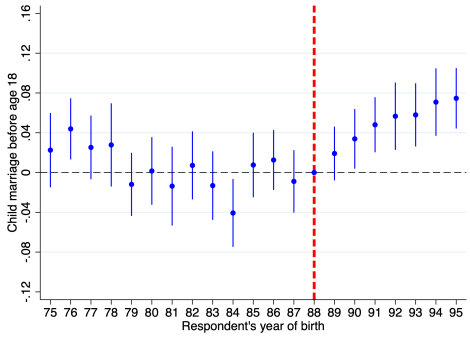
(a) Including wealth fixed effects



(b) Using rest of India as control group



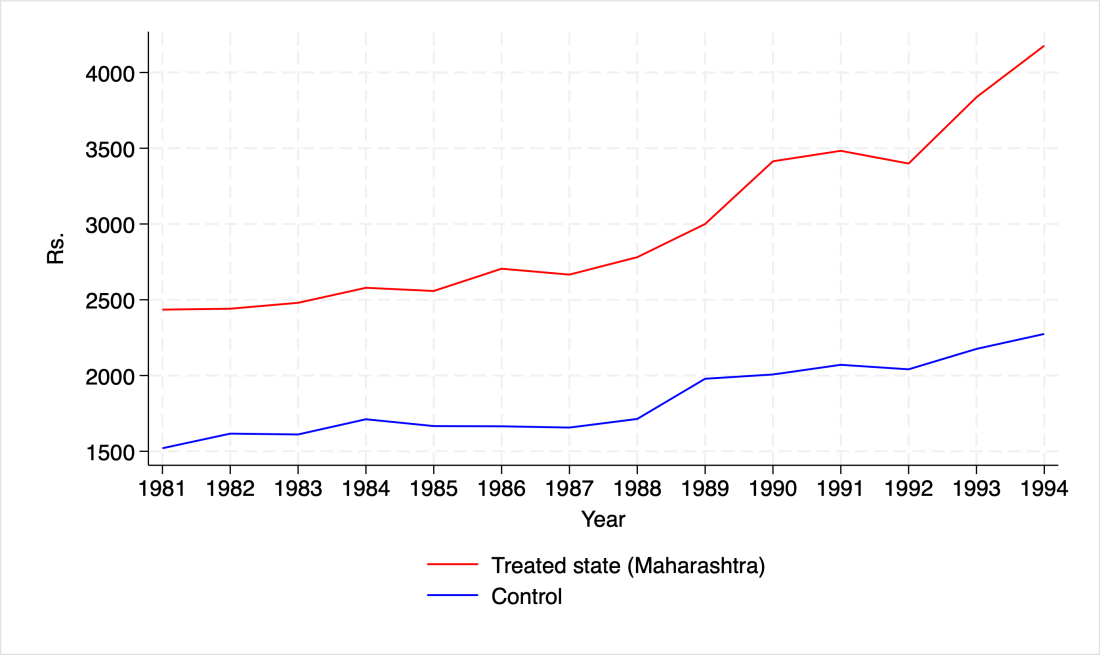
(c) Omitting Andhra Pradesh from control group



(d) Ommitting Mumbai from treatment group

Note: The table shows results of estimating specification 2 where the dependent variable is child marriage which is equal to 0 if the woman was married by the age of 18 and 1 otherwise. All regressions control for year of birth and district fixed effects, a dummy variable=1 if the woman reports to be lower caste and a dummy variable=1 if the woman is Hindu. Robust standard errors are clustered at the district level. The data comes from the 2016 and 2021 waves of the India National Family and Health Survey.Panel (a) plots the results of equation 1 using the entire rest of India as the control group rather than only bordering states. Panel (b) plots the results of equation 1 with the addition of wealth quintile fixed effects. Panels (c) and (d) ommit Andhra Pradesh and Mumbai from the sample respectively.

Appendix Figure 7: Per capita net state domestic product at constant (1980-81) prices by treatment status, 1981-1994



Note: Based on Government of India, Ministry of Statistics and Programme Implementation official economic statistics, file code GSDPCRNS. To calculate control group real net domestic product, I calculate implied population levels each year using total net domestic product values and per capita net domestic product values. Control group net domestic product per capita is calculated as the sum of control states total net domestic product divided by the sum of the control states implied population for each year.