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Unwanted daughters: The impact of a ban on sex-selection on the educational attainment of women

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Abstract

We study whether legal restrictions on prenatal discrimination against females leads to a shift by parents towards postnatal discrimination. We exploit the staggered introduction of a ban on sex-selective abortions across states in India to find that a legal restriction on abortions in India led to an increase in the number of females born, as well as a widening in the gender gap in educational attainment. Females born in states affected by the ban are 2.3, 3.5 and 3.2 percentage points less likely to complete Grade 10, complete Grade 12 and enter university relative to males. These effects are concentrated among non-wealthy households that lacked the resources to evade the ban. Investigating mechanisms, we find that the relative reduction in investments in female education were not driven by family size but because surviving females were now relatively unwanted. Discrimination is amplified among higher order births and among females with relatively few sisters. Finally, these negative effects exist despite the existence of a marriage market channel through which parents increase investments in their daughters' education to increase the probability that they make a high-quality match.

JEL-Classification: I21, J13, J16, O12, O15

Keywords: sex ratio, education, fertility, economics of gender, discrimination, abortion, India

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1 Introduction

Starting from the influential work of Amartya Sen in 1990, there has been a substantial body of research documenting the phenomenon of missing women in countries with a strong social and cultural preference for sons (Sen, 1992). Countries in Asia and North Africa have skewed sex-ratios emerging out of sex-selective abortions, female infanticide, and the gross neglect of the health and nutrition of females (Chao et al., 2019). In response to this demographic crisis, several countries, including India and China, have passed laws prohibiting sex-selective abortions in order to reverse alarming imbalances in the sex-ratio (Gupta, 2019). However, where such laws cannot change the underlying social norms driving son preference, they may simply encourage households to shift to gender discrimination at different margins (Goodkind, 1996).

The passing of abortion laws has been found to lead to improved health and educational outcomes for the marginal child (Gruber et al., 1999; Pop-Eleches, 2006; Ananat et al., 2009). More specifically, access to sex-selective abortions in countries with strong son preference has led to improved health outcomes for surviving females, even as the number of female births has declined (Lin et al., 2014; Hu and Schlosser, 2015; Anukriti et al., 2020). Surviving girls are both more wanted and more likely to be born into smaller families because parents are able to terminate unwanted female foetuses, leading to reductions in female infant and child mortality. Conversely, the removal of access to sex-selective abortions, while intended to improve female survival rates at birth, could also have the perverse effect of widening gender discrimination at other margins of parental investments.

This paper analyses the impact of a ban on sex-selective abortions on parental investments in the education of their daughters. The relative increase in the number of female births could affect educational attainment in a number of ways. One, in line with the quantity-quality trade-off observed with respect to health outcomes, an increase in the number of unwanted female births could lead to lower parental investments in schooling due to discrimination, including through the channel of poorer health outcomes. Two, females could be born into larger families after the ban since families resort to fertility-stopping rules, where they keep trying for sons, leading to increased competition among siblings for household resources (Clark, 2000). Three, changing access to sex-selection could alter the characteristics of households into which girls are born. On the one hand, sex-selective abortions in India have been found to be more prevalent among educated, urban, upper-caste, and wealthy women (Bhalotra and Cochrane, 2010; Jha et al., 2006; Borker et al., 2017). If the ban was successfully implemented for all families, girls could subsequently be born disproportionately into higher-income households, where they would receive higher levels of educational investments (Edlund, 1999). On the other hand, wealthier families may be better able to circumvent the ban on sex-selection through the use of expensive, underground private-sector abortion clinics, leading to more girls being born into poorer families, with lower investments in schooling. Four, the relative increase in females also results in a form of a "marriage squeeze" against females in the marriage market, leading to parents investing more in desirable characteristics of their daughters, including in their education, so as to increase the probability of their matching with a high-quality groom (Lafortune, 2013). The net impact on the gender gap in educational attainment is therefore ambiguous.

In this paper, we exploit intertemporal and geographical variation in India in the implementation of laws prohibiting the use of sex-screening technology and sex-selective abortions to identify the impact of the ban on the probability of female births and on the educational attainment of surviving females. Laws banning the use of sex-screening technologies were introduced to India gradually from 1988 to 2002, presenting us with a natural experiment which can be used to identify the impact of these laws on our outcomes of interest. The first state to pass such legislation was Maharashtra in 1988, with a national law extending to what were then 26 additional states, barring the state of Jammu and Kashmir (henceforth JK), in 1994. Finally, in 2002, a law was passed in JK as well.

We first examine whether the ban did have an impact on the probability of female births. Nandi and Deolalikar (2013) have previously used village-level measures of the sex-ratio to identify an increase in the birth of females after the ban. We are able to confirm these results, using individual-level data from the National Family and Health Survey, to identify the impact of living in a state where a ban was implemented (a treated state) after 1996 on the probability of a female birth. Following from previous studies that establish that the sex of the firstborn child is quasi-random and that the use of sex-selective abortions is concentrated among families that have a firstborn girl (Bhalotra and Cochrane, 2010; Anukriti et al., 2020), we estimate the impact of being born in a treated state after the ban was implemented into a household with a firstborn girl, compared to a household with a firstborn boy. We find a significant increase in the probability of a female birth of 2.5 percentage points, which is even larger among less wealthy households (in the bottom 60% of the wealth distribution). We next estimate the impact of the ban on the gender gap in educational attainment. We compare long-term schooling outcomes of females with males in treated states to find that the ban resulted in increased gender discrimination. Females were 2.3, 3.5 and 3.2 percentage points less likely to complete Grade 10, complete Grade 12 and enter university in treated states, where there had been a significant increase in female births. Again, these results are driven by changes in educational investments made by less-wealthy households in the bottom 60% of the wealth distribution. Finally, we explore additional mechanisms that explain our results. We find that our results are driven by the fact that the surviving girls are relatively unwanted, particularly in non-wealthy households, rather than by an increase in the total number of children due to the use of the fertility stopping rule. We also find marriage market effects of the increase in female births after the ban, with women more likely to be unmarried by the ages of 15 and 18, and an increase in the age at marriage for those who are married. This suggests that the negative impact on the education of girls would have been further amplified in the absence of these marriage market effects.

While there have been several studies that have analysed the impact of changing access to sex-selective abortions on health outcomes, there is little work done on the impact on educational attainment. Anukriti (2013) examines the impact of changing sex ratios on the educational outcomes of women, finding that an increase in male-female ratios leads to a reduction in educational attainment for women relative to men, due to an increase in the bargaining power of women in the marriage market. There are a few ways our analysis is different. Her main explanatory variable, sex ratio at birth, is defined and constructed at the state-year level. This variable could be correlated with other state-level outcomes, such as changes in development, rural infrastructure, and societal norms, that affect women's education. Legal interventions targeted at sex ratios, on the other hand, provide an opportunity to test the impact of changing sex ratios on the educational attainment of women, without the usual endogeneity concerns that affect such analyses. Utilising an exogenous legal change, Kalsi (2015) finds that the legalisation of abortion in Taiwan led to an increase in university enrolment rates among females born at higher birth orders, but not for males, and argues this is an example of parents substituting from postnatal discrimination towards prenatal discrimination. However, in her paper, the change that takes place affects the whole country at once and it is difficult to separate out time trends in female educational attainment from the impact of the legal change. Our paper adds to this literature by exploiting the staggered implementation of a ban on sex-selective abortions in India to present causal estimates of the effect of changes in sex ratios on the gender gap in educational attainment.

In doing so, we also provide robust evidence using alternative data to confirm the results of Nandi and Deolalikar (2013) on the impact of the legislation banning sex selective abortions on the probability of female births. Nandi and Deolalikar (2013) use measures of village-level sex-ratios taken from the Census in 1991 and 2001 to show that the passing of this legislation led to a relative increase in the number of females aged from 0-6 years in treated states. Nandi (2015) uses childbirth data from the District Level Household Survey (DLHS) to confirm that the ban led to an increase in female births in the treated state. However, one weakness of these two studies is that they only take a single control state, Maharashtra, into account in their analysis. We re-estimate and confirm their results using much more granular data on the complete fertility history of approximately 235,000 women and using the quasi-random exogeneity of the sex of the firstborn child to show that the probability of a girl being born increases in treated states, to families with firstborn girls when compared to families with firstborn boys. We also include two control states in Maharashtra and JK.

Finally, this paper contributes to a literature on the unintended consequences of laws and regulatory actions aimed at improving the welfare of girls and women, particularly in a patriarchal environment with strong social norms around son preference. Bhalotra et al. (2018) find that passing legislation guaranteeing equal inheritance rights to women exacerbates son-preference among Indian parents, leading to an increase in female foeticide, mortality and son-biased fertility stopping. Anukriti (2018) finds that a conditional cash transfer program offered by a state government in India to reward couples for having either fewer children or more girls actually led to an increase in the male-female sex ratio as families with a strong preference for sons became more likely to have only one child – as long as the child was male, despite there being higher financial incentives offered for a single girl child. Our evidence on the impact of educational outcomes also raises concerns that punitive measures against the use of sex-selective abortions that only address a proximate cause of a skewed sex ratio can simply lead to other forms of gender discrimination and the relative neglect of females.

Section 2 of this paper provides the context on the ban on sex-selective abortions in India as well as the theoretical motivation for this paper. Section 3 describes the data, Section 4 describes the empirical strategy, and Section 5 presents the results, as well as several robustness checks. Section 6 explores the underlying mechanisms driving the results and Section 7 discusses the findings of the paper.

2 Background and theoretical motivation

2.1 The ban on sex-selective abortions

The social, religious, and economic structures of south Asian and North African countries have led to strong son-preference amongst parents. Sen (1992) calculated that this son preference drastically reduced the number of women in these populations, with approximately 100 million women missing worldwide, of which 37 million were in India, as a direct consequence of the gross neglect of female nutrition and health. The introduction and rapid spread of prenatal sex detection technology (such as ultrasound technology, amniocentesis, etc.) in India in the 1980s further allowed parents to manipulate the sex composition of their children (Hu and Schlosser, 2015; Anukriti et al., 2020). Sex-detection techniques were first introduced to India in 1971, followed by a rapid rise in the number of clinics providing sex determination and abortion services in the 1980s (Bhalotra and Cochrane, 2010; Nandi and Deolalikar, 2013). Bhalotra and Cochrane (2010) estimate that the diffusion of these technologies led to the selective abortion of as many as 480,000 girls per year between 1995-2005. As a result, there was a steep fall in female-male child sex ratios from 964 girls per 1000 boys in 1971 to 914 girls per 1000 boys in 2011.[1]

In response to the increasingly imbalanced sex ratios, the Pre-Conception and Pre-Natal Diagnostics Techniques Act (henceforth the PNDT Act) was passed by the Indian national Parliament in 1994 and came into effect in 1996. The main aim of the act was to stop female foeticide by prohibiting the use of prenatal diagnostic methods, such as ultrasound and amniocentesis technology, for sex-detection. By preventing the use of screening techniques, the PNDT Act was indirectly a ban on sex-selective abortions.

The implementation of the ban, however, was staggered across different states over a fourteen-year period. The state of Maharashtra had already implemented its own version of the PNDT act called the Maharashtra Regulation of Pre-natal Diagnostic Techniques Act of 1988, enacted in 1989. Second, the PNDT Act of 1994 did not apply to the state of JK due to the state's status under the Indian Constitution allowing for its complete autonomy in internal administration. The Jammu And Kashmir Preconception and Prenatal Sex Selection/Determination Act was subsequently passed in 2002. As a result, both JK and Maharashtra remained unaffected by the passing of the national PNDT Act in 1994.

[1]These ratios are calculated from Census data for total number of children in the 0–6 years age group.

Several studies have argued that this ban was ineffective because sex ratios continued to worsen after its implementation (George and Dahiya, 1998; Arnold et al., 2002; Visaria, 2008; Kishwar, 2015). However, they ignore the fact that the fall in female-male sex ratios could have been far greater if it were not for the legal intervention, given the improvements and increasing affordability of diagnostic technology during this period. In particular, this research fails to account for heterogeneity across states due to the staggered implementation of the ban and does not compare the rate of change in sex-selection across states that were early or late adopters of the ban (Nandi and Deolalikar, 2013).

This was also a period of wider diffusion of sex-screening technologies, such as the ultrasound. Bhalotra and Cochrane (2010) and Anukriti et al. (2020) find that sex-selection increased both during an early diffusion period of this technology from 1985-1994, when ultrasound scanners were first imported into India, as well as during a late diffusion period from 1995-2005, when ultrasound scanners were manufactured locally. They find that sex-selection increased in both these time periods, compared with their baseline period of 1973-84. Given that these results are relative to an earlier time period and do not incorporate any cross-sectional variation across Indian states generated by changing access to sex-screening technologies, we find these results are consistent with ours. Even as the practice of sex-selection became gradually more widespread in all parts of India, there is likely to be variation across states in access to these technologies during this period.

2.2 Theoretical motivation

With the introduction of the PNDT act, we would expect that parent's ability to manipulate the sex composition of their children would be suddenly inhibited in all newly treated states, with the exception of Maharashtra and JK. This would lead to the increase in total number of girls born in these families. With no change in the underlying social norms of son preference in a patriarchal society, it is likely that many of the girls born after the implementation of the ban are unwanted by their families, leading to lower investments being made in their health, nutrition, and education. The impact on the education of females in particular could be both a direct consequence of lower resources being allocated towards their schooling, or an indirect consequence of their poorer health, due to low early investments in healthcare, feeding back into worse educational outcomes.

Sex-selective abortion in India has been found to be more prevalent among wealthier families (Jha et al., 2006; Bhalotra and Cochrane, 2010; Borker et al., 2017).

Borker et al. (2017) explain this as an endogenous feature of their marriage market model of positive assortative matching within castes. If the ban was successfully implemented for all families, girls born after the ban would be born disproportionately in higher-income households, where they would receive higher levels of educational investments. However, if liquidity-constrained households are unable to get access to private ultrasounds and abortion facilities that can allow them to circumvent the effects of the ban, then the effects on female births and education will be amplified among non-wealthy households.

Households in treated states will therefore experience more female births after the introduction of the ban, and the gender gap in schooling will decline through both a selection effect and a treatment effect: if more girls are born into less wealthy families after the implementation of the ban, the average years of schooling for women would mechanically decrease, as women in wealthier families are more likely to have higher years of schooling relative to those in poorer families. The relative increase in unwanted girls in poorer families would also reduce investments in the health and education of girls in those families, relative to similarly situated families in control states.

Further, sex-ratios becoming less male-biased would also have impacts on women's bargaining power in the marriage market. With more women in the marriage market, we would expect to see the average age of marriage increasing for women as they take longer to find suitable matches (Angrist, 2002; Abramitzky et al., 2011), and a possible reduction in the spousal age gap as there are fewer unmarried older men in the population (Edlund, 1999). Moreover, parents of daughters may increase investments in desirable characteristics such as education so as to increase the probability of their daughters making a high-quality match. Since education is perceived to have high returns in the marriage market, especially for women, parents would increase investments in the schooling of their daughters in response to the increasing relative scarcity of marriageable men. The net effect on the educational attainment of women would be theoretically ambiguous and remains to be empirically tested.

3 Data and descriptive statistics

This paper uses survey data from the fourth round of the National Family Health Survey – NFHS-4 – which is a nationwide household survey representative at the district-level of India (International Institute for Population Sciences and ICF, 2017). The survey includes 601,509 households, with a total of 699,686 women and 112,122 men, from both rural and urban areas of each of the 640 districts listed in the 2011 Census, covering all states and union territories.

Information is collected on the fertility history of all women between the ages of 15 and 49 in every sampled household. We use these fertility histories to create a child-level dataset of all births that took place to surveyed women, and we use this dataset to estimate the impact of the ban on female births. Information is also collected on the educational attainment of every person above the age of five in each of the sampled households. We create a dataset of educational attainment of 700,214 men and women born during the period of implementation of state-level and national-level bans on sex-selective abortions, between 1989 and 2002, to estimate the impact of the ban on educational outcomes. For further analysis disaggregating our results by sibling composition, we are able to match unmarried children to their mothers from the household roster. For marriage outcomes, we get information on whether women are married from the household roster itself. We are also able to match married women to their husbands through the household roster.

Table 1 presents the unadjusted sample means of each of our main dependent variables – the probability of a female birth and educational attainment measured by completion of Grade 10, completion of Grade 12 and entry into university. For probability of female births, we present the difference between female births in households with firstborn females and female births in households with firstborn males. For educational attainment, we present the gender difference between female and male educational attainment.

4 Empirical strategy

To identify the impact of the ban on sex-selective abortions, we exploit the geographical and inter-temporal variation in the implementation of the ban. The national ban which came into effect in 1996 did not affect Maharashtra, which had already enacted its own ban in 1988, or JK, which did not pass a ban on sex-selection until 2002. These two states comprise the control states in the 1996 treatment of the ban on sex-selection. The treated group includes all other 34 Indian states. All these states with their treatment status are depicted in Figure A1 in the appendix.

The time period of interest is divided into the pre-treatment period (1989-1996) and the post-treatment period (1997-2002), and is restricted at both ends by the control states' introduction of their own bans: the Maharashtra Regulation of Pre-natal Diagnostic Techniques Act of 1988, implemented in 1989, and the Jammu And

Kashmir Preconception and Prenatal Sex Selection/Determination Act of 2002, implemented in 2003. Our sample, therefore, includes all women born between 1989 and 2002. We use the current age provided at the time of interview to assign the birth years of women. Since individuals born in 1996 could have been conceived before the ban came into force, we drop all people born in the year 1996 from the sample.

4.1 Impact on female births

Before studying the impact of the ban on the gender gap in educational attainment, we first establish the impact of the ban on the probability of female births. To identify this impact, we exploit three sources of variation based on the year of birth, state of residence, and whether the child was born into a household with a firstborn girl or firstborn boy, to estimate a triple difference-in-differences estimator. The year of birth captures whether the child was born before or after the the implementation of the ban in 1996. The state of residence captures whether they were born in a control or a treated state. The third source of variation is the sex of the firstborn child. Previous studies have identified that the sex of the firstborn child is both quasirandom and has an impact on whether families opt for sex-selective abortion in the future (Bhalotra and Cochrane, 2010; Bhalotra et al., 2018; Anukriti et al., 2020). This is based on the assumption that families rarely opt for sex-selective abortions during the first birth. Families with firstborn girls are much more likely to opt for sex-selective abortions for their later children to attain their desired sex composition, when compared to families with firstborn boys. The sex of the firstborn child, therefore, presents a source of exogenous variation in whether a family is more or less likely to be affected by a ban on sex-selection. Children born after a ban on sex selection was implemented in their state to mothers who already had a firstborn daughter constitute the group most likely to be affected by the ban. These are families could have wanted to opt for sex-selective abortions for children born at a birth order greater than two, but are unable to detect the sex of their child after 1996 due to the ban.

The data for this analysis constitutes all births to all female respondents of the NFHS-4 survey, that took place between 1989 and 2002, at a birth order of greater than one. We estimate the following equation:

$$
\begin{aligned}
FemaleBirth_{ist} = \beta_0 &+ \beta_1 Treat * Post * FirstbornGirl_{ist} + \beta_2 Treat * Post_{ist} \\
&+ \beta_3 Treat * FirstbornGirl_{ist} + \beta_4 Post * FirstbornGirl_{ist} \\
&+ \gamma_t FirstbornGirl_i + \phi_s FirstbornGirl_i \\
&+ \mathbf{X'_{ist}}\tau + \omega_{st} + \epsilon_{ist}
\end{aligned} \tag{1}
$$

The dependent variable, $FemaleBirth_{ist}$, is a binary indicator for whether a child i of birth order 2 or higher, born in state s in the year t, is a female. $Treat_s$ indicates if the child was born in a treated state (any of the 34 states other than Maharashtra or JK). $Post_t$ indicates if the child was born in the post-ban period (1997-2002). $FirstbornGirl_i$ indicates if the child is born in a family where the firstborn is a girl.

Since equation (1) is specified as a linear probability model, the impact of the ban on the probability of a female birth can directly be inferred from the coefficient on the triple interaction variable, β_1. This measures the differential impact of the ban on the probability of a female child being born in a treated state in a household with a firstborn girl, relative to a household with a firstborn boy.

X_{ist} includes socioeconomic and demographic characteristics of the household: caste, religion, education and sex of the household head, residence in a rural area and household wealth quintile. We also include fixed effects for $FirstbornGirl$, states and birth cohorts, as well as the pairwise interactions of all three ($\gamma_t FirstbornGirl_i$, $\phi_s FirstbornGirl_i$, and ω_{st}). This gives us a flexible specification allowing for birth cohort effects to vary by state and by firstborn sex, and for state fixed effects to vary by firstborn sex. Since the ban on sex-selection was at the state level, standard errors are clustered at the state level. We also present p-values from a wild cluster bootstrap (Cameron et al., 2008), correcting for the small number of untreated clusters.

4.2 Impact on female educational attainment

We next test the main hypothesis of this paper: that the ban had a differential impact on the educational attainment of women relative to men in treated states. Two sources of variation are identical to those in the previous section: state of residence and year of birth. We additionally interact these two terms with an indicator for female to identify the differential impact of the ban on women relative to men. The data for this analysis includes all individuals listed in the household roster of the NFHS-4 households that were born between 1989 and 2002. The estimating equation is the following:

$$
\begin{aligned}
Y_{ist} = \beta_0 &+ \beta_1 Treat * Post * Female_{ist} + \beta_2 Treat * Post_{ist} \\
&+ \beta_3 Treat * Female_{ist} + \beta_4 Post * Female_{ist} \\
&+ \gamma_t Female_i + \phi_s Female_i \\
&+ \mathbf{X'_{ist}}\tau + \omega_{st} + \epsilon_{ist}
\end{aligned}
\tag{2}
$$

The dependent variables represented by Y_{ist} are indicator variables for different levels of education attained by person i in state s born in the year t: completing Grade 10, completing Grade 12, and entering university. $Treat_s$ indicates if the person was born in a treated state (all states other than Maharashtra or JK). $Post_t$ indicates if the person was born in the post-ban period (1997-2002). $Female_i$ indicates if the person is a female.

The coefficient β_1 captures the differential impact of the ban on the educational attainment of women in treated states relative to men. Under the assumption that the main mediating factor between the ban and education of women is the ban's impact on the probability of a female birth, β_1 estimates the effect of the increased birth of females on female educational attainment in treated states.

X_{ist} includes the same socioeconomic and demographic characteristics of the household as in the previous section: religion, caste, sex of the household head, education of the household head, residence in a rural area and household wealth quintile. We also include fixed effects for $Female_i$, states and birth cohorts, as well as the pairwise interactions of all three ($\gamma_t Female_i$, $\phi_s Female_i$, and ω_{st}). Again, we allow for a flexible specification where birth cohort effects vary by state and by sex, and state fixed effects vary by sex. Since the ban on sex-selection was at the state level, standard errors are clustered at the state level. We also present p-values from a wild cluster bootstrap Cameron et al. (2008), correcting for the small number of untreated clusters.

5 Results

5.1 Impact on female births

The results of equation (1) are presented in Table 2. Across columns 1-3, the coefficient on the triple interaction term, $Treat * Post * FirstGirl_{ist}$, is positive and significant, indicating an increase of 2.5-2.6 percentage points in the probability of a birth being female in households with firstborn girls, relative to households with firstborn boys, in treated states after the ban was implemented. All specifications include fixed effects for all pairwise interactions between $Treat$, $Post$, and $FirstbornGirl$, an indicator for whether a child is born into a house with a firstborn girl, as well as state fixed effects, birth year fixed effects. Specifications in columns 2 and 3 control for household characteristics including religion, caste, whether the household head is male and his or her highest educational attainment, whether the household is located in a rural area, and household wealth quintile. The specifications in column 3 additionally controls for all pairwise interactions of fixed effects between state, birth year and firstborn girl (that is, state-year fixed effects, state-firstborn girl fixed effects, and firstborn girl-birth year fixed effects). We are, therefore, confident that we have controlled for confounding trends in educational attainment across these different dimensions. In the specification in column 4 we control for mother fixed effects. It is only with the inclusion of mother fixed effects that the coefficient on the triple interaction, while still positive is no longer statistically significant, implying that the results are driven by changes not within families but at the extensive margin, with a larger number of families being more likely to have daughters.

We next disaggregate the results across wealthy and non-wealthy households. As discussed above, wealthy families have a greater ability to evade the ban on sex-selection than households that are liquidity constrained, so we would expect to see larger effects for less-wealthy households in treated states. These results are presented in Table 3, where we separate the samples into births in wealthy and non-wealthy households, with wealthy households belonging to the upper two wealth quintiles (top 40% of the household wealth distribution) and non-wealthy households belonging to the lower three wealth quintiles (bottom 60% of the household wealth distribution).

The coefficient on $Treat * Post * FirstGirl_{ist}$, is large, positive and statistically significant across the first three columns for non-wealthy households, implying a 4.1-4.3 percentage point increase in the probability of a female birth in households with firstborn girls in treated states. Conversely, the coefficient is insignificantly different from zero in all columns for the non-wealthy sample, suggesting that the main impact of the ban is driven by liquidity-constrained households. As above, these results are robust to the inclusion of controls for household characteristics, as well as flexible time trends in female births at the state level and across firstborn girl and firstborn boy households.

These results are in line with those reported by Nandi and Deolalikar (2013), though we use a different dataset of birth histories at the level of the mother, a richer set of controls, fixed effects and time trends, and two control states in Maharashtra and JK. We find that the implementation of the ban on sex-selection led to an increase in the number of female births in treated states relative to control states, particularly among less-wealthy households who find it more difficult to get access to private sources of sex-screening technologies and sex-selective abortions.

5.2 Impact on female educational attainment

Given the positive impact of the ban on the birth of female children in treated states, we next turn to the main hypothesis we test in this paper: that the increasing number of unwanted girls born after the ban results in a decrease in their educational attainment. The results of equation (2) are presented in Table 4. The upper panel presents the impact of the ban comparing all 34 treated states to the control states of Maharashtra and JK, while the lower panel categorises treated states as only those nine states bordering the control states of Maharashtra and Kashmir. These border states include Gujarat, Madhya Pradesh, Chhattisgarh, Telangana, Andhra Pradesh, Goa, and Karnataka for Maharashtra, and Himachal Pradesh and Punjab for JK. The comparison states for Maharashtra are identical to those specified by Nandi and Deolalikar (2013), with the addition of Goa.

The outcome variables of interest are whether a person has completed Grade 10 (10 years of formal schooling), completed Grade 12 (12 years of formal schooling), and enrolled in a tertiary higher education institute (above 12 years of formal schooling). Across all 6 columns, β_1, the coefficient on the triple interaction term, $Treat * Post * Female_{ist}$, is negative and statistically significant indicating that the growth in female educational attainment in treated states was slower after the implementation of the ban, compared to the control states of JK, and relative to male educational attainment. The probability of graduating Grade 10, Grade 12, and entering university is 2.3, 3.5, and 3.4 percentage points lower, respectively, for females compared to males, in treated states compared to control states. When taking into account the correction for small clusters, the precision of the estimates reduces but the coefficients remain significant for graduating Grade 10 when using data from all states, and for graduating Grade 10 and entering university as well as marginally significant for graduating Grade 12, when restricting the sample to border states.

All specifications include fixed effects for all pairwise interactions between $Treat$, $Post$, and $Female$, as well as state fixed effects, birth year fixed effects and an indicator for whether the individual is a female. Specifications in columns 1, 3 and 5 control for household characteristics including religion, caste, whether the household head is male and his or her highest educational attainment, whether the household is located in a rural area, and household wealth quintile. As above, specifications in columns 2, 4 and 6 additionally control for all pairwise interactions of fixed effects between state, birth year and female (that is, state-year fixed effects, state-female fixed effects, and female-birth year fixed effects), controlling for confounding trends in educational attainment across these different dimensions.

These results are consistent with the increased incidence of female births in treated states that we discussed in the previous section. An increase in the number of unwanted girls in families that are now unable to get access to sex-selective abortion technology, substitute postnatal discrimination for prenatal discrimination, and relatively reduce their investments in their daughters, in line with the results documented in Anukriti et al. (2020) for health outcomes.

The two control states of Maharashtra and JK are defined as such because neither saw a change in the status of the legislation banning sex-detection through the use of screening technologies. However, it is possible that the impact of treatment with this legislation is heterogeneous and asymmetric with respect to always-takers and never-takers of the treatment. To rule out this possibility, we estimate these results using just Maharashtra and just JK as control states. These results are presented in the appendix, in table A1, and the estimated coefficients are of a very similar size in both cases.

In the previous section, we established that the impact of the ban on the probability of a female birth was largely driven by the increased birth of females among non-wealthy households in the sample. In Table 5, we disaggregate the results on the impact of the ban on education as well, across wealthy and non-wealthy households, in the top 40% and bottom 60% of the household wealth distribution respectively. In line with results on female births, we find that the impact of the ban on female educational attainment is larger for non-wealthy families; the coefficient β_1 is negative, larger, and significant across all specifications and for all three outcomes. Among non-wealthy households treated by the ban, the probability of completing Grade 10 for females reduces by 4.1 percentage points, of completing Grade 12 reduces by 5 percentage points, and of entering university reduces by 3.5 percentage points, relative to males, when comparing outcomes across treated and control states. In contrast, among wealthy households, our estimates are smaller in size and insignificantly different from zero. When the sample is restricted to the two control states and their bordering states, we find a similar result. In non-wealthy households treated by the ban, the probability of a female completing Grade 10, completing Grade 12, and of entering university reduces by 3.9, 4.5 and 3.9 percentage points respectively, relative to males, in treated states in comparison to control states. Conversely, for wealthy families the effects are insignificantly different from zero.

5.3 Robustness

5.3.1 Differential pre-treatment trends for educational outcomes

Although we control for fixed effects for female, state, birth year, as well as pairwise interactions between all three, we further demonstrate that there is no evidence of differential pre-treatment trends across treated and control states prior to the introduction of the nationwide ban in 1996. We regress the probability of graduating Grade 10, Grade 12, and entering university on the triple interaction between treat, female and birth year, and control for the full set of fixed effects for female, state, birth year and the pairwise interactions between all of them. We also include all controls for household characteristics that were included in the main regressions. The coefficients on the interactions between treat, female and birth year, are shown in Figure 1. There is no evidence of any differential trends prior to 1996, the year of treatment.

5.3.2 Changes in son preference

While we argue that the changes in the birth of females are driven by changing access to ultrasound technology for the purposes of sex-detection, it could be possible that there are differential changes in the desired fertility and son preference of parents in treated and control states, taking place around the same time that the ban was implemented. While we allow for changes in fertility and son preference over time by including state-birth year fixed effects, as well as female-birth year fixed effects, we also directly explore whether son preference follows differential time trends across treatment and control states. For this analysis, we follow Bhalotra et al. (2018) and Anukriti et al. (2020) in using answers to questions on the ideal number of sons, ideal number of daughters, and ideal number of children of either sex in the NFHS-4 survey to create a variable called the ideal fraction of boys wanted by a woman, as a measure of her son preference. We estimate a version of equation (1):

$$
\begin{aligned}
IdealFractionBoys_{ist} = &\ Treat * Post * FirstbornGirl_{ist} + \beta_2 Treat * Post_{ist} \\
&+ \beta_3 Treat * FirstbornGirl_{ist} + \beta_4 Post * FirstbornGirl_{ist} \\
&+ \gamma_t FirstbornGirl_i + \phi_s FirstbornGirl_i \\
&+ \mathbf{X'_{ist}}\tau + \omega_{st} + \epsilon_{ist}
\end{aligned} \tag{3}
$$

The dependent variable is $IdealFractionBoys_{ist}$, which is the ideal fraction of sons wanted by mother i in state s at time period t. As in previous estimations, $Treat_s$ indicates if the child was born in a treated state (all states other than Maharashtra or JK), while $FirstbornGirl$ indicates if the mother had a firstborn daughter as opposed to a firstborn son. $Post_t$ indicates if woman's first child was born in the post-PNDT period (1997-2002). Assuming the woman's self-reported fertility preferences at the time of the survey in 2015/2016 reflect her preferences around the time she had her first child, the coefficient of interest, β_1, estimates the differential impact on the desired number of sons for women in treated states relative to control states, after the implementation of the ban. In a country with rigid and static social norms around son preference, we believe this is a reasonable assumption. As before, we also control for household characteristics and fixed effects for state, year of birth of first child, firstborn girl, and pairwise combinations of all three.

The results for this regression are given in Table A2 in the appendix. In both columns 1 and 2, the coefficient on $Treat * Post_{ist}$ is insignificantly different from zero, suggesting that womens' degree of son-preference appears to have not changed over time in treated states differently from control states. In columns 3, 4 and 5, the coefficient on $Treat * Post * FirstbornGirl_{ist}$ is close to zero, further establishing that there are no differential changes in son preference between mothers with firstborn girls and firstborn boys.

6 Mechanisms

In the previous section, we argue that that the increase in the number of unwanted girls has led to lower investments in their human capital. In this section we discuss potential mechanisms that can explain our results. The first channel through which the gender gap in education could increase is through a selection effect whereby girls are more likely to be born into families that cannot afford access to increasingly expensive sex-screening and sex-selective abortion technologies. As a result, girls are more likely to be born into less-wealthy families than previously, driving down their education levels, relative to boys. As we discuss, girls being differentially born into households that are less able to invest resources in them is clearly one important mechanism at play since our results are driven by changes in the behaviour of households in the bottom 60% of the wealth distribution. However, wealth alone cannot explain the change in behaviour among non-wealthy households before and after the implementation of the ban in treated states relative to control states. We explore potential mechanisms with a focus on households in the bottom 60% of the wealth distribution since these are the households that are driving the results on the impact on education.

Another dimension of selection that we explore is the limited access of households located in rural areas to alternative sources of sex-screening technologies in the private sector, compared to households in urban areas. We show heterogeneity in the results on the impact on education across whether households are located in rural or urban locations below.

In addition, changes in family behaviours on account of the implementation of the ban on sex-selection will also explain these results. After the ban, girls are more likely to be born into families where they are less wanted, leading to increased discrimination against them. Since most girls after the ban are relatively more likely to be born into families which would have previously resorted to sex-screening and sex-selective abortions, these girls will face increasing levels of discrimination. While this channel is clearly an important one, its effect could be further mediated by other factors. First, girls could be increasingly born into larger families as parents resort to fertility stopping rules to have a desired number of boys, now that they are unable to rely on sex-selective abortions. Siblings face more rivalry for fixed resources in larger families, which could lead to an increasing gender gap. Second, the impact on gender gaps in education of more girls being born is likely to be mediated by sibling age and sex composition, including birth order, number of brothers and sisters, and whether the family had a firstborn son or not. Third, the increasing number of girls being born could lead to more competition between women for men in the marriage market. The resulting "marriage squeeze" caused by the relative increase in the number of women (Caldwell et al., 1983) could lead to women making compensatory investments to increase the probability of their making a higher quality match, including through higher investments in education. We discuss each of these mechanisms in turn below.

6.1 Impact in rural and urban areas

In addition to wealth, physical access to illegal sources of sex-screening and sex-selection technologies is also likely to affect exposure of households to the treatment. Households in rural areas have lower access to health facilities in general, including private sector health facilities, compared to households in urban areas. These households are more likely to be affected by the ban, facing a higher incidence of female births as well as a bigger potential impact on schooling. We accordingly re-estimate equations (1) and (2) separately for households located in rural and urban areas. The results are presented in the appendix in Table A3. All of the results on education are being driven by households located in rural areas. Female births are between 6.2 and 6.4 percentage points more likely to occur in rural households than in urban households in treated states after the implementation of the ban, when comparing families with firstborn girls and firstborn boys. Similarly, women from rural households in treated states are 6.2, 6.1 and 4.5 percentage points less likely than men to complete Grade 10, complete Grade 12 and enter university, respectively, after the implementation of the ban. In contrast, there is no impact on the gender gap in education in urban households in treated states.

This analysis does not account for the fact that men and women may grow up in a rural (urban) area and then migrate away to an urban (rural) area, particularly after marriage. 20% of men and 40% of women are observed to be married. There is no way to identify if they spent their school-going years in the same location; however, given the historically low rate of rural-urban migration in India (Munshi and Rosenzweig, 2016), we do not believe this is an important source of bias in our results.

6.2 Impact on fertility

For families that continue to have a strong preference for sons but who now find it relatively more difficult to get access to sex-screening technologies and sex-selective abortions, it is plausible that they will aim to achieve their desired sex composition among their children through changes in fertility stopping behaviour. In particular, such families may continue to have children until they have a son. We would then expect to see higher fertility among families with firstborn girls in treated states after the implementation of the ban. Should this be the case, girls born in treated states may face a penalty in the allocation of household resources because of being born into larger families, compared to boys.

To assess if this mechanism is affecting eventual educational attainment by women, we follow Anukriti et al. (2020) in testing for this in two ways. First, we use a mother-year dataset of births and estimate equation 1 with the dependent variable as an indicator of whether a birth has taken place in a given year. We redefine *Post* to include all women who had all of their children after 1996, compared to women who had all of their children before 1996. Mothers who have children both before and after 1996 are excluded from the sample. We include all controls as in equation 1 and additionally control for mother's age fixed effects and birth order fixed effects. If mothers of firstborn girls are more likely to have children in treated states after the implementation of the ban, we would expect to see a positive coefficient on the term $Treat*Post*FirstGirl$. Second, we regress total fertility and excess fertility of every mother on the term $Treat * Post * FirstbornGirl$, where *Post* is redefined to include all mothers in the sample who have all of their children after 1996, as opposed to all mothers in the sample who have all of their children prior to 1996. Total fertility is defined as the total number of children a mother has had, while excess fertility is defined as the difference between the total number of children a mother has had and her self-reported ideal number of children. We further include all pairwise interactions between $Treat$, *Post* and $FirstbornGirl$, as well as fixed effects for state, birth year, firstborn girl, and pairwise interactions between all three.

These results are shown in the appendix in Table A4. Considering the coefficient on the triple interaction term, $Treat * Post * FirstbornGirl$, we find no significant effect on the probability of a birth, total fertility or excess fertility in any of the specifications. The coefficient on *Post* $*$ *FirstbornGirl* is positive in columns 1, 3, and 5, indicating that families with firstborn girls did have higher fertility after the introduction of the ban compared to before, but there are no differences between any of the treated and control states in this trend. In columns 3-4, we additionally control for ideal number of children and ideal fraction of boys, while in columns 5-6, we control for ideal fraction of boys. As expected, the total number of children born to a woman increases in her ideal number of children and ideal fraction of boys.

Therefore, while families are more likely to have more daughters in treated states after the implementation of the ban, the total number of children born to each family does not significantly change. The likely mechanism for the impact of the ban on educational outcomes, therefore, is not the reduced allocation of resources per child due to a higher number of children within families with firstborn girls.

6.2.1 Age and sex composition of siblings

Previous research has established that gender discrimination in the allocation of household resources across siblings is mediated by the age and sex composition of children within a family. In families with more than one child, children become rivals for resources, and in societies with strong son preference, this competition for household resources furthers gender discrimination. First, children of higher birth orders typically receive lower investments than children of lower birth orders in societies with strong elder son-preference, but whether the gender gap changes with birth order is theoretically ambiguous (Behrman, 1988; Jayachandran, 2017). While both laterborn girls and boys are disadvantaged relative to early-born girls and boys, laterborn girls could face a relatively larger penalty if they are born into larger families, or if they are more likely to be born into families that practice gender discrimination. In our context, if discrimination against girls is amplified at higher birth orders, then we would expect to see larger effects of the ban on sex-screening technologies on laterborn girls relative to firstborn girls.

Second, siblings with sisters face a smaller penalty in terms of the household resources they receive compared to siblings without sisters in societies with credit-constrained families and strong male bias (Parish and Willis, 1993; Garg and Morduch, 1998; Morduch, 2000; Lei et al., 2017). Since investments in the human capital of boys is perceived to have a higher return, the addition of more girls to a family reduces the competition faced by existing siblings and increases investments made in them. Again, whether the benefits of additional sisters is greater for boys or for girls is theoretically ambiguous. Garg and Morduch (1998) finds no differences in the impact of having more sisters by gender in sub-Saharan Africa, suggesting that the shape of returns to human capital investments are similar across boys and girls. In contrast, Lei et al. (2017) find that men in China tend to benefit more from having sisters than women, perhaps because more resources are concentrated in them because they are educated to higher levels. If a similar result holds in India, we would expect to find increasingly wider gender gap in educational attainment among men and women with sisters, compared to men and women without sisters.

Third, as we discuss previously, the use of sex-screening technologies and sex-selective abortions is concentrated among families with firstborn girls in India, compared to families with firstborn boys (Bhalotra and Cochrane, 2010; Anukriti et al., 2020). Given this, it is plausible that families with firstborn girls in treated states are more likely to experience the birth of unwanted daughters after 1996. In these families, we may expect to see a larger impact of the ban on sex-screen technologies on educational outcomes.

We next test for whether the impact of the ban on educational outcomes also varies across these different dimensions. In order to do this, we use the birth histories of all women in the NFHS-4 survey to identify birth order of all children and whether they are born into families with firstborn girls or boys. These variables are then matched back to the educational attainment recorded in the household rosters. This means that while the matching is successful for individuals who are still living in their households of birth, it is not successful for women who are married and are no longer living in their household of birth. To ensure that the samples remain comparable, we re-estimate equation (2) for this smaller sample as well in table A5. The results are very similar as those estimated previously in Table 5, suggesting that non-random selection into early marriage may not be biasing our results.

The results of the analysis of the impact of sibling age and sex composition are presented in Table A6. In addition to all the control variables and fixed effects we include in the previous estimations, we also control for the total number of children in all specifications. In the first panel, we consider the impact of the ban on educational outcomes separately for firstborn children (columns 1, 3 and 5) and laterborn children (columns 2, 4 and 6). The negative impact of the ban on the educational outcomes of women is relatively larger when comparing men and women born at higher birth orders, than it is when comparing firstborn men and women. Firstborn women face no penalty relative to firstborn men in treated states compared to control states when considering any of the measures of educational attainment. On the other hand, laterborn women are 5.9 percentage points less likely to complete Grade 10, 8.7 percentage points less likely to complete Grade 12 and 4.3 percentage points less likely to enter university. The gender gap, therefore, does appear to worsen at higher birth orders, and laterborn girls face a sharper decline in resources allocated to them than laterborn boys.

In the second panel, we estimate the impact of the ban on the educational outcomes of women separately for those women who have no sisters and women with at least one sister. Having sisters has different effects on the gender gap depending on the measure of educational attainment being considered. For completing Grade 10, the gender gap improves for women with sisters compared to women without sisters, but for completing Grade 12 and entering university, the gender gap worsens for women with sisters compared to women without sisters. Women without sisters are 7.3 percentage points less likely to have completed Grade 10 in treated states than men without sisters, while women with sisters are 2.3 percentage points less likely to have completed Grade 10 in treated states compared to men with sisters. However, women without sisters enter university at similar rates compared to men without sisters, while women with sisters are 5.1 percentage points less likely to enter university than men with sisters. We interpret these results to mean that for educational attainment levels that require higher household investments, men benefit more than women from having more non-rival sisters, similar to Lei et al. (2017). The gender gap, therefore, worsens with the presence of more women in a family.

In the third panel, we estimate the impact of the ban on educational outcomes separately for individuals born into families with firstborn girls and firstborn boys. Among those who have enrolled in university, the estimated relative impact of the ban on educational outcomes of women, compared to men, in treated vs control states, is indeed larger for those women born into families with firstborn girls, rather than firstborn boys, pointing to the increased probability of unwanted girls being born to families with first-born girls. However, for completing Grade 12, there is no significant difference across individuals in firstborn girl and boy families, and for completing Grade 10, the impact is, in fact, even larger among individuals in firstborn boy families.

6.3 Impact on marital outcomes

An increase in the number of females at birth due to the prohibition on sex-selective screening and abortions can create a version of the "marriage squeeze" (Caldwell et al., 1983) where the relative increase in the number of females generates increased competition over scarce males in the marriage market. Such marriage squeezes can change the bargaining power between men and women in the marriage market and have been associated with higher dowry payments by women's families (Rao, 1993, 2000) and narrower spousal age gaps, particularly due to women marrying at an older age (Chiplunkar and Weaver, 2019). Investments in schooling are another mechanism through which parents could seek to increase the probability of their daughter making a high-quality match (Chiappori et al., 2009), and recent evidence from India finds that parents believe there exist high returns to education in the marriage market (Adams-Prassl and Andrew, 2019). If the increased number of women due to the ban on sex-selective screening leads to higher investments in the schooling of girls, then the average estimated effect of the ban on sex-selection on the gender gap in education incorporates an *increase* in educational investments that takes place in response to the changing bargaining power of women in the marriage market.

While we cannot separate out the impact on education through this channel, we can consider whether the implementation of the ban had any effects at all on the marriage market. To do this, we estimate the effect of the implementation of the ban on the age of marriage of women as well as the spousal age gap. As Anderson (2007) argues, the pressures of the marriage squeeze lead to brides postponing their age of marriage with a resultant fall in the spousal age gap. We test for this by estimating the following equation on the sample of all women born between 1989 and 2002:

$$\begin{aligned} Y_{ist} = \beta_0 + \beta_1 Treat * Post_{ist} + \mathbf{X'_{ist}}\tau \\ + \omega_s + \delta_t + \epsilon_{ist} \end{aligned} \tag{4}$$

The dependent variable, Y_{ist}, is an indicator for being married at 15 years and being married at 18 years, and, for a smaller sample of already-married women, the age of marriage and the spousal age gap, for woman i born in state s in year t. We control for household characteristics and include state and year of birth fixed effects. Standard errors are clustered at the state level. The controls for household characteristics include caste, religion, whether the household head is male and his or her highest educational attainment, and whether the household is located in a rural area. β_1 captures the differential impact of the ban on sex-screening and sex-selective abortions on the dependent variable in treated states relative to those in control states, comparing outcomes before and after the ban.

The results of equation (4) are presented in Table A7, and indicate that women are, in fact, delaying marriage in treated states. They are 5.3 percentage points less likely to be married at the age of 15, and the coefficient on marriage at 18 is negative as well, though only marginally significant. This is reflected as well in an increase in the age of marriage for married women of 0.568 years. The coefficient on spousal age gap is negative, though not significant. The sample of married women in column 4 is smaller than in column 3 because we obtain spousal age by matching women to their husbands as long as they live in the same house. For women whose husbands live in another household or who are no longer married, we are unable to capture the spousal age gap. We continue to restrict the analysis to only non-wealthy households, though given the wealth stratification in marriage, with households of similar wealth marrying into one another, we believe this is a reasonable assumption. The results are very similar if we include all households.

The ban has clear impacts on the marriage market, along expected lines. It is plausible that this also influences decisions about educational investment for both men and women, with a presumed impact of relatively increasing the educational attainment of women. If so, our estimates combine both a decline in educational investments in unwanted girls as well as an increase in investments to improve their marriage prospects.

7 Discussion and conclusion

We find that the ban on sex-selective abortions led to a significant increase in the births of females in treated states in households with firstborn girls, and that the ban led to a corresponding decrease in the educational attainment of females relative to males. Women are less likely to complete Grade 10, Grade 12 and enter university, compared to men in treated states, and these results are driven by households in the bottom 60% of the wealth distribution. Women born at higher birth orders and women with relatively more sisters are more adversely affected as well.

Aside from the primary channel of increased rates of female survival at birth, another channel through which women's welfare could increase as a result of the ban on sex-selective abortions is through the marriage market. In particular, with greater gender parity in the marriage market, the age of marriage for women rises and parents are more likely to invest in their education so as to increase the chances of matching with a high-quality groom. Higher age at marriage is also associated with a number of non-educational outcomes, such delayed childbirth, reduced fertility and greater autonomy. However, our results suggest that such improvements, at least with respect to educational attainment, are currently swamped by the decline in parental investments in females due to their being increasingly unwanted.

A study conducted concurrently with this paper, by Sanjay and Dey (2019), also studies the causal impact of the PNDT Act on female educational attainment and finds that the ban on sex-selective abortions led to an increase in female educational attainment in absolute terms. There are several differences between this study and ours. First, Sanjay and Dey (2019) use data from only one state, JK, as a control for treated states. Further, their pre-treatment cohort, rather than being just prior to the ban, constitutes women born between the years 1973 and 1978, while the post-group is 1996 and 2001. They do not estimate changes in female education relative to male education, and they do not make use of the quasi-exogenous variation in the sex of the firstborn child. Given their difference-in-differences framework with a single state, there could be several other changes taking place in JK that could be biasing their results.

What our results emphasise is that efforts to eliminate gender discrimination can backfire unless they take into account the underlying social drivers of son preference. Policies targeted at reducing prenatal discrimination can lead to the increased prevalence of postnatal discrimination, as in the case of educational investments, and specific actions may need to be taken to address these perverse forms of inequality as well. There is no magic bullet for gender equality, and cracking down on abortion clinics can only go so far if the underlying social norms favouring sons remain unchanged.

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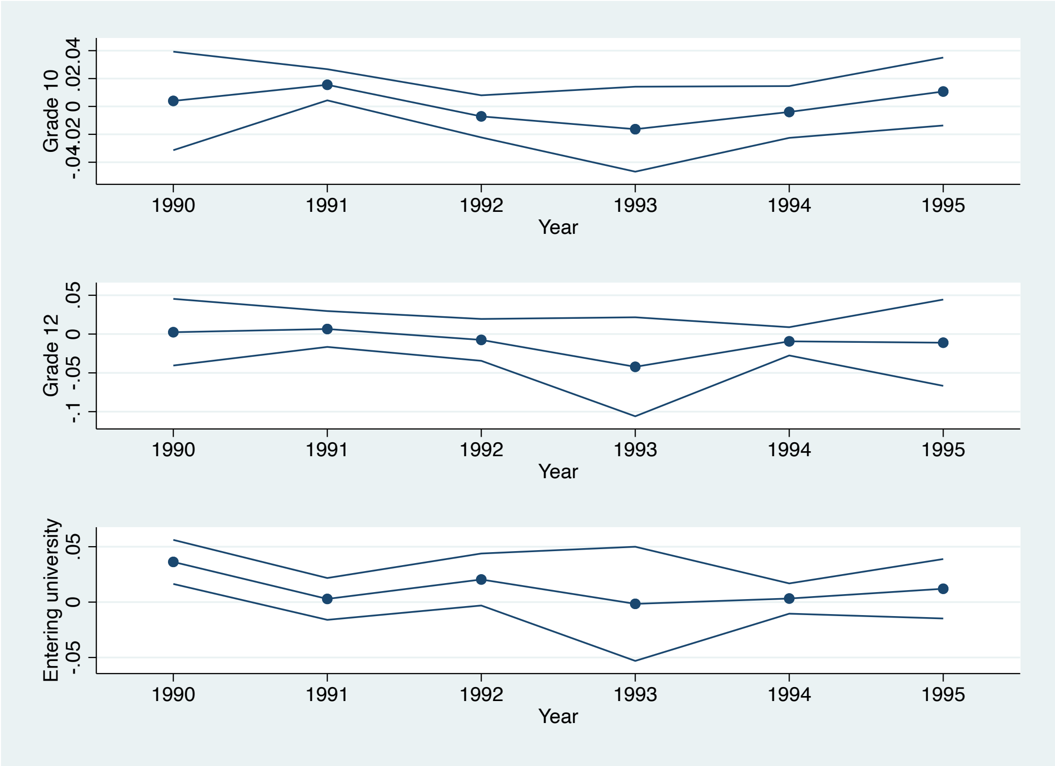
A Appendix

Figure 1: Testing for parallel trends prior to the introduction of the ban



This figure tests for differential trends in educational outcomes (graduating Grade 10, graduating Grade 12, and entering university) during the period from 1989-1995, prior to the treatment in the 1996. Each graph plots the coefficients on the $Treat * Female * BirthYear$ indicator variables for all birth years in the period 1990-1996, with 1989 as the excluded category. The regressions include controls for all fixed effects for state, female and birth year, and all pairwise interactions between all three. Controls for household characteristics are also included in these regressions.

Table 1: Unadjusted sample means by state, year, gender and firstborn sex

	Control States		Treated States	
	1989-95	1997-2002	1989-95	1997-2002
FirstbornGirl-FirstbornBoy Gap				
Probability of female birth	0.01	−0.04	−0.01	−0.01
	(0.01)	(0.01)	(0.00)	(0.00)
Observations	5,547	9,747	68,008	127,471
Female-Male Gap				
Completing Grade 12	−0.15	−0.02	−0.10	−0.00
	(0.01)	(0.01)	(0.00)	(0.00)
Completing Grade 12	−0.13	−0.00	−0.07	0.00
	(0.01)	(0.00)	(0.00)	(0.00)
Entering university	−0.08	−0.00	−0.04	0.00
	(0.01)	(0.00)	(0.00)	(0.00)
Observations	17,940	16,734	191,643	200,682

Notes: The first row indicates the difference in the probability of birth of a female in a household with a firstborn girl and a household with a firstborn boy. The next three rows indicate the difference in the completion rates of Grade 10 and 12 and entry rate into university between females and males. Standard errors of these estimated sample means are given in parentheses.

Table 2: Impact of ban on female births

Female Births	(1)	(2)	(3)	(4)
Treat x Post x Firstborn Girl	0.026**	0.026**	0.025**	0.013
	(0.010)	(0.010)	(0.010)	(0.020)
	[0.17]	[0.11]	[0.10]	[0.13]
Household Characteristics	No	Yes	Yes	No
Firstborn Girl x Birthyear FE	No	No	Yes	Yes
State x Birthyear FE	No	No	Yes	Yes
Firstborn Girl x State FE	No	No	Yes	No
Mother FE	No	No	No	Yes
Observations	332,686	332,686	332,686	253,001
$Adj.R^2$	0.001	0.001	0.002	-0.016

Notes: : *, **, *** denote significance at the 10%, 5%, and 1% confidence levels, respectively. The dependent variable in all columns is an indicator for a given birth being female. All regressions include state, birth year, and firstborn girl fixed effects. Household characteristics in columns 2 and 3 include religion, caste, sex of the household head, education of the household head, whether the household is located in a rural area, and the wealth quintile of the household. Standard errors clustered by state are in parentheses. Wild cluster bootstrap p-values are in square brackets.

Table 3: Impact of ban on female births – wealthy vs non-wealthy households

	Non-wealthy			Wealthy		
	(1)	(2)	(3)	(4)	(5)	(6)
Treat x Post x Firstborn Girl	0.042***	0.043***	0.041***	−0.004	−0.005	−0.002
	(0.004)	(0.004)	(0.005)	(0.028)	(0.028)	(0.030)
	[0.14]	[0.12]	[0.07]	[0.77]	[0.85]	[0.99]
X_i	No	Yes	Yes	No	Yes	Yes
Firstborn Girl x Birthyear FE	No	No	Yes	No	No	Yes
State x Birthyear FE	No	No	Yes	No	No	Yes
Firstborn Girl x State FE	No	No	Yes	No	No	Yes
Observations	210,773	210,773	210,773	121,913	121,913	121,913
$Adj.R^2$	0.001	0.001	0.001	0.002	0.003	0.004

Notes: : *, **, *** denote significance at the 10%, 5%, and 1% confidence levels, respectively. The dependent variable in all columns is an indicator for a given birth being female. The estimations in columns 1-3 include only non-wealthy households and in column 4-6 include only the wealthy ones. Non-wealthy households are those in the bottom three quintiles of the wealth distribution (bottom 60%) while the wealthy households are from the top two quintiles (top 40%). All regressions include state, birth year, and firstborn girl fixed effects. Household characteristics in columns 2, 3, 5 and 6 include religion, caste, sex of the household head, education of the household head, whether the household is located in a rural area, and the wealth quintile of the household. Standard errors clustered by state are in parentheses. Wild cluster bootstrap p-values are in square brackets.

Table 4: Impact on education

	All states					
	Completed Grade 10		Completed Grade 12		Entered university	
	(1)	(2)	(3)	(4)	(5)	(6)
Treat x Post x Female	−0.022***	−0.023***	−0.034***	−0.035***	−0.031***	−0.032***
	(0.007)	(0.006)	(0.011)	(0.011)	(0.009)	(0.010)
	[0.220]	[0.092]	[0.210]	[0.159]	[0.330]	[0.135]
Household characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Female x Birthyear FE	No	Yes	No	Yes	No	Yes
State x Birthyear FE	No	Yes	No	Yes	No	Yes
Female x State FE	No	Yes	No	Yes	No	Yes
Observations	700,214	700,214	700,214	700,214	700,214	700,214
Adj. R^2	0.31	0.33	0.29	0.31	0.23	0.25
	Border states					
Treat x Post x Female	−0.016*	−0.015**	−0.025*	−0.025*	−0.035**	−0.036**
	(0.008)	(0.007)	(0.011)	(0.012)	(0.012)	(0.013)
	[0.267]	[0.052]	[0.243]	[0.109]	[0.256]	[0.057]
Household Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Female x Birthyear FE	No	Yes	No	Yes	No	Yes
State x Birthyear FE	No	Yes	No	Yes	No	Yes
Female x State FE	No	Yes	No	Yes	No	Yes
Observations	239,399	239,399	239,399	239,399	239,399	239,399
Adj. R^2	0.32	0.33	0.28	0.29	0.21	0.22

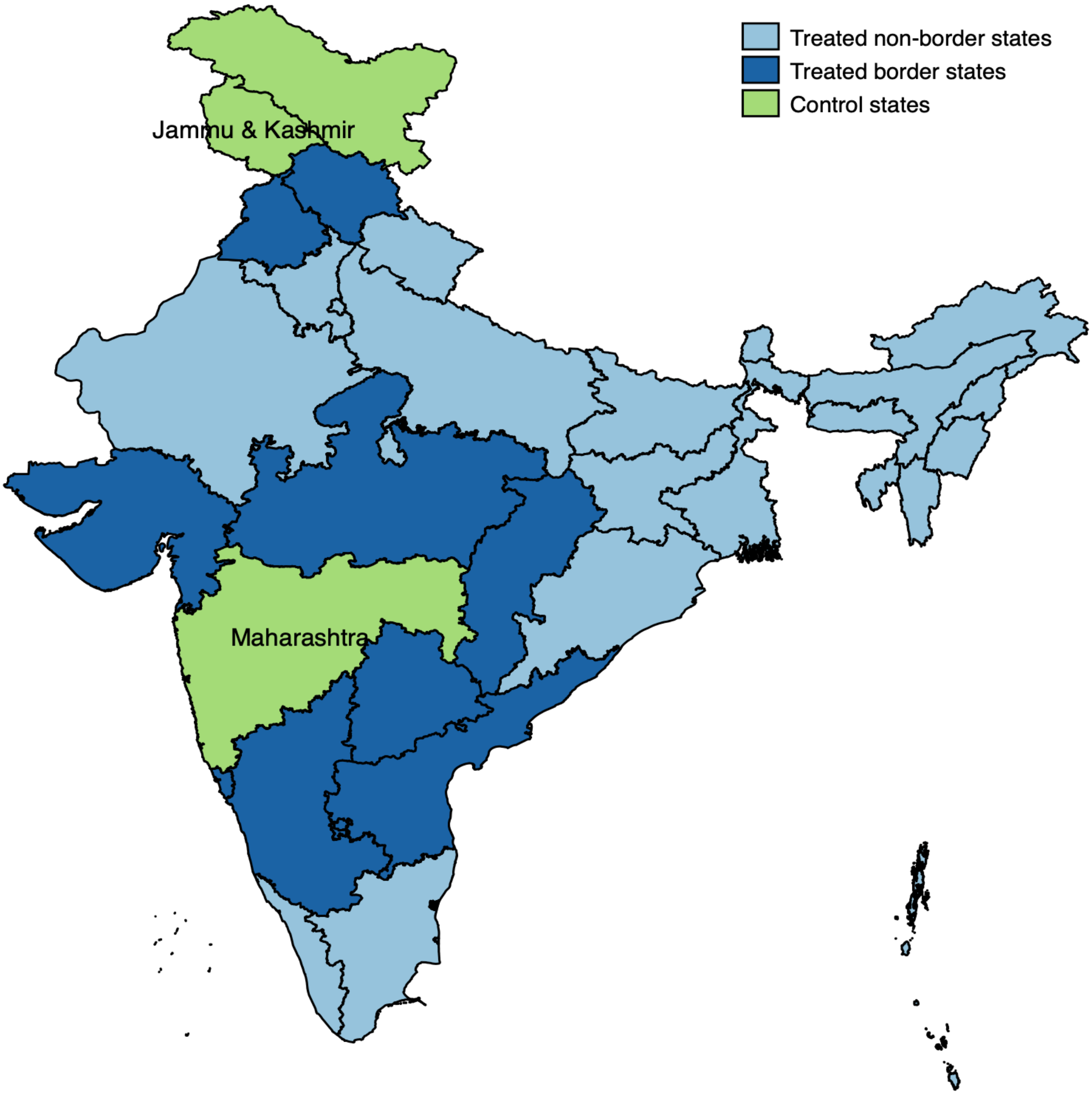
Notes: *, **, *** denote significance at the 10%, 5%, and 1% confidence levels, respectively. Regressions in the top panel are estimated using data from all 36 states. Regressions in the bottom panel are estimated using data from the two control states and only 11 treated states which border them states. The dependent variable in columns 1 and 2 is an indicator for whether the individual has completed Grade 10. The dependent variable in columns 3 and 4 is an indicator for whether the individual has completed Grade 12. The dependent variable in columns 5 and 6 is an indicator for whether the individual has entered university. Household characteristics in columns 1-6 include religion, caste, sex of the household head, education of the household head, whether the household is located in a rural area, and the wealth quintile of the household. All regressions also include state, birth year, and female fixed effects. Standard errors clustered by state are in parentheses. Wild cluster bootstrap p-values are in square brackets.

Table 5: Impact on education: high and low wealth families

	All states					
	Completed Grade 10		Completed Grade 12		Entered university	
	Poor	Non-poor	Poor	Non-poor	Poor	Non-poor
Treat x Post x Female	−0.041***	0.005	−0.050***	−0.014*	−0.035***	−0.031**
	(0.007)	(0.012)	(0.018)	(0.008)	(0.011)	(0.012)
	[0.076]	[0.765]	[0.122]	[0.224]	[0.105]	[0.149]
Household characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Female x Birthyear FE	Yes	Yes	Yes	Yes	Yes	Yes
State x Birthyear FE	Yes	Yes	Yes	Yes	Yes	Yes
Female x State FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	426,999	273,215	426,999	273,215	426,999	273,215
Adj.R^2	0.25	0.33	0.21	0.34	0.16	0.28
	Border states					
Treat x Post x Female	−0.039**	0.019	−0.045*	0.004	−0.039**	−0.034*
	(0.013)	(0.016)	(0.021)	(0.010)	(0.013)	(0.018)
	[0.133]	[0.415]	[0.112]	[0.671]	[0.048]	[0.148]
Household characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Female x Birthyear FE	Yes	Yes	Yes	Yes	Yes	Yes
State x Birthyear FE	Yes	Yes	Yes	Yes	Yes	Yes
Female x State FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	151,213	88,186	151,213	88,186	151,213	88,186
Adj.R^2	0.24	0.35	0.19	0.34	0.13	0.26

Notes: *, **, *** denote significance at the 10%, 5%, and 1% confidence levels, respectively. Regressions in the top panel are estimated using data from all 36 states. Regressions in the bottom panel are estimated using data from the two control states and only 11 treated states which border them states. The dependent variable in columns 1 and 2 is an indicator for whether the individual has completed Grade 10. The dependent variable in columns 3 and 4 is an indicator for whether the individual has completed Grade 12. The dependent variable in columns 5 and 6 is an indicator for whether the individual has entered university. Columns 1, 3, and 5 run the specification on the only the non-wealthy families in our sample, the bottom 60% of the wealth distribution. Columns 2, 4, and 6 run the specification on the only the wealthy families in our sample, the top 40% of the wealth distribution. Household characteristics in all columns include religion, caste, sex of the household head, education of the household head, whether the household is located in a rural area, and the wealth quintile of the household. All regressions also include state, birth year, and female fixed effects, as well as pairwise interactions between the three. Standard errors clustered by state are in parentheses. Wild cluster bootstrap p-values are in square brackets.

Figure A1: Map of Indian States



This map indicates treated border states, treated non-border states and control states.

Table A1: Impact on education: Maharashtra and JK

	Completed Grade 10		Completed Grade 12		Entered University	
	Mah	JK	Mah	JK	Mah	JK
Treat x Post x Female	−0.019***	−0.028***	−0.042***	−0.020***	−0.039***	−0.021***
	(0.005)	(0.005)	(0.007)	(0.007)	(0.007)	(0.007)
Household Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Female x Birthyear FE	Yes	Yes	Yes	Yes	Yes	Yes
State x Birthyear FE	Yes	Yes	Yes	Yes	Yes	Yes
Female x State FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	677,323	669,657	677,323	669,657	677,323	669,657
Adj.R^2	0.335	0.333	0.275	0.308	0.221	0.246

Notes: *, **, *** denote significance at the 10%, 5%, and 1% confidence levels, respectively. The dependent variable in columns 1 and 2 is an indicator for whether the individual has completed Grade 10. The dependent variable in columns 3 and 4 is an indicator for whether the individual has completed Grade 12. The dependent variable in columns 5 and 6 is an indicator for whether the individual has entered university. Columns 1, 3, and 5 include only Maharashtra as a control state (i.e., we drop all women from J&K from our sample). Similarly, columns 2, 4 and 6 include only J&K as a control state. All regressions also include state, birth year, and female fixed effects as well as pairwise interactions between all three. Household characteristics in all columns include religion, caste, sex of the household head, education of the household head, whether the household is located in a rural area, and the wealth quintile of the household. Standard errors clustered by state are in parentheses.

Table A2: Changes in son preference

	(1)	(2)	(3)	(4)	(5)
Treat x Post x Firstborn Girl			−0.004	−0.005	−0.005
			(0.004)	(0.004)	(0.004)
Treat x Post	−0.003	−0.003	−0.001	−0.001	
	(0.003)	(0.003)	(0.002)	(0.002)	
Household Characteristics	No	Yes	No	Yes	Yes
Firstborn Girl x Year of First Birth FE	No	No	No	No	Yes
State x Year of First Birth FE	No	No	No	No	Yes
Firstborn Girl x State FE	No	No	No	No	Yes
Observations	204,898	204,898	204,897	204,898	204,844
Adj.R^2	0.028	0.032	0.043	0.047	0.064

Notes: *, **, *** denote significance at the 10%, 5%, and 1% confidence levels, respectively. The dependent variable is the ideal fraction of sons wanted by a mother. All regressions also include state, birth year, and female fixed effects. Household characteristics in columns 2, 4 and 5 include religion, caste, sex of the household head, education of the household head, whether the household is located in a rural area, and the wealth quintile of the household. Standard errors clustered by state are in parentheses.

Table A3: Impact on education: Rural and urban households

	Female births					
	Rural			Urban		
	(1)	(2)	(3)	(4)	(5)	(6)
Treat x Post x Firstborn Girl	0.063***	0.064***	0.062***	−0.014	−0.013	−0.012
	(0.006)	(0.006)	(0.005)	(0.024)	(0.023)	(0.025)
Household Characteristics	No	Yes	No	No	Yes	No
FirstbornGirl x State FE	No	No	Yes	No	No	Yes
State x Birthyr FE	No	No	Yes	No	No	Yes
FirstbornGirl x Birthyr FE	No	No	Yes	No	No	Yes
Observations	153,400	153,400	153,383	57,371	57,371	57,368
Adj.R^2	0.001	0.001	0.000	0.001	0.001	0.001

Notes: *, **, *** denote significance at the 10%, 5%, and 1% confidence levels, respectively. The dependent variable in columns 1 and 2 is an indicator for whether the individual has completed Grade 10, in columns 3 and 4 is an indicator for whether the individual has completed Grade 12, and in columns 5 and 6 is an indicator for whether the individual has entered university. Columns 1, 3, and 5 include only rural households and columns 2, 4 and 6 include only urban ones. All regressions include state, birth year, and female fixed effects. Household characteristics in all specifications include religion, caste, sex of the household head, education of the household head, and the wealth quintile of the household. Standard errors clustered by state are in parentheses.

Table A4: Impact on fertility

	Birth		Total Fertility		Excess Fertility	
	(1)	(2)	(3)	(4)	(5)	(6)
Treat x Post x FirstbornGirl	−0.001	−0.002	−0.040	−0.056	−0.021	−0.028
	(0.005)	(0.005)	(0.041)	(0.047)	(0.041)	(0.043)
Treat x Post	0.028		0.154		0.105	
	(0.023)		(0.147)		(0.132)	
Treat x FirstbornGirl	0.001		0.006		−0.014	
	(0.005)		(0.063)		(0.050)	
Post x FirstbornGirl	0.009**		0.121***		0.100***	
	(0.004)		(0.023)		(0.024)	
Ideal number of children			0.663***	0.656***		
			(0.023)	(0.022)		
Ideal fraction of boys			0.116***	0.079***	0.082*	0.047
			(0.031)	(0.028)	(0.045)	(0.040)
Household characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Firstborn Girl x State FE	No	Yes	No	Yes	No	Yes
State x Post FE	No	Yes	No	Yes	No	Yes
Firstborn Girl x Post FE	No	Yes	No	Yes	No	Yes
Observations	1,141,409	1,141,409	74,261	74,261	74,261	74,261
Adj.R^2	0.052	0.055	0.389	0.397	0.136	0.145

Notes: *, **, *** denote significance at the 10%, 5%, and 1% confidence levels, respectively. The data in these specifications includes all women who gave birth to children between the years 1989-2002. Mothers who have children both before and after 1996 are excluded from the sample. The specification in columns 1 and 2 use a mother-year dataset of births and the dependent variable is an indicator of whether a birth has taken place in a given year. These specifications also control for the mother's age fixed effects and total parity for the mother. The dependent variable in columns 3 and 4 is total fertility (the total number of children a mother has had) and columns 5 and 6 is excess fertility (the difference between the total number of children a mother has had and her self-reported ideal number of children). All regressions include state, post and firstborn girl fixed effects. Household characteristics in all columns include religion, caste, sex of the household head, education of the household head, whether the household is located in a rural area, and the wealth quintile of the household. Standard errors clustered by state are in parentheses.

Table A5: Impact on education for matched households

	Graduated 10	Graduated 12	Entered University
Treat x Post x Female	−0.036***	−0.061***	−0.031***
	(0.012)	(0.011)	(0.010)
Household Characteristics	Yes	Yes	Yes
Female x Birthyear FE	Yes	Yes	Yes
State x Birthyear FE	Yes	Yes	Yes
Female x State FE	Yes	Yes	Yes
Observations	206,609	206,609	206,609
$Adj.R^2$	0.33	0.30	0.22

Notes: *, **, *** denote significance at the 10%, 5%, and 1% confidence levels, respectively. The sample in this estimation includes only those women who were matched back to the educational attainment recorded in the household rosters, i.e., those women still living in their households of birth. The dependent variable in column 1 is an indicator for whether the individual has completed Grade 10, in column 2 it is an indicator for whether the individual has completed Grade 12, and in column 3 is an indicator for whether the individual has entered university. Household characteristics in all columns include religion, caste, sex of the household head, education of the household head, whether the household is located in a rural area, and the wealth quintile of the household. All regressions also include state, birth year, and female fixed effects, as well as pairwise interactions between the three. Standard errors clustered by state are in parentheses.

Table A6: Impact on education by sibling composition

	Completed Grade 10		Completed Grade 12		Entered university	
Birth Order	1	>1	1	>1	1	>1
Treat x Post x Female	−0.007	−0.059**	−0.023	−0.087***	−0.014	−0.043***
	(0.034)	(0.025)	(0.016)	(0.012)	(0.013)	(0.010)
Household Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Female x Birthyear FE	Yes	Yes	Yes	Yes	Yes	Yes
State x Birthyear FE	Yes	Yes	Yes	Yes	Yes	Yes
Female x State FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	72,194	134,414	72,194	134,414	72,194	134,414
Adj.R^2	0.35	0.32	0.32	0.29	0.24	0.21
Number of sisters	0	>0	0	>0	0	>0
Treat x Post x Female	−0.073***	−0.025**	−0.050***	−0.069***	0.014	−0.051***
	(0.012)	(0.012)	(0.013)	(0.012)	(0.015)	(0.011)
Household Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Female x Birthyear FE	Yes	Yes	Yes	Yes	Yes	Yes
State x Birthyear FE	Yes	Yes	Yes	Yes	Yes	Yes
Female x State FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	48,723	157,876	48,723	157,876	48,723	157,876
Adj.R^2	0.39	0.31	0.36	0.28	0.27	0.21
Firstborn sex	First girl	First boy	First girl	First boy	First girl	First boy
Treat x Post x Female	−0.034*	−0.068**	−0.082***	−0.077***	−0.071***	−0.014
	(0.020)	(0.031)	(0.012)	(0.016)	(0.012)	(0.013)
Household Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Female x Birthyear FE	Yes	Yes	Yes	Yes	Yes	Yes
State x Birthyear FE	Yes	Yes	Yes	Yes	Yes	Yes
Female x State FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	95,776	110,827	95,776	110,827	95,776	110,827
Adj.R^2	0.35	0.33	0.32	0.29	0.25	0.21

Notes: *, **, *** denote significance at the 10%, 5%, and 1% confidence levels, respectively. Across all three panels, the dependent variable in columns 1 and 2 is an indicator for whether the individual has completed Grade 10, in columns 3 and 4 it is an indicator for whether the individual has completed Grade 12 and in columns 5 and 6 it is an indicator for whether the individual has entered university. In the upper panel, the specifications in columns 1, 3 and 5 include only firstborn children (i.e., with birth order equalling 1) while in columns 2, 4, and 6 they include laterborn children (i.e.,with birth order greater than 1). In the middle panel, the specifications in columns 1, 3 and 5 include only individuals with no sisters while in columns 2, 4, and 6 they include all individuals with at least one sister. For the lowest panel, the specifications in columns 1, 3 and 5 include only those individuals whose oldest sibling is female (i.e., are born into a household with a firstborn boy), and in columns 2, 4, and 6 the specifications include only those whose oldest sibling is male (i.e., are born into a household with a firstborn girl). All specifications include state, birth year, and female fixed effects, as well as pairwise interactions of the three. Household characteristics in all specifications include religion, caste, sex of the household head, education of the household head, whether the household is located in a rural area, and the wealth quintile of the household. Standard errors clustered by state are in parentheses.

Table A7: Impact on age of marriage

	Married at 18	Married at 15	Age of marriage	Spousal age gap
Treat x Post	−0.098	−0.053***	0.568**	−0.382
	(0.061)	(0.018)	(0.242)	(0.293)
Household Characteristics	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Birthyear FE	Yes	Yes	Yes	Yes
Observations	170,895	170,895	85,915	68,709
$Adj.R^2$	0.206	0.079	0.111	0.090

Notes: *, **, *** denote significance at the 10%, 5%, and 1% confidence levels, respectively. The dependent variable in column 1 and 2 is an indicator for being married at 15 years and being married at 18 years respectively. The specifications in columns 3 and 4 analyse a smaller sample of already-married women, and the dependent variables here are age of marriage and the spousal age gap respectively. All specifications include state, birth year, and female fixed effects, as well as pairwise interactions of the three. Household characteristics in all columns include religion, caste, sex of the household head, education of the household head, whether the household is located in a rural area, and the wealth quintile of the household. Standard errors clustered by state are in parentheses.