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ORIGINAL PAPER

Unwanted daughters: the unintended consequences of a ban on sex-selective abortions 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, focusing on the impact on educational attainment. We exploit the differentially timed introduction of a ban on sex-selective abortions across states in India. We find that a legal restriction on abortions 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, respectively, 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 was not driven by family size but because surviving females became relatively unwanted, whereas surviving males became relatively more valued, leading to an increasing concentration of household resources on them. 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. This suggests that policymakers need to address the unintended welfare consequences of interventions aimed at promoting gender equity.

Keywords Sex ratio · Education · Fertility · Economics of gender · Discrimination · Abortion · India

JEL classification: I21 · J13 · J16 · O12 · O15

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1 Introduction

Starting from the influential work of Sen (1992), a substantial body of research has documented the phenomenon of missing women in countries with a strong social and cultural preference for sons. Countries in Asia and North Africa have skewed sex ratios due to 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), including outcomes that extend well beyond birth, such as the likelihood of engaging in criminal activity in early adulthood (Donohue et al. 2001), as well as fertility decisions made by those children (Gutierrez 2022). In these studies, all drawn from the USA and Europe, the child born at the margin of abortion is found to be relatively wanted and born at a time when parents can provide a relatively nurturing environment, leading to improved human capital outcomes. Similarly, 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. 2021).

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. First, 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. Second, 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). Third, changing access to sex-selective abortion 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. 2019). 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. Fourth, 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 in 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, followed by a national law in 1994 extending to what were then 26 additional states and excepting the state of Jammu and Kashmir (henceforth JK). 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 build on these results by using a nationally representative individual-level dataset and by using household-level quasi-exogenous variation in exposure to the ban. 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. 2021), 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. Relative to males, 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 was a significant increase in female births. More specifically, while female educational attainment is unchanged, male educational attainment *increases*. 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, compared to surviving sons, particularly in non-wealthy households, rather than by an increase in the total number of children due to the use of a 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 several studies have analysed the impact of changing access to sex-selective abortions on health outcomes, there is little work 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. Our analysis is different in several ways. 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 affects the whole country at once, making it 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 differentially timed 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 passage of this legislation led to a relative increase in the number of females aged from 0 to 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 by 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.

A study conducted concurrently with this paper, by Sanjay and Dey (2019), also studies the causal impact of the Pre-Conception and Pre-Natal Diagnostics Techniques (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 that 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 their treatment 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, other changes taking place in JK could biasing their results, particularly when comparing cohorts that were born so far apart in time.

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. (2020) 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

Sex-detection techniques such as ultrasound and amniocentesis 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 and 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.[1,2]

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

[2] Another reason for the steep fall in female-male child sex ratios during this period is due to the fall in desired fertility, aided by expanded access to contraception (Jayachandran 2017). As parents desire smaller families, in a context of strong son preference, they are more likely to manipulate the sex of their children in order to have a desired number of male children.

In response to the increasingly imbalanced sex ratios, the Pre-Conception and Pre-Natal Diagnostics Techniques Act (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 ban was implemented at different stages in different states over a fourteen-year period. In the first stage, the state of Maharashtra implemented its own version of the PNDT Act called the Maharashtra Regulation of Pre-natal Diagnostic Techniques Act of 1988, enacted in 1989. In the second stage, the PNDT Act of 1994 was passed in all other states except JK which was specifically excluded from its ambit. In the third stage, the Jammu And Kashmir Preconception and Prenatal Sex Selection/Determination Act was passed in 2002. As a result, both Maharashtra and JK remained unaffected by the passing of the national PNDT Act in 1994, which came into effect in 1996.

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). 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 differentially timed implementation of the ban across states 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).

With the introduction of the PNDT act, we would expect that parents' ability to manipulate the sex composition of their children would be inhibited in all treated states, but not in Maharashtra and JK. This would lead to an increase in the total number of girls born to families in these treated states after the implementation of the ban. 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 and 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. 2019). Borker et al. (2019) 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 disproportionately born 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, or temporarily migrate to neighbouring states where these facilities are still available, then the effects on female births and education will be amplified among less wealthy households.

The net impact on education, therefore, will depend on the size and direction of a treatment effect and a selection effect. If more girls are born into wealthy families, the net effect on female education will be ambiguous since the treatment and selection effects will operate in opposing directions. However, if more girls are born into liquidity-constrained families, then households in treated states will experience more female births after the introduction of the ban, and the gender gap in schooling will decline through both the treatment effect and the selection effect. 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 implemented in India that is representative at the district level (International Institute for Population Sciences and ICF 2017). The survey includes 601,509 households 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 marital 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 16 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.[3] 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.

In Table 17, we show differences between the treated and control states by a number of variables by using data from the second round of the National Family Health Survey (NFHS-2) conducted in 1998–1999, which is representative of the differences across these states at the time that the PNDT Act was implemented in 1996. In general, treated states tend to be more rural with poorer educational outcomes than the control states. To control for both time-varying and time-invariant differences across states, we include state and state-year fixed effects in our analysis, which we discuss in the next section on empirical strategy.

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 Fig. 1.

The time period of interest is divided into the pre-treatment period (1989–1995) and the post-treatment period (1997–2002), and is restricted at both ends by the introduction of their bans in the two control states: the Maharashtra Regulation of Prenatal 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 individuals born between 1989 and 2002. We use the current age provided at the time of interview to assign the birth years of individuals. 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.

Our empirical strategy therefore uses two different sets of counterfactuals: households in Maharashtra which had already been affected by the legislation passed in 1989, and were therefore untreated by the PNDT Act passed in 1996, and households in JK, which were yet to receive a ban on sex-selective abortions till 2002. The use of both counterfactuals in estimating the treatment effect of the PNDT Act assumes that the effects

[3] The school system in India is organized as primary school (1st to 5th grade), upper primary or middle-school (6th to 8th grade), junior secondary (9th to 10th grade), and higher secondary (11th to 12th grade). Upon completion of grade 10, students take a secondary school certificate (SSC) examination to proceed to higher secondary education. The SSC examination is therefore the first important examination taken by schoolchildren in India, akin to the GSCEs in the UK, and is necessary to access any higher level of formal education.

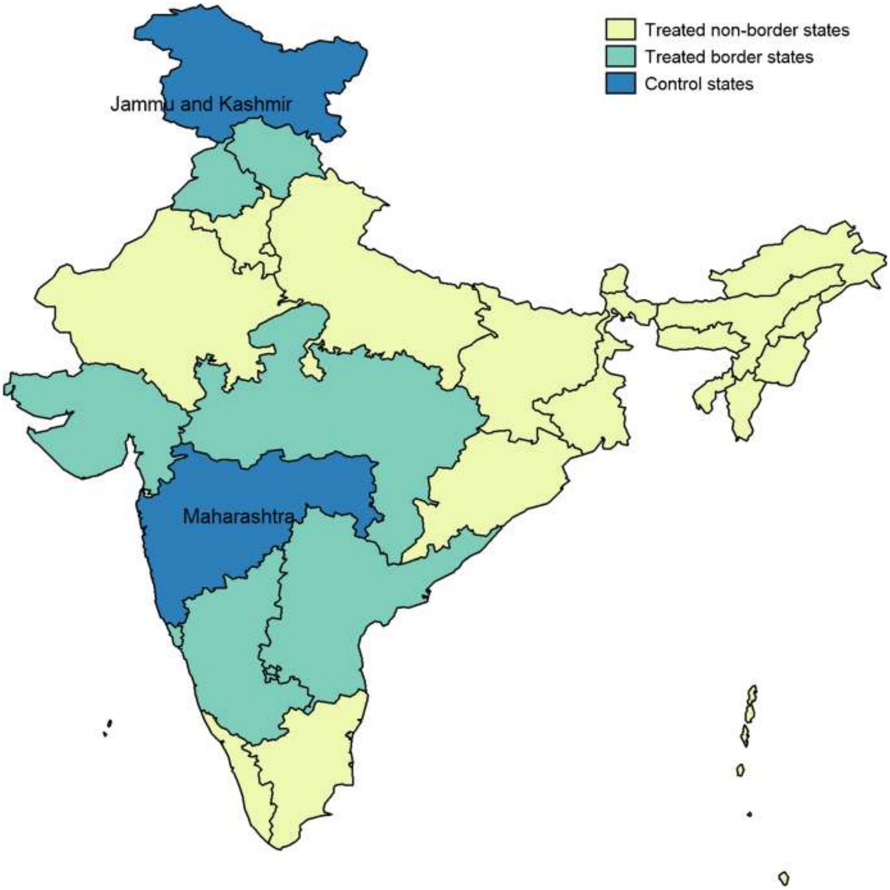

Fig. 1 Map of Indian states. This map indicates treated border states, treated non-border states, and control states

of the ban are level effects only, and become constant quickly over time. To rule out the possibility that the treatment effects continue to vary over a longer period of time, we also estimate the treatment effect of the PNDT Act separately using only Maharashtra and only Jammu and Kashmir as the control groups respectively (Table 6). These results are very similar to our main results suggesting that the treatment effects come into place fairly quickly and remain constant over time, increasing our confidence in the use of units treated well before 1996, and units treated well after 1996 as control groups with respect to a change that affects only treated units in 1996.

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 quasi-random and has an impact on whether families opt for sex-selective abortion in the future (Bhalotra and Cochrane 2010; Bhalotra et al. 2020; Anukriti et al. 2021). 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 that may have wanted to opt for sex-selective abortions for children born at a birth order greater than one, but are unable to detect the sex of their child after 1996 due to the ban.

To establish that the sex of the firstborn child is indeed quasi-random and itself unaffected by the implementation of a ban on sex selection, we run the following regression:

$$\begin{aligned} FemaleBirth_{ist} =& \beta_0 + \beta_1 Treat_s * Post_t + \mathbf{X'}_{\mathbf{ist}}\tau \\ &+ \gamma_t + \phi_s + \epsilon_{ist} \end{aligned} \tag{1}$$

where the dependent variable, $FemaleBirth_{ist}$, is a binary indicator for whether a firstborn child i, 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). 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 states and birth cohorts.

The data for this analysis constitutes all firstborn births to all female respondents of the NFHS-4 survey that took place between 1989 and 2002. The results are shown in Table 1. The estimated coefficients across all specifications are insignificantly different from zero, indicating that the ban did not change the probability of female births among firstborn children in the sample.

Having established the exogeneity of the sex of the firstborn child, we next investigate if the ban did change the probability of female births at higher birth orders. 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 first estimate the following equation for two different samples of individuals — those born into firstborn female families and those born into firstborn male families:

Table 1 Impact of ban on female births among firstborn children

	(1)	(2)
All states		
Treat *x* Post	0.003	0.003
	(0.006)	(0.006)
Observations	190,752	190,752
Adj. R^2	0.002	0.002
Border states		
Treat *x* Post	0.003	0.003
	(0.007)	(0.007)
Household characteristics	No	Yes
Observations	67,220	67,220
Adj. R^2	0.001	0.001

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. The dependent variable in all columns is an indicator for a given birth being female. The sample only includes firstborn children (i.e., birth order of 1). The upper panel includes all states and the lower panel includes only border states. All regressions include state, birth year, and birth order fixed effects. Household characteristics in column 2 include religion, caste, sex of the household head, education of the household head, whether the household is located in a rural area, the wealth quintile of the household, and the number of older brothers. Standard errors clustered by state are in parentheses

$$\begin{aligned} FemaleBirth_{ist} =& \beta_0 + \beta_1 Treat_s * Post_t + \mathbf{X'}_{\mathbf{ist}}\tau \\ & + \gamma_t + \phi_s + \epsilon_{ist} \end{aligned} \tag{2}$$

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). 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 birth order fixed effects and a variable capturing the number of older brothers of the individual since this affects the incentives of parents to undertake a sex-selective abortion.

Since the equation 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 β_1 coefficient, in families with firstborn males and firstborn females separately.

To assess whether the differences across these two types of families are significant, we run the following triple difference specification:

$$\begin{aligned} FemaleBirth_{ist} =& \beta_0 + \beta_1 Treat_s * Post_t * FirstbornGirl_i + \beta_2 Treat_s * Post_t \\ & + \beta_3 Treat_s * FirstbornGirl_i + \beta_4 Post_t * FirstbornGirl_i \\ & + \gamma_t FirstbornGirl_i + \phi_s FirstbornGirl_i \\ & + \mathbf{X'}_{\mathbf{ist}}\tau + \omega_{st} + \epsilon_{ist} \end{aligned} \tag{3}$$

Here, $FirstbornGirl_i$ indicates if the child is born in a family where the firstborn is a girl and the β_1 coefficient 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. 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: year of birth and state of residence. 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. We first estimate the following equation separately for males and females:

$$\begin{aligned} Y_{ist} =& \beta_0 + \beta_1 Treat_s * Post_t + \mathbf{X'}_{\mathbf{ist}} \tau \\ &+ \gamma_t + \phi_s + \epsilon_{ist} \end{aligned} \tag{4}$$

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: whether they have completed grade 10, completed grade 12, and entered university. $Treat_s$ indicates if the person was born in a treated state (any of the 34 states other than Maharashtra or JK). $Post_t$ indicates if the person was born in the post-ban period (1997–2002). 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. The coefficient β_1 captures the impact of the ban on the educational attainment of males and females separately.

To test whether the coefficients are significantly different across males and females, we estimate the following:

$$\begin{aligned} Y_{ist} =& \beta_0 + \beta_1 Treat_s * Post_t * Female_i + \beta_2 Treat_s * Post_t \\ &+ \beta_3 Treat_s * Female_i + \beta_4 Post_t * Female_i \\ &+ \gamma_t Female_i + \phi_s Female_i \\ &+ \mathbf{X'}_{\mathbf{ist}} \tau + \omega_{st} + \epsilon_{ist} \end{aligned} \tag{5}$$

$Female_i$ 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 the gender gap in educational attainment in treated states.

We also include state, birth year, and female fixed effects, as well as the pairwise interactions of all three ($\gamma_t Female_i$, $\phi_s Female_i$, and ω_{st}). Again, this allows 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.

To exploit the variation across firstborn male and firstborn female families, we also estimate this triple difference specification separately for these two types of families. However, we are able to extract birth order information for only those women who remain in the home of their birth. For those women who have moved away — most frequently to their marital homes — we are not able to observe their birth order. We discuss the implication of this on our results in the next section.

5 Results

5.1 Impact on female births

The results of estimating Eqs. (2) and (3) are presented in Table 2. Columns 1–3 present the results of the differences-in-difference estimate of the impact of the ban on female births in the sample as a whole, and among firstborn male and firstborn female families separately. 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 JK. These border states include Gujarat, Madhya Pradesh, Chhattisgarh, Telangana, Andhra Pradesh, Goa, and Karnataka for Maharashtra, and Himachal Pradesh and Punjab for JK. There is a significant increase in the probability of a female birth among firstborn female families with no such change among firstborn male families. Column 4 includes estimates of a pooled regression including a triple interaction term, $Treat_s * Post_t * FirstbornGirl_i$. The coefficient on this term is positive and significant, indicating an increase of 2.5–2.9 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. The specification in column 4 also includes fixed effects for all pairwise interactions between *Treat*, *Post*, and *FirstbornGirl* (that is, state-birth year fixed effects, state-firstborn girl fixed effects, and firstborn girl-birth year fixed effects), an indicator for whether a child is born into a house with a firstborn girl, as well as state fixed effects and birth year fixed effects. All specifications control for household characteristics.

Table 2 Impact of ban on female births–higher-order births

	All	FG	FB	All
	(1)	(2)	(3)	(4)
All states				
Treat *x* Post	0.003	0.015**	−0.010	
	(0.004)	(0.006)	(0.007)	
Treat *x* Post *x* FirstbornGirl				0.025**
				(0.011)
				[0.07]
Observations	332,686	163,227	169,459	332,686
Adj.R^2	0.001	0.003	0.001	0.002
Border states				
Treat *x* Post	0.009**	0.023**	−0.006	
	(0.004)	(0.008)	(0.007)	
Treat *x* Post *x* FirstbornGirl				0.029**
				(0.012)
				[0.08]
Household characteristics	Yes	Yes	Yes	Yes
Firstborn Girl *x* State FE	No	No	No	Yes
State *x* Birthyear FE	No	No	No	Yes
Firstborn Girl *x* Birthyear FE	No	No	No	Yes
Observations	113,949	56,682	57,267	113,949
Adj.R^2	0.002	0.005	0.001	0.003

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. The dependent variable in all columns is an indicator for a given birth being female. The upper panel includes all states and the lower panel includes only border states. All regressions include state, birth year and birth order fixed effects, and number of older brothers, and column 4 includes firstborn girl fixed effects. The sample in columns 1 and 4 includes births in all households, column 2 includes births in households with a firstborn girl, and column 3 includes births in households with a firstborn boy. All columns include births at a birth order greater than 1. 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. Wild cluster bootstrap *p*-values are in brackets

We are, therefore, confident that we have controlled for potentially confounding trends in female births across these different dimensions.[4]

These results are in line with those reported by Nandi and Deolalikar (2013), though we use a different empirical strategy with quasi-random variation in exposure to the ban, 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

[4] It is possible that there exists variation in enforcement of this ban across different states by treatment. While we do not have any data on quality of compliance with the PNDT by state, we do have state fixed effects in all our estimations and state-year fixed effects as well in the triple difference specifications. To the extent that enforcement of this particular ban by the police is likely to be correlated with general administrative and police quality of the state, this variation will get swept out by the fixed effects. To rule out the possibility of variation in PNDT-specific compliance, we also re-estimate the results by excluding the states of Maharashtra and JK one by one: our results remain unchanged.

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.

We next show the estimated impact of the ban on birth orders of 2, 3, and higher than 3 separately (Table 3). The results suggest that the impact of the ban on female births is being driven by an increased number of female births at a birth order of 3 among firstborn female families.

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 relative to boys. The results of Eqs. (4) and (5) are presented in Table 4. Again, 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 only includes nine bordering states.

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 university or any other tertiary higher education institute (above 12 years of formal schooling).

We find that across all educational outcomes, male educational attainment rises among males born in treated states after the ban while female educational attainment remains unchanged. These differences are significant: the coefficient on the triple difference interaction term is negative and significant across all specifications (columns 3, 6, and 9), indicating that the growth in female educational attainment in treated states was slower after the implementation of the ban, compared to the control states, 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. 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 state and birth year, as well as controls for household characteristics. The triple difference specifications 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 leads them to substitute postnatal discrimination for prenatal discrimination, and relatively reduces their investments in their daughters, in line with the results documented in (Anukriti et al. 2021) for health outcomes. While we

Table 3 Impact of ban on female births at different birth orders

	2	3	>3
Firstborn female families			
Treat *x* Post	0.015	0.024***	0.004
	(0.016)	(0.005)	(0.014)
Household characteristics	Yes	Yes	Yes
Observations	71,228	46,717	45,282
Adj. R^2	0.004	0.003	0.003
Firstborn male families			
Treat *x* Post	−0.011***	−0.008	−0.004
	(0.004)	(0.015)	(0.016)
Household characteristics	Yes	Yes	Yes
Observations	82,024	47,455	39,980
Adj. R^2	0.001	0.001	0.001
All families			
Treat *x* Post *x* Firstborn Girl	0.027*	0.029**	0.001
	(0.015)	(0.014)	(0.007)
Household characteristics	Yes	Yes	Yes
Firstborn Girl *x* Birthyear FE	Yes	Yes	Yes
State *x* Birthyear FE	Yes	Yes	Yes
Firstborn Girl *x* State FE	Yes	Yes	Yes
Observations	153,251	94,172	85,257
Adj. R^2	0.002	0.002	0.002

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. The dependent variable in all columns is an indicator for a given birth being female. The samples only include children born at birth orders greater than 1. The regression in column 1 is estimated for births at birth order 2; in column 2, birth order 3; in column 3, all birth orders greater than 3. All regressions include state and birth year fixed effects and number of older brothers. Household characteristics 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

do not observe an absolute decline in the educational attainment of females after the ban came in, we do observe an increase in the educational attainment of males. We take this to mean that the impact of an increased number of female births, relative to male births, resulted in families increasingly focusing household resources on their male children, leading to an increase in male educational attainment but no such increase in female educational attainment. The estimated effects in Table 4 are the average effects across households with different gender compositions of children. In families with both boys and girls, the estimated effects imply a relative reallocation of resources away from daughters towards sons.

Our results on the increased probability of female births are primarily driven by firstborn female households as the use of sex-screening technologies and sex-selective abortions is concentrated among families with firstborn girls in India, compared

Table 4 Impact of ban on education

	Completed grade 10			Completed grade 12			Entered university		
	Female	Male	All	Female	Male	All	Female	Male	All
All states									
Treat *x* Post	0.044	0.065**		0.043	0.077**		0.023	0.054**	
	(0.033)	(0.030)		(0.029)	(0.037)		(0.016)	(0.020)	
Treat *x* Post *x* Female			−0.023***			−0.035***			−0.032***
			(0.006)			(0.011)			(0.010)
			[0.097]			[0.155]			[0.122]
Observations	351,816	348,398	700,214	351,816	348,398	700,214	351,816	348,398	700,214
Adj.R^2	0.31	0.32	0.33	0.28	0.29	0.31	0.22	0.23	0.25
Border states									
Treat *x* Post	0.048	0.064*		0.057	0.082*		0.036	0.071**	
	(0.034)	(0.031)		(0.034)	(0.043)		(0.021)	(0.025)	
Treat *x* Post *x* Female			−0.015**			−0.025*			−0.036**
			(0.007)			(0.012)			(0.013)
			[0.048]			[0.114]			[0.059]
Household characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Female *x* State FE	No	No	Yes	No	No	Yes	No	No	Yes
State *x* Post FE	No	No	Yes	No	No	Yes	No	No	Yes
Female *x* Post FE	No	No	Yes	No	No	Yes	No	No	Yes
Observations	118,383	121,016	239,399	118,383	121,016	239,399	118,383	121,016	239,399
Adj.R^2	0.322	0.313	0.331	0.275	0.280	0.291	0.213	0.218	0.223

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. The upper panel includes all states and the lower panel includes only border states. The dependent variable in columns 1, 2, and 3 is an indicator for whether the individual has completed grade 10. The dependent variable in columns 4, 5, and 6 is an indicator for whether the individual has completed grade 12. The dependent variable in columns 7, 8, and 9 is an indicator for whether the individual has entered university. Columns 1, 4, and 7 include only females in the sample; columns 2, 5, and 8 include only males; and columns 3, 6, and 9 include both. All regressions also include state and birth year fixed effects. Columns 3, 5, and 9 include female fixed effects as well as pairwise interactions between state, birth year, and female indicators. 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. Wild cluster bootstrap p-values are in brackets

to families with firstborn boys. Given this, we would ideally want to test variation in the impact on the gender gap in educational attainment across firstborn female and firstborn male households. In order to do this, we use the birth histories of all women in the NFHS-4 survey to identify the 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, which raises potential concerns about selection bias since women who marry early are likely to be different from women to marry later, particularly in ways relating to educational attainment.

We present the results of the impact of the ban on the gender gap in educational outcomes separately for individuals born into families with firstborn girls and firstborn boys in Table 5. Among those who have enrolled in university, the estimated relative impact of the ban on educational outcomes of women, compared to men, in treated compared to 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 10 and grade 12, the differences between firstborn female and male families are no longer significantly different from zero.

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 states that had the ban in place throughout this period, compared to states which did not. To rule out this possibility, we estimate these results using just Maharashtra and just JK as control states separately. These results are presented in Table 6 and the estimated coefficients are of a very similar size in both cases.[5]

We also estimate the impact of the ban on gender differentials in grade 12 completion and in entering university, conditional on having completed the preceding level of schooling (that is, grade 10 and grade 12 respectively). The results are presented in Table 7. The impact of the ban on the gender gap in conditional schooling outcomes is no longer significant, as with the unconditional schooling outcomes, suggesting that inequality in gender outcomes is generated in early life and reflected in the differential rates of junior school completion.

[5] Another possibility is that educational reforms that took place at the same time as the ban was implemented in 1996 could be confounding our results but given how we specify the triple difference estimator, with fixed effects for state-year, state-female, and female-year interactions, these reforms would need to be varying in their impact by state and by gender, and would need to be contemporaneous with the ban. First, we do not find any information on any such educational policies implemented in either Maharashtra or JK. Second, most government policies that have been implemented in recent decades have sought to *reduce* gender bias in educational outcomes, not to increase it (for example, the 2001 national educational programme universalising elementary education — Sarva Shiksha Abhiyan), so our results indicating a rise in gender bias in educational outcomes still support our hypothesis of an increased number of unwanted female births. Third, our estimates are robust to excluding each of these two states from the analysis one by one.

Table 5 Impact of ban on education by sex of the firstborn child

	Completed grade 10			Completed grade 12			Entered university		
	Female	Male	All	Female	Male	All	Female	Male	All
Treat *x* Post *x* Firstborn Girl	0.033	0.002		0.025*	0.028***		0.002	0.047***	
	(0.033)	(0.008)		(0.014)	(0.004)		(0.011)	(0.005)	
Treat *x* Post *x* Firstborn Girl *x* Female			0.030			−0.000			−0.041***
			(0.040)			(0.015)			(0.014)
Household characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firstborn Girl *x* Birthyear FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firstborn Girl *x* Female	No	No	Yes	No	No	Yes	No	No	Yes
Firstborn Girl *x* State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Female *x* Birthyear FE	No	No	Yes	No	No	Yes	No	No	Yes
State *x* Birthyear FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Female *x* State FE	No	No	Yes	No	No	Yes	No	No	Yes
Observations	142,558	198,680	341,239	142,558	198,680	341,239	142,558	198,680	341,239
Adj. R^2	0.42	0.36	0.38	0.43	0.34	0.37	0.37	0.26	0.31

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. The dependent variable in columns 1, 2, and 3 is an indicator for whether the individual has completed grade 10. The dependent variable in columns 4, 5, and 6 is an indicator for whether the individual has completed grade 12. The dependent variable in columns 7, 8, and 9 is an indicator for whether the individual has entered university. Columns 1, 4, and 7 include only females; columns 2, 5, and 8 include only males; and columns 3, 6, and 9 include both. All regressions include state, birth year and firstborn female fixed effects, and pairwise interactions of all three. Columns 3, 6, and 9 include all threeway interactions between Treat, Post, Firstborn Girl, and Female. 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 6 Impact on education: Maharashtra and JK

	Completed grade 10			Completed grade 12			Entered university		
	Female	Male	All	Female	Male	All	Female	Male	All
Maharashtra only									
Treat *x* Post	0.078***	0.095***		0.073***	0.117***		0.033**	0.072***	
	(0.018)	(0.017)		(0.017)	(0.016)		(0.014)	(0.014)	
Treat *x* Post *x* Female			−0.019***			−0.045***			−0.041***
			(0.005)			(0.007)			(0.006)
Household characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Female *x* Birthyear FE	No	No	Yes	No	No	Yes	No	No	Yes
State *x* Birthyear FE	No	No	Yes	No	No	Yes	No	No	Yes
Female *x* State FE	No	No	Yes	No	No	Yes	No	No	Yes
Observations	340,204	337,119	677,323	340,204	337,119	677,323	340,204	337,119	677,323
Adj. R^2	0.31	0.32	0.33	0.28	0.30	0.31	0.22	0.24	0.25
JK only									
Treat *x* Post	−0.000	0.026		0.005	0.022		0.011	0.030**	
	(0.018)	(0.017)		(0.017)	(0.017)		(0.015)	(0.014)	
Treat *x* Post *x* Female			−0.028***			−0.020***			−0.021***
			(0.005)			(0.007)			(0.007)
Household characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Female *x* Birthyear FE	No	No	Yes	No	No	Yes	No	No	Yes
State *x* Birthyear FE	No	No	Yes	No	No	Yes	No	No	Yes
Female *x* State FE	No	No	Yes	No	No	Yes	No	No	Yes
Observations	336,816	332,841	669,657	336,816	332,841	669,657	336,816	332,841	669,657
Adj. R^2	0.31	0.32	0.33	0.28	0.29	0.31	0.22	0.23	0.25

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. The upper panel includes only Maharashtra as a control state and the bottom panel includes only JK as a control state. The dependent variable in columns 1, 2, and 3 is an indicator for whether the individual has completed grade 10. The dependent variable in columns 4, 5, and 6 is an indicator for whether the individual has completed grade 12. The dependent variable in columns 7, 8, and 9 is an indicator for whether the individual has entered university. Columns 1, 4, and 7 include only females; columns 2, 5, and 8 include only males; and columns 3, 6, and 9 include both. All regressions also include state and birth year fixed effects, and columns 3, 5, and 9 include female fixed effects as well as pairwise interactions between all state, birth year, and female indicators. 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 7 Impact of ban on conditional educational attainment

	Completed grade 10			Completed grade 12			Entered university		
	Female	Male	All	Female	Male	All	Female	Male	All
Treat *x* Post	0.044	0.065**		0.043	0.077**		0.023	0.054**	
	(0.033)	(0.030)		(0.029)	(0.037)		(0.016)	(0.020)	
Treat *x* Post *x* Female			−0.023***			−0.024			−0.012
			(0.006)			(0.037)			(0.029)
Household characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Female *x* Birthyear FE	No	No	Yes	No	No	Yes	No	No	Yes
State *x* Birthyear FE	No	No	Yes	No	No	Yes	No	No	Yes
Female *x* State FE	No	No	Yes	No	No	Yes	No	No	Yes
Observations	351,816	348,398	700,214	351,816	348,398	292,836	351,816	348,398	170,031
Adj. R^2	0.31	0.32	0.33	0.28	0.29	0.30	0.22	0.23	0.19

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. The dependent variable in columns 1, 2, and 3 is an indicator for whether the individual has completed grade 10. The dependent variable in columns 4, 5, and 6 is an indicator for whether the individual has completed grade 12. The dependent variable in columns 7, 8, and 9 is an indicator for whether the individual has entered university. The sample in columns 4, 5, and 6 only includes those who have completed grade 10, and in columns 7, 8, and 9 only for those who have completed grade 12. All regressions also include state and birth year fixed effects, and columns 3, 5, and 9 include female fixed effects as well as pairwise interactions between state, birth year, and female indicators. 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

5.3 Heterogeneity by wealth

We next disaggregate the results across households by five wealth quintiles.[6]

As discussed above, wealthy families are more likely to practice sex selection but also have a greater ability to evade the ban on sex selection than households that are

[6] The wealth quintiles are derived from the wealth index in NFHS-4, which is a composite measure of the household's cumulative living standard. Using principal component analysis, the householders are attributed scores according to the number and kinds of consumer goods they own, and characteristics of their housing (such as source of drinking water, toilet facilities, and flooring materials). Then, national wealth quintiles are formed by assigning the score of the household to each member, ranking all people in the household population by their score, and dividing the distribution into 5 equal bands (of 20% of the total population each). We use the observed wealth quintile of the household in which an individual is located at the time of the survey (the marital household in the case of married women) as a proxy for the wealth of the household at the time that educational decisions were taken. Given the low degree of intergenerational socioeconomic mobility in India, particularly in rural areas (Emran and Shilpi 2015; Asher et al. 2021), we are confident that a high level of switching across wealth quintiles is not driving our results.

liquidity constrained. If the latter effect dominates, we would expect to see larger effects for less-wealthy households in treated states. These results are presented in Table 8.

The coefficient on $Treat_s * Post_t * FirstGirl_i$ is large, positive, and statistically significant across the first three columns for households in the bottom three wealth quintiles, implying a 2.8–6.7 percentage point increase in the probability of a female birth in households with firstborn girls in treated states. For the sample of border states only, the effect size ranges from 4.4 to 5.9 percentage points, again concentrated only in the first three wealth quintiles. In both samples, the largest effects are observed for the households in the lowest wealth quintile. Conversely, the coefficient is insignificantly different from zero for households in the fourth and fifth wealth quintiles, 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.

One potential reason for why particularly wealthy households appear to be unaffected by the ban is that they may be in a position to temporarily migrate to neighbouring states to evade the ban. To rule out this possibility, we also estimate these specifications by (a) excluding border states and (b) including border states but excluding border districts. The results remain unchanged, and are available in Table 9.

In short, the impact of the ban on female births was driven by less-wealthy households who find it more difficult to get access to private sources of sex-screening technologies and sex-selective abortions. In Table 10, we disaggregate the results on the impact of the ban on education as well, across five wealth quintiles. In line with results on female births, we find that the impact of the ban on female educational attainment is largest for households in the bottom three wealth quintiles, although households in the fourth quintile also observe a relative fall for females in completion rates of grade 12 and entry into university. These results hold across the sample of all states as well across border states only. Among these relatively less wealthy households, the probability of completing grade 10 for females reduces by 2.8–5.4 percentage points, of completing grade 12 reduces by 4.1–5.4 percentage points, and of entering university reduces by 2.8–6.3 percentage points, relative to males, when comparing outcomes across treated and control states.

5.4 Robustness

5.4.1 Differential pre-treatment trends for educational outcomes

Although we control for fixed effects for female, state, and 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,

Table 8 Impact of ban on female births at different wealth quintiles

Female births	(1)	(2)	(3)	(4)	(5)
All states					
Treat *x* Post *x* Firstborn Girl	0.067***	0.026*	0.028**	−0.010	0.006
	(0.012)	(0.014)	(0.011)	(0.020)	(0.043)
	[0.12]	[0.40]	[0.17]	[0.89]	[0.74]
Observations	68,029	72,332	70,412	65,363	56,550
Adj.R^2	0.001	0.001	0.001	0.004	0.006
Border states					
Treat *x* Post *x* Firstborn Girl	0.059***	0.031	0.044**	−0.006	−0.005
	(0.013)	(0.020)	(0.018)	(0.026)	(0.045)
	[0.09]	[0.34]	[0.15]	[0.90]	[0.88]
Household characteristics	Yes	Yes	Yes	Yes	Yes
Firstborn Girl *x* State FE	Yes	Yes	Yes	Yes	Yes
State *x* Birthyear FE	Yes	Yes	Yes	Yes	Yes
Firstborn Girl *x* Birthyear FE	Yes	Yes	Yes	Yes	Yes
Observations	24,932	25,352	23,553	21,565	18,547
Adj.R^2	0.000	0.001	0.002	0.003	0.009

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. The dependent variable in all columns is an indicator for a given birth being female. The upper panel includes all states and the lower panel includes only border states. The regressions in each column are estimated for each wealth quintile, with column 1 including the least wealthy households and column 5 including the most wealthy households. Household characteristics 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, birth order and firstborn female fixed effects, and number of older brothers. Standard errors clustered by state are in parentheses. Wild cluster bootstrap *p*-values are in brackets

Table 9 Impact of ban on female births in non-border states and non-border districts

	Non-border states		Non-border districts	
Female Births	(1)	(2)	(3)	(4)
Treat *x* Post *x* Firstborn Girl	0.090***	0.100***	0.039***	0.036***
	(0.025)	(0.028)	(0.011)	(0.012)
Household characteristics	Yes	Yes	Yes	Yes
Firstborn Girl *x* Birthyear FE	No	Yes	No	Yes
State *x* Birthyear FE	No	Yes	No	Yes
Firstborn Girl *x* State FE	No	Yes	No	Yes
Observations	258,786	258,785	312,369	312,368
Adj. R^2	0.023	0.023	0.001	0.002

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. The dependent variable in all columns is an indicator for a given birth being female. The samples only include children born at birth orders greater than 1. Columns 1 and 2 include all non-border states and columns 3 and 4 include all non-border districts. All regressions include state, firstborn female, birth year and birth order fixed effects, and number of older brothers. Household characteristics 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 10 Impact of ban on education for different wealth quintiles

	(1)	(2)	(3)	(4)	(5)
All states					
Completed grade 10					
Treat *x* Post *x* Female	−0.049***	−0.028**	−0.045***	−0.003	0.017
	(0.011)	(0.011)	(0.013)	(0.009)	(0.015)
	[0.111]	[0.156]	[0.081]	[0.781]	[0.641]
Observations	136,156	144,827	146,016	141,836	131,379
Adj.R^2	0.22	0.26	0.28	0.32	0.37
Completed grade 12					
Treat *x* Post *x* Female	−0.054***	−0.049**	−0.044**	−0.036***	0.012
	(0.015)	(0.023)	(0.019)	(0.010)	(0.011)
	[0.118]	[0.118]	[0.133]	[0.226]	[0.537]
Observations	136,156	144,827	146,016	141,836	131,379
Adj.R^2	0.16	0.22	0.26	0.31	0.38
Entered university					
Treat *x* Post *x* Female	−0.033**	−0.042***	−0.028***	−0.049***	−0.007
	(0.013)	(0.015)	(0.008)	(0.009)	(0.019)
	[0.131]	[0.113]	[0.111]	[0.136]	[0.701]
Observations	136,156	144,827	146,016	141,836	131,379
Adj.R^2	0.13	0.16	0.19	0.24	0.33
Border states					
Completed grade 10					
	(1)	(2)	(3)	(4)	(5)
Treat *x* Post *x* Female	−0.038*	−0.025	−0.054***	0.009	0.034
	(0.020)	(0.017)	(0.015)	(0.013)	(0.023)
	[0.215]	[0.267]	[0.029]	[0.552]	[0.372]
Observations	51,020	51,036	49,157	46,501	41,685

Table 10 (continued)

	(1)	(2)	(3)	(4)	(5)
Adj.R^2	0.20	0.25	0.29	0.33	0.39
Completed grade 12					
Treat *x* Post *x* Female	−0.041*	−0.044	−0.047**	−0.021*	0.035*
	(0.022)	(0.025)	(0.019)	(0.011)	(0.017)
	[0.216]	[0.285]	[0.074]	[0.257]	[0.176]
Observations	51,020	51,036	49,157	46,501	41,685
Adj.R^2	0.15	0.19	0.24	0.30	0.38
Entered university					
Treat *x* Post *x* Female	−0.030*	−0.045**	−0.038***	−0.063***	0.004
	(0.016)	(0.016)	(0.010)	(0.013)	(0.027)
	[0.188]	[0.042]	[0.041]	[0.045]	[0.941]
Household characteristics	Yes	Yes	Yes	Yes	Yes
Female *x* Birthyear FE	Yes	Yes	Yes	Yes	Yes
State *x* Birthyear FE	Yes	Yes	Yes	Yes	Yes
Female *x* State FE	Yes	Yes	Yes	Yes	Yes
Observations	51,020	51,036	49,157	46,501	41,685
Adj.R^2	0.11	0.13	0.17	0.22	0.31

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. The dependent variable for the regressions in the first and fourth panels is an indicator for whether the individual has completed grade 10, in the second and fifth panels is indicator for whether the individual has completed grade 12, and for the third and sixth panels it is an indicator for whether the individual has entered university. The first, second, and third panels include all states and the fourth, fifth, and sixth panels include only border states. The regressions in each column are estimated for each wealth quintile, with column 1 including the least wealthy households and column 5 including the most wealthy households. All estimations include household characteristics such as 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 brackets

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 Fig. 2. There is no evidence of any differential trends prior to 1996, the year of treatment.

5.4.2 Restricting analysis to births before 2000

One concern with the results using the full sample of individuals born between 1989 and 2002 is that there is a small number of individuals born in 2000 and after who have completed grade 12 or entered university by the year 2016 (Table 18). Adding these individuals to the main schooling regressions could add noise to the estimations. To check that this is not the case, we estimate the main results for completion of grade 12 and entering university on a sample of individuals born before 2000 and present these results in Table 19. The estimated coefficients are very similar to the main results: male educational outcomes increase for both completion of grade 12 and entering university, and the difference between male and female educational attainment is significantly different from 0.

5.4.3 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. (2020) and Anukriti et al. (2021) 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 Eq. (3) as follows:

$$\begin{aligned} IdealFractionBoys_{ist} = & Treat_s * Post_t * FirstbornGirl_i + \beta_2 Treat_s * Post_t \\ & + \beta_3 Treat_s * FirstbornGirl_i + \beta_4 Post_t * FirstbornGirl_i \\ & + \gamma_t FirstbornGirl_i + \phi_s FirstbornGirl_i \\ & + \mathbf{X'}_{\mathbf{ist}} \tau + \omega_{st} + \epsilon_{ist} \end{aligned} \tag{6}$$

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 34 states other than Maharashtra or JK), while $FirstbornGirl_i$ 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

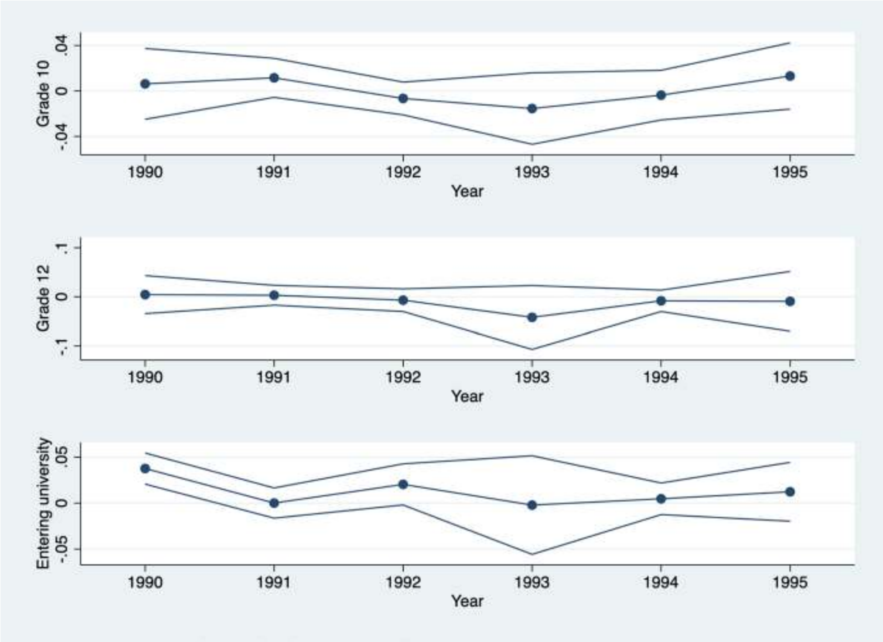

Fig. 2 Testing for parallel trends prior to the introduction of the ban. This figure tests for differential trends in educational outcomes (graduating grade 10, graduating grade 12, and entering university) during the period 1989–1995, prior to the treatment in 1996. Each graph plots the coefficients on the $Treat * Female * Birthyear$ indicator variables for all birth years during the period 1990–1995, with 1989 as the omitted 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 the regressions

post-ban 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 20 in the Appendix. In columns 1 and 2, we present the estimates of a difference-in-difference estimator, and in both cases the coefficient on $Treat * Post$ is insignificantly different from 0. In columns 3, 4, and 5, the coefficient on $Treat_s * Post_t * FirstbornGirl_i$ is insignificantly different from zero, suggesting that women's degree of son-preference appears to have not changed over time among mothers of firstborn females compared to mothers of firstborn males, when comparing treated states with control states.

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.

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 in educational attainment. Second, the impact of more girls being born on gender gaps in education is likely to be mediated by sibling age and sex composition, including birth order and number of brothers and sisters. 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 families making compensatory investments in their daughters to increase the probability of their making a higher quality match, including through increased 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. The implementation of the PNDT Act specifically imposed stringent regulations on the use of ultrasound technology through several ways (Phutke et al. 2018): only radiologists with medical degrees and trained obstetricians were permitted to conduct ultrasound tests and the sale of ultrasound machines were regulated and restricted to providers with such training. Most of these professionals are to be found only in urban areas, compared to rural areas. At the same time, government laboratories and clinics were much more closely monitored than private-sector institutions (Retherford and Roy 2003). Households in rural areas have lower access to health facilities in general, including private-sector health facilities, compared to households in urban areas. Even given these low levels, the access to sex-detection techniques and abortions fell more sharply in rural areas with the PNDT than it did in urban areas. Given this, rural 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 Eqs. (3) and (5) separately for households located in rural and urban areas. The results are presented in Table 11. We find that female births increased only in rural areas and not in urban areas, with the probability of female births in firstborn female families increasing by 6.2–6.4 percentage points. Correspondingly, 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. The differences between the urban and rural coefficients are significant for all three educational outcomes (estimates from the pooled models are provided in Table 12).

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. Twenty percent of men and 40% of women in the data 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. (2021) in testing for this in two ways. First, we use a mother-year dataset of births and estimate Eq. (3) 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 Eq. (3) and additionally control for mother's age fixed effects. If mothers of firstborn females 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 Table 13. 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 the treated and control states. 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

Table 11 Impact on female births and education: rural and urban households

	Rural			Urban		
Female births	(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)	(0.023)	(0.022)	(0.024)
Household characteristics	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	153,401	153,401	153,384	57,372	57,372	57,369
Adj.R^2	0.000	0.001	0.001	0.001	0.001	0.001
	Completed grade 10		Completed grade 12		Entered university	
	Rural	Urban	Rural	Urban	Rural	Urban
Treat *x* Post *x* Female	−0.062***	0.007	−0.061**	−0.024	−0.045***	−0.012
	(0.012)	(0.007)	(0.025)	(0.015)	(0.011)	(0.019)
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	297,410	129,582	297,410	129,582	297,410	129,582
Adj.R^2	0.235	0.262	0.187	0.239	0.123	0.192

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. In the top panel, the dependent variable is in all columns is an indicator for a given birth being female. In the top panel, columns 1, 2, and 3 include only births in rural households and columns 4, 5, and 6 include only births in urban households. All regressions also include state, birth year, and firstborn girl fixed effects. In the bottom panel, 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. In both panels, the samples include only households in the bottom 60% of the wealth distribution. Household characteristics in all specifications include religion, caste, sex of the household head, and education of the household head. Standard errors clustered by state are in parentheses

Table 12 Impact of ban on education (quadruple difference with rural-urban variation)

	Completed grade 10 (1)	Completed grade 12 (2)	Entered university (3)
Treat *x* Post *x* Female *x* Rural	−0.072***	−0.037***	−0.034***
	(0.011)	(0.014)	(0.011)
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	426,999	426,999	426,999
Adj. R^2	0.255	0.218	0.166

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. The dependent variable in column 1 is an indicator for whether the individual has completed grade 10, in column 2 is an indicator for whether the individual has completed grade 12, and column 3 is an indicator for whether they have entered university. In both panels, the samples include only households in the bottom 60% of the wealth distribution. Household characteristics in all columns include religion, caste, sex of the household head, education of the household head, and whether the household is located in a rural area. All regressions include state, birth year and female fixed effects, and pairwise interactions between all three, as well as threeway interactions between Treat, Post, Female, and Rural. Standard errors clustered by state are in parentheses

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.

6.3 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 females and males are disadvantaged relative to early-born females and males, laterborn females 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 females is amplified at higher birth orders, then we would expect to see larger effects of the ban on sex-screening technologies on laterborn females relative to firstborn females.

Second, siblings with sisters face a smaller penalty in terms of the household resources they receive compared to siblings without sisters in societies

Table 13 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

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. The data in these specifications includes all women who gave birth to children between the years 1989 and 2002. Mothers who have children both before and after 1996 are excluded from the sample. The specification in columns 1 and 2 uses 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). The samples include only households in the bottom 60% of the wealth distribution. All regressions include state, birth year, and firstborn female 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

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 males are perceived to have a higher return compared to similar investments in females, the addition of more females to a family reduces the competition faced by existing siblings and increases investments made in them. Again, whether the benefits of additional sisters are greater for males or for females 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 is similar across males and females. In contrast, Lei et al. (2017) find that males in China tend to benefit more from having sisters than females, 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 an increasingly wider gender gap in educational attainment among males and females with sisters, compared to males and females without sisters.

We next test for whether the impact of the ban on educational outcomes varies across these two dimensions. As discussed previously in Section 5.2, this analysis can be done only for unmarried women who live in their natal households, since we cannot observe birth order and sibling composition for women who live in their marital households.

The results of the analysis of the impact of sibling age and sex composition are presented in Table 14. 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, 4, and 7) and laterborn children (columns 2, 5, and 8). 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. From the estimation of a pooled model, we can see that these differences are significant for completion of grade 12 and entry into university (columns 6 and 9). 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 2.5 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. From the estimation of a pooled model, we see that these differences are significant for completion of grade 10 and entry into university (columns 3 and 9). We interpret these results to mean that for educational attainment levels that require high household investments, men benefit more than women from having more sisters, similar to Lei et al.

Table 14 Impact on education by sibling composition

	Completed grade 10			Completed grade 12			Entered university		
Birth order	1	> 1	All	1	> 1	All	1	> 1	All
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)	
Treat *x* Post *x* Female *x* Birth order > 1			−0.053			−0.056***			−0.018**
			(0.046)			(0.013)			(0.009)
Observations	72,194	134,414	206,609	72,194	134,414	206,609	72,194	134,414	206,609
Adj. R^2	0.35	0.32	0.34	0.32	0.29	0.30	0.24	0.21	0.23
Number of sisters	**0**	**> 0**	**All**	**0**	**> 0**	**All**	**0**	**> 0**	**All**
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)	
Treat *x* Post *x* Female *x* No sisters			−0.044***			0.017			0.049***
			(0.011)			(0.013)			(0.012)
Household characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Female *x* Birthyear FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State *x* Birthyear FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Female *x* State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	48,723	157,876	206,609	48,723	157,876	206,609	48,723	157,876	206,609
Adj. R^2	0.389	0.315	0.338	0.355	0.281	0.304	0.269	0.208	0.226

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. The dependent variable in columns 1, 2, and 3 is an indicator for whether the individual has completed grade 10. The dependent variable in columns 4, 5, and 6 is an indicator for whether the individual has completed grade 12. The dependent variable in columns 7, 8, and 9 is an indicator for whether the individual has entered university. In the upper panel, columns 1, 4, and 7 include only individuals at birth order 1; columns 2, 5, and 8 include only individuals at birth order greater than 1; and columns 3, 6, and 9 include all individuals. In the lower panel, columns 1, 4, and 7 include only individuals with no sisters; columns 2, 5, and 8 include only individuals with sisters; and columns 3, 6, and 9 include all individuals. In both panels, the samples include only households in the bottom 60% of the wealth distribution. All regressions include state, birth year and female fixed effects, and pairwise interactions between al three, as well and number of siblings. Regressions in columns 3, 6, and 9 also include all threeway interactions between Treat, Post, Female, and Birth order >1/No sisters. Household characteristics in all specifications include religion, caste, sex of the household head, and education of the household head. Standard errors clustered by state are in parentheses

(2017). The gender gap, therefore, worsens with the presence of more women in a family.

6.4 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 2021). 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 as they search longer for suitable matches, 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:

$$\begin{aligned} Y_{ist} = &\beta_0 + \beta_1 Treat_s * Post_t + \mathbf{X'}_{\mathbf{ist}}\tau \\ &+ \omega_s + \delta_t + \epsilon_{ist} \end{aligned} \tag{7}$$

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 Eq. (7) are presented in Table 15, 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.

What are women doing if they are delaying marriage? One answer is that women are working more. We estimate Eq. (7) with labour force participation as the outcome variable and find that labour force participation does appear to increase by 5.6 percentage points (column 5, Table 15) among women born in treated states after the implementation of the ban. This could be another way in which women increase their value on the marriage market. Alternatively, given that these are women in relatively less wealthy households, they could simply be forced to work for a living because they are not able to get married as early as before.

Table 15 Impact on age of marriage and employment

	Married at 18	Married at 15	Age of marriage	Spousal age gap	Employed
Treat *x* Post	−0.098	−0.053***	0.568**	−0.382	0.056***
	(0.061)	(0.018)	(0.242)	(0.293)	(0.019)
Household characteristics	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes
Birthyear FE	Yes	Yes	Yes	Yes	Yes
Observations	170,895	170,895	85,915	68,709	29,490
Adj. R^2	0.206	0.079	0.111	0.090	0.030

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. The dependent variable in columns 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 are age of marriage and the spousal age gap, respectively. Column 5 includes both married and unmarried women and the dependent variable is an indicator for working in the past 12 months. The samples include only households in the bottom 60% of the wealth distribution. All specifications include state and birth year fixed effects. Household characteristics in all columns include religion, caste, sex of the household head, education of the household head, and whether the household is located in a rural area. Standard errors clustered by state are in parentheses

The increase in the number of females caused by the ban has clear impacts on the marriage market, along expected lines. It is plausible that this marriage squeeze 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 and 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 and 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 their matching with a high-quality groom. Higher age at marriage is also associated with a number of non-educational outcomes, such as increased labour force participation, which we observe in our data, 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.

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. For example, under, Dhanlakshmi, a conditional cash transfer scheme in India, parents received payments not just on the birth of daughters but also after immunising them and enrolling them in primary school. The scheme was successful in raising female births as well as improving health and educational outcomes for girls (Biswas et al. 2021). However, cracking down on abortion clinics can only go so far if the underlying social norms favouring sons remain unchanged.

Appendix

Table 16 Unadjusted sample means by state, year, gender, and firstborn sex

	Control states		Treated states	
	1989–95	1997–2002	1989–95	1997–2002
FirstbornFemale–FirstbornMale 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 10	−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

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 grades 10 and 12 and entry rate into university between females and males. Standard errors of these estimated sample means are given in parentheses

Table 17 Additional summary statistics

	All	Control	Treated	*p*-value
Demographic characteristics (HH head)				
Male	0.92	0.94	0.92	0.000
	(0.27)	(0.24)	(0.28)	
Hindu	0.76	0.61	0.77	0.000
	(0.43)	(0.49)	(0.42)	
Muslim	0.13	0.32	0.11	0.000
	(0.34)	(0.47)	(0.31)	
Scheduled Caste	0.85	0.97	0.84	0.000
	(0.36)	(0.17)	(0.37)	
Scheduled Tribe	0.11	0.02	0.12	0.000
	(0.31)	(0.14)	(0.32)	
Socio-economic characteristics (all HH members)				
Share of female pop. (sex ratio)	0.49	0.49	0.49	0.015
	(0.50)	(0.50)	(0.50)	
Avg. age	25.47	25.92	25.42	0.000
	(19.15)	(18.87)	(19.18)	
Share married	0.63	0.61	0.63	0.000
	(0.48)	(0.49)	(0.48)	
1st Wealth quintile	0.16	0.06	0.17	0.000
	(0.36)	(0.24)	(0.37)	
2nd Wealth quintile	0.17	0.08	0.18	0.000
	(0.37)	(0.28)	(0.38)	
3rd Wealth quintile	0.20	0.19	0.20	0.000
	(0.40)	(0.39)	(0.40)	
4th Wealth quintile	0.23	0.31	0.22	0.000
	(0.42)	(0.46)	(0.41)	
5th Wealth quintile	0.25	0.35	0.24	0.000
	(0.43)	(0.48)	(0.42)	
Level of education (all HH members)				
Completed grade 10	0.16	0.20	0.16	0.000
	(0.37)	(0.40)	(0.37)	
Completed grade 12	0.08	0.10	0.08	0.000
	(0.28)	(0.29)	(0.27)	
Entered university	0.10	0.12	0.10	0.000
	(0.30)	(0.33)	(0.29)	
Avg. educational attainment (years)	4.10	4.82	4.02	0.000
	(4.64)	(4.80)	(4.62)	
Healthcare visits (in the last 12 months)				
Healthcare facility visit	0.60	0.77	0.59	0.000
	(0.49)	(0.42)	(0.49)	
Observations	516,035	49,718	466,317	

This table shows differences between the treated and control states across key variables using data from the National Family Health Survey — 2 (1998–1999). Standard errors clustered by state are in parentheses

Table 18 Observations by birthyear

	Completed grade 10	Completed grade 12	Entered university
1989	19,998	14,271	9,105
1990	25,111	17,915	11,591
1991	26,480	19,478	12,643
1992	24,321	18,329	12,120
1993	27,891	20,832	13,497
1994	27,759	20,906	13,289
1995	29,449	21,629	12,839
1996	30,585	21,548	11,065
1997	31,422	18,409	6,704
1998	32,216	12,943	2,537
1999	25,440	4,569	324
2000	16,231	700	28
2001	5,801	54	10
2002	723	13	9
N	323,427	191,596	105,761

This table presents the number of students completing grade 10 (in column 1), completing grade 12 (in column 2), and entering university (in column 3), by birth year

Table 19 Impact of ban on education for those born before 2000 only

	Completed grade 12			Entered university		
	Female	Male	All	Female	Male	All
Treat *x* Post	0.053	0.087*		0.035**	0.064***	
	(0.033)	(0.048)		(0.015)	(0.022)	
Treat *x* Post *x* Female			−0.034**			−0.030**
			(0.017)			(0.011)
Household characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Female *x* Birthyear FE	No	No	Yes	No	No	Yes
State *x* Birthyear FE	No	No	Yes	No	No	Yes
Female *x* State FE	No	No	Yes	No	No	Yes
Observations	268,364	258,357	526,721	268,364	258,357	526,721
Adj. R^2	0.27	0.25	0.27	0.23	0.23	0.24

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. The dependent variable in columns 1, 2, and 3 is an indicator for whether the individual has completed grade 12. The dependent variable in columns 4, 5, and 6 is an indicator for whether the individual has entered university. The sample in all specifications is restricted to only births before 2000. Columns 1 and 4 include only females, columns 2 and 5 include only males, and columns 3 and 6 include both. All regressions also include state and birth year fixed effects, and columns 3 and 6 include female fixed effects as well as pairwise interactions between state, birth year, and female indicators. 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 20 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

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. The dependent variable is the ideal fraction of sons wanted by a mother. All regressions also include state, birth year, and firstborn girl 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

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Declarations

Conflict of interest The authors declare no competing interests.

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