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Shifting gendered social norms: Impact of a mass media campaign on child health in India

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Abstract

Policy measures that seek to address son preference through restrictions on the tools of sex-selective abortions, without addressing the underlying causes, have been found to generate negative welfare consequences for unwanted surviving girls. Unlike these top-down supply-side measures, demand-side measures that focus on increasing the demand for girls by shifting social norms of son preference can mitigate these adverse welfare consequences. We study the impact of an intervention aimed at reducing discrimination against girls, which has both supply-side and demand-side elements. The intervention, implemented in India between 2015-18 included a mass media campaign designed to increase the perception of the value of a female child, while also tightening the policing of illegal sex-selective abortions. We exploit variation in the timing of exposure to the programme across Indian districts as well as quasi-exogenous variation in the sex of the firstborn child to identify the impact of the programme and find that it led to an increased proportion of female births as well as a reduction in the gender gap in mortality in intensively treated families. The main mechanism that explains our results is a relative increase in health investments in daughters, such as breastfeeding and vaccinations.

JEL-Classification: I18; I12; J13; J16; O1
Keywords: missing women, son preference, infant mortality, health investments, media intervention

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1 Introduction

A key policy challenge in many patriarchal societies with a strong social norm of son preference is how to tackle the problem of "missing women", as exemplified by a male-biased sex ratio (Sen, 1992). Many governments around the world, including in China, India, South Korea, Taiwan and Vietnam, have responded to this challenge with top-down measures such as bans on all abortions, bans on sex-selective abortions, and restrictions on the use of ultrasound and other foetal sex-screening technologies. These supply-side measures seek to limit access to the tools of prenatal sex selection without necessarily affecting the demand for prenatal sex-selection: as such, while they may lead to a more balanced sex ratio, in the absence of changes to underlying social norms, they may also displace prenatal discrimination to postnatal margins (Goodkind, 1996). Families that cannot practice prenatal sex selection may begin to discriminate against unwanted girls, either directly or indirectly, leading to worsening gender gaps in health and educational outcomes (Dasgupta and Sharma, 2021, 2022; Rastogi and Sharma, 2022). In contrast, demand-side measures seek to shift the demand for male and female children by changing underlying social norms, particularly around son preference. Such policies have previously taken the form of mass media campaigns that inform, educate and advocate for more progressive gender norms, and shift perceptions about the economic and social value of daughters. Could such policies mitigate the adverse welfare consequences of increased female births by increasing investments in surviving daughters?

In this paper, we examine the impact of a policy intervention with both supply-side and demand-side elements on the probability of female births as well as child mortality and health outcomes. The supply-side elements seek to restrict access to sex-selective abortions while the demand-side elements comprise a mass media campaign that promotes the value of daughters and encourages families to invest in their health and education. A policy with both supply-side and demand-side elements would lead to an increase in female births, but the impact on the gender gap in health outcomes would be theoretically ambiguous. If the supply-side measures dominate, we would expect to see an increase in female births with worsening health outcomes for "unwanted" daughters relative to sons. In particular, the increased births of unwanted girls can lead to reduced investments in their human capital and well-being, worsening their health outcomes. This could take place on account of open discrimination against girls, compared to boys. It could also result from families resorting to the use of fertility stopping rules where they keep having children until they achieve a desired number of sons. In this case, girls are disproportionately born into larger families, where they face increased competition for sibling resources, leading to a widening gender gap in health outcomes across the entire population. However, demand-side measures could mitigate these adverse consequences by directly increasing the demand for girls, which results in both increased female births as well as increased human capital investments in now desired daughters. If demand-side measures dominate, the negative impact on the gender gap in health outcomes could be reversed entirely if families increase health investments in their daughters.

We estimate the impact of a mass media campaign launched in India – the *Beti Bachao Beti Padhao* (Save Girls, Educate Girls) programme – aimed at increasing levels of gender sensitisation, promoting the perception that girls are as valuable as boys, creating incentives for female education and reducing gender discrimination. This campaign was rolled out along with several supply-side interventions strengthening the implementation of the existing legal restrictions on sex screening and sex-selective abortions. The rollout of the campaign was staggered across districts: during the first phase, 100 districts were covered, during the second phase, an additional 160 districts were introduced, and finally the campaign was extended to all 639 districts in the entire country in the third phase.

We exploit intertemporal and spatial variation in the rollout of the programme, as well as quasi-exogenous variation in the sex of the firstborn child, to identify the impact of the programme on the probability of female births and on the gender gap in health outcomes among families that are most intensively affected by the treatment. Previous research has found that Indian families have strong *eldest* son preference (Jayachandran and Pande, 2017): while prenatal sex selection is not common at the first birth order, families with firstborn females are significantly more likely to resort to sex-selective abortions at higher birth orders than families with firstborn males (Anukriti et al., 2021). Families with firstborn females thus constitute a group of families which are intensively affected by the programme, since it is these families who largely engage in prenatal sex selection. Using the quasi-exogenous variation in the sex of the firstborn child, we estimate a triple difference estimator that estimates the impact of the intervention on the gender gap in health outcomes in firstborn female families treated by the programme, compared with firstborn male families treated by the programme.

Strikingly, we find that the programme led simultaneously to an increase in the proportion of female births and a decrease in the gender gap in mortality in intensively treated families. This relative decrease in mortality for girls was driven by increasing investments in female children, such as increased breastfeeding and vaccinations of both pregnant women and their daughters. Fertility continued to increase in firstborn female families treated by the ban, suggesting that families did resort to the fertility stopping rule to achieve a desired number of sons once sex-selective abortions were increasingly restricted. However, the increased competition of sibling resources did not disadvantage female children in particular, as families were also more likely to invest in their daughters, compared to their sons. Our approach is robust to potential bias emerging from non-random placement of the programme across districts, and we find no evidence of pre-existing trends that could be driving our results.

We provide the first estimates of the treatment effects of a mass media campaign on the gender gap in mortality and health investments, allowing us to comment on the relative efficacy of a supply-side policy – the ban on access to sex-selective abortions – compared to a policy that incorporates demand-side interventions seeking to shift the underlying level of son preference. There is a growing body of literature that points to the potential for demand-side interventions to change hardwired social preferences and norms (DellaVigna, Stefano and La Ferrara, Eliana, 2015). In the context of gender norms, Jensen and Oster (2009) and La Ferrara et al. (2012) find that access to cable television reduces fertility in India and Brazil, respectively. In the Indian context, the authors find a decline in son preference as well. Exposure to educational entertainment has been found to shift attitudes towards domestic violence in Nigeria (Banerjee et al., 2019). Dhar et al. (2018) find that a school-based intervention that engaged adolescents in classroom discussions about gender equality in Haryana, India, made children's attitudes more supportive of gender equality. Levy et al. (2020) review the public health literature and identify two programmes with media components that were successful in changing attitudes about gender norms as well as increased use of contraception. However, ours is the only study of a programme, to our knowledge, that has shifted investments in health as well as mortality outcomes. While our analysis is limited to the short-to-medium run since we only observe data on children between 1 and 5 years after the implementation of this programme, our results are promising and suggest the importance of demand-side elements, such as media-based efforts, in shifting social norms through gender sensitisation efforts. This has important insights for policy design for countries struggling to reduce pervasive and deep-rooted gender discrimination and to address the problem of missing women.

The rest of the paper is organised as follows. Section 2 presents the background on the media intervention campaign, as well as theoretical motivation on the likely impact of the policy on gender bias; Section 3 describes the data and presents descriptive statistics; Section 4 presents the empirical strategy; Section 5 presents the results along as well as a discussion of potential mechanisms; and Section 6 concludes the discussion.

2 Background to the programme

The context for this study is India, where sex-ratios have long been male-biased. Census data from 2011 put the sex ratio at 943 females per 1000 males, with considerable variation across states from 877 females per 1000 males in Haryana to 1084 females per 1000 males in Kerala. A major cause of the male-biased sex ratio has been the widespread use of ultrasound technology since the 1980s (Bhalotra and Cochrane, 2010), which is used to determine the sex of the foetus, followed by the selective abortion of female foetuses. The national and state governments responded to the increasingly skewed sex ratio at birth by banning sex-selective abortions and placing restrictions on access to ultrasounds through a series of legislations passed between 1989 and 2002.

These bans were found to be effective in increasing female births (Nandi and Deolalikar 2015), but they have also led to worsening gender gaps in human capital outcomes due to relatively reduced investments in girls compared to boys. Lower investments were the outcome of outright discrimination, as in the case of lower educational investments in unwanted daughters leading to widening gender gaps in educational outcomes (Rastogi and Sharma, 2022). They also resulted from indirect discrimination such as increasing family size as families, in the absence of access to abortion, begin to rely on the fertility stopping rule to achieve a desired number of sons (Dasgupta and Sharma, 2021). Girls were disproportionately born into larger families after the bans on abortions were enacted, and faced increased sibling competition for resources.

In 2015, a mass media campaign called the *Beti Bachao Beti Padhao* (BBBP) programme, or the "Save daughters, educate daughters" programme, was launched in some districts in India to improve the male-biased sex ratio and reduce gender inequality. More specifically, the programmes's goals were to: improve the sex ratio in selected districts by 2 percentage points every year, reduce the gender differential in under-5 mortality rates from 7 percentage points to 1.5 percentage points, improve female nutrition by reducing the number of anaemic and underweight girls, and increase the enrollment of girls in secondary education to 82 percent (Ministry of Women and Child Development, 2019).[1] The mass communication campaign involved spreading awareness and disseminating information through radio jingles in Hindi and regional languages, televised messages, community engagement through mobile exhibition vans, social media and field publicity using handouts, brochures, text messages on mobile phones in English, Hindi and regional languages. Some other measures in the context of health included improvements in the prenatal and postnatal care of mothers, and the provision of counselling to ensure the equitable care

[1]Document retrieved from https://wcd.nic.in/sites/default/files/Guideline_5.pdf

of female infants, as well as the training of front-line health workers to make them more sensitive to these concerns. [2]

Ensuring the effective implementation of the ban on sex detection and sex-selective abortions was also a part of the BBBP programme. Local officials were made to monitor the sex ratio at birth and register all births through the Civil Registration System. All pregnancies were to be registered along with the provision of antenatal care (ANC) and postnatal services. All genetic laboratories and clinics conducting any preconception and prenatal diagnostic counselling or tests were to be registered and a database of complaints about violations of the ban was to be maintained. Sting operations were conducted to unearth the illegal practice of sex selection (Ministry of Women and Child Development, 2019).

As it focused on both a more stringent implementation of the ban on prenatal sex selection as well as encouraging changes in social norms through advocacy and media campaigns, the programme has both supply-side and demand-side elements to address gender discrimination. This provides a unique setting to examine the efficacy of legal bans when coupled with demand-side interventions that can change the underlying son preference that drives gender discrimination. While the supply-side elements of the programme would be anticipated to lead to more female births, the demand-side elements of the programme could mitigate the adverse effects of discrimination against "unwanted" girls, either by directly increasing investments in girls, or, indirectly, by reducing fertility as families are encouraged to be satisfied with the birth of a daughter and not to persist in trying for a desired number of sons.

The programme was initially launched in 100 districts in 2015 (Phase 1), and was expanded to 61 additional districts in 2016 (Phase 2). It was expanded to the remaining 468 districts in the rest of the country in 2018 (Phase 3). Pilot districts were selected on the basis of their child sex-ratios (CSR or ratio of female to male children aged between 0 and 6 years) according to the 2011 Census.[3] By 2018, the entire country was covered by the programme. The budgetary allocation of funds from 2015-2018 was in excess of Rs 11 billion (GoI, 2019). In a short period of time the BBBP programme has become very well

[2]State governments and district-level officials were also asked to improve data collection of birth registrations and the district-level sex ratio at birth through the existing network of health workers and local government structures. On the educational front, measures include universal enrollment of females in school and construction of toilets specifically for the use of females, as well as the integration of gender-related awareness in the educational curriculum, and gender-sensitisation training of police and judicial personnel.

[3]For the first phase, 100 districts were selected from all states in the following way: 87 districts from 23 states which had a CSR below the national average of 918, eight from eight states with a CSR above the national average of 918 but showing a declining trend, and five from five states with a CSR above the national average, showing an improving trend `https://wcd.nic.in/sites/default/files/Guideline_5.pdf`.

known: a recent survey of 14 states finds nearly 88 per cent of respondents were aware of the programme (Sinha et al., 2020).

So far there has been very limited research on the impact of the programme. Gupta et al. (2018) examine the short-run impacts of the program in Haryana but they are only able to compare outcomes from before and after the implementation of the programme. They find a significant improvement in the sex ratio at birth in favour of females when analysing data from 2005-2016 for the state of Haryana. We use the staggered timing in the roll out of the program across districts to estimate if the relative mortality and health investments for girls improve in districts exposed to the programme, compared to girls in untreated districts. Additionally, we exploit the quasi-exogenous assignment of the gender of the firstborn child to estimate the impact of the programme on the gender gap in relatively intensively treated families – those with firstborn females – compared to less intensively treated families. To our knowledge, these are the first causal estimates of the impact of the programme on child health outcomes.

3 Data

To examine the impacts of the BBBP programme, we pool retrospective birth data from two rounds of the National Family Health Survey (NFHS), a national household survey, conducted in 2015-16 and 2019-2020. For fertility outcomes, we use pooled data from retrospective birth histories of all women aged between 15-49 years to construct a dataset of all births that take place in a ten year period between 2011-2020. This dataset includes over 3.2 million mother-year observations on almost 450,000 unique women. For child health and mortality outcomes, we organise the data at the level of the child and our data includes approximately 640,00 child observations. In addition, we consider anthropometric outcomes of approximately 390,000 children as well as health investments such as vaccinations of 200,000-300,000 children.

The data also includes a rich set of mother and household characteristics including mother's age, mother's age at childbirth, whether the mother has completed primary education, total children ever born to the mother, religion, caste, whether the household is located in an urban area, household wealth index and total number of members in the household.

We present the sample variation in the exposure to the BBBP programme by the birth year of the child in Table 1. In Table 2, we present the summary statistics of the key outcome and control variables in the sample by exposure to the programme. Finally, we also show the distribution of the sample across the two rounds of the NFHS in Table A1.

4 Empirical Strategy

4.1 Impact on female births

We first examine the impact of the BBBP programme on the proportion of female births. The dataset we use is a full fertility history of approximately 446,000 mothers through the period 2011-2020. The observations are defined at the mother-birthyear level, and include all births to all mothers during this ten year period. Women who had no births during this period are excluded from the analysis. We estimate the following equation:

$$\begin{aligned} Y_{bmdy} &= \beta_0 + \beta_1 \text{FirstbornFemale}_m + \beta_2 (\text{Treat} \times \text{FirstbornFemale})_{mdy} \\ &+ \eta \text{X}_m + \delta_{dy} + \lambda_b + \epsilon_{bmdy} \end{aligned} \tag{1}$$

where Y_{bmdy} is, as of any given year y, the current proportion of female births out of all births for a mother m from district d, and at parity b. $Treat_y$ takes the value one for all years after the programme has been implemented in that district and zero otherwise. *Firstborn Female*$_m$ is defined at the mother level, taking the value one if mother m has a firstborn female child and zero otherwise. We include birth order fixed effects (λ_b) and district-year fixed effects (δ_{dy}). Further, the estimation includes X_m, a vector of socioeconomic and demographic characteristics comprising mother's age, whether the mother completed primary education, mother's religion, mother's caste, whether the house is located in an urban area, number of members in the household, household wealth index, and sex composition of adults in the household. We also include a survey indicator for whether the mother was surveyed in the fifth round of the NFHS. Standard errors are clustered by district.

The main coefficient of interest, β_2, estimates the change in the proportion of female children out of all children for a mother that we observe in any given year after exposure to the BBBP programme, in firstborn female families compared to firstborn male families. The results of this estimation are presented in Table 3. Our first finding is that mothers in treated districts were more likely to have female children at birth orders of greater than one (column 4), while there is no significant effect on the probability of a female being born as the firstborn child (column 3). This supports the existing evidence that sex-selective abortions primarily take place at a birth order of higher than one and not for the firstborn child, and so, restricting access to sex-selective abortions leads to an increase in female births at higher birth orders. We find that this effect is driven by increases in the proportion of female births among firstborn female families, both in a sample of children born at all birth orders and those born at birth orders of greater than 1 (columns 2 and 5). In both samples, the estimated increase in the proportion of female births is significantly different from zero at the 1% level.

We also consider the proportion of female children ever born to a woman as of the year of her most recent birth (columns 6 and 7). Here, we compare the proportion of female children born to women whose most recent birth was after exposure to the programme to women whose most recent birth was before exposure to the programme. In these regressions we control not for parity fixed effects but for the total number of children born to a woman. Again, we find an increase in the proportion of female children born into treated firstborn female families compared to firstborn male families, which is significant at the 1% level.

In an alternative specification, we use a binary variable for whether child i born to mother m in year y is female as the dependent variable in the above specification. The coefficient of interest on the interaction term between $Treat$ and $FirstbornFemale$ now captures the increased probability that a birth in a given year is female in treated firstborn female families relative to treated firstborn male families. The results are presented in columns 8 and 9 of Table 3. As above, we find that the probability of a birth being female relatively increases among firstborn female families by 1.6 percentage points after exposure to treatment compared to firstborn male families. This difference is also significant at the 1% level.

All our results on female births provide evidence that exposure to treatment had the greatest impact among families that were more likely to favour sons for subsequent births – those families which had a firstborn female child. These families would have been more likely to resort to sex-selective abortions in the absence of the programme. However, exposure to the programme reduces their access to illegal ultrasounds and abortions, while also potentially shifting the social norms that drive their preferences for the birth of sons, leading to lower demand for sons. In section 5.3 we discuss the comparative importance of supply-side and demand-side incentives in increasing female births.

4.2 Impact on the gender gap in mortality

Having established that the programme did lead to a rise in female births, particularly among firstborn female families, we next test whether the programme led to a relative change in the gender gap in mortality and health investments in intensively treated families. As discussed previously, increased births of unwanted daughters could lead to increased direct or indirect discrimination against them, particularly if families discriminate against unwanted girls or if girls are disproportionately born into larger families as their parents look to have more sons. On the other hand, the media intervention could shift social norms and lead to reduced discrimination against girls. We use the full fertility history of all surveyed women in the fourth and fifth rounds of the NFHS described above to create a child-level dataset on over 660,000 births that took place between 2011 and 2020. We have already shown in the previous section that the programme had no effect on the sex of the firstborn child, so we are able to leverage quasi-exogenous variation in the sex of the firstborn child to identify the effect on the gender gap in health outcomes. We estimate the following triple difference specification:

$$\begin{aligned} Y_{ibmdty} &= \beta_0 + \beta_1(\text{Treat} \times \text{FirstbornFemale} \times \text{Female})_{ibmdty} \\ &+ \beta_2(\text{Treat} \times \text{FirstbornFemale})_{mty} + \beta_3(\text{Treat} \times \text{Female})_{ibmdty} \\ &+ \beta_4(\text{FirstbornFemale} \times \text{Female})_{ibmdty} + \beta_5\text{Female}_{ibmdty} \\ &+ \beta_6\text{FirstbornFemale} + \eta\text{X}_{my} + \delta_{dy} + \tau_t + \rho_b + \epsilon_{ibmdty} \end{aligned} \tag{2}$$

where Y_{ibmdty} captures a range of mortality and health investment outcomes for child i of birth order b born to mother m in district d in month t and year y. *Treat*$_{dty}$ takes the value one if the child is born after the programme was implemented in the district and zero otherwise. *Female*$_{ibmdty}$ takes the value one if the child's sex is female and zero otherwise. *Firstborn Female*$_m$ is defined at the mother level, taking the value one if mother m of child i has a firstborn female child and zero otherwise. We include the triple interaction of these three variables as well as all pairwise interactions between them. We include district-birth year fixed effects (δ_{dy}), birth month fixed effects (τ_t), and birth order fixed effects (ρ_b). Further, the estimation includes X_{my}, a vector of socioeconomic and demographic characteristics comprising mother's age at birth, mother's age at the time of the survey, whether the mother completed primary education, mother's weight for height, mother's religion, mother's caste, whether the house is located in an urban area, number of members in the household, household wealth index and sex composition of adults in the household. We also include a survey indicator for whether the mother was surveyed in the fifth round of the NFHS. Standard errors are clustered at the district level.

β_1, the coefficient on the interaction of triple interaction term between *Treat*, *Firstborn Female* and *Female*, is our coefficient of interest and captures whether the gender gap in health outcomes of children born into firstborn female families are differentially affected by the programme compared to the gender gap in health outcomes of children born into firstborn male families. Since the $Treat$ variable varies both over time and across districts, this coefficient is identified through four sources of variation: spatial and intertemporal variation in exposure to treatment, quasi-exogenous variation in the sex of the firstborn child, and the sex of the child. In addition to including district-year fixed effects, we additionally allow district and year fixed effects to vary by firstborn female family and by whether the child is female, allowing for a very flexible specification that can control for a number of confounding variables.

As discussed earlier, districts were selected for the programme based on their pre-programme child sex ratio as calculated from 2011 Census data. We control for this variation through the use of district-year fixed effects which capture level differences in the districts assigned to different treatment phases.

We use two measures of child mortality: neonatal mortality (if a child died before completing 1 month) and infant mortality (if the child died before completing 1 year). We are not able to consider under-five mortality (if a child died before completing 5 years) because most children have not been fully exposed to five years of the programme. In addition to mortality, we also consider health outcomes, including (i) indicators for health investments that could affect mortality and other health outcomes such as ante-natal care (ANC) visits, whether a mother has received tetanus shots while pregnant and breastfeeding duration, and ii) the vaccine status of the children for a number of important vaccines typically received up to six weeks after birth, including hepatitis B, DPT, pentavalent, polio, and rotavirus vaccines.A detailed note on variable definitions can be found in subsection 1.1.

5 Results

5.1 Impact of the BBBP programme on child mortality and health investments

The results of the impact of the BBBP intervention on mortality are presented in Table 4. The estimated coefficient on the triple interaction term between *Treat*, *Firstborn Female* and *Female* is negative and significantly different from zero at the 5% level in the case of neonatal mortality. The coefficient is also negative and also of a very similar size for infant mortality, though this is not significantly different from zero. This indicates that the gender gap in child mortality, particularly at the neonatal level, declined in families that were most intensively treated by the programme.

We disaggregate these results by the sex of the child to show the estimated coefficients on $Treat$ and $FirstbornFemale$ for the entire sample, and for boys and girls separately (Table A2). Exposure to the treatment does not affect the relative mortality among children in firstborn female families relative to firstborn male families. However, these results mask movements in opposite directions for boys and girls. Girls in firstborn female families see a decline in their mortality, relative to firstborn male families, and this difference is statistically significant. On the other hand, the coefficient on the double interaction term is positive for boys, even though it is insignificantly different from zero. This provides evidence that our main results are being driven by relative declines in female mortality, compared to male mortality, in intensively treated families.

This is a striking set of results, particularly when compared to the estimated impact of supply-side measures such as restrictions on abortion, which we discuss in more detail in Section 6. The key distinction between the BBBP programme and abortion restrictions is the prominent mass media campaign that was aimed at shifting social norms around the perceived desirability of daughters. As a result, treated families are both more likely to have more daughters and more likely to treat them better as well.

We next present results on the gender gap in health investments in children made by families in Table 5 and Table 6. We first consider the impact of the programme on prenatal investments such as antenatal visits by a care-worker and number of tetanus shots a mother received while she was pregnant. We also consider several postnatal investments such as months of breastfeeding and whether a child received a routine vaccination within six weeks after birth. Among antenatal investments, we would expect to see a narrowing of the gender gap in investments in intensively treated families either because families are now unable to identify the sex of the foetus (a supply-side mechanism), or because their desire to investment in girls who may be born is now higher (a demand-side mechanism). Among prenatal investments, we find that mothers of girls are 6.7 percentage points more likely to receive a tetanus shot while pregnant than mothers of boys, when comparing the gender gap among treated firstborn female and treated firstborn male families. This difference is statistically significant at the 1% level. There is, however, no difference in the number of antenatal visits received by a government healthcare worker during pregnancy.

Among postnatal investments, we find a significant narrowing of the gender gap across a range of outcomes. We focus on months of breastfeeding as well as vaccine status for vaccines due within six weeks after birth. We find that the gender gap in months of breastfeeding narrows by 1.48 months, a result significant at the 1% level. This change is substantial, representing 10% of the average number of months for which a child is breastfed. We also find significant declines in the gender gap in vaccine status for DPT, pentavalent, and polio vaccines, and a marginally significant decline in the gender gap in the uptake of the hepatitis B vaccine. In all four of these vaccines, there are significant gaps in uptake by gender across the sample, with boys being more likely to receive the vaccine on average. As a result of the programme, girls are now 1.5-3.2 percentage points more likely to receive the shot than boys in intensively treated families living in treated districts. We focus on vaccines due at the six week mark since the uptake of these vaccines is driven by health-seeking behaviour by parents, unlike vaccines given at birth in any institutional facility, such as BCG and hepatitis and polio vaccines administered at birth.

Could these improved outcomes for girls be driven by improved government services targeted specifically at girls under the BBBP programme rather than on account of changes in parental behaviour? The BBBP programme does include a number of health-related interventions targeted at the prenatal and postnatal health of both mother and baby but there is no reason these interventions should be targeted differently at children in firstborn female compared to children in firstborn male families. Moreover, we do not observe any differences in the number of ANC visits received by a mother while she is pregnant.

In sum, in stark contrast to results that find a worsening of female child mortality health outcomes as a result of supply-side measures such as bans on prenatal sex-selection, a policy with demand-side elements is able to mitigate and outright reverse some of these adverse consequences. We observe lower relative mortality outcomes for girls, potentially driven by increased parental investments such as in breastfeeding and in vaccinations. These indicate significant benefits from a policy that is aimed at increasing gender equity through interventions aimed at shifting social norms.

5.2 Pre-intervention trends and results by phase

Though our results are robust to the inclusion of a wide range of fixed effects that control for potentially confounding changes over the same period, we may still be concerned that our results are being driven by pre-programme trends. We test for the presence of pre-intervention trends by restricting our analysis to children born before their districts were exposed to the programme. We interact the variables $FirstbornFemale$ and $Female$ with an indicator for each of five years preceding treatment and regress our mortality outcomes on these interactions and the full set of control variables from the main estimation equation. The results of this estimation are in Table A3. All the coefficients on the lagged interaction terms are insignificantly different from 0, providing some assurance that our estimates are not biased.

We also estimate the impact of the programme separately for districts that are part of the same phase. Since our main estimating strategy relies on the staggered implementation of a policy measure, our results may be biased because of heterogeneous treatment effects across districts that received the treatment at different points in time (Goodman-Bacon, 2021; Borusyak et al., 2021; Sun and Abraham, 2021; Callaway and Sant'Anna, 2021; de Chaisemartin and D'Haultfœuille, 2020). To verify robustness, we estimate the mortality results separately by each of the three phases; that is, we estimate the impact of treatment on the gender gap in mortality among firstborn female families relative to firstborn male families separately for Phase 1, Phase 2 and Phase 3 districts. In doing so, we eliminate any spatial variation arising from differential timing of the treatment across districts and rely only on variation over time and by sex of the firstborn child. These results are presented in Table A4. We find that coefficients are very similar across all the three phases and across the entire sample, though they are now significantly different from 0 only for the largest sample of Phase 3 districts. Due to the demanding nature of our specification, our coefficients are imprecisely measured for the two smaller samples of Phase 1 and Phase 2 districts. However, the stability of the size of the coefficients suggests that heterogeneous treatment effects across districts treated earlier and later are not driving the main results.

5.3 Impact on fertility and son preference

One mechanism that drives differential impacts on gender is through fertility. Prior research has found that in response to increasing and decreasing access to the tools of prenatal sex selection, families decrease and increase their use of the fertility stopping rule, respectively, in order to achieve their desired number of sons. With wider access to ultrasound, fertility declined as parents were able to reach their desired number of sons in fewer births (Anukriti et al., 2021). On the other hand, as prenatal sex selection and access to ultrasound was banned, fertility increased as parents kept on having children until they had a desired number of sons (Dasgupta and Sharma, 2021). In both cases, the change in fertility was concentrated in firstborn female families, which were intensively affected by changes in access to prenatal sex selection. Changes in sibling size could disproportionately affect girls, since they are born into either larger or smaller families, with potential effects on their access to parental resources. Changing fertility can also affect boys who are now more likely to be born at higher birth orders and may be particularly vulnerable to the adverse health effects of the increase in birth order.

To investigate the effects of BBBP programme on fertility, we use a similar estimation framework as equation (1). Specifically, we test if fertility increases relatively more in firstborn female families as compared to firstborn male families after the implementation of the policy. We use a mother-year level dataset with approximately 3.2 million mother-year observations on all mothers who had at least one child during the period 2011-20. Mothers enter the panel in 2011 or, if they are below the age of 11 years in 2011, once they turn 11. We run the following estimation:

$$\begin{aligned} Y_{mdy} &= \beta_0 + \beta_1 \text{Treat}_{mdy} + \beta_2 \text{FirstbornFemale}_{mdy} + \\ &+ \beta_3 (\text{Treat} \times \text{FirstbornFemale})_{mdy} + \beta_4 X_{mdy} + \delta_{dy} + \epsilon_{mdy} \end{aligned} \quad (3)$$

where Y_{mdy} is an indicator for whether mother m in district d gives birth in year y. We include district-birth year fixed effects (δ_{dy}) and a set of controls X_{mdy} that include mother's age, whether the mother completed primary education, mother's religion, mother's caste, whether the house is located in an urban area, number of members in the household, household wealth index, and sex composition of adults in the household. We also include a survey indicator for whether the mother was surveyed in the fifth round of the NFHS. Standard errors are clustered by district. The main coefficient of interest is β_3, the coefficient on the interaction between an indicator for a firstborn female family and the treatment indicator.

The results are presented in Table 7. We find that fertility increased among women after exposure to the programme: women were 0.4 percentage points more likely to give birth after exposure to the programme (Column 1). These effects are concentrated among firstborn female families, where a child is 0.5 percentage points more likely to be born in any given year after exposure to the programme compared to firstborn male families. This estimate is significant at the 1% level. In other words, the programme leads to an increase in the likelihood of female births, as well as an increase in the probability of any birth. This suggests that firstborn female families are continuing to use the fertility stopping rule to achieve a desired numbers of sons.

We next look directly at whether the programme shifted stated son preference. Surveyed women report their ideal number of sons and daughters, and ideal number of children. We divide the ideal number of sons by the ideal number of total children to construct an ideal fraction of sons variable, which we use as a measure of son preference. Since the ideal number of sons and children is reported only once by every surveyed woman at the time she is surveyed, we do not have any spatial variation in exposure to the programme; most women in the fourth round are surveyed just as the programme was launched and all women in the fifth round are surveyed after their district has received the programme. However, we can estimate trends in son preference between the fourth and fifth rounds of the NFHS, as well as how these trends vary in firstborn female families compared to firstborn male families. These results are presented in Table 8. We find no evidence of any significant difference in son preference across survey rounds, even when we restrict our analysis to those women who had a child within a year of being surveyed. This confirms that son preference itself has not been changed by the programme, and places in context our finding that fertility continues to increase among intensively treated families as they attempt to achieve a desired number of sons.

This suggests that the media intervention may not have been entirely successful in shifting social norms of son preference. However, the adverse welfare consequences of a pure supply-side measure such as a ban on sex-selective abortions are mitigated. We observe an increase in all births, and in female births, among intensively treated families. However, we do not observe a worsening in health outcomes of girls relative to boys among the same families. Presumably, any negative effects from the increased competition for sibling resources that are disproportionately faced by girls are cancelled out or even reversed by the benefits from increased care and investments in female child health.

6 Discussion

In this paper, we have examined the impact of a large-scale intervention to tackle the gender discrimination emerging from son preference that focuses both on supply-side measures that reduce access to sex-selective abortions, as well as a mass media intervention that seeks to shift social norms and increase the demand for girls. First, we find that the policy does lead to an increase in female births, particularly in firstborn female families which are relatively intensively treated by the programme. Second, we find that despite the rise in births, there is no quantity-quality tradeoff between increased births and worsened health outcomes for girls. Families do not discriminate against unwanted daughters, and, in fact, reallocate spending towards their daughters to narrow the gender gap in mortality. The main mechanisms leading to decreased mortality of girls is increased parental investments in early health outcomes, including lengthened breastfeeding and increased take-up of vaccinations. Finally, we show that stated son preference does not change and fertility does rise in firstborn female families exposed to the treatment. In short, while the programme has been successful in shifting perceptions about the value of daughters and the need to invest in their health, it has not yet successfully shifted the norm of son preference.

Even so, these results are a striking contrast to the impact of supply-side restrictions on access to prenatal sex selection. In a related paper, we find that bans on sex-selective abortions led to a 25% increase in neonatal mortality among all children born in firstborn female families, driven by a sharp increase in fertility. Moreover, the bans on ultrasound and prenatal sex selection were associated with a rise in gender inequality in health outcomes

(Dasgupta and Sharma, 2021). The contrast in impacts of a ban on sex-selection and the BBBP programme emphasises the importance of demand-side elements to policies that seek to eliminate gender discrimination, rather than simply focus on top-down approaches that address some tools of discrimination without addressing the underlying causes.

In fact, our results on the gender gap in mortality have more in common with previous results on the *increased* access to the tools of prenatal sex selection. In their study of the reverse process of increased access to ultrasound, Anukriti et al. (2021) find that the gender gap in mortality outcomes among firstborn female families decreases by 1.5 percentage points after wider access to ultrasound. Our finding is in the same direction, though smaller than theirs: we find a decline of 0.7 percentage points in the gender gap in mortality outcomes among firstborn female families. Crucially, however, the BBBP programme is accompanied by *rising* female births, while the gains in the post-ultrasound era come at the cost of a large number of missing women.

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Table 1: Summary of BBBP treatment by year

	BBBP Treatment					
	Phase 1		Phase 2		Phase 3	
	0	1	0	1	0	1
2011	24.06	0.00	20.07	0.00	16.85	0.00
2012	24.88	0.00	20.43	0.00	17.63	0.00
2013	24.74	0.00	20.75	0.00	17.11	0.00
2014	24.22	0.00	19.76	0.00	17.03	0.00
2015	2.10	28.70	17.65	0.00	13.50	0.00
2016	0.00	19.74	1.35	25.51	9.15	0.00
2017	0.00	15.79	0.00	22.90	7.54	0.00
2018	0.00	15.68	0.00	22.39	1.18	41.19
2019	0.00	14.14	0.00	19.82	0.00	42.25
2020	0.00	5.95	0.00	9.38	0.00	16.56

Note: *BBBP Treatment* takes the value 1 if BBBP has been implemented in a mother's district in a particular year, 0 otherwise. The sample is restricted to children born between 2011 and 2020. The first two columns relate to children born in Phase 1 districts, the second two columns relates to children born in Phase 2 districts and the last two columns relate to children born in Phase 3 districts.

Table 2: Summary of Key Variables

	Full Sample	Phase 1			Phase 2			Phase 3		
	(1) Full sample	(2) Pre	(3) Post	(4) Difference	(5) Pre	(6) Post	(7) Difference	(8) Pre	(9) Post	(10) Difference
Neonatal Mortality	0.0265	0.0245	0.0245	0.0007	0.0268	0.0268	0.0019	0.0257	0.0257	0.0011*
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Infant Mortality	0.0420	0.0466	0.0466	-0.0080***	0.0470	0.0470	-0.0040**	0.0610	0.0610	-0.0209***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Female (=1)	0.4798	0.4729	0.4729	-0.0037	0.4802	0.4802	-0.0121***	0.4831	0.4831	-0.0006
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Firstborn female family (=1)	0.5187	0.5285	0.5285	-0.0015	0.5241	0.5241	-0.0090**	0.5213	0.5213	-0.0047**
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Child birth order	2.2259	2.0749	2.0749	0.0733***	2.0985	2.0985	0.1073***	2.1438	2.1438	0.1256***
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Mother's age	28.5792	27.2038	27.2038	2.1645***	27.4415	27.4415	1.8816***	26.0343	26.0343	2.9201***
	(0.01)	(0.02)	(0.02)	(0.03)	(0.04)	(0.04)	(0.05)	(0.02)	(0.02)	(0.02)
Mother completed primary education	0.7179	0.8266	0.8266	-0.0670***	0.8120	0.8120	-0.0867***	0.7857	0.7857	-0.0980***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Mother's age at birth	25.0326	25.2285	25.2285	-0.5222***	25.6826	25.6826	-0.6152***	25.3111	25.3111	-0.3235***
	(0.01)	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)	(0.04)	(0.02)	(0.02)	(0.02)
Mother's weight for height	119.3184	122.4331	122.4331	0.3870**	122.5292	122.5292	-0.2612	117.3438	117.3438	1.1073***
	(0.03)	(0.12)	(0.12)	(0.15)	(0.18)	(0.18)	(0.21)	(0.08)	(0.08)	(0.09)
Household head Hindu (=1)	0.7273	0.7105	0.7105	-0.0048*	0.6954	0.6954	-0.0285***	0.7670	0.7670	-0.0340***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Household head Scheduled Caste (=1)	0.4255	0.3810	0.3810	-0.0120***	0.3195	0.3195	-0.0122***	0.4540	0.4540	-0.0054***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Whether household is urban	0.4939	0.6363	0.6363	-0.0905***	0.6647	0.6647	-0.0953***	0.6131	0.6131	-0.1714***
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Number of household members	6.2758	6.5763	6.5763	-0.1123***	6.5856	6.5856	0.0283	6.2575	6.2575	-0.0775***
	(0.00)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)
Wealth index (1-5)	2.6391	3.4457	3.4457	-0.1046***	3.3583	3.3583	-0.1084***	2.4382	2.4382	-0.0249***
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)	(0.01)
N	700847	41678	41678	103539	17772	17772	64746	70549	70549	532562

Note: Neonatal Mortality (*NNM*) takes the value 1 if a child died before completing 1 month, 0 otherwise. Infant Mortality (*IMR*) takes the value 1 if a child died before completing 1 year, 0 otherwise. The sample is restricted to children born between 2011 and 2020.
Standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3: Impact of BBBP on female births

	Proportion of Female Children					Sex Ratio		Probability of Female Birth	
	(1) All Birth Orders	(2) All Birth Orders	(3) Birth Order=1	(4) Birth Order>=2	(5) Birth Order>=2	(6) Last Birth Year	(7) Last Birth Year	(8) Female	(9) Female
Treat X Firstborn Female		0.01806***			0.01478***		0.07956***		0.01646***
		(0.00283)			(0.00274)		(0.00375)		(0.00445)
Treat	0.00428		-0.00187	0.00776**		0.01178**		0.00505	
	(0.00385)		(0.00858)	(0.00379)		(0.00543)		(0.00520)	
Firstborn Female		0.63360***			0.39751***		0.55176***		0.37238***
		(0.00413)			(0.00267)		(0.00508)		(0.00476)
Observations	648081	648076	245388	402693	402682	422716	422711	648081	648076
Mean of Dep. Variable	0.5064	0.5064	0.4826	0.5216	0.5216	0.4853	0.4853	0.4783	0.4783
SD	0.398	0.398	0.500	0.315	0.315	0.377	0.377	0.500	0.500

Standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: *Proportion of female children* is the ratio of the total number of female children to the total number of children born to a mother. Columns 1 and 2 include all births during 2011-2020. Column 3 includes all first order births. Columns 4 and 5 include all higher-order births during 2011-2020. Columns 6 and 7 estimates the *sex ratio* of children ever born to a mother as measured in the year of her last birth. *Female* takes the value 1 if a child is female, 0 otherwise. *Treat* takes the value 1 if BBBP has been implemented in a mother's district in a particular year, 0 otherwise. *Firstborn Female* takes the value 1 if firstborn child is female, 0 otherwise. All estimations control for mother's age in a particular year, whether the mother has completed primary education, religion, caste, whether the house is located in an urban area, number of members in the household, household wealth index and sex composition of adults in the household. Additionally columns 2, 5, 7 and 9 include district fixed effects, year fixed effects, district-year fixed effects and parity fixed effects while columns 1, 3, 4, 6 and 8 include district fixed effects, year fixed effects and parity fixed effects. Columns 6 and 7 do not include parity fixed effects, but instead include the total number of children born. The sample is restricted to women who gave birth to at least one child between 2011 and 2020. Standard errors are clustered at the district level.

Table 4: Impact of BBBP on gender gap in child mortality by firstborn female family

	Model 1		Model 2	
	(1) NNM	(2) IMR	(3) NNM	(4) IMR
Treat x Female x Firstborn Female	-0.00708**	-0.00697	-0.00691**	-0.00662
	(0.00279)	(0.00428)	(0.00280)	(0.00430)
Treat x Female	0.00496	0.00570	0.00511**	0.00538
	(0.00315)	(0.00462)	(0.00208)	(0.00335)
Treat x Firstborn Female	-0.00280	-0.00392	0.00272	0.00044
	(0.00341)	(0.00446)	(0.00196)	(0.00290)
Observations	666030	586235	666030	586235
Mean of Dep. Variable	0.0272	0.0420	0.0272	0.0420
SD	0.163	0.201	0.163	0.201

Standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Neonatal Mortality (*NNM*) takes the value 1 if a child died before completing 1 month, 0 otherwise. Infant Mortality (*IMR*) takes the value 1 if a child died before completing 1 year, 0 otherwise. Children who have not attained these respective ages are excluded from the regression. *Treat* takes the value 1 if BBBP has been implemented in a mother's district in a particular year, 0 otherwise. *Female* takes the value 1 if the child's sex is female, 0 otherwise. *Firstborn Female* takes the value 1 if firstborn child is female, 0 otherwise. All estimations control for mother's age, mother's age at birth, whether the mother has completed primary education, religion, caste, whether the house is located in an urban area, number of members in the household, household wealth index and sex composition of adults in the household. Model 1 includes birth order fixed effects, district fixed effects, birth year fixed effects, birth month fixed effects, and district-year fixed effects. Model 2 additionally firstborn female- district fixed effects, female-district fixed effects, firstborn female- birth year fixed effects, female- birth year fixed effects and firstborn female- female fixed effects. The sample is restricted to children born between 2011 and 2020. Standard errors are clustered at the district level.

Table 5: Impact of BBBP on gender gap in child health investments by firstborn female family

	(1) ANC	(2) Duration Breastfed	(3) TET
Treat x Female x Firstborn Female	-0.16534	1.39876***	0.06705***
	(0.15434)	(0.18031)	(0.01570)
Treat x Female	0.22644*	-0.55090**	-0.03220
	(0.13083)	(0.26733)	(0.01985)
Treat x Firstborn Female	-0.11440	-1.05254***	-0.04160**
	(0.13993)	(0.26801)	(0.01668)
Observations	666030	353605	336271
Mean of Dep. Variable	12.2653	15.2376	1.9256
SD	8.131	12.514	0.743

Standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: *ANC* refers to the number of antenatal care visits that took place in utero, top-coded at 20. *TET* refers to the number of tetanus injections the mother received before birth. *Breastfeeding Duration* refers to the duration of breastfeeding in months. *Treat* takes the value 1 if BBBP has been implemented in a mother's district in a particular year, 0 otherwise. *Female* takes the value 1 if the child's sex is female, 0 otherwise. *Firstborn Female* takes the value 1 if firstborn child is female, 0 otherwise. All estimations control for mother's age, mother's age at birth, whether the mother has completed primary education, religion, caste, whether the house is located in an urban area, number of members in the household, household wealth index and sex composition of adults in the household and additionally includes birth order fixed effects, district fixed effects, birth year fixed effects, birth month fixed effects, district-year fixed effects, firstborn female- district fixed effects, female-district fixed effects, firstborn female- birth year fixed effects, female- birth year fixed effects and firstborn female- female fixed effects. The sample is restricted to children born between 2011 and 2020. Standard errors are clustered at the district level.

Table 6: Impact of BBBP on gender gap in child vaccination outcomes by firstborn female family

	(1) DPT 1	(2) Pentavalent 1	(3) Polio 1	(4) Hepatitis 1	(5) Rotavirus 1
Treat x Female x Firstborn Female	0.01545*	0.03219**	0.01537*	0.01457	0.01596
	(0.00821)	(0.01453)	(0.00912)	(0.00888)	(0.01526)
Treat x Female	0.00104	-0.02549	-0.00870	-0.00065	-0.02694
	(0.01868)	(0.02448)	(0.02196)	(0.01866)	(0.02177)
Treat x Firstborn Female	-0.00652	-0.02330	0.00033	-0.01451	0.00773
	(0.01465)	(0.01809)	(0.01311)	(0.01491)	(0.01441)
Observations	343130	121944	344143	340506	121455
Mean of Dep. Variable	0.8612	0.8327	0.8622	0.7974	0.4109
SD	0.346	0.373	0.345	0.402	0.492

Standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note:*DPT 1* takes the value 1 if the child had received the first dose of DPT vaccine, 0 otherwise. *Pentavalent 1* takes the value 1 if the child had received the first dose of Pentavalent vaccine, 0 otherwise. *Polio 1* takes the value 1 if the child had received the first dose of oral Polio vaccine, 0 otherwise. *Hepatitis 1* takes the value 1 if the child had received the first dose of Hepatitis B vaccine, 0 otherwise. *Rotavirus 1* takes the value 1 if the child had received the first dose of Rotavirus vaccine, 0 otherwise. *Treat* takes the value 1 if BBBP has been implemented in a mother's district in a particular year, 0 otherwise. *Female* takes the value 1 if the child's sex is female, 0 otherwise. *Firstborn Female* takes the value 1 if firstborn child is female, 0 otherwise. All estimations control for mother's age, mother's age at birth, whether the mother has completed primary education, religion, caste, whether the house is located in an urban area, number of members in the household, household wealth index and sex composition of adults in the household and additionally includes birth order fixed effects, district fixed effects, birth year fixed effects, birth month fixed effects, district-year fixed effects, firstborn female- district fixed effects, female-district fixed effects, firstborn female- birth year fixed effects, female- birth year fixed effects and firstborn female-female fixed effects. The sample is restricted to children born between 2011 and 2020. Standard errors are clustered at the district level.

Table 7: Impact of BBBP on fertility

	(1) Any Birth	(2) Any Birth
Treat X Firstborn Female		0.00454***
		(0.00120)
Treat	0.00416*	
	(0.00237)	
Firstborn Female		0.01820***
		(0.00075)
Observations	3331250	3331250
Mean of Dep. Variable	0.1940	0.1940
SD	0.395	0.395

Standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: *Any birth* takes the value 1 if a mother had a birth in a given year. Columns 1 and 2 include all mother-years during 2011-2020. *Treat* takes the value 1 if BBBP has been implemented in a mother's district in a particular year, 0 otherwise. *Firstborn Female* takes the value 1 if firstborn child is female, 0 otherwise. All estimations control for mother's age in a particular year, whether the mother has completed primary education, religion, caste, whether the house is located in an urban area, number of members in the household, household wealth index and sex composition of adults in the household. We additionally include district-year fixed effects and parity fixed effects. The sample is restricted to woman who gave birth to at least one child between 2011 and 2020. Standard errors are clustered at the district level.

Table 8: Impact of BBBP on Son Preference

	All Mothers	Recent Births
	(1)	(2)
	Son preference	Son preference
Treat X Firstborn Female	-0.04990	-0.13278
	(0.08940)	(0.15864)
Observations	243123	50415
Mean of Dep. Variable	0.6795	0.7687
SD	3.951	4.788

Standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: *Son Preference* is the ratio of the number of sons to daughters preferred by the mother. *Treat* takes the value 1 if BBBP has been implemented in a mother's district in a particular year, 0 otherwise. *Firstborn Female* takes the value 1 if firstborn child is female, 0 otherwise. All estimations control for mother's age in a particular year, whether the mother has completed primary education, religion, caste, whether the house is located in an urban area, number of members in the household, household wealth index and sex composition of adults in the household. We additionally include district-year fixed effects and parity fixed effects. Column 1 looks at the sample of all mothers. Columns 2 restricts to those who had recent births (at most 1 year before the survey). The sample is restricted to woman who gave birth to at least one child between 2011 and 2020. Standard errors are clustered at the district level.

1 Appendix

1.1 Variable definitions

1. Indicators for health investments

    (a) Antenatal Care (ANC) is the number of antenatal visits a pregnant woman had while her child was in utero. This data is collected for all children born to a surveyed woman upto five years before the survey. The value of these visits was topcoded at 20 visits, while the the children whose mothers did not go for antenatal care were coded as 0. According to WHO recommendations, there should be a minimum of eight antenatal visits to decrease perinatal mortality and improve women's experience of care.

    (b) Tetanus injections (TET) reports how many tetanus toxicoid vaccinations were given to a pregnant woman while her child was in utero. This data is collected for all children born to a surveyed woman upto five years before the survey. According to WHO recommendations, in case the mother is not previously vaccinated or in the case of unknown vaccination status of mother, she should be given two doses of tetanus toxicoid vaccination one month apart, with the second dose given at least two weeks before the delivery.

    (c) Duration breastfed reports the number of months for which a child was breastfed. This data is collected for all children born to a surveyed woman up to five years before the survey and includes the cases where (a) the child's mother was still breastfeeding at the time of interview and (b) the child had been breastfed until his/her death. According to WHO guidelines, children should be exclusively breastfed for the first 6 months of life, after which breastfeeding should continue with complementary feeding up to 2 years of age or beyond.

    (d) Vaccination indicators for Hepatitis B, DPT, Pentavalent, Polio, and Rotavirus report whether the child received the first shot for each of these vaccines usually administered within the first six weeks of birth. For Hepatitis B, this excludes the shot received at birth, if any.

Table A1: Proportion of the sample by NFHS Rounds

	Full Sample	Phase 1	Phase 2	Phase 3
Fourth Round NFHS	36.11	36.62	37.91	35.79
Fifth Round NFHS	63.89	63.38	62.09	64.21
Total	100.00	100.00	100.00	100.00

Note: The sample includes children born between 2011 and 2020. The first column relates to children born in any of the districts in, the second column relates to children born in Phase 1 districts, the third column relates to children born in Phase 2 districts and the last column relates to children born in Phase 3 districts.

Table A2: Impact of BBBP on gender gap in child mortality by firstborn female family

	Full Sample		Male		Female	
	(1) NNM	(2) IMR	(3) NNM	(4) IMR	(5) NNM	(6) IMR
Treat x Firstborn Female	0.00009	-0.00188	0.00289	0.00045	-0.00433**	-0.00597*
	(0.00137)	(0.00203)	(0.00199)	(0.00296)	(0.00214)	(0.00320)
Firstborn Female	-0.00230***	-0.00402***	-0.00390***	-0.00723***	0.00093	0.00150
	(0.00069)	(0.00080)	(0.00113)	(0.00137)	(0.00098)	(0.00127)
Observations	666030	586235	346654	305291	319339	280813
Mean of Dep. Variable	0.0272	0.0420	0.0302	0.0451	0.0239	0.0383
SD	0.163	0.201	0.171	0.207	0.153	0.192

Standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Note: Neonatal Mortality (*NNM*) takes the value 1 if a child died before completing 1 month, 0 otherwise. Infant Mortality (*IMR*) takes the value 1 if a child died before completing 1 year, 0 otherwise. Children who have not attained these respective ages are excluded from the regression. *Treat* takes the value 1 if BBBP has been implemented in a mother's district in a particular year, 0 otherwise. *Firstborn Female* takes the value 1 if firstborn child is female, 0 otherwise. All estimations control for child's sex, mother's age, mother's age at birth, whether the mother has completed primary education, religion, caste, whether the house is located in an urban area, number of members in the household, household wealth index and sex composition of adults in the household and additionally includes birth order fixed effects, district fixed effects, birth year fixed effects, birth month fixed effects, and district-year fixed effects. The sample is restricted to children born between 2011 and 2020. Standard errors are clustered at the district level.

Table A3: Analysis of pre-treatment trends

	(1) NNM	(2) IMR
Treatment Year - 5 x Female x Firstborn Female	0.00226	0.00265
	(0.00367)	(0.00431)
Treatment Year - 4 x Female x Firstborn Female	0.00195	0.00315
	(0.00368)	(0.00461)
Treatment Year - 3 x Female x Firstborn Female	-0.00099	-0.00185
	(0.00418)	(0.00538)
Treatment Year - 2 x Female x Firstborn Female	-0.00126	-0.00018
	(0.00444)	(0.00503)
Treatment Year - 1 x Female x Firstborn Female	-0.00253	-0.00457
	(0.00387)	(0.00519)
Observations	534773	497923
Mean of Dep. Variable	0.0274	0.0402
SD	0.163	0.196

Note: Neonatal Mortality (*NNM*) takes the value 1 if a child died before completing 1 month, 0 otherwise. Infant Mortality (*IMR*) takes the value 1 if a child died before completing 1 year, 0 otherwise. Children who have not attained these respective ages are excluded from the regression. *Treat* takes the value 1 if BBBP has been implemented in a mother's district in a particular year, 0 otherwise. *Female* takes the value 1 if the child's sex is female, 0 otherwise. *Firstborn Female* takes the value 1 if firstborn child is female, 0 otherwise. All estimations control for mother's age, mother's age at birth, whether the mother has completed primary education, religion, caste, whether the house is located in an urban area, number of members in the household, household wealth index and sex composition of adults in the household. We additionally include birth order fixed effects, district fixed effects, birth year fixed effects, birth month fixed effects, district-year fixed effects, firstborn female- district fixed effects, female-district fixed effects, firstborn female- birth year fixed effects, female- birth year fixed effects and firstborn female- female fixed effects. The sample is restricted to children born in districts that were not yet exposed to the treatment. Standard errors are clustered at the district level.

Table A4: Impact of BBBP on gender gap in child mortality by firstborn female family - by Phase

	Phase 1		Phase 2		Phase 3	
	(1) NNM	(2) IMR	(3) NNM	(4) IMR	(5) NNM	(6) IMR
Treat x Female x Firstborn Female	-0.00592	-0.00119	-0.00542	-0.00697	-0.00609*	-0.00653
	(0.00592)	(0.00786)	(0.00734)	(0.00826)	(0.00355)	(0.00619)
Treat x Female	0.00301	-0.01910	-0.00921	-0.02431	-0.00192	-0.01006
	(0.00811)	(0.01566)	(0.02469)	(0.04455)	(0.00743)	(0.00910)
Treat x Firstborn Female	0.00176	0.00563	-0.01636	0.01931	-0.00899	-0.01047
	(0.01931)	(0.02674)	(0.02732)	(0.04689)	(0.00897)	(0.01013)
Observations	99255	87558	60500	53340	506271	445333
Mean of Dep. Variable	0.0250	0.0401	0.0277	0.0424	0.0276	0.0423
SD	0.156	0.196	0.164	0.202	0.164	0.201

Standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Neonatal Mortality (*NNM*) takes the value 1 if a child died before completing 1 month, 0 otherwise. Infant Mortality (*IMR*) takes the value 1 if a child died before completing 1 year, 0 otherwise. Children who have not attained these respective ages are excluded from the regression. *Treat* takes the value 1 if BBBP has been implemented in a mother's district in a particular year, 0 otherwise. *Female* takes the value 1 if the child's sex is female, 0 otherwise. *Firstborn Female* takes the value 1 if firstborn child is female, 0 otherwise. All estimations control for mother's age, mother's age at birth, whether the mother has completed primary education, religion, caste, whether the house is located in an urban area, number of members in the household, household wealth index and sex composition of adults in the household and additionally includes birth order fixed effects, district fixed effects, birth year fixed effects, birth month fixed effects, district-year fixed effects, firstborn female-district fixed effects, female-district fixed effects, firstborn female- birth year fixed effects, female- birth year fixed effects and firstborn female- female fixed effects. The sample is restricted to children born between 2011 and 2020. Standard errors are clustered at the district level.