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Ashoka University Economics Discussion Paper 58

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May 2021

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Can legal bans on sex detection technology reduce gender discrimination?

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May 1, 2021

Abstract

Bans on sex-selective abortions, typically implemented to make sex ratios more equitable, may have adverse welfare consequences in terms of increased gender discrimination against surviving 'unwanted' girls. Exploiting geographic and intertemporal variation in the implementation of a ban on sex-screening and sex-selection across different states in India, we examine the extent to which prenatal gender discrimination is substituted by postnatal discrimination after the enforcement of the ban. In particular, we study whether the ban on sex-selective abortions worsens relative health and mortality outcomes for girls as compared to boys. Using the observation that sex-selective abortions are more likely to occur among families with firstborn girls, we compare our treatment effects across families with firstborn girls and firstborn boys. Our findings indicate that the ban increased the gender gap in mortality, health outcomes and health investments through two main channels: an increase in the proportion of unwanted girls who face increased discrimination and an increase in fertility in intensively treated families with firstborn girls, leading to greater competition among siblings for resources. We contrast our results with the impact of a policy that, in addition to strengthening supply-side measures, also contains demand-side elements aimed at shifting social norms through a mass media gender sensitisation intervention. Our results suggest that demand-side interventions that directly target social norms reduce the adverse welfare consequences of pure supply-side restrictions.

JEL-Classification: I18; I12; J13; J16; 01
Keywords: sex selective abortion; missing women, PNDT, ultrasound, legal ban, son preference, gender discrimination, skewed sex ratio

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We would like to acknowledge funding from Centre for Social and Behaviour Change (CSBC), Ashoka University for supporting this project. We would like to thank S.Anukriti, Sonia Bhalotra, Monica Dasgupta and Arindam Nandi for their excellent comments and suggestions. We thank Sonal Agarwal, Roopal Jain and Kavya Singh for excellent research assistance.

1 Introduction

The problem of "missing women" Sen (1992) has emerged as a direct outcome of the widespread phenomenon of sex-selective abortions of female fetuses, as well as the gross neglect of female children, in deeply patriarchal and gender-biased societies. Governments in such countries, such as India, China and South Korea, have frequently responded by introducing legal restrictions on the use of sex-screening technologies and the practice of sex-selective abortions, sometimes banning abortions altogether. However, in societies with strong son preference, while such bans may reduce the occurrence of sex-selective abortions, they can also encourage households to shift from prenatal discrimination against females to postnatal discrimination ((Goodkind, 1996; Das Gupta, 2019)).

The benefits of abortions for the wellbeing of the marginal child are well established (Gruber et al., 1999; Pop-Eleches, 2006; Ananat et al., 2009). In patriarchal societies, access to sex-selective abortions has also been found to lead to improvements in the health of surviving female children, even as the number of female births has declined (Almond et al., 2010; Lin et al., 2014; Hu and Schlosser, 2015; Anukriti et al., 2020). Conversely, a ban on sex-selective abortions or on abortions altogether could theoretically lead to increased female births but also higher rates of discrimination against surviving "unwanted" girls. Particularly in the presence of pervasive gender discrimination, there might be a trade-off between prenatal and postnatal gender discrimination: a policy that leads to an improvement in the sex ratio at birth in favour of girls *without* shifting underlying social norms, like that of son preference, may only lead to parents substituting postnatal gender discrimination in place of prenatal gender selection by discriminating against unwanted surviving daughters. These adverse welfare consequences of popular supply-side policy reforms that only affect access to the tools of prenatal discrimination, without shifting the demand for sons, have been a relatively understudied area.

In this paper, we examine the impact of a ban on the use of sex-screening technologies and sex-selective abortions on both the relative number of female and male births, as well as the gender gap in health outcomes and health investments by households. There are two potential channels through which a ban on sex-selective abortions could affect the gender gap in child health. First, an increase in the number of female births which are relatively "unwanted" could lead to increased postnatal discrimination by households against such girls through reduced parental care and investments in breastfeeding, immunisation and nutritional intake (Oster, 2009). Second, with a ban on the use of sex-selection technologies, families are more likely to rely on fertility stopping rules to attain the desired sex composition of their children. A selective continuation of fertility for firstborn female families can result in a situation where females are disproportionately born into larger families and face greater sibling competition for parental resources (Clark, 2000; Jensen, 2012, 2003).

Next, we ask how the estimated treatment effects of a supply-side policy, seeking to restrict access to abortion and to ultrasound technology, compare with a policy with additional demand-side elements that seek to shift underlying social norms determining the household demand for male and female children through a gender sensitisation campaign. If supply-side policies lead to increased postnatal gender discrimination, can a policy intervention with a demand-side gender sensitisation component reverse some of these negative welfare consequences?

To answer these questions, we exploit a natural experiment in India created through the staggered implementation of laws banning the use of sex-screening technology and sex-selective abortions to identify the impact of the ban on the proportion of female births and on the gender gap in child mortality, health outcomes and health investments by households. Such laws were introduced to different Indian states gradually from 1988 to 2002: first the state of Maharashtra banned sex-screening technologies such as ultrasound in 1988. A national law was then passed in 1994 to cover all remaining states except for Jammu and Kashmir (henceforth JK). Finally, JK passed its own law in 2002. We use the resultant geographical and intertemporal variation in exposure to the ban to answer the following question of interest: do girls born after the ban suffer from higher mortality, poorer health outcomes and reduced health investments by their parents relative to boys? To answer this we compare girls and boys born before and after the ban, in treated versus control states. Our data allows us to control for a range of individual, family, state and time-level confounding factors.

We additionally leverage results from previous studies that establish that the sex of the firstborn child is quasi-random, and that the use of sex-selective abortion is relatively intensive among families with a firstborn girl (Bhalotra and Cochrane, 2010a; Anukriti et al., 2020), and estimate the differential impact of being born in a treated state to a family with a firstborn girl, compared to a firstborn boy. If parents with firstborn girls are relatively intensively treated by the ban, they are likely to restore to fertility stopping behaviour so as to achieve their desired sex composition among their children. Children born into these larger families would face greater sibling competition for resources, leading to relatively worse mortality, health outcomes and parental investments.

Our results are striking. We find that while exposure to the ban on sex-selection technology increased the relative odds of a female birth, it has also led to a worsening of the gender gap in mortality outcomes, health outcomes such as height-for-age and weight-for-age, and health investments such as months for which the child is breastfed and vaccination status. Children in firstborn female families are disproportionately affected by the ban, with mortality and health outcomes worsening for children on average in such families. Additionally, we find that fertility went up in firstborn female families.

We then compare these results with the treatment effects of a mass media campaign launched in 100 districts in India – the Beti Bachao Beti Padhao (Save Girls, Educate Girls) programme – aimed at increasing levels of gender sensitisation, promoting the value 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. We find that the media campaign did reverse some of the costs of a pure supply-side policy by reducing the gender gap in neonatal mortality and increasing household investments in girls, relative to boys. These results are muted, however, perhaps because the campaign is still in its infancy, but also because of the considerable challenges involved in shifting hardwired social norms like a deep-rooted son preference.

Our paper provides the most comprehensive estimates on the adverse health consequences of a ban on access to sex-screening technologies. A previous study found that the Indian ban led to significant improvements in the sex ratio at birth (Nandi and Deolalikar, 2013a) but the results on the impact on child health have been limited to a single study that finds no significant negative consequences of the ban (Nandi, 2015b). Our study improves upon this previous work by implementing a relatively demanding empirical specification that includes mother fixed effects and state-specific time trends, allowing us to control for a number of confounding factors arising from family-level heterogeneity and geographic variation in social norms. Further, we use a much more extensive and rich dataset, covering child births across 25 years, that allows us to compare results of various policy disruptions relating to birth technology.

A similar study of the converse effect finds evidence of reduced gender discrimination and improved health outcomes of girls relative to boys with *increased* access to ultrasound technology (Anukriti et al., 2020). We compare their results to ours and show that the access to and restriction of ultrasound technologies and abortions leads to symmetrically equal but opposite effects: the former leading to fewer female births and improved mortality and health outcomes for surviving females, while the latter improves the relative odds of a female birth but widens the gender gap in mortality and health.

We also provide the first estimates of the treatment effects of the mass media campaign on mortality, health outcomes 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. While our analysis is limited by the fact that we only observe data on children up to 18 months after the implementation of this programme, our preliminary results are promising and suggest the importance of demand-side elements such as media-based efforts to shift social norms through gender sensitisation efforts. This has important insights on policy design for countries struggling to reduce pervasive and deep rooted gender discrimination and the problem of missing women.

The rest of the paper is organised as follows. Section 2 presents the background on the legal ban on sex-screening and sex-selective abortions and the media intervention campaign, as well as theoretical motivation on the likely impact of the policies on gender bias; Section 3 describes the data and presents descriptive statistics; Section 4 presents the empirical strategy; Section 5 presents the results along with robustness checks; Section 6 discusses potential mechanisms that explain our results; and Section 7 concludes the discussion.

2 Background and theoretical motivation

Governments in developing countries have resorted to three broad categories of methods to tackle high rates of fetal sex selection in favour of males. First, governments have enacted bans on the use of sex-detection technologies, often accompanied by rigorous prosecution of technology providers. Second, governments have attempted to shift underlying social norms and perceptions about the worth of a female child through media outreach and related interventions, with a view to enhance gender equality, improve sex ratios and health outcomes for females, by reducing the demand for sex selection (Das Gupta, 2019; Guo et al., 2016). Third, governments have also offered conditional cash transfers, financial incentives to households to encourage them to give birth to female children, rather than abort them (Anukriti, 2018). While bans impose supply side restrictions on access to sex-detection technologies, the other two methods help in reducing the demand for sex selection.

Supply-side measures like bans have been popular, and implemented by governments in China, India and South Korea. However, bans can be difficult to enforce in countries with low state capacity. In addition to enforcement concerns, bans can also have negative welfare consequences for females by displacing prenatal gender discrimination towards postnatal gender discrimination towards unwanted girls that are born as a consequence of the ban on sex selection and screening (Das Gupta, 2019; Park and Cho, 1995). In particular, there is evidence that prenatal sex selection may help improve life chances of the females that are born, as they are actively wanted by households that choose to have them. In India, Anukriti et al. (2020) find that the diffusion of ultrasound technology led to a decline in gender-based discrimination in health inputs. In the pre-ultrasound period, boys were more likely to be breastfed and vaccinated, but after the diffusion of ultrasound technology, gender gaps in breastfeeding and vaccinations declined significantly.[1] They also find that families with firstborn girls are less likely to have larger families through the fertility stopping rule, whereby families continue to have children until their desired sex composition has been achieved. Accordingly, the spread of ultrasound technologies led to a decline in the gap in sibling size between firstborn girl and firstborn boy families, and an increase in birth spacing in firstborn girl families compared to firstborn boy families. Removing the availability of such prenatal sex selection techniques may conversely increase the number of unwanted girls, with households reducing the level of early life investments in their daughters, either through direct discrimination or through pursuing the fertility stopping rule and having larger families, leading to adverse health outcomes.

Other concerns with supply-side measures are that newer technologies for sex selection such as low-cost maternal blood tests are likely to be even harder to monitor and to regulate. Bans on sex-selective abortions have also reduced women's access to safe abortions for other non-discriminatory reasons. Additionally, there may be concern that households shift to riskier forms of abortion techniques in the absence of a formal provider that imposes a health cost on the mother. Moreover, it is not clear that bans on sex screening and selection lead to changes in the underlying level of son preference in the population which might be deep rooted in cultural norms. Jayachandran (2015) argues cultural norms such as patrilocality help explain the male-biased sex ratio in India and China. Ebenstein (2014) highlight the connection between historical subsistence patterns of agriculture and higher rates of coresidence with sons and higher sex ratios at birth today.

Demand-side measures, on the other hand, have the potential to shift underlying social norms and perceptions that give rise to the son preference that generates gender discrimination in the first place. Countries like China, South Korea and India have implemented traditional and social media interventions designed to increase the perception of the worth of a girl child, and if these are successful, they can potentially increase the demand for girls without any adverse consequences on investments in their health.

There is a body of growing literature that points to the potential for demand-side interventions to change hardwired social preferences and norms. Jensen and Oster (2009) find that the introduction of cable television in rural India increased women's autonomy in making fertility decisions and decreased the acceptability of domestic violence and son

[1]This is in contrast to a study in China that finds that access to prenatal sex discrimination in the form of ultrasound technology does not have a significant effect on the gender gap in breastfeeding or in access to vaccines (Almond et al., 2010).

preference. In the context of Brazil, La Ferrara et al. (2012) finds that access to television, especially novelas, significantly lowers fertility among women.Using a a school-based intervention in the state of Haryana in India that engaged adolescents in classroom discussions about gender equality, Dhar et al. (2018) find that the programme made children's attitudes more supportive of gender equality particularly among boys.

In this paper, we identify the impacts of a supply-side intervention, through a ban on the use of sex-screening technologies, and contrast that with a policy that has complimentary demand-side components, primarily involving a media campaign to shift social norms and preferences for girls. We compare and contrast the welfare consequences of each, both in terms of the impact on the relative odds of a female birth, as well as impacts on healthcare investments in girls by households, with direct effects on health outcomes such as mortality, height and weight. The context for both interventions is India: a ban on sex detection techniques was enacted in a staggered manner across Indian states between 1988 and 2003, while a media campaign was launched more recently in 100 districts in 2015. We discuss both interventions in the next sections.

2.1 Supply-side measures: Legal ban on sex screening and sex selective abortions

The earliest form of sex detection technology in India was the amniocentesis procedure, whose introduction in the 1970s was followed by a rapid increase in the number of private clinics offering sex determination and abortion services (Nandi and Deolalikar, 2013b). Ultrasound technologies were introduced after 1985, with a significant increase in their availability after 1995, driven by the liberalisation of imports into India (Anukriti et al., 2020). Ultrasound technology had significant advantages over the amniocentesis procedure: it cost half as much, was highly accessible, and some machines were portable, allowing for their availability in rural and hard-to-reach areas (Arnold et al., 2002). The use of sex detection and sex selection of fetuses rapidly became widespread leading to an increasingly male-skewed sex ratio (Bhat and Zavier, 2003). Bhalotra and Cochrane (2010b) estimate that the spread of ultrasound technologies led to the selective abortion of around 480,000 girls per year during 1995-2005.

In response to the vast scale of sex-selective abortions believed to be taking place, the identification of the sex of the fetus was banned in all public healthcare facilities in 1978. In 1988, Maharashtra became the first state in country to ban all prenatal sex determination and disclosure of the sex of the fetus in private clinics as well. A national law covering all other states, except Jammu and Kashmir, was introduced with the Pre-Natal Diagnostic Techniques (Regulation and prevention of misuse) Act (PNDT Act) in 1994, which was brought into force in 1996. Jammu and Kashmir enacted its own ban in 2002.

The PNDT Act imposed stringent punishments on both the medical practitioner and the client, including fines of between Rs 10,000-50,000, imprisonment of 3-5 years, and the revoking of medical licences and confiscation of equipment.[2] Ultrasound usage was restricted to very specific circumstances, and all tests had to be recorded by the doctor.

There have been multiple challenges in implementation and enforcement, and there exists a widespread belief that many households are able to circumvent the bans by accessing illegal private sector ultrasound providers (Visaria, 2008; Arnold et al., 2002). Implementation in India is believed to be particularly weak, compared to China and South Korea. For one, abortions in India are legal, and take place at different locations from the ultrasound clinics, making it difficult to prove that the reason for an elective abortion is the sex of the fetus (Guo et al., 2016). This is in contrast to South Korea where barring a few exceptions like medical conditions, abortion itself was not legal until recently[3] (Das Gupta (2019)). South Korea also benefits from the presence of a well-regulated health sector in a much smaller population, making it easier to enforce a ban, while China benefits from a high level of state capacity even at the level of small communities (Das Gupta, 2019). However, even in China, punitive action against sex selection technology providers was found to have little impact on child sex ratios; instead, the close monitoring of women having a second child was found to have greater success in preventing sex-selective abortions (Guo et al., 2016).

Nonetheless, a small empirical literature that has examined the impact of the Indian ban on sex screening and sex selection on child sex ratios finds that it led to an increase in the birth of girls. Using census data and Maharashtra as a control state, Nandi and Deolalikar (2013b) estimate that the national ban of 1994 led to an increase in the female-male sex ratio of 14-26 points, driven by changes in rural areas. In fact, these estimates are a likely lower bound since the child sex ratio they use is defined for children between the ages of 0 and 6 years, even though children aged between 4 and 6 years were never exposed to the treatment. Using a different dataset, the District Level Household Survey (2002–2004), Nandi (2015a) finds that the ban led to an increase of 1 percent in the odds of a female birth across all treated states (compared to Maharashtra). Exploiting the quasi-random gender of the firstborn child, and the fact that families with firstborn girls are more likely to pursue sex selection during subsequent pregnancies than families with firstborn boys, Rastogi and

[2]Article 24 of the PCPNDT Act, Government of India, available at https://www.indiacode.nic.in/bitstream/123456789/8399/1/pre-conception-pre-natal-diagnostic-techniques-act-1994.pdf

[3]A 2019 order decriminalised abortion in South Korea, effective from 2021

Sharma (2020) find that the implementation of the ban led to an increase in the probability of a female birth of 2.5 percentage points, with this effect being concentrated among relatively low-wealth families in rural areas, where access to private ultrasound and abortion clinics is likely to have been significantly less.

However, if the bans on sex screening and selection have no impact on the underlying level of son preference in the population, then there are potentially adverse welfare consequences for surviving girls if they are relatively unwanted. Nandi (2015a) considers the impact of the ban on infant mortality for girls in the first year of birth. Using data from the District Level Household Survey (2002–2004), the study finds that the child sex ratio at birth improves in favour of girls but that there is no impact on infant mortality rate for girls. Using a specification with mother fixed effects, the author finds a small but significant increase in the relative infant mortality for girls. However, this study does not consider any other health outcomes. Rastogi and Sharma (2020) find that the ban led to a decline in educational investments in girls relative to boys, but do not consider health outcomes in their study.

Using a richer dataset over multiple domains of health and pooling all four rounds of the National Family and Health Survey, we examine the impact of the ban on a wider range of health outcomes. Our estimating strategy includes a wider set of controls in the form of mother fixed effects, in addition to state-specific time trends and birth year fixed effects, as well as the plausibly exogenous variation in the gender of the firstborn child.

2.2 Complementary demand-side measures: The Beti Bachao Beti Padhao (BBBP) programme

The Beti Bachao Beti Padhao (BBBP) programme was launched in 2015, primarily to address worsening sex ratios in several Indian districts as well as to promote women's empowerment and gender equality. The objectives of the programme are to prevent sex-selective abortions, ensure the survival and protection of the girl child and to reduce gender gaps in access to education. The initiative mainly involved a mass communication campaign targeted at shifting social norms and perceptions about the worth of the girl child, as well as some additional actions in selected districts where the child sex ratio had increased in favour of males between 2001 and 2011. The budgetary allocation of funds for the last three years has been in excess of Rs 11 billion (Scheme, 2018). In a short period of time the BBBP programme has become very well known: a recent survey of 14 states finds nearly 88 per cent of respondents were aware of the programme (NCAER Report, 2020).

The programmes's stated aims were to achieve the following goals by 2018-19: improve the sex ratio in the selected districts by 2 percentage points every year, reduce the gender differentials 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.

The mass communication campaign involves 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 hand-outs, brochures, text messages on mobile phones in English, Hindi and regional languages.[4]

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 complete database of complaints about violations of the ban was to be maintained. Sting operations were conducted to unearth the illegal practice of sex selection (Scheme, 2018).

As it focused on both a more stringent implementation of the ban as well as encouraging changes in social norms through advocacy and media campaigns, the programme has both supply-side and demand-side elements. This provides a unique and interesting 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.

The programme was initially launched in 100 districts in 2014-15 (Phase 1), and was expanded to 61 additional districts in 2015-16 (Phase 2). The mass media campaign was launched at the national level, with focused interventions in programme districts. So far there has been very limited research on the impact of the programme. Gupta et al. (2018) do 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

[4]Other actions included a renewed focus on the enforcement of the ban on sex detection and sex-selective abortions. 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. Some other measures in the context of health include improvements in the prenatal and postnatal care of mothers, and the provision of counselling to ensure the equitable care of female infants, as well as the training of front-line health workers to make them more sensitive to these concerns. 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.

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 the Phase 1 BBBP districts after implementation, compared to the control districts. Additionally, we exploit the quasi-exogenous assignment of the gender of the firstborn child and test if children in families with firstborn girls are affected differently from families with firstborn boys. To our knowledge, these are the first causal estimates of the impact of the programme.

3 Data and Descriptive Statistics

To examine the impacts of the ban on sex screening and the gender sensitisation media campaign on fertility and mortality outcomes, we pool data from all four rounds of the National Family Health Survey, a national household survey conducted in 1992-93, 1998-99, 2005-06 and 2015-16. For fertility outcomes, we use retrospective birth histories of all women aged between 15-49 years to construct a dataset of all births that take place between 1975 and 2016. This dataset includes over 7.9 million mother-year level observations on almost 300,000 unique women.

For child health and mortality outcomes, we organise the data at the level of the child: our mortality dataset includes around 2 million child observations, while the child health data set, which covers children born 3 to 5 years before each survey round, includes around 400,000 child observations.

The data also includes a rich set of mother and household characteristics including mothers age, whether the mother has completed primary education, total children ever born to the mother, religion, caste, whether the household belongs to urban sector, household wealth index and total number of members in the household.

4 Empirical Strategy

To identify the impact of the ban on sex screening and sex selection on postnatal discrimination against female children born after the ban was implemented, we exploit the staggered roll-out of the ban across different states and across time. Maharashtra was the first state to enact a ban on sex screening in 1988, implemented in 1989, followed by the enactment of a national ban in 1994, implemented in 1996, covering all other states except Jammu and

Kashmir (JK)[5]. JK enacted its own ban in 2002 which was implemented in 2003. Using this geographic and intertemporal variation, we identify if there is a systematic increase in post-natal discrimination among female children who are born after the ban was implemented in their state.

We additionally exploit the quasi-exogenous variation in the gender of the firstborn child of a family, and, under the assumption that families with firstborn girls are more likely to practice sex selection at higher birth orders (Anukriti et al., 2020), we are able to identify the treatment effect of the ban on the average outcomes for children born in such families compared to children born in families with firstborn boys.

Das Gupta and Mari Bhat (1997) explore the relationship between fertility decline and postnatal gender bias in a cross-country setting to find evidence of two countervailing effects: first, that discrimination against girls tends to be higher at higher birth orders. As fertility declines, there are fewer births at higher orders inducing a mechanical reduction in discrimination against girls. The authors term this a 'parity effect'. At the same time, however, due to the decline in fertility, the gender discrimination at any given birth order increases, leading to more discrimination against girls: they term this the 'intensification effect'. The net effect on gender discrimination of a decline in fertility is ambiguous; the authors conclude that the two effects cancel each other out.

In our setting, if the ban leads to higher fertility, on average, and among firstborn girl families in particular, the gender discrimination is likely to go up following the parity effect. The intensification effect will not apply in our setting as all the new female births are more likely to be unwanted. If the proportion of unwanted girls increases after the implementation of the ban, this likely affects their mortality in the post-treatment period, on account of higher family size or lower investments in girls (actively or mechanically), which is likely to go up with birth order. Moreover, if firstborn female families are more likely to resume fertility stopping behaviour after the ban, we should see an increase in the probability of any birth in such families, with such higher fertility being associated with higher mortality and worse health outcomes. The effect of increased fertility on gender discrimination among children born to firstborn female families is ambiguous though. On the one hand, as parents in firstborn female families resort to fertility stopping behaviour, females mechanically face higher sibling competition, and on the other hand, in the absence of sex-selective abortions, the average birth order for male births goes up, where they might be biologically more vulnerable to increased sibling competition. Hence, theoretically, we should predict that after the ban (i) average outcomes for females should worsen, as the proportion of unwanted female births goes up, (ii) the average outcomes for children in

[5] Handbook for the Act available at, https://www.pndt.gov.in/WriteReadData/mainlinkfile/File100.pdf

firstborn female families comparatively worsen as compared to firstborn male families from the relative increase in fertility, and (iii) the gender discrimination against girls in firstborn girl families may or may not increase as outcomes worsen for both males and females in such families.

We use the intertemporal, spatial and family level variation in exposure to the ban to compare and contrast outcomes between males and females on a range of health outcomes, including mortality, malnutrition, and health investments, such as vaccinations and health visits. Our main estimating framework controls for mother fixed effects in addition to birth year and birth order fixed effects and state-specific time trends, allowing for a flexible specification that controls for a number of confounding factors. The inclusion of mother fixed effects allows us to compare children who are differentially exposed to the policy intervention but born to the same mother, controlling for innate mother- and family-level unobservable heterogeneity that could affect our outcome variables. Specifically, we are able to estimate how gender gap in outcomes between children born to the same mother changes in response to the policy.

Additionally, the inclusion of state-specific time trends allows us to control for confounding pre-intervention trends in different states that could be driving our results. These are important sources of confounding variation: prior evidence suggests there is significant heterogeneity in son preference, fertility, female births and gender discrimination by observable and unobservable characteristics of households as well as across states and over time[6]. For regressions relating to the BBBP intervention, we substitute district-specific time trends in place of state-specific time trends.

[6]Nandi (2015a) find that socio-economically disadvantaged groups like scheduled castes and scheduled tribes are more likely to have female births. Households that are relatively more well off as compared to the poorest quintile are less likely to have female births but also have lower female mortality rates. Rastogi and Sharma (2020) find that families in bottom 60% of the wealth distribution are more likely to be affected by the implementation of the ban on sex-screening since they are less able to access private sector clinics. In addition to variation by mother, there is significant variation across geography as well: Carranza (2014) associates geographical variation in soil textures in India with infant and child sex ratios through the channel of variation in relative demand for female labour. Districts with larger fractions of loamy soil have significantly more male-biased sex ratios. Alesina et al. (2018) finds that high plough usage is associated with male-biased sex ratios through sex-selective abortions due to the relative undervaluing of female labour in these societies. Anukriti et al. (2020) also find evidence of heterogeneity by region, with excess female mortality and fertility declines in the post-ultrasound diffusion period being stronger in states with a higher level of son preference (regions with above-median sex ratios at birth).

4.1 Mortality and child health

Our first set of outcome variables pertains to child mortality and child health indicators. We run the following estimating equation:

$$\begin{aligned} Y_{imst} &= \beta_0 + \beta_1 \text{Treat}_{imst} + \beta_2 \text{Female}_{imst} + \beta_3 (\text{Treat} \times \text{Female})_{imst} + \delta_{st} \\ &+ \tau_t + \phi_m + \epsilon_{imst} \end{aligned} \tag{1}$$

where Y_{imst} is an indicator for mortality, health outcomes and health investments for child i born to mother m in state s in year t. We include birth year fixed effects (τ_t), state-specific time trends (δ_{st}) and mother fixed effects (ϕ_m) and birth order fixed effects. We use three measures of child mortality: neonatal mortality (if a child died before completing 1 month), infant mortality (if the child died before completing 1 year) and under-five mortality (if a child died before completing 5 years). The sample includes children born between 1985 and 2005. We omit children born prior to 1985 in order to compare birth outcomes of children who were born after the early diffusion period of ultrasound technology that starts from 1985 (Anukriti et al., 2020).We also omit children born after 2005 so as to ensure that our treated group remains comparable to the control group, which might be a less plausible assumption to make with the use of longer-term data. Regressions exclude all children who have not attained the required age for the respective mortality measure. We check for robustness of our result in the sample with the full range of birth years 1975-2016.

Next, we estimate a variant of equation 1 with the exclusion of mother fixed effects and with the inclusion of household/mother level controls sibling size, mother's age at birth, whether the mother and father have completed primary education, mother's weight for height, religion, caste, whether the house is located in an urban area, number of members in the household, household wealth index, number and sex composition of adults in the household.

Health outcomes include (i) a set of objective biomarkers such as height for age, weight for age and body mass index (BMI); (ii) indicators for health investments that could affect these biomarkers such as ante-natal care (ANC) visits, whether a child has received tetanus shots and breastfeeding duration. A detailed note on variable definitions and construction can be found in Appendix A.

For regressions estimating the impact of the ban on sex screening and sex selection on child mortality and health, the variable $Treat_{imst}$ takes value one if child i born to mother m is born in state s in the year t where ban has been implemented and zero otherwise. The underlying geographic and temporal variation comes from the staggered implementation of the policy across states. For all states except JK and Maharashtra, the year of implementation is 1996, for Maharashtra it is 1988 and for JK, 2003.

For the regressions estimating the impact of the BBBP intervention on child mortality and health, we use variation in the timing of the policy at the district-month-year level. Instead of state-specific time trends, we include district-specific time trends, and we cluster standard errors at the district level. We estimate the following equation:

$$\begin{aligned} Y_{imdt} &= \beta_0 + \beta_1 \text{Treat}_{imdt} + \beta_2 \text{Female}_{imdt} + \beta_3 (\text{Treat} \times \text{Female})_{imdt} \\ &+ \delta_{dt} + \tau_t + \phi_m + \epsilon_{imdt} \end{aligned} \tag{2}$$

where Y_{imdt} is an indicator for mortality, health outcomes and health investments for child i born to mother m in district d in year t. We include birth year fixed effects (τ_t), state-specific time trends (δ_{dt}) and mother fixed effects (ϕ_m) and birth order fixed effects. where the $Treat_{imdt}$ variable indicates whether the child i born to mother m was born in a phase one BBBP district and exposed to the BBBP intervention at any point in their life. For estimating the impact on neonatal mortality, we define the treatment group as including all births that took place after June 2015 in the phase one districts; for infant mortality, the corresponding definition of treatment includes all births that took place after June 2014 in treated districts ; and for under-five mortality, all births that took place in treated districts after June 2010. It would have been ideal to look at the impact of the full exposure to policy for all years of life used in the calculation of the respective mortality figures in neonatal, infant and under five mortality. However, the NFHS-4 data was collected in 2015-16 and only allows us to compare cohorts with and without partial exposure to the programme in terms of our outcomes of interest.

β_3, the coefficient on the interaction of $Treat_{imst}$ and an indicator for $Female_{imst}$, is our coefficient of interest and captures whether girls born to the same mother are differentially affected by the implementation of the treatment compared to their brothers, and provides direct evidence on the combined impact of the ban on sex screening and selection as well as the media intervention on postnatal gender discrimination. We include mother fixed effects, birth year fixed effects and district-time trends in all regressions.

4.1.1 Heterogeneity of policy impact by firstborn female family

Given the evidence that families where the firstborn child is a female are more likely to pursue sex selection at higher birth orders (Anukriti et al., 2020), the intensity of the treatment (that is, the ban) is likely to be higher for such families. Following Anukriti et al. (2020), we exploit the quasi-exogenous variation in the gender of the firstborn child of the family to identify the treatment effect of both the ban and the media intervention by comparing outcomes across families with firstborn girls and firstborn boys. If families with firstborn girls are more intensively treated by the policies, outcomes should change by a greater extent in such families.

In the following specification, we test whether children born to firstborn female families have systematically worse outcomes in mortality and health, as compared to firstborn male families in the post-ban period. Similar to the previous estimating equation, we interact the $Treat_{imst}$ variable with an indicator of whether the family has a firstborn daughter to run the following estimation:

$$Y_{imst} = \beta_0 + \beta_1 \text{Treat}_{imst} + \beta_2 (\text{Treat} \times \text{FirstbornFemale})_{imst} + \delta_{st} + \tau_t + \phi_m + \epsilon_{imst} \quad (3)$$

where Y_{imst} is either an indicator for mortality outcome or health outcome for child i born to mother m in state s, born in year t. We include birth year fixed effects (τ_t), state-specific time trends (δ_{st}) and mother fixed effects (ϕ_m) and birth order fixed effects. Next, we estimate a version of equation 3 without mother fixed effects and which additionally includes the term $FirstbornFemale$ (varying at mother level), and the set of controls which are - child's birth order, sibling size, mother's age at birth, whether the mother and father have completed primary education, mother's weight for height, religion, caste, whether the house is located in an urban area, number of members in the household,household wealth index, number and sex composition of adults in the household.

The variable $FirstbornFemale_{imst}$ is defined at the mother level, taking the value one if mother m of child i has a firstborn girl child and zero otherwise. Treat_{imst} has the same interpretation as above. Here, β_3 captures the systematic difference in outcomes of children born into firstborn female families exposed to the policy shock compared to those children born into firstborn male families.

We estimate the results separately for children born at higher birth orders because females born at higher birth orders may suffer a higher degree of discrimination than females born at lower birth orders. This is evident from previous studies that find that sex selection increases at higher order births (Bhalotra and Cochrane, 2010a; Rosenblum, 2013).

Standard errors are clustered at the state (for PNDT regressions) or the district level (for BBBP regressions). For the PNDT regressions, we also present p-values from a wild cluster bootstrap (Cameron et al., 2008), correcting for the small number of untreated clusters.

5 Results

5.1 Impact of the ban on sex screening and selection

5.1.1 Child mortality

Table 1 presents the estimates for mortality outcomes (neonatal, infant and child mortality). Columns 1, 2 and 3 present the estimates for birth orders 2 and above, while columns 4, 5 and 6 present the estimates for children born at all birth orders. If discrimination against female children increases at higher birth orders, we should expect the effects to be larger in the first three columns.

Our main coefficient of interest is the coefficient on the interaction term $Treat \times Female$. This coefficient is positive and significant for neonatal mortality and infant mortality (at the 1 percent level), both for the sample of all births as well as the restricted sample of birth orders 2 and above. This provides strong evidence that the implementation of a ban on sex-screening and sex-selective abortions led to a large and significant increase in the gender gap in mortality for surviving girls relative to boys. The coefficient for under-five mortality is positive for both samples as well, significant at the 10 percent level for the sample of children born at birth orders of 2 and above, and at the 5 percent level for the sample including all births. The coefficient on *Female* is negative implying that mortality is relatively lower for females in the absence of the ban on sex-screening and selection. This is in line with the biological phenomenon of boys being more vulnerable to early mortality than girls when very young (Drevenstedt et al., 2008; Pongou, 2013).

In terms of effect size, these treatment effects are considerable. Across the various specifications, neonatal mortality for girls is found to be around 17-18 percent higher in the post-intervention period as compared to boys for the same mother. Similarly, infant mortality for girls is 13-15 percent higher and under-five mortality is around 10-12 percent higher compared to boys born to the same mother. We find that the estimated coefficients are typically larger for the regressions with the sample restricted to children of birth order 2 and above, suggesting an intensification in gender discrimination at higher birth orders.

One concern with the mother fixed effects regression is that the estimated coefficients are driven by variation in the sample of mothers with multiple children, who could be systematically different from mothers with single births. We feel this is less of a concern as the proportion of mothers with single births is very low in our sample (6.3 percent). Moreover, given that sex screening and selection is more likely to happen at higher order births, this should not affect our results. Nonetheless, to rule out that our results are being driven by sample selection effects, in a modified specification (Table 2), we confirm that our results are robust to the removal of mother fixed effects. In these specifications we include a number of demographic characteristics, including child's birth order, sibling size, mother's age at birth, whether the mother and father have completed primary education, mother's weight-height ratio, religion, caste, whether the house is located in an urban area, number of members in the household, household wealth index, number and sex composition of adults in the household. We additionally include birth year fixed effects, state fixed effects and state specific time trends. The results are robust to this alternative specification. We find that the estimated coefficient on *Treat* $\times$ *Female* is positive and significant for all mortality outcomes in both birth order samples, which is in line with the results that include mother fixed effects. As before, we find that the ban had a greater impact on the gap in mortality at higher order births [7].

How far do these results align with the existing literature? Table A.1 presents a comparison of our results with Nandi (2015b), the only other paper to examine this question. We show the results for the regression of infant mortality on the interaction of PNDT treatment and female, a specification followed by Nandi (2015b), although with a different dataset, the District Level Household Survey. That study found the coefficient on the interaction term to be negative and insignificant. We build upon this specification by including mother fixed effects, birth year and state-specific time trends. While we find the coefficient to be positive and significant at 10 percent even without the inclusion of any fixed effects, upon including all three fixed effects we find the results are significant at the 5 percent level. One of the robustness checks in Nandi (2015b) that uses mother fixed effects in a shorter sample period (comparing cohorts born in 1990–1995 with those born in 1997–2002) does find that the law was associated with a 0.06 percent increase in the relative infant mortality of girls in the all-states sample. However, the impact is positive but insignificant in the sample that includes Maharashtra and its neighboring states.

We also present the estimated effects of the ban on sex-selective abortions on mortality after excluding the states of Jammu and Kashmir and Maharashtra in Table 3. The identification in this specification relies on the intertemporal variation in ban exposure within the family. Columns 1, 2 and 3 present the estimates for birth orders 2 and above, while columns 4, 5 and 6 present the estimates for children born at all birth orders. We find that the results are robust to the exclusion of both the states, with the estimate for *Treat* $\times$ *Female* being positive and significant for all mortality outcomes, for both birth order samples. The results are also robust to the exclusion of mother fixed effects and the inclusion of the

[7] We also check for heterogeneity in the results by socioeconomic status and find that the effects are more pronounced among poor households (results available on request).

household-level controls (Table 4). The sample of children is the same as that used in Table 3 and we additionally include birth year fixed effects, and state-specific time trends.

Next, we present the effect of the legal ban on relative mortality of children born in firstborn female families after the ban in Table 5. Here our main coefficient of interest is the coefficient on the interaction between the treatment variable and the female firstborn family indicator. Columns 1, 2 and 3 present the estimates for birth orders 2 and above, while columns 4, 5 and 6 present the estimates for children born at all birth orders.

We find that the coefficient on the interaction term *Treat* × *Firstborn Female* is positive and significant for all the mortality outcomes implying that after the ban, relative mortality was higher for children born in female firstborn families. The coefficient on *Treat* is negative and significant for under-five mortality, implying that mortality is relatively lower in the post-ban period for male firstborn families. The coefficient on *Treat* is negative and significant for all mortality outcomes, when the sample includes all birth orders. Since families with firstborn girls are more intensively treated by the ban, the rise in mortality in such families after the ban suggests that the ban had a significant negative impact on child mortality. In the sample of children with birth order two and above, neonatal mortality is higher by around 15 percent for firstborn female families after the ban. The relative difference in infant mortality between children from firstborn female and firstborn male families is higher by around 14 percent after the ban and the corresponding figure for under-five mortality is 11 percent.

The channel that the intensively treated firstborn female families are more likely resorting to the fertility stopping rule can explain these results. We examine this channel in Section 6.2.

We confirm that these results are robust to the exclusion of mother fixed effects and the inclusion of the same set of demographic characteristics as in Table 2, while also including birth year fixed effects and state-specific time trends (Table 6). We also restrict the sample to children of birth orders 2 and above. We find that the coefficient on *Treat* × *Firstborn Female* is positive and significant across all mortality outcomes.

5.1.2 Analysis of pre-intervention trends

Using an event study design, we plot the trends in infant, neonatal and under-5 mortality rates for ten years prior to and after the enactment of the ban on sex-selective abortions in Figures 1 and 2.

Figure 1 plots the estimated coefficients on the interaction of female and birth year relative to the ban for all mortality outcomes, both ten years prior to the enactment of the ban and up to ten years after the implementation of the ban. For neonatal mortality the graph shows a significant increase immediately after the implementation of the ban.

Figure 2 plots the estimated coefficients on the interaction of the indicator for firstborn female family and birth year relative to the ban, for birth orders restricted to 2 and above. As before, the regression includes mother fixed effects, state-specific time trends, birth year and birth order fixed effects. Standard errors are clustered at the state level. The x axis plots event time relative to the implementation of the ban with the 0 mark on the x axis indicating the year in which the ban was implemented in a state in which a mother is located. This is not a specific calendar year as the implementation of the ban is staggered across states. Birth order is restricted to 2 and above.

We see an upward trend for all three mortality outcomes after the ban. Importantly, we don't see any significant coefficients in the pre-intervention period suggesting that confounding pre-intervention trends are not driving our results.

5.1.3 Health outcomes and investments

Table 7 presents the estimates of the ban on sex-selective abortions on child health outcomes – height for age (HFA), weight for age (WFA) and body mass index (BMI) – in a similar regression framework as that of the mortality regressions. The model controls for mother fixed effects in addition to birth year fixed effects and state-specific time trends. The main coefficient of interest is the coefficient on the interaction term between *Treat* and the indicator for a female child. The sample is restricted to birth years ranging from 1988 and 2005 since the earliest health outcome data is only available from 1988. Columns 1, 2 and 3 present the estimates for birth orders 2 and above and columns 4, 5 and 6 present the estimates for all birth orders.

We find that the estimated coefficients on the interaction between *Treat* and *Female* are negative and statistically significant for child height for age (HFA) and weight for age (WFA), indicating that exposure to the ban led to an increase in the gender gap in malnutrition. Girls born after the ban are relatively shorter compared to boys born to the same mother. The estimate for body mass index (BMI) is negative but statistically insignificant. The effects are larger in the sample of children with birth order 2 and above (Columns 1, 2 and 3). Here the difference between girls and boys' z score for height for age decreases by 0.17 standard deviations with exposure to the ban. The corresponding figures for weight for age and BMI are 0.16 and 0.09 standard deviations, which are about 6-9 percent of the corresponding mean values of these variables. The bootstrapped p value for the interaction terms (clustered at the state level) are included in the bottom panel. Our estimates are significant at the 5 percent level.

The coefficient for female child is positive, implying that girls have better nutrition scores than boys, which is likely to reflect the biological phenomenon that very young boys are more vulnerable as compared to girls. The presence of gender-based discrimination, however, tilts favourable health outcomes towards boys, relative to girls. We additionally show our results are robust to running the same specification for the entire period from 1985-2016. These results are available on request.

Table 8 presents the treatment effects on health investments such as breastfeeding duration in months for the child, number of antenatal care visits and number of tetanus shots received by the mother. As before, we control for mother fixed effects in addition to birth year fixed effects and state-specific time trends. The sample includes children born between 1988 and 2005. Columns 1, 2 and 3 present the estimates for birth orders 2 and above and columns 4, 5 and 6 present the estimates for all birth orders.

For birth orders 2 and above, the estimated coefficients for months of breastfeeding and number of tetanus shots are negative and significant, implying that there is a relative worsening of these health investments for girls after the implementation of the ban, as compared to boys born to the same mother. The estimate for antenatal care visits is not statistically significant. Here, as well, we find that the estimates go up in magnitude for birth order 2 and above. The effect size of the decline in immunisation is around 1.72 percent of the mean of ante-natal care visits, 1.92 percent for tetanus vaccines and around 1 percent for breastfeeding time.

5.2 Impact of the BBBP intervention on child mortality

5.2.1 Child mortality

Table 9 presents the mortality results of the BBBP policy in the mother fixed effects regression framework. We restrict the sample to children born after 2005, up to ten years before the implementation of the programme, so as to ensure closer comparability between our control group and the treatment group. Columns 1, 2 and 3 present the estimates for children of birth orders 2 and above and columns 4, 5 and 6 present the estimates for all birth orders.

The coefficient on the interaction term between *Treat* and *Female* is negative but statistically insignificant for neonatal mortality. When we restrict the sample to birth order 2 and above, this estimate is of similar size and still insignificant. One reason for this could be that the sample of exposed children includes a large number of children who have only been partially exposed to the policy. As we discuss in the previous section, the treated cohorts for neonatal, infant and under-five mortality include those children who would have had any exposure to the policy during the period of the first month, first year and first five years from birth respectively.

Next, we examine if the policy differentially affects outcomes in firstborn female families. Table 10 presents the mortality results (neonatal, infant and child mortality) for the BBBP policy, where the main coefficient of interest is the coefficient on the interaction between *Treat* and an indicator for a firstborn female family. The sample is restricted to children born after 2005. Since we are using the firstborn female indicator we restrict our sample to children with birth order 2 and above. The triple interaction term is positive and insignificant for infant and under-5 mortality. It is negative and significant for neonatal mortality, implying that neonatal mortality was lower for a female firstborn family after the implementation of the intervention, suggesting that the BBBP policy had positive health impacts for children.

5.2.2 Health outcomes and interventions

Table 11 presents the estimates for the BBBP policy on child height for age (HFA), weight for age (WFA) and body mass index (BMI). The gender sensitisation campaigns carried out under the programme could influence household investments for children that can plausibly reduce mortality, improve health investments and thereby health outcomes. The estimated coefficients of interest are the coefficients on the interaction between Treat and Female. The sample is restricted to children born in or after 2005. Columns 1, 2 and 3 present the estimates for birth orders 2 and above and columns 4, 5 and 6 present the estimates for all birth orders. The specification includes mother fixed effects, birth year fixed effects, and state-specific time trends. The post-treatment variable takes the value 1 for children below the age of one year during the survey.

For birth orders 2 and above, the coefficient on *Treat* × *Female* is positive for HFA but insignificant. However, for the sample without any birth order restrictions, the coefficients are negative while being statistically insignificant for all outcomes. These null results contrast with the adverse welfare consequences noted for girls following the implementation of the ban on sex-screening and sex selection, discussed in the previous section.

Table 12 presents the treatment effects of the BBBP policy on child height for age (HFA), weight for age (WFA) and body mass index (BMI), by the firstborn female family indicator.

The sample includes children born in or after 2005. Child's birth order is restricted to 2 and above.

The coefficient on this interaction is positive for all outcomes, and statistically significant for height for age and weight for age. This implies that children born into female firstborn families gain relatively more in terms of anthropometric outcomes after the implementation of the policy in phase 1 districts. Given that these are the families which are likely to engage in gender-biased discrimination in an environment where they have limited access to sex-screening technologies, our results suggest that the media campaign mitigated the negative welfare impact of the ban on sex selection by improving outcomes in firstborn girl families.

We present the treatment effects of the BBBP policy on duration of breastfeeding, using the mother fixed effects regression framework for the sample of children born between 2005 and 2016, in Table 13, by birth order and age. For birth orders 2 and above and children under the age of 24 months, the coefficients on the interaction between *Treat* and *Female* are positive and statistically significant for months of breastfeeding, implying that relative health investments improve for female children with higher birth order after the intervention in phase 1 districts. We further check this effect is higher for females in the firstborn female families.[8]

In sum, there is evidence that the significant negative effects of the ban on sex screening are either mitigated or outright reversed in the case of the media intervention, suggesting significant benefits from the implementation of a gender-equity focused policy with a strong demand-side component.

6 Mechanisms

6.1 Proportion of girls born

To explore the mechanism behind the estimated impacts of both policies, we estimate the effect of both policies on the proportion of female births to all births that have taken place to a given mother by any given year. If the legal ban had any impact on preventing sex-selection, we should see an increase in the proportion of female births after the ban on average (who are more likely to be unwanted and thus face lower investments in the face of

[8]This result is available on request.

existing son-bias). To examine this channel, we estimate the following equation:

$$Y_{mst} = \beta_0 + \beta_1 \text{Treat}_{mst} + \beta_2 X_{mst} + \delta_{st} + \tau_t + \epsilon_{mst} \quad (4)$$

where Y_{mst} is the proportion of girls born to mother m in state s in the year t. We include birth year fixed effects (τ_t) and state-specific time trends (δ_{st}). The set of controls X_{mst} included are child's birth order, sibling size, mother's education, mother's current age, mother's age at birth, mother's weight for height, religion, caste, living in an urban area, total number of household members, household wealth, and family structure. The variable $Treat$ has the same interpretation as before.

These results are presented in Table 14. We find that the ban on sex selection was followed by an increase by around 2 percentage in the proportion of female births, validating that the ban did have bite and is the key channel that explains our results on enhanced gender discrimination. The BBBP programme has so far not had an impact on female births, but we are only able to consider short-term effects.

6.2 Impact on fertility

One possible mechanism for the results we see is that families with a strong son preference will pursue a fertility stopping rule in absence of access to technologies that will allow them to select the sex of their children (Jayachandran and Kuziemko, 2011). Such families will continue to have children until they achieve their desired sex composition. The corresponding increase in the size of the family will mechanically lead to greater sibling competition for scarce household resources, resulting in reduced health investments per child, and potential adverse impacts on the health of their children. Following the results of Anukriti et al. (2020) we test whether firstborn female families are more likely to see a disproportional increase in fertility following the ban. To investigate the effects of the policies on fertility, we use a similar estimation framework as (1). We compare families with firstborn females to families with firstborn males to identify differential effects on the probability of a birth of a child of any gender.

Specifically, we test if fertility increases by relatively more in firstborn female families as compared to firstborn male families after the implementation of the policy. To provide a comparison of the relative effects of different policy shocks on fertility, we provide estimates of the impact of the ban on sex-selective abortions along with the BBBP campaign. We also benchmark these treatment effects against the impact of wider accessibility to ultrasound through the "early diffusion" period as defined in Anukriti et al. (2020). We interact the

$Treat_{mst}$ variable with an indicator of whether the family has a firstborn daughter to run the following estimation:

$$
\begin{aligned}
Y_{mst} &= \beta_0 + \beta_1 \text{Treat}_{mst} + \beta_2 \text{FirstbornFemale}_{mst} + \\
&+ \beta_3 (\text{Treat} \times \text{FirstbornFemale})_{mst} + \beta_4 X_{mst} + \tau_t + \delta_{st} + \phi_m + \epsilon_{mst} \qquad (5)
\end{aligned}
$$

where Y_{mst} is an indicator for fertility for mother m in state s in year t. We include birth year fixed effects (τ_t) and state-specific time trends (δ_{st}) and a set of controls X_{mst} include mothers education, mothers age, total children born, religion, caste, living in an urban area, total number of household members, and family structure.

In (5), we include birth year fixed effects (τ_t). For PNDT regressions we additionally include state-specific time trends δ_{st} for BBBP regressions we include district-specific time trends for BBBP .

The main coefficient of interest is β_3, the coefficient on the interaction between an indicator for a firstborn female family and treatment by the respective policy. We follow the definition of the ultrasound technology diffusion period as in Anukriti et al. (2020). *Early Diffusion* equals 1 if a child is born in or after year 1985 and before 1995 and 0 otherwise. *Treat PNDT* takes the value 1 if the PNDT was implemented in a child's state by the time of her birth.

Table 15 compares and contrasts the estimated impact of the the legal ban with that of the early diffusion of ultrasound technology on fertility with the exclusion of mother fixed effects.

We find that the coefficient on *Early Diffusion* × *Firstborn Female* is negative and significant. This is in line with the results of Anukriti et al. (2020) implying that firstborn female families see a greater reduction in probability of birth after increased access to ultrasound technology during the 1985-1995 period. The estimated coefficient for *PNDT* × *Firstborn Female* is positive and highly significant (at one percent), implying that the probability of birth goes up relatively more for a female firstborn family (an increase of about 0.4 percent of the mean), after the implementation of the ban on sex screening and sex-selective abortions. These results are symmetric and in opposite directions, suggesting that the impact of the diffusion of ultrasound technology on fertility was offset by the impact of the ban on the use of such technology to pursue sex-selective abortions.

7 Discussion

Enforcing a ban on sex screening and sex selection has been one of the primary policy responses to deal with male-skewed sex ratios in patriarchal countries like China, India and South Korea. The extent to which these supply-side restrictions on the tools of sex selection work in changing discrimination against girls has been relatively understudied in the literature. In particular, if there is no change to demand-side parameters affecting parental son preference, will households that are prevented from undertaking sex-selective abortions shift from prenatal sex discrimination to postnatal discrimination against females?

A large body of evidence documents the manifestation of son preference in the form of discriminatory practices by parents (Das Gupta and Mari Bhat, 1997; Das Gupta, 2019; Jayachandran and Kuziemko, 2011) and son-biased fertility stopping behaviour (Bhalotra and Cochrane, 2010b; Bhat and Zavier, 2003; Jensen, 2003). Using a natural experiment generated by the staggered roll out of a legal ban on sex-selective abortions in India, we find evidence of increased postnatal discrimination in surviving children along both these margins. We find while the ban did increase the probability of girls being born, the surviving girls faced relatively higher levels of mortality and lower health investments compared to boys. Using quasi-random variation in the sex of the firstborn child, we find the mortality and health outcomes relatively worsen for children in firstborn female families who are likely to be more intensively treated by the ban. This goes hand in hand with the finding that fertility increases relatively more in firstborn female families after the ban. We contrast these effects on fertility with the impact of early diffusion of ultrasound technology on fertility and confirm that the impact of the ban on sex-selective abortions is symmetrically opposite to the effect of the advent of ultrasound technology.

Our analysis shows that the ban on sex-selective abortions significantly increased postnatal gender discrimination against girl children in India. We find that gender gaps become higher after the ban for neonatal, infant and under five child mortality, consistent with an increasing gender gap in postnatal health investments. We also find an increase in the probability of births and a reduction in birth spacing in households with a firstborn female relative to households with a firstborn male. While a higher number of females are born, they are treated unequally compared to boys, leading to lower survival rates and lower parental investments such as breastfeeding and immunisations. This is consistent with a pattern of fertility stopping behaviour by families affected by the ban, particularly firstborn girl families, which has adverse welfare consequences on surviving children, as a result of the quantity-quality trade-off.

We contrast these findings with the treatment effects of a policy initiative that combines both supply-side restrictions on access to sex-selection technology as well as complementary demand-side measures aimed at increasing the perceived value of female children. We find that the significant negative effects of the ban on sex-selective abortions are either mitigated or outright reversed in the presence of strong demand-side components in the policy.

Our results challenge the unambiguously welfare-improving assessment of legal bans on sex selection. In particular, it is important to recognise that top-down supply-side policies, in the face of hardwired social norms that drive son preference, have serious unintended welfare consequences for the health and mortality of surviving females. This is even more of a concern if we factor in the dynamic nature of human capital formation, where early-life deficits can reinforce lower investments in the life-cycle of an individual (Heckman et al., 2006; Currie and Vogl, 2013).

Of course, making substantial shifts in deep-rooted social norms can be difficult. In that context, our study also informs the design of complementary policies that can ameliorate the adverse impacts of supply-side measures. What is clear is that persisting with punitive measures against sex-selective abortions alone does increase female survival but adversely affects discrimination on other fronts. We build on a small but growing body of evidence which indicates that top-down approaches around fertility choice may not yield desired results and can backfire unless there is a change in the parameters that are at the roots of gender discrimination.

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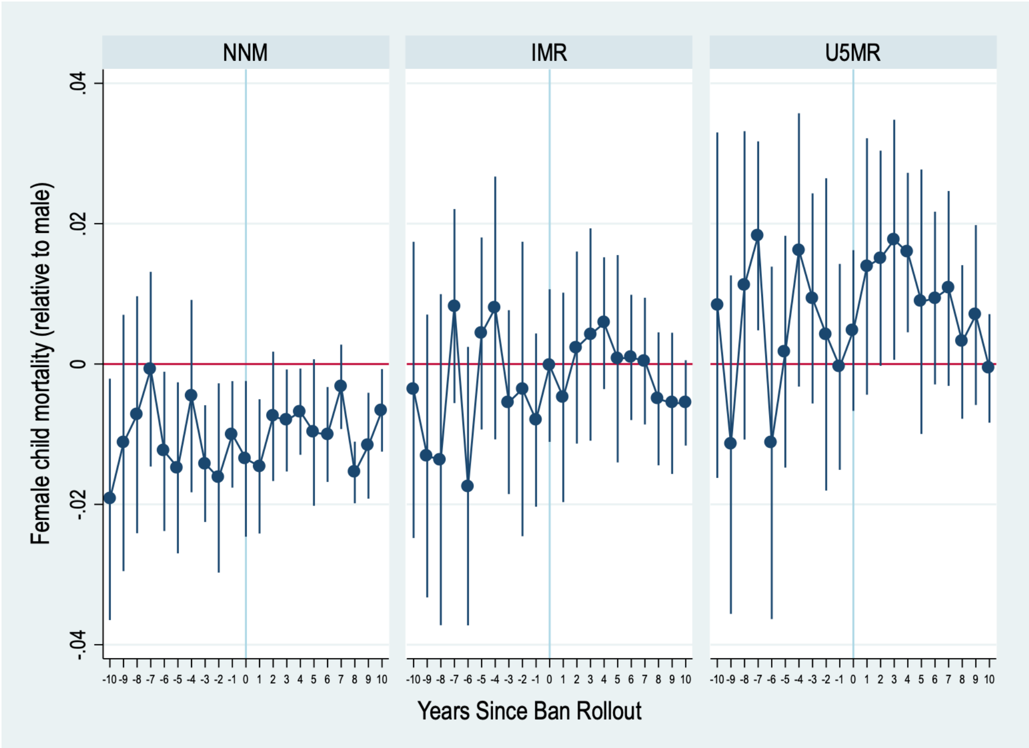

Figure 1: The estimates plotted are from the regression of child mortality on the interaction between $Treat$ and $Female$, for 10 years before and after the ban. We plot neonatal mortality (NNM) in panel 1, infant mortality (IMR) in panel 2 and under 5 mortality (U5MR) in panel 3. Neonatal Mortality (NNM) takes the value 1 if a child died before completing 1 month, 0 otherwise. Infant Mortality (IMR) takes the value 1 if the child died before completing 1 year, 0 otherwise. Under-five mortality (U5MR) takes the value 1 if a child died before completing 5 years, 0 otherwise. Birth orders are restricted to 2 and above. Additional controls include state-specific time trends, child birth year, birth order and mother fixed effects. Standard errors are clustered at the state level. The plot displays 95 percent confidence intervals.

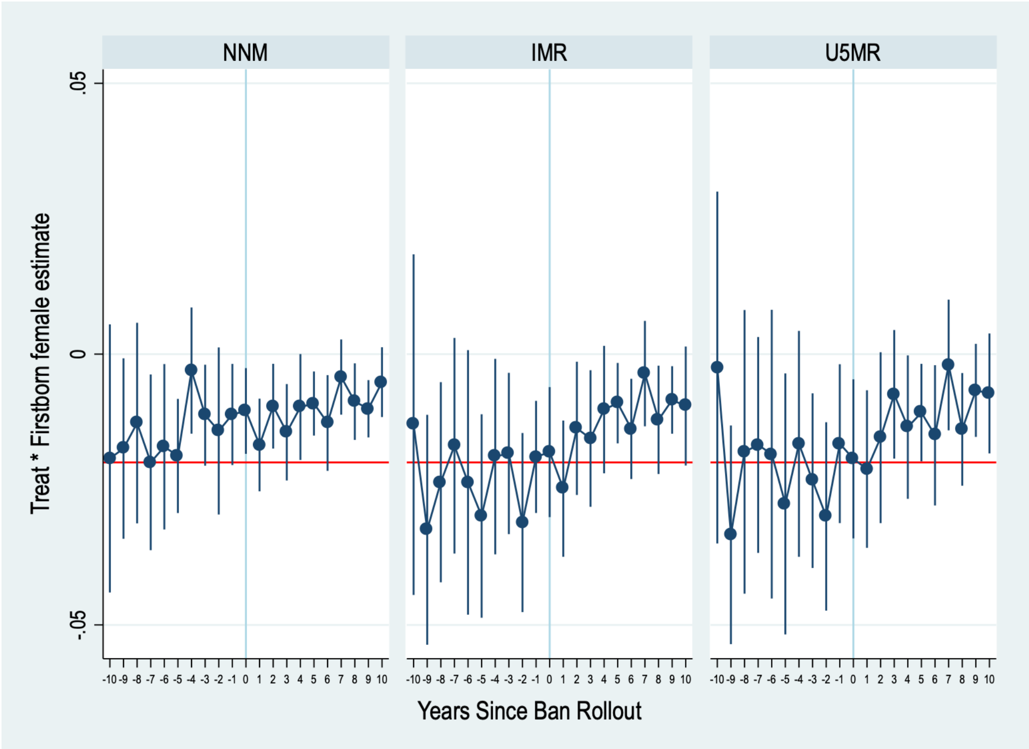

Figure 2: The estimates plotted are from the regression of child mortality on the interaction between $Treat$ and $FirstbornFemale$, for 10 years before and after the ban. We plot neonatal mortality (NNM) in panel 1, infant mortality (IMR) in panel 2 and under 5 mortality (U5MR) in panel 3. Neonatal Mortality (NNM) takes the value 1 if a child died before completing 1 month, 0 otherwise. Infant Mortality (IMR) takes the value 1 if the child died before completing 1 year, 0 otherwise. Under-5 mortality (U5MR) takes the value 1 if a child died before completing 5 years, 0 otherwise. Birth orders are restricted to 2 and above. Additional controls include state-specific time trends, child birth year, birth order and mother fixed effects. Standard errors are clustered at the state level. The plot displays 95 percent confidence intervals.

Table 1: Differential impact of PNDT on the gender gap in child mortality (with mother fixed effects)

	Birth orders 2 and above			All birth orders		
	(1)	(2)	(3)	(4)	(5)	(6)
	NNM	IMR	U5MR	NNM	IMR	U5MR
Treat	0.000379	0.000514	-0.00269	-0.00385	-0.00314	-0.00415*
	(0.00261)	(0.00195)	(0.00250)	(0.00262)	(0.00225)	(0.00242)
Female	-0.0161***	-0.00994***	-0.00428	-0.0162***	-0.0139***	-0.0100***
	(0.00160)	(0.00266)	(0.00410)	(0.00139)	(0.00183)	(0.00309)
Treat × Female	0.0108***	0.00939***	0.00818**	0.00784***	0.00905***	0.00778***
	(0.00212)	(0.00317)	(0.00380)	(0.00181)	(0.00208)	(0.00273)
Bootstrapped p value	0.000500	0.0128	0.0586	0.00350	0.00110	0.00930
Observations	367006	367006	367006	648798	648798	648798
Mean of Dep. Variable	0.0455	0.0683	0.0803	0.0437	0.0628	0.0723
SD	0.208	0.252	0.272	0.205	0.243	0.259

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 the child died before completing 1 year, 0 otherwise. Under-5 mortality (U5MR) takes the value 1 if a child died before completing 5 years, 0 otherwise. Children who have not attained these respective ages are excluded from the regression. $Treat$ takes the value 1 if the PNDT Act has been implemented in the state in which a child is located, 0 otherwise. $Female$ takes the value 1 if the child's gender is female, 0 otherwise. All estimations include mother fixed effects, birthyear and birth order fixed effects, and state-specific time trends. The sample includes children born between 1985 and 2005. The first three columns include children born at a birth order of two or higher, while the last three columns include children born at all birth orders. Standard errors are clustered at the state level. The wild cluster bootstrap p values for the interaction terms are included in "bootstrapped p value".

Table 2: Differential impact of PNDT on the gender gap in child mortality (without mother fixed effects)

	Birth orders 2 and above			All birth orders		
	(1)	(2)	(3)	(4)	(5)	(6)
	NNM	IMR	U5MR	NNM	IMR	U5MR
Treat	0.00416**	0.00105	0.00216	0.000458	-0.00108	-0.000790
	(0.00180)	(0.00180)	(0.00237)	(0.00160)	(0.00157)	(0.00170)
Female	-0.0174***	-0.0158***	-0.0131***	-0.0212***	-0.0220***	-0.0201***
	(0.00170)	(0.00198)	(0.00332)	(0.00194)	(0.00189)	(0.00280)
Treat × Female	0.00800***	0.00759**	0.00654*	0.00717***	0.00793***	0.00674**
	(0.00185)	(0.00299)	(0.00358)	(0.00189)	(0.00254)	(0.00285)
Bootstrapped p value	0.00230	0.0433	0.109	0.00360	0.0151	0.0393
Observations	439339	439339	439339	693628	693628	693628
Mean of Dep. Variable	0.0385	0.0578	0.0680	0.0414	0.0594	0.0682
SD	0.192	0.233	0.252	0.199	0.236	0.252

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 the child died before completing 1 year, 0 otherwise. Under-5 mortality (U5MR) takes the value 1 if a child died before completing 5 years, 0 otherwise. Children who have not attained these respective ages are excluded from the regression. $Treat$ takes the value 1 if the PNDT Act has been implemented in the state in which a child is located, 0 otherwise. $Female$ takes the value 1 if the child's gender is female, 0 otherwise. All estimations include birthyear fixed effects, and state-specific time trends. The set of controls included are child's birth order, sibling size, mother's age at birth, whether the mother and father have completed primary education, mother's weight for height, religion, caste, whether the house is located in an urban area, number of members in the household, household wealth index, number and sex composition of adults in the household. The sample includes children born between 1985 and 2005. The first three columns include children born at a birth order of two or higher, while the last three columns include children born at all birth orders. Standard errors are clustered at the state level. The wild cluster bootstrap p values for the interaction terms are included in "bootstrapped p value".

Table 3: Differential impact of PNDT on the gender gap in child mortality (without JK and Maharashtra and with mother fixed effects)

	Birth orders 2 and above			All birth orders		
	(1)	(2)	(3)	(4)	(5)	(6)
	NNM	IMR	U5MR	NNM	IMR	U5MR
Treat	-0.0283	-0.115*	-0.218***	0.0192	-0.0196	-0.0894
	(0.0485)	(0.0595)	(0.0793)	(0.0522)	(0.0711)	(0.0880)
Female	-0.0160***	-0.00964***	-0.00392	-0.0163***	-0.0139***	-0.00998***
	(0.00161)	(0.00268)	(0.00416)	(0.00143)	(0.00188)	(0.00319)
Treat × Female	0.0112***	0.0108***	0.00937**	0.00800***	0.00998***	0.00886***
	(0.00227)	(0.00329)	(0.00401)	(0.00198)	(0.00222)	(0.00286)
Bootstrapped p value	0.00120	0.00750	0.0413	0.00570	0.00130	0.00750
Observations	342644	342644	342644	603174	603174	603174
Mean of Dep. Variable	0.0473	0.0714	0.0841	0.0458	0.0662	0.0762
SD	0.212	0.258	0.278	0.209	0.249	0.265

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 the child died before completing 1 year, 0 otherwise. Under-5 mortality (U5MR) takes the value 1 if a child died before completing 5 years, 0 otherwise. Children who have not attained these respective ages are excluded from the regression. $Treat$ takes the value 1 if the PNDT Act has been implemented in the state in which a child is located, with the sample including all states except for JK and Maharashtra, 0 otherwise. $Female$ takes the value 1 if the child's gender is female, 0 otherwise. All estimations include mother fixed effects, birthyear and birth order fixed effects, and state-specific time trends. The sample includes children born between 1985 and 2005. The first three columns include children born at a birth order of two or higher, while the last three columns include children born at all birth orders. Standard errors are clustered at the state level. The wild cluster bootstrap p values for the interaction terms are included in "bootstrapped p value".

Table 4: Differential impact of PNDT on the gender gap in child mortality (without JK and Maharashtra and without mother fixed effects)

	Birth orders 2 and above			All birth orders		
	(1)	(2)	(3)	(4)	(5)	(6)
	NNM	IMR	U5MR	NNM	IMR	U5MR
Treat	-0.0586***	-0.0544**	-0.0624**	-0.0519**	-0.0503**	-0.0643***
	(0.0184)	(0.0227)	(0.0270)	(0.0212)	(0.0192)	(0.0202)
Female	-0.0175***	-0.0160***	-0.0133***	-0.0215***	-0.0225***	-0.0205***
	(0.00173)	(0.00209)	(0.00350)	(0.00200)	(0.00199)	(0.00298)
Treat × Female	0.00898***	0.00976***	0.00891**	0.00818***	0.00995***	0.00896***
	(0.00179)	(0.00268)	(0.00333)	(0.00183)	(0.00220)	(0.00250)
Bootstrapped p value	0.000200	0.000100	0.0133	0.00150	0.000500	0.000400
Observations	409158	409158	409158	644628	644628	644628
Mean of Dep. Variable	0.0402	0.0607	0.0715	0.0434	0.0625	0.0719
SD	0.196	0.239	0.258	0.204	0.242	0.258

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 the child died before completing 1 year, 0 otherwise. Under-5 mortality (U5MR) takes the value 1 if a child died before completing 5 years, 0 otherwise. Children who have not attained these respective ages are excluded from the regression. $Treat$ takes the value 1 if the PNDT Act has been implemented in the state in which a child is located, 0 otherwise. $Female$ takes the value 1 if the child's gender is female, 0 otherwise. All estimations include birthyear fixed effects, and state-specific time trends. The sample excludes the states JK and Maharashtra. The sample includes children born between 1985 and 2005. The first three columns include children born at a birth order of two or higher, while the last three columns include children born at all birth orders. Standard errors are clustered at the state level. The wild cluster bootstrap p values for the interaction terms are included in "bootstrapped p value".

Table 5: Differential impact of the PNDT on child mortality by families with firstborn females (with mother fixed effects)

	Birth orders 2 and above			All birth orders		
	(1)	(2)	(3)	(4)	(5)	(6)
	NNM	IMR	U5MR	NNM	IMR	U5MR
Treat	0.000728	-0.00282	-0.00618**	-0.00960***	-0.0106***	-0.0115***
	(0.00333)	(0.00329)	(0.00297)	(0.00308)	(0.00324)	(0.00332)
Treat × Firstborn Female	0.00923***	0.0155***	0.0149***	0.0190***	0.0238***	0.0226***
	(0.00315)	(0.00338)	(0.00375)	(0.00179)	(0.00271)	(0.00341)
Bootstrapped p value	0.0223	0.000900	0.00520	0	0	0
Observations	367006	367006	367006	648798	648798	648798
Mean of Dep. Variable	0.0455	0.0683	0.0803	0.0437	0.0628	0.0723
SD	0.208	0.252	0.272	0.205	0.243	0.259

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 the child died before completing 1 year, 0 otherwise. Under-5 mortality (U5MR) takes the value 1 if a child died before completing 5 years, 0 otherwise. Children who have not attained these respective ages are excluded from the regression. $Treat$ takes the value 1 if the PNDT Act has been implemented in the state in which a child is located, 0 otherwise. $FirstbornFemale$ takes value 1 if firstborn child is female and 0 otherwise. All estimations include mother fixed effects, birthyear and birth order fixed effects, and state-specific time trends. The sample includes children born between 1985 and 2005. The first three columns include children born at a birth order of two or higher, while the last three columns include children born at all birth orders. Standard errors are clustered at the state level. The wild cluster bootstrap p values for the interaction terms are included in "bootstrapped p value".

Table 6: Differential impact of the PNDT on child mortality by families with firstborn females (without mother fixed effects)

	(1) NNM	(2) IMR	(3) U5MR
Treat	0.00489**	0.000487	0.00131
	(0.00194)	(0.00204)	(0.00229)
Firstborn Female	-0.0150***	-0.0201***	-0.0212***
	(0.00160)	(0.00217)	(0.00260)
Treat × Firstborn Female	0.00587***	0.00820***	0.00785***
	(0.00153)	(0.00212)	(0.00246)
Bootstrapped p value	0.00900	0.00850	0.0211
Observations	439339	439339	439339
Mean of Dep. Variable	0.0385	0.0578	0.0680
SD	0.192	0.233	0.252

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 the child died before completing 1 year, 0 otherwise. Under-5 mortality (U5MR) takes the value 1 if a child died before completing 5 years, 0 otherwise. Children who have not attained these respective ages are excluded from the regression. $Treat$ takes the value 1 if the PNDT Act has been implemented in the state in which a child is located, 0 otherwise. $FirstbornFemale$ is defined at the family level, taking value 1 if firstborn child is female and 0 otherwise. The set of controls included are child's birth order, sibling size, mother's age at birth, whether the mother and father have completed primary education, mother's weight for height, religion, caste, whether the house is located in an urban area, number of members in the household, household wealth index, number and sex composition of adults in the household. All estimations include birthyear fixed effects, and state-specific time trends. The sample includes children born between 1985 and 2005 and children with birth order of two or above. Standard errors are clustered at the state level. The wild cluster bootstrap p values for the interaction terms are included in "bootstrapped p value".

Table 7: Differential impact of the PNDT on the gender gap in child health outcomes (with mother fixed effects)

	Birth orders 2 and above			All birth orders		
	(1)	(2)	(3)	(4)	(5)	(6)
	HFA	WFA	BMI	HFA	WFA	BMI
Treat	2.498***	1.350***	-0.102	2.664***	1.491***	-0.284
	(0.684)	(0.181)	(0.727)	(0.489)	(0.196)	(0.574)
Female	0.122	0.0690	0.0392	0.123*	0.0699	0.0335
	(0.0982)	(0.0617)	(0.0391)	(0.0662)	(0.0495)	(0.0349)
Treat × Female	-0.171**	-0.162***	-0.0983	-0.125**	-0.113**	-0.0712
	(0.0738)	(0.0551)	(0.0696)	(0.0568)	(0.0455)	(0.0453)
Bootstrapped p value	0.0278	0.0361	0.255	0.0501	0.0338	0.165
Observations	14244	14244	14244	23779	23779	23779
Mean of Dep. Variable	-2.257	-1.970	-0.727	-2.149	-1.883	-0.713
SD	1.705	1.231	1.357	1.667	1.224	1.349

Standard errors in parentheses

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

Note: HFA refers to child's height-for-age z-score. WFA refers to child's Weight-for-age z-score. BMI refers to child's body mass index z-score. *Treat* takes the value 1 if the PNDT Act has been implemented in the state in which a child is located, 0 otherwise. *Female* takes the value 1 if the child's gender is female, 0 otherwise. All estimations include mother fixed effects, birthyear and birth order fixed effects, and state-specific time trends. The sample includes children born between 1988 and 2005. The first three columns include children born at a birth order of two or higher, while the last three columns include children born at all birth orders. Standard errors are clustered at the state level. The wild cluster bootstrap p values for the interaction terms are included in "bootstrapped p value".

Table 8: Differential impact of the PNDT on the gender gap in child health investments (with mother fixed effects)

	Birth orders 2 and above			All birth orders		
	(1)	(2)	(3)	(4)	(5)	(6)
	ANC	TET	Breastfed	ANC	TET	Breastfed
Treat	-0.929**	-1.306***	-13.91***	-1.134**	-0.294	-14.06***
	(0.449)	(0.375)	(2.103)	(0.475)	(0.247)	(1.658)
Female	-0.114***	-0.0179	0.0210	-0.0832***	-0.0295**	-0.000672
	(0.0266)	(0.0120)	(0.0883)	(0.0155)	(0.0115)	(0.0979)
Treat × Female	0.0543	-0.0369**	-0.424*	-0.0409	-0.0269**	-0.126
	(0.0596)	(0.0136)	(0.223)	(0.0366)	(0.0121)	(0.161)
Bootstrapped p value	0.445	0.0250	0.0817	0.289	0.0404	0.437
Observations	18984	18677	26974	30347	29908	44022
Mean of Dep. Variable	1.896	1.222	12.81	2.388	1.400	12.64
SD	2.503	1.190	9.121	2.873	1.196	9.061

Standard errors in parentheses

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

Note: "ANC" refers to number of ante-natal care visits that took place in utero, top-coded at 20. "TET" refers to number of tetanus injections the mother received before birth. "Breastfed" refers to duration of breastfeeding in months. $Treat$ takes the value 1 if the PNDT Act has been implemented in the state in which a child is located, 0 otherwise. $Female$ takes the value 1 if the child's gender is female, 0 otherwise. All estimations include mother fixed effects, birthyear and birth order fixed effects, and state-specific time trends. The sample includes children born between 1988 and 2005. The first three columns include children born at a birth order of two or higher, while the last three columns include children born at all birth orders. Standard errors are clustered at the state level. The wild cluster bootstrap p values for the interaction terms are included in "bootstrapped p value".

Table 9: Impact of BBBP programme on gender gap in child mortality (with mother fixed effects)

	Birth orders 2 and above			All birth orders		
	(1)	(2)	(3)	(4)	(5)	(6)
	NNM	IMR	U5MR	NNM	IMR	U5MR
Female	-0.0130***	-0.00833***	-0.000620	-0.0148***	-0.0132***	-0.00864***
	(0.00148)	(0.00193)	(0.00317)	(0.00107)	(0.00135)	(0.00217)
Treat	0.00840	-0.0434	-0.0524**	0.0273*	-0.0212	-0.0417***
	(0.0222)	(0.0285)	(0.0215)	(0.0146)	(0.0206)	(0.0148)
Treat × Female	-0.0372	0.0278	0.0121	-0.0301	0.00663	-0.000185
	(0.0257)	(0.0422)	(0.0377)	(0.0185)	(0.0269)	(0.0240)
Observations	244567	222734	124308	449230	405111	213497
Mean of Dep. Variable	0.0448	0.0663	0.110	0.0390	0.0563	0.0982
SD	0.207	0.249	0.313	0.194	0.231	0.298

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 the child died before completing 1 year, 0 otherwise. Under-5 mortality (U5MR) takes the value 1 if a child died before completing 5 years, 0 otherwise. $Treat$ takes the value 1 if the child belongs to one of the 100 districts in which the BBBP scheme was introduced and if the child is born after June 2015 for NNM, after June 2014 for IMR, and after June 2010 for U5MR regressions, 0 otherwise. $Female$, defined at the child level, takes value 1 if child's gender is female, 0 otherwise. All estimations include mother fixed effects, birthyear and birth order fixed effects, and state-specific time trends. The sample includes children born between 2005 and 2016. The first three columns include children born at a birth order of two or higher, while the last three columns include children born at all birth orders. Standard errors are clustered at the district level.

Table 10: Impact of BBBP programme on gender gap in child mortality by firstborn female family (with mother fixed effects)

	(1) NNM	(2) IMR	(3) U5MR
Treat	0.0290	-0.0376	-0.0515*
	(0.0290)	(0.0295)	(0.0275)
Treat × Firstborn female	-0.0613**	0.0102	0.00718
	(0.0301)	(0.0322)	(0.0310)
Observations	244567	222734	124308
Mean of Dep. Variable	0.0448	0.0663	0.110
SD	0.207	0.249	0.313

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 the child died before completing 1 year, 0 otherwise. Under-5 mortality (U5MR) takes the value 1 if a child died before completing 5 years, 0 otherwise. $Treat$ takes the value 1 if the child belongs to one of the 100 districts in which the BBBP scheme was introduced and if the child is born after June 2015 for NNM, after June 2014 for IMR, and after June 2010 for U5MR regressions, 0 otherwise. $FirstbornFemale$ takes value 1 if firstborn child is female and zero otherwise, 0 otherwise. All estimations include mother fixed effects and birthyear and birth order fixed effects. The sample includes children born between 2005 and 2016. The sample includes children of birth orders 2 and above. Standard errors are clustered at the district level.

Table 11: Impact of BBBP programme on child health outcomes (with mother fixed effects)

	Birth orders 2 and above			All birth orders		
	(1)	(2)	(3)	(4)	(5)	(6)
	HFA	WFA	BMI	HFA	WFA	BMI
Female	0.575***	0.359**	0.0923	0.332***	0.177*	0.0242
	(0.158)	(0.154)	(0.148)	(0.116)	(0.101)	(0.108)
Treat	0.145	0.0434	-0.149	0.378	0.213	-0.0217
	(0.490)	(0.356)	(0.395)	(0.316)	(0.228)	(0.267)
Treat × Female	0.0409	-0.0791	-0.0812	-0.104	-0.279	-0.327
	(0.528)	(0.406)	(0.459)	(0.393)	(0.274)	(0.325)
Observations	2220	2220	2220	4697	4697	4697
Mean of Dep. Variable	-1.558	-1.748	-1.127	-1.348	-1.548	-1.024
SD	1.840	1.355	1.503	1.780	1.293	1.475

Standard errors in parentheses

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

Note: HFA refers to child's height-for-age z-score. WFA refers to child's Weight-for-age z-score. BMI refers to child's body mass index z-score. *Treat* takes the value 1 if the child belongs to one of the 100 districts in which the BBBP scheme was introduced and if the child is born after June 2014, 0 otherwise. *Female*, defined at the child level, takes the value 1 if child's gender is female, 0 otherwise. All estimations include mother fixed effects, birthyear and birth order fixed effects, and state-specific time trends. Sample includes children born from 2005-2016, and children below the age of 1 at the time of the survey. The first three columns include children born at a birth order of two or higher, while the last three columns include children born at all birth orders. Standard errors are clustered at the district level.

Table 12: Impact of BBBP programme on child health outcomes by firstborn female family (with mother fixed effects)

	(1)	(2)	(3)
	HFA	WFA	BMI
Treat	-0.522*	-0.653**	-0.443
	(0.306)	(0.270)	(0.310)
Treat × Firstborn Female	1.011**	0.897***	0.284
	(0.433)	(0.305)	(0.394)
Observations	2220	2220	2220
Mean of Dep. Variable	-1.558	-1.748	-1.127
SD	1.840	1.355	1.503

Standard errors in parentheses

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

Note: HFA refers to child's height-for-age z-score. WFA refers to child's Weight-for-age z-score. BMI refers to child's body mass index z-score. $Treat$ takes the value 1 if the child belongs to one of the 100 districts in which the BBBP scheme was introduced and if the child is born after June 2014, 0 otherwise. $FirstbornFemale$, defined at the mother level, takes the value 1 if the child belongs to a family with a firstborn girl child and 0 otherwise. All estimations include mother fixed effects, birthyear fixed effects, and state-specific time trends. All specifications include birth year, birth order and state fixed effects. Sample size includes children born in and after 2005, and children below age 1 and children of birth orders 2 and above. Standard errors are clustered at the district level.

Table 13: Differential impact of BBBP on the gender gap in breastfeeding, duration in months (with mother fixed effects)

	Children below 59 months		Children below 24 months	
	(1)	(2)	(3)	(4)
	Birth order 2 plus	All birth orders	Birth order 2 plus	All birth orders
Treat	1.583	1.583**	-5.947***	-1.262
	(1.172)	(0.691)	(1.372)	(1.031)
Female	-0.450*	-0.185	0.237	0.281
	(0.265)	(0.242)	(0.488)	(0.310)
Treat × Female	1.037	-0.173	4.711***	1.259
	(1.478)	(0.951)	(1.622)	(1.206)
Observations	8368	16930	254	580
Mean of Dep. Variable	5.193	5.064	1.376	1.557
SD	8.612	8.384	2.527	2.582

Standard errors in parentheses

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

Note: The dependent variable is breastfeeding, with duration in months. $Treat$ takes the value 1 if the child belongs to one of the 100 districts in which the BBBP scheme was introduced and if the child is born after June 2014, 0 otherwise. $Female$ takes the value 1 if the child's gender is female, 0 otherwise. All estimations include mother fixed effects, birthyear and birth order fixed effects. The sample includes children born in the year 2005 and above for BBBP. The first two columns include children below 59 months, and the next two include children below 24 months, with columns (1) and (3) for children of birth orders above 2 and columns (2) and (4) for children of all birth orders. Standard errors are clustered at the district level. The wild cluster bootstrap p values for the interaction terms are included in "bootstrapped p value".

Table 14: Impact of PNDT and BBBP on proportion of girls born to a mother

	(1) All birth orders	(2) Birth order 2 plus
Treat PNDT	0.0204^{***}	0.0306^{***}
	(0.00481)	(0.00629)
Observations	648798	367006
Mean of Dep. Variable	0.474	0.487
SD	0.499	0.500

Standard errors in parentheses

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

	(1) All birth orders	(2) Birth order 2 plus
Treat BBBP	-0.0261	-0.0379
	(0.0164)	(0.0256)
Observations	394932	212355
Mean of Dep. Variable	0.494	0.508
SD	0.500	0.500

Standard errors in parentheses

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

Note: The dependent variable is the proportion of girls to total children born to a mother, calculated as of any given year. $TreatPNDT$ is whether the PNDT Act is implemented in the state. $TreatBBBP$ takes the value 1 if the child belongs to one of the 100 districts in which the BBBP scheme was introduced and if the child is born after June 2015. All specifications include year fixed effects and state-specific time trends. For the PNDT regression, the sample includes children born between 1985 and 2005, and the regression for BBBP includes children born between 2005 and 2016. The first column presents the results for all birth orders while the second column presents the results for birth orders 2 and above. The set of controls included are child's birth order, sibling size, mother's education, mother's age, mother's age during birth, mother's weight for height, religion, caste, living in an urban area, total number of household members, household wealth, and family structure.

Table 15: Impact of different policy interventions on fertility (without mother fixed effects)

	(1) Early Diff	(2) PNDT
Treat EarlyDiff	0.0458	
	(0.0936)	
Firstborn Female	0.000488*	-0.00449***
	(0.000250)	(0.000559)
Treat EarlyDiff × Firstborn Female	-0.00424***	
	(0.000848)	
Treat PNDT		-0.00365***
		(0.00123)
Treat PNDT × Firstborn Female		0.00668***
		(0.000771)
Observations	7924625	7924625
Mean of Dep. Variable	0.0730	0.0730
SD	0.260	0.260

Standard errors in parentheses

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

Note: The dependent variable takes the value 1 if the woman gave birth in that year, and 0 otherwise. $TreatPNDT$ takes the value 1 if the PNDT Act is implemented in the state where the mother is located, 0 otherwise. $TreatEarlyDiff$ takes the value 1 if the woman gives birth between 1985 and 1994, and 0 if woman gives birth in or after 1995. $FirstbornFemale$ takes value 1 if the child belongs to a family with a firstborn girl child and 0 otherwise. The set of controls included are mothers education, mothers age, total children born, religion, caste, living in an urban area, total number of household members, household wealth, and family structure. Both specifications include birth year fixed effects and state-specific time trends. Standard errors are clustered at the state level.

Table A.1: Comparison with Nandi (2015) with birth order restriction

	(1) Without FE	(2) With FE
Treat	-0.0172***	-0.0147*
	(0.00408)	(0.00830)
Female	-0.0126***	-0.0118***
	(0.00336)	(0.00337)
Treat × Female	0.00822*	0.00670**
	(0.00425)	(0.00327)
Bootstrapped p value	0.0853	0.0482
Observations	55026	55026
Mean of Dep. Variable	0.0597	0.0567
SD	0.237	0.231

Standard errors in parentheses

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

Note: *Treat* is whether the PNDT Act is implemented in the state. *Female*, defined at the child level, takes value 1 if child's gender is female. This table follows the specification in Nandi (2015), wherein they regress the interaction of female index child and PNDT on infant mortality. This specification includes children with birth orders 2 and above. Column 1 includes no fixed effects while column 2 includes birth year, state fixed effects and state specific time trends. Controls include child's birth order, urban, number of household members, caste, religion, wealth index, whether the household is female headed, age of household head, mother and fathers primary education, mother's age at birth. Standard errors are clustered at the state level.

Appendix A Variable definitions

1. Set of objective biomarkers

   (a) Height for age z-score captures the height for age z-score value for surviving children born in three to five years before the survey. According to the WHO global database on child growth and malnutrition a height for age z score between -2 & -3 is characterized as moderate chronic malnutrition, while that below -3 corresponds to severe chronic malnutrition.

   (b) Weight for age z-score captures the weight for age z-score value for surviving children born in three to five years before the survey. Low child weight for age indicates acute/chronic malnutrition. According to WHO global database on child growth and malnutrition weight for age z-score between -2 & -3 corresponds to moderate malnutrition, while that below -3 corresponds to severe malnutrition.

   (c) Body Mass Index z-score measures the BMI for age z-score value for surviving children born in three to five years before the survey. BMI is a anthropometric measure which relates body weight to body height. This is calculated by dividing body wight (in kilograms) by height (meters) squared.

2. Indicators for health investments

   (a) The number of antenatal visits the women had while the child was in utero. The value of these visits were 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) This variable reports if and how many tetanus toxicoid vaccinations were given to mother while the child was in utero for children born in three to 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) Breastfeeding refers to months of breastfeeding for the children born in three to five years before the survey including the cases where (a) the child's mother was still breastfeeding at the interview time and (b) the child had been breastfed until his/her death. On a population basis, exclusive breastfeeding for 6 months is the best way of feeding infants, and after that infants should be continued with breastfeeding for up to 2 years of age or beyond along with complementary foods.