Can legal bans to sex detection technology reduce gender discrimination?

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Abstract

Bans on abortions and pre-natal sex detection, 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 first study whether a legal restriction on sex selection worsens the relative outcomes for girls as compared to boys for the same mother and, second, whether outcomes for children in firstborn female families relative to firstborn male families worsen when those children are exposed to the ban. Our findings indicate that the ban increased the gender gap in mortality, health outcomes and health investments. There are two main mechanisms explaining our results: an increase in discrimination against unwanted girls and an increase in fertility in intensively treated families, leading to greater competition among siblings for resources. We complement this analysis by contrasting these results with the impact of a policy that, in addition to strengthening supply-side measures, also contains demand-side elements by shifting social norms through a media intervention. Our evidence suggests potential usefulness of demand-side interventions that directly address underlying social norms in order to reduce gender bias.

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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, the practice of sex-selective abortions, and sometimes banning all 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., 2020b). 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 *without* shifting underlying social norms, like strong 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 girls to boys born 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 who are relatively "unwanted" could manifest through increased postnatal discrimination by households against such girls. Second, with ban on accessing sex selection technology, families are more likely to rely on fertility-stopping rules to attain the desired sex mix of their children, where girls mechanically face greater competition for resources since they will have more siblings (Clark, 2000).

Next, we ask how these estimated treatment effects of a supply-side policy, seeking to restrict access to abortion and to ultrasound technology, compare with a policy with demand-side effects, that seeks to shift underlying social norms determining the household demand for male and female children, through a gender sensitisation mass media campaign. If supply-side policies lead to increased postnatal gender discrimination, can a policy intervention with 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 question we compare girls and boys born to the same mother, before and after the ban, in treated versus control states.

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 abortions is relatively intensive among families with a firstborn girl (Bhalotra and Cochrane, 2010a; Anukriti et al., 2020b), and estimate the differential impact of being born in a treated state on families with firstborn girls, compared to firstborn boys. If families with firstborn girls are relatively intensively treated by the ban, leading to more births of all children, there should be a bigger impact on mortality, health outcomes and investments on children in such families.

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 in such families. In addition to a widening gender gap in health outcomes and mortality outcomes, it increased fertility in firstborn girl families compared to firstborn boy families.

We then compare these results with the treatment effects of a mass media campaign launched in 100 districts in India, aimed at increasing levels of gender sensitisation and reducing the demand for gender discrimination. This campaign was implemented 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 implementation of the media campaign did seem to reverse some of the costs of a 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. Previous studies have found that this Indian ban has 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 suggests there are no significant negative consequences of the ban (Nandi, 2015a). Our study improves upon this previous work by implementing a demanding empirical strategy, that controls for mother fixed effects and state-specific time trends, allowing us to control for a host of confounding factors arising from family-level heterogeneity and geographic variation in social norms. Further, we use a much more extensive and richer dataset, covering child births across 25 years that allows us to compare results of various policy disruptions around birth technology.

A similar study of the converse effect finds evidence of the improved health outcomes of girls relative to boys leading from the *increased* access to ultrasound technology in the period from 1985-1995 (Anukriti et al., 2020a). 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 girls, while the latter improves the relative odds of a female birth but leads to a widening of the gender gap in mortality and health.

We also provide the first estimates of the treatment effects of the mass media campaign – the Beti Bachao Beti Padhao (Save Girls, Educate Girls) programme – on mortality, health outcomes and health investments, allowing us to comment on the relative efficacy of a supply-side policy (legal restriction on access to ultrasound technology) with a policy that incorporates demand-side interventions seeking to shift the 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 potential usefulness of combining demand-side elements such as efforts to shift social norms along with supply-side policies that prohibit and prosecute gender-based discrimination. This could have an important bearing on policy design for countries struggling with extensive gender discrimination and 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 boys. First, governments have enacted bans on the use of sex detection technologies, accompanied with rigorous prosecution of technology providers. Second, governments have attempted to shift underlying social norms and perceptions about the worth of a girl child through media outreach and related interventions, with a view to enhance gender equality, improve sex ratios and health outcomes for girls, by reducing the demand for sex selection (Gupta, 2019; Guo et al., 2016). Third, governments have also offered 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 girls 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 (Gupta, 2019; Park and Cho, 1995). In particular, there is evidence that prenatal sex selection may help improve life chances of the girls that are born, as they are actively wanted by households that choose to have them. In India, Anukriti et al. (2020a) 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] Anukriti et al. (2020a) also find that families with firstborn girls are less likely to have larger families by following the fertility stopping rule, whereby they 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. 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.

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 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.

In this paper, we identify the impacts of both a supply-side intervention – a ban on the use of sex-screening technologies – and a demand-side intervention – a media campaign designed to shift social norms and preferences for girls – to compare and contrast the welfare consequences of each, both in terms of the impact on the sex ratio, as well as impacts on healthcare investments in girls by households, with direct effects on health outcomes such as 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.

[1] Conversely, a study in China 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) use an indicator for child having received any of the following vaccines - BCG, IPV, DPT and measles - to identify any association between ultrasound availability and a gender gap in vaccinations, and find insignificant effects. For months of breastfeeding, the introduction of ultrasound technology is associated with a 0.9 month decrease in breastfeeding duration for male children.

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 introduced in the 1970s. This 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., 2020a). 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; Bhalotra and Cochrane (2010b) estimate that the spread of ultrasound technologies led to the selective abortion of up to 480,000 girls between 1995-2005.

In response to the scale of sex-selective abortions believed to be taking place, sex determination 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 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 in 1994, which was brought into force in 1996. The state of Jammu and Kashmir enacted its own ban in 2002. A stricter version of the national PNDT Act was also introduced in 2002, banning sex selection at the time of conception, and imposing 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 abortion itself is illegal, barring a few

[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

exceptions (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 (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 (Guo et al., 2016).

Nonetheless, a small empirical literature that has examined the impact of the ban on sex screening and sex selection on child sex ratios finds that the Act led to an increase in the birth of girls. Using Census data and Maharashtra as a control state, Nandi and Deolalikar (2013b) identify the causal impact of the passing of the national PNDT Act in 1994 on child sex ratios and find that the PNDT Act accounted for an increase in the female-male sex ratio of 14-26 points, leading to an increase in surviving girls between the ages of 0-6 years particularly in rural areas. In fact, these treatment effects are potentially underestimates since their estimated child sex ratio is defined for children between the ages of 0 and 6, even though children aged between 4 and 6 were never exposed to the treatment. Using a different dataset, the District Level Household Survey (2002–2004), Nandi (2015b) find that the PNDT act led to an increase of 1 percent in the odds of a female birth across all treated states. 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 Sharma (2020) find that the implementation of the Act 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 be 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 (2015b) considers the impact of the Act 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 authors find a small but significant increase in the relative infant mortality for girls. However, he does not consider any other health outcomes. Rastogi and Sharma (2020) find that the PNDT Act 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 from the National Family and Health Survey, we examine the impact of the PNDT 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) scheme

The Beti Bachao Beti Padhao (BBBP) Scheme 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 scheme 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 boys between 2001 and 2011. The budgetary allocation of funds for the last three years has been in excess of Rs 11 billion (Scheme, 2018).

The programmes's stated aims are to improve the sex ratio in the selected districts by 2 points in a year (Scheme, 2018), reduce the gender differentials in under 5 mortality rates from 7 points in 2017 to 1.5 points per year, improve nutrition by reducing the number of anaemic and underweight girls, and increase the enrollment of girls in secondary education to 82 percent by 2018-19.

The mass communication campaign involves spreading awareness and disseminating information through radio jingles in Hindi and regional languages, television messages, community engagement through mobile exhibition vans, social media and field publicity. Awareness generation through hand-outs, brochures, SMS text messages on mobile phones in English, Hindi and regional languages.[3]

Ensuring the proper implementation of the PNDT scheme was a part of the BBBP scheme. Sex ratio at birth was required to be monitored and plans were to be made accordingly for the most affected regions. All births were to be registered through the

[3]Other actions included a renewed focus on the enforcement of the PNDT Act. State governments and district-level officials were also asked to improve data collection on 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 prenatal and postnatal care of mothers, and counselling to ensure the equitable care of female infants, as well the training of front-line health workers to make them more sensitive to these concerns. On the educational front, measures include universal enrollment of girls in school and construction of toilets specifically for the use of girls, as well as the integration of gender-related awareness in educational curricula, and gender-sensitisation training of the police and judicial personnel.

Civil Registration System (CRS). All pregnancies were to be registered along with the provision of ANC and post natal services. All genetic laboratories and clinics conducting any pre-conception and pre-natal diagnostic counseling or tests were to be registered. A complete database of complaints was to be maintained. Decoys were conducted to unearth the illegal practice of sex selection. Information about the ongoing PNDT court cases was to be followed up (Scheme, 2018).

As it focused on both a more stringent implementation of the PNDT Act as well as encouraging changes in social norms through advocacy and media campaigns, the programme has elements of both supply-side and demand-side interventions. 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 scheme 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 scheme. They find a significant improvement in the sex ratio at birth in favour of girls when analysing data from 2005-2016. We use the staggered timing in the roll out of the program 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 impacted any 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, we use the fourth round of the National Family Health Survey, a national household survey implemented in 2015-16 in India that is representative at the district level (International Institute for Population Sciences and ICF, 2017). The survey includes 601,509 households from both rural and urban areas of each of the 640 districts listed in the 2011 Census, covering all states and union territories. We use retrospective birth histories for women aged 15-49 years to construct a data set of all births that take place between 1975 and 2016, with a rich set of associated mother and child-level demographic and health outcomes. The fertility analysis includes the birth history of 699,686 women, including 21,251,298 observations at the mother-year level.

To analyse the impacts of the ban and the media campaign on child health outcomes, we use pooled child health data from all four rounds of the NFHS, conducted in 1992-93, 1998-99, 2005-06 and 2015-16. We organise the mortality and the child health data at the level of the child: our mortality data set includes 1,315,617 child observations, while the child health dataset covers children born 3 to 5 years before any of the survey rounds, and includes 393,167 child observations.

The data includes a rich set of demographic characteristics such as mother's characteristics including mothers age, whether the mother has completed primary education, total children ever born to the mother, and household characteristics such as 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 1989, followed by the implementation of a national ban in 1996, covering all other states except Jammu and Kashmir (JK)[4]. 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 postnatal discrimination among girl 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., 2020a), we are able to identify the treatment effect of the ban on such families compared to families with firstborn boys. We consider 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 birthyear 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

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

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 hoe gender gap in outcomes between children born to same mother, change, 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 state-time dimension [5]. For regressions relating to the BBBP intervention, we substitute district-specific time trends in place of state-specific time trends.

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:

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

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 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]Nandi (2015b) 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 lower female mortality rates. Anukriti et al. (2020a) 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). 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) further show 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.

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., 2020a). However, our results are robust to including births from 1975 as well. 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 also exclude all children who have not attained the required age for the respective mortality measure.

Health outcomes include (i) a set of objective biomarkers such as height for age (1a) , weight for age (1b) and body mass index (BMI) (1c) ; (ii) indicators for health investments that could affect these biomarkers such as ante-natal care (ANC) visits (2a) , whether a child has received tetanus shots (2b) and breastfeeding duration (2c). A detailed note on variable definitions and construction can be found in Appendix A.

For regressions estimating the impact of the PNDT Act banning sex-screening and sex-selection on child mortality and health, the variable $Treat_{imst}$ takes the 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 utilise the policy variation at the district-month-year level. We replace the state specific time trends with district specific time trends in the previous model. We estimate the following equation and cluster the standard errors at the district level:

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

the $Treat_{imdt}$ variable is defined as the interaction between an indicator variable, $BBBP_{imdt}$ indicating whether the child i born to mother m was born in phase one BBBP district, and an indicator variable, $PostBBBP_{imdt}$ indicating whether the child was exposed to the intervention at any point in life. Accordingly, the definition of the $PostBBBP_{imdt}$ in the regression is relative to the outcome of interest so as to include all children who have been even partially exposed to the treatment in the first month, first year and first five years of their lives, respectively. For neonatal mortality, all births that took place after June 2015 in treated districts are assumed to be treated by the intervention; for infant mortality, all births that took place after June 2014 in treated districts are assumed to be treated; and for under-five mortality, all births that took place in treated districts after June 2010 are assumed to be treated. It would have been ideal to look at the impact of the full exposure to policy for all the years relevant to the outcome variable, that is, for children who have been exposed for the entire period of one month, one year or five years to the policy. 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 on on 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 policy compared to their brothers, and provides direct evidence on the impact of the ban on sex screening and selection as well as the media intervention on post-natal gender discrimination. We include mother fixed effects, birthyear 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 girl are more likely to pursue sex selection at higher birth orders (Anukriti et al., 2020a), the intensity of treatment is likely to be higher for such families. Following Anukriti et al. (2020a), 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:

$$\begin{aligned} Y_{imst} &= \beta_0 + \beta_1 \text{Treat}_{\text{imst}} + \beta_2 FirstbornFemale_{\text{imst}} \\ &+ \beta_3 (\text{Treat} \times FirstbornFemale)_{\text{imst}} + \delta_{st} + \tau_t + \phi_m + \epsilon_{\text{imst}} \end{aligned} \quad (3)$$

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. 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 in firstborn female families exposed to the policy shock in question compared to firstborn male families.

All regressions include mother fixed effects, birth year fixed effects, and state-time trends. 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). Our main coefficient of interest is the coefficient on the interaction term Treat*X*Female. 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.

The coefficient on the interaction term Treat*X*Female is positive and significant for neonatal mortality (at the 1 percent level) and infant mortality (at the 5 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, though significant only 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. Neonatal mortality for girls is 22 percent higher in the post-intervention period as compared to boys for the same mother. Similarly, infant mortality for girls is 13 percent higher and under-five mortality is 9.4 percent higher compared to boys born to the same mother. We find that the estimated coefficients are 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.

Since we additionally have data available from the fourth round of the NFHS collected in 2015-16, we show our results are robust to using a larger sample with all births from 1985-2016 (Table A1). The estimated coefficients are smaller but remain statistically significant. The smaller size of the coefficients likely reflects the lower comparability of births that take place between 2006 and 2016 with births that take place prior to the introduction of the ban.

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 (8.48 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 A2), we check for robustness of the results removing the mother fixed effects and including 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 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. 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 estimate for Treat$X$Female is positive and significant for all mortality outcomes, for both birth order specifications, which is in line with the results that include mother fixed effects. As before, we find that the ban disproportionately affected the gender gap in mortality at higher order births.

In Table 2 we present the mortality estimates by firstborn female children using a similar estimation framework as above. Here our main coefficient of interest is the coefficient on the interaction between the treatment variable and the female firstborn family indicator. The child's birth order is restricted to 2 and above. Columns 1, 2 and 3 present the estimates for the full sample (1985 to 2016) and columns 4, 5 and 6 for the restricted sample (1985-2005). We find that the coefficient on the interaction term $TreatXFirstbornFemale$ is positive and significant for all the mortality outcomes. The coefficient on $Treat$ is negative and significant for under-five mortality, implying that mortality is lower in post-PNDT period for male firstborn families. 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 negative impact on child mortality within these families. Our results for $TreatXFirstbornFemale$ are in line with (Anukriti et al., 2020b), who find the coefficient to be positive and significant as well, implying that for female first born families post the ultrasound, mortality was higher.

We check for the robustness of these results (Table A4) again by excluding the mother fixed effects and including the same set of demographic characteristics (as in Table A2), while also including birthyear fixed effects and state-specific time trends. We find that the coefficient on $TreatXFirstbornFemale$ is positive and significant for all mortality outcomes, both in the full sample (1985-2016) and the restricted sample (1985-2005).

We also check for any heterogeneity by income in our results, by separately estimating the regressions for poor and non-poor households. Table A3 presents the estimates for mortality outcomes (neonatal, infant and child mortality), for all birth orders. Our main coefficient of interest is the coefficient on the interaction term $TreatXFemale$. Columns 1, 2 and 3 present the estimates for households in the bottom 60% of the income distribution, while columns 4, 5 and 6 present the estimates for households in the top 40% of the income distribution. We find $TreatXFemale$ is positive and significant for all mortality outcomes for the poor sample, and insignificant in the rich sample, implying that mortality is more likely among the poor for a female child after the implementation of the PNDT.

5.1.2 Robustness: Analysis of pre-intervention trends

Using an event study design, we check the trends in infant, neonatal and under 5 mortality rates for up to ten years prior to and after the enactment of the ban 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 10 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 ban roll out.The 0 mark on the x axis depicts 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. The graphs plot the coefficients on the interaction between female child and birth year relative to the enactment of the ban on sex screening and sex selection. 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. In all three cases, the pre-intervention coefficients are not significantly different from zero suggesting that there is no evidence that pre-intervention trends are driving our results.

We see an upward trend for all three mortality outcomes post 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 3 presents the estimates of PNDT policy 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 PNDT led to an increase in the gender gap in malnutrition. Girls born after the ban are relatively shorter compared to boys for the same mother. The estimate for body mass index (BMI) is negative but statistically insignificant. The magnitudes go up 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 goes down by .161 standard deviation with exposure to PNDT policy. 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 finding that very young boys have more vulnerable health outcomes than girls in the absence of any discrimination. 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 4 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 in Table 4 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 PNDT.

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 ANC visits is not statistically significant. Here, as well, we find that the estimates go up in magnitude for birth order 2 and above.

5.2 Impact of the BBBP intervention

5.2.1 Child mortality

Table 5 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 and significant (at 10 percent) for neonatal mortality, implying that the policy helped in reduction of the relative neonatal mortality for girls, in the phase 1 districts as compared to boys in the same family. However, when we restrict the sample to birth order 2 and above, this estimate is of the same size but is no longer significantly different from 0. 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 A6 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, 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 6 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 post-treatment exposure is defined as 1 for children below the age of one year during the survey.

For birth orders 2 and above, the coefficients on $TreatxFemale$ are positive but insignificant for all outcomes. However, for the sample without any birth order restrictions, the coefficients are negative while being statistically insignificant for all outcomes. These 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 A7 presents the treatment effects of the BBBP policy on child height for age (HFA), weight for age (WFA) and body mass index (BMI), where we interact the main treatment variable with the firstborn female 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.

Columns 7 and 8 of Table 4 present the treatment effects of the BBBP policy on duration of breastfeeding, using the mother fixed effects regression framework. Columns 7 presents the estimates for birth orders 2 and above and columns 8 presents the estimates for all birth orders.

For birth orders 2 and above, 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 after the intervention in phase 1 districts. The result is however not robust to the sample of all children without any birth order restriction, where the coefficient for breastfed is still positive but statistically insignificant.

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 ban, 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 confirm that the estimated impacts of the PNDT and BBBP policies are indeed a direct outcome of an increase in the birth of unwanted girls, 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. We estimate the following equation:

$$Y_{mst} = \beta_0 + \beta_1 \text{Treat} + \beta_2 X_{\text{mst}} + \delta_{st} + \tau_t + \epsilon_{\text{mst}} \tag{4}$$

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 variable $Treat$ has the same interpretation as before. These results are presented in Table 8. We find that the ban on sex selection was followed by an increase in the proportion of female births, but that the BBBP programme has so far not had an impact on female births.

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.

Additionally, an increase in fertility could also lead to a negative effect on post-natal gender bias. Das Gupta and Mari Bhat (1997) explore the relationship between fertility decline and post-natal 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 imposition leads to higher fertility, the gender discrimination is likely to go up following the parity effect. The intensification effect will not apply in our setting, as we believe, all the new female births are more likely to be unwanted. If the number 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.

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 a given policy. To provide a comparison of the relative effects of different policy interventions on fertility, we provide estimates of the impact of the PNDT ban 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. (2020a). We interact the $Treat_{mst}$ variable with an indicator of whether the family has a firstborn daughter to run the following estimations. We estimate two models, one with (5). We also estimate the same without mother fixed effects with the inclusion of birth year fixed effects and state-specific time trends and a set of controls including mothers education, mothers age, total children born, religion, caste, living in an urban area, total number of household members, and family structure.

$$
\begin{aligned}
Y_{mst} &= \beta_0 + \beta_1 \text{Treat}_{\text{mst}} + \beta_2 FirstbornFemale_{\text{mst}} + \\
&+ \beta_3 (\text{Treat} \times FirstbornFemale)_{\text{mst}} + \epsilon_{\text{mst}} \qquad (5)
\end{aligned}
$$

where Y_{mst} is an indicator for fertility for mother m in state s in year t.

In (3), we include birth year fixed effects (τ_t). For PNDT regressions we additionally include state specific time trends δ_{st} and 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 the respective policy. We follow the definition of the ultrasound technology diffusion period as in Anukriti et al. (2020a). $EarlyDiffusion$ 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. The BBBP treatment takes the value 1 if the child is born in and after July 2015 and in the district of Phase 1 implementation.

Table 7 presents the estimated impact of these interventions on fertility where the dependent variable is an indicator variable for whether a birth takes place to a given mother during a given year. Columns 1, 2 and 3 present the estimates of the impact of the shocks of early diffusion of ultrasound technology, the implementation of the PNDT and the impact of BBBP on fertility respectively.

We find that the coefficient on $EarlyDiffusionXFirstbornFemale$ is negative and significant. This is in line with the results of Anukriti et al. (2020a) implying that firstborn female families see a reduction in probability of birth after increased access to ultrasound technology during the 1985-1995 period. The estimated coefficient for $PNDTxfirstbornfemale$ is positive and highly significant (at one percent), implying that the probability of birth goes up for a female firstborn family, after the implementation of the ban on sex-screening. Finally, the coefficient on the interaction between $BBBP$ and $Firstbornfemale$ is positive but insignificant. The overall fertility results are robust to varying the sample period.

Columns 4 and 5 present the estimates of the impact of the shocks of early diffusion of ultrasound technology and the implementation of the PNDT on fertility, by firstborn female families, and with mother fixed effects. The sample for PNDT (column 5) restricts birth years to children born between 1985 and 2005. We find the interaction of the term

Early Diffusion with the indicator for firstborn female family to be negative and significant. The interaction term for the PNDT treatment with firstborn female family is positive and significant, implying that for a female firstborn family, fertility increases after the passing of the PNDT ban on sex-screening and selection, compared to firstborn male families.

6.3 Birth spacing

We investigate the impact of the PNDT policy on birth spacing for mothers with 2 children, defined as the number of months between the first and the second birth. Anukriti et al. (2020a) compares birth spacing between the first two births for firstborn male and firstborn female families before and after ultrasound access. They find in the pre-ultrasound era, the gap was about a month, which came down with access to the technology. In our setting, we expect that this gap to widen with the ban on technology. We compare families with firstborn females to firstborn males to identify the differential effects on birth spacing.

$$\begin{aligned} Y_{mst} &= \beta_0 + \beta_1 \text{Treat}_{\text{mst}} + \beta_2 FirstbornFemale_{\text{st}} + \\ &+ \beta_3 (\text{Treat} \times FirstbornFemale)_{\text{mst}} + \beta_4 X_{\text{st}} + \phi_{ps} + \delta_{st} + \epsilon_{\text{st}} \end{aligned} \tag{6}$$

where Y_{mst} is defined as the number of months between first and second birth for woman m. ϕ_{ps} is the firstborn girl x state fixed effects while δ_{st} is the year of first birth x state fixed effects. X_{st} are the controls which include mother's age, education, total number of children, religion, caste, wealth index, living in an urban region, total members in the household, household family structure. We find the interaction First Born Female X Treat to be negative and significant, implying that for first born female families post the implementation of PNDT, birth spacing reduces.

7 Discussion

In this study we use the staggered roll out of the PNDT Act in India and find that while the legal restriction in sex determination improves the relative odds of a female birth, it worsens relative mortality and health outcomes for girls as compared to boys. Moreover, we find that firstborn female families are more likely to have a child after the implementation of the ban, and that mortality and child health outcomes worsen in these families with exposure to the ban. Our results are robust to varying birth order and birth year restrictions.

How far do these results align with the existing literature? Table 9 presents a comparison of our results with Nandi (2015a), 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 (2015a), although with a different dataset, the District Level Household Survey. In his paper, finds 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 (2015a) 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 for the sample that includes Maharashtra and its neighboring states.

In sum, using a different dataset that spans a longer period of time, and using a more demanding specification that allows for the flexible control of a larger number of potentially confounding pre-intervention trends, we find contrasting results to Nandi (2015a) that challenge the unambiguously welfare-improving assessment of the PNDT policy. While the PNDT did increase the probability of girls being born, the surviving girls faced relatively higher levels of mortality and lower health investments compared to boys. We additionally show how these costs are higher in families with firstborn females, which are more intensively treated by the ban. Given that variation in the sex of the firstborn child is quasi-exogenous, this provides causal evidence of the impact of the treatment.

We also present the first estimates of the BBBP program, which combines a demand-driven approach to reduce gender discrimination through a media campaign aimed at gender sensitisation, in addition to strengthening existing bans on ultrasound use. We find that this policy seems to help in reducing neonatal mortality in the sample of all children and improve breastfeeding duration for female children in sample of children with birth order 2 and above. The policy seems to benefit relatively the firstborn female families in terms of improving nutritional outcomes such as height for age and weight for age. Our analysis for BBBP estimates is limited to studying only short run results, as the data is only available till 2016.

While a legal ban on sex selection has become a common policy response for governments across developing countries to tackle gender discrimination, it is important to take note of potential unintended consequences of such top-down supply-side policies on health and mortality outcomes for surviving girls, which fail to take into account strong social norms driving son preference in patriarchal societies. Studies identifying gender differences in postnatal investments have important policy implications. For example, if the reason for lower investment in girls is because they are part of larger and poorer families, then transfers could be made to those families to help girls. However, if parents prefer sons to daughters, then transfers to the same families might not help the girls but may be diverted towards boys. In that case, interventions may have to be targeted specifically to girls, rather than to families as a whole.

Das Gupta (2019) provides a meta analysis on efficacy of bans on sex selection on post natal gender discrimination. There is mixed evidence on the impact of such bans on gender discrimination and largely seem to suggest that bans are likely to have negative effects on unwanted girls and their mothers. In contrast, other policies, like mass messaging might work better in reducing son preference.

The findings from our study have an important bearing on the design of policy targeting to reduce gender discrimination in the population. Our results are in line with some existing evidence on unintended consequence of such top down policies around fertility choice. Anukriti (2018) finds unintended consequence of financial incentive policy scheme which targeted in reducing fertility and sex ratio in the Indian state of Haryana. Their study demonstrates that provision of financial incentives to parents to improve the sex ratio and reduce fertility in the presence of pervasive son preference can have a limited effect and may backfire to increase sex selection. We build on this small but growing body of evidence which seems to suggest that top-down approaches may not yield desired results unless there is a change in the parameters that are at the roots of gender discrimination.

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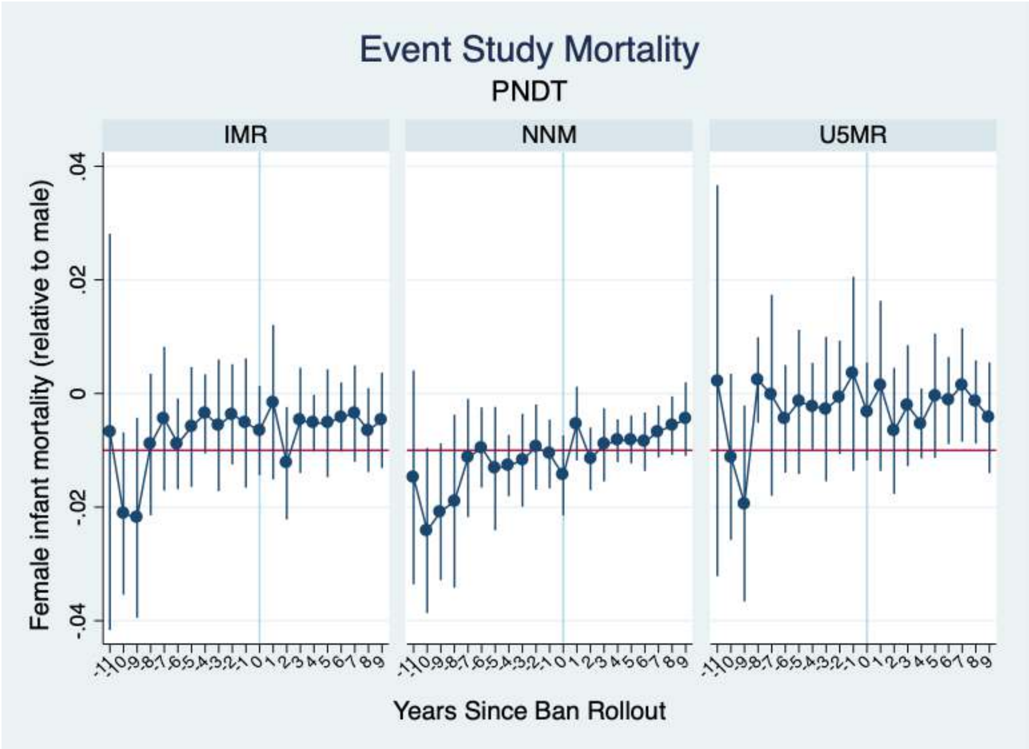

Figure 1: The estimates plotted are from the regression of child mortality on the interaction between $Treat$ and $Female$, for 10 years before and after the ban. Birth orders are restricted to 2 and above. Additional controls include state-specific time trends, child birth year, birth order and mother fixed effects. Standard errors are clustered at the state level. The plot displays 95 percent confidence intervals.

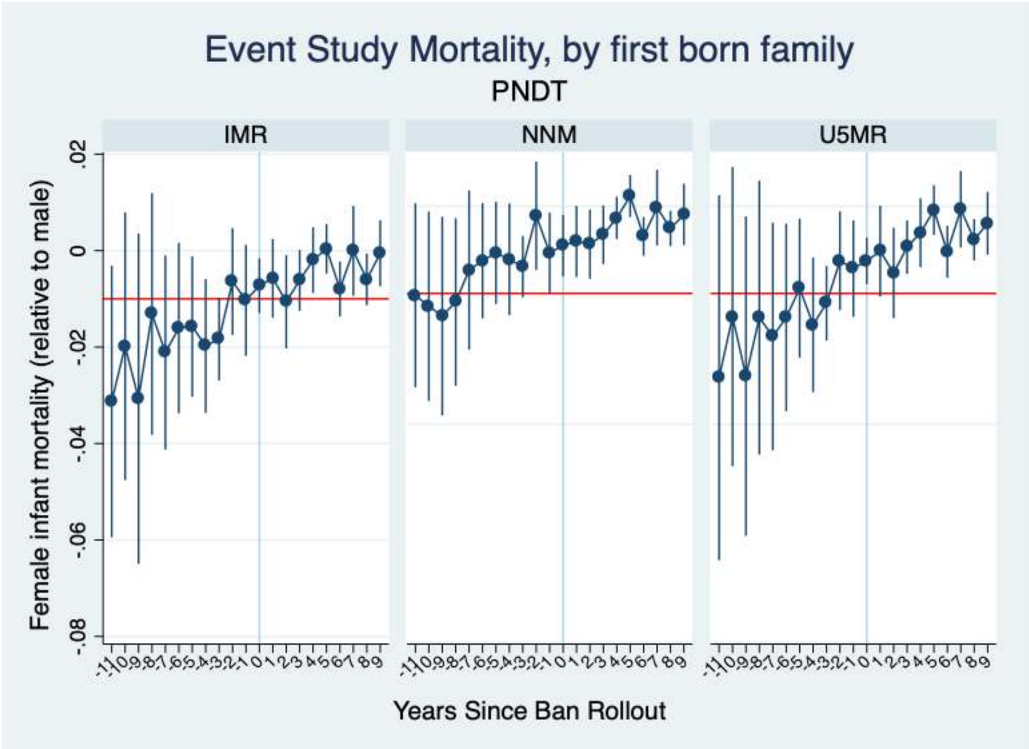

Figure 2: The estimates plotted are from the regression of child mortality on the interaction between $Treat$ and $FirstbornFemale$, for 10 years before and after the ban. Birth orders are restricted to 2 and above. Additional controls include state-specific time trends, child birth year, birth order 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

	Birth orders 2 and above			All birth orders		
	(1)	(2)	(3)	(4)	(5)	(6)
	NNM	IMR	U5MR	NNM	IMR	U5MR
Treat	0.000621	0.000896	-0.00216	-0.00417*	-0.00336*	-0.00423*
	(0.00250)	(0.00188)	(0.00244)	(0.00228)	(0.00191)	(0.00211)
Female	-0.0154***	-0.00895***	-0.00318	-0.0152***	-0.0125***	-0.00846***
	(0.00159)	(0.00255)	(0.00398)	(0.00145)	(0.00155)	(0.00279)
Treat X Female	0.0104***	0.00885***	0.00748*	0.00733***	0.00829***	0.00686**
	(0.00212)	(0.00320)	(0.00382)	(0.00186)	(0.00213)	(0.00274)
Bootstrapped p value	0.000500	0.0186	0.0740	0.00630	0.00210	0.0197
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. Infant Mortality (IMR) takes the value 1 if the child died before completing 1 year. Under-5 mortality (U5MR) takes the value 1 if a child died before completing 5 years. 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. $Female$ takes the value 1 if the child's gender is female. All estimations include mother fixed effects, birthyear fixed effects, and state-specific time trends. The sample includes children born between 1985 and 2005. The first 3 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 the PNDT on child mortality by families with firstborn females

	1985-2016			1985-2005		
	(1)	(2)	(3)	(4)	(5)	(6)
	NNM	IMR	U5MR	NNM	IMR	U5MR
Treat	-0.000547	-0.00396	-0.00846***	0.00102	-0.00234	-0.00550*
	(0.00305)	(0.00262)	(0.00238)	(0.00321)	(0.00316)	(0.00286)
Treat X First Born Female	0.00867**	0.0144***	0.0145***	0.00880***	0.0148***	0.0140***
	(0.00338)	(0.00357)	(0.00373)	(0.00319)	(0.00332)	(0.00369)
Bootstrapped p value	0.0451	0.00150	0.00710	0.0360	0.000600	0.00670
Observations	675306	653584	556263	367006	367006	367006
Mean of Dep. Variable	0.0433	0.0646	0.0825	0.0455	0.0683	0.0803
SD	0.204	0.246	0.275	0.208	0.252	0.272

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. Infant Mortality (IMR) takes the value 1 if the child died before completing 1 year. Under-5 mortality (U5MR) takes the value 1 if a child died before completing 5 years. 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. $FirstbornFemale$, defined at the mother level, takes the value 1 if the child belongs to a family with a firstborn girl child and zero otherwise. All estimations include mother fixed effects, birthyear fixed effects, and state-specific time trends. The sample includes children born at a birth order of 2 or higher. The first 3 columns include children born during 1985-2016, while the last three columns include children born during 1985-2005. 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 the PNDT on the gender gap in child health outcomes

	Birth orders 2 and above			All birth orders		
	(1)	(2)	(3)	(4)	(5)	(6)
	HFA	WFA	BMI	HFA	WFA	BMI
Treat	2.646^{***}	1.431^{***}	-0.132	2.703^{***}	1.494^{***}	-0.318
	(0.604)	(0.189)	(0.701)	(0.521)	(0.285)	(0.566)
Female	0.129	0.0736	0.0391	0.128^{*}	0.0732	0.0338
	(0.0975)	(0.0615)	(0.0388)	(0.0657)	(0.0493)	(0.0349)
Treat X Female	-0.161^{*}	-0.156^{**}	-0.0993	-0.120^{**}	-0.110^{**}	-0.0714
	(0.0786)	(0.0562)	(0.0689)	(0.0569)	(0.0452)	(0.0445)
Bootstrapped p value	0.0261	0.0325	0.253	0.0403	0.0332	0.150
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. $Female$ takes the value 1 if the child's gender is female. All estimations include mother fixed effects, birthyear fixed effects, and state-specific time trends. The sample includes children born between 1988 and 2005. The first 3 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 the PNDT and BBBP on the gender gap in child health investments

	Birth orders 2 and above				All birth orders			
	(1) ANC	(2) TET	(3) Breastfed	(4) Breastfed	(5) ANC	(6) TET	(7) Breastfed	(8) Breastfed
Treat PNDT	-1.026**	-1.345***	-13.22***		-0.847*	-0.159	-13.78***	
	(0.422)	(0.386)	(2.272)		(0.449)	(0.269)	(1.960)	
Female	-0.113***	-0.0179	-0.000838	0.754	-0.0844***	-0.0301**	-0.0100	0.338
	(0.0275)	(0.0117)	(0.105)	(0.530)	(0.0169)	(0.0115)	(0.109)	(0.308)
Treat PNDT X Female	0.0530	-0.0374**	-0.552**		-0.0351	-0.0244*	-0.253	
	(0.0593)	(0.0140)	(0.235)		(0.0383)	(0.0127)	(0.188)	
Treat BBBP				-5.670***				-1.640
				(1.818)				(1.128)
Treat BBBP X Female				3.822**				1.550
				(1.699)				(1.273)
Bootstrapped p value	0.447	0.0265	0.00900		0.381	0.0630	0.192	
Observations	18984	18677	26974	254	30347	29908	44022	580
Mean of Dep. Variable	1.896	1.222	12.81	1.376	2.388	1.400	12.64	1.557
SD	2.503	1.190	9.121	2.527	2.873	1.196	9.061	2.582

Standard errors in parentheses

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

Note: "Breastfed" refers to duration of breastfeeding in months. "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. $Treat$ takes the value 1 if the PNDT Act has been implemented in the state in which a child is located. $Female$ takes the value 1 if the child's gender is female. All estimations include mother fixed effects, birthyear fixed effects, and state-specific time trends. The sample includes children born between 1988 and 2005. The first 3 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: Impact of BBBP programme on gender gap in child mortality

	Birth orders 2 and above			All birth orders		
	(1)	(2)	(3)	(4)	(5)	(6)
	IMR	U5MR	NNM	IMR	U5MR	NNM
Female	-0.00787***	-0.000419	-0.0126***	-0.0131***	-0.00888***	-0.0148***
	(0.00193)	(0.00319)	(0.00149)	(0.00135)	(0.00218)	(0.00107)
Female X Treat	0.0273	0.0118	-0.0399	0.00636	-0.000171	-0.0318*
	(0.0425)	(0.0377)	(0.0256)	(0.0270)	(0.0240)	(0.0185)
Observations	222734	124308	244567	405111	213497	449230
Mean of Dep. Variable	0.0663	0.110	0.0448	0.0563	0.0982	0.0390
SD	0.249	0.313	0.207	0.231	0.298	0.194

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. Infant Mortality (IMR) takes the value 1 if the child died before completing 1 year. Under-5 mortality (U5MR) takes the value 1 if a child died before completing 5 years. $Treat$ 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. $Female$, defined at the child level, takes value 1 if child's gender is female. All estimations include mother fixed effects, birthyear fixed effects, and state-specific time trends. The sample includes children born between 2005 and 2016. The first 3 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 6: Impact of BBBP programme on child health outcomes

	Birth orders 2 and above			All birth orders		
	(1)	(2)	(3)	(4)	(5)	(6)
	HFA	WFA	BMI	HFA	WFA	BMI
Female	0.561***	0.346**	0.0864	0.316***	0.158	0.0142
	(0.157)	(0.157)	(0.150)	(0.115)	(0.101)	(0.110)
Female X Treat	0.0842	0.0465	0.0690	-0.0976	-0.241	-0.267
	(0.536)	(0.407)	(0.455)	(0.394)	(0.271)	(0.319)
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. $Female$, defined at the child level, takes the value 1 if child's gender is female. All estimations include mother fixed effects, birthyear 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 3 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 7: Impact of different policy interventions on fertility

Outcome variable: Fertility	Without mother fixed effects			With mothers fixed effects	
	(1)	(2)	(3)	(4)	(5)
Treat Early Diffusion	0.100***			-0.0153**	
	(0.00328)			(0.00602)	
First Born Female	0.00736***	-0.0120***	0.00317***		
	(0.000737)	(0.00120)	(0.000103)		
Treat Early Diffusion X First Born Female	-0.0180***			-0.0149***	
	(0.00193)			(0.00189)	
Treat PNDT		-0.00507**			0.0417***
		(0.00188)			(0.00713)
Treat PNDT X First Born Female		0.0246***			0.0192***
		(0.00210)			(0.00173)
Treat BBBP			0.000258		
			(0.000452)		
Treat BBBP X First Born Female			0.000944		
			(0.000710)		
Observations	21150435	21150435	21150435	21251298	10322179
Mean of Dep. Variable	0.0599	0.0599	0.0599	0.0599	0.0682
SD	0.237	0.237	0.237	0.237	0.252

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. $TreatEarlyDiffusion$ takes the value 1 if the woman gives birth between 1985 and 1994, and 0 if woman gives birth in or after 1995. $TreatBBBP$ 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. $FirstbornFemale$ takes value 1 if the child belongs to a family with a firstborn girl child and zero otherwise. The first three columns present the results without mother fixed effects and include a set of controls including mothers education, mothers age, total children born, religion, caste, living in an urban area, total number of household members, and family structure. All three specifications include birth year fixed effects and state-specific time trends. The last two columns present the results with the inclusion of mother fixed effects. Standard errors are clustered at the state level. The sample includes all children born between 1985-2016 for the first four columns. The last column presents the results for children born between 1985 and 2005.

Table 8: 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 includes mother fixed effects, 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.

Table 9: Comparison with Nandi (2015) with birth order restriction

	(1)	(2)
	Without FE	With FE
Treat	-0.0172***	-0.0147*
	(0.00408)	(0.00830)
Female	-0.0126***	-0.0118***
	(0.00336)	(0.00337)
Treat X 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 is without 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.

Table 10: Outcome variable : Birth spacing

	(1)
	Birth spacing (in months)
First Born Female	1.665***
	(0.0659)
Treat	0.0919
	(0.0549)
First Born Female X Treat	-0.270**
	(0.111)
Observations	5266795
Mean of Dep. Variable	39.97
SD	25.30

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. $FirstbornFemale$ takes value 1 if the child belongs to a family with a firstborn girl child and zero otherwise. All estimations include fixed effects firstborn girl x state and year of first birth x state. Sample includes mothers with exactly two births. The dependent variable is defined as the number of months between a woman's first and second birth. Standard errors are clustered at the state level. This result uses the fertility data set.

Table A1: Differential impact of PNDT on the gender gap in child mortality (children born in 1985-2016)

	Birth orders 2 and above			All birth orders		
	(1)	(2)	(3)	(4)	(5)	(6)
	NNM	IMR	U5MR	NNM	IMR	U5MR
Treat	0.000562	0.000800	-0.00381*	-0.00520***	-0.00496***	-0.00586***
	(0.00245)	(0.00192)	(0.00217)	(0.00146)	(0.00117)	(0.00159)
Female	-0.0153***	-0.00879***	-0.00321	-0.0151***	-0.0124***	-0.00855***
	(0.00161)	(0.00265)	(0.00408)	(0.00139)	(0.00155)	(0.00278)
Treat X Female	0.00697***	0.00545**	0.00562	0.00490***	0.00528***	0.00527**
	(0.00169)	(0.00257)	(0.00343)	(0.00146)	(0.00161)	(0.00232)
Bootstrapped p value	0.00290	0.0743	0.146	0.0148	0.00920	0.0379
Observations	675306	653584	556263	1201598	1157719	969001
Mean of Dep. Variable	0.0433	0.0646	0.0825	0.0399	0.0574	0.0734
SD	0.204	0.246	0.275	0.196	0.233	0.261

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. Infant Mortality (IMR) takes the value 1 if the child died before completing 1 year. Under-5 mortality (U5MR) takes the value 1 if a child died before completing 5 years. 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. $Female$ takes the value 1 if the child's gender is female. All estimations include mother fixed effects, birthyear fixed effects, and state-specific time trends. The sample includes children born between 1985 and 2016. The first 3 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 A2: 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 X 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	0.00170	0.0388	0.115	0.00400	0.0156	0.0433
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: NNM takes value 1 if a child died before completing 1 month. IMR takes value 1 if the child died before completing 1 year. U5MR takes value 1 if a child died before completing 5 years. Mortality definitions exclude those kids who have not attained the said age. $Treat$ 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. All specifications include birth year fixed effects, state fixed effects and state specific time trends. The first 3 columns present the results for no restriction on birth order while the last 3 columns restrict birth orders to 2 and above. The sample includes children born between 1985 and 2005. Standard errors are clustered at the state level. The wild cluster bootstrap p values for the interaction terms are included. Controls include child's birth order, sibling size, mother's age, mother and fathers primary education, mother's age at birth, mother's weight for height, religion, caste, urban, number of members in the household, sex composition of adult members in the household, household wealth index.

Table A3: Impact of PNDT on child mortality: Heterogeneity by household income (all birth orders)

	Poor			Rich		
	(1)	(2)	(3)	(4)	(5)	(6)
	NNM	IMR	U5MR	NNM	IMR	U5MR
Treat	0.000450	-0.00288*	-0.00290	-0.0117***	-0.00629**	-0.00887**
	(0.00200)	(0.00156)	(0.00215)	(0.00323)	(0.00282)	(0.00396)
Treat X Female	0.00704***	0.00718***	0.00632**	0.00188	0.00225	0.00323
	(0.00189)	(0.00201)	(0.00261)	(0.00242)	(0.00299)	(0.00429)
Bootstrapped p value	0.0116	0.00430	0.0486	0.502	0.526	0.633
Observations	834339	803176	668098	367259	354543	300903
Mean of Dep. Variable	0.0466	0.0675	0.0876	0.0266	0.0374	0.0459
SD	0.211	0.251	0.283	0.161	0.190	0.209

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. Infant Mortality (IMR) takes the value 1 if the child died before completing 1 year. Under-5 mortality (U5MR) takes the value 1 if a child died before completing 5 years. 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. $Female$ takes the value 1 if the child's gender is female. All estimations include mother fixed effects, birthyear fixed effects, and state-specific time trends. Columns 1, 2 and 3 present the estimates for households in the bottom 60% of the income distribution, while columns 4, 5 and 6 present the estimates for households in the top 40% of the income distribution. The sample includes children born between 1985 and 2016, and children with 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 A4: Differential impact of the PNDT on child mortality by families with firstborn females (without mother fixed effects)

	1985-2016			1985-2005		
	(1)	(2)	(3)	(4)	(5)	(6)
	NNM	IMR	U5MR	NNM	IMR	U5MR
Treat	-0.00227	-0.00549	-0.00526	0.00489**	0.000487	0.00131
	(0.00263)	(0.00328)	(0.00575)	(0.00194)	(0.00204)	(0.00229)
Treat X First Born Female	0.00774***	0.00952***	0.00832***	0.00587***	0.00820***	0.00785***
	(0.00154)	(0.00199)	(0.00241)	(0.00153)	(0.00212)	(0.00246)
Bootstrapped p value	0.00140	0.00210	0.0147	0.00810	0.00820	0.0217
Observations	810350	782060	659783	439339	439339	439339
Mean of Dep. Variable	0.0357	0.0540	0.0738	0.0385	0.0578	0.0680
SD	0.186	0.226	0.261	0.192	0.233	0.252

Standard errors in parentheses

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

Note: NNM takes value 1 if a child died before completing 1 month. IMR takes value 1 if the child died before completing 1 year. U5MR takes value 1 if a child died before completing 5 years. $Treat$ is whether the PNDT Act is implemented in the state. $FirstBornFemale$, defined at the family level, takes value 1 if child i belongs to a family with firstborn girl child and zero otherwise. Mortality definitions exclude those kids who have not attained the said age. The first 3 columns present the results for full sample (1985-2016) while the last 3 columns present the results for the restricted sample (1985-2005). This specification includes state specific time trends, birth year fixed effects and state fixed effects. Birth orders are restricted to 2 and above. Standard errors are clustered at the state level. The wild cluster bootstrap p values for the interaction terms are included. Controls include child's birth order, sibling size, mother's age, mother and fathers primary education, mother's age at birth, mother's weight for height, religion, caste, urban, number of members in the household, household wealth index, household family structure.

Table A5: Differential impact of the PNDT on the gender gap in child health outcomes by firstborn female family

	1988-2016			1988-2005		
	(1)	(2)	(3)	(4)	(5)	(6)
	HFA	WFA	BMI	HFA	WFA	BMI
Treat	1.937***	-0.149	-2.091***	3.645***	1.024***	-1.445**
	(0.300)	(0.237)	(0.294)	(0.432)	(0.172)	(0.567)
Treat X First Born Female	-1.690***	0.470***	1.907***	-1.357***	0.556***	1.788***
	(0.165)	(0.123)	(0.0916)	(0.141)	(0.108)	(0.0959)
Bootstrapped p value	0.400	0.133	0.192	0.403	0.128	0.174
Observations	67529	67529	67529	14244	14244	14244
Mean of Dep. Variable	-1.935	-1.863	-0.885	-2.257	-1.970	-0.727
SD	1.704	1.190	1.349	1.705	1.231	1.357

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. $FirstbornFemale$, defined at the mother level, takes the value 1 if the child belongs to a family with a firstborn girl child and zero otherwise. All estimations include mother fixed effects, birthyear fixed effects, and state-specific time trends. The sample includes children born at a birth order of 2 or higher. Columns 1, 2 and 3 include all births between 1988-2016, while Columns 4, 5 and 6 include all births between 1988 and 2005. 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 A6: Impact of BBBP programme on gender gap in child mortality by firstborn female family

	(1)	(2)	(3)
	IMR	U5MR	NNM
Treat	-0.0376	-0.0522*	0.0293
	(0.0294)	(0.0273)	(0.0290)
Treat X First Born Female	0.00962	0.00729	-0.0619**
	(0.0319)	(0.0307)	(0.0301)
Observations	222734	124308	244567
Mean of Dep. Variable	0.0663	0.110	0.0448
SD	0.249	0.313	0.207

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. Infant Mortality (IMR) takes the value 1 if the child died before completing 1 year. Under-5 mortality (U5MR) takes the value 1 if a child died before completing 5 years. $Treat$ 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. $FirstbornFemale$, defined at the mother level, takes the value 1 if the child belongs to a family with a firstborn girl child and zero otherwise. All estimations include mother fixed effects and birthyear 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 A7: Impact of BBBP programme on child health outcomes by firstborn female family

	(1) HFA	(2) WFA	(3) BMI
Treat	-0.462	-0.581**	-0.377
	(0.297)	(0.267)	(0.310)
First Born Female X Treat	0.971**	0.852***	0.241
	(0.421)	(0.302)	(0.382)
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. $FirstbornFemale$, defined at the mother level, takes the value 1 if the child belongs to a family with a firstborn girl child and zero otherwise. All estimations include mother fixed effects, birthyear fixed effects, and state-specific time trends. All specifications include birth year and state FE. Sample size includes children born in and after 2005, and children below age 1. The sample includes children of birth orders 2 and above. Standard errors are clustered at the district level.

Table A8: Outcome variable: Proportion of girls, PNDT, split sample

	(1) Male first born family	(2) Female first born family
Treat PNDT	0.00117	0.0205***
	(0.00606)	(0.00574)
Observations	222274	217065
Mean of Dep. Variable	0.484	0.467
SD	0.500	0.499

Standard errors in parentheses

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

	(1) Male first born family	(2) Female first born family
Treat BBBP	-0.0256*	0.00766
	(0.0149)	(0.0210)
Observations	156298	178697
Mean of Dep. Variable	0.490	0.463
SD	0.500	0.499

Standard errors in parentheses

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

Note: $TreatPNDT$ 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 B3P scheme was introduced and if the child is born after June 2015. The dependent variable is proportion of girls which is defined as the ratio of the number of girls to the total number of children, by their birth years and mother id. All specifications includes year and state 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 after 2005. The first column presents the results for male first born family while the second column presents the results for female first born family. Controls included are child's birth order, sibling size, mother's age, mothers education, mothers age at birth, mothers weight for height, and household characteristics of religion, caste, urban sector, number of household members, wealth index and family structure.

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.