Prenatal Sex Selection and Girls' Well-Being: Evidence from India

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ABSTRACT

We study the impacts of prenatal sex selection on girls' well-being in India. We show that high sex ratios at birth reflect the practice of prenatal sex selection and apply a triple difference strategy to examine whether changes in health outcomes of girls relative to boys within states and over time are systematically associated with changes in sex-ratios at birth. We find that an increase in prenatal sex selection is associated with a reduction in girls' malnutrition. We further explore various underlying channels linking between prenatal sex selection and girls' outcomes.

Keywords: Son preference, prenatal sex selection, ultrasound, sex ratio at birth, gender discrimination, child health.

JEL codes: J13, J16, I1, O12

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I. INTRODUCTION

Son preference in India and other Asian countries has been documented extensively. A large number of studies have shown that parental preference for boys is manifested in gender differences in the intrahousehold allocation of resources and medical care, which result in gender differentials in child outcomes such as nutrition, morbidity and mortality and, in extreme cases, female infanticide (see, for example, Arnold et al., 1998; Deaton, 1997; George, 1997; Miller, 1981; Kishor, 1993; Rose, 1999; Rosenzweig and Schultz, 1982; Pande, 2003; and Barcellos et al., 2010). The diffusion of prenatal sex determination technologies since the 1980s has provided parents with an alternative way to achieve the preferred sex composition of children. Indeed, starting from the late 1980s, there has been a steep increase in male to female ratios at birth, which has been attributed to the increasing practice of sex selective abortion. A growing number of studies have examined the factors affecting the prevalence of prenatal sex selection and have assessed its effect on sex ratios at birth.[1] Little is known, however, about the impact of this phenomenon on the well-being of the girls who are born.

Prenatal sex selection may affect girls' outcomes through different channels. First, girls might be more likely to be born into households that have weaker son preference so that they would be more likely to be wanted.[2] Second, with access to prenatal sex selection, parents may be less likely to rely on fertility stopping rules to achieve their desired sex composition of children so that girls might be born into smaller families and receive a larger share of household inputs. Third, girls' outcomes might change if the characteristics and living environment of households that do not practice prenatal sex selection are different from those that practice it. Finally, prenatal sex selection may affect girls' outcomes if an increase in the scarcity of girls changes women's value in labor and marriage markets subsequently affecting parental incentives to invest in girls.

In this paper, we study the impact of prenatal sex selection on the well-being of girls in India and investigate some of the different channels through which prenatal sex selection may affect girls' outcomes. India is an interesting environment for studying the impact of prenatal sex selection due to its large regional variations in parental preference for sons and the striking differences in the use of prenatal sex selection technologies over time and across regions. Using

[1] We review this literature in the next section.

[2] This is a similar rationale to that proposed by Donohue and Levitt (2001) and Ananat et al. (2009) in the context of abortion legalization in the US.

a comprehensive data set on child outcomes and household characteristics, we apply a *triple-differences* approach to examine whether changes in the nutritional status and mortality rates of girls relative to boys within states and over time are systematically associated with changes in the prevalence of prenatal sex selection. In the absence of a direct measure of parental access to prenatal sex selection, we use the ratio of males to females at birth (MFR) in the child's state and birth cohort and provide evidence showing that increases in MFR at birth reflect the practice of prenatal sex selection starting from the late 1980s. We then analyze whether changes in the outcomes of girls within states and over time in comparison with boys are associated with changes in MFR at birth. We then turn to explore the various channels that might link between prenatal sex selection and girls' outcomes by analyzing whether changes in MFR within states and over time appear to be associated with changes in household characteristics, fertility, breastfeeding practices, and son preference.

Our paper is related to a limited number of recent studies, most of them conducted concurrently with our own, that examine the effects of prenatal sex selection on girls' outcomes. Shepherd (2008) compares between families with high versus low likelihood of using prenatal sex selection in India and finds inconclusive evidence for a link between prenatal sex selection and a decline in female child's mortality. Lin et al. (2009) exploit variation in the use of prenatal sex selection in Taiwan over time and birth order generated by the legalization of abortion and find a positive association between prenatal sex selection and female survival. Almond et al. (2010) use variation in the diffusion of ultrasound technology across regions in China and find that prenatal sex determination is associated with an increase in girls' neonatal mortality but do not find any changes in postnatal investments in girls as manifested in breastfeeding duration and vaccination rates.[3]

Our paper adds to this limited number of studies in several aspects. First, compared to some other countries, India's high degree of heterogeneity in female discrimination and the differences in the practice of prenatal sex selection across regions makes it an optimal case for studying the effects of prenatal sex selection. This is also due to India's large variation in family size across households, which provides an opportunity to examine the extent and impact of prenatal sex selection across different parities and study the effects of prenatal sex selection on

[3] Other recent related studies are Portner (2010), which looks at the determinants of sex-selective abortion in India though not their effect on outcomes, and Bhardadwaj and Nelson (2010) which examine gender differences in prenatal investment in countries with strong son preference, including India, although they do not estimate the impact of sex-selective abortion.

fertility decisions. Second, we conduct a more comprehensive analysis on the impacts of prenatal sex selection by examining both mortality and nutritional outcomes. Our analysis of nutritional status provides important findings on intermediate outcomes that are prevalent in a high proportion of Indian children and have vital life-long consequences for human capital development and well-being (see, e.g. Currie, 2009 and Case and Paxson, 2010). Third, we go a step further and explore different channels through which prenatal sex selection affects child outcomes such as selection, family size, preferences, and breastfeeding duration. Finally, our empirical approach, which is based on triple-differences models, allows us to control for several confounding factors such as regional variation in son preference and differential trends across regions, providing a powerful way to reject alternative explanations for the observed results.

Our results show that an increase in the practice of prenatal sex selection is associated with a reduction in the prevalence of malnutrition among girls. This negative association appears to be stronger in some subpopulations in which discrimination against girls is usually more pronounced (for example in rural areas and among children of parity higher than one). Our results are robust to the inclusion of state-year fixed effects suggesting that our findings are unlikely to be driven by state specific time varying unobserved factors associated with changes in the incidence of prenatal sex-selection. Our results are also robust to several checks that assess the likelihood of possible biases due to state-specific differential trends by gender associated with prenatal sex selection.

Additional results show no evidence that prenatal sex selection leads to a selection of girls into households with more resources since we do not observe a differential improvement in household socio-economic characteristics of girls. However, we find that girls are more likely to be born in families with weaker son preferences. We also find evidence of a larger reduction in family size and an increase in breastfeeding duration for girls. On the other hand, prenatal sex selection does not appear to be associated with a reduction in girls' mortality or a regional decline in reported preferences for sons.

The rest of the paper is organized as follows: In the next section, we review the literature and describe the institutional background of unbalanced sex ratios and prenatal sex selection in India. Section III discusses the conceptual framework for analyzing the effects of prenatal sex selection on child outcomes. Section IV describes the data. Section V lays out the empirical strategy and Section VI presents the results. Finally, Section VII concludes.

II. BACKGROUND AND INSTITUTIONAL FRAMEWORK

Imbalanced sex ratios have been documented in India as early as the 19th century and throughout the 20th century (see Visaria, 1971; Miller, 1981, 1984; Dyson and Moore, 1983; and Sen, 1992, 2003 who introduced the concept of "Missing Women"). Until the late 1970s, gender imbalances were mostly manifested at older ages and not particularly at birth and were attributed to excess female mortality due to maltreatment and neglect and in extreme cases to female infanticide (see, for example, Dreze and Sen, 1997 and Das Gupta, 1987).[4]

Several studies have documented sharp increases in male to female ratios at birth since the late 1980s, especially in northern and western states, which are regions historically known for strong son preference and gender discrimination (see, for example, Das Gupta and Bhat, 1997; Arnold et al., 2002; Bhat, 2002; Bhaskar and Gupta, 2007; and Retherford and Roy, 2003). These studies have shown that the increase in sex ratios at birth is concurrent with the spread of prenatal sex determination technologies, thus suggesting that sex selective abortion is likely to be a major contributing factor.[5] Bhalotra and Cochrane (2010) estimate that about 0.48 million girls per year were selectively aborted in India during 1995-2005, which represents 6.2 percent of all potential female births. Estimates for northern and western regions are considerably higher. For example, Kulkarni (2007) estimates that out of 168,997 expected female births in Punjab in 2001, 19 percent (31,648) went missing.

Sex selective abortion requires two steps: sex determination of the fetus and an abortion. Abortion was legalized in India under the Medical Termination of Pregnancy Act (MTP) in 1972. Despite its legalization, most abortions are still practiced in unofficial and non-regulated facilities.[6] Access to abortion services is not difficult in India, even in the remotest areas of the

[4] Other possible explanations attributed to the lower number of females include census underenumeration of girls or different patterns of age misreporting by sex. While these factors might explain part of the gender imbalances, Visaria (1971), Miller (1981, 1984), and other researchers show that the main cause for imbalanced sex ratios until the late 1970s was sex differentials in mortality.

[5] Changes in the enumeration of girls or misreporting of age are unlikely to explain this upward trend since similar increases in sex ratios at birth are also observed among Indian populations living in the US, Canada, and the UK where birth registration is nearly complete and accurate (see Abrevaya, 2009; Almond and Edlund, 2008; Almond et al., 2009; and Dubuc and Coleman, 2007). We provide in the next section further evidence which suggests that prenatal sex selection is the main cause for the upward trend in sex ratios at birth.

[6] It is extremely difficult to obtain precise figures on abortion rates. Using indirect estimation techniques based on 1991 birth rates and population counts, Chhabra and Nuna (1993) assess that about 6.7 million abortions are carried out every year, with the majority of them taking place in informal facilities. A similar estimate (6.4 million) is recently provided by Duggal and Ramachadran (2004).

country (Duggal, 2004). Its costs vary by region, type of facility, method, and gestation period ranging from US$4.5 to US$16.5 (Ravindran, 2002; Sundar, 2003).

Sex determination during pregnancy was first made possible in the late 1970s by the use of amniocentesis (Jefferey et al., 1984) and became more accessible with the introduction of ultrasound technologies in the 1980s. In subsequent years, a continued decline in desired fertility coupled with a slower decline in the total number of desired sons increased the pressure to have sons at lower parities, thus raising the demand for prenatal sex selection (Das Gupta and Bhat, 1997). At the same time, economic development and trade liberalization accelerated the supply of prenatal sex determination technologies. Following the policy reforms of the 1990s, multinationals such as General Electric started manufacturing ultrasound machines in India improving access to prenatal sex determination and increasing the practice of prenatal sex selection throughout the country (Geroge, 2006). The diffusion process took place from urban to rural areas and from households of high socioeconomic status to those of low socioeconomic status (Khanna, 1997).[7]

In an attempt to eliminate the practice of sex selective abortion and in response to increasing public pressure from several NGOs and women's organizations, the government of India passed the Prenatal Diagnostic Techniques Regulations and Misuse Act (PNDT Act) in 1994, thus making it illegal to use ultrasound or amniocentesis in order to determine the sex of a fetus. However, this legislation proved to be ineffective and the practice of sex selective abortion continued to spread (see e.g., George, 2002 and Kishwar, 1995). In subsequent years, several private ultrasound clinics opened, mobile clinics and portable ultrasound machines made prenatal sex selection accessible in small towns and rural areas (Krugman, 1998), and most of the sex selective abortions moved to underground clinics (Everett, 1998).

The publication of sex ratio figures from the 2001 Census revealed a further increase in MFR at ages 0-6, which led to increased public pressure on the government to enforce and expand the legal power of the PNDT Act.[8] Recent reports indicate some improvement in the

[7] Ultrasound costs vary widely across regions and between formal and informal facilities. Arnold et al. (2002) provide an average estimate of US$10-20.

[8] In 2001, the Centre for Enquiry into Health (CEHAT) and the Mahila Sarvangeen Utkarsh Mandal (MASUM) filed a public interest litigation against the Union of India and all the state governments for the non-implementation of the PNDT Act and for the inclusion of all emerging technologies that could be used for sex selective abortion. As a result, the Indian Government decreed in 2001 that manufacturers of ultrasound equipment could sell the machines only to registered clinics, which must maintain strict records of their use. One of the amendments to the Act, approved by the Indian Parliament in 2003, was the inclusion of technologies that allow sex selection during the preconception and pre-implantation

enforcement of the Act. However, sex selective abortion is still being practiced extensively and enforcement of the law appears to be difficult, if not impossible, to achieve (see e.g., Subramanian and Selvaraj, 2009; Portner, 2010).[9]

III. CONCEPTUAL FRAMEWORK

There are various channels through which parental preference for sons can affect girls' outcomes in the absence of a technology that allows them to realize their preferences for sex composition. First, parents can allocate more resources to children that provide them with higher utility (i.e. boys) as long as boys and girls are substitutes in parents' utility function (Rosenzweig and Schultz, 1982) or when capital markets are imperfect (Becker and Tomes, 1979 and 1986).[10]

Parents can also attempt to adjust excess fertility and undesired sex composition of children through selective allocation of resources that results in the mortality of the less-desired children (Simmons et al., 1982). Alternatively, if parents continue to bear children until they achieve their desired number of sons, girls will have, on average, a larger number of siblings than boys (Yamaguchi, 1989). Therefore, even if parents treat boys and girls equally within a household, girls can be expected to grow up in larger families with fewer resources per child (Jensen, 2003). In addition, parental preference for boys is likely to affect birth spacing (Ben-Porath and Welch, 1976) and duration of breastfeeding (Jayachandran and Kuziemko, 2009) since parents may wish to have a child sooner following the birth of a daughter, especially if they are approaching the end of the mother's fecund years. Both factors are known to affect children's health.

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stage. Another important addition was the requirement on the part of clinics and doctors who provide ultrasound services to maintain written records that specify the specific reason why an ultrasound or amniocentesis test had been recommended. For a description of the PNDT Act and its enforcement, see Mallik (2003) and George (2002).

[9] For the state of implementation and enforcement of the Act see the section on the PNDT Act in annual reports published by the Ministry of Health and Family Welfare at http://pndt.gov.in/index1.asp?linkid=15 and the report of the National Commission for Women, India at http://ncw.nic.in/Pdfreports/PC_PNDT_REPORT.pdf.

[10] Differences in parents' utility from boys and girls might be rooted in taste differences or in differences in the costs and benefits of raising boys versus girls (Ben-Porath and Welch, 1976). Arnold et al. (1998) provide a review of the literature and highlight three main channels that affect parental preferences for sons. The first is economic utility due to differences in labor market productivity between boys and girls, security in the case of illness, support in old age and inheritance rules. A second channel is related to social utility, which provides families with more sons a higher status and additional sources of income in the form of dowry payments. A third channel is religious utility, which is derived from the performance by sons of important religious functions, especially upon the death of his parents. Girls, in contrast, are seen as an economic drain on family resources because of the dowry system and the high cost of weddings.

Increased access to prenatal sex selection may improve the outcomes of girls who are born through various channels. First, girls may be more likely to be born into families that have a lower disutility from girls and therefore may suffer less from discrimination in resource allocation within the household. As suggested by Goodkind (1996), prenatal sex selection may substitute for postnatal sex discrimination. Second, prenatal sex selection allows parents to achieve their preferred number of boys and girls without the need of having additional children. [11] Consequently, family size will not necessarily be larger for girls. Prenatal sex selection might also improve girls' outcomes if parents of girls internalize the potential increase in the value of women as they become scarcer in marriage and labor markets.[12] On the other hand, prenatal sex determination might have a negative impact on girls' outcomes if parents reduce prenatal investment in female fetuses in response to prenatal sex determination rather than having an abortion (see, for example, Bharadwaj and Nelson, 2010 and Almond et al., 2010). Finally, girls' outcomes might also change (improve or worsen) through selection if parents who practice prenatal sex selection have different characteristics than parents who do not. Taken together, these various channels imply that the sign of the overall impact of prenatal sex determination on the outcomes of girls who are born cannot be determined a priori and remains an empirical question.

IV. DATA

The data for our empirical analysis are taken from the National Family Health Survey (NFHS). The NFHS is a large-scale, multi-round survey conducted in a representative sample of households throughout India.[13] Three rounds of the survey were conducted in 1992-3, 1998-9 and 2005-6. Each round covered approximately 90,000 households which contained more than

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[11] On the other hand, the ability to choose a child's gender increases the expected utility of children and might therefore increase the likelihood of having additional children. Family size might also increase with access to prenatal sex selection if the net cost of raising boys is lower than the cost of raising girls (Ben-Porath and Welch, 1976) or if boys yield higher utility than girls and there is substitution between utility from children and ordinary consumption or between utility from boys and utility from girls. Therefore, the impacts of prenatal sex selection on family size are ambiguous. Nevertheless, the gender gap in family size is expected to narrow or even reverse.

[12] This and the first channel imply that girls are becoming "more wanted". Although the first channel works through selection across households with different preferences, whereas this channel works through a general equilibrium effect changing preferences over time for all households, even those who do not practice prenatal sex selection.

[13]The NFHS was designed along the lines of the Demographic and Health Survey (DHS) that has been conducted in many developing countries since the 1980s.

500,000 individuals and was designed to provide state-level and national-level estimates. The survey includes detailed information on the demographic and socioeconomic background of the household members, as well as additional modules designed to investigate health, fertility and mortality. One major advantage of these data is that they record complete birth histories (including deaths and children living outside the household) of a large number of women over a long period, making it feasible to compute MFR at birth by state and cohort.[14]

We pooled the three survey rounds and selected only households with ever married mothers aged between 15 and 49 in order to obtain a consistent sample across the three rounds. **Table 1** reports summary statistics for boys and girls included in our main samples (columns 1, 2, 4, and 5) and differences by gender (columns 3 and 6). The sample reported in columns 1-3 (*nutrition sample*) includes the youngest two children aged less than three of ever-married women sampled in one of the three NFHS rounds who have valid anthropometric data.[15] There are 76,500 children (36,940 girls and 39,560 boys) who satisfy these criteria. The sample reported in columns 4-6 (*mortality sample*) includes all children of ever-married women born within the 10 years preceding each survey round. This adds up to 357,809 children (172,472 girls and 185,337 boys). Household characteristics reported in the table are used as control variables in the empirical analysis.

Most children (about 75 percent) in both samples live in rural areas. About half of the children have mothers with no formal education and about 30 percent have fathers with no formal education. Mothers' age at first birth is relatively low at 19, on average. At the time of the survey, mothers in the *nutrition sample* were 25 years old, on average, and they had an average of 3 children. Mothers in the *mortality sample* are 29 years old on average at the time of the survey and have an average of about 4 children.[16]

Columns 3 and 6 show that girls tend to be born into more disadvantaged families than boys. Their families have lower wealth levels, lower parental education, and a lower degree of

[14] Note that MFR estimates reported in many studies are based on census data which record only children who are alive and live in the household at the time of the census. Therefore, they are affected by differential mortality and living arrangements.

[15] The first round of the NFHS collected anthropometric data for the youngest two children in the household who were under four years of age. The second round restricted the anthropometric data collection to the youngest two children in the household under the age of three, while the third round extended the data collection to all children in the household under five years of age. To be consistent across survey rounds, we restrict the sample to the youngest two children under three years of age. In practice, our results are insensitive to these restrictions.

[16] Note that the average number of children in our samples does not represent completed fertility as most women are still in their fertile years.

exposure to mass media.[17] A possible explanation for differences in the family characteristics of girls versus boys is the practice of sex selective abortion. Another fact worth noting is that girls appear to have more siblings than boys in *the mortality sample* (columns 4-6), which is consistent with parental stopping rules in fertility behavior and son preference. There is no similar difference in the *nutrition sample* (columns 1-3), probably due to the fact that a large proportion of children in the sample comes from households with incomplete fertility. Nevertheless, we see in both samples that mothers of girls are more likely to report that they want to have another child. Differences in this variable are larger in the *nutrition sample* since mothers are more likely to have uncompleted fertility. We will return to the differences in household characteristics and family size in section VI.2 when we discuss possible mechanisms underlying our main results.

V. EMPIRICAL STRATEGY

Assessing the causal effect of prenatal sex selection on girls' outcomes is a challenging task. Experiment with random assignment is not, and will never be, feasible to implement. Moreover, practice of prenatal sex selection is not easily observed or measured. Our empirical strategy is to use male-female ratios at birth (MFR) to proxy prevalence of prenatal sex selection and exploit the variation in MFR across states and over time to estimate the impacts on girls' outcomes while trying best to isolate other confounding factors. In subsection V.1 we show how male-female ratios at birth (MFR) vary across time and states and demonstrate that increases in MFR at birth are a consequence of prenatal sex selection. In subsection V.2 we explain how we use this variation to study the impacts of prenatal sex selection on girls' outcomes.

V.1. Incidence of prenatal sex selection and male-female ratios at birth (MFR)

As mentioned above, one potential limitation of this analysis is that we do not directly observe the practice of sex selective abortion. However, we do observe its consequences, primarily the abnormal sex ratio at birth. **Figure 1** shows state variation in male-female ratios (MFR) at age 0

[17] The index for media exposure is defined by the sum of indicators for exposure to TV, radio and newspapers or magazines. Each indicator receives a value of one if the mother reported exposure of at least once a week or almost every day. The wealth index is a constructed index provided in the NFHS data. The index is based on household assets and housing characteristics and denotes the wealth quintile of the household relative to all households sampled in the same survey round.

as reported in census records from 1961 to 2001.[18] Until the 1980s, MFR at birth did not exceed the normal ranges of 103-107 males per 100 females found in various large-scale studies (for example, Visaria, 1971 and Jacobsen et al., 1999). Increases in MFR at birth become evident at the transition points of 1981, 1991 and 2001, which overlaps with the diffusion of ultrasound technology in India.

As shown in **Figures 2a and 2b,** which plot sex ratios in urban and rural areas respectively, MFR at age 0 has also increased in rural areas since the 1980s though the trend has lagged somewhat behind that in urban areas. This is also noted by Retherford and Roy (2003) who examine sex ratios at birth over the first two rounds of the NFHS and report higher sex ratios in urban as opposed to rural areas in the first round but find no evidence of a rural/urban differential in the second round once socioeconomic variables are controlled for.

Interestingly, there are large variations in MFR (both in their levels and in their growth rates) even across those states that appear to have a strong preference for boys. To illustrate this point, **Table 2** reports MFR at birth by state for various census years (columns 1-5) and indicators of fertility and preference for number and sex composition of children based on tabulations from the first round of the NFHS (columns 6-9).[19] States are grouped by region.

The largest increases in sex ratios at birth are found in the northern and western states, which are characterized by a strong degree of son preference. In Punjab, for example, while MFR was within the normal range between 1961 and 1981, it increased dramatically between 1981 and 2001 from 106 to 129. In Gujarat, MFR remained at 103 between 1961 and 1981 but increased from 103 to 116 between 1981 and 2001. Both northern and western states appear to have strong son preference as manifested by the ideal sex ratio reported by mothers (1.46 and 1.33) and the proportion of mothers who desire a larger number of sons than daughters (0.48 and 0.42).

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[18] MFR from census records number of children aged 0 to 11 months who are alive on census date. So, they also reflect gender differentials in infant mortality rates and living arrangements. In the next paragraphs we refer to MFR at age 0 as MFR at birth since changes in mortality and living arrangements by gender had only a negligible contribution to the overall trends in this variable.

[19] Women with living children were asked: “If you could go back to the time you did not have any children and could choose exactly the number of children to have in your whole life, how many would that be?” Women with no living children were asked, “If you could choose exactly the number of children to have in your whole life, how many would that be?” All women who gave a numerical response to the question on the ideal number of children were also asked how many of these children they would like to be boys, how many they would like to be girls, and for how many the sex would not matter.

While strong son preference is found in states with upward trends in MFR, we also observe that in several states in the northeast, central, and eastern regions with similar strong preferences for sons there was either no increase in MFR or only a mild one. In Madhya Pradesh, for example, mothers reported an ideal sex ratio of 1.44 but sex ratios at birth remained close to natural levels (MFR of 106 in 2001). Southern states are usually characterized by a low degree of son preference and stable sex ratios at birth.

Evidence reported in Table 2 suggest that strong son preference cannot alone explain state variation in MFR since there are several states with strong son preference that have not shown any significant increase in MFR. Indeed, a factor that distinguishes between states with similar son preference but different incidence in prenatal sex-selection is economic development. Northern and western states are generally more economically developed than states in the northeast, central and eastern regions.[20] On the other hand, development and income levels in many of the northern and western states are comparable to those in southern states where sex ratios have remained balanced.[21]

Overall, evidence above suggests that the primary factors which characterize states with an increasing MFR are a strong preference for boys and a higher degree of development and modernization (in combination). Still, there are some exceptions, such at the state of Rajasthan which is poorer and less developed but exhibits an increasing trend in MFR. There is also a clear geographical pattern that points to a higher incidence of prenatal sex selection in northern and western states. The fact that there are states with a strong preferences for boys and states with high levels of development that have not exhibited significant increases in MFR provide us with a heterogeneous group of states that are comparable to states with increasing MFR across different dimensions.

Figures 1-2 and Table 2 show a high variation in MFR across states and over time. But is the increase in MFR directly related to the practice of prenatal sex selection? To examine this question, we test whether the propensity of giving birth to a boy is higher among families who

[20] Appendix **Table 1A** presents several economic and demographic indicators by state for the years 1991-2. As can be seen, northern and western states have a higher wealth index, income per capita, share of households with electricity, and a higher degree of exposure to mass media (TV).

[21] In terms of women's educational level and religion, it is hard to find a clear pattern that differentiates states with increases in MFR from the rest. For example, MFR has increased significantly in Punjab, which has a high proportion of Sikhs and also in Himachal Pradesh where the majority of the population is Hindu.

might feel a stronger pressure to have a son and whether this propensity increased when prenatal sex determination became feasible. In **Table 3**, we report the likelihood of a male birth at parity *N* (two or three) as a function of the sex composition of the older siblings who were alive at the time of conception using a linear probability model. We examine two samples: children born between 1975 and 1989 and children born from 1990 onwards. This split is meant to proxy for the availability of ultrasound technology.[22] As seen in column 1, the probability of a male birth during the pre-ultrasound period did not vary significantly across households according to the sex composition of their previous children. In contrast, column 5 shows that during the post-ultrasound period this probability was significantly higher for households that had only girls. Estimates of the differential probabilities are virtually unchanged after adding controls for household characteristics (columns 2 and 6). As seen in columns 3-4 and 7-8, the differential increase in the probability of a male birth according to sex composition of previous children is evident both in rural, and, to a larger extent, in urban areas.

In **Figure 3** we plot estimates for the differential probability of a male birth by year of birth using a 5-year moving average and comparing between households with no boys among previous children and households that have already one (for parity two) or two boys (for parity three). Estimates for parity two (panel a) show that prior to 1990s the differential probability of a male birth did not vary by the sex composition of previous children. However, starting from the 1990s we see a continuous increase in the probability of a male birth among households with no boys. The figure for parity three (panel b) shows that the increase in the differential probability started a bit earlier (towards the end of 1980s), it increased over time until the mid-1990s and remained stable since then, probably due to the reduction in family size, which shifted the practice of prenatal sex selection to lower parities.

Evidence presented in Table 3 and Figure 3 shows that the likelihood of a male birth increased significantly in the 1990s among households who presumably had a stronger desire for a son. This suggests that increases in MFR at birth are likely to be a result of access to prenatal sex selection. In order to further examine the link between son preference and sex ratios at birth, additional information on ultrasound use and abortions is needed. Information on the incidence of abortions is difficult to obtain since a large share of abortions take place in

[22] The sample split at 1990 is used for illustration purposes. Similar but smaller estimates for the post-ultrasound period are obtained when we use 1985 as a cutoff year and higher estimates are obtained if we split the sample at 1995.

unofficial and non-regulated facilities. In addition, abortion is usually misreported, especially if it is carried out for sex selection purposes. We therefore examine patterns of ultrasound use.

The second and third round of the NFHS survey asked mothers whether they performed an ultrasound test for each of their births during the three (in round 2) or five (in round 3) years prior to the survey. While most ultrasound tests are performed as part of routine antenatal checkups, the association between ultrasound tests and sex ratios can provide suggestive evidence for the practice of prenatal sex selection. In columns 1 through 4 of **Table 4** we report the differential likelihood that a mother performed an ultrasound test during a pregnancy of parity N as a function of the sex composition of her N-1 previous children. As clearly seen in the table, mothers with no living sons are more likely to perform an ultrasound test in pregnancies at parities 2 and 3. This increased likelihood is present in both urban and rural areas.

The link between ultrasound use and prenatal sex selection can be further examined by looking at the associations between a male birth and the sex composition of older children among mothers who reported conducting an ultrasound test during pregnancy. Estimates shown in columns 5 trough 8 suggest that mothers who reported doing an ultrasound test during pregnancy have a significantly higher probability of giving birth to a boy if they have no older sons. For example, among mothers who performed an ultrasound test during pregnancy, those who have no sons are 7 percentage points more likely to have a boy at parity two and 10 percentage points more likely to have a boy at parity three relative to mothers who have one or two older sons respectively. The increase in the likelihood to have a boy is significantly higher both in the urban and in the rural sample.

In summary, the evidence presented in this subsection suggests that increases in MFR at birth are likely to be induced by the increasing practice of prenatal sex selection rather than by biological or environmental factors. We therefore use MFR at the state level as a proxy for the practice of prenatal sex selection.

V.2. Effect of Prenatal Sex Selection

We exploit variation in the timing and extent of prenatal sex-selection across states and examine whether changes in MFR within states and over time are systematically associated with changes in girls' outcomes relative to boys. We look at changes in girls' outcomes relative to boys who are born in the same sate and year to account for unobserved time-varying factors at the state level associated with changes in MFR. Specifically, we estimate the following equation:

$$(1)\, y_{ist} = \alpha_{s0} + \alpha_{s1}\, female_i + \delta_{t0} + \delta_{t1}\, female_i + x_i\,'\beta + \pi_0 MFR_{st} + \pi_1 \left(MFR_{st} * female_i\right) + \varepsilon_{ist}$$

where y_{ist} is the outcome of child *i* in state *s* born in year *t*, α_{s0} and α_{s1} are vectors of gender-specific state fixed effects, δ_{t0} and δ_{t1} are vectors of gender-specific year-of-birth fixed effects and x_i is a vector of individual characteristics that include indicators for twin birth, residence in an urban area, religion, mother's and father's level of education, mother's age (grouped), wealth quintiles, mass media exposure and mother's age at first birth. MFR_{st} is the Male-Female Ratio at birth for the cohort born in year *t* in state *s*.[23] ε_{ist} is the error term, which is composed of a state-specific random element that allows for any type of correlation within observations of the same state across time and an individual random element.[24]

The parameter of interest is π_1, which captures the effect of increases in MFR on changes in girls' outcomes that can be attributed to prenatal sex selection.[25] This approach is essentially a continuous version of a *triple-difference* estimation strategy. This strategy has the advantage that it allows us to control for state-level fixed factors that differentially affect boys and girls (for example, the degree of discrimination against girls in a state). We can also control for state-time-varying factors that affect boys and girls similarly within each state and cohort and which might be correlated with changes in MFR over time (for example, improvement in access to health care facilities). In addition, we control for differential trends in boys' and girls' outcomes at the national level in a very flexible way by including gender-specific year of birth fixed effects.

Identification relies on the assumption that changes in MFR within a state over time are unrelated to other unobserved factors that could *differentially* affect male and female outcomes. In this regard, it is important to note that we always include in our model the main

[23] We use a smoothed version of MFR which is computed as a 7-year moving average of the ratio of the number of male births to female births by year and state based on the pooled data of the three rounds of the NFHS survey. As we show in the next section, our results are robust to alternative smoothing methods.

[24] In all the specifications, we use the national sampling weights provided in the surveys and cluster standard errors by state. As we show in the next section our results are not sensitive to the use of this specific weighting scheme.

[25] Note that π_1 captures the overall effect of an increase in the sex-ratio at birth on girls' outcomes. This includes also any possible general equilibrium effect resulting from parents internalizing future changes in women's value due to an increase in the scarcity of girls. We will turn back to this issue in Section VI below when we examine the possible channels linking between prenatal sex selection and girls' outcomes.

effect of MFR, which gives an indicator of the effect of unobserved time varying factors at the state level correlated with MFR, on boys' outcomes. As we show later, estimates of MFR are never significant and have inconsistent signs across outcomes, reducing one of the major concerns regarding omitted variable bias. Our results are also robust to replacing the main MFR effect with a set of full interactions between state- and year-of-birth fixed effects, suggesting that state-time varying factors are unlikely to confound our findings. We further assess the plausibility of the main identifying assumption in the next subsection, where we also consider more general specifications that allow for gender-specific coefficients in all covariates and control for time-varying state characteristics interacted with gender, thus also allowing them to have a differential effect by gender.

Another concern regarding identification could arise if increases in MFR within a state over time were related to increasing discriminatory preferences against girls rather than increasing access to prenatal sex selection technology. While we cannot rule out this alternative explanation, we note that in this case, our estimates would be biased against finding any improvement in girls' outcomes and would therefore provide a lower bound of the effects of prenatal sex selection.

We first estimate equation (1) using a sample of children born at all parities. We also estimate the same equation while stratifying the sample by parity. Estimates based on a sample of children at all parities (as opposed to estimates that condition on parity) are less susceptible to selection bias due to secular decreasing trends in family size or due to reductions in family size generated by prenatal sex selection. Nonetheless, it is interesting to examine differential effects by parity given that the extent of prenatal sex selection varies by birth order and the likelihood that girls are treated differently at different parities.[26]

Note that we do not include household fixed effects in the estimating equations. This is due to several reasons: First, identification in a household fixed-effect model would rely on a comparison between siblings of the opposite sex. However, households with children of opposite sex cannot be viewed as a randomly selected sample since the sex composition of children is affected by parental stopping rules in fertility behavior and prenatal sex selection. In addition, given that our main sample includes children born within three years prior to the survey date, a household fixed-effects model would generate estimates for a selected sample of

[26] Kishor and Gupta (2009) show that MFR increased over time at parities two, three and four and, to a lesser extent, at parity one, while it seems to have remained relatively stable and at normal levels at parities higher than four. For evidence on differential treatment of girls by parity, see Mishra et al. (2004).

households (i.e. those which had two births within the last three years). Note also that girls' outcomes might change due to selection into different households or a differential reduction in family size. We are therefore interested in the well-being of girls across all families and not necessarily within the same household.

VI. EMPIRICAL RESULTS

1. MAIN RESULTS ON NUTRITIONAL OUTCOMES

Girls' well-being can be analyzed across two dimensions: the allocation of household inputs such as material resources, time, health care and feeding practices or children's outcomes such as nutrition, morbidity and mortality. We focus our analysis on children's nutrition and mortality since they embed information on various types of parental input. These outcomes are measured more easily than most others and are therefore less likely to suffer from measurement error or recall bias. We do not examine morbidity since it is more likely to suffer from bias due to gender differences in diagnosis and recall.[27]

We measure children's nutritional status by anthropometric indicators based on height, weight and age. In particular, we consider three indicators of malnutrition: stunting, underweight and wasting. All three are defined based on z-scores, which are computed by subtracting the median and dividing by the standard deviation of a reference population of the same age and gender. Specifically, a child is considered stunted if his/her height-for-age is at least 2 standard deviations below the median of the reference population (or the associated z-score is smaller than -2). An underweight child has a weight-for-age at least 2 standard deviations below the median, and a wasted child has a weight-for-height at least 2 standard deviations below the median.

The three indicators capture malnutrition from different perspectives. Stunting reflects long-term malnutrition or cumulative nutrition from conception and is also affected by recurrent or chronic illnesses. Wasting measures acute malnutrition and represents the failure to receive adequate nutrition in the period immediately preceding the survey and may be the result of inadequate food intake or a recent episode of illness leading to weight loss. An important feature of the wasting indicator is that it does not depend on the accuracy of age reporting. On the other hand, it is more sensitive to seasonal shocks. Underweight is a composite index of

[27] For example, Timaeous et al. (1998) report sex bias in reporting and recalling episodes of disease and sex differentials in childhood risks of illness and illnesses severity perception.

chronic or acute malnutrition. Note that z-scores are normalized by gender and age so that they take into account that boys and girls may follow different growth trajectories.[28] Our analysis uses z-scores based on the US National Center for Health Statistics (NCHS) standard, which was the most commonly used measure until 2006.[29] About 18 percent of the children aged 0 to 35 months included in our sample have missing values in at least one of the anthropometric indicators. Nevertheless, we do not find any significant gender differences in the likelihood of having a missing value in these indicators. Moreover, we do not find any associations between state variation in MFR and the likelihood of missing anthropometric data for girls or boys.[30]

A methodological challenge in the analysis of nutritional outcomes on the basis of anthropometric data is the availability of these data only for surviving children. Therefore, if prenatal sex selection has any impact on mortality, our analysis of nutrition will be based on a selective sample. For example, if increases in MFR are associated with a reduction in female child mortality, they might lead to an increase in the proportion of girls who are close to a survival threshold, thus attenuating the estimated effect of MFR on nutritional status. As shown below, the effects on mortality are negligible or not significant, which mitigates the concern about selectivity issues in the nutritional status results.

Table 5 reports estimates for *MFR* and *female*MFR* from a linear probability model for the likelihood of being underweight, wasted, or stunted.[31] Estimates from a basic model that includes no covariates, except for a female dummy, and gender-specific state and year-of-birth fixed effects are reported in columns 3 and 4. Columns 5 and 6 report estimates for the full model specified in equation (1) which controls also for the household characteristics reported in Table 1. The key parameter of interest is the coefficient of the interaction term *female*MFR* (column 6), which is negative for all three outcomes and, except for stunting, is statistically

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[28] In an alternative specification, we included age-in-months and its interaction with *female* (instead of child's year-of-birth) in order to control for any differences in child outcomes that could result from differences in the age distribution of boys and girls. The results are virtually identical to those presented here.

[29] A new international reference population was published by the World Health Organization in 2006. As we show in Table A2, our results are not sensitive to the specific reference chart used to define z-scores.

[30] Height was not measured in the first round of the NFHS in Andhra Pradesh, Himachal Pradesh, Madhya Pradesh, Tamil Nadu and West Bengal because height measuring boards were not available at that time (IIPS, 1995). Nevertheless, our results for weight are highly consistent with the results for height and our main results are unchanged when we limit the sample to states that have anthropometric data in the three survey rounds. We therefore believe that the lack of height data in round 1 for some states is unlikely to bias the main results.

[31] Marginal effects from logit models provide similar results.

significant. Note that the coefficients for the main effect of *MFR* (column 5) have inconsistent signs across outcomes and are never significant, suggesting that changes in MFR are unrelated to changes in the nutritional outcomes of boys. This last finding is important since it suggests that increases in MFR at the state level are unlikely to be associated with other changes in unobservables that affect children's nutritional status. It is also worth noting that our estimates for the interaction between MFR and female are largely unchanged after the addition of household covariates (see columns 4 and 6) minimizing some concerns of omitted variable bias.

Taken together, the results suggest that girls' nutritional status improved relatively more in regions where the incidence of prenatal sex selection (as proxied by MFR) increased. In other words, an increase in the practice of prenatal sex selection appears to be associated with a reduction in the incidence of malnutrition among surviving girls.

Note that the key *female*MFR* coefficient reflects the differential change in girls' outcomes that results from a one-unit increase in MFR. For example, the estimate for underweight from column 6 indicates that a one standard deviation increase in MFR (7 points) is associated with a 4 percentage point reduction in the proportion of girls who are underweight, which is a reduction of 8 percent relative to the mean outcome for girls. Taking Punjab as an example, this would mean that the 20-point increase in MFR observed between the first and third round of the NFHS is associated with a 10 percentage point reduction in the proportion of girls who are underweight. An alternative metric can be computed using population figures from the 1991 and 2001 census. During this period, the fraction of missing girls from the total number of expected female births increased in Punjab by 11 percentage points (from 11 to 22 percent) while our estimates predict a reduction in the proportion of underweight girls of 6 percentage points.[32]

Previous studies of gender discrimination show differential patterns for urban and rural populations and usually find a higher incidence of discrimination against girls in rural areas (see, for example, Rosenzweig and Shultz, 1982; Simmons, 1982; Subramaniam and Deaton, 1991; and Deaton, 1997). Following this approach, we also look at the differential effects of prenatal

[32] According to census figures, MFR in Punjab increased from 1.171 in 1991 to 1.285 in 2001. Therefore, the predicted decrease in the proportion of underweight girls is (1.285-1.171)*0.536=0.06. The proportion of missing girls is computed using the census figures for the total number of boys and girls aged zero in both periods (232,630 and 198,740 in 1991 and 176,541 and 137,349 in 2001) and estimating the expected number of girls under the assumption of a natural MFR of 1.05 (221,552 for 1991 and 168,134 for 2001).

sex selection on girls' outcomes in rural and urban areas.[33] We stratify the sample by place of residence (urban/rural) and then estimate equation (1) separately for the two samples.[34]

Estimates for the rural/urban stratification are reported in Panels A and B of **Table 6.** Column 2 shows that children are more likely to be malnourished in rural as opposed to urban areas. Estimates for *MFR* and its interaction with *female* (reported in columns 3 and 4) show that relative improvements in girls' nutritional status associated with increases in MFR are larger in rural areas than in urban areas. These results suggest that the improvement of girls' nutritional status following the diffusion of prenatal sex selection is more pronounced in rural areas.[35] The larger improvement in girls' outcomes in rural areas is consistent with the literature in showing that gender discrimination in resource allocation is more prevalent among more constrained families.

We also stratify the analysis according to parity since our earlier results show that prenatal sex selections are more likely to occur at higher parities. Panels C and D of **Table 6** report estimates of the effect of *MFR* and its interaction with *female* for the sample of children born at parity one and the sample of children born at parity two and above. The results provide some evidence for the stronger effect of MFR in improving girls' nutritional status at parities higher than one (in particular with respect to wasting). This is consistent with the fact that prenatal sex selection is more prevalent at parities higher than one and therefore is likely to reduce the proportion of unwanted girls at these parities.

1.c. Robustness Checks

We performed additional tests to assess the validity of our identifying assumptions and check for the robustness of the main results. The results of these tests are reported in Appendix **Table A3**. In order to facilitate comparison, we reproduce the estimates obtained in our main specification in the first row of the table. Overall, the additional estimates are qualitatively similar across various models and specifications.

[33] Note that our earlier analysis showed that while prenatal sex selection was more prevalent in urban areas, there is an increasing trend in the use of prenatal sex selection in rural regions.

[34] We measure MFR at the state level since there are insufficient observations in the NFHS data to compute separate sex ratios for the urban and rural samples within each state for each birth cohort. Evidence from census data suggests a high correlation between MFR in rural and urban areas within each state. For example, the correlation is about 0.7 for the 1991 census and 0.9 for the 2001 census.

[35] Note also that since most (about 75%) of the population lives in rural areas in a majority of states, our estimates for the rural sample are more precise thanks to the larger sample size and better proxy for MFR which uses state-level data.

First, we assess the validity of our main identifying assumption that there are no changes in unobserved factors associated with MFR that could affect girls' outcomes. Note that this is partially addressed by the triple-difference strategy which controls for state-level time-varying factors that affect boys and girls similarly by including a main MFR effect. As we note before, the main effect of MFR is never significant and is inconsistent across outcomes suggesting that conditional on other covariates included in our model, changes in MFR are not associated with changes in other factors that affect boys' outcomes. We also consider a more flexible specification with the main MFR effect being replaced by a set of full interactions between state- and year-of-birth fixed effects to allow for unobserved state-specific time-varying factors. Estimates for the interaction term between female and MFR are virtually unchanged for the three outcomes (see Table A3 row 2) suggesting that our results are unlikely to be driven by state-time-varying unobserved factors associated with MFR that affect child outcomes.

Still, there may be state-time-varying factors that differentially affect boys and girls. This could take place if increases in MFR are related to more rapid economic development and modernization, which in turn may affect girls' and boys' health outcomes differentially (for example, by providing better health care access or by means of a reduction in discrimination against girls). A possible way to account for unobserved time-varying factors that differentially affect both genders would be to control for state-gender-specific trends. However, this approach is less suitable to our set-up as we do not have enough observations from cohorts born before prenatal sex selection to extrapolate a long-term trend. Moreover, MFR evolves following a close to linear trend after the diffusion of prenatal sex selection. Therefore, the inclusion of gender-state-specific linear trends absorbs most of the variation in MFR and magnifies any possible measurement error in this variable (Griliches and Hausman, 1986).

We therefore apply an alternative approach where we estimate expanded versions of equation (1) in which we control for household and time-varying state characteristics and their interactions with gender. Specifically, we estimate three models with alternative sets of additional controls. In the first set, we add interactions between household covariates and a female dummy to the basic model. In the second and third specifications, we include, in addition to gender-specific controls for household characteristics, two alternative sets of state-level time-varying covariates interacted with gender. The first set of state variables is compiled from the NFHS data and includes state means of the wealth index and the proportion of households in urban areas, proportion with electricity access, and proportion with access to TV. The alternative set contains state information on net domestic product and per-capita net domestic product (compiled from reports of the India's central bank -- the Reserve Bank of India).

The estimates for the three expanded models, reported in rows 3 through 5 of **Table A3,** are largely similar to those obtained in our main specification suggesting that our results are unlikely to be driven by unobserved changes at the state level correlated with MFR that had a differential impact on boys and girls.

We further assess the likelihood of the main identifying assumption by examining the sensitivity of our results to the inclusion of additional covariates associated with improvement in health outcomes. We first add to our main specification controls for birth weight and its interaction with female. While birth weight could itself be affected by prenatal sex selection, we are interested in the sensitivity of our results to the inclusion of this covariate and its interaction with female as it should absorb any changes at the state level that differentially affect boys and girls nutritional status at birth. Unfortunately, only 30 percent of the sample has data on birth weight. However, almost all children (99 percent) have valid information regarding their size at birth as reported by mothers (very large, larger than average, average, smaller than average or very small). We find that mother's report on size at birth is highly correlated with birth weight for the subsample of children who have valid data on birth weight. We therefore impute birth weight for those with missing data using size at birth and all additional covariates.[36] We then re-estimate our main model after including controls for birth weight, an indicator for imputed birth weight, and their interactions with female. In an alternative specification, we include indicators for size at birth and their interactions with female instead of birth weight.

The results of these two specifications are reported in rows 6 and 7 of **Table A3** and confirm that our estimates of MFR and their interaction with female are largely unaffected by the inclusion of these additional covariates.[37] This result suggests that for our estimates to be biased, there should be unobserved time-varying factors correlated with MFR that differentially affect boys' and girls' nutritional outcomes *after* birth but not *at birth*.

[36] Specifically, we impute birth weight by estimating separate regressions for each gender and survey round of birth weight on size at birth, year of birth, state fixed effects, and all household covariates.

[37] Note that if prenatal sex determination affects birth weight the estimates from these two specifications that control for birth weight and its interaction with female would be attenuated. Interestingly, we find that our estimates are not reduced after controlling for birth weight. Indeed, in an additional analysis we estimated two models where we regressed birth weight or an indicator for small size at birth on MFR and its interaction with female and all additional covariates specified in equation (1) and found that the estimates of MFR and its interaction with female were not significant.

As an additional check, we also estimated a specification that controls for delivery at a health facility and its interaction with female in an attempt to proxy for improvements in the availability of health care that could differentially affect boys and girls outcomes. Again in this case, we find that our results (reported in row 8 of the table) are not sensitive to the addition of these additional covariates.

We also tested the sensitivity of our results to the specific definition of the anthropometric indicators. As noted above, in 2006 the World Health Organization published new growth standards for assessing children's nutritional status. We re-defined the anthropometric indicators using the new WHO tables and re-estimated the main specification. Estimates reported in row 9 of the table show that, despite the nontrivial nonlinear changes in the definition of outcomes, the coefficient of *MFR* and its interaction with *female* remain virtually unchanged.

In rows 10-12 of the table we assessed the robustness of our results to the specific definition of the main explanatory variable. Note that our basic specification includes MFR in a linear fashion although it may be the case that the relationships between MFR and the outcomes of interest are nonlinear. We therefore re-estimated equation (1) after replacing MFR with its natural logarithm. The estimated effects reported in row 10 are of comparable magnitude to our main results.[38] We also experimented with different ways of smoothing MFR. For example, we considered both a shorter and a longer window for computing the moving average. The results, which are reported in rows 11 and 12, are similar to those obtained from our main specification.

Finally, we also looked at the sensitivity of our results to the weighting scheme of the data by re-estimating our model using unweighted data (row 13) and using state weights (row 14). All the estimates are similar to the main results.

To further assess the robustness of our results, we also estimate the impacts of prenatal sex selection using a discrete version of the triple-differences approach. We stratify states in two groups: *treated* states (states in the northern and western regions) which, as discussed earlier, exhibit stronger son preference and an increasing trend in the practice of prenatal sex selection and *comparison* states (which include all other states). We then examine the differential change

[38] For example, the coefficient on female*ln(MFR) in the underweight regression is -0.609 (s.e.=0.147) which implies that a 20-point increase in MFR from 1.05 to 1.25 (or 17%) is associated with a decrease of 10 percentage points (17*0.609) in the proportion of underweight girls, which is similar to the results reported in Table 5.

in girls' and boys' outcomes in northern and western states relative to other states over the three survey rounds. In **Appendix 1**, we discuss in more detail the classification of states and the estimated model. Estimates from this specification, reported in appendix **Table A2,** are highly consistent with our main results and show an improvement in girls' nutritional status in treated states. Also in accordance with our main results, we find no changes in the nutritional status of boys in treated states relative to other states.

2. MECHANISMS

The improvement in girls' nutritional status may result from an increase in the proportion of girls born into households who actually want them (substitution of prenatal discrimination for postnatal discrimination). It could also be the result of selection, such that girls are born into families with better endowments. An additional channel that could explain the results is family size, which may have declined due to lessened reliance on stopping rules in fertility behavior. Finally, girls might be treated better if their parents internalize the change in the future value of women as they become a scarce commodity in marriage and labor markets, even in families that do not practice prenatal sex selection. We explore below these different channels. The assessment of the relative contribution of each channel is beyond the scope of this paper and is left to future research.

2.a. Are girls born into better-endowed families?

In order to assess the selection channel, we examine whether family characteristics of girls (such as parental education, mother's age at first birth and household wealth status) improved relative to boys in states where MFR has increased. This is done by estimating a triple-differences equation in which we regress each of the family characteristics on *MFR* and *female*MFR* in a model that controls for state fixed effects, year of birth fixed effects and their interactions with a female indicator.

Table 7 reports estimates for the main MFR effect and the interaction between *MFR* and *female*. The table also reports gender differences in household characteristics (column 3). Estimates of the main MFR effect reported in column 4 (rows 1-6) suggest that states with increasing MFR experienced improvement in some family characteristics, in particular, an increase in the level of parental education and mother's age at first birth and a decline in the likelihood of living in a rural area. This is consistent with the fact that the practice of prenatal sex selection is related to economic development. On the other hand, there is no association between MFR and maternal age or the household wealth index.

Estimates for the interaction between *MFR* and *female* reported in column 5 (rows 1-6) are all small, not significant and have inconsistent signs across the different variables. These results suggest that there is no differential improvement in household characteristics among families with girls relative to families with boys and therefore the improvement in girls' nutritional status is unlikely to be explained by the fact that girls are born into *"better"* families.

The positive association between MFR and some household characteristics raises some concerns regarding our main results for girls' nutritional outcomes if an improvement in household characteristics has a differential effect on the nutritional status of boys and girls. Nevertheless, as reported in Table A3 and discussed in the previous section, our main results were virtually identical to those obtained from an enriched model in which we allow for a differential effect of household characteristics by gender. This suggests that the improvement in parental education or other household characteristics did not have a differential effect on girls' nutritional status.

2.b. Are girls born into smaller families?

As noted above, a direct consequence of parental stopping rules in fertility behavior and son preference is that girls are more likely to be born in larger families. With access to prenatal sex selection, parents of girls would not necessarily need to have more children in order to attain the desired sex composition. As a result, girls might fare better due to an increase in family resources per child. We therefore examine whether family size has differentially changed for girls relative to boys in regions with increasing MFR. To this end, we regress family size on *MFR* and *female*MFR*. The model also controls for state fixed effects, year of birth effects and their interactions with a female indicator.

Row 7 of Table 7 reports the coefficients of *MFR* and *MFR* interacted with *female*. The estimate reported in column 4 shows a negative association between MFR and family size which may be due to various factors. One possibility is reverse causality. That is, a desire to have fewer children may increase the demand for sons at lower parities, thus increasing the incidence of prenatal sex selection. A second possible channel may be related to unobserved factors that affect both MFR and fertility. For example, economic development is generally associated with a reduction in fertility and an increase in access to ultrasound technology. A third channel may arise due to a causal link from access to prenatal sex selection to family size. Parents with access to prenatal sex selection are more likely to attain their preferred sex composition of children without the need of having additional children. The first two channels should have a similar effect on family size for boys and girls while the last one is expected to have a differential effect by gender with larger reductions for girls. As seen in column 5, the estimate for the interaction between *female* and *MFR* is indeed negative (-0.923) and marginally significant (s.e.=0.534). Moreover, estimates reported in the last row of the table show a significant larger reduction in the average number of older siblings for girls relative to boys (estimate=-0.980; s.e.=0.557) which is explained by the reduction in the probability that girls are born at higher parities. These results confirm two expected consequences of prenatal sex selection: a larger reduction in family size for girls relative to boys and a decline in the proportion of girls born at higher parities. Both factors are likely to increase the amount of household resources available for girls.

2.c. Are girls more likely to be wanted?

An additional expected consequence of prenatal sex selection is that girls would be more likely to be born in families that want them. Girls might also be more likely to be wanted if their parents internalize the potential change in the value of women as they become a scarce commodity even in families that do not practice prenatal sex selection. A unique feature of our study is that we are able to assess these channels by examining the association between MFR and parental preferences for sons. We focus on two measures of son preference: the ratio of the ideal number of sons to the ideal number of daughters reported by mothers and an indicator that equals one if the mother reports a desire to have a larger number of sons than daughters. We estimate our main model using these two measures of son preference as dependent variables. Note that we cannot establish a causal relationship between MFR and son preference since the link between the two can go in both directions. In addition, there may be other unobserved factors that affect both simultaneously. Nevertheless, it is still interesting to examine the association between MFR and son preference, in particular, the differential association by child gender as any bias related to reverse causality or omitted variables is expected to affect boys and girls similarly.

We report in Table 8 estimates for MFR and its interaction with female for the sample of children aged 0 to 35 months.[39] We also report estimates for a sample that excludes extreme cases of ideal MFR (the upper 0.1 percent of the sample with an ideal sex ratio larger than 4). In all specifications and samples, estimates for the main MFR effect are small and not significant.[40] In contrast, the interaction between MFR and female is always negative and significant or marginally significant. Taken together, these results show two important things. First, increases in MFR do not appear to be associated with a decline in son preference. Second, girls are more likely to be born in families with weaker preferences for sons in states where MFR is increasing. That is, an increasing incidence of prenatal sex selection increases the chances that girls are born in families that actually want them. On the other hand, we do not find a change in son preference in states with increasing sex ratios at birth.

2.d. Are girls receiving better treatment from parents?

An improvement in girls' nutritional status may be a direct consequence of better care and treatment in the family since girls are more likely to be wanted when prenatal sex selection is available. Parental treatment is difficult to assess since we do not directly observe parents behavior. Nevertheless, we can examine mothers' reports on breastfeeding duration as one possible indicator for child care and treatment.[41]

Medical and public health research has found that breastfeeding has large benefits for children's health, especially in environments with poor sanitary conditions (see, for example,

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[39] The sample size is larger than our nutrition sample since we do not restrict it to children who have valid anthropometric data.

[40] To address the issue of reverse causality, we also analyze a separate sample of younger women aged 15 to 20 and examine the relationship between their reported son preference and the MFR computed based on children born 3 or 5-10 years prior to the survey date. Presumably reverse causality would be less relevant in these cases. We look at two different samples: ever married women aged 15-20 and ever married women aged 15-20 who have no children (we cannot look at all women unconditional on marital status because son preference was asked only to married women). The results reported in **Appendix Table A4** confirm that son preference changed little in states with increasing sex ratios.

[41] Another potential measure of parental input is vaccination. However, as discussed in Barcellos et al. (2010), evidence from the NFHS data on gender differential in vaccination rates has been mixed. The results vary dramatically depending on whether vaccination is measured based on information recorded on vaccination cards or based on mothers' reports. In this context, selection bias could be a big concern since very few mothers (only about 30%) had a vaccination card and they are also more likely to have a card for boys than for girls. An additional problem with vaccination data from the NFHS is an apparent decline in the record of immunization information in the third round of the NFHS in some states due to logistic and methodological issues, especially among children with no vaccination card (see Chandran et al. 2011). For these reasons, we do not focus on this outcome.

The World Health Organization, 2000). The WHO recommends that breastfeeding in developing countries should continue, in combination with supplementary foods, up to the child's second birthday or beyond (WHO, 1991).

Although breastfeeding is nearly universal in India, girls tend to be breastfed for a shorter period than boys. Parents with son preference may breastfeed their sons for a longer period than their daughters either due to direct discriminatory treatment or passively through stopping rules in fertility behavior as mothers stop breastfeeding their daughters sooner in order to conceive again quickly (Jayachandran and Kuziemko, 2010). With access to prenatal sex selection girls' breastfeeding duration might increase either due to lower parental discrimination in child care and treatment or due to a decline in parental pressure to conceive a son after the birth of a daughter.

We examine the effect of prenatal sex selection on girls' breastfeeding duration by estimating equation (1) with the dependent variables being indicators of whether the child was breastfed for at least 12, 18 or 24 months. These indicators were chosen in order to account for heaping of observations at six–month intervals (either due to rounding error in duration reporting or actual propensity to breastfeed up to a focal point). In order to take into account the possibility of right-censoring in duration, the indicators are defined conditional on children being at least 12, 18 or 24 months old, respectively, at the time of the survey.

We estimate the model using our main sample (the youngest two children born within 3 years prior to each survey round) as well as for samples stratified by rural/urban residence and parity (i.e., the same samples used for the analysis on nutritional status). Results are reported in **Table 9.** The differences in outcome means reported in column 3 show a disadvantage for girls in breastfeeding duration that widens with age. For example, girls are one percentage point less likely than boys to be breastfed for at least 12 months, about 4 percentage points less likely to be breastfed for at least 18 months and almost 6 percentage points less likely to be breastfed for at least 24 months. Girls' disadvantage in breastfeeding duration is larger in rural areas and in parities higher than one (Panels B and E).

Estimates for the interaction between *MFR* and *female* suggest some increase in duration of girls' breastfeeding at long durations (24 months or longer) in states with higher increases in MFR. Similar to the results for nutritional status we find a larger improvement in rural areas and at parities larger than one. These results are broadly consistent with the findings from Tables 5 and 6 and suggest that breastfeeding practices may be another channel through which prenatal sex selection improves girls' nutritional outcomes. The increase in breastfeeding duration is also consistent with our results that show an increase in the proportion of girls who are wanted and a larger reduction of family size for girls. As discussed earlier, these two factors are likely to explain gender difference in breastfeeding duration.

3. MORTALITY

Gender preferences and discrimination may also affect child mortality. Indeed, a large literature has documented excess female mortality which becomes evident after the age of one month. In this section, we examine the effect of prenatal sex selection on child mortality. We focus on the following set of binary outcomes: neonatal mortality (death under 1 month of age), post-neonatal mortality (death between 1 month and 12 months of age, conditional on surviving up to 1 month), child mortality (death between 1 and 5 years of age, conditional on surviving up to 1 year) and under-five mortality (death before 5 years of age).[42] For each outcome, we estimate a linear probability model with a specification similar to that of equation (1).[43] We consider two samples: the first includes the last two children born within 3 years prior to each survey and parallels the sample selection criteria used for the nutritional outcomes and the second includes all children born within 10 years prior to each survey.

The results are reported in **Table 10**. Consistent with the findings of previous studies, we find excess female mortality after the age of one month. Nevertheless, we do not find a larger reduction in female child mortality in states with a high incidence of prenatal sex selection. The coefficients on the key interaction term *MFR*female* (reported in columns 5 and 8) are roughly zero in all outcomes and samples and none of them is statistically significant. We did some further analysis stratifying the sample according to place of residence (rural/urban) and parity, but did not find any consistent evidence for reductions in female mortality.

The results for child mortality are somewhat at odds with our previous findings on nutritional outcomes, family size and breastfeeding duration. Particularly puzzling is why we find a differential improvement in female nutritional status but do not see any significant reduction in excess female mortality. One possible explanation is that families that substitute between prenatal and postnatal female discrimination are not the same families characterized by excess

[42] In order to deal with possible right-censoring in duration, the indicators are only defined for children who would be "old enough" (for example 1 month, 1 year and 5 years old, respectively) at the time of the survey.

[43] Results from logit models (not reported here) provide similar findings.

female child mortality. A second possible explanation is that differential recall (by gender) of deaths and measurement error may be biasing the estimated MFR effects towards zero. The problem may be more severe with longer recall periods as is the case for the second sample which includes all children born within 10 years prior to each survey. By comparison, the analysis of nutrition outcomes of living children (born within 3 years prior to the survey) is based on anthropometric measures observed at the time of the survey and is thus less likely to be affected by these biases. Indeed, the estimates for the interaction between *MFR* and *female* for the mortality outcomes using the sample of younger children are negative although not precise enough to be statistically significant.

A third possible explanation is that while the nutritional status of surviving girls has improved the magnitude of the improvement is still not large enough to reduce the likelihood of death for marginal girls. Medical research suggests that the leading contributors to child mortality are respiratory ailments and infectious and gastro-intestinal diseases. Malnutrition, while often underlying and exacerbating these diseases, is not by itself a fatal factor, except in severe or extreme cases.[44] It is also possible that other types of parental investment, such as preventative care, have a more important impact on reducing diseases and thus mortality.

VII. CONCLUSION

In this paper, we study the impact of prenatal sex selection on the well-being of girls in India. To the extent that prenatal sex selection constitutes a substitute for postnatal gender discrimination, the situation of girls born after sex determination technologies became available might improve as girls would be more likely to be born in families that want them. Prenatal sex selection may also affect girls' well-being through a differential reduction in family size or a selection of girls born into different families if the characteristics of households that use prenatal sex selection differ from those that do not. In addition, girls' well-being could be affected if parents internalize any potential future changes in the value of girls in marriage and labor markets generated by an increase in the scarcity of women.

[44] Our indicators of malnutrition (i.e. anthropometric measures that are two standard deviations below the median of the reference population) are often thought to reflect mild cases. Indeed, when examining comparable samples (children aged 0-35 months), we find only a moderate correlation between the malnutrition indicators and mortality at the state level in each of the survey rounds. Our results are consistent with those of Hill and Upchurch (1995) who report a lack of association between female mortality disadvantage and nutritional disadvantage across several developing countries.

We explore these issues using data from the National Family Household Survey. We proxy parental access to prenatal sex selection using the ratio of male to female births in the year and state in which the child was born and provide evidence showing that starting in the 1990s high sex ratios at birth reflect the practice of prenatal sex selection. We then analyze whether changes in girls' outcomes within states and over time in comparison to boys are associated with changes in sex ratios at birth.

We find that an increase in the practice of prenatal sex selection is associated with a differential reduction in the incidence of malnutrition among surviving girls. This negative association appears to be stronger for girls born in rural households and at higher birth parities. We find *no association* between increases in prenatal sex selection and boys' outcomes suggesting that our findings for girls are unlikely to be driven by unobserved state-time-varying factors associated with the increasing practice of prenatal sex selection. Moreover, our results are highly robust to several checks that assess the likelihood of possible biases due to state-specific differential trends by gender associated with prenatal sex selection.

An exploration of the channels linking between prenatal sex selection and girls' outcomes shows no evidence for a selection of girls into families with different observable socio-demographic characteristics. However, consistent with some of the expected consequences of prenatal sex selection, we find an increase in the proportion of girls born in families with weaker son preferences. In addition, we find evidence of a larger reduction in family size for girls and a larger decline in the average number of older siblings for girls, which is explained by the reduction in the probability that girls are born at higher parities. We also find some suggestive evidence of better treatment of girls as reflected in breastfeeding duration. On the other hand, we do not find a decline in excess female child mortality or a reduction in son preference as reported by mothers in states with an increasing incidence of prenatal sex selection.

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Table 1. Summary Statistics

	Nutrition sample			Mortality sample		
	Girls	Boys	Difference	Girls	Boys	Difference
	(1)	(2)	(3)	(4)	(5)	(6)
Urban	0.236	0.237	-0.002	0.232	0.235	-0.003
			(0.003)			(0.002)
Index of mass media exposure	0.799	0.824	-0.025	0.738	0.755	-0.017
			(0.010)			(0.005)
Wealth index	2.82	2.86	-0.041	2.74	2.77	-0.030
			(0.009)			(0.009)
Mother's Age	25.2	25.3	-0.098	28.7	28.7	-0.047
			(0.024)			(0.019)
Mother's age at 1st birth	19.0	19.0	0.000	18.7	18.7	-0.023
			(0.025)			(0.013)
Mother's education						
No education	0.541	0.538	0.004	0.613	0.607	0.006
			(0.004)			(0.003)
Primary school	0.158	0.148	0.009	0.146	0.147	-0.001
			(0.003)			(0.002)
Secondary school	0.247	0.256	-0.010	0.200	0.203	-0.004
			(0.005)			(0.002)
Higher	0.053	0.057	-0.004	0.040	0.042	-0.002
			(0.002)			(0.001)
Missing	0.001	0.001	0.000	0.001	0.001	0.000
			(0.000)			(0.000)
Father's education						
No education	0.300	0.291	0.009	0.341	0.339	0.002
			(0.003)			(0.002)
Primary school	0.193	0.189	0.004	0.205	0.201	0.003
			(0.005)			(0.003)
Secondary school	0.381	0.389	-0.008	0.346	0.350	-0.004
			(0.006)			(0.002)
Higher	0.119	0.124	-0.005	0.101	0.103	-0.002
			(0.003)			(0.002)
Missing	0.006	0.007	-0.001	0.006	0.007	0.000
			(0.001)			(0.000)
Religion						
Hindu	0.792	0.792	0.001	0.791	0.794	-0.003
			(0.002)			(0.002)
Muslim	0.158	0.156	0.002	0.161	0.156	0.005
			(0.003)			(0.001)
Other religion	0.049	0.052	-0.003	0.047	0.049	-0.002
			(0.002)			(0.002)
Missing	0.001	0.001	0.000	0.001	0.001	0.000
			(0.000)			(0.000)
Number of children in the family	2.920	2.927	-0.007	3.861	3.736	0.125
			(0.009)			(0.015)
Mother wants another child	0.486	0.363	0.123	0.292	0.211	0.081
			(0.009)			(0.006)
Sample size	36,940	39,560	76,500	172,472	185,337	357,809

Notes: The table reports summary statistics for boys and girls (cols. 1,2,4, and 5) included in the analysis samples and differences between the characteristics of girls and boys (cols. 3 and 6). Standard errors of the differences clustered at the state level are reported in parenthesis. The samples pool rounds 1, 2, and 3 of the NFHS. The *nutrition sample* reported in columns 1-3 includes the last two children under three years of age of ever married women with valid anthropometric data. The *mortality sample* reported in columns 4-6 includes all children born within the last 10 years preceding the survey date of ever married women. Observations are weighted using national-level weights.

Table 2. Male Female Ratios and Fertility Preferences by State

	Male-Female Ratio (MFR) at age 0					Fertility preferences			
	1961 (1)	1971 (2)	1981 (3)	1991 (4)	2001 (5)	Number of children (6)	Ideal number of children (7)	Ideal MFR (8)	Share who wants more sons than daughters (9)
North	**103**	**103**	**105**	**111**	**117**	**2.46**	**2.75**	**1.46**	**0.49**
Delhi	104	105	105	110	117	2.36	2.52	1.25	0.30
Haryana	N/A	104	108	115	124	2.45	2.56	1.41	0.45
Himachal Pradesh	102	104	103	108	115	2.29	2.36	1.30	0.37
Jammu & Kashmir	102	103	107	N/A	114	2.58	2.77	1.48	0.49
Punjab	103	105	106	117	129	2.46	2.57	1.46	0.48
Rajasthan	103	102	104	108	112	2.49	3.02	1.55	0.58
West	**103**	**103**	**104**	**108**	**113**	**2.23**	**2.56**	**1.29**	**0.38**
Gujarat	103	103	103	109	116	2.24	2.60	1.33	0.42
Maharashtra	103	103	105	107	111	2.22	2.54	1.27	0.36
Northeast	**98**	**102**	**102**	**104**	**104**	**2.73**	**3.33**	**1.33**	**0.40**
Arunachal Pradesh	N/A	109	100	101	103	2.55	4.67	1.41	0.43
Assam	98	101	N/A	105	105	2.74	3.17	1.38	0.44
Manipur	102	94	101	102	106	2.89	3.74	1.36	0.43
Meghalaya	N/A	106	100	101	104	2.78	4.62	1.01	0.14
Mizoram	N/A	102	N/A	99	100	2.66	4.29	1.18	0.33
Nagaland	64	101	103	102	102	2.99	4.03	1.12	0.28
Tripura	99	106	106	103	105	2.43	2.57	1.28	0.33
Sikkim	95	88	101	105	106	2.32	2.23	1.13	0.22
Central	**100**	**102**	**104**	**107**	**110**	**2.47**	**3.28**	**1.52**	**0.55**
Madhya Pradesh	101	99	101	104	106	2.30	3.12	1.44	0.52
Uttar Pradesh	100	104	105	109	112	2.55	3.36	1.55	0.57
East	**99**	**100**	**103**	**106**	**106**	**2.29**	**3.03**	**1.41**	**0.45**
Bihar	101	102	104	108	107	2.38	3.40	1.56	0.56
Orissa	97	98	102	103	106	2.23	3.01	1.36	0.45
West Bengal	99	98	103	104	104	2.19	2.58	1.25	0.31
South	**100**	**99**	**102**	**104**	**105**	**2.08**	**2.48**	**1.17**	**0.23**
Andhra Pradesh	99	98	101	103	104	1.99	2.75	1.25	0.33
Goa	105	105	105	104	106	2.34	2.69	1.20	0.28
Karnataka	101	101	102	104	106	2.30	2.53	1.20	0.27
Kerala	101	99	102	104	103	2.07	2.62	1.12	0.18
Tamil Nadu	99	99	101	103	105	2.00	2.08	1.07	0.11

Notes: Columns 1-5 report male-female ratios (MFR) at age zero by state for various census years. Columns 6-9 report indicators for fertility, desired fertility, and son preferences based on mothers' reports in the first round of the NFHS. Tabulations for Sikkim are based on the second round of the NFHS as Sikkim was not sampled in the first round. Summary statistics reported in columns 6-9 are computed using state-level weights. N/A denotes data not available.

Table 3. Differential Probability of a Male Birth at Parity N
as a Function of the Sex Composition of Older Siblings

	Born between 1975-1989				Born after 1989			
	Full Sample		Rural	Urban	Full Sample		Rural	Urban
	No controls (1)	Full controls (2)	Full controls (3)	Full controls (4)	No controls (5)	Full controls (6)	Full controls (7)	Full controls (8)
A. Parity 2 (omitted category=Boy)								
Girl	-0.004	-0.004	-0.005	0.000	0.020	0.020	0.016	0.031
	(0.003)	(0.003)	(0.004)	(0.013)	(0.007)	(0.007)	(0.008)	(0.010)
Sample Size	*50,175*	*50,175*	*34,624*	*15,551*	*80,424*	*80,424*	*51,350*	*29,074*
B. Parity 3 (omitted category=Boy-Boy)								
Girl-Girl	-0.006	-0.007	-0.009	0.000	0.032	0.030	0.018	0.067
	(0.010)	(0.009)	(0.008)	(0.024)	(0.010)	(0.010)	(0.008)	(0.014)
Girl-Boy	0.009	0.009	0.007	0.016	0.019	0.019	0.021	0.016
	(0.007)	(0.007)	(0.010)	(0.019)	(0.009)	(0.009)	(0.009)	(0.014)
Boy-Girl	-0.004	-0.004	-0.008	0.009	0.005	0.005	-0.003	0.028
	(0.010)	(0.010)	(0.007)	(0.030)	(0.008)	(0.008)	(0.010)	(0.012)
Sample Size	*39,042*	*39,042*	*27,963*	*11,079*	*55,289*	*55,289*	*34,849*	*15,895*

Notes: The table reports the differential probability of a male birth at parity 2 (panel A) and parity 3 (panel B) as a function of the sex composition of older siblings. The samples include all children born in the 15 years prior to each survey date. Estimates reported in columns 1-4 are for children born between 1975 and 1989. Estimates reported in columns 5-8 are for children born in 1990 or afterwards. Columns 1, 2, 5, and 6 report estimates for the full sample. Columns 3, 4, 7, and 8 report estimates from samples stratified by rural/urban residency. Regression estimates reported in columns 2-4 and 6-8 are from models that control also for twin status, mother's age, mother's education, mother's age at first birth, indicators for mother's religion, father's education, mother's mass media exposure, wealth, and rural/urban status. Observations are weighted using national-level weights. Standard errors clustered at the state level are reported in parenthesis. Sample sizes are reported in *italics* .

Table 4. Sex-Ratios and Ultrasound Use

	Ultrasound Test				Pregnancy outcome = boy conditional on doing ultrasound test			
	Full Sample		Rural	Urban	Full Sample		Rural	Urban
	No controls (1)	Full controls (2)	Full controls (3)	Full controls (4)	No controls (5)	Full controls (6)	Full controls (7)	Full controls (8)
A. Parity 2 (omitted category=1 son)								
No sons	0.022	0.020	0.023	0.016	0.071	0.070	0.064	0.081
	(0.007)	(0.006)	(0.005)	(0.012)	(0.016)	(0.016)	(0.020)	(0.022)
Sample Size	*20,265*	*20,265*	*12,104*	*8,161*	*6,225*	*6,225*	*2,323*	*3,902*
B. Parity 3 (omitted category=2 sons)								
No sons	0.102	0.087	0.075	0.128	0.103	0.101	0.161	0.063
	(0.022)	(0.021)	(0.022)	(0.024)	(0.028)	(0.029)	(0.056)	(0.040)
1 son	0.027	0.021	0.010	0.059	-0.007	-0.008	0.087	-0.083
	(0.011)	(0.012)	(0.014)	(0.028)	(0.040)	(0.043)	(0.076)	(0.040)
Sample Size	*11,777*	*11,777*	*7,822*	*3,955*	*2,398*	*2,398*	*991*	*1,407*

Notes: Columns 1 through 4 report the differential likelihood that a mother performs an ultrasound test during pregnancy as a function of the sex composition of her older children. Columns 5 through 8 report the differential likelihood of a male birth as a function of the sex composition of the older children among mothers who performed an ultrasound test during that pregnancy. Panels A and B report estimates for parities 2 and respectively. Columns 1,2,5, and 6 report estimates for the full sample of mothers. Columns 3, 4, 7, and 8 report estimates for samples stratified by rural/urban residency. Regression estimates reported in columns 2-4 and 6-8 are from models that control also for twin status, mother's age, mother's education, mother's age at first birth, indicators for mother's religion, father's education, mother's mass media exposure, wealth, and rural/urban status. Observations are weighted using national-level weights. Standard errors clustered at the state level are reported in parenthesis. Sample sizes are reported in *italics* .

Table 5. Effects on Nutritional Status of Children

			Basic specification		Full controls	
Outcome	Sample size (1)	Outcome mean (2)	MFR (3)	Female *x* MFR (4)	MFR (5)	Female *x* MFR (6)
Underweight	76,314	0.485	-0.162	-0.535	-0.095	-0.536
			(0.336)	(0.160)	(0.318)	(0.132)
Wasted	69,784	0.179	-0.050	-0.247	-0.029	-0.250
			(0.284)	(0.109)	(0.275)	(0.104)
Stunted	69,571	0.433	0.316	-0.287	0.386	-0.293
			(0.230)	(0.180)	(0.236)	(0.164)

Notes: Means of the dependent variables are reported in Column 2. Columns 3 and 4 report regression estimates for MFR and MFR interacted with a female dummy from a linear probability model that controls for state and year of birth fixed effects and their interactions with gender. Columns 5 and 6 report regression estimates from a model that controls also for twin status, mother's age, mother's education, mother's age at first birth, indicators for mother's religion, father's education, mother's mass media exposure, wealth, and rural/urban status. The sample includes children aged 0 to 35 months born in the last two births of ever married women sampled in rounds 1-3 of the NFHS surveys. Sample sizes are smaller for wasted and stunted because height measures were not taken in round 1 for the following 5 states: Andhra Pradesh, Himachal Pradesh, Madhya Pradesh, Tamil Nadu and West Bengal. Observations are weighted using national-level weights. Standard errors clustered at the state level are reported in parenthesis.

Table 6. Effects on Nutritional Status of Children by Place of Residence and Parity

Outcome	Sample size (1)	Outcome mean (2)	MFR (3)	Female *x* MFR (4)
		A. Rural		
Underweight	53,158	0.513	-0.123	-0.547
			(0.299)	(0.182)
Wasted	48,191	0.185	0.046	-0.301
			(0.289)	(0.126)
Stunted	48,037	0.458	0.345	-0.360
			(0.252)	(0.193)
		B. Urban		
Underweight	23,156	0.395	0.055	-0.407
			(0.409)	(0.185)
Wasted	21,593	0.158	-0.212	-0.147
			(0.290)	(0.222)
Stunted	21,534	0.351	0.335	0.312
			(0.235)	(0.274)
		C. Parity 1		
Underweight	22,820	0.428	0.027	-0.565
			(0.280)	(0.186)
Wasted	20,892	0.160	0.231	-0.137
			(0.237)	(0.203)
Stunted	20,868	0.379	0.508	-0.285
			(0.241)	(0.196)
		D. Parity>1		
Underweight	53,494	0.508	-0.137	-0.542
			(0.334)	(0.144)
Wasted	48,892	0.186	-0.111	-0.317
			(0.304)	(0.178)
Stunted	48,703	0.454	0.345	-0.325
			(0.245)	(0.199)

Notes: Means of the dependent variables are reported in column 2. Columns 3 and 4 report regression estimates for MFR and MFR interacted with female for samples stratified by place of residence (Panels A and B) and parity (Panels C and D). Regression estimates come from a model that includes state and year of birth fixed effects and their interactions with female and controls also for the covariates specified in table 5. The sample includes children aged 0 to 35 months born in the last two births of ever married women sampled in rounds 1-3 of the NFHS surveys. Observations are weighted using national-level weights. Standard errors clustered at the state level are reported in parenthesis.

Table 7. Family Characteristics of Girls vs. Boys

Outcome	Sample size (1)	Outcome mean (2)	Females -Males (3)	MFR (4)	Female *x* MFR (5)
Mother's age	76,500	25.3	-0.098	2.952	-1.817
			(0.024)	(2.268)	(1.703)
Mother's age at 1st birth	76,500	19.0	0.000	4.863	0.785
			(0.025)	(1.704)	(0.873)
Mother's education	76,394	3.62	-0.118	6.489	0.243
			(0.042)	(1.769)	(1.419)
Father's education	76,032	6.15	-0.137	4.538	0.682
			(0.051)	(1.215)	(2.275)
Wealth index	76,500	2.84	-0.041	-0.268	-0.039
			(0.009)	(0.623)	(0.522)
Rural	76,500	0.763	0.002	-0.300	0.158
			(0.003)	(0.121)	(0.159)
Number of children	76,500	2.92	-0.007	-1.421	-0.923
			(0.009)	(0.672)	(0.534)
Number of older siblings	76,500	1.82	-0.026	-1.172	-0.980
			(0.009)	(0.593)	(0.557)

Notes: Column 2 reports means of the dependent variables and column 3 reports the female-male differential. Columns 4 and 5 report regression estimates for MFR and MFR interacted with a female dummy from a model that includes gender specific state and year of birth fixed effects. The sample includes children aged 0 to 35 months born in the last two births of ever married women sampled in rounds 1-3 of the NFHS surveys. Observations are weighted using national-level weights. Standard errors clustered at the state level are reported in parenthesis.

Table 8. MFR and Son Preference

Outcome	Sample	Sample size (1)	Outcome mean (2)	Females -Males (3)	MFR (4)	Female *x* MFR (5)
Ideal MFR	Full	86,521	1.324	-0.070	0.008	-0.202
				(0.008)	(0.191)	(0.128)
Ideal MFR	Ideal MFR ≤ 4	86,412	1.318	-0.066	-0.042	-0.214
				(0.007)	(0.184)	(0.111)
Wants more sons than daughters	Full	88,405	0.351	-0.063	0.078	-0.181
				(0.005)	(0.166)	(0.113)
Wants more sons than daughters	Ideal MFR ≤ 4	88,298	0.350	-0.062	0.071	-0.181
				(0.005)	(0.166)	(0.112)

Notes: The first and second rows reports regression estimates where the dependent variable is defined as the ratio of the ideal number of sons to the ideal number of daughters reported by mothers (ideal MFR). The third and fourth row reports regression estimates where the dependent variable is an indicator that equals one if the mother reported a larger ideal number of sons relative to her ideal number of daughters. Means of the dependent variables are reported in Column 2. Column 3 reports differences in means of the dependent variables between females and males. Columns 4 and 5 report regression estimates from a model that includes state and year of birth fixed effects and their interactions with female and controls also for twin status, mother's age, mother's education, mother's age at first birth, indicators for mother's religion, father's education, mother's mass media exposure, wealth, and rural/urban status. The sample includes children aged 0 to 35 months born in the last two births of ever married women sampled in rounds 1-3 of the NFHS surveys. Observations are weighted using national-level weights. Standard errors clustered at the state level are reported in parenthesis.

Table 9. Breastfeeding Duration

Outcome Breastfed for:	Sample size (1)	Outcome mean (2)	Females -Males (3)	MFR (4)	Female *x* MFR (5)
		A. Full Sample			
At least 12 months	59,670	0.881	-0.011	0.093	-0.058
(age>=12 months)			(0.004)	(0.132)	(0.133)
At least 18 months	43,295	0.707	-0.038	0.128	-0.206
(age>=18 months)			(0.007)	(0.190)	(0.230)
At least 24 months	29,537	0.544	-0.056	0.037	0.391
(age>=24 months)			(0.007)	(0.186)	(0.155)
		B. Rural			
At least 12 months	41,183	0.906	-0.013	0.001	-0.033
(age>=12 months)			(0.004)	(0.129)	(0.115)
At least 18 months	29,624	0.744	-0.045	0.109	-0.276
(age>=18 months)			(0.006)	(0.160)	(0.208)
At least 24 months	20,265	0.583	-0.069	-0.068	0.507
(age>=24 months)			(0.008)	(0.228)	(0.228)
		C. Urban			
At least 12 months	18,487	0.805	-0.009	0.129	-0.040
(age>=12 months)			(0.008)	(0.294)	(0.289)
At least 18 months	13,671	0.594	-0.020	0.054	0.005
(age>=18 months)			(0.013)	(0.382)	(0.394)
At least 24 months	9,272	0.421	-0.018	0.373	-0.358
(age>=24 months)			(0.010)	(0.402)	(0.542)
		D. Parity 1			
At least 12 months	17,789	0.829	-0.007	0.217	0.036
(age>=12 months)			(0.008)	(0.182)	(0.148)
At least 18 months	12,913	0.618	-0.015	0.159	-0.001
(age>=18 months)			(0.008)	(0.208)	(0.463)
At least 24 months	8,650	0.438	-0.017	0.370	0.162
(age>=24 months)			(0.013)	(0.237)	(0.267)
		E. Parity>1			
At least 12 months	41,881	0.903	-0.013	0.039	-0.126
(age>=12 months)			(0.006)	(0.167)	(0.176)
At least 18 months	30,382	0.743	-0.047	0.127	-0.295
(age>=18 months)			(0.010)		
At least 24 months	20,887	0.585	-0.070	-0.056	0.492
(age>=24 months)			(0.009)	(0.208)	(0.232)

Notes: Means of the dependent variables are reported in Column 2. Column 3 reports differences in means of the dependent variables between females and males. Columns 4 and 5 report regression estimates for MFR and MFR interacted with female from a linear probability model that controls for state and year of birth fixed effects and their interactions with female and the set of covariates specified in Table 5. In addition, the models control for age in months and age in months interacted with a female dummy. Panel A reports estimates for the full sample. Panels B and C report estimates for samples stratified by rural/urban residence. Panels D and E report estimates for samples stratified by parity. All samples include children born within the last 35 months prior to survey date. The minimum age restriction used in each of the regressions is specified in the first column of the table. Observations are weighted using national-level weights. Standard errors clustered at the state level are reported in parenthesis.

Table 10. Effects on Mortality

Outcome	Sample (1)	Children Born within 36 months preceding survey: Sample size (2)	Outcome mean (3)	Females -Males (4)	MFR (5)	Female *x* MFR (6)	Children Born within 120 months preceding survey: Sample size (7)	Outcome mean (8)	Females -Males (9)	MFR (10)	Female *x* MFR (11)
Neonatal mortality	Age ≥	98,922	0.042	-0.007	0.010	-0.024	356,361	0.048	-0.007	0.001	-0.019
(death between 0-29 days)	1 month			(0.002)	(0.031)	(0.047)			(0.001)	(0.031)	(0.034)
Post-neonatal mortality	Age ≥	63,961	0.023	0.002	-0.004	-0.049	309,689	0.028	0.003	0.000	0.006
(Death between 1 month-12 months)	12 months			(0.001)	(0.033)	(0.061)			(0.001)	(0.030)	(0.028)
Child Mortality	Age ≥						174,978	0.029	0.010	0.067	0.023
(Death between age 1 and before age 5)	60 months								(0.003)	(0.034)	(0.040)
Under 5 mortality	Age ≥						188,924	0.108	0.004	0.062	0.031
(Death before fifth birthday)	60 months								(0.005)	(0.080)	(0.067)

Notes: The sample for columns 2-6 includes all children aged 0 through 35 months born in the last two births to ever married women sampled in rounds 1-3 of the NFHS surveys. The sample for columns 7-11 includes all children born in the last ten years prior to survey date to ever married women sampled in rounds 1-3 of the NFHS surveys. The minimum age restriction used for each outcome is specified in column 1. Columns 3 and 8 report means of the dependent variables and columns 4 and 9 report the female-male differential in the dependent variables. Columns 5 and 6 and columns 10 and 11 report regression estimates for MFR and MFR interacted with a female dummy from a linear probability model that includes gender specific state and year of birth fixed effects and control also for the covariates specified in table 5. Observations are weighted using national-level weights. Standard errors clustered at the state level are reported in parenthesis.

Figure 1: Male to Female Ratio at Birth

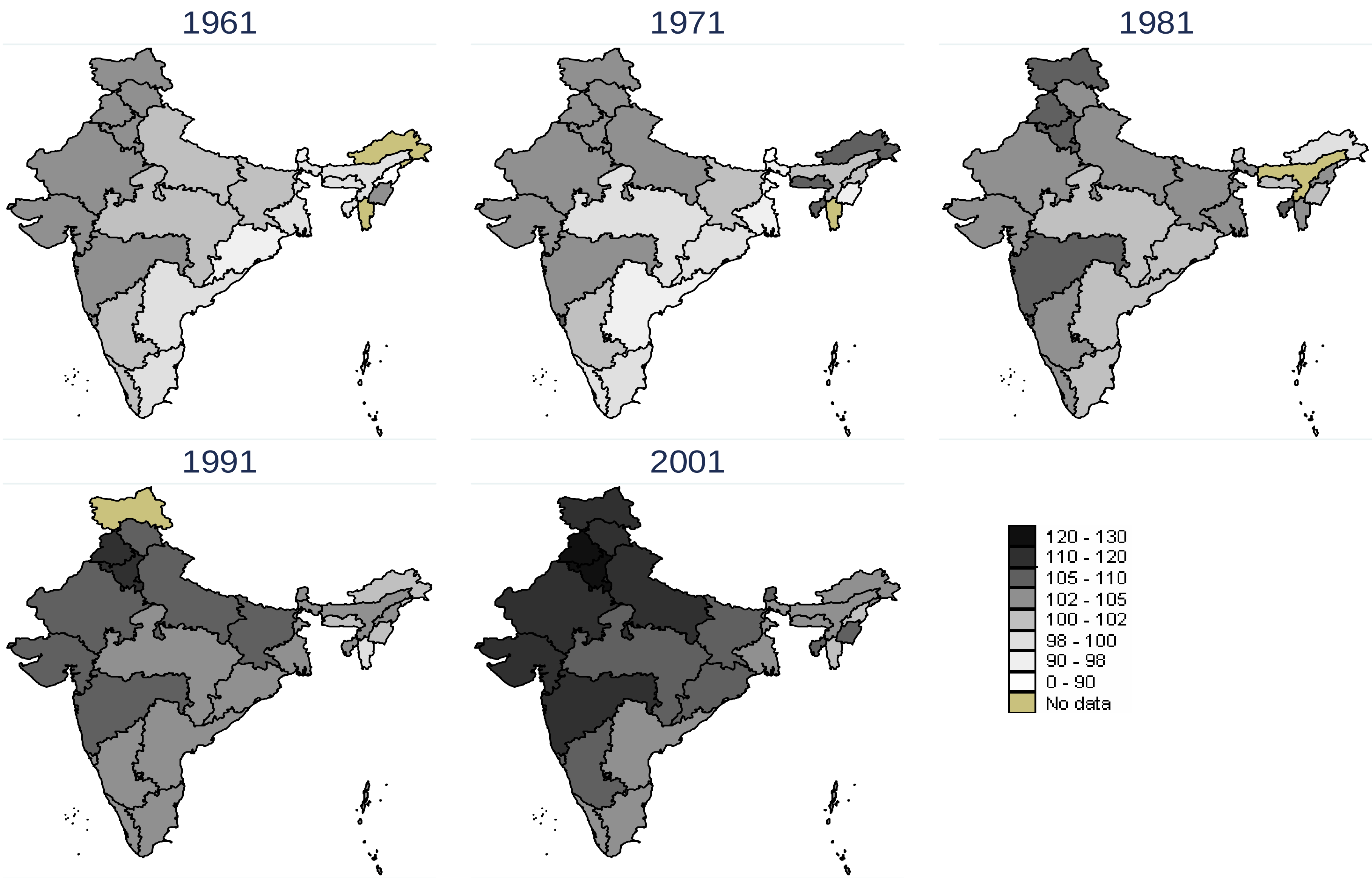


Figure 2a. Male to Female Ratio at Age 0 for the Urban Population

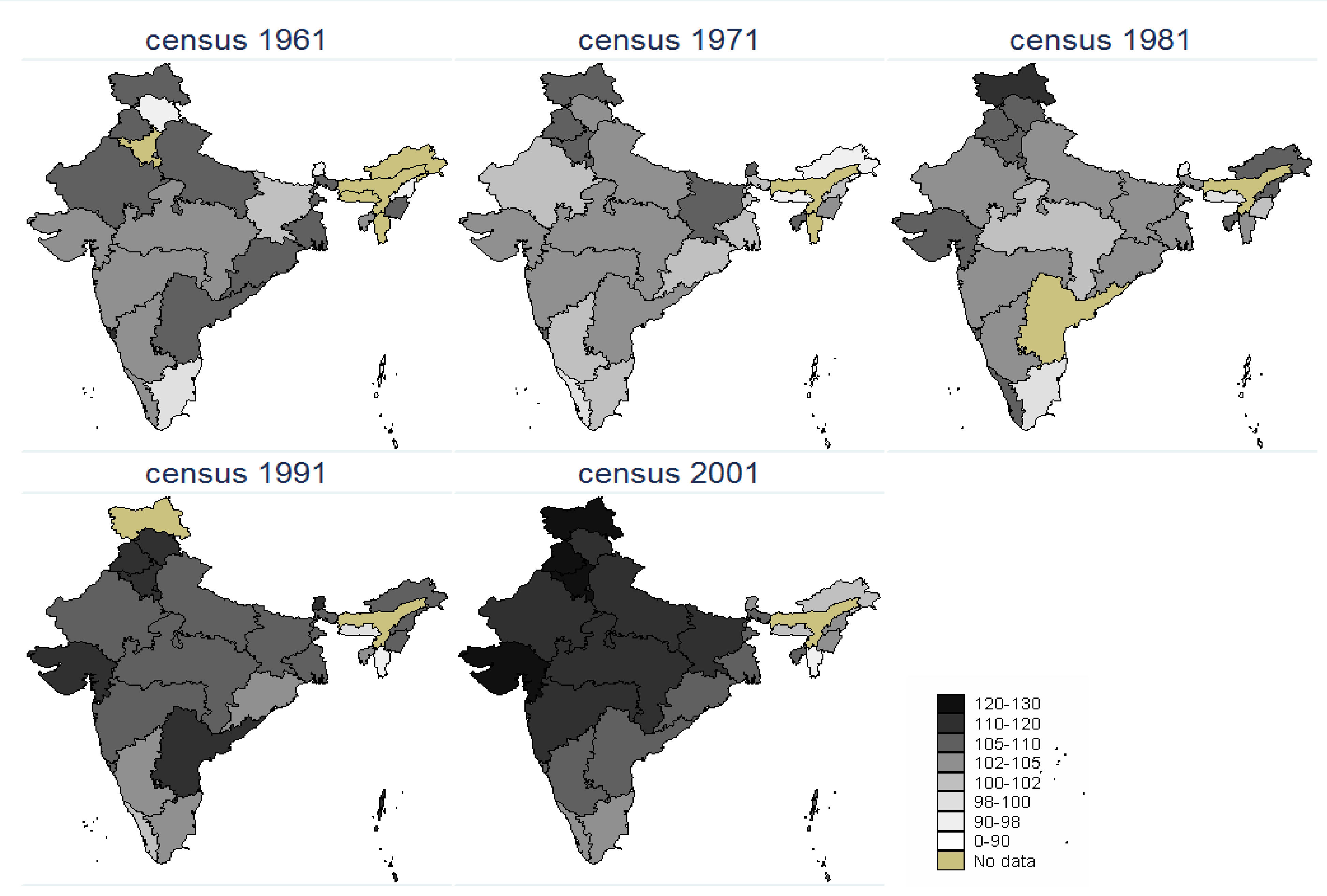


Figure 2b. Male to Female Ratio at Age 0 for the Rural Population

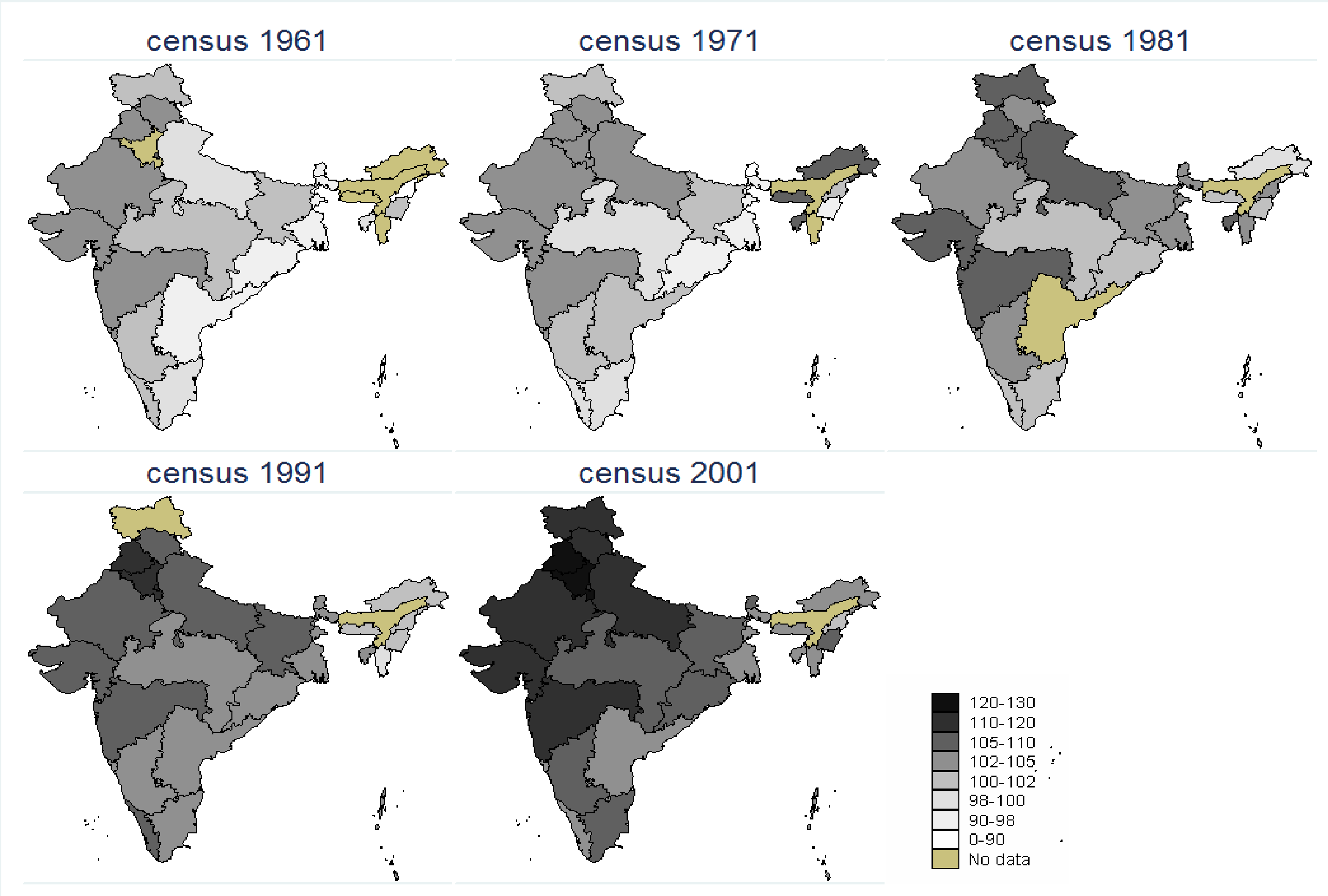
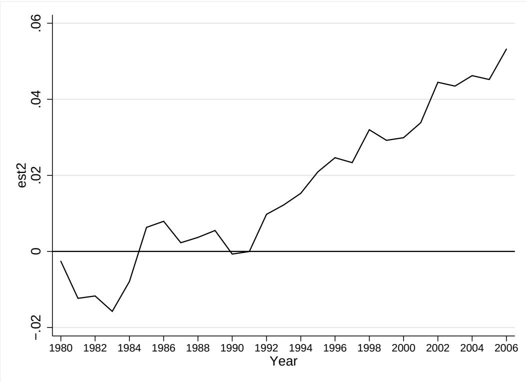


Figure 3: Differential Probability of a Male Birth by Year of Birth



(a) Parity 2

(b) Parity 3

Notes: The figure plots estimates for the differential probability of a male birth by year of birth using a 5-year moving average and comparing between households with no boys among previous children and households that have already one boy (panel a for parity two) or two boys (panel b for parity three).

Appendix for online publication

Discrete version of triple-differences estimation

We stratify states in two groups: treated states (states in the northern and western regions) which exhibit stronger son preference and an increasing trend in the practice of prenatal sex selection and comparison states (which include all other states). Specifically, we identify the following eight states as treated: Gujarat, Haryana, Himachal Pradesh, Maharashtra, Punjab, Rajasthan, Jammu and Kashmir, and Delhi. This list coincides with the classification of Bhat (2002) and adds the states of Jammu and Kashmir and Rajasthan to the classification proposed by Retherford and Roy (2003).

Figure A1 plots the Male-Female ratio at birth by survey round in treated and comparison states for the cohorts of children born in the last 3 years prior to each survey date. The figure shows that MFR in treated states is higher than in comparison states. Moreover, while MFR increases sharply in treated states over the three survey rounds, MFR in comparison states appears to remain relatively stable.[1]

We then analyze the differential change in girls' and boys' outcomes in northern and western states relative to other states over the three survey rounds by estimating the following equation:

$$(2)\ y_{is\tau} = \alpha_{s0} + \alpha_{s1}\, female_i + \delta_{\tau 0} + \delta_{\tau 1}\, female_i + x_i'\beta + \gamma_{\tau 0} Treated_s + \gamma_{\tau 1}(Treated_s * female_i) + \varepsilon_{is\tau}$$

where $y_{is\tau}$ is the outcome of child *i* in state *s* and in survey round τ, α_{s0} and α_{s1} are vectors of gender-specific state fixed effects, $\delta_{\tau 0}$ and $\delta_{\tau 1}$ are vectors of gender-specific survey-round fixed effects, x_i is a vector of individual characteristics that includes the same covariates as in equation (1) and "Treated" is an indicator that equals 1 if child *i* was born in a state with a high incidence of prenatal sex selection and 0 otherwise. The parameters of interest are $\gamma_{\tau 1}\ (\tau = 1998; 2005)$ which denote the differential change in

[1] We do not claim that prenatal sex selection is not practiced at all in other states but rather that its effects are expected to be smaller relative to the treated group.

girls' outcomes between 1992 (the year of the first round of the NFHS) and 1998 or 2005 (the years of the second and third rounds of the NFHS survey, respectively) in states with an upward trend in prenatal sex selection relative to states in which prenatal sex selection is rare and has not increased over time.

Results from this specification are reported in **Appendix Table A2** and are highly in line with our main results. The coefficients on the triple interaction terms, *female*round2*treated* and *female*round3*treated*, are negative for the three nutritional status indicators and their magnitude is larger (in absolute terms) in the third round relative to the second round (see columns 5 and 7), which is consistent with the upward trend in MFR over the three survey rounds reported in **Figure A1**. Moreover, the coefficients for the two interaction terms, round2*treated and round3*treated, are small, not significant and have inconsistent signs over the survey rounds and outcomes, thus suggesting that there were no major changes over time in the nutritional status of boys in treated states relative to other states. These findings suggest that girls' nutritional status improved more in states with an increasing trend in the use of prenatal sex selection.

Table A1. State Characteristics

	1990 NDP (Rs. Crore - 10 m. Rs)	1990 NDP PC (Rs.)	Household characteristics			Religion			Mother's characteristics		
			Urban	Wealth Index	HH with electricity	Hindu	Muslim	Other	Avg. years of schooling	TV exposure	Illiterate
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
North			**0.29**	**3.55**	**0.75**	**0.79**	**0.05**	**0.16**	**2.84**	**0.40**	**0.66**
Delhi	10,243	11,057	0.92	4.79	0.96	0.82	0.10	0.08	6.35	0.83	0.37
Haryana	12,238	7,508	0.26	3.90	0.86	0.89	0.04	0.07	3.01	0.49	0.64
Himachal Pradesh	2,521	4,910	0.10	3.62	0.92	0.97	0.01	0.02	3.62	0.47	0.50
Jammu & Kashmir	2,908	3,816	0.18	3.74	0.88	0.77	0.17	0.06	3.91	0.50	0.57
Punjab	16,738	8,318	0.28	4.26	0.94	0.38	0.01	0.61	3.88	0.57	0.53
Rajasthan	18,281	4,191	0.20	2.79	0.54	0.92	0.06	0.02	1.36	0.18	0.82
West			**0.39**	**3.56**	**0.77**	**0.81**	**0.11**	**0.08**	**3.85**	**0.44**	**0.52**
Gujarat	24,180	5,891	0.35	3.60	0.78	0.89	0.09	0.02	3.61	0.39	0.55
Maharashtra	58,137	7,439	0.42	3.54	0.76	0.76	0.13	0.11	3.97	0.47	0.50
Northeast			**0.16**	**2.69**	**0.31**	**0.61**	**0.21**	**0.18**	**3.13**	**0.22**	**0.55**
Arunachal Pradesh	460	5,398	0.15	3.17	0.62	0.35	0.01	0.64	2.25	0.29	0.70
Assam	9,498	4,281	0.12	2.44	0.20	0.67	0.28	0.04	2.80	0.18	0.59
Manipur	723	3,976	0.32	3.55	0.64	0.62	0.06	0.31	4.44	0.38	0.48
Meghalaya	767	4,375	0.19	3.10	0.43	0.09	0.02	0.89	3.26	0.24	0.51
Mizoram	306	4,474	0.49	3.82	0.76	0.02	0.00	0.98	5.69	0.25	0.08
Nagaland	579	4,990	0.21	3.64	0.78	0.05	0.01	0.94	4.11	0.23	0.43
Tripura	917	3,370	0.20	2.96	0.47	0.87	0.08	0.05	4.01	0.34	0.42
Sikkim	213	5,302	0.14	3.73	0.80	0.60	0.01	0.38	3.72	0.56	0.49
Central			**0.21**	**2.69**	**0.44**	**0.86**	**0.12**	**0.01**	**2.01**	**0.21**	**0.75**
Madhya Pradesh	26,515	4,049	0.22	2.85	0.65	0.93	0.05	0.02	1.98	0.27	0.74
Uttar Pradesh	49,496	3,590	0.20	2.62	0.34	0.83	0.16	0.01	2.03	0.19	0.76
East			**0.19**	**2.46**	**0.24**	**0.83**	**0.15**	**0.02**	**2.40**	**0.21**	**0.66**
Bihar	22,787	2,660	0.15	2.32	0.17	0.82	0.16	0.02	1.78	0.13	0.78
Orissa	9,664	3,077	0.15	2.42	0.29	0.97	0.01	0.02	2.16	0.16	0.67
West Bengal	31,500	4,673	0.27	2.67	0.30	0.76	0.22	0.02	3.30	0.33	0.51
South			**0.31**	**3.39**	**0.65**	**0.82**	**0.11**	**0.07**	**3.72**	**0.43**	**0.54**
Andhra Pradesh	29,867	4,531	0.26	3.20	0.65	0.88	0.08	0.04	2.48	0.39	0.69
Goa	1,024	8,797	0.50	4.32	0.92	0.67	0.05	0.27	5.38	0.71	0.34
Karnataka	20,551	4,598	0.33	3.27	0.66	0.86	0.11	0.03	3.13	0.40	0.61
Kerala	12,173	4,200	0.28	3.89	0.61	0.54	0.26	0.19	6.76	0.42	0.16
Tamil Nadu	27,674	4,983	0.35	3.42	0.66	0.88	0.06	0.06	4.07	0.50	0.50

Notes: The table reports selected economic and demographic characteristics by state. Data on net domestic product (NDP) and net domestic product per capita (NDP PC) reported in columns 1 and 2 are based on reports of the Reserve Bank of India. Data on NDP are reported in Crore (10 million Rupees). Data on NDP PC are reported in Rupees. Means reported in columns 3-11 are based on tabulations from the first round of the NFHS. Means for Sikkim are based on the second round of the NFHS. Summary statistics reported in columns 3-11 are computed using state-level weights.

Table A2. Effects on Nutritional Status of Children by Region and Survey Round

Outcome	Sample size (1)	Outcome mean (2)	round 2 x treated (3)	female x round 2 x treated (4)	round 3 x treated (5)	female x round 3 x treated (6)
Underweight	76,314	0.485	0.034	-0.043	0.000	-0.064
			(0.040)	(0.019)	(0.040)	(0.025)
Wasted	69,784	0.179	0.004	-0.022	-0.009	-0.056
			(0.023)	(0.017)	(0.033)	(0.016)
Stunted	69,571	0.433	0.037	-0.018	0.032	-0.034
			(0.032)	(0.020)	(0.040)	(0.024)

Notes: Column 2 reports means of the dependent variables. Columns 3-6 report estimates from a triple-differences model that compares changes in nutritional outcomes of girls versus boys in treated versus comparison states over the second and third survey round relative to the first survey round. The treated group includes the following states: Gujarat, Haryana, Himachal Pradesh, Maharashtra, Punjab, Jammu and Kashmir, Rajasthan, and Delhi. The models control for state fixed effects and survey round indicators interacted with gender. In addition the model controls for twin status, mother's age, mother's age at first birth, and indicators for mother's religion, mother's education, father's education, mother's mass media exposure index, wealth index, and rural/urban status. Observations are weighted using national-level weights. Standard errors clustered at the state level are reported in parenthesis.

Table A3. Robustness Checks

		Underweight		Wasted		Stunted	
		MFR (1)	Female x MFR (2)	MFR (3)	Female x MFR (4)	MFR (5)	Female x MFR (6)
1	Main Results	-0.095	-0.536	-0.029	-0.250	0.386	-0.293
		(0.318)	(0.132)	(0.275)	(0.104)	(0.236)	(0.164)
2	State x Year of birth FE	---	-0.560	---	-0.267	---	-0.304
			(0.134)		(0.101)		(0.165)
3	Household covariates interacted with gender	-0.107	-0.514	-0.036	-0.236	0.379	-0.276
		(0.319)	(0.126)	(0.278)	(0.105)	(0.239)	(0.164)
4	State varying controls 1 + HH covariates interacted with gender	-0.091	-0.433	0.140	-0.135	0.415	-0.217
		(0.254)	(0.115)	(0.181)	(0.114)	(0.175)	(0.112)
5	State varying controls 2 + HH covariates interacted with gender	-0.303	-0.384	-0.009	-0.227	0.168	-0.206
		(0.289)	(0.092)	(0.240)	(0.110)	(0.170)	(0.127)
6	Birth weight interacted with gender	-0.055	-0.549	-0.028	-0.250	0.415	-0.303
		(0.327)	(0.149)	(0.270)	(0.124)	(0.269)	(0.175)
7	Size at birth interacted with gender	-0.085	-0.525	-0.031	-0.235	0.383	-0.276
		(0.325)	(0.129)	(0.282)	(0.100)	(0.237)	(0.150)
8	Delivery at health facility interacted with gender	-0.092	-0.518	-0.029	-0.243	0.395	-0.288
		(0.318)	(0.132)	(0.274)	(0.105)	(0.235)	(0.166)
9	New z-scores	-0.019	-0.554	0.060	-0.211	0.260	-0.216
		(0.258)	(0.139)	(0.250)	(0.093)	(0.203)	(0.119)
10	Log MFR	-0.099	-0.609	-0.029	-0.295	0.463	-0.330
		(0.344)	(0.147)	(0.302)	(0.105)	(0.254)	(0.188)
11	5-year span for MFR	-0.103	-0.426	-0.015	-0.184	0.175	-0.222
		(0.269)	(0.131)	(0.199)	(0.077)	(0.205)	(0.161)
12	9-year span for MFR	0.130	-0.591	0.080	-0.316	0.540	-0.236
		(0.386)	(0.159)	(0.295)	(0.138)	(0.308)	(0.193)
13	Unweighted regression	0.011	-0.474	0.151	-0.290	0.186	-0.245
		(0.235)	(0.112)	(0.203)	(0.103)	(0.137)	(0.101)
14	State weights	0.011	-0.471	0.136	-0.286	0.201	-0.267
		(0.240)	(0.111)	(0.204)	(0.095)	(0.142)	(0.096)

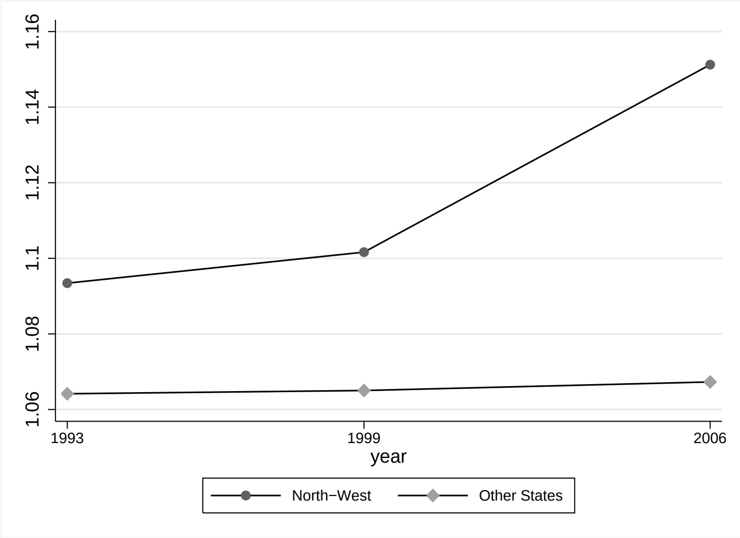
Notes: The table reports estimates from various robustness checks. See section VI of the paper for a detailed explanation of each test.

Table A4. Son Preference and MFR

	MFR of cohorts born within 3 years prior to survey date		MFR of cohorts born between 5-10 years prior to survey date	
	Ever married age 15-20 (1)	Ever married age 15-20 with no children (2)	Ever married age 15-20 (3)	Ever married age 15-20 with no children (4)
Ideal MFR	0.084	0.142	-0.234	-0.139
	(0.237)	(0.238)	(0.169)	(0.180)
	1.286	*1.266*	*1.286*	*1.266*
Wants more sons than daughters	0.157	0.125	-0.106	0.220
	(0.202)	(0.160)	(0.269)	(0.264)
	0.326	*0.303*	*0.326*	*0.303*
Number of women	29,734	13,789	29,734	13,789

Notes: The table reports regression estimates for state average MFR of cohorts born within three years prior to the survey date (columns 1 and 2) and state average MFR of cohorts born between 5 and 10 years prior to survey date (columns 3 and 4). The first row reports regression estimates where the dependent variable is defined as the ratio of the ideal number of sons to the ideal number of daughters reported by mothers (ideal MFR). The second row reports regression estimates where the dependent variable is an indicator that equals one if the mother reported a larger ideal number of sons relative to her ideal number of daughters. All estimates come from models that control for state fixed-effects and indicators for mother's year of birth, mother's age, mother's education, mother's age at first birth, mother's religion, father's education, mother's mass media exposure, wealth, and rural/urban status. The sample in columns 1 and 3 includes all ever married women sampled in rounds 1-3 of the NFHS surveys. The sample in columns 2 and 4 includes all ever married women with no children sampled in rounds 1-3 of the NFHS surveys. Means of the dependent variables are reported in italics. Observations are weighted using national-level weights. Standard errors clustered at the state level are reported in parenthesis.

Figure A1. Male-Female Ratio at Birth by Survey Round - Children aged 0-35 Months



Notes: The figure plots average Male-Female ratio at birth for the cohorts of children born within 3 years prior to each survey round.