DISCRIMINATION BEGINS IN THE WOMB: EVIDENCE OF SEX-SELECTIVE PRENATAL INVESTMENTS

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ABSTRACT. This paper investigates whether boys receive preferential prenatal treatment in a setting where son preference is present. Using micro health data from India, we highlight sex-selective prenatal investments as a new channel via which parents can practice discriminatory behavior. We find that mothers visit antenatal clinics and receive tetanus shots more frequently when pregnant with a boy. Preferential prenatal treatment of males is greater in regions known to have strong son preference and among women whose previous children are female. We successfully rule out other mechanisms such as selective recall, medical complications that might cause male babies to receive greater prenatal care in general, son preference-based fertility stopping rules and reverse causality due to sex selective abortions. Our calculations suggest that sex-selective prenatal care in tetanus use explains between 4-10.5% of excess female neonatal mortality in India. We find similar results using data from other countries like China, Bangladesh and Pakistan; thus, we show the extent of sex selective prenatal care in large parts of South and Southeast Asia.
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1. INTRODUCTION

Sex-based discrimination has been studied extensively in the context of son preference in South and Southeast Asia (Dreze and Sen 1989, Gupta 1987, Qian 2008). Differential care given to boys over girls and sex-selective abortions has resulted in an estimated 30 to 70 million "missing" women in India and China alone. While one might expect economic growth to erode such discrimination, son preference (as evidenced by skewed sex ratios) has been persistent despite high growth rates in these countries (Gupta et al. 2003). As a result, a large literature has tried to explain the observed skewed ratios through post-birth discrimination strategies. Some of the channels examined are (but not limited to) differential vaccination rates (Oster 2009), allocation of household resources (Pitt and Rosenzweig 1990), breastfeeding behavior (Jayachandran and Kuziemko 2009) and parental time allocation (Barcellos, Carvalho, and Lleras-Muney 2010). The papers that do examine sex-based discrimination before birth focus on sex-selective abortions (Portner 2009, Meng 2010, Bhalotra and Cochrane 2010). However, an unanswered question in this literature is whether parents invest less in prenatal care when pregnant with a girl, while still carrying the girl to term.[1] Such discrimination can have sizeable consequences as prenatal care is an essential component of the overall health of the child.

Maternal inputs during pregnancy can affect important outcomes such as neonatal survival and birth weight (Gortmaker 1979, Bharadwaj and Eberhard 2010). In India, attending prenatal care is correlated with a 27% decrease in the probability of neonatal mortality (NFHS). Tetanus shots taken during pregnancy play a particularly important role in neonatal survival;[2] about 38% of child deaths (under 5 years) occur in the neonatal stage. Neonatal tetanus is the leading cause of neonatal deaths in India (Zupan and Aahman 2005, Gupta and Keyl 1998) and results in nearly 200,000 neonatal deaths per year in South and Southeast Asia (UNICEF 2000). Early childhood health not withstanding, we also know from previous research that in utero events and childhood endowments affect later life health, IQ and labor market outcomes (Almond and Mazumder 2005, Black, Devereux, and Salvanes 2007, Behrman and Rosenzweig 2004, Almond, Chay, and Lee 2002).

This paper examines whether sex-selective prenatal care occurs in countries of South and Southeast Asia, with an emphasis on India.[3] We find significant differences in the prenatal health care choices of women when they are pregnant with boys relative to when they are pregnant with girls. In India women are 1.1 percentage points more likely to attend prenatal care when pregnant with a boy and receive a significantly greater number of tetanus shots. In northern India, where sex discrimination is known to be more prevalent, women are 4.6% more likely to seek prenatal care and 3% percent more likely to receive tetanus shots if they are pregnant with a boy. In the same region, women are 16% more

[1]Osmani and Sen (2003) examine fetal health in the context of sex-based discrimination, however, they do so from the channel of maternal health, and do not examine direct discrimination based on the sex of the fetus.

[2]Blencowe et al (2010) summarize decades of research on the importance of tetanus immunization during pregnancy by concluding that there is "clear evidence of the high impact of two doses of tetanus toxoid immunization given at least 4 weeks apart on neonatal tetanus." After examining field studies that use various methods, they estimate that the decrease in tetanus-related neonatal morality due to vaccination is around 94%. Other estimates from developing countries range from 70% in rural Bangladesh to 88% in India (Rahman et al. 1982, Gupta and Keyl 1998) .

[3]In this paper we are not able to distinguish between taste-based and statistical discrimination. Hence, in this exercise, we simply document *differential* treatment for sons relative to daughters. The mechanism that drives these actions could be a taste for sons or a demand for sons based on the rates of returns to or costs of raising a son.

likely to deliver their baby in a non-home environment if pregnant with a boy. We also find that women whose previous children were mainly girls tend to discriminate more when the current fetus is male (see Figure 1). Moreover, for a subset of the Indian data, we find that prenatal discrimination occurs largely among mothers who report having received an ultrasound during pregnancy. We find similar evidence in other countries of South and Southeast Asia were sex discrimination has been documented. For example, in China, women pregnant with boys are nearly 6% percent more likely to seek prenatal care. Mothers in Pakistan are 6% more likely to take iron supplements and mothers in Bangladesh attend prenatal care 7% more frequently when pregnant with a boy.

Apart from examining a new parental avenue for gender discrimination, we also bring new perspective to the vast literature on parental investments (Rosenzweig and Zhang 2009, Ashenfelter and Rouse 1998, Behrman, Rosenzweig, and Taubman 1994) that examines whether schooling or nutrition-based investments reinforce (or are affected by) the distribution of initial endowments. The notion of "initial endowments" is often related to birth weight (Loughran, Datar, and Kilburn 2004) or the residual of a human capital production function (Pitt and Rosenzweig 1990).[4] Our paper adds to the literature on parental investments by showing that initial endowments (even *within* families) are subject to preferences over gender. Thus, beyond the usual concerns with endogenous endowment formation like maternal behavior, genetic correlations et cetera, we put forth gender preferences as an additional channel for consideration when examining the impact of initial endowments on short and long term outcomes.

A common policy to address sex discrimination is to prohibit health professionals from revealing the sex of the fetus during ultrasound exams, as India did in the mid-1990s. Despite the legal efforts of the government, sex-selective abortions have risen in recent years in India (Arnold, Kishor, and Roy 2002, Bhalotra and Cochrane 2010) and the policy focus has been on trying to stamp it out; we make the point that even if all policy efforts were diverted to reduce the incidence of sex-selective abortions, an unintended consequence of such efforts could be a rise in differential investments in prenatal care. Our calculations suggest preferential treatment in one such investment, tetanus shots, can explain 4-10.5% of the excess female neonatal mortality. Hence, if gender equality is a priority, policy must be concerned about the possibility of discriminatory prenatal care leading to long term differences in the outcomes for men and women.

There are several identification problems that arise in the analysis of sex-based discrimination. The four main problems we address are selective recall, biological characteristics of male fetuses that may drive the need for additional prenatal care, son preference-based fertility stopping rules and sex-selective abortions. First, what if mothers are more likely to remember prenatal investments when they give birth to a boy? We mitigate this concern by examining periods before ultrasound technology became widely available and by assessing prenatal care that takes place early in the pregnancy, before sex determination is possible. We do not find any evidence that male fetuses received extra prenatal care before ultrasound technology became widespread or before parents know the gender of the fetus. Aside from helping us rule out selective recall bias, this result suggests that this form of gender discrimination

[4]More recently, Aizer and Cunha (2010) measure initial endowment as scores from the Bailey test administered to 8 month old babies.

is a recent phenomenon and one that likely goes hand-in-hand with technological advances (portable ultrasounds in particular). Second, what if the biological needs of a male fetus dictates greater prenatal care? To address this issue, we examine data on pregnancy complications. We do not find that women experience increased medical complications during pregnancies that result in a male birth. Moreover, if there were biological reasons for differential prenatal investments along the dimensions of iron pills or tetanus shots,[5] then we should observe that males in countries with no son preference also receive more prenatal investments. We do not find any evidence towards this when we examine countries like Thailand, Ghana and Sri Lanka where son preference is rather weak.

Third, what if son preference-based stopping rules increase the probability of observing a male birth at higher birth orders? We account for this by directly including birth order and existing sex ratios of the other children as controls in all specifications, as well as restricting the analysis to children who are "young enough" at the time of the survey (we adopt this approach from Barcellos, Carvalho, and Lleras-Muney (2010)). As preferences for gender composition might be correlated with sex of the last observed child, looking only at the most recent birth when the child is "young enough" allows us to study prenatal investments before parents are able to adjust their fertility based on the sex of the most recent child.[6]

Fourth, the presence of sex-selective abortions - which we do not observe in our data - are problematic to our approach for several reasons. In the presence of selective abortions, our regression samples omit the subset of mothers who abort female fetuses. Any selection bias that results from their omission will likely lead to an *underestimate* of the true gender gap in prenatal care as families that perform sex-selective abortions represent a highly discriminatory part of the population and these mothers would not have sought prenatal care had they been forced to take their female pregnancies to term. Another concern arises if the receipt of prenatal care influences the gender of observed live births via ultrasounds done at the time of prenatal check up and subsequent selective abortion. We show that such reverse causation does not explain our results by examining investment behavior for the sample of mothers who have attended prenatal care at least once and have thus already made abortion-related decisions. Even in this restricted sample, we find that mothers who are pregnant with boys *continue* to seek more prenatal care than their counterparts carrying daughters. This result is critical, as it indicates that sex determination leads to other forms of gender discrimination, above and beyond its effect on skewed sex ratios through sex-selective abortions. Finally, mothers who sex-selectively abort may have systematically different preferences over prenatal care; if this is true, then non-randomness of fetal gender due to sex-selective abortions and unobserved preferences of these women may lead to a classic omitted variable bias. To address this issue, we show that even among children of the *same* mother, boys tend to receive greater prenatal care than girls. Hence, if omitted parental preferences are time

[5]In several conversations with medical practitioners, we learned that while there are significant biological differences among female and male fetuses, these differences do not result in differential medical recommendations regarding prenatal care visits, tetanus shots or iron pills.

[6]A caveat here is that this method assumes no sex selective abortion. We show that even after adjusting the Barcellos et al (2010) strategy to account for potential reverse causality due to sex selective abortions, our estimated gender gap is still sizeable and significant. Moreover, we provide reasons to believe that these estimates represent a lower bound for the true level of sex-selective prenatal care. We explain this in further detail later in the paper.

invariant, then comparing the outcomes of siblings accounts for those variables. In our regressions, we also control for wealth and education, two critical factors that may determine both likelihood of committing selective abortion and receiving prenatal care (Bhalotra and Cochrane 2010, Portner 2009).

The remainder of this paper is organized as follows: Section 2 provides a simple methodological framework for examining whether sex-selective prenatal investments occur and discusses possible econometric biases, Section 3 describes the various data sets we use in this paper, Section 4 discusses the results and their ability to explain excess female neonatal mortality as well as presents a wide array of robustness checks that we employ and Section 5 concludes.

2. METHODOLOGY AND ESTIMATION ISSUES

Papers examining son preference in the US have examined the role of gender bias in differences in prenatal care (Dahl and Moretti 2004, Lhila and Simon 2008) using receipt of ultrasound scanning during pregnancy as indication that the parents know the sex of the child. Unfortunately, data on ultrasound receipt is inconsistent across the rounds of the National Fertility and Health Survey (for a select subset of the Indian sample we do have this information; we discuss the use of this data in detail in the results section). However, we rely on the idea that in the absence of ultrasounds or other methods of sex determination, there should be no systematic reason to find that males receive greater prenatal care. This section describes our basic estimation strategy and outlines the various problems that could hinder inference as well as our attempt to deal with each potential source of bias.

2.1. **Basic Specification**

Our strategy is built on the premise that under equal treatment or lack of knowledge of fetal gender, the eventual outcome of the pregnancy in terms of the gender of the child should not affect prenatal investments. To the best of our knowledge, doctor recommendations regarding basic prenatal investments like iron pills, tetanus shots or regular prenatal check ups do not vary systematically by the gender of the child. Thus finding that antenatal visits, consumption of iron supplements and tetanus shots are more likely during a pregnancy that results in a male is strong suggestive evidence of discrimination.

The empirical methodology this paper adopts is quite simple. If parents want to discriminate based on the sex of fetus, pregnancies that result in a male child should be pregnancies with greater observed prenatal care along various dimensions. The basic specification we estimate is:

$$C_{ihj} = \beta Male_{ihj} + \eta \mathbf{X}_{ihj} + D_j + \epsilon_{ihj} \tag{1}$$

Where C_{ihj} is the type of prenatal investment for child i in household h in state j such as prenatal care, iron pills, tetanus shots, et cetera. $Male_{ihj}$ takes the value of 1 when the child is male. The questions are retrospective, so the woman is asked about type of prenatal care while pregnant with a given child and then that particular child's sex is noted (more details concerning the survey data can be found in the next section). $\mathbf{X}_{ihj}$ is a host of control variables that include birth order, age and education of the mother, dummies of year of birth of the child, wealth quintiles and a dummy for whether or not the mother resides in an urban area. D_j captures state fixed effects. If prenatal sex discrimination exists and if males are favored, we should find that β is greater than zero.[7] Several important identification issues emerge when following this approach. We now review each problem and our proposed solutions in detail.

2.2. Selective recall

It is possible to find a positive β if mothers are simply more likely to report receiving prenatal care when pregnant with a boy even if actual prenatal care is not gender-biased. If males are indeed preferred, then activities that led to a male birth might be better remembered. To counter this potential selective recall concern we adopt two approaches. First we rely on the timing of spread of ultrasound technology. Ultrasound availability in India is well documented. There are reports in India that the first ultrasound clinic was opened in the Punjab in 1979 (Washington Post, May 2006), but widespread use of ultrasound was not achieved until the mid to late 1990s (Miller 2001, Bhalotra and Cochrane 2010).[8] The advent of ultrasounds - in particular, portable sonogram machines - has made sex determination less risky, easier to access and less expensive (about $12 each, according to The Economist, March 2010). Anecdotal evidence suggests that even rural areas are visited by itinerant doctors who carry ultrasound machines from town to town, offering sex determination without official prenatal care (New York Times, May 2001).[9] Thus, to tackle to issue of selective recall, we estimate of equation 1 using the NFHS survey conducted in 1992, *before* ultrasounds spread to many regions in India. If mothers are no more likely to remember prenatal care when they deliver boys than when they deliver girls, we expect to find that β is small and statistically insignificant for this sample.

A second approach is to exploit the timing of prenatal care. Sex determination is typically possible in the third or fourth month of pregnancy. In the absence of selective recall, we should find that prenatal care taken early in the pregnancy before sex determination is possible does not systematically differ for female versus male fetuses. Thus we would expect β to be small and statistically indistinguishable from zero for prenatal investments made during the first four months of gestation.

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[7]A related issue is that β might vary depending on the sex ratio of the previous children. Due to son preference-based fertility stopping rules, "who" becomes a mother at each birth order is a selected sample. Suppose we restrict the sample to people whose previous children are all girls (conditional on family size).

$$C_{ihj} = \beta_G Male_{ihj} + \gamma \mathbf{X}_{ihj} + D_j + v_{ihj}$$

The coefficient we get on $Male$ in this sample (β_G) will likely be different from the coefficient on $Male$ if we were to estimate equation 2 for families whose previous births are all male (call this β_M). Hence, β from equation 1 should be interpreted as a weighted averaged of β_G and β_M, where the weights depend on the fraction of the population that discriminate against girls in their last birth. We show estimates of β_G and β_M for various birth orders in Figure 1. As expected $\beta_G > \beta_M$ across most of the birth orders.

[8]Prior to ultrasounds, sex determination was accomplished primarily through the use of amniocentesis, a more invasive procedure involving the removal of amniotic fluid through a needle inserted into the maternal abdomen. For an excellent review on the timing of ultrasound technology spread see Bhalotra and Cochrane (2010).

[9]As we present results from China later in the paper, it is useful to mention that in China, ultrasound technology became available as early as 1965 in a few counties but coverage did not accelerate until the 1980s; by the end of the 1980s much of the country had access to an ultrasound machine (Meng 2010). For details on the spread of ultrasound machines and its consequences for sex-selective abortion in China, please see Meng (2010).

2.3. **Medical complications**

It is possible that male fetuses simply require more prenatal care than female fetuses. Hence, a concern could be that medical reasons rather than gender discrimination drive parents to give more prenatal care to male fetuses than female fetuses. We attempt to rule out this alternate explanation by examining data on pregnancy complications. The NFHS collects detailed data on pregnancy complications such as fatigue, night blindness, excessive bleeding, et cetera. Our concern would be mitigated if pregnancies that end in a male birth are not associated with significantly more complications than those ending in a female birth.

2.4. **Son preference based fertility stopping rules**

One potential source of bias in equation 1 arises due to son preference-based stopping rules. A consequence of son preference-based fertility stopping rule is that the probability the youngest child is male is increasing in the age of the last child, as parents have more time to adjust their total fertility following the birth (Barcellos, Carvalho, and Lleras-Muney 2010). Conditional on family size, this would imply that a family whose most recent birth was female would have weaker son preference even after controlling for birth order and the existing sex ratio of the child's siblings. Since the questions on prenatal care are asked only for the youngest child of the mother, our results are susceptible to bias due to such a stopping rule. As a robustness check, we employ the methodology developed in Barcellos, Carvalho, and Lleras-Muney (2010).

The main idea behind the Barcellos, Carvalho, and Lleras-Muney (2010) methodology is to examine families where the last child is "young enough" such that parents have not had time to adjust their fertility based on the gender of the most recent birth - for this sample, parents who have just had a girl are similar to parents to have just had a boy, conditional on the sex ratio of the previous children and the number of children. However, their methodology relies on the absence of sex-selective abortions. Nevertheless, we include it in our robustness check and find the estimates to be unchanged; if anything the "young enough" sample results are slightly larger in magnitude than the overall sample results.

2.5. **Sex selective abortions**

The potential for sex-selective abortions brings about three additional concerns in our estimation: sample selection bias, reverse causality and omitted variables bias. These concerns are certainly related, but dealing with each separately provides insight into various estimation techniques we use to account for these issues.

2.5.1. *Sample selection.* Because we only observe the gender and prenatal care of pregnancies resulting in live births, our sample omits those female fetuses who were terminated before birth. This introduces bias into our estimates if those who abort female fetuses would have given their unborn daughters significantly different levels of prenatal care if forced to take them to term than those who choose to take female fetuses to term. We believe that parents who perform sex selective abortions are those for whom son preference (and female discrimination) is strongest; if these parents were forced to carry the female fetus to term, it is likely that these girls would receive *less* prenatal care than those born to parents who prefer to take their female pregnancies to term. Hence, we expect our results to be *underestimates* of the true extent of gender discrimination in prenatal care.

2.5.2. *Reverse causality.* The presence of sex-selective abortions could also bring into question the direction of causation between prenatal care and fetal gender. If ultrasounds are a routine procedure taken during formal prenatal visits, then women who seek prenatal care may discover they are carrying girls and choose to abort, leading to a mechanical correlation between the gender of the fetus and measures of prenatal care. Our estimation of equation 1 compares the prenatal care received by boys and girls who have not been selectively aborted. Because fewer girls survive past the first prenatal checkup (and thus drop out of our sample), we should observe that a higher proportion of boys receive prenatal care than girls under sex-selective abortions. This would lead us to the false conclusion that the gender of the unborn child determines prenatal care - our estimate of β in equation 1 would be positive - when in fact prenatal care determines the gender of the children we observe in our sample.

Without information on the exact timing of ultrasound receipt in relation to subsequent prenatal care (which is not available in the NFHS), we are unable to isolate the direction of causation between the *first* prenatal visit and fetal gender. However, we can identify the causal effect of fetal gender on *additional* prenatal care, conditional on knowing the gender of the child and choosing to take the pregnancy to full term. If we assume that women who have been to at least 1 prenatal checkup know the sex of their unborn child, then their decision to pursue additional prenatal care is not subject to the same argument of reverse causality because they make these subsequent decisions after choosing not to abort their unborn child. In practice, we can restrict the estimation sample to those women who have gone to at least 1 prenatal visit (where we assume that they learned the sex of the child) and estimate the following regression:

$$Additional\ C_{ihj} = \beta Male_{ihj} + \eta \mathbf{X}_{ihj} + D_j + \epsilon_{ihj}$$

β now captures the gender differential in prenatal care that occurs after the first checkup and is free of any reverse causality concerns. We can further restrict the sample to those women whose first prenatal checkup occurred after the fifth month of pregnancy and are thus the most likely to learn the sex of the fetus during the first checkup. Note that this approach does not solve the problem related to sample selection, and the possibility of sex-selective abortions still leads to a potential underestimate of the true degree sex-selective prenatal investments.

2.5.3. *Other omitted variables.* If we instead interpret the problem of sex-selective abortions as a case in which the propensity to perform selective abortions is an omitted variable in our regressions, we are left with a classic problem of endogeneity; the sex of the child is no longer random and is potentially correlated with ϵ_{ihj}. In general, the direction of bias depends on the relationship between factors that influence sex-selective abortions and how these factors affect the demand for prenatal care. In our attempt to deal with the this type of bias, we also control for various factors like wealth and education which might be important determinants of sex-selective abortions. If abortions are costly, then also including a control for family wealth is important, as wealthier families are both more likely to have a male child (by aborting female fetuses)[10] and better able to afford prenatal care.[11]

If parental preferences over gender composition of children and factors that jointly determine sex-selective abortions and prenatal care are time invariant, then a mother fixed effects specification should be a robust way of countering the endogeneity concerns raised in the previous section. In some cases we have information on prenatal care for the previous two births of the same woman. In this instance, we can test whether sons receive greater prenatal care using a mother fixed effects specification. The basic specification in this case is:

$$C_{ih} = \phi Male_{ih} + \eta \mathbf{X}_{ih} + M_h + \epsilon_{ih} \tag{2}$$

Where C_{ih} is the type of prenatal investment for child i born to a mother in household h. $Male_{ih}$ takes the value of 1 when the child is male, $\mathbf{X}_{ih}$ consists of control variables such as dummy variables for year of birth of the child, birth order and the existing sex ratio of children. M_h captures mother fixed effects including time invariant preferences for gender and prenatal care. Hence, if prenatal sex discrimination exists, we should find that ϕ is greater than zero.

As long as parental preferences for gender composition and unobserved determinants of selective abortions and prenatal care are captured by the mother fixed effect, there is no reason to think that $Male_{ih}$ is correlated with ϵ_{ih} in equation 2 and the fixed effects specification provides an alternative way of examining the presence of selective prenatal care. However, the caveat is that the sample only includes mothers who have given birth *twice* in the five years prior to the survey. Hence, there might be some concerns with drawing conclusions about the general population from this sample.

3. Data

The data on pregnancies and prenatal investments used in this paper come from a wide array of sources that vary by country. The Indian sample is created using the 1998-9 and 2005-6 rounds of the National Family Health Survey (NFHS). The Bangladeshi sample draws from four waves of the Demographic and Health Survey (DHS), including the 1996-7, 1999-2000, 2004 and 2007 rounds. Lastly, the Chinese data come from the China Health and Nutrition Survey (CHNS), an ongoing project that collects panel data from 9 provinces. For this paper, we use the 1991, 1993, 1997, 2000, 2004 and 2006 rounds. Additional robustness checks uses samples drawn from other DHS rounds in Pakistan (2006-7), Ghana (1993, 1998, 2003, 2008), Sri Lanka (1987) and Thailand (1987). The NFHS and all DHS rounds are comprised of nationally representative samples with respect to each country. Appendix Table 1 displays general descriptions of all samples used in this paper.

Although the data in the paper are collected from many different sources, the method of constructing the estimation samples is very similar across all countries. Within each country we use

[10]While the daily agricultural wage in India was around 57 rupees/day in 1998-9 (and also in 2000-1), the cost of an abortion ranges from Rs. 500 (by makeshift midwives) to over Rs. 5000 when performed by a doctor. Because the wealth quintile calculated by DHS is nationally representative, we employ national sampling weights in all regressions that include wealth.

[11]According to Portner (2010) women with at least one boy and women with less than 8 years of education almost never practice sex-selective abortions during subsequent pregnancies. We get largely similar results when we restrict our sample to mothers who have had at least one boy and with low levels of wealth and education (results not shown).

the sample of ever married women generally between the ages of 15 and 49. Information is collected retrospectively about the pregnancy history of each woman, including detailed prenatal investment data from the most recent pregnancy previous to the survey. In the 1998 round of the NFHS, mothers report information about their two most recent pregnancies, allowing for the construction of a panel dataset suitable for fixed effects estimation (see previous section). We collect basic information such as age and educational attainment about mothers and wealth quintile of the family, as well as geographical data about their place of residence which is used to generate the spatial fixed-effects included in all subsequent regressions. Summary statistics for mother characteristics are presented in Appendix Table 2 for India (not shown for the remaining countries). Average educational attainment is generally low but displays considerable variation across countries. In India, the average mother in the sample is 28 years old and has completed only primary school.

With the exception of the fixed effects specifications, we restrict our attention to the most recent birth previous to the survey. In order to obtain the most accurate information, we consider only those births that have occurred in the 5-year span leading up to the survey round. Appendix Table 2 indicates that about 55% of pregnancies are male in India. In countries with low or no son preference (Ghana, Sri Lanka and Thailand), male pregnancies occur only 51% to 52% of the time; however in countries with stronger son preference (China, Bangladesh, Pakistan), the ratio is generally higher, with 56% of Chinese pregnancies resulting in live birth being male.[12] We focus our attention on the following measures of prenatal investments, although not all variables are available for all rounds in all countries: prenatal care and the number of visits, tetanus shots received and iron supplements taken during pregnancy and whether the mother chose to deliver her child in a health facility or at home. Appendix Table 2 displays the summary statistics for these outcomes of interest. Prenatal care and receipt of tetanus shots is fairly common, occurring in about 72% and 78% of pregnancies in India, respectively. However, Indian women choose to give birth in a non-home facility for only 35% of pregnancies.

4. RESULTS

We first present all of our results and robustness checks for the Indian case. In section 5.2, we show that the gender gap in prenatal care extends beyond India and can be found in other countries where son preference is known to be prevalent (Pakistan, Bangladesh, China); we also present results for countries with little known son preference (Ghana, Sri Lanka and Thailand) as a benchmark for comparison. Finally, in section 5.3 we use our results to assess the impact of differential prenatal care on excess female neonatal mortality in India.

In Table 1, we estimate equation 1 for various subsamples of the National Family Health Survey in India. Overall, we see that males tend to receive significantly more prenatal investments across almost every type of prenatal care (Appendix Table 3 shows that the coefficient of interest is stable across specifications where the independent variables are added one at a time). When we restrict the analysis to the northern region of India (Punjab, Haryana, Himachal Pradesh, Uttar Pradesh

[12] Perhaps due to the One Child Policy, birth order is not available in the Chinese data and most mothers in the sample have one or no children. Instead, we include pregnancy number as a control variable.

and Rajasthan), we see a much larger magnitude of discrimination for certain types of prenatal care treatments; this difference between the northern sample and full sample is significant for prenatal care, the number of prenatal visits, the days iron supplements are taken and the place of delivery (results not shown but are available upon request). This is consistent with other studies that find more skewed sex ratios in these regions (Jha et al. 2006), suggesting higher levels of son preference as well as greater availability of ultrasound technology (as noted earlier, Punjab was one of the first states to receive this technology). Mothers attend prenatal checks up more frequently (9%) and take more tetanus shots (4%) when pregnant with a boy. Moreover, mothers are nearly 3 percentage points more likely to invest in prenatal care when pregnant with a boy. We see slightly larger magnitudes (compared to the full sample) for samples where the previous children of the women are majority female (the children prior to the latest birth), although the differences in magnitudes in this sample relative to those in the full sample are not significant (results not shown; available upon request). If son preference is present, we should find that samples where women previously have had female children should be even more likely to differentially invest if their most recent pregnancy is a boy. For this sample of majority female in the past children of the mother, we find the magnitudes to be quite large - males are 1.7 percentage points more likely to receive tetanus shots than females, and nearly 1.5 percentage points more likely to receive some sort of prenatal care. Hence, for India, we find strong, consistent evidence that women utilize more prenatal care options when pregnant with a boy than when they are pregnant with a girl.

As mentioned earlier, we have ultrasound receipt information for a subset of the Indian sample. While the 2005-6 round asks about ultrasound usage during each pregnancy in the past 5 years, the 1998-99 round only asks about ultrasound usage among the sample of women who had at least one prenatal check up. Having ultrasound receipt information is critical to our work as ultrasounds are a likely necessity to know the sex of the child. In order to make the samples comparable, we first pool the surveys and restrict the sample to those women who had at least 1 prenatal check up. Within this sample, we examine whether mothers pregnant with males and receiving an ultrasound visit prenatal clinics multiple times. Because this sample of women are those who have already attended a prenatal checkup, they are the most likely to pursue additional prenatal care. Accordingly, the high sample means of these outcomes (often as high as 95-98%) lead us to believe that an extreme value distribution is more appropriate and thus we employ logit specifications when using this sample. Table 2 Panel A finds that males are more likely to make multiple prenatal visits when an ultrasound is received. They are also more likely to receive a tetanus shot when they report having had an ultrasound, although this is statistically significant only for the northern region and for the sample whose previous children are mainly female. These results stand in contrast to those for women who do not receive ultrasounds and are therefore unlikely to know the sex of their unborn child. With the exception of tetanus shots in the full sample, women who do not report receiving ultrasounds do not systematically discriminate in favor of male fetuses. The differences in coefficients on the male dummy variable in the two samples (those with and without ultrasounds) are statistically significant across all specifications, with the exception of tetanus in the full sample and antenatal visits in the majority female sample.

Panel B examines all births in the 2005-6 survey (since ultrasound information was asked of everyone, not just mothers who had a prenatal check up). We use similar outcome measures as

Panel A to keep matters consistent, but also because a very large fraction of those who report having had an ultrasound also report having attended prenatal care at least once (98.75%). Panel B is also consistent with our results so far, showing that women who receive ultrasounds take differentially better care of their male fetuses (although the results for the sample with majority female is not statistically significant). In the samples of women who did not receive an ultrasound during their pregnancies, we find no evidence of gender discrimination in prenatal care, although the difference in coefficients across the ultrasound and non-ultrasound samples is statistically significant only for antenatal checkups in the full and northern samples.

However, there are several important caveats involved with using the ultrasound data. First, the ultrasound variable is likely be measured with noise. Given the illegality of sex determination, many women may be reluctant to admit that they have received an ultrasound during their pregnancy. Moreover, as discussed in an earlier section, ultrasound technology has become available even through unofficial channels. Women who determine the sex of their baby without having to engage in formal prenatal care may be less likely to recall or report that they have received an ultrasound. For both of these reasons, we might expect the proportion of our sample who actually received ultrasounds to be much higher than the 14% and 27% reported in the 1998-9 and 2005-6 rounds of the NFHS, respectively.

4.1. **Robustness Checks**

As mentioned earlier, four main identification issues arise when examining sex-selective prenatal care - selective recall, male fetus-specific pregnancy complications, endogeneity due to son preference-based fertility stopping rules and bias due to sex-selective abortions. The results in Tables 5-10 show that our findings are robust to these concerns.

First, we estimate the relationship in equation 1 for births prior to widespread availability of ultrasound or sex determination technology. In Table 3, we do not find any evidence towards prenatal discrimination among births that occurred in the late 1980s in India. As mentioned earlier, ultrasound technology appears to have become widespread in the 1990s. Under selective recall, we should find mothers reporting greater prenatal care for male babies even in the absence of ultrasound receipt. Another way to rule out the possibility of selective recall is to examine prenatal care outcomes that occur before fetal gender is detectable. In Table 4 we exploit the timing of the first prenatal checkup and show that there is no gender gap in prenatal care that occurs within the first four months of pregnancy, when the sex of the fetus is unknown. In contrast, there is a large and significant gap in care that takes place in the final five months of pregnancy. Thus we believe that the existence of selective recall cannot explain this pattern of discrimination in our results, even within the same pregnancy. [13]

Second, we estimate whether being pregnant with a boy leads to more complications during the pregnancy. If carrying a male were more physically taxing than carrying a girl, then we might find that women pregnant with boys are more likely to seek prenatal care for reasons other than gender

[13] While we lack data on the timing of prenatal visits after the first, the outcome for column 2 of Table 4 is constructed using information on the timing of the first visit and the total number of prenatal visits. See the notes to the table for a detailed description of how this variable is constructed.

discrimination. Table 5 estimates whether being pregnant with a boy is significantly related to complications during pregnancy in India. Except for the category of "night blindness" we do not find any evidence to support the idea that male fetuses *medically* require greater prenatal care through increased complications. Moreover, the size of the coefficient on night blindness is extremely small compared to the average level of night blindness experienced by mothers in the sample. [14]

Third, following Barcellos, Carvalho, and Lleras-Muney (2010) we restrict the sample to families where the youngest child is less than 2 years old to minimize the bias due to families adjusting their fertility after realizing the sex of the child. The first three columns of Table 6 display the results using this sample. For outcomes such as prenatal care, tetanus shots and delivery in a non-home facility, our original results hold and the magnitudes of the gender gap are slightly larger. One drawback of this method is that it relies on the assumption of no sex-selective abortions. As we indicated earlier, the presence of selective abortions can bring about two important sources of bias: sample selection and reverse causation. While the bias due to sample selection likely works in the opposite direction of our result, reverse causation could potentially bias our estimates upwards. To account for this possibility, we further restrict the sample to those who have made at least one prenatal visit and focus on whether these women seek higher levels of *additional* prenatal care when carrying boys than when carrying girls (columns 3 - 6 of Table 6). Again, we find that even in this restricted sample, male fetuses receive significantly more care than female fetuses and the magnitude of the gender gap remains substantial. Thus we find no evidence that any bias due to gender-based stopping rules is driving our results.[15]

Our final concern has to do with reverse causality due to sex selective abortions. If mothers learn the sex of their child during a prenatal visit and choose to abort if female fetuses, then we would find a mechanical correlation between prenatal care visits and sex of the child due to sex selective abortions, rather than sex selective prenatal care. As we proposed in an earlier section, one approach that minimizes the potential for reverse causality is to restrict the sample to those who have made at least one prenatal visit and examine the gender gap in subsequent prenatal care. The first three columns of Table 7 display the results for this approach. We find that even in this sample (in which reverse causation is highly unlikely) there remains a sizeable gender gap in prenatal care undertaken after the first visit. Moreover, when we take an even more conservative approach by restricting the sample to women who make their first prenatal visit in the final five months of pregnancy (for whom the assumption of discovering the sex of the child during the first visit is most credible), we find that there remains a high degree of gender discrimination in prenatal care. Although the gender gap is not

[14]A concern might be that if women carrying male fetuses *do* need greater prenatal care, then perhaps Table 5 does not reflect differential complications by male because mothers take greater prenatal care while pregnant with a male. We rule out this possibility by showing that for the sample that does not receive any prenatal care, we find that carrying a male child does not lead to more complications (table not shown, available upon request). The other category that shows up significant in this regression is anemia. However, the sign on this is negative, suggesting that mothers when carrying a male do more things to avoid becoming anemic - a common way to do this is to take iron pills. This is consistent with the finding that mothers practice greater prenatal care when pregnant with a male.

[15]Table 8 (mother fixed effects) is another way to account for the endogeneity problem if preferences over gender composition are time invariant. When we restrict the Barcellos et al (2010) sample even further to those children under the age of 1 at the time of the survey, the dummy on male is no longer significant across many specifications (results not shown). However, the magnitude of the coefficients are similar to those in Table 1, suggesting that our original estimates do not suffer heavily from bias arising from son preference-based stopping rules.

significant for tetanus shots and non-home delivery, we believe this is largely due to the considerable drop in sample size and more importantly, the magnitude of the of the gender gap is consistent with our main results, indicating that reverse causality is not likely to be sole explanation for our results.

Lastly, Table 8 estimates the fixed effects specification in equation 2 for India where we have data on the previous two births of the mothers. We find similarly consistent results for a wide range of prenatal investments. Even within families, mothers appear to make more investments when pregnant with a boy as opposed to a girl. Compared with the estimates of Table 1, the fixed effects estimates are slightly larger in magnitude, although the samples are not the same (the mother fixed effects sample contains mothers who gave birth twice in the five years prior to the survey in 1998). Mothers are 4.3% more likely to consume iron pills and visit prenatal clinics nearly 4% more frequently when pregnant with a boy.

4.2. **Results from other countries**

Since the DHS collects extensive prenatal care data, we can extend our analysis to other countries in South and Southeast Asia.[16] We estimate equation 1 for China, Bangladesh, and Pakistan - countries where son preference and gender discrimination has been well established in previous studies (Das Gupta et al. 2003) - and find that the gender bias in prenatal care is not limited to India but is rather pervasive in Southeast Asian countries with a history of son preference. As part of a larger robustness check, we estimate equation 1 for Sri Lanka and Thailand where son preference is weak (Arnold, Kishor, and Roy 2002, Hua 2001, Prachuabmoh, Knodel, and Alers 1974). As a final robustness check, we investigate whether sex-selective prenatal care is practiced in Ghana, a country with no known son preference (Garg and Morduch 1998).

The first four rows of Appendix Table 4 display the results of estimating 1 for countries that are known to have son preference: China, Bangladesh, and Pakistan (both the full sample and the region of Punjab).[17] Overall, the results from these samples exhibit patterns consistent with sex-selective discrimination in prenatal care. In China, women are 5% more likely to get some prenatal care when pregnant with a boy, and visit antenatal clinics nearly 10% more frequently. In Bangladesh, women are 2.8 percentage points more likely to get a tetanus shot when pregnant with a boy. We do not find significant estimates in the decision to seek prenatal care, although we do find that women visit prenatal clinics 7% more frequently when pregnant with a boy. In Pakistan, we find that women visit prenatal clinics more often and are 6% more likely to consume iron pills when pregnant with a boy. In Paskistani Punjab, a region with a large number of missing women (Gechter 2010), the magnitude of discrimination is even larger for some prenatal outcomes; for example, mothers are 13% more likely to take iron pills. Taken all together, the evidence in Appendix Table 4 implies that the practice of sex-selective prenatal investments extends beyond India and is widespread across areas with well documented son preference.

[16]In principle, *all* DHS countries can be used in this analysis. Based on our reading of the literature on son preference and gender discrimination, we believe we have focussed on a part of the world where this is most relevant.
[17]Note that not all outcomes are available for China.

Finally, we estimate equation 1 for countries with no (or at least lesser) established son preference. The last three rows of Appendix Table 4 displays the estimates for Sri Lanka, Ghana and Thailand. While almost all specifications are statistically insignificant, what is relevant for us is that the magnitudes are quite small. At a minimum, these coefficients are smaller than what we found for countries with known son preference. The estimates in Sri Lanka and Thailand are consistent with lower levels of son preference and none are statistically significant. These results also help rule out the possibility of factors confounding son preference in prenatal investments; there is no evidence of selective recall or biological factors which indicate that boys are more likely to receive prenatal care for reasons other than son preference.

4.3. **Impact on excess female neonatal mortality**

A question of interest in this context is, "How many more girls would there be under equal treatment of prenatal care"? In this paper, we attempt to answer this question by examining the gender differential in maternal tetanus vaccinations rather than general prenatal care. This is mainly because prenatal care is multidimensional in nature and can vary from facility to facility; this makes it difficult to assess the causal role that prenatal care plays in determining infant or child mortality. However, tetanus is a rather specific infection to which neonates are particularly susceptible. Moreover, as mentioned earlier, tetanus shots have a large impact on reducing neonatal deaths due to tetanus. Hence, in this section, we calculate (with some assumptions) the number of girls that would have been saved in the neonatal stage had there been no gender bias in the receipt of tetanus immunizations.

While neonatal deaths occur more frequently among males, this does not mean that there are no "excess" female deaths in the neonatal stage. In our sample for India, the observed neonatal death rate is 2.24% for girls. Female neonatal mortality rate in Ghana and Italy is around 1.93%. Using the sex ratio in neonatal mortality from these countries (since they are presumed to be free of son preference), we impute a neonatal mortality rate for women in India to be around 1.94%.[18] Thus excess female neonatal mortality - the amount that the rate exceeds what we expect under equal treatment - is 0.31 percentage points in India.[19]

Our estimates from Table 1 suggest that males are 1.6% more likely to receive tetanus shots than females (this is our smallest effect across all specifications for India). This implies that for every 100 boys, only 98.4 girls receive tetanus shots. If we take estimates from Rahman et al. (1982), we would believe that babies face a mortality rate that is 3.03 times higher in the neonatal stage if the mother did not receive a tetanus shot. Since 80.3% of all mothers pregnant with girls receive tetanus shots, the implied neonatal mortality rate for those whose mothers were received the shots is 1.6% and 4.85% for those whose mothers did not.

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[18]Ulizzi and Zonta (2002) find that the sex ratio in neonatal deaths is 0.59. Given that we observe a 958 neonatal deaths among boys in our sample, the natural rate for girls would be 1.94% in order to maintain the proper sex ratio. That is, the number of neonatal deaths among girls that we expect in order to yield the sex ratio of 0.59 is given by 958/(958+x)=0.590, i.e. 665.7 deaths. Since we have 34,239 female births in our sample, this implies a natural or equal treatment neonatal mortality rate of 665.7/34,239=1.94% for girls.

[19]Please see the appendix for details on all calculations in this section.

This means that had the 1.6 girls that did not receive tetanus shots actually received one, 0.012 more girls would have survived than in the case of differential treatment. Hence, unequal allocation of tetanus shots can explain around 3.9-4.0% of the "excess" female mortality in the neonatal stage (depending on whether we use the benchmark estimate from Italy or Ghana). If instead we use our largest estimates that males are 4.34% more likely to receive tetanus shots (from Table 8, the mother fixed effects table), we conclude that unequal allocation of tetanus immunizations can explain around 10.1%-10.5% of the excess female neonatal mortality (again, depending on which estimate for equal treatment we use). Therefore, we believe that discriminatory practices with regards to tetanus vaccinations during the prenatal period can explain between 4-10.5% of the excess female mortality in the neonatal period.[20]

5. Conclusion

This paper examines whether preference for sons in certain countries in South and Southeast Asia leads parents to differentially invest in their unborn children. We find evidence that in countries known to have son preference - namely India, China, Bangladesh and Pakistan - parents invest in greater prenatal care when pregnant with a boy. We successively rule out confounding factors such as biological biases, the presence of sex-selective abortion, son preference-based fertility rules and selective recall of prenatal care. Moreover, we find no evidence of sex-selective prenatal care in countries with weak or no son preference nor do we see gender biased investments in years before widespread availability of sex determination. Hence, the weight of the evidence points towards gender discrimination in prenatal investments. Specifically in India, we find sex-selective prenatal care in tetanus to have important consequences in relation to female neonatal mortality rates. Female neonatal mortality is higher than what it should be under equal treatment in India; we estimate that equal treatment of tetanus shots should decrease this gap by 4-10.5%.

We believe our results contribute to the literature in three ways. First, our paper adds to the growing body of work examining consequences of son preference in South and Southeast Asia. We believe we are the first to give empirical evidence that such son preference leads to sex-selective *pre*natal investments in these regions. These sizeable gender differentials in prenatal investments are likely to be the cause of the large observed disparity in short-term health outcomes between girls and boys. In our Indian sample, boys are born with greater birth weight and have a lower probability of being low birth weight.[21] While this is not causal, it is consistent with boys receiving greater prenatal care. Correlations between various dimensions of prenatal care (such as tetanus shot receipt and iron pill supplements) and outcomes such as neonatal deaths and birth weight show that babies that receive some prenatal care are better off. Hence, sex-selective prenatal care can be associated with differential birth weight and neonatal death rates among boys and girls.

Second, policy in countries like India is focused on a natural and important outcome of sex-based discrimination - survival rates of females measured via sex ratios at different ages. Given the

[20] If we instead use our estimates from the ultrasound sample, we find a lower bound estimate that discrimination in tetanus shot receipt explains 0.6% of excess female neonatal mortality.

[21] The correlations discussed in this section are not shown but are available upon request.

findings from the vast literature linking early childhood health (such as birth weight) and later life outcomes, our results imply that effect of gender discrimination in prenatal care might also be seen in the long run via decreased labor market opportunities or decreased educational attainment for women. Hence, even if the imbalance of sex ratios improves over time, we should worry about the possibility of sex-selective prenatal care.

Third, we provide a unique perspective on the literature concerned with parental investments based on child endowments. Our study brings into question the very process of the endowment formation - child endowments, often measured as birth weight are themselves the result of parental preferences over gender. Hence, studies investigating such relationships in developing countries with son preference must seriously consider the possibility that parents differentially invest based on the sex of their unborn child.

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Appendix A. Calculating the contribution of differential tetanus immunizations to excess female mortality

Girls are more likely to survive than boys in the neonatal period for genetic and biological reasons. We use female neonatal mortality rate in the Ghanian DHS data as a measure of the "natural" neonatal mortality rate for girls. Restricting the sample to the 1998, 2003 and 2008 rounds (in order to be comparable to the NFHS time frame used in our regressions), the female neonatal mortality rate is 1.93%. When we use the results of a study in Italy (Ulizzi and Zonta 2002) we impute a natural rate of 1.94%; thus we are confident that this represents an accurate measure of neonatal mortality among girls in the absence of differential treatment and use it in all calculations below.[22] The neonatal mortality rate is 2.24% among girls in our sample from India. This implies that the excess female neonatal mortality is 2.24-1.93=0.31 percentage points.

According to Rahman et al. (1982), babies are 67% less likely to die in the neonatal period if their mothers received tetanus shots during pregnancy; this implies that babies whose mothers did *not* receive tetanus shots are 3.03 times as likely to die.[23] As mentioned before, the neonatal mortality rate is 2.24% among girls in the Indian sample. Since 80.3% of all mothers pregnant with girls receive tetanus shots, the implied neonatal mortality rate for those whose mothers were received the shots solves 0.803x + 3.03(1-0.803)x=2.24. This yields a mortality rate of 1.6% for female children born to women who received tetanus shots and 4.85% for those whose mothers did not.

Our estimates in Table 1 show that women are 1.6% less likely to receive tetanus shots when pregnant with girls than when pregnant with boys. This means that for every 100 boys who receive tetanus immunization through their mothers, only 98.4 girls do. If mothers were equally likely to receive tetanus shots (regardless of fetal gender) then the remaining 1.6 girls out of 100 would have tetanus immunity. Under equal treatment the number of girls who die from tetanus is 0.23(0.016)100=0.368 per 100, where 23% of neonatal deaths are due to tetanus in India (UNICEF 2000) and the neonatal mortality rate is 1.6% (calculated above).[24] Under differential treatment, where 1.6 girls are born to mothers who have not had tetanus shots, 0.23((1.6)0.0485+(100-1.6)0.016)=0.380 girls die per 100 because the 1.6 girls without tetanus immunity face a higher mortality rate of 4.85% (calculated above). Thus, the difference in tetanus shots leads to a difference in observed neonatal mortality of 0.380-0.368=0.012 deaths per 100 girls.

Therefore, the gender gap in tetanus shots can explain 0.012/0.31=3.86% of excess female neonatal deaths in India (or 3.98% if we use the Italian benchmark). If we repeat all of the calculations using the upper bound of our estimates for India (the mother fixed effect specification, results in Table

[22]Ulizzi and Zonta (2002) find that the sex ratio in neonatal deaths is 0.59. Given that we observe a 958 neonatal deaths among boys in our sample, the natural rate for girls would be 1.94% in order to maintain the proper sex ratio. That is, the number of neonatal deaths among girls that we expect in order to yield the sex ratio of 0.59 is given by 958/(958+x)=0.590, i.e. 665.7 deaths. Since we have 34,239 female births in our sample, this implies a natural neonatal mortality rate of 665.7/34,239=1.94% for girls.

[23]We consider this to be a conservative measure, as Blencowe et al (2010) find an 94% reduction in neonatal tetanus when mothers are immunized.

[24]Again, this is likely to be a conservative estimate; Gupta and Keyl (1998) find that tetanus accounts for 23-73% of all neonatal deaths.

4) we find that differential tetanus treatment accounts for 10.1% of the gap between the natural and observed rates of neonatal mortality (10.5% using the imputed rate from Italy). Hence we believe that the gender bias in prenatal tetanus immunizations can explain 4%-10.5% of excess female neonatal mortality.

FIGURE 1. Gender Differentials in Prenatal Care, by birth order and sex composition of previous children

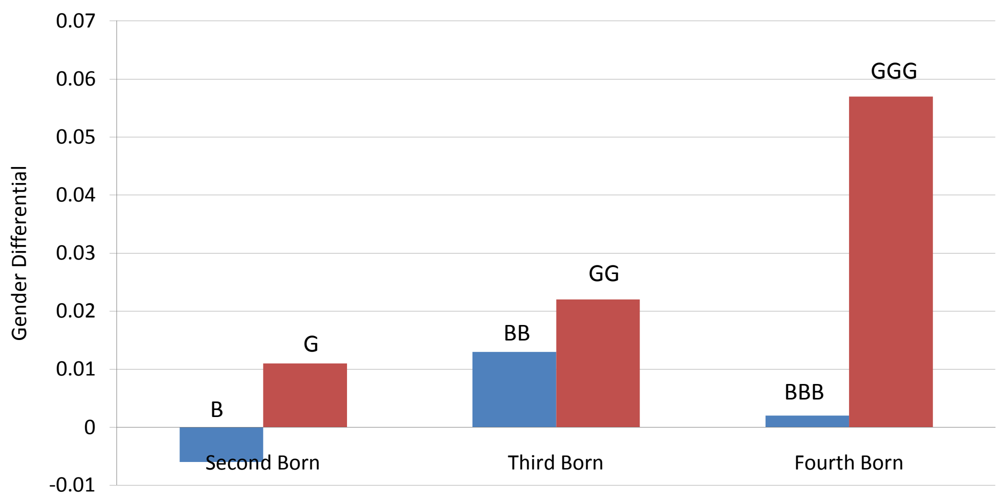

TABLE 1. Sex-Selective Prenatal Investments in India

	Outcomes						
Coefficient on Male in various samples	Prenatal Care (1=Yes, 0=No)	Number of Prenatal visits	Tetanus Shot (1=Yes, 0=No)	Number of Tetanus Shots	Iron Pills (1=Yes, 0=No)	Days Took Iron Supplement	Non-Home Delivery (1=Yes, 0=No)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Full Sample	0.011**	0.058*	0.011**	0.039***	0.000	0.349	0.017***
	(0.006)	(0.030)	(0.005)	(0.014)	(0.006)	(0.938)	(0.005)
Mean of Dependent Variable	0.688	2.780	0.777	1.680	0.590	39.7	0.312
Observations	32233	32012	32017	32017	32166	17698	31073
R-squared	0.311	0.477	0.191	0.176	0.242	0.296	0.374
Northern Region	0.028**	0.189***	0.022*	0.056**	0.004	2.603*	0.038***
	(0.011)	(0.048)	(0.011)	(0.027)	(0.012)	(1.452)	(0.009)
Mean of Dependent Variable	0.601	2.122	0.685	1.435	0.488	29.0	0.236
Observations	8369	8304	8324	8324	8349	4161	8106
R-squared	0.274	0.466	0.182	0.175	0.228	0.291	0.413
Majority Female Sample	0.015*	0.073*	0.017**	0.051**	-0.001	-0.169	0.007
	(0.008)	(0.044)	(0.008)	(0.021)	(0.009)	(1.439)	(0.008)
Mean of Dependent Variable	0.704	2.864	0.793	1.714	0.603	41.7	0.324
Observations	14413	14302	14321	14321	14387	7904	13941
R-squared	0.317	0.482	0.186	0.172	0.240	0.301	0.371

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Notes: Sample is restricted to most recent birth of ever married women (ages 15-49) within 5 years previous to survey. NFHS surveys from 1998 and 2004 used. Days took iron supplements is available only for the 2004 survey. National sample weights are used in all regressions. Controls include: state fixed effects, birth year fixed effects, survey year fixed effects, household wealth quintile fixed effects, mother's age, mother's education, dummy for urban, birth order, and existing sex ratio of children (defined as the ratio of boys to the total number of births prior to the most recent one). Northern region is defined as the following states: Haryana, Himachal Pradesh, Punjab, Rajasthan, Uttar Pradesh, and New Dehli.

TABLE 2. Sex-Selective Prenatal Investments in India: Births to Women with and without Ultrasounds (Logit specification)

	Full Sample				Northern Region				Majority Female			
	At least 2 Antenatal Visits		Tetanus Shot (1=Yes,0=No)		At least 2 Antenatal Visits		Tetanus Shot (1=Yes,0=No)		At least 2 Antenatal Visits		Tetanus Shot (1=Yes,0=No)	
PANEL A - 1998 & 2005 Survey data, restricted to women who had atleast 1 prenatal check up	With Ultrasound	Without Ultrasound	With Ultrasound	Without Ultrasound	With Ultrasound	Without Ultrasound	With Ultrasound	Without Ultrasound	With Ultrasound	Without Ultrasound	With Ultrasound	Without Ultrasound
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Male	0.530**	0.117*	0.170	0.041	0.692**	0.080	0.938**	0.110	0.589*	0.085	0.673**	0.106
	(0.223)	(0.061)	(0.233)	(0.080)	(0.299)	(0.118)	(0.396)	(0.153)	(0.314)	(0.094)	(0.314)	(0.123)
Constant	3.184***	2.388***	5.517***	2.292***	2.226	0.362	5.690***	1.809**	6.865***	2.161***	3.548***	1.302**
	(1.086)	(0.300)	(1.137)	(0.307)	(1.441)	(0.575)	(1.852)	(0.822)	(1.574)	(0.404)	(1.364)	(0.526)
P-value of the test that the coefficient on Male is the same in the with and without ultrasound samples	0.074		0.600		0.057		0.051		0.152		0.093	
Mean of Dependent Variable	0.963	0.868	0.974	0.938	0.950	0.844	0.971	0.923	0.964	0.872	0.975	0.941
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Birth Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5171	16870	5970	16891	1605	3772	1542	3785	2226	7522	2541	7542

	Full Sample				Northern Region				Majority Female			
	At least 2 Antenatal Visits		Tetanus Shot (1=Yes,0=No)		At least 2 Antenatal Visits		Tetanus Shot (1=Yes,0=No)		At least 2 Antenatal Visits		Tetanus Shot (1=Yes,0=No)	
PANEL B - 2005 Data, all women	With Ultrasound	Without Ultrasound	With Ultrasound	Without Ultrasound	With Ultrasound	Without Ultrasound	With Ultrasound	Without Ultrasound	With Ultrasound	Without Ultrasound	With Ultrasound	Without Ultrasound
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Male	0.479**	0.036	0.075	0.058	0.675**	0.064	0.729*	0.125	0.351	-0.015	0.376	0.047
	(0.213)	(0.053)	(0.245)	(0.058)	(0.318)	(0.093)	(0.401)	(0.099)	(0.289)	(0.082)	(0.334)	(0.090)
Constant	4.500***	1.292***	3.055***	1.887***	0.937	-1.399***	3.841**	0.500	4.635***	0.929**	2.310	1.377**
	(1.029)	(0.266)	(0.996)	(0.309)	(1.344)	(0.411)	(1.699)	(0.457)	(1.129)	(0.431)	(1.597)	(0.554)
P-value of the test that the coefficient on Male is the same in the with and without ultrasound samples	0.043		0.946		0.065		0.143		0.224		0.093	
Mean of Dependent Variable	0.947	0.555	0.971	0.761	0.944	0.574	0.971	0.685	0.948	0.567	0.970	0.776
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Birth Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4557	13138	4759	13164	1240	3035	1249	3044	2074	5701	2083	5716

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: Coefficients (not marginal effects) reported. Sample is restricted to most recent birth (within 5 years previous to the 2005-6 survey only) of ever married women. Other controls included are mother's age and education, birth order of most recent birth, dummies for each household wealth quintile, an urban area dummy and existing sex ratio of children (defined as the ratio of boys to the total number of births prior to the most recent one).

TABLE 3. Sex-Selective Prenatal Investments in India: Pre-Ultrasound Period (births in 1992 and earlier)

	Prenatal Care (1=Yes, 0=No)	Number of Prenatal visits	Tetanus Shot (1=Yes, 0=No)	Number of Tetanus Shots	Iron Pills (1=Yes, 0=No)	Non-Home Delivery (1=Yes, 0=No)
	(1)	(2)	(3)	(4)	(5)	(6)
Male	-0.001	0.072	-0.011	0.023	-0.001	-0.001
	(0.012)	(0.066)	(0.012)	(0.023)	(0.013)	(0.011)
Mean of Dependent Variable	0.632	2.530	0.725	2.114	0.624	0.309
Observations	4218	4216	4211	3049	4218	4217
R-squared	0.328	0.441	0.240	0.119	0.239	0.384

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: Sample is restricted to most recent birth occuring in 1992 or earlier by ever married women (ages 15-49). Wealth index is not available for over 90% of the sample so it is not included. Controls include: state fixed effects, birth year fixed effects, survey year fixed effects, mother's age, mother's education, dummy for urban, birth order, and existing sex ratio of children (defined as the ratio of boys to the total number of births prior to the most recent one).

TABLE 4. Sex-Selective Prenatal Investments in India: Timing of Prenatal Care

	Prenatal Care Received WITHIN First Four Months of Pregnancy (1=Yes, 0=No)	Prenatal Care Received AFTER First Four Months of Pregnancy (1=Yes, 0=No)
	(1)	(2)
Male	0.007	0.013**
	(0.006)	(0.006)
Mean of Dependent Variable	0.659	0.435
Observations	32233	32233
R-squared	0.244	0.321

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: Sample is restricted to most recent birth of ever married women (ages 15-49) within 5 years previous to survey. Receiving prenatal care after four months of pregnancy is defined as 1 if women make their first prenatal visit after four months of pregnancy or if they make their first prenatal visit during the first four months of pregnancy but make multiple visits over the course of the pregnancy and as 0 otherwise. Controls include: state fixed effects, birth year fixed effects, survey year fixed effects, mother's age, mother's education, dummy for urban, birth order, and existing sex ratio of children (defined as the ratio of boys to the total number of births prior to the most recent one). National sample weights are used in all regressions.

TABLE 5. Gender and Pregnancy Complications in India

	Night Blindness (1=Yes, 0=No)	Blurred Vision (1=Yes, 0=No)	Convulsions (1=Yes, 0=No)	Swelling (1=Yes, 0=No)	Fatigue (1=Yes, 0=No)	Anemia (1=Yes, 0=No)	Excessive Bleeding (1=Yes, 0=No)	Any Complication (1=Yes, 0=No)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Male	0.009**	0.003	0.002	-0.006	0.006	-0.015**	0.000	0.001
	(0.003)	(0.004)	(0.004)	(0.005)	(0.005)	(0.007)	(0.002)	(0.005)
Mean of Dependent Variable	0.117	0.133	0.125	0.250	0.482	0.256	0.039	0.596
Observations	32225	32236	32225	32237	32236	13911	32217	32252
R-squared	0.059	0.087	0.058	0.025	0.054	0.059	0.008	0.052

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: Sample is restricted to most recent birth of ever married women (ages 15-49) within 5 years previous to the survey. Controls include: state fixed effects, birtl year fixed effects, survey year fixed effects, mother's age, mother's education, dummy for urban, birth order, and existing sex ratio of children (defined as the ratio of boys to the total number of births prior to the most recent one). National sample weights are used in all regressions .

TABLE 6. Sex-Selective Prenatal Investments in India: Children Aged 0-23 months at Time of Survey

	Full Sample			**Mothers Who Receive At Least 1 Prenatal Checkup**		
	Prenatal Care (1=Yes, 0=No)	Tetanus Shot (1=Yes, 0=No)	Non-Home Delivery (1=Yes, 0=No)	At least 2 Prenatal Visits	At least 2 Tetanus Shots (1=Yes,0=No)	Non-hone Delivery (1=Yes, 0=No)
	(1)	(2)	(3)	(4)	(5)	(6)
Male	0.014*	0.013*	0.020***	0.016**	0.017**	0.022**
	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)	(0.009)
Mean of Dependent Variable	0.685	0.776	0.297	0,880	0.831	0.399
Observations	18149	18061	17581	12897	12914	12583
R-squared	0.313	0.186	0.360	0.097	0.070	0.331

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: Sample is restricted to most recent birth of ever married women (ages 15-49) within 1 year previous to the survey. Sample is further restricted to births of children aged 0-23 months at the time of the survey. Days took iron supplements is available only for the 2004 survey. Controls include: state fixed effects, birth year fixed effects, survey year fixed effects, mother's age, mother's education, dummy for urban, birth order, and existing sex ratio of children (defined as the ratio of boys to the total number of births prior to the most recent one). National sample weights are used in all regressions .

TABLE 7. Sex-Selective Prenatal Investments in India: Additional Prenatal Care

	All with At Least 1 Prenatal Visit			All with First Prenatal Checkup during the Final 5 Months of Prengancy		
	At least 2 Prenatal Visits	At least 2 Tetanus Shots (1=Yes,0=No)	Non-hone Delivery (1=Yes,	At least 2 Prenatal Visits	At least 2 Tetanus Shots (1=Yes,0=No)	Non-hone Delivery (1=Yes,
	(1)	(2)	(3)	(4)	(5)	(6)
Male	0.013**	0.013**	0.018***	0.027**	0.012	0.018
	(0.005)	(0.006)	(0.007)	(0.011)	(0.011)	(0.012)
Mean of Dependent Variable	0.889	0.848	0.419	0.813	0.783	0.280
Observations	22983	23016	22351	7547	7630	7365
R-Squared	0.091	0.063	0.342	0.082	0.069	0.216

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Notes: Sample is restricted to most recent birth (within 5 years previous to the 2005-6 survey only) of ever married women. Controls include: state fixed effects, birth year fixed effects, survey year fixed effects, mother's age, mother's education, dummy for urban, birth order, and existing sex ratio of children (defined as the ratio of boys to the total number of births prior to the most recent one). National sample weights are used in all regressions .

TABLE 8. Sex-Selective Prenatal Investments in India: Mother Fixed Effects Specification

	Prenatal Care (1=Yes, 0=No)	Number of Prenatal visits	Tetanus Shot (1=Yes, 0=No)	Number of Tetanus Shots	Iron Pills (1=Yes, 0=No)	Non-Home Delivery (1=Yes, 0=No)
	(1)	(2)	(3)	(4)	(5)	(6)
Male	0.022	0.112*	0.031**	0.082**	0.029*	0.004
	(0.015)	(0.064)	(0.015)	(0.034)	(0.016)	(0.013)
Birth Order	0.017	0.209	0.018	0.016	0.032	-0.019
	(0.041)	(0.181)	(0.040)	(0.095)	(0.043)	(0.036)
Existing Sex Ratio of Children	0.020	0.093	-0.017	-0.021	0.006	-0.016
	(0.018)	(0.084)	(0.018)	(0.044)	(0.021)	(0.017)
Mean of Dependent Variable	0.626	2.297	0.715	1.460	0.544	0.267
Mother Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Birth Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	6304	6293	6287	6287	6329	6308
R-squared (within)	0.030	0.005	0.011	0.007	0.013	0.003
Number of Mothers	3968	3962	3956	3956	3982	3969

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: Sample is restricted to two most recent births of ever married women (ages 15-49). Sample includes only mothers who report prenatal investment information for at least 2 births in the 5 years within previous to the survey. Existing sex ratio is defined as the ratio of boys to the total number of births prior to the most recent one.

APPENDIX TABLE 1. Description of Regression Samples.

	Country						
	India	China	Bangladesh	Pakistan	Ghana	Sri Lanka	Thailand
Survey years	(1992-3), 1998-9, 2005-6	1991, 1993, 1997, 2000, 2004, 2006	1996-7, 1999-2000, 2004	2006-7	1993, 1998, 2003, 2008	1987	1987
Birth years	1995-2006	1989-2006	1991-2007	2001-2007	1988-2008	1982-1987	1982-1987
Number of observations	36755	1482	15916	5063	14290	2190	1986
Level of spatial fixed effects	State	Community	Region	District	Region	Region	Region
Number of communities, states or regions	29	235	6	54	10	7	5

APPENDIX TABLE 2. Summary Statistics for India

Mother Characteristics	Observations	Mean	Standard Dev.	Min	Max
Age	36755	28.08	5.52	15	49
Education	36755	0.90	1.00	0	3
National Wealth Quintile	36755	2.95	1.37	1	5

Pregnancy Characteristics	All		Male		Female	
	Mean (Standard Dev.)	Observations	Mean (Standard Dev.)	Observations	Mean (Standard Dev.)	Observations
Male	0.55	36755				
Birth order	3.48	36755	3.46	20041	3.50	16714
	(1.78)		(1.76)		(1.79)	
Existing Sex Ratio of Children	0.49	36755	0.47	20041	0.51	16714
	(0.39)		(0.39)		(0.39)	
Ultrasound Receipt (1=Yes,0=No): 1998-9 only	0.14	9140	0.14	4986	0.13	4154
Ultrasound Receipt (1=Yes,0=No): 2004-5 only	0.27	18888	0.28	10353	0.26	8535
Prenatal Care (1=Yes, 0=No)	0.72	32233	0.73	17503	0.71	14730
Number of Prenatal visits	3.09	32012	3.16	17377	3.01	14635
	(3.17)		(3.20)		(3.13)	
Tetanus Shot (1=Yes, 0=No)	0.78	32017	0.79	17376	0.77	14641
Number of Tetanus Shots	1.64	32017	1.66	17376	1.61	14641
	(1.03)		(1.02)		(1.04)	
Iron Pills (1=Yes, 0=No)	0.61	32166	0.62	17458	0.61	14708
Days Took Iron Supplement	45.53	17698	46.32	9654	44.58	8044
	(63.08)		(63.52)		(62.55)	
Non-Home Delivery (1=Yes, 0=No)	0.35	31073	0.36	16869	0.33	14204

Notes: Education of mother is the highest level of educational attainment: 0 = no education, 1 = primary school, 2 = secondary school, 3 = higher education. Sample includes most recent births by ever married women (ages 15-49) within 5 years previous to the survey. Existing sex ratio is defined as the ratio of boys to the total number of births prior to the most recent one. Statistics describe the sample and are thus not weighted.

APPENDIX TABLE 3. Sex-Selective Prenatal Investments in India: Robustness to Different Sets of Control Variables

	Dependent Variable: Prenatal Care (1=Yes, 0=No)				
	No Controls	Geographic and Survey Controls	Adding Child-level Controls	Adding Mother-level Controls	Adding Household-level Controls
	(1)	(2)	(3)	(4)	(5)
Male	0.016**	0.017***	0.016***	0.013**	0.011**
	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)
Urban		0.133***	0.116***	0.071***	0.026***
		(0.007)	(0.007)	(0.007)	(0.007)
Birth Order			-0.042***	-0.033***	-0.028***
			(0.002)	(0.002)	(0.002)
Existing Sex Ratio of Children			-0.029***	-0.025***	-0.023***
			(0.007)	(0.007)	(0.007)
Mother's Age				0.002**	0.156***
				(0.001)	(0.010)
Mother's Education				0.087***	0.196***
				(0.003)	(0.012)
Family Wealth is in 2nd Quintile					0.001
					(0.001)
Family Wealth is in 3rd Quintile					0.058***
					(0.004)
Family Wealth is in 4th Quintile					0.064***
					(0.009)
Family Wealth is in 5th Quintile					0.115***
					(0.009)
Constant	0.679***	0.939***	1.100***	0.968***	0.916***
	(0.005)	(0.011)	(0.020)	(0.024)	(0.025)
State Fixed Effects	No	Yes	Yes	Yes	Yes
Year Fixed Effects	No	Yes	Yes	Yes	Yes
Birth Year Fixed Effects	No	No	Yes	Yes	Yes
Dummy Variable for Each HH Wealtl	No	No	No	No	Yes
Observations	32233	32233	32233	32233	32233
R-squared	0.000	0.250	0.276	0.300	0.311

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Notes: Sample is restricted to most recent birth of ever married women (ages 15-49) within 1 year previous to the survey. Days took iron supplements is available only for the 2004 survey. Existing sex ratio is defined as the ratio of boys to the total number of births prior to the most recent one. National sample weights are used in all regressions.

APPENDIX TABLE 4. Sex-Selective Prenatal Care in Other Countries

	Outcome				
Coefficient on Male in various countries	Prenatal Care (1=Yes, 0=No)	Number of Prenatal visits	Tetanus Shot (1=Yes, 0=No)	Number of Tetanus Shots	Non-Home Delivery (1=Yes, 0=No)
	(1)	(2)	(3)	(4)	(5)
China	0.046*	0.346*	na		na
	(0.027)	(0.205)			
Bangladesh	0.003	0.076**	0.028***	0.039*	0.001
	(0.009)	(0.037)	(0.009)	(0.021)	(0.003)
Pakistan	0.018	0.184*	0.020	0.016	0.006
	(0.015)	(0.100)	(0.016)	(0.039)	(0.014)
Pakistan (Punjab Region)	0.019	0.268*	0.015	0.014	0.026
	(0.021)	(0.152)	(0.023)	(0.056)	(0.020)
Sri Lanka	0.002	na	0.010	na	0.014
	(0.008)		(0.016)		(0.014)
Thailand	0.005	na	0.020	na	0.014
	(0.017)		(0.022)		(0.018)
Ghana	-0.013**	0.010	0.004	0.003	0.003
	(0.006)	(0.078)	(0.009)	(0.024)	(0.010)

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Notes: Sample is restricted to most recent birth of ever married women (under the age of 52 in China and ages 13-49 in Bangladesh) within 5 years previous to the survey. Tetanus information and wealth index is not available for China. Wherever available, controls include: state fixed effects, birth year fixed effects, survey year fixed effects, mother's age, mother's education, dummy for urban, birth order, and existing sex ratio of children (defined as the ratio of boys to the total number of births prior to the most recent one). National sample weights are used in all regressions. Due to the One Child Policy in China, we do not control for existing sex ratio and we include pregnancy number rather than birth order.