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 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 address other mechanisms such as selective recall, medical complications that might cause male fetuses to receive greater prenatal care in general, son preference-based fertility stopping rules and biases due to sex-selective abortions. Our calculations suggest that sex-selective prenatal care in maternal tetanus vaccination explains between 2.6–7.2 percent of excess female neonatal mortality in India.

I. Introduction

Sex-based discrimination has been studied extensively in the context of son preference in South and Southeast Asia (Drèze and Sen 1989; Gupta 1987; Qian 2008). Differential care favoring boys over girls and sex-selective abortions have

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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).

A large literature has tried to explain the skewed gender ratios through postbirth 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 2011), 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 (Pörtner 2010; 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 fetus 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 percent decrease in the probability of neonatal mortality (NFHS). Tetanus shots taken during pregnancy play a particularly important role in neonatal survival.[2] 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). About 38 percent of child (younger than five years) deaths occur in the neonatal stage; moreover, prenatal care is highly correlated with postnatal care such as breastfeeding and immunizations (NFHS), indicating that discrimination faced in utero persists and perhaps accumulates even after birth. Early childhood health notwithstanding, 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 women's prenatal health care choices when they are pregnant with boys relative to when they are pregnant with girls. In India, women are 1.8 percentage points (3 percent over the mean) more likely to attend prenatal care at least twice when pregnant

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 four 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 percent. Other estimates from developing countries range from 70 percent in rural Bangladesh to 88 percent 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.

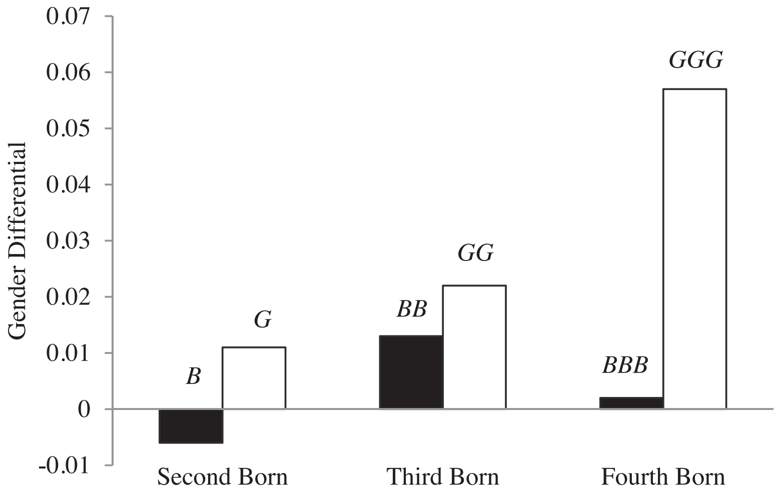

Figure 1
Gender Differentials in Prenatal Care, by Birth Parity and Sex Composition of Previous Children

Figure 1 illustrates the differential in the likelihood that mothers attend a prenatal check-up when pregnant with a boy versus when pregnant with a girl for each birth parity and given the sex composition of the previous children. For example, *B* represents the differential between boys and girls born into a family with 1 existing son, while *G* represents the differential between boys and girls born into a family with 1 existing daughter; *BB* represents the differential between boys and girls born into a family with 2 existing sons, and so forth.

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 percent 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 percent more likely to deliver their baby in a nonhome 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 where sex discrimination has been documented. For example, in China women pregnant with boys are 4.6 percentage points more likely to seek prenatal care. Mothers in Pakistan are 2.6 percentage points more likely to take iron supplements and mothers in Bangladesh attend prenatal care 7 percent 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, etc., we propose 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 mitigate 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 policy has focused on trying to eliminate it entirely; 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.[5] Our calculations suggest preferential treatment in one such investment, tetanus shots, can explain 2.6–7.2 percent 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 (a version of reporting bias in this context), biological characteristics of male fetuses that may drive the need for additional prenatal care, son preference-based fertility stopping rules, and sex-selective abortions. We discuss the problems raised by each and our solution to these issues in great detail in the subsequent section. To the extent we are able to test for potential biases in our data, our results appear to not be driven by these concerns.

II. Methodology and Estimation Issues

Papers examining son preference in the United States have studied the role of gender bias in differences in prenatal care (Dahl and Moretti 2008; 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 son preference-based stopping rules, male-specific medical complications, sex-selective abortions or ultrasounds and other methods of sex determination, there should be no systematic reason to find that males receive greater prenatal care. This

4. More recently, Aizer and Cunha (2010) measure initial endowment as scores from the Bailey test administered to 8 month old babies.
5. This is also in line with a recent paper by Hu and Schlosser (2011), who find that in areas with greater sex-selective abortions, girls receive *better* care post birth. This is presumably due to the fact that only those parents who really want a girl have a girl in those areas. Thus, focusing solely on abolishing sex-selective abortions could lead to worse post birth outcomes for girls.

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.

A. Basic Specification

Our strategy is built on the premise that under equal treatment or lack of knowledge of fetal gender, the pregnancy's eventual gender outcome 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 checkups do not vary systematically by the gender of the child. Thus finding that antenatal visits, consumption of iron supplements, or 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:

$$(1) \quad C_{ihj} = \beta Male_{ihj} + \eta \mathbf{X}_{ihj} + D_j + \varepsilon_{ihj}$$

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, etc. $Male_{ihj}$ takes the value of one when the child is male. The questions are retrospective, so the woman is asked about type of prenatal care received 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, birth year fixed effects, household wealth quintile fixed effects, 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.[6] Several important identification issues emerge when following this approach. We now review each problem and our proposed solutions in detail.

B. 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 rules is that the probability the youngest child is male is increasing with 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

6. 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).

$$C_{ihj} = \beta_G Male_{ihj} + \gamma X_{ihj} + D_j + \nu_{ihj}$$

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 the above equation for families whose previous births are all male (call this β_M). Hence, β from Equation 1 should be interpreted as a weighted average 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.

controlling for birth order and the existing sex ratio of the child's siblings. Because the survey 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, the Barcellos, Carvalho, and Lleras-Muney (2010) methodology relies on the absence of sex-selective abortions. Nevertheless, we employ it as a 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.

C. 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.

1. Sample Selection

Because we only observe the gender and prenatal care of pregnancies resulting in live births, our sample omits those female fetuses that 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. Reverse Causality

The presence of sex-selective abortions also could 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. 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 National Family Health Survey or the Reproductive and Child Health Survey), 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 one 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 one prenatal visit (where we assume that they learned the sex of the child) and estimate the following regression:

$$(2)\quad Additional\ C_{ihj} = \beta Male_{ihj} + \eta \mathbf{X}_{ihj} + D_j + \varepsilon_{ihj}$$

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 extent sex-selective prenatal investments.

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 the 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 this type of bias, we control for various factors like wealth and education, which might be important determinants of sex-selective abortions. If abortions are costly, then including a control for family wealth is important, as wealthier families are both more likely to have a male child (by aborting female fetuses)[7] and better able to afford prenatal care.[8]

Apart from wealth and education, we use three additional variables to address the possibility that sex-selective abortions are driving our results. First, the data from the 1998–99 round of the National Family Health Survey (NFHS) allow us to observe the

7. While the daily agricultural wage in India was around 57 rupees/day in 1998–99 (and also in 2000–2001), 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.

8. According to Pörtner (2010) women with at least one boy and women with less than eight 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).

abortion history of the mother (unfortunately, this information is not available in other rounds of the NFHS). Both types of abortions (induced and spontaneous) are recorded separately in the data. While there are likely to be measurement issues in abortion reporting, these data allows us to consider the "fraction of pregnancies aborted" as a measure for the propensity to sex-selectively abort, the omitted variable of concern. Note that while the fraction of induced abortions positively predicts the likelihood of observing a male birth, the fraction of spontaneous abortions negatively predicts male births (in regressions these are not statistically significant). This is consistent with the notion that spontaneous abortions are biologically more likely to occur for male fetuses than for female fetuses, whereas induced abortions are more likely to reflect sex-selective abortions. However, if mothers with a high probability of committing sex-selective abortions systematically underreport induced abortions, our results are still somewhat biased. This is certainly a caveat while interpreting these results. The second additional variable we include captures income shocks, proxied by rainfall shocks (measured at the state level and recorded as a 30 percent deviation from the historical mean) in the estimation. If income shocks determine both parents' ability to control the gender of their child through sex-selective abortion (since abortion is an expensive procedure) and parents' ability to invest in prenatal care, then controlling for income shocks will help account for this source of omitted variable bias. The final variable we add is the gender of the first-born child. Families where the first-born child is male are less likely to pursue sex-selective abortions since the "need" for a boy is already filled (Pörtner 2010). Hence controlling for the gender of the first-born is yet another way to control for the likelihood of sex-selectively aborting in future pregnancies.

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 above. 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:

$$(3)\quad C_{ih} = \phi Male_{ih} + \eta \mathbf{X}_{ih} + M_h + \varepsilon_{ih}$$

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 one 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_{ih} captures mother fixed effects including time-invariant preferences for gender and prenatal care. 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 1 and the fixed effects specification provides an alternative way of examining the presence of selective prenatal care. A valid concern with this estimation strategy is that it treats mothers who have had a girl and then a boy and mothers who have had a boy and then a girl the same way. This is not true, however, if we believe fertility stopping rules to play an important role. To deal with endogenous spacing and bias due to fertility stopping rules in the mother effects specifications, we can condition on families where the first born is male. Conditional on the gender of the first born (specifically the first born being male), subsequent birth outcomes are free from spacing or stopping rule bias.

However, a 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. These concerns are discussed in more detail in the results section.

D. 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. A similar issue arises if parents who have boys selectively report more prenatal care due to a social desirability bias towards boys. To counter these potential selective recall and reporting concerns 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).[9] 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).[10] Thus, to tackle to issues of selective recall and reporting, we estimate 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 or report 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/reporting, 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.

Finally, if the social desirability bias for boys is stronger for some families, we would expect that these unobserved traits are constant within families. In this case, the

9. 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).

10. 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).

mother fixed effects specification we employ should mitigate any remaining concerns with bias arising from selective reporting.

E. 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 and RCH (Reproductive and Child Health Surveys) collect detailed data on pregnancy complications such as fatigue, night blindness, excessive bleeding, et cetera. Our concern would be mitigated if pregnancies that result in a male birth are not associated with significantly more complications than those resulting in a female birth.

III. 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 1998–99 and 2005–2006 rounds of the National Family Health Survey (NFHS). In addition, we use two rounds of the Reproductive and Child Health (RCH) surveys from India to replicate our basic results.[11] The RCH is a much larger database; however, we do not use it as the basis for our main results for two important reasons. First, the use of the 1992 NFHS is important in establishing that our results are not seen at a time when ultrasound use was not as prevalent—the RCH only has data starting in 1998. Second, the RCH does not appear to collect information on wealth quintiles which is an important control variable in this case as wealthier people are perhaps more likely to obtain sex-selective abortions. Regardless, we use the RCH to show that our results are not simply an artifact of using the NFHS.

The Bangladeshi sample draws from four waves of the Demographic and Health Survey (DHS), including the 1996–97, 1999–2000, 2004, and 2007 rounds. The Chinese data come from the China Health and Nutrition Survey (CHNS), an ongoing project that collects panel data from nine provinces. For this paper, we use the 1991, 1993, 1997, 2000, 2004, and 2006 rounds. Additional robustness checks use samples drawn from other DHS rounds in Pakistan (2006–2007), Ghana (1993, 1998, 2003, 2008), Sri Lanka (1987) and Thailand (1987). The NFHS, RCH, and all DHS rounds are comprised of nationally representative samples with respect to each country. Appendix Table A1 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 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 preg-

11. The RCH is a representative survey from India covering approximately 1000 households per district. For details, please see http://www.rchiips.org/.

nancy 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 data set 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 A2 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 five-year span leading up to the survey round. Appendix Table A2 indicates that about 55 percent of pregnancies are male in India. In countries with low or no son preference (Ghana, Sri Lanka, and Thailand), male pregnancies occur only 51 percent to 52 percent of the time; however, in countries with stronger son preference (China, Bangladesh, Pakistan), the ratio is generally higher, with 56 percent of Chinese pregnancies resulting in a 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, iron supplements taken during pregnancy, and whether the mother chose to deliver her child in a health facility or at home. Appendix Table A2 displays the summary statistics for these outcomes of interest and means of dependent variables are presented in each table. Prenatal care and receipt of tetanus shots is fairly common, occurring in about 72 percent and 78 percent of pregnancies in India, respectively. However, Indian women choose to give birth in a nonhome facility for only 35 percent of pregnancies. Online Appendix Table A1 details the loss in number of observations due to missing control variables or due to lack of questions appearing in certain surveys.

IV. Results

Table 1 estimates the simple specification as in Equation 1 with sequential addition of variables in Columns 1–5 using "at least two prenatal visits" as the dependent variable. We examine other dependent variables in subsequent tables. Since we rely on this dependent variable to deal with the possibility of sex-selective abortion via reverse causality (we do not have the timing of other variables for example), this is our main outcome variable of choice. The coefficient on male drops from 0.024 to 0.018 when all control variables are added. While this difference is not statistically significant, it is important to note that mother's characteristics and household wealth might be correlated with omitted variables like the propensity to sex-selectively abort, while also influencing the outcome variable. This is one reason

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.

Table 1
Prenatal Investments and Gender of the Child

	Dependent Variable: Mother Attends at Least Two Prenatal Checkup during Pregnancy (1=Yes, 0=No)								
	No Controls (1)	Geographic and Survey Controls (2)	Adding Child-level Controls (3)	Adding Mother-level Controls (4)	Adding Household-level Controls (5)	Northern States Only (6)	Majority Female Sample (7)	Ultrasound Sample (Pooled) (8)	Non-ultrasound Sample (Pooled) (9)
Male	0.024***	0.025***	0.023***	0.020***	0.018***	0.029***	0.021**	0.530**	0.117*
	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)	(0.011)	(0.008)	(0.223)	(0.061)
Urban		0.167***	0.149***	0.096***	0.044***	0.076***	0.041***	0.792**	0.341***
		(0.007)	(0.007)	(0.007)	(0.008)	(0.016)	(0.011)	(0.325)	(0.112)
Birth order			–0.045***	–0.038***	–0.033***	–0.035***	–0.029***	–0.158*	–0.153***
			(0.002)	(0.002)	(0.002)	(0.004)	(0.004)	(0.095)	(0.025)
Existing sex ratio of children			–0.030***	–0.026***	–0.023***	–0.010	–0.086**	–0.000	–0.062
			(0.007)	(0.007)	(0.007)	(0.014)	(0.042)	(0.259)	(0.082)
Mother's age				0.004***	0.003***	0.006***	0.004***	0.029	0.029***
				(0.001)	(0.001)	(0.001)	(0.001)	(0.028)	(0.009)
Mother's education				0.099***	0.067***	0.071***	0.067***	0.433***	0.220***
				(0.003)	(0.004)	(0.008)	(0.006)	(0.127)	(0.045)

Family wealth is in second quintile					0.052***	0.070***	0.060***	–0.605	0.057
					(0.009)	(0.019)	(0.013)	(0.411)	(0.083)
Family wealth is in third quintile					0.110***	0.115***	0.114***	0.157	0.251***
					(0.009)	(0.020)	(0.014)	(0.414)	(0.093)
Family wealth is in fourth quintile					0.162***	0.228***	0.163***	0.413	0.444***
					(0.010)	(0.021)	(0.016)	(0.439)	(0.116)
Family wealth is in fifth quintile					0.215***	0.292***	0.213***	0.896*	0.878***
					(0.012)	(0.025)	(0.018)	(0.482)	(0.195)
Constant	0.597***	0.779***	1.059***	0.828***	0.738***	0.477***	0.334***	3.184***	2.388***
	(0.005)	(0.012)	(0.174)	(0.166)	(0.171)	(0.058)	(0.039)	(1.086)	(0.300)
Mean of dependent variable	0.610	0.610	0.610	0.610	0.610	0.521	0.628	0.959	0.868
State fixed effects	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Birth year fixed effects	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dummy variable for each household wealth quintile	No	No	No	No	Yes	Yes	Yes	Yes	Yes
Observations	32,012	32,012	32,012	32,012	32,012	8,304	14,302	5,171	16,870
R-squared	0.001	0.269	0.294	0.324	0.335	0.314	0.336	—	—

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 five 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. National sample weights are used in all regressions. Columns 8 and 9 show logit coefficients.

why the addition of these control variables influences the magnitude of the coefficient on male.[13]

When we restrict the analysis to the northern region of India in Column 6 (Punjab, Haryana, Himachal Pradesh, Uttar Pradesh, and Rajasthan), we see a much larger magnitude of discrimination; mothers pregnant with boys in North Indian states are nearly three percentage points more likely to attend prenatal care at least twice (as opposed to 1.8 percentage points for the country as a whole). 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).[14] 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 (comparing Columns 7 and 5). 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 that mothers pregnant with a boy are 2.1 percentage points more likely to attend prenatal care two times or more (also see Figure 1). Hence, for India, we find strong, consistent evidence that women utilize more prenatal care when pregnant with a boy than when they are pregnant with a girl.

Columns 8 and 9 break the sample up by ultrasound receipt. Because receipt of ultrasound is important to understand the results, we examine this in detail in Table 2, along with another prenatal care outcome of tetanus shots. As mentioned earlier, we have ultrasound receipt information for a subset of the Indian sample. While the 2005–2006 round asks about ultrasound usage during each pregnancy in the past five years, the 1998–99 round only asks about ultrasound usage among the sample of women who had at least one prenatal checkup. 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 one prenatal checkup. Within this sample, we examine whether mothers pregnant with males and receiving ultrasounds 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 percent) 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 women pregnant with 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

13. The differences in coefficient sizes across specifications in Columns 1–5 reflect only changes due to adding covariates and not changes sample size, as the sample is held constant across these specifications.

14. A concern might be that the northern states are the states also with the greatest proportion of sex-selective abortions. To deal with the spatial diffusion of ultrasound technology and sex-selective abortions, we examine states with lower than median and lower than the 75th percentile in the *growth* of male gender ratios from 1992–2005. Even for these samples we find a statistically significant gender gap (results not shown, available upon request).

Table 2a

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

	Full Sample				Northern Region				Majority Female			
	At Least Two Antenatal Visits		Tetanus Shot (1=Yes,0=No)		At Least Two Antenatal Visits		Tetanus Shot (1=Yes,0=No)		At Least Two Antenatal Visits		Tetanus Shot (1=Yes,0=No)	
Panel A, 1998 and 2005 Survey data, restricted to women who had at least one prenatal check up	With Ultrasound (1)	Without Ultrasound (2)	With Ultrasound (3)	Without Ultrasound (4)	With Ultrasound (5)	Without Ultrasound (6)	With Ultrasound (7)	Without Ultrasound (8)	With Ultrasound (9)	Without Ultrasound (10)	With Ultrasound (11)	Without Ultrasound (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	5,171	16,870	5,970	16,891	1,605	3,772	1,542	3,785	2,226	7,522	2,541	7,542

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 five years previous to the 2005–2006 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 2b

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

	Full Sample				Northern Region				Majority Female			
	At Least Two Antenatal Visits		Tetanus Shot (1=Yes,0=No)		At Least Two 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 (1)	Without Ultrasound (2)	With Ultrasound (3)	Without Ultrasound (4)	With Ultrasound (5)	Without Ultrasound (6)	With Ultrasound (7)	Without Ultrasound (8)	With Ultrasound (9)	Without Ultrasound (10)	With Ultrasound (11)	Without Ultrasound (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	4,557	13,138	4,759	13,164	1,240	3,035	1,249	3,044	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 five years previous to the 2005–2006 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).

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 2005–2006 survey (because ultrasound information was asked of everyone, not just mothers who had a prenatal checkup). 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 percent). 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 are 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 with using the ultrasound data. First, the ultrasound variable is likely to 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 percent and 27 percent reported in the 1998–99 and 2005–2006 rounds of the NFHS, respectively.

Next, we explore whether our results hold when we examine various subsamples to account for behaviors like son preference-based stopping rules, sex-selective abortion, selective recall, and medical complications. Using the same outcome variable as in Table 1 (more than two prenatal checkups), we first examine whether son preference stopping rules might bias our results. As explained in Section II, following Barcellos, Carvalho, and Lleras-Muney (2010) we restrict the sample to families where the youngest child is less than two years old at the time of the survey to minimize the bias due to families adjusting their fertility after realizing the sex of the child. Column 1 of Table 3 shows that even under this restriction, we see a gender gap in prenatal care outcomes. Under a more severe restriction of examining children younger than one, we still find the presence of a gender gap, and in fact the magnitudes are larger for this subsample (although not statistically different from the magnitudes observed for the larger population). One drawback of this method is that it relies on the assumption of no sex-selective abortions. We can examine these age cutoffs further in samples where we deal with sex-selective abortions and show that our results still hold (Online Appendix Table A8).

We then examine whether sex-selective abortions might be driving the results.

Table 3
Accounting for Stopping Rules and Reverse Causality Due to Sex-Selective Abortions

	Dependent Variable: Mother Attends at Least Two Prenatal Checkups (1=Yes, 0=No)				
	Children Ages Two Years or Less (1)	Children Ages One Year or Less (2)	Conditional on at Least One Visit (3)	Conditional on at Least One Visit (First Visit in Final Five Months of Pregnancy) (4)	Mother Fixed Effects (5)
Male	0.022***	0.030***	0.013**	0.027**	0.029***
	(0.008)	(0.011)	(0.005)	(0.011)	(0.010)
Mean of dependent variable	0.602	0.597	0.889	0.813	0.593
State fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
birth year fixed effects	Yes	Yes	Yes	Yes	Yes
Dummy variable for each household wealth quintile	Yes	Yes	Yes	Yes	Yes
Observations	18,058	9,247	22,983	7,547	2,692
R-squared	0.337	0.338	0.091	0.082	0.017

Robust standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$
Notes: With the exception of the age-restricted Columns 1 and 2 and the fixed effects regression (Column 5), the sample is restricted to most recent birth of ever-married women (ages 15–49) within five 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. National sample weights are used in all regressions. 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).

There are two main issues when examining sex-selective abortions in this context. First, to deal with the possibility of reverse causality, we look for a gender gap in prenatal care *after* an initial visit to prenatal care is completed. If mothers learn the sex of their child during the first prenatal visit and choose to abort female fetuses, then we would find a mechanical correlation between prenatal care visits (particularly in the case of greater than two prenatal care visits) and sex of the child due to sex-selective abortions, rather than due to sex-selective prenatal care. Column 3 of Table 3 shows that even in this sample (in which reverse causation is highly unlikely) there remains a sizable 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.[15] If we consider the coefficient on male from Table 1 Column 5 to include a "sex-selective abortion" effect and a "gender-discrimination effect," then the results from Table 3, Column 3 likely reflects just the discrimination effect. Unfortunately, the results from Table 3, Column 3 and 4 do not provide a clear direction regarding the sign of the bias due to sex-selective abortion. The larger point however, is that these results are not entirely different from the results for the main sample. Hence, it would appear that reverse causality is not a big concern here.

If we consider the factors or characteristics correlated with sex-selective abortions to be time-invariant, then a mother fixed effects approach is another way of dealing with omitted variables bias (see Section II for a discussion on this). Table 3, Column 5 estimates the fixed effects specification in Equation 3 for India where we have data on the previous two births of the mothers. We find similarly consistent results with this specification. 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). For the same sample, however, OLS estimates yield similar results suggesting the role of mother level unobservables to be quite small. Mothers are 2.9 percentage points more likely to visit prenatal care at least twice when pregnant with a boy. An issue with the mother fixed effects estimates is that spacing might be endogenous and is precisely time-varying. Thus, the mother fixed effects results might still be biased. To account for endogenous spacing issues, we conducted tests where we restricted the sample to mothers where the first born was male and our results hold even for this sample (results not shown, available upon request).

An important caveat here is that the sample size is quite small. Moreover, the sample consists of mothers who gave birth twice in the five years prior to the survey *and* have children of differing gender. Hence, we urge some caution while extrapolating these results to the general population.[16]

We also use three additional control variables to examine the possibility of sex-

15. We also show that the results hold when we change the outcome variable to "at least three prenatal visits" or "at least four prenatal visits", et cetera. (Online Appendix Table 9).

16. Statistical test reveal, for example, that this sample is richer and more educated than the full sample. A further caveat about mother fixed effects results is that we do not control for the sex ratio of previous children. Since the sex ratio variable is only defined for birth orders two and above, including this variable forces us to

Table 4
Accounting for Sex-selective Abortions

	Dependent Variable: At Least Two Prenatal Visits (1=Yes; 0=No)				
	(1)	(2)	(3)	(4)	(5)
Male	0.029***	0.028***	0.028***	0.028***	0.028***
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
Proportion of all aborted pregnancies	0.065*				0.053
	(0.035)				(0.044)
Proportion of induced aborted pregnancies		0.079			0.027
		(0.060)			(0.081)
Negative rain shock (year of conception)			0.006		0.006
			(0.015)		(0.015)
Firstborn child was male				0.006	0.006
				(0.013)	(0.014)
Mean of dependent variable	0.573	0.573	0.565	0.573	0.565
State fixed effects	Yes	Yes	Yes	Yes	Yes
Survey year fixed effects	Yes	Yes	Yes	Yes	Yes
Birth year fixed effects	Yes	Yes	Yes	Yes	Yes
Dummy for each wealth quintile	Yes	Yes	Yes	Yes	Yes
Observations	13,877	13,877	10,363	13,877	10,363
R-squared	0.379	0.379	0.385	0.379	0.385

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 five years previous to the 1998–99 survey only. Proportion of all aborted pregnancies is defined as total abortions/total pregnancies. Proportion of induced aborted pregnancies is defined as total induced abortions/total pregnancies. Negative rain shock is equal to one if the yearly rainfall at the state level is more than 30 percent above or below the 20-year state-specific average during the year of conception. Rainfall information not available for all states. Other controls include 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.

selective abortions driving our results. These controls are the fraction of pregnancies ending in an abortion, rainfall shocks and gender of the first born child. Table 4 shows the results for outcome variable of at least two prenatal visits as in the previous tables. Columns 1 through 5 show that the addition of these controls does not alter the main findings.[17] The data used for this table only uses the 1998–99 survey round as questions related to abortions were not asked in subsequent rounds of the NFHS.

In Table 5, we examine whether our results suffer from recall bias. If mothers

examine an even smaller sample. For consistency, including this variable does not change the import of our results. However, for some of the outcome variables examined, we lose precision of the estimates.

17. We examine other prenatal care outcomes with these additional control variables in Appendix Table A2.

Table 5
Sex-Selective Prenatal Investments in India: Pre-ultrasound Period (births in 1992 and earlier)

	At Least Two Visits (1=Yes, 0=No) (1)	Prenatal Care (1=Yes, 0=No) (2)	Number of Prenatal Visits (3)	Tetanus Shot (1=Yes, 0=No) (4)	Number of Tetanus Shots (5)	Iron Pills (1=Yes, 0=No) (6)	Nonhome Delivery (1=Yes, 0=No) (7)
Male	0.003	0.005	0.046	0.008	0.027	0.017**	0.004
	(0.008)	(0.008)	(0.033)	(0.008)	(0.020)	(0.008)	(0.006)
Mean of dependent variable	0.549	0.457	2.077	0.598	1.354	0.509	0.189
Observations	16,998	16,970	16,130	16,879	16,919	16,957	16,998
R-squared	0.301	0.279	371	0.263	0.249	0.264	0.311

Robust standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$
Notes: Sample is restricted to most recent birth occurring within five years previous to the 1992 survey round by ever-married women (ages 15–49). Controls include state fixed effects, birth year fixed effects, survey year fixed effects, mother's age, mother's education, dummy for urban, birth order, dummies for wealth quintiles and existing sex ratio of children (defined as the ratio of boys to the total number of births prior to the most recent one). All regressions include national sample weights. Number of Prenatal visits is trimmed at the 95 percent level.

Table 6
Sex-Selective Prenatal Investments in India: Timing of Prenatal Care

	Prenatal Care Received Within First Four Months of Pregnancy (1=Yes, 0=No) (1)	Prenatal Care Received After First Four Months of Pregnancy (1=Yes, 0=No) (2)
Male	0.007	0.013**
	(0.006)	(0.006)
Mean of dependent variable	0.659	0.435
Observations	32,233	32,233
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 five years previous to survey. Receiving prenatal care after four months of pregnancy is defined as one 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.

simply report having taken better prenatal care for males due to some sort of recall bias, then results using data from a time when ultrasounds were not widespread should also show males receiving greater prenatal care. For a wide range of prenatal care outcomes, we show that there does not appear to be a gender gap for births occurring between 1987 and 1992. If we test for an overall gender gap in prenatal care using an aggregated measure across all binary outcome measures (following Kling, Liebman, and Katz 2007), we find that males born in 1992 and earlier are 0.004 percentage points *less* likely to receive any care although this aggregate effect is not statistically significant (p-value 0.677). 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 6 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. Here we assume that the sex of the fetus is not known during the first four months of pregnancy, however, the findings presented in this table are robust to a range of different timing assumptions. For example, fetal gender does not predict prenatal care within the first two or three months, when it is extremely unlikely that a mother knows the gender of her baby. Thus we believe that the existence of selective recall cannot explain this pattern of discrimination in our results, even within the same pregnancy.[18]

Finally, 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 discrimination. In Table 7 we estimate whether being pregnant with a boy is significantly related to complications during pregnancy in India.[19] 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.[20]

A. Evidence from Other Countries

Since the DHS collects extensive prenatal care data, we can extend our analysis to other countries in South and Southeast Asia.[21] We estimate Equation 1 for China, Bangladesh, and Pakistan. These are countries where son preference and gender discrimination has been well established in previous studies (Gupta et al. 2003). We find that the gender bias in prenatal care is not limited to India but is 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). Finally, we investigate whether sex-selective prenatal care is practiced in Ghana, a country with no known son preference (Garg and Morduch 1998).

A caveat while interpreting these results is that, with the exception of China, we were not able to obtain very detailed information on aspects such as access to ultrasound, or the extent of sex-selective abortion (the Chinese case is well documented in Meng 2010). In the case of Pakistan, while sociological surveys by Zubair et al. (2006) suggest that the extent of sex-selective abortions in Pakistan is quite low, Miller (2001) suggests the opposite. However, both papers suggest rather widespread access and use of ultrasound technology beginning in the mid-1990s for the use of fetal sex

18. While we lack data on the timing of prenatal visits after the first, the outcome for Column 2 of Table 6 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.

19. Appendix Table A6 replicates these estimates using the RCH. The results are very similar.

20. A concern might be that if women carrying male fetuses *do* need greater prenatal care, then perhaps Table 7 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.

21. 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 focused on a part of the world where this is most relevant.

Table 7
Gender and Pregnancy Complications in India

	Night Blindness (1=Yes, 0=No) (1)	Blurred Vision (1=Yes, 0=No) (2)	Convulsions (1=Yes, 0=No) (3)	Swelling (1=Yes, 0=No) (4)	Fatigue (1=Yes, 0=No) (5)	Anemia (1=Yes, 0=No) (6)	Excessive Bleeding (1=Yes, 0=No) (7)	Any Complication (1=Yes, 0=No) (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	32,225	32,236	32,225	32,237	32,236	13,911	32,217	32,252
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 five years previous to the 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.

determination. We were unable to get more detailed information for other countries in our sample.

The first four rows of Table 8 display the results of estimating Equation 1 for countries that are known to have son preference: China, Bangladesh, and Pakistan (both the full sample and the region of Punjab).[22] Overall, the results from these samples exhibit patterns consistent with sex-selective discrimination in prenatal care. In China, women are 4.6 percentage points more likely to get some prenatal care when pregnant with a boy, and visit antenatal clinics nearly 10 percent more frequently (relative to the mean). 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 percent more frequently when pregnant with a boy. In Pakistan, we find that women visit prenatal clinics more often and are 2.6 percentage points more likely to consume iron pills when pregnant with a boy. In Pakistani 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 4.8 percentage points more likely to take iron pills. Taken all together, the evidence in Table 8 implies that the practice of sex-selective prenatal investments extends beyond India and is widespread across areas with well documented son preference.

Finally, we estimate Equation 1 for countries with no (or at least lesser) established son preference. The last three rows of Table 8 displays the estimates for Sri Lanka, Thailand, and Ghana. 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.

B. Other Outcome Variables and Additional Robustness Checks

In Appendix Tables A3–A5 and Online Appendix Tables A2–A4 we examine whether males are more likely to get at least one prenatal visit, whether they get more prenatal visits (number of prenatal visits), whether mothers are more likely to take a tetanus shot while pregnant with a male, the number of tetanus shots the mother takes while pregnant with a male, and whether the delivery took place at home. The various columns in each of the table tackle each of the concerns we listed in Section II. Taken together, the results suggest that males get better prenatal care. If we aggregate the effects across all of the binary measures of prenatal care (tetanus shot receipt, prenatal visit, iron pill use, and delivery in a nonhome facility), we find that males are 1.6 percent more likely to receive care and that this gender gap in the aggregated measure of care is statistically significant (p-value 0.011).[23]

As mentioned in Section III, we can make use of a larger sample containing infor-

22. Note that not all outcomes are available for China.

23. Our methodology follows Kling, Liebman, and Katz (2007) to aggregate across the four outcomes: tetanus shot receipt, prenatal checkup, iron pill use, and delivery in a nonhome facility. Results using iron pill use as the outcome variable are not shown: most of these effects are small and statistically insignificant. Because all of these outcomes are binary, the aggregate measure is not normalized but instead is a simple unweighted mean of the coefficient on the male dummy variable.

Table 8
Sex-Selective Prenatal Care in Other Countries

	Dependent Variable					
Coefficient on Male in various countries	Prenatal Care (1=Yes, 0=No) (1)	Number of Prenatal visits (2)	Tetanus Shot (1=Yes, 0=No) (3)	Number of Tetanus Shots (4)	Iron Pills (1=Yes, 0=No) (5)	Nonhome Delivery (1=Yes, 0=No) (6)
China	0.046*	0.346*	na	na	na	na
	(0.027)	(0.205)				
Bangladesh	0.003	0.076**	0.028***	0.039*	na	0.001
	(0.009)	(0.037)	(0.009)	(0.021)		(0.003)
Pakistan	0.018	0.184*	0.020	0.016	0.026*	0.006
	(0.015)	(0.100)	(0.016)	(0.039)	(0.015)	(0.014)
Pakistan (Punjab region)	0.019	0.268*	0.015	0.014	0.048**	0.026
	(0.021)	(0.152)	(0.023)	(0.056)	(0.022)	(0.020)
Sri Lanka	0.002	na	0.010	na	na	0.014
	(0.008)		(0.016)			(0.014)
Thailand	0.005	na	0.020	na	na	0.014
	(0.017)		(0.022)			(0.018)
Ghana	–0.013**	0.010	0.004	0.003	0.005	0.003
	(0.006)	(0.078)	(0.009)	(0.024)	(0.010)	(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 (younger than 52 in China and ages 13–49 in Bangladesh) within five 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.

mation on prenatal care in the RCH. The RCH, however, is not as rich as the NFHS, and thus in Appendix Table A6 we replicate estimations following the full sample specifications in Table 1 Column 5, 6 and 7. Our basic results hold when we use this data set. Online Appendix Table A5 shows the results for complications during pregnancy and its correlation with a male birth using the RCH. Again, it appears that instances where a woman is pregnant with a boy are not more likely to result in more complications.

C. 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 section, 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. 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 percent for girls. Female neonatal mortality rate in Ghana and Italy is around 1.93 percent. Using the sex ratio in neonatal mortality from these countries (because they are presumed to be free of son preference), we impute a neonatal mortality rate for women in India to be around 1.94 percent.[24] Thus excess female neonatal mortality—the amount that the rate exceeds what we expect under equal treatment—is 0.31 percentage points in India.[25]

Our estimates from Appendix Table A4 suggest that males are 1.1 percent 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 98.9 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 percent of all mothers pregnant with girls receive tetanus shots, the implied neonatal mortality rate for those whose mothers received the shots is 1.6 percent and 4.85 percent for those whose mothers did not.

This means that had the 1.6 girls that did not receive tetanus shots actually received one, 0.008 more girls would have survived than in the case of differential treatment. Hence, unequal allocation of tetanus shots can explain around 2.58–2.67 percent of

24. Ulizzi and Zonta (2002) find that the sex ratio in neonatal deaths is 0.59. Given that we observe 958 neonatal deaths among boys in our sample, the natural rate for girls would be 1.94 percent 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, in other words 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 percent for girls.

25. Please see the Appendix for details on all calculations in this section.

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 2.9 percent more likely to receive tetanus shots (from Appendix Table A4, the mother fixed effects column), we conclude that unequal allocation of tetanus immunizations can explain around 7.0–7.2 percent 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 2.6–7.2 percent of the excess female mortality in the neonatal period.[26]

V. Conclusion

This paper examines whether preference for sons in India leads parents to differentially invest in their unborn children. We find evidence that parents invest in greater prenatal care when pregnant with a boy. We largely 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 technologies. Hence, the weight of the evidence points towards gender discrimination in prenatal investments. In addition, 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 alone should decrease this gap by 2.6–7.2 percent. In reality, prenatal care is most often multidimensional in nature and women who seek tetanus shots are likely to receive other types of care as well (even within the same visit), further improving health outcomes for their unborn children. If we knew the causal effects of bundled prenatal care on neonatal and infant mortality, we would be able to explain a greater proportion of excess female mortality.

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. 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 infants who receive some prenatal care are better off in terms of lower mortality and higher birth weight.[27] 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 findings from the vast literature linking early childhood health (such as birth weight) and later life outcomes, our results imply that effect of gender

26. 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 percent of excess female neonatal mortality.
27. These correlations from the NFHS data are available upon request.

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 these relationships in developing countries with son preference must seriously consider the possibility that parents differentially invest based on the sex of their unborn child.

Appendix 1

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 Ghanaian 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), female neonatal mortality rate is 1.93 percent. When we use the results of a study in Italy (Ulizzi and Zonta 2002) we impute a natural rate of 1.94 percent; 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.[28] The neonatal mortality rate is 2.24 percent 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 percent 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.[29] As mentioned before, the neonatal mortality rate is 2.24 percent among girls in the Indian sample. Since 80.3 percent 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 percent for female children born to women who received tetanus shots and 4.85 percent for those whose mothers did not.

Our estimates in Appendix Table A4 show that women are 1.1 percent less likely

28. Ulizzi and Zonta (2002) find that the sex ratio in neonatal deaths is 0.59. Given that we observe 958 neonatal deaths among boys in our sample, the natural rate for girls would be 1.94 percent 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 $958/(958+x) = 0.590$, in other words 665.7 deaths. Because we have 34,239 female births in our sample, this implies a natural neonatal mortality rate of 665.7/34,239 = 1.94 percent for girls.

29. We consider this to be a conservative measure, as Blencowe et al. (2010) find a 94 percent reduction in neonatal tetanus when mothers are immunized.

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 98.9 girls do. If mothers were equally likely to receive tetanus shots (regardless of fetal gender) then the remaining 1.1 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 percent of neonatal deaths are due to tetanus in India (UNICEF 2000) and the neonatal mortality rate is 1.6 percent (calculated above).[30] Under differential treatment, where 1.6 girls are born to mothers who have not had tetanus shots, $0.23((1.1)0.0485 + (100 - 1.1)0.016) = 0.376$ girls die per 100 because the 1.1 girls without tetanus immunity face a higher mortality rate of 4.85 percent (calculated above). Thus, the difference in tetanus shots leads to a difference in observed neonatal mortality of $0.376 - 0.368 = 0.008$ deaths per 100 girls.

Therefore, the gender gap in tetanus shots can explain $0.004/0.31 = 2.58$ percent of excess female neonatal deaths in India (or 2.67 percent 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 Appendix Table A4) we find that differential tetanus treatment accounts for 7.23 percent of the gap between the natural and observed rates of neonatal mortality (7.0 percent using the imputed rate from Italy). Hence we believe that the gender bias in prenatal tetanus immunizations can explain 2.6–7.2 percent of excess female neonatal mortality.

30. Again, this is likely to be a conservative estimate; Gupta and Keyl (1998) find that tetanus accounts for 23–73 percent of all neonatal deaths.

Appendix Table A1
Description of Regression Samples

	Country						
	India	China	Bangladesh	Pakistan	Ghana	Sri Lanka	Thailand
Survey years	(1992–93), 1998–99, 2005–2006	1991, 1993, 1997, 2000, 2004, 2006	1996–97, 1999–2000, 2004	2006–2007	1993, 1998, 2003, 2008	1987	1987
Birth years	1995–2006	1989–2006	1991–2007	2001–2007	1988–2008	1982–87	1982–87
Number of observations	36,755	1,482	15,916	5,063	14,290	2,190	1,986
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 A2
Summary Statistics for India

	All		Male		Female	
	Mean (Standard Deviation)	Observations	Mean (Standard Deviation)	Observations	Mean (Standard Deviation)	Observations
Mother characteristics						
age	28.10	36,755	28.10	20,041	28.10	16,714
	(5.52)		(5.46)		(5.58)	
Education	0.90	36,755	0.921	20,041	0.879	16,714
	(1.00)		(1.00)		(0.996)	
National wealth quintile	2.95	36,755	2.99	20,041	2.90	16,714
	(1.37)		(1.37)		(1.37)	
Pregnancy characteristics						
male	0.55	36,755				
Birth order	3.48	36,755	3.46	20,041	3.50	16,714
	(1.78)		(1.76)		(1.79)	

Existing sex ratio of children	0.49	36,755	0.47	20,041	0.51	16,714
	(0.39)		(0.39)		(0.39)	
Ultrasound receipt (1=Yes,0=No): 1998–99 only	0.14	9,140	0.14	4,986	0.13	4,154
Ultrasound receipt (1=Yes,0=No): 2004–2005 only	0.27	18,888	0.28	10,353	0.26	8,535
Prenatal care (1=Yes, 0=No)	0.72	32,233	0.73	17,503	0.71	14,730
Number of prenatal visits	2.72	30,774	3.16	17,377	3.01	14,635
	(2.59)		(3.20)		(3.13)	
Tetanus shot (1=Yes, 0=No)	0.78	32,017	0.79	17,376	0.77	14,641
Number of tetanus shots	1.64	32,017	1.66	17,376	1.61	14,641
	(1.03)		(1.02)		(1.04)	
Iron pills (1=Yes, 0=No)	0.61	32,166	0.62	17,458	0.61	14,708
Days took iron supplement	45.53	17,698	46.32	9,654	44.58	8,044
	(63.08)		(63.52)		(62.55)	
Nonhome delivery (1=Yes, 0=No)	0.35	31,073	0.36	16,869	0.33	14,204

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 five 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. Number of prenatal visits is trimmed at the 95 percent level.

Appendix Table A3
Sex-Selective Prenatal Investments in India: Any Prenatal Care

	Dependent Variable: Any Prenatal Care (1=Yes, 0=No)									
	No Controls (1)	Geographic and Survey Controls (2)	Adding Child-level Controls (3)	Adding Mother-level Controls (4)	Adding Household-level Controls (5)	Northern States Only (6)	Majority Female Sample (7)	Children Ages 2 Years or Less (8)	Children Ages 1 Year or Less (9)	Mother Fixed Effects (10)
Male	0.016**	0.017***	0.016***	0.013**	0.011**	0.028**	0.015*	0.014*	0.018*	0.027***
	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)	(0.011)	(0.008)	(0.007)	(0.010)	(0.010)
Urban		0.133***	0.116***	0.071***	0.026***	0.055***	0.032***	0.023**	0.037***	
		(0.007)	(0.007)	(0.007)	(0.007)	(0.015)	(0.010)	(0.010)	(0.014)	
Birth order			–0.042***	–0.033***	–0.028***	–0.025***	–0.027***	–0.028***	–0.028***	–0.005
			(0.002)	(0.002)	(0.002)	(0.005)	(0.004)	(0.003)	(0.005)	(0.023)
Existing sex ratio of children			–0.029***	–0.025***	–0.023***	–0.013	–0.084**	–0.022**	–0.035***	
			(0.007)	(0.007)	(0.007)	(0.014)	(0.042)	(0.009)	(0.012)	
Mother's age				0.002**	0.001	0.003**	0.002**	0.001	0.000	
				(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	
Mother's education				0.087***	0.058***	0.070***	0.055***	0.055***	0.056***	
				(0.003)	(0.004)	(0.007)	(0.005)	(0.005)	(0.007)	

Family wealth is in second quintile					0.064***	0.081***	0.070***	0.054***	0.034**	
					(0.009)	(0.020)	(0.013)	(0.012)	(0.016)	
Family wealth is in third quintile					0.115***	0.139***	0.116***	0.107***	0.094***	
					(0.009)	(0.020)	(0.014)	(0.012)	(0.017)	
Family wealth is in fourth quintile					0.156***	0.214***	0.156***	0.144***	0.124***	
					(0.010)	(0.021)	(0.015)	(0.013)	(0.018)	
Family wealth is in fifth quintile					0.196***	0.268***	0.187***	0.191***	0.174***	
					(0.012)	(0.024)	(0.017)	(0.015)	(0.021)	
Constant	0.679***	0.939***	1.100***	0.968***	0.916***	0.740***	0.820***	0.931***	1.119***	0.730***
	(0.005)	(0.011)	(0.020)	(0.024)	(0.025)	(0.116)	(0.034)	(0.030)	(0.059)	(0.082)
Mean of Dependent Variable	0.688	0.688	0.688	0.688	0.688	0.601	0.704	0.685	0.684	0.692
State fixed effects	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Birth year fixed effects	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dummy variable for each household wealth quintile	No	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Observations	32,233	32,233	32,233	32,233	32,233	8,369	14,413	18,149	9–290	2–692
R-squared	0–000	0.250	0–276	0–300	0.311	0–287	0–317	0–313	0–317	0–025

Robust standard errors in parentheses

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

Notes: With the exception of Columns 8–10, the sample is restricted to most recent birth of ever-married women (ages 15–49) within five 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. National sample weights are used in all regressions.

Appendix Table A4
Sex-Selective Prenatal Investments in India: Tetanus shot during pregnancy

	Dependent Variable: Any Prenatal Care (1=Yes, 0=No)									
	No Controls (1)	Geographic and Survey Controls (2)	Adding Child-level Controls (3)	Adding Mother-level Controls (4)	Adding Household-level Controls (5)	Northern States Only (6)	Majority Female Sample (7)	Children Ages 2 Years or Less (8)	Children Ages 1 Year or Less (9)	Mother Fixed Effects (10)
Male	0.018***	0.017***	0.015***	0.013**	0.011**	0.022*	0.017**	0.013*	0.014	0.029***
	(0.006)	(0.006)	(0.006)	(0.005)	(0.005)	(0.011)	(0.008)	(0.007)	(0.010)	(0.010)
Urban		0.103***	0.083***	0.043***	0.000	0.004	–0.003	–0.017*	0.000	
		(0.006)	(0.006)	(0.006)	(0.007)	(0.014)	(0.009)	(0.009)	(0.013)	
Birth order			–0.048***	–0.039***	–0.034***	–0.028***	–0.033***	–0.031***	–0.030***	–0.023
			(0.002)	(0.002)	(0.002)	(0.005)	(0.004)	(0.003)	(0.005)	(0.023)
Existing sex ratio of children			–0.034***	–0.030***	–0.028***	–0.013	0.015	–0.031***	–0.034***	
			(0.006)	(0.006)	(0.006)	(0.014)	(0.042)	(0.008)	(0.011)	
Mother’s age				0.001*	0.001	–0.000	0.002*	–0.000	–0.002	
				(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	
Mother’s education				0.079***	0.050***	0.059***	0.047***	0.051***	0.049***	
				(0.003)	(0.003)	(0.007)	(0.005)	(0.004)	(0.006)	

Family wealth is in second quintile					0.067***	0.065***	0.070***	0.055***	0.047***	
					(0.009)	(0.021)	(0.013)	(0.012)	(0.016)	
Family wealth is in third quintile					0.140***	0.178***	0.138***	0.133***	0.119***	
					(0.009)	(0.021)	(0.013)	(0.012)	(0.017)	
Family wealth is in fourth quintile					0.178***	0.257***	0.170***	0.167***	0.149***	
					(0.009)	(0.021)	(0.014)	(0.012)	(0.017)	
Family Wealth is in fifth Quintile					0.185***	0.280***	0.179***	0.189***	0.173***	
					(0.011)	(0.023)	(0.016)	(0.014)	(0.020)	
Constant	0.768***	0.909***	1.081***	0.970***	0.903***	0.736***	0.809***	0.909***	0.939***	0.782***
	(0.004)	(0.012)	(0.020)	(0.024)	(0.025)	(0.057)	(0.035)	(0.028)	(0.038)	(0.044)
Mean of dependent variable	0.777	0.777	0.777	0.777	0.777	0.685	0.793	0.776	0.775	0.764
State fixed effects	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Birth year fixed effects	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dummy variable for each household Wealth quintile	No	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Observations	32,017	32,017	32,017	32,017	32,017	8,324	14,321	18,061	9,260	2,684
R-squared	0.000	0.109	0.149	0.173	0.191	0.219	0.186	0.186	0.181	0.012

Robust standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$
Notes: With the exception of Columns 8 - 10, the sample is restricted to most recent birth of ever-married women (ages 15–49) within five 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. National sample weights are used in all regressions.

Appendix Table A5
Sex-Selective Prenatal Investments in India: Nonhome Delivery

	Dependent Variable: Nonhome Delivery (1=Yes, 0=No)						
	No Controls (1)	Geographic and Survey Controls (2)	Adding Child-level Controls (3)	Adding Mother-level Controls (4)	Adding Household-level Controls (5)	Northern States Only (6)	Majority Female Sample (7)
Male	0.025***	0.025***	0.024***	0.019***	0.017***	0.038***	0.007
	(0.007)	(0.006)	(0.006)	(0.005)	(0.005)	(0.009)	(0.008)
Constant	0.298***	0.489***	0.604***	0.240***	0.210***	–0.012	0.208***
	(0.005)	(0.018)	(0.022)	(0.026)	(0.026)	(0.043)	(0.039)
Mean of dependent variable	0.312	0.312	0.312	0.312	0.312	0.236	0.324
State fixed effects	No	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	No	Yes	Yes	Yes	Yes	Yes	Yes
Birth year fixed effects	No	No	Yes	Yes	Yes	Yes	Yes
Dummy variable for each household wealth quintile	No	No	No	No	Yes	Yes	Yes
Observations	31,073	31,073	31,073	31,073	31,073	8,106	13,941
R-squared	0.001	0.268	0.287	0.349	0.374	0.288	0.371

Male	0.049***	0.007	0.020***	0.011	0.018***	0.018	0.010
	(0.013)	(0.008)	(0.007)	(0.010)	(0.007)	(0.012)	(0.010)
Constant	0.452***	0.283***	0.270***	0.245***	0.211***	0.124**	–0.647***
	(0.065)	(0.039)	(0.033)	(0.044)	(0.032)	(0.055)	(0.044)
Mean of dependent variable	0.780	0.308	0.297	0.294	0.419	0.280	0.281
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Birth year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dummy variable for each household wealth quintile	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5,941	16,295	17,581	9,006	22,351	7,365	2,687
R-squared	0.220	0.249	0.360	0.373	0.342	0.216	0.010

Robust standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$
Notes: With the exception of the fixed effects regression (Column 14) and Columns 10–11, the sample is restricted to most recent birth of ever-married women (ages 15–49) within five 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. National sample weights are used in all regressions. 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).

Appendix Table A6
Sex-Selective Prenatal Investments in India: RCH 1998–2004

	Dependent Variable							
Coefficient on Male in various samples	At Least 2 Visits (1=Yes, 0=No) (1)	Prenatal Care (1=Yes, 0=No) (2)	Number of Prenatal visits (3)	Tetanus Shot (1=Yes, 0=No) (4)	Number of Tetanus Shots (5)	Given Iron Pills (1=Yes, 0=No) (6)	Number of Iron Supplements Taken Per Day (7)	Nonhome Delivery (1=Yes, 0=No) (8)
Full sample	0.005***	0.007***	0.035***	0.010***	0.021***	0.005***	0.005	0.011***
	(0.002)	(0.002)	(0.008)	(0.002)	(0.004)	(0.002)	(0.003)	(0.002)
Mean of dependent variable	0.482	0.553	1.994	0.720	1.404	0.508	0.637	0.278
Observations	233,687	230,971	230,946	230,755	227,776	230,637	229,813	230,383
R-squared	0.305	0.298	0.441	0.171	0.189	0.232	0.151	0.321
Northern region	0.007**	0.010***	0.038***	0.012***	0.020***	0.010***	0.012**	0.011***
	(0.003)	(0.003)	(0.012)	(0.003)	(0.007)	(0.003)	(0.005)	(0.003)
Mean of dependent variable	0.429	0.498	1.492	0.675	1.280	0.435	0.545	0.202
Observations	76,461	75,293	75,291	75,250	74,464	75,249	74,984	75,121
R-squared	0.251	0.251	0.341	0.145	0.149	0.143	0.111	0.212
Majority female sample	0.017***	0.015***	0.076***	0.013***	0.031***	0.013***	0.013**	0.017***
	(0.003)	(0.003)	(0.015)	(0.003)	(0.007)	(0.003)	(0.006)	(0.003)
Mean of dependent variable	0.523	0.595	2.192	0.750	1.478	0.547	0.680	0.304
Observations	80,685	79,514	79,510	79,466	78,610	79,434	79,204	79,325
R-squared	0.301	0.294	0.444	0.161	0.178	0.229	0.147	0.328

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). RCH/DLHS surveys from 1998 and 2002 used. Controls include state fixed effects, birth year fixed effects, survey round fixed effects, mother's age, mother's education, mother's literacy status, father's education, father's literacy status, household building type, caste, religion fixed effects, household size, 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.

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