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

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

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1. Introduction

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

Maternal inputs during pregnancy can affect important outcomes such as neonatal survival and birth weight (Gortmaker 1979, Bharadwaj and Eberhard 2010). In India, attending prenatal care is correlated with a 27% decrease in the probability of neonatal mortality (NFHS). Tetanus shots taken during pregnancy play a particularly important role in neonatal survival[2]. 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% of child deaths (under 5 years) occur in the neonatal stage; moreover, prenatal care is highly correlated with postnatal care such as breastfeeding and immunizations (NFHS),

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

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

indicating that the discrimination faced in utero persists and perhaps accumulates even after birth. Early childhood health not withstanding, we also know from previous research that in utero events and childhood endowments affect later life health, IQ and labor market outcomes (Almond and Mazumder 2005, Black, Devereux, and Salvanes 2007, Behrman and Rosenzweig 2004, Almond, Chay, and Lee 2002).

This paper examines whether sex-selective prenatal care occurs in countries of South and Southeast Asia, with an emphasis on India.[3] We find significant differences in the prenatal health care choices of women when they are pregnant with boys relative to when they are pregnant with girls. In India women are 1.1 percentage points more likely to attend prenatal care when pregnant with a boy and receive a significantly greater number of tetanus shots. In northern India, where sex discrimination is known to be more prevalent, women are 4.6% more likely to seek prenatal care and 3% percent more likely to receive tetanus shots if they are pregnant with a boy. In the same region, women are 16% more likely to deliver their baby in a non-home environment if pregnant with a boy. We also find that women whose previous children were mainly girls tend to discriminate more when the current fetus is male (see Figure 1). Moreover, for a subset of the Indian data, we find that prenatal discrimination occurs largely among mothers who report having received an ultrasound during pregnancy. We find similar evidence in other countries of South and Southeast Asia were sex discrimination has been documented. For example, in China, women pregnant with boys are nearly 6% percent more likely to seek prenatal care. Mothers in Pakistan are 6% more likely to take iron supplements and mothers in Bangladesh attend prenatal care 7% more frequently when pregnant with a boy.

Apart from examining a new parental avenue for gender discrimination, we also bring new perspective to the vast literature on parental investments (Rosenzweig and Zhang 2009, Ashenfelter and Rouse 1998, Behrman, Rosenzweig, and Taubman 1994) that examines whether schooling or nutrition-based investments reinforce (or are affected by) the distribution of initial endowments. The notion of "initial endowments" is often related to birth weight (Loughran, Datar, and Kilburn

[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.

2004) or the residual of a human capital production function (Pitt and Rosenzweig 1990).[4] Our paper adds to the literature on parental investments by showing that initial endowments (even *within* families) are subject to preferences over gender. Thus, beyond the usual concerns with endogenous endowment formation like maternal behavior, genetic correlations et cetera, we put forth gender preferences as an additional channel for consideration when examining the impact of initial endowments on short and long term outcomes.

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

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

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

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

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

Fourth, the presence of sex-selective abortions - which we do not observe in our data - are problematic to our approach for several reasons. In the presence of selective abortions, our regression samples omit the subset of mothers who abort female fetuses. Any selection bias that results from their omission will likely lead to an *underestimate* of the true gender gap in prenatal care as families that perform sex-selective abortions represent a highly discriminatory part of the population and these mothers would not have sought prenatal care had they been forced to take their female pregnancies to term. Another concern arises if the receipt of prenatal care influences the gender of observed live births via ultrasounds done at the time of prenatal check up and subsequent selective abortion. We show that such reverse causation does not explain our results by examining

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

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

investment behavior for the sample of mothers who have attended prenatal care at least once and have thus already made abortion-related decisions. Even in this restricted sample, we find that mothers who are pregnant with boys *continue* to seek more prenatal care than their counterparts carrying daughters. This result is critical, as it indicates that sex determination leads to other forms of gender discrimination, above and beyond its effect on skewed sex ratios through sex-selective abortions. Finally, mothers who sex-selectively abort may have systematically different preferences over prenatal care; if this is true, then non-randomness of fetal gender due to sex-selective abortions and unobserved preferences of these women may lead to a classic omitted variable bias. To address this issue, we show that even among children of the *same* mother, boys tend to receive greater prenatal care than girls. Hence, if omitted parental preferences are time invariant, then comparing the outcomes of siblings accounts for those variables. In our regressions, we also control for wealth and education, two critical factors that may determine both likelihood of committing selective abortion and receiving prenatal care (Bhalotra and Cochrane 2010, Portner 2009).

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

2. Methodology and estimation issues

Papers examining son preference in the US have examined the role of gender bias in differences in prenatal care (Dahl and Moretti 2004, Lhila and Simon 2008) using receipt of ultrasound scanning during pregnancy as indication that the parents know the sex of the child. Unfortunately, data on ultrasound receipt is inconsistent across the rounds of the National Fertility and Health Survey (for a select subset of the Indian sample we do have this information; we discuss the use of this data in detail in the results section). However, we rely on the idea that in the absence of ultrasounds or other methods of sex determination, there should be no systematic reason to find

that males receive greater prenatal care. This section describes our basic estimation strategy and outlines the various problems that could hinder inference as well as our attempt to deal with each potential source of bias.

2.1. **Basic Specification**

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

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

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

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

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

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

The coefficient we get on $Male$ in this sample (β_G) will likely be different from the coefficient on $Male$ if we were to estimate equation 2 for families whose previous births are all male (call this β_M). Hence, β from equation 1 should

Several important identification issues emerge when following this approach. We now review each problem and our proposed solutions in detail.

2.2. **Selective recall**

It is possible to find a positive β if mothers are simply more likely to report receiving prenatal care when pregnant with a boy even if actual prenatal care is not gender-biased. If males are indeed preferred, then activities that led to a male birth might be better remembered. 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).[8] The advent of ultrasounds - in particular, portable sonogram machines - has made sex determination less risky, easier to access and less expensive (about $12 each, according to The Economist, March 2010). Anecdotal evidence suggests that even rural areas are visited by itinerant doctors who carry ultrasound machines from town to town, offering sex determination without official prenatal care (New York Times, May 2001).[9] Thus, to tackle to issues of selective recall and reporting, we estimate of equation 1 using the NFHS survey conducted in 1992, *before* ultrasounds spread to many regions in India. If mothers are no more likely to remember 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.

be interpreted as a weighted averaged of β_G and β_M, where the weights depend on the fraction of the population that discriminate against girls in their last birth. We show estimates of β_G and β_M for various birth orders in Figure 1. As expected $\beta_G > \beta_M$ across most of the birth orders.

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

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

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 in families for some families (particularly for those who have boys), we would expect that these unobserved traits are constant within families. In this case, the mother fixed effects specification we employ should mitigate any remaining concerns with bias arising from selective reporting.

2.3. **Medical complications**

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

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

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

our results are susceptible to bias due to such a stopping rule. As a robustness check, we employ the methodology developed in Barcellos, Carvalho, and Lleras-Muney (2010).

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

2.5. **Sex selective abortions**

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

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

2.5.2. *Reverse causality.* The presence of sex-selective abortions could also bring into question the direction of causation between prenatal care and fetal gender. If ultrasounds are a routine

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

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

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

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

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

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

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

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

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

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

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

3. Data

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

Although the data in the paper are collected from many different sources, the method of constructing the estimation samples is very similar across all countries. Within each country we use the sample of ever married women generally between the ages of 15 and 49. Information is collected retrospectively about the pregnancy history of each woman, including detailed prenatal investment data from the most recent pregnancy previous to the survey. In the 1998 round of the NFHS, mothers report information about their two most recent pregnancies, allowing for the construction of a panel dataset suitable for fixed effects estimation (see previous section). We collect basic information such as age and educational attainment about mothers and wealth quintile of the

family, as well as geographical data about their place of residence which is used to generate the spatial fixed-effects included in all subsequent regressions. Summary statistics for mother characteristics are presented in Appendix Table 2 for India (not shown for the remaining countries). Average educational attainment is generally low but displays considerable variation across countries. In India, the average mother in the sample is 28 years old and has completed only primary school.

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

4. Results

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

[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.

a benchmark for comparison. Finally, in section 5.3 we use our results to assess the impact of differential prenatal care on excess female neonatal mortality in India.

In Table 1, we estimate equation 1 for various subsamples of the National Family Health Survey in India. Overall, we see that males tend to receive significantly more prenatal investments across almost every type of prenatal care (Appendix Table 3 shows that the coefficient of interest is stable across specifications where the independent variables are added one at a time). 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 non-home facility), we find that males are 1.6% more likely to receive care and that this gender gap in the aggregated measure of care is statistically significant (p-value 0.011).[13] When we restrict the analysis to the northern region of India (Punjab, Haryana, Himachal Pradesh, Uttar Pradesh and Rajasthan), we see a much larger magnitude of discrimination for certain types of prenatal care treatments; this difference between the northern sample and full sample is significant for prenatal care, the number of prenatal visits, the days iron supplements are taken and the place of delivery (results not shown but are available upon request). This is consistent with other studies that find more skewed sex ratios in these regions (Jha et al. 2006), suggesting higher levels of son preference as well as greater availability of ultrasound technology (as noted earlier, Punjab was one of the first states to receive this technology). Mothers attend prenatal checks up more frequently (9%) and take more tetanus shots (4%) when pregnant with a boy. Moreover, mothers are nearly 3 percentage points more likely to invest in prenatal care when pregnant with a boy. We see slightly larger magnitudes (compared to the full sample) for samples where the previous children of the women are majority female (the children prior to the latest birth), although the differences in magnitudes in this sample relative to those in the full sample are not significant (results not shown; available upon request). If son preference is present, we should find that samples where women previously have had female children should be even more likely to differentially invest if their most recent pregnancy is a boy. For this sample of majority female in the past children of the mother, we find the magnitudes to be quite large - males are

[13]Our methodology follows Kling and Liebman (2004) to aggregate across the 4 outcomes: tetanus shot receipt, prenatal checkup, iron pill use and delivery in a non-home facility. Since 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.

1.7 percentage points more likely to receive tetanus shots than females, and nearly 1.5 percentage points more likely to receive some sort of prenatal care. Hence, for India, we find strong, consistent evidence that women utilize more prenatal care options when pregnant with a boy than when they are pregnant with a girl.

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

Panel B examines all births in the 2005-6 survey (since ultrasound information was asked of everyone, not just mothers who had a prenatal check up). We use similar outcome measures as Panel A to keep matters consistent, but also because a very large fraction of those who report having

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

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

4.1. **Robustness Checks**

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

First, we estimate the relationship in equation 1 for births prior to widespread availability of ultrasound or sex determination technology. In Table 3, we do not find any evidence towards prenatal discrimination among births that occurred in the late 1980s in India. If we again test for an overall gender gap using an aggregated measure across all binary outcome measures (following Kling and Liebman 2004), 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 4 we exploit the timing of the first prenatal checkup and show that there is no gender gap in prenatal care that occurs within the first four months of pregnancy, when the sex of the fetus is unknown. In contrast, there is a large and significant gap in care that takes place in the final five months of pregnancy. Here we assume that the sex of the fetus is not known during the first 4 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 2 or 3 months, when it is extremely unlikely that a mother knows the gender of her baby.[14] Thus we believe that the existence of selective recall cannot explain this pattern of discrimination in our results, even within the same pregnancy. [15][16]

Second, we estimate whether being pregnant with a boy leads to more complications during the pregnancy. If carrying a male were more physically taxing than carrying a girl, then we might find that women pregnant with boys are more likely to seek prenatal care for reasons other than gender discrimination. Table 5 estimates whether being pregnant with a boy is significantly related to complications during pregnancy in India. Except for the category of "night blindness" we do not find any evidence to support the idea that male fetuses *medically* require greater prenatal care through increased complications. Moreover, the size of the coefficient on night blindness is extremely small compared to the average level of night blindness experienced by mothers in the sample. [17]

[14]Including a measure of family wealth as a control in these regressions is very important. Without such a control, we find a positive correlation between the gender of the fetus and prenatal care even within the first 3 months of a pregnancy. We believe that this correlation is driven by the omission of a proper wealth control, as wealthier people tend to have sex-selective abortions and are more likely to receive prenatal care.

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

[16]We find that women in our sample are more likely to report having had an ultrasound for pregnancies resulting in sons. Given the evidence against selective recall presented here, we believe that the positive correlation between fetal gender and ultrasound receipt reflects the presence of sex selective abortions (addressed below) rather than selective recall.

[17]A concern might be that if women carrying male fetuses *do* need greater prenatal care, then perhaps Table 5 does not reflect differential complications by male because mothers take greater prenatal care while pregnant with a male. We

Third, following Barcellos, Carvalho, and Lleras-Muney (2010) we restrict the sample to families where the youngest child is less than 2 years old to minimize the bias due to families adjusting their fertility after realizing the sex of the child. The first three columns of Table 6 display the results using this sample. For outcomes such as prenatal care, tetanus shots and delivery in a non-home facility, our original results hold and the magnitudes of the gender gap are slightly larger. One drawback of this method is that it relies on the assumption of no sex-selective abortions. As we indicated earlier, the presence of selective abortions can bring about two important sources of bias: sample selection and reverse causation. While the bias due to sample selection likely works in the opposite direction of our result, reverse causation could potentially bias our estimates upwards. To account for this possibility, we further restrict the sample to those who have made at least one prenatal visit and focus on whether these women seek higher levels of *additional* prenatal care when carrying boys than when carrying girls (columns 3 - 6 of Table 6). Again, we find that even in this restricted sample, male fetuses receive significantly more care than female fetuses and the magnitude of the gender gap remains substantial. Thus we find no evidence that any bias due to gender-based stopping rules is driving our results.[18]

Our final concern has to do with reverse causality due to sex selective abortions. If mothers learn the sex of their child during a prenatal visit and choose to abort if female fetuses, then we would find a mechanical correlation between prenatal care visits and sex of the child due to sex selective abortions, rather than sex selective prenatal care. As we proposed in an earlier section, one approach that minimizes the potential for reverse causality is to restrict the sample to those who have made at least one prenatal visit and examine the gender gap in subsequent prenatal care. The first three columns of Table 7 display the results for this approach. We find that even in this sample (in which reverse causation is highly unlikely) there remains a sizeable gender gap in prenatal

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.

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

care undertaken after the first visit. Moreover, when we take an even more conservative approach by restricting the sample to women who make their first prenatal visit in the final five months of pregnancy (for whom the assumption of discovering the sex of the child during the first visit is most credible), we find that there remains a high degree of gender discrimination in prenatal care. Although the gender gap is not significant for tetanus shots and non-home delivery, we believe this is largely due to the considerable drop in sample size and more importantly, the magnitude of the of the gender gap is consistent with our main results. Appendix Table 4 confirms that this finding is robust to different assumptions about when fetal gender is known and to additional outcome variables. We find that women who make their first prenatal visit in the final 5 (or 6) months of pregnancy are significantly more likely to make multiple visits (at least 2, 3 or 4) when pregnant with a male. This is consistent with mothers discriminating against female fetuses after the sex of the fetus is revealed in the first visit.[19] Overall, these results indicate that reverse causality is not likely to be sole explanation for our results.

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

4.2. **Results from other countries**

Since the DHS collects extensive prenatal care data, we can extend our analysis to other countries in South and Southeast Asia.[20] We estimate equation 1 for China, Bangladesh, and Pakistan - countries where son preference and gender discrimination has been well established in

[19]Note that regardless of the outcome (at least 2, 3 or 4 prenatal visits), the relevant comparison group is those who have only 1 prenatal checkup. This is because those who continue to make prenatal visits past the first are those who do so knowing the sex of their baby.

[20]In principle, *all* DHS countries can be used in this analysis. Based on our reading of the literature on son preference and gender discrimination, we believe we have focussed on a part of the world where this is most relevant.

previous studies (Das Gupta et al. 2003) - and find that the gender bias in prenatal care is not limited to India but is rather pervasive in Southeast Asian countries with a history of son preference. As part of a larger robustness check, we estimate equation 1 for Sri Lanka and Thailand where son preference is weak (Arnold, Kishor, and Roy 2002, Hua 2001, Prachuabmoh, Knodel, and Alers 1974). As a final robustness check, we investigate whether sex-selective prenatal care is practiced in Ghana, a country with no known son preference (Garg and Morduch 1998).

The first four rows of Appendix Table 5 display the results of estimating 1 for countries that are known to have son preference: China, Bangladesh, and Pakistan (both the full sample and the region of Punjab).[21] Overall, the results from these samples exhibit patterns consistent with sex-selective discrimination in prenatal care. In China, women are 5% more likely to get some prenatal care when pregnant with a boy, and visit antenatal clinics nearly 10% more frequently. In Bangladesh, women are 2.8 percentage points more likely to get a tetanus shot when pregnant with a boy. We do not find significant estimates in the decision to seek prenatal care, although we do find that women visit prenatal clinics 7% more frequently when pregnant with a boy. In Pakistan, we find that women visit prenatal clinics more often and are 6% more likely to consume iron pills when pregnant with a boy. In Paskistani Punjab, a region with a large number of missing women (Gechter 2010), the magnitude of discrimination is even larger for some prenatal outcomes; for example, mothers are 13% more likely to take iron pills. Taken all together, the evidence in Appendix Table 5 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 Appendix Table 4 displays the estimates for Sri Lanka, Ghana and Thailand. While almost all specifications are statistically insignificant, what is relevant for us is that the magnitudes are quite small. At a minimum, these coefficients are smaller than what we found for countries with known son preference. The estimates in Sri Lanka and Thailand are consistent with lower levels of son preference and none are statistically significant. These results also help rule out the possibility of factors confounding son preference in prenatal investments;

[21]Note that not all outcomes are available for China.

there is no evidence of selective recall or biological factors which indicate that boys are more likely to receive prenatal care for reasons other than son preference.

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

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

While neonatal deaths occur more frequently among males, this does not mean that there are no "excess" female deaths in the neonatal stage. In our sample for India, the observed neonatal death rate is 2.24% for girls. Female neonatal mortality rate in Ghana and Italy is around 1.93%. Using the sex ratio in neonatal mortality from these countries (since they are presumed to be free of son preference), we impute a neonatal mortality rate for women in India to be around 1.94%.[22] Thus excess female neonatal mortality - the amount that the rate exceeds what we expect under equal treatment - is 0.31 percentage points in India.[23]

Our estimates from Table 1 suggest that males are 1.6% more likely to receive tetanus shots than females (this is our smallest effect across all specifications for India). This implies that for every 100 boys, only 98.4 girls receive tetanus shots. If we take estimates from Rahman et al. (1982), we would believe that babies face a mortality rate that is 3.03 times higher in the neonatal

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

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

stage if the mother did not receive a tetanus shot. Since 80.3% of all mothers pregnant with girls receive tetanus shots, the implied neonatal mortality rate for those whose mothers were received the shots is 1.6% and 4.85% for those whose mothers did not.

This means that had the 1.6 girls that did not receive tetanus shots actually received one, 0.012 more girls would have survived than in the case of differential treatment. Hence, unequal allocation of tetanus shots can explain around 3.9-4.0% of the "excess" female mortality in the neonatal stage (depending on whether we use the benchmark estimate from Italy or Ghana). If instead we use our largest estimates that males are 4.34% more likely to receive tetanus shots (from Table 8, the mother fixed effects table), we conclude that unequal allocation of tetanus immunizations can explain around 10.1%-10.5% of the excess female neonatal mortality (again, depending on which estimate for equal treatment we use). Therefore, we believe that discriminatory practices with regards to tetanus vaccinations during the prenatal period can explain between 4-10.5% of the excess female mortality in the neonatal period.[24]

5. Conclusion

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

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

reality, prenatal care is most often mutlidimensional 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. These sizeable gender differentials in prenatal investments are likely to be the cause of the large observed disparity in short-term health outcomes between girls and boys. In our Indian sample, boys are born with greater birth weight and have a lower probability of being low birth weight.[25] While this is not causal, it is consistent with boys receiving greater prenatal care. Correlations between various dimensions of prenatal care (such as tetanus shot receipt and iron pill supplements) and outcomes such as neonatal deaths and birth weight show that babies that receive some prenatal care are better off. Hence, sex-selective prenatal care can be associated with differential birth weight and neonatal death rates among boys and girls.

Second, policy in countries like India is focused on a natural and important outcome of sex-based discrimination - survival rates of females measured via sex ratios at different ages. Given the findings from the vast literature linking early childhood health (such as birth weight) and later life outcomes, our results imply that effect of gender discrimination in prenatal care might also be seen in the long run via decreased labor market opportunities or decreased educational attainment for women. Hence, even if the imbalance of sex ratios improves over time, we should worry about the possibility of sex-selective prenatal care.

Third, we provide a unique perspective on the literature concerned with parental investments based on child endowments. Our study brings into question the very process of the endowment formation - child endowments, often measured as birth weight are themselves the result

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

of parental preferences over gender. Hence, studies investigating such relationships in developing countries with son preference must seriously consider the possibility that parents differentially invest based on the sex of their unborn child.

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APPENDIX A. CALCULATING THE CONTRIBUTION OF DIFFERENTIAL TETANUS IMMUNIZATIONS TO EXCESS FEMALE MORTALITY

Girls are more likely to survive than boys in the neonatal period for genetic and biological reasons. We use female neonatal mortality rate in the Ghanian DHS data as a measure of the "natural" neonatal mortality rate for girls. Restricting the sample to the 1998, 2003 and 2008 rounds (in order to be comparable to the NFHS time frame used in our regressions), the female neonatal mortality rate is 1.93%. When we use the results of a study in Italy (Ulizzi and Zonta 2002) we impute a natural rate of 1.94%; thus we are confident that this represents an accurate measure of neonatal mortality among girls in the absence of differential treatment and use it in all calculations below.[26] The neonatal mortality rate is 2.24% among girls in our sample from India. This implies that the excess female neonatal mortality is 2.24-1.93=0.31 percentage points.

According to Rahman et al. (1982), babies are 67% less likely to die in the neonatal period if their mothers received tetanus shots during pregnancy; this implies that babies whose mothers did *not* receive tetanus shots are 3.03 times as likely to die.[27] As mentioned before, the neonatal mortality rate is 2.24% among girls in the Indian sample. Since 80.3% of all mothers pregnant with girls receive tetanus shots, the implied neonatal mortality rate for those whose mothers were received the shots solves $0.803x + 3.03(1-0.803)x=2.24$. This yields a mortality rate of 1.6% for female children born to women who received tetanus shots and 4.85% for those whose mothers did not.

Our estimates in Table 1 show that women are 1.6% less likely to receive tetanus shots when pregnant with girls than when pregnant with boys. This means that for every 100 boys who receive tetanus immunization through their mothers, only 98.4 girls do. If mothers were equally likely to receive tetanus shots (regardless of fetal gender) then the remaining 1.6 girls out of 100 would have tetanus immunity. Under equal treatment the number of girls who die from tetanus is

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

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

0.23(0.016)100=0.368 per 100, where 23% of neonatal deaths are due to tetanus in India (UNICEF 2000) and the neonatal mortality rate is 1.6% (calculated above).[28] Under differential treatment, where 1.6 girls are born to mothers who have not had tetanus shots, 0.23((1.6)0.0485+(100-1.6)0.016)=0.380 girls die per 100 because the 1.6 girls without tetanus immunity face a higher mortality rate of 4.85% (calculated above). Thus, the difference in tetanus shots leads to a difference in observed neonatal mortality of 0.380-0.368=0.012 deaths per 100 girls.

Therefore, the gender gap in tetanus shots can explain 0.012/0.31=3.86% of excess female neonatal deaths in India (or 3.98% if we use the Italian benchmark). If we repeat all of the calculations using the upper bound of our estimates for India (the mother fixed effect specification, results in Table 4) we find that differential tetanus treatment accounts for 10.1% of the gap between the natural and observed rates of neonatal mortality (10.5% using the imputed rate from Italy). Hence we believe that the gender bias in prenatal tetanus immunizations can explain 4%-10.5% of excess female neonatal mortality.

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

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

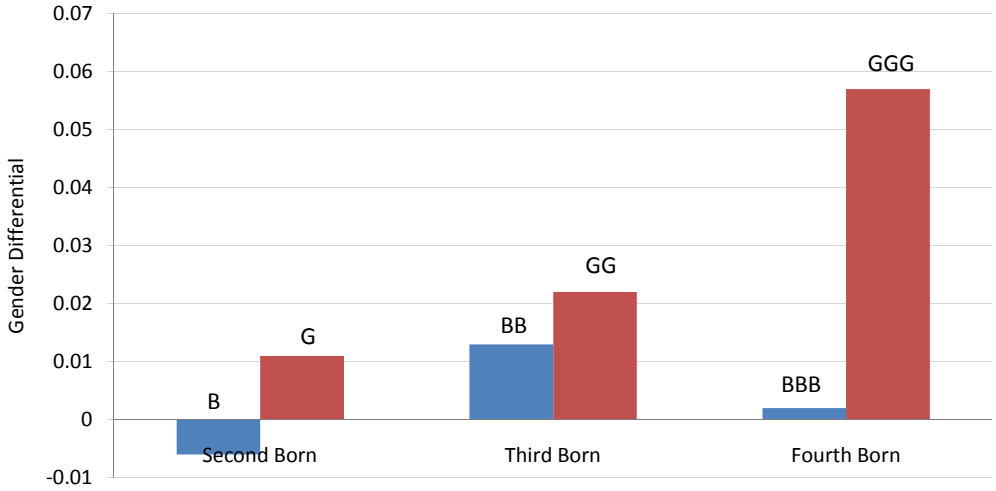

TABLE 1. Sex-Selective Prenatal Investments in India

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

Robust standard errors in parentheses

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

TABLE 5. Gender and Pregnancy Complications in India

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

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

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

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

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

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

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

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

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

Robust standard errors in parentheses

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

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

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

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

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

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

APPENDIX TABLE 1. Description of Regression Samples.

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

APPENDIX TABLE 2. Summary Statistics for India

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

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

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

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

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

Robust standard errors in parentheses

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

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

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

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

Robust standard errors in parentheses

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

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

APPENDIX TABLE 5. Robustness Checks for Gender Discrimination in Additional Prenatal Care in India

	All with First Prenatal Checkup during the Final 6 Months of Pregnancy			All with First Prenatal Checkup during the Final 5 Months of Pregnancy		
	At least 2 Prenatal Visits	At least 3 Prenatal Visits	At least 4 Prenatal Visits	At least 2 Prenatal Visits	At least 3 Prenatal Visits	At least 4 Prenatal Visits
	(1)	(2)	(3)	(4)	(5)	(6)
Male	0.024***	0.022*	0.045***	0.027**	0.025*	0.040**
	(0.009)	(0.012)	(0.016)	(0.011)	(0.015)	(0.019)
Mother's Age	0.004***	0.006***	0.006***	0.005***	0.007***	0.006**
	(0.001)	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)
Mother's Education	0.023***	0.047***	0.075***	0.018***	0.037***	0.050***
	(0.006)	(0.008)	(0.011)	(0.007)	(0.010)	(0.014)
Urban	0.028**	0.044***	0.072***	0.026	0.058**	0.095***
	(0.012)	(0.016)	(0.022)	(0.016)	(0.023)	(0.031)
Birth Order	-0.015***	-0.020***	-0.028***	-0.018***	-0.026***	-0.031***
	(0.004)	(0.005)	(0.007)	(0.005)	(0.006)	(0.008)
Existing Sex Ratio of Chil	-0.012	-0.006	0.002	-0.001	0.010	0.017
	(0.011)	(0.015)	(0.019)	(0.013)	(0.018)	(0.024)
Constant	0.829***	0.732***	0.646***	0.757***	0.230***	0.596***
	(0.038)	(0.053)	(0.068)	(0.045)	(0.072)	(0.077)
Mean of Dependent Varial	0.836	0.748	0.573	0.813	0.698	0.474
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Dummy Variable for Each	Yes	Yes	Yes	Yes	Yes	Yes
Birth Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	10661	7250	4482	7547	4881	2918
R-Squared	0.075	0.165	0.351	0.082	0.182	0.376

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

Notes: Sample is restricted to most recent birth (within 5 years previous to the 2005-6 survey only) of ever married women. Existing sex ratio is defined as the ratio of boys to the total number of births prior to the most recent one.

Family Planning and Fertility: Estimating Program Effects using Cross-sectional Data*

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Abstract

This paper uses a novel set of instruments to identify the effects of family planning programs when there is potentially non-random program placement and only cross-sectional data are available, a situation common in many developing countries. The idea behind the instruments is that areas compete for resources. Competition is captured by the relative ranking of characteristics across areas, which is not correlated with the outcomes of interest. The instruments are ranking of area education levels, urbanization, and population size. Using data from Ethiopia we find that access to family planning substantially reduces the number of children ever born for women without education; the reduction is especially pronounced for women younger than 20 and older than 30. Completed fertility, measured as children ever born for women aged 40 to 45, falls by more than one birth with access to family planning. These effects are statistically significant and substantially larger than previous studies have found. For women who have gone to school there is no evidence of an impact of family planning on fertility. Based on a relative small reduction in child mortality we argue that the effect on fertility is due to family planning access and not the impact of the concurrent presence of health facilities. Finally, family planning access reduces unwanted fertility, especially for older women.

Keywords: Family planning, fertility, program evaluation, Ethiopia

JEL codes:

1 Introduction

Many countries, especially in Africa, still have high fertility rates. This has potentially significant implications for women's and children's health and economic development. Motivated by these concerns, policy discussions often focus on the role of family planning programs in helping individuals manage and implicitely lower their fertility. The effectiveness of family planning programs is, however, still an under-researched question. Despite the lack of evidence, there appears to be a consensus in economics that family planning programs are ineffective.[1] This is partly because of the lack of convincing evidence that these programs reduce fertility and partly because standard models of fertility decisions suggest that many people in developing countries have little incentive to reduce the number of children when children are potentially productive on the family farm and when the cost of women's time is low.

There are a number of reasons why there is little convincing evidence on the effectiveness of family planning programs. First, studies of family planning programs have often covered periods of rapid economic development and fertility decline, making it difficult to isolate the effects of family planning programs. Secondly, there is little emphasis on how family planning affects women of different education levels. Evidence from the USA shows that better-educated women are not more efficient users of modern contraceptives than less-educated women, but the better-educated women are more likely to know how to use and more efficient at using "ineffective" contraceptive methods such as withdrawal or rhythm (Rosenzweig and Schultz 1989). This suggests that the lower a woman's education level, the more likely she is to benefit from access to modern methods.

Finally, when an organisation decides where to place programs it is likely to respond to area characteristics. Some of these characteristics may be unobservable to the researcher and correlated with the outcomes of interest. This correlation can lead to biased estimates of program effectiveness (Rosenzweig and Wolpin 1986; Pitt, Rosenzweig and Gibbons 1993). It is therefore important to find a method to address non-random program placement, but the nature of fertility decisions

[1] Instead there is an emphasis on factors that influence fertility demand such as household poverty and girls' schooling.

poses obstacles. Suppose a government aims to reduce fertility. Two cases illustrate the problems of non-random program placement. In the first case, the government places programs in areas that are more "receptive" to reducing fertility. Simply comparing fertility in areas with and without family planning will then overestimate the impact of expanding the program. In the second case, the government places programs in the high fertility areas. If information on prior fertility is not available comparing fertility across areas will underestimate the effectiveness of the program. As these examples show, without information on the placement process it is difficult to assess the direction of the potential bias.

The most straight-forward way of overcoming non-random placement is to randomize the allocation of programs and compare the outcomes of interest between the treatment and control areas (Duflo and Kremer 2005). Although experiments appear to offer an attractive means of avoiding issues of non-random placement, they are subject to a number of drawbacks when examining the effectiveness of family planning programs. First, because of the cumulative nature of fertility, an experiment has to run for a substantial period of time before one can assess the effect on fertility. Any observed short-run effects may simply reflect changes in spacing-patterns rather than changes in the overall number of children. Modern contraceptives offer more control over the timing of births and even if completed fertility decreases, fertility for certain age groups may increase. This makes short-term changes unreliable indicators of the final number of children. Secondly, experiments are prone to problems like short-term health scares such as the one experienced by an experiment in Zambia (Ashraf, Field and Lee 2009). Other methods are also affected by this, although the longer the period covered and the broader area coverage is, the less the problem will be. Thirdly, it is not clear to what extent an experiment in, say, Bangladesh can inform family planning programs in Ethiopia given the substantial differences in the structure of the economies, cultural context, and the issues facing the population. Finally, in many settings family planning programs have been in existence for a substantial period of time and it is obviously cost-effective to try to use information that can be derived from these programs.

An alternative to a randomized design is to use longitudinal data. If these data are available,

program effects can be estimated using fixed effects to remove unobservable characteristics that influence program placement and fertility. There are two caveats to this approach. First, there must be a sufficient number of areas that receive a program between the (minimum) two data points. Secondly, the time period between the surveys must be long enough for the program to have an effect. If these conditions are not fulfilled it is difficult to identify the program effects with any precision.[2]

For the reasons above and the scarcity of available experimental and longitudinal data covering long enough periods of time, researchers are often faced with using cross-sectional data for analysing program effects. This paper uses cross-sectional data linked with information on area characteristics to evaluate Ethiopia's family planning program. During field work we asked Ethiopian NGOs responsible for the introduction of community based reproductive health (CBRH) agents what factors influenced their decisions on where to place new programs. Two factors stood out: accessibility and the extent to which the area was considered "receptive" to the family planning idea. The important difference between these factors is that accessibility is, in principle, measurable whereas receptiveness to family planning is generally unobserved by the researchers. We do not claim that the Ethiopian government distributes family planning programs according to the same criteria as the NGOs, but the example illustrates that there may be unobserved factors that influence both whether to place a program in an area and whether women in the area will use the services.

To address non-random program placement in this setting, one can estimate the program placement decision model and use predicted placement to evaluate the effects of the program. Identification comes from variables that are argued to influence program placement but not women's fertility decisions. The challenge is that many of the obvious candidates for variables that affect program placement also affect fertility either directly or through other pathways such as child mortality. An example of a variable that affects both program placement and fertility is how urbanised an area is. Cost of children may be higher in more urban areas reducing fertility and at the same time it

[2]There are also additional problems with using fixed effects, such as measurement error bias. For a discussion of this and other problems in the study of family planning see, for example, Angeles, Guilkey and Mroz (1998).

may be less costly for the government to place programs in areas there are more urban because of easier access. The direction of the effect of urbanisation on probability of having a program is less important than the fact that urbanisation substantially impact whether a program will be placed in an area. That rules out using urbanisation directly as an identifying variable.

The identification strategy introduced here is based on limited resources and, therefore, competition between areas for these resources. To fix ideas assume that there are only three areas, A, B and C, and that the three areas compete for resources from the government. Using the urbanisation example, we expect that the urbanisation of area A will affect fertility in area A, but the degree of urbanisation of areas B and C will have little or no effect on fertility in area A. Because the three areas compete for resources the *relative* degree of urbanisation may, however, affect the program placement decision. A straight-forward way to capture competition and the role of relative characteristics is to convert variables that are expected to be important in determining placement into rankings and use the ranking as identifying variables.[3] Imagine that urbanisation ranks the three areas such that A is greater than B and B greater than C and that the higher rank an area has the more likely it is to receive a program. Identification is achieved because the rank an area receives is predominantly dependent on other areas' absolute value of the ranked variable. Specifically, assume that the underlying value for area B increases. Unless it increases enough to surpass area A the ranking will not change even though the increase in the value of the ranked characteristics may directly affect fertility.

There are two major advantages to this approach. First, the instruments are easy to create from readily available secondary data like a census or, possibly, even from the primary data set itself. Secondly, the instruments are intuitive in that they mimic expectations about the underlying resource allocation process. In other words, ranks likely reflect what policy makers care about

[3] Pitt and Menon (2010) used average characteristics of other areas, such as education level, for their instruments. A potential issue with this approach is that if network effects are important these averages might not serve as valid instruments. One could use the ratio of the averages to the overall (national) average, but the drawback of this is that it requires a weighting of the characteristics based, for example, on distance between the areas. This weighting is essentially set outside the model by the researcher. It is possible to assign a unit weight to all characteristics and achieve identification, but as one increases the number of areas in the survey the matrix will eventually become of non-full rank.

when distributing programs but are not directly related to fertility. Furthermore, the process is agnostic about what ranks actually determine placement.

The paper makes three contributions to the literature. First, it uses the new set of instruments described to estimate program placement effects and identify the effect of family planning on fertility. Second, it shows how the effect of access to family planning is critically dependent on the education level of women. Third, whereas the scant evidence we have comes from very ambitious, costly program (Matlab) or dynamic macro-economic settings (Indonesia and Colombia), discussed in the next section, this paper presents results for the effect of family planning in a situation where there has been relatively low economic growth over the period in question.[4]

We find that access to family planning in Ethiopia has a statistically significant and economically large impact on fertility of women with no schooling. (about 65% of women 30 years or older in the 2007 Census). The reductions in fertility are concentrated among the youngest women and the oldest women indicating that access to family planning leads to a postponement of birth among younger women and a reduction in completed fertility. The reduction in completed fertility at more than 1 child is large compared to other studies. There are no discernible effects of family planning on fertility for women who have ever attended school. Using data on child mortality, timing of first birth and unwanted fertility, we argue that the reduction in fertility is due to access to family planning and not to improvement in child health and survival coming from the health facilities in which the family planning services are offered. Finally, we find that failing to take account of non-random program placement results in a downward bias in the estimated effectiveness of family planning, although the results nonetheless still show a statistically significant and substantial impact.

[4]Ethiopia's GNI per capita in PPP went from just over USD 300 in 1980 to USD 480 in 2003.

2 Literature Review

Probably the best-known example of a family planning program experiment comes from Matlab, Bangladesh. It began in 1978 when about half of the villages in the region were assigned a very intensive family planning program and the other half continued to be served by the standard government family planning program. Fertility was 24 percent lower in the villages that received the intensive family planning program compared to the villages that received only the standard family planning program (Phillips, Simmons, Koenig and Chakraborty 1988). It has, however, been argued that these results reflect a level of program intervention and intensity unlikely to be sustainable because the program was exceedingly expensive; per woman reached the program cost 35 times more than the standard government family planning program (Pritchett 1994). Each averted birth cost USD 180 in 1987, equivalent to 120 percent of Bangladeshs GDP per capita at the time. More recent work on using the same villages in Matlab from 1974 to 1996 finds a decline in fertility of about 15 percent in the program villages compared with the control villages (Sinha 2005; Joshi and Schultz 2007).

Longitudinal data has been used to identify family planning program effects in the Philippines and Indonesia (Rosenzweig and Wolpin 1986; Pitt et al. 1993; Gertler and Molyneaux 1994). For Indonesia, Pitt et al. (1993) found that family planning programs had a negative effect on fertility, although it is very imprecisely estimated and not statistically significant, whereas Gertler and Molyneaux (1994) found that family planning programs were responsible for only 4 to 8% of the decline in fertility from 1982 to 1987.[5] In the Philippines family planning prgrams had a significant and positive effect on child health as measured by both (standardised) weight and height. Cross-sectional estimates showed substantial bias compared with the fixed effects estimates.

Cross-sectional data has also been used to estimate the effect of family planning programs. One approach, used for evaluating the effect of Columbia's family planning program, Profamilia, assumed that the process of allocating programs is to all extent and purposes arbitrary (Miller

[5]Gertler and Molyneaux (1994) do, however, argue that the rapid decline was only possible because of already-existing access.

2010). Profamilia is found to reduce lifetime fertility by around half a child, which means that the family planning program is responsible for only 10 percent of the the sharp decline in fertility in Colombia over the period when the program was implemented. It does, however, appear that Profamilia led to a substantial postponement of first birth and that, in turn, led to higher education levels for young women. A potential problem is that even if (available) observables do not affect placement, unobservables characteristics may still impact placement, resulting in bias.

Closer to the method used here is an evaluation of family planning programs in Tanzania (Angeles et al. 1998).[6] There family planning was found to have a negative effect on fertility, although the effect varies with the type and distance to outlet and age of the woman when the program was introduced. A woman exposed to family planning through her fertile lifespan is predicted to have 4.13 children compared with 4.71 children in the absence of family planning programs. Identification comes from variables that are proported to influence program placement, but are unrelated to the individual fertility decision. The main issue is that some of the variables used to identify placement (such as child mortality levels and the presence of other family planning services) are likely to be correlated with unobservable variables that influence both placement and fertility decisions.

3 Family Planning in Ethiopia

Ethiopia is a classic example of a high fertility country (World Bank 2007). Its current total fertility rate, the predicted number of children a woman will have over her reproductives ages, is estimated at 5.4. During the period 1990 to 2005 Ethiopia's total fertility rate declined by about one child and the use of contraceptives tripled from 5 percent to 15 percent, with a most of the increase coming from modern methods, especially injectable contraceptives (Central Statistical Authority [Ethiopia] and ORC Macro 2006). Population growth resulting from such high fertility is believed to come at a high cost to living standards. The average land holding per rural person was estimated at only 0.21 ha in 1999, down from 0.5 ha in the 1960s. This, couples with lack of agricultural

[6]For Indonesia, Angeles, Guilkey and Mroz (2005) report using the same approach, but finds no evidence of non-random placement of family planning programs.

productivity growth, has contributed to a (rapidly growing) core group of five to seven million who are chronically food insecure. Spatial resettlement of about two million people from the highlands to the lowlands, adopted as one of a series of policy measures by the Coalition for Food Security Commission to tackle the problem of chronic food insecurity in many highland weredas, is unlikely to provide a sustainable solution in light of the estimated annual increase of Ethiopias population by two million people.

In 1993 the government of Ethiopia adopted a population policy; the overall objective was to harmonize the country's population growth rate with that of the economy, specifically to achieve a total fertility rate of 4 children per women by 2015. One of the major strategies in the policy aimed to expand access to family planning programs so that by 2015 contraceptive prevalence would reach 44 percent (Transitional Government of Ethiopia 1993).

Ethiopia has historically had a very low level of contraceptive use and has one of the lowest contraceptive prevalence rates in Sub-Saharan Africa. According to the first-ever national survey on fertility and family planning in 1990 only 4 percent of women of reproductive age were using some family planning methods and less than 3 percent were using modern contraceptives (Transitional Government of Ethiopia 1993). Results from the 2005 Demographic and Health Survey (DHS) show that 15 percent of married women use some method of contraception and that the majority of them rely on a modern method (Central Statistical Authority [Ethiopia] and ORC Macro 2005).[7] The Ethiopia DHS 2005 shows that the most commonly used modern methods are injectable contraceptives at 10 percent and oral contraceptives at 3 percent. The other modern methods are used substantially less: condoms, female sterilization, IUD and any traditional method accounted for less than 1 percentage point each.

[7] Other studies have found use rates in line with the DHS number or higher (Pathfinder International Ethiopia 2004; Essential Services for Health in Ethiopia 2005). The Essential Services for Health in Ethiopia (ESHE) conducted three region-wide surveys in SNNP, Oromia and Amhara regions between 2003 and 2004. The studies showed prevalence rates for modern contraceptives to be 14 percent, 16 percent and 14 percent in the Amhara, Oromia and SNNP regions. The average modern contraceptive prevalence rate for the three regions combined was 15 percent (Essential Services for Health in Ethiopia 2005). In September 2004, Pathfinder International Ethiopia conducted a survey on family planning and fertility in Amhara, Oromia, SNNP and Tigray regions. The use of modern methods was the highest in Oromia (24 percent) followed by Tigray (20.4 percent), Amhara (20.5 percent) and SNNP region (17.1 percent) (Pathfinder International Ethiopia 2004).

4 Data and Estimation Strategy

We use three data sources to evaluate the impact of the availability of contraception on fertility. The first is a contraceptive use survey collected under the auspices of Pathfinder International – Ethiopia (Pathfinder International Ethiopia 2005). The second is a health facility survey we collected to augment the Pathfinder survey. Finally, we supplement our study with data from the 1994 census of Ethiopia.

The Pathfinder survey was collected in September 2004 and covered the four largest regions: Amhara, Oromia, SNNPR and Tigray.[8] The Pathfinder survey's objective was to provide information on the level of knowledge, attitude and practice of family planning. The survey used a stratified multi-stage sampling design in four regions combined with urban-rural residence for each region. Weredas (districts) constituted the primary sampling units and a total of 58 weredas were sampled. A total of 176 communities (PA/kebeles), 113 rural and 63 urban, were surveyed in these 58 districts. Weights are provided to make the sample representative at for the 4 regions and for each of the strata. We use these weights for all descriptive and regression analyses as well as take into account the sampling design.

We conducted the Wereda Health Facility and CBRH (WHFC) survey in July 2005 to collect information on health facilities, family planning services and Community Based Reductive Health (CBRH) programs available in the 58 Pathfinder districts. The information came from health departments or social sector departments. In each weredas general questions were asked regarding the entire wereda and detailed questions were asked about the communities covered by the Pathfinder Survey.

It was not possible to identify 5 communities that are therefore dropped, leaving 171 communities. After data collection uncertainty arose about whether some of the urban communities surveyed in the facility survey were the same as in the Pathfinder survey and to be cautious we

[8]The four regions cover 86 percent of Ethiopia's population. Ethiopia is divided into 9 regions, with each regions further divided into zones and there are currently 68 zones in Ethiopia. Each zone is divided into weredas (or woredas, also sometimes called districts) and each wereda is divided into a combination of Kebeles (urban areas) and Peasant Associations (PAs) in rural areas. Kebeles and PAs are the smallest administrative unit of local government.

therefore drop an additional 26 communities. Furthermore, 9 communities are dropped because essential information are missing from the facility survey, including the presence of health facilities and when they were introduced. Finally, 16 additional communities were dropped because we were not able to find census data for the areas or because other important information was missing. Our final sample consists of 109 communities covering a total of just over 2,700 women, of which just over 2,000 remain after excluding never married and never partnered women.

4.1 Estimation Strategy

Our approach is to first estimate the determinants of the decision on whether to place a program P in area k and then to estimate the program effect on the individual outcome y_i. The system of equations is then:

$$P_k = X_k\alpha_1 + Z_k\alpha_2 + v_k, \quad (1)$$

$$y_i = X_k\beta_1 + X_i\beta_2 + P_k\beta_3 + \varepsilon_i, \quad (2)$$

where X_k is a vector of exogenous variable that are area specific, Z_k is a vector of area specific exogenous variables that affect program placement but do not affect the individual fertility decision, and individual characteristics are captured by X_i. Whether a program is available in the area, P_k, is the main variable of interest and β_3 measures the program's impact on the outcome of interest. The main outcome of interest is number of children ever born. In addition, we estimate the effects of family planning on various measures of mortality, timing of first birth, recent birth or pregnancy and whether last birth or pregnancy was wanted. Unfortunately, lack of birth histories in the data means that we cannot examine how the timing of births respond to family planning.

Using a modified two-stage method, β_3 can be estimated under relatively relaxed conditions (Wooldridge 2002, Chapter 18). The first stage estimates the determinants of the placement decision. In the second stage, the individual decision equation is estimated by IV, using the fitted probabilities from the first stage for P_k, $\mathbf{X_k}$ and $\mathbf{X_i}$ as instruments. An attractive feature of this

approach is that the results are robust even if the placement equation is not correctly specified.

In addition to the instrumental variable results, we also present OLS results, where Equation (2) is estimated under the assumption that there is no correlation between program placement and unobserved area characteristics. All regressions take account of the multi-stage sampling design and apply sample weights. Access to family planning is measured for each of the 109 communities in which women in the sample reside and standard errors would be biased downwards if no correction is applied to account for this clustering (Moulton 1990). Standard errors for both OLS and IV regressions are therefore clustered at the community level.

4.2 Family Planning Programs

The three main facilities or programs that might influence individual fertility decisions are health facilities, family planning services and CBRH programs. For all sample communities we have information on whether a health facility is available, when the facility opened, whether family planning services are offered at the health facility and, if so, the year it began offering family planning services. There are health facilities that do not offer family planning, but family planning is never offered outside of health facilities during the period we study.

A community is considered to have access to family planning if there is either a facility with family planning in the community or the community is less than 40 kilometers to the closest facility with family planning. Although the distance may appear long, most women only visit the family planning program every three months, either to pick up more pills or renew the injection. Furthermore, there is only one community where there is 40 km to the closest family planning program and the second-longest distance is 30 km. For urban communities the maximum distance to the closest facility is 3.5 kilometers. The average distance from those communities that did not have a health facility with family planning is around 10 kilometers. To determine when women in a rural community had access to family planning we use the year family planning services were first offered in that administrative area. For urban areas we use the year the closest health facility began offering family planning services whether or not the health facility is located in the urban

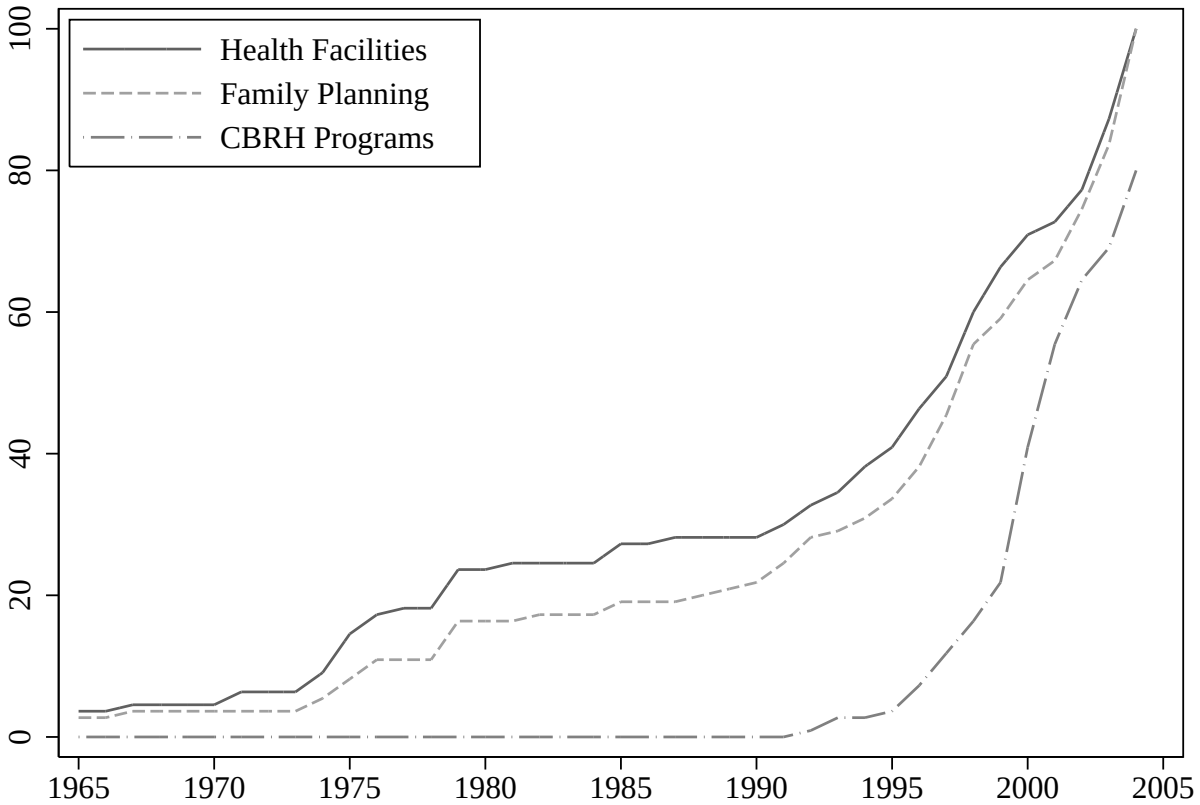
area or a neighbouring area.

The definition of access leads to two potential issues. First, it is not possible to estimate the extent to which distance to a family planning program is an important factor in use. Although our conversations with providers indicates that many of their clients do, indeed, travel substantial distances to receive family planning services, increasing distances must at some point lower use rates. In that case, our definition of access will tend to bias downward the estimated effect of access. Secondly, we only have information on access to the closest family planning program. Some areas may be coded as only having had family planning services for a relatively short period if a new health center recently opened in the area, even though the neighbouring area already offered family planning services. Similarly, it is possible that changes in facility type might not be reflected in the start date, i.e. a change from clinic to center that results in access to a wider set of services. These issues are also likely to result in a downward bias of the estimated effect of access.

Figure 1 shows the development in access to health facilities, family planning services and CBRH programs over time.[9] We focus on the effects of having access to family planning services in 1990, when approximately 20 percent of all communities in the sample had access to a family planning program. The prevalence of programs was essentially constant the decade before 1990. A majority of the women who had access in 1990 therefore have been exposed to the program for almost 25 years at the time of the survey allowing sufficient time to identify long-term effects on fertility. There was a substantial expansion in access to health facilities and family planning programs after 1990 with coverage going from 50 to 100 percent over from 1997 to 2005. The effect of the increase in program coverage is to bias downward the estimated the effect of the program.

Table 1 shows the descriptive statistics for the dependent variable and the explanatory variables used for estimating program placement. There are two categories of explanatory variables. First, variables that affect both placement and the individual decisions. Secondly, the instruments that

[9]The introduction of CBRH program is an interesting development, but happened too recently and to too many areas simultaneously to allow for an analysis of long-term effects on fertility. Access to health facilities and family planning services track closely making it impossible to estimate whether there is an independent effect of access to health facilities.



Source: The World Bank Wereda Health Facility and CBRH survey.

Figure 1: Percent Communities with access to Health Facilities, Family Planning or CBRHA

are assumed to only affect the program placement.

The first set of variables are variables expected to affect the placement decision that may also affect individual fertility decisions. The district level variables are the total area of the district, the average yearly rainfall and its square and the elevation of the district and its square. At the community level the variables include a dummy for whether is it an urban area (or in other words, whether it is a kebele), and a dummy for whether there is a market in the area. The accessibility of the area is captured by two variables: Whether the area can be reached by car all year or only during the dry season (the excluded category is no road access).

We use the rank of variables as instruments in the placement decision estimation. Each variable is ranked with 1 assigned to the smallest value and ties are assigned the same value, so that the sum

Table 1: Descriptive Statistics for Program Placement

	Mean	Standard Error	Min	Max
Dependent Variable				
Family planning program in 1990 (ratio)	0.19	0.39	0.00	1.00
Zone characteristics				
Percent with no education in zone	79.20	5.94	30.07	94.83
Percent with 1-6 years of education in zone	11.62	5.24	2.05	37.45
Percent with 7-8 years of education in zone	2.39	1.24	0.20	10.70
District characteristics				
Total area (square km/100)	14.53	9.57	0.00	53.81
Avg. yearly rainfall (mm/100)	11.91	4.05	4.46	20.48
Avg. yearly rainfall squared $(mm/100)^2/100$	1.58	1.02	0.20	4.19
Elevation (m/100)	19.68	4.25	8.65	29.26
Elevation squared $(m/100)^2/100$	4.05	1.65	0.75	8.56
Community characteristics				
Urban (rate)	0.04	0.19	0.00	1.00
Market in area (rate)	0.35	0.48	0.00	1.00
Road access - all year (rate)	0.41	0.49	0.00	1.00
Road access - dry season (rate)	0.39	0.49	0.00	1.00
Population / 1000	3.23	5.28	0.35	96.94
Ranking of Zones (Nationally)				
Zone population rank	21.76	9.37	1.00	36.00
Zone urbanisation rank	19.08	8.76	1.00	36.00
Zone percent with no education rank	18.88	9.31	1.00	36.00
Zone Percent with 1-6 years of education rank	17.21	9.73	1.00	36.00
Zone Percent with 7-8 years of education rank	18.59	9.46	1.00	36.00
Ranking of Communites (Within Zones)				
Community population rank	2.27	1.40	1.00	10.00
Number of communities		109		

Notes. Estimated means and standard errors based on sample frame and weights. The ranking of zones is based on the available sample, with 1 assigned to the smallest value and ties are assigned the same value, so that the sum of the ranks is preserved. For communities the ranking is based on the sample available within a zone.

of the ranks is preserved. That is, for a given variable an observation's rank is 1 plus the number of values that are lower than that observation's value. Five variables are ranked at the zonal level for the 36 zones in the sample and one variable is ranked within zones. For zones, the ranked variables are the size of the population, the degree of urbanisation (measured as the percent of the population who live in urban areas) and the percentage of adults with various levels of education (none, primary or 1-6 years, and 7-8 years). These ranks are all based on data from the 1994 Census. The means of the rankings are not all equal to 19 because not all zones have the same

number of communities and because weights are applied to calculate the means. The communities are ranked within each zone by their population size. The maximum number of communities within a zone is ten, while for five zones there is only one community in the survey. Although it would be advantageous to have more information at the community level, the set of possible variables is limited by the lack of information available at that level from published census reports.

4.3 Individual Data

As discussed early, we posit that the impact of family planning on fertility is highly dependent on a woman's schooling. The lower a woman's education, the more likely she is to benefit from access to family planning services (Rosenzweig and Schultz 1989). This is especially so in Ethiopia where injectable contraceptives are the main method. Injectable contraceptives are ideal for women without education because they do not require any user action except the visit to a family planning clinic every 3 months.[10] In addition to the expected larger effect of family planning for women with no education, the age profile of fertility and the effect of other factors on fertility are likely to be different across education groups. Instead of trying to correctly specify how age and other factors affect fertility across education groups, the main sample consists of all women who have ever been married or lived together with a man and who have no education. Among the original sample, 65% of women never attended school. Table 2 shows the descriptive statistics for the individual level data.[11]

The main dependent variable is the number of children a woman had given birth to at the time of the survey (children ever born) which averages just over 4. The large number of births reflects the high fertility rate in Ethiopia, especially considering that the average age of the women in the sample is just over 28 years.[12]

Age is captured using five year age groups with the excluded category being aged 15-19. With

[10]This also makes them attractive for women who do not want to reveal to their partner that they are using contraceptives (Ashraf et al. 2009).

[11]The descriptive statistics for the full sample is available on request.

[12]For comparison the equivalent number for Guatemala is 2.8 and Guatemala has one of the highest total fertility rates in Latin America (Pörtner 2008).

Table 2: Descriptive Statistics for Women
Ages 15-45 With No Schooling

	Mean	Standard Error	Min	Max
Children even born	4.14	2.73	0.00	13.00
Age 20-24	0.17	0.37	0.00	1.00
Age 25-29	0.21	0.41	0.00	1.00
Age 30-34	0.20	0.40	0.00	1.00
Age 35-39	0.18	0.38	0.00	1.00
Age 40-45	0.17	0.38	0.00	1.00
Orthodox	0.54	0.50	0.00	1.00
Muslim	0.26	0.44	0.00	1.00
Community characteristics				
Total area (square km/100)	15.51	10.10	0.00	53.81
Avg. yearly rainfall (mm/100)	11.97	4.25	4.46	20.48
Avg. yearly rainfall squared $(mm/100)^2/100$	1.61	1.05	0.20	4.19
Elevation (m/100)	19.36	4.18	8.65	29.26
Elevation squared $(m/100)^2/100$	3.92	1.62	0.75	8.56
Urban (rate)	0.04	0.20	0.00	1.00
Market in area (rate)	0.35	0.48	0.00	1.00
Road access - all year (rate)	0.41	0.49	0.00	1.00
Road access - dry season (rate)	0.41	0.49	0.00	1.00
Percent with no education in zone	79.97	6.40	30.07	94.83
Percent with 1-6 years of education in zone	11.17	5.42	2.05	37.45
Percent with 7-8 years of education in zone	2.28	1.27	0.20	10.70
Population / 1000	3.08	4.90	0.35	96.94
Access to family planning (rate)	0.18	0.38	0.00	1.00
Observations		1326		

Notes. Estimated means and standard errors based on sample frame and weights.

the high population growth rate in Ethiopia younger cohorts are larger than older cohorts, but the percentage that have married or lived with a partner is smaller for young women compared to older women explaining the lower percentages of the two youngest age groups (15-19 and 20-24) in the sample. Just over half of the women are Orthodox Christian, a quarter are Muslim and the last mainly other Christian. The remaining variables are community characteristics used for the first stage.

Because there is no information on migration of women the definition of access to family planning implicitly assume that a woman has spent her entire life in the area where she was found during the survey. Given the relocation policy in Ethiopia this might be a problematic assumption,

but in the absence of additional information other assumptions would be just as arbitrary. Finally, the survey did not ask for birth histories. It is therefore not possible to directly examine how the timing of births responds to the introduction of family planning.

5 Results

Table 3 presents the results from the determinants of placement estimation. The dependent variable is whether a given community was within 40 kilometers of the nearest family planning program in 1990.[13] Most of the variables have the expected signs. Areas that have a market are statistically significantly more likely to also have access to family planning services. Furthermore, urban areas and areas with easier access, as measured by whether there is road access by car either all year or during the dry season, are more likely to have a program, although the effects are not statistically significant.

The main variables of interest are the rank variables that identify program placement. All instruments are individually statistically significant or close to. Zones with larger population and more urbanised are statistically significantly more likely to have access to a family planning program. Within a zone, communities with relatively smaller population are more likely to have access to family planning programs. For education policy makers appear to target zones with a relatively larger share of people with between 1 and 6 years of education. The effects of the two other education ranking, no education and 7 to 8 years of education, are negative. One interpretation of the education rank variables is that the government was actively trying to place family planning programs in areas where the population is less educated but not overwhelmingly lacking in education. Presumably those with more education are likely to live in areas where there are other means of obtaining family planning services or have lower desired fertility. The F-test for all instruments being jointly equal to zero is 6.14. Despite the low number of observations, the F-test indicates that the instruments perform well.

[13] The first stage results for other cut-off years are available on request. Using immediately surrounding years do not substantially change the results.

Table 3: First Stage Probit – Determinants of Family Planning Program Placement

Variable	Program available in 1990
Total area of district	−0.021
	(0.027)
Rainfall (mm/100)	−0.717**
	(0.338)
Rainfall squared $(mm/100)^2/100$	3.715**
	(1.478)
Elevation (m/100)	0.999**
	(0.425)
Elevation squared $(m/100)^2/100$	−3.050***
	(1.134)
Urban area	0.474
	(0.989)
Market in PA/kebele	0.994***
	(0.368)
Road access - all year	0.296
	(0.558)
Road access - dry season	−0.120
	(0.540)
Percent with no education in zone	−0.018
	(0.165)
Percent with 1-6 years of education in zone	−1.601***
	(0.393)
Percent with 7-8 years of education in zone	3.427***
	(1.003)
PA/kebele population / 1000	0.063
	(0.044)
Constant	0.745
	(14.332)
Ranking of Zones	
Total population	0.155***
	(0.032)
Urbanisation	0.085***
	(0.026)
Percent with no education	−0.126
	(0.078)
Percent with 1-6 years of education	0.785***
	(0.180)
Percent with 7-8 years of education	−0.579***
	(0.139)
Ranking of PA/kebeles within Zone	
Total population	−0.511***
	(0.163)
All ranks equal to zero F(6,96)	6.14***
Observations	109

Notes. * sign. at 10%; ** sign. at 5%; *** sign. at 1%. Weighted probit with robust clustered standard errors in parentheses estimated using Stata's svy command. Dependent variable is whether a family planning program was available within 40 km of community in 1990.

5.1 Effect on Fertility

Table 4 presents the results for the effect of access to family planning in 1990 on the number of children ever born.[14] Models I and II assume that program placement is exogenous and estimate the effect of family planning using OLS. Models III and IV treat program placement as endogenous and use the predicted probability of access to a family planning program from Table 3 as instrument.[15] Models I and III estimate the average effect of access to family planning services on children ever born across all women in the sample. Because the effect of access is likely to vary by age, Models II and IV include interactions between family planning access and the five year age group dummies.

Table 4: Effect of Family Planning Access on
Number of Children Ever Born for Women Without Schooling

	Children Ever Born			
	OLS		Instrumental Variable[a]	
	Model I	Model II	Model III	Model IV
Family planning	−0.687***		−0.892***	
	(0.215)		(0.323)	
Family planning × age 15-19		−0.656**		−1.052**
		(0.288)		(0.412)
Family planning × age 20-24		−0.219		−0.281
		(0.254)		(0.465)
Family planning × age 25-29		−0.302		−0.899**
		(0.236)		(0.448)
Family planning × age 30-34		−0.919**		−0.925
		(0.395)		(0.590)
Family planning × age 35-39		−0.928***		−0.700*
		(0.339)		(0.418)
Family planning × age 40-45		−0.932*		−1.269**
		(0.487)		(0.604)

Notes. * sign. at 10%; ** sign. at 5%; *** sign. at 1%. Robust standard errors clustered at PA level in parentheses. Family planning indicates whether there was a family planning within 40 km in 1990. Additional variables not shown are region dummies, ethnic group dummies, five year age group dummies, dummies for religion, area of wereda, rainfall and rainfall squared of wereda, dummy for urban area, dummy for market in area, and dummies for road access all year and road access only during dry season. Number of observations for all models is 1340. Results for including other explanatory variables are in Table A-1.
[a] Weighted IV estimation using Stata's svy command with family planning access treated as endogenous. In Model III the predicted probability of a family planning program in the area is the instrument. In Model IV the predicted probability of program in area interacted with age dummies are the instruments.

The average effect of access to family planning on children ever born is negative and strongly

[14] Table A-1 shows the full results.

[15] Choosing a different cut-off year does not substantially change the results for years immediately around 1990. The results for other years are available on request.

statistically significant for both OLS and IV. The OLS estimate indicates that providing family planning reduces the number of children ever born by 0.7 children. Taking account of program placement leads to an even larger estimated impact of access to family planning with fertility falling by 0.9 children. Given the sample's average the effect is equivalent to an approximately 20 percent reduction in the number of children born per woman. A possible interpretation the IV estimate being larger than the OLS estimate is that among areas with similar observable characteristics, the government targeted areas with higher fertility first when introducing the program.

The results for access interacted with age groups are less precise than the average effects although most are still statistically significant. The OLS results show that the reduction in number of children is 0.6 for the youngest age group, aged 15-19, smaller and not statistically significant for women between 20 to 29 and then large and statistically significant at just below 1 for women aged 30 to 45. Except for women aged 35 to 39, the IV effects are larger than the OLS effects. For women less than 20 years old taking account of program placement almost doubles the effect of family planning on number of children; the IV result indicates that family planning access decreases the number of children by 1 for the youngest women. In other words, young women substantially delay their child bearing when they have access to family planning. For the oldest age group, women aged 40 to 45, the IV results are also larger than the OLS results. Because few women have children after passing 45 years of age the estimated effect for the oldest age group is a good indicator of the impact of family planning access on completed fertility. According to the IV results access to family planning decreases completed fertility by 1.2 children among women without education. To place this in perspective, women who late received access to family planning will have approximately 5.7 children by the time they end child bearing, whereas women with access for most of their fertile period will have approximately 4.5 children.

For women who have passed first grade or above there is no impact of access to family planning on fertility.[16] OLS results show that the average effect for women with 1 to 5 years of education is 0.1 and the average effect for women with 6 to 12 years of education is 0.05. Using the IV results

[16]Table A-2 presents results for the sample of all women.

the effect for women with 1 to 5 years of education is 0.04 and for women with 6 to 12 years of education the effect is 0.4. Neither result is statistically significant. Using the same age groups as above for the two education groups leads to no consistent results.

5.2 Family Planning or Health Facilities?

An important question is whether the effects on fertility arise from access to family planning services or the concurrent health facilities. Access to health facilities are unlikely to directly reduce fertility but we would expect that health facilities reduce child mortality. A reduction in expected child mortality would in turn allow parents to achieve a desired number of surviving children with fewer births (Sah 1991; Schultz 1997; Wolpin 1997).

In Ethiopia government family planning programs are offered only at health facilities and not as standalone clinics. As Figure 1 shows there is a close correspondence between the presence of health facilities and family planning programs; in 1990 18 percent of women had access to family planning and a health facility while an additional 6 percent had access to only a health facility. The low number of women with access to health facilities only makes it impossible to estimate the effects of access to only health facilities with any degree of confidence. Substituting health facility for family planning service in the models above lead to smaller and less statistically significant effects using OLS.[17] The smaller OLS estimates is a first indication that the effect on fertility is mainly due to access to family planning at facilities and not access to health facilities alone.

Given the close connection between access to health facilities and family planning estimating the effects of health facilities on child mortality does not help to isolate the effect of health facilities on fertility for two reasons. First, if family planning reduces fertility there will be fewer children who can die and more resources will be available to the children born. Secondly, even in the absence of a reduction in fertility, access to family planning can improve the survival chances of children by allowing parents to better space births. In other words, if we observe a reduction in

[17]The IV estimations the first stage for health facilities performs worse than for the family planning with the F-statistics for the instruments jointly equal to zero close to 3. Results for both OLS and IV are available on request.

Table 5: Effect of Family Planning Access on Mortality of Children for Women Without Schooling

	OLS		Instrumental Variable[a]	
	Model I	Model II	Model III	Model IV
		Any Children Died		
Family planning	0.008		−0.008	
	(0.050)		(0.079)	
Family planning × age 15-19		−0.094		−0.111
		(0.088)		(0.138)
Family planning × age 20-24		0.013		0.060
		(0.055)		(0.097)
Family planning × age 25-29		−0.000		0.012
		(0.075)		(0.098)
Family planning × age 30-34		0.007		−0.073
		(0.084)		(0.117)
Family planning × age 35-39		0.010		0.019
		(0.098)		(0.141)
Family planning × age 40-45		0.036		−0.005
		(0.086)		(0.130)
		Number of Dead Children		
Family planning	−0.094		−0.110	
	(0.097)		(0.163)	
Family planning × age 15-19		−0.215		−0.284
		(0.168)		(0.259)
Family planning × age 20-24		−0.122		−0.154
		(0.097)		(0.189)
Family planning × age 25-29		0.023		−0.053
		(0.136)		(0.171)
Family planning × age 30-34		−0.274*		−0.090
		(0.156)		(0.330)
Family planning × age 35-39		−0.264		−0.269
		(0.231)		(0.315)
Family planning × age 40-45		0.155		0.024
		(0.270)		(0.367)
		Share of Children that Died		
Family planning	−0.002		0.011	
	(0.019)		(0.031)	
Family planning × age 15-19		−0.072*		−0.096
		(0.042)		(0.074)
Family planning × age 20-24		−0.015		−0.010
		(0.026)		(0.050)
Family planning × age 25-29		−0.002		0.026
		(0.026)		(0.033)
Family planning × age 30-34		−0.010		0.003
		(0.035)		(0.057)
Family planning × age 35-39		−0.018		−0.003
		(0.037)		(0.049)
Family planning × age 40-45		0.042		0.046
		(0.043)		(0.061)

Notes. * sign. at 10%; ** sign. at 5%; *** sign. at 1%. Robust standard errors clustered at PA level in parentheses. Family planning indicates whether there was a family planning within 40 km in 1990. Additional variables not shown are region dummies, ethnic group dummies, five year age group dummies, dummies for religion, area of wereda, rainfall and rainfall squared of wereda, dummy for urban area, dummy for market in area, and dummies for road access all year and road access only during dry season. Number of observations for all models is 1242. Complete results including other explanatory variables are available on request.

[a] Weighted IV estimation using Stata's svy command with family planning access treated as endogenous. In Model III the predicted probability of a family planning program in the area is the instrument. In Model IV the predicted probability of program in area interacted with age dummies are the instruments.

child mortality there is no guarantee that the reduction will be the result of access to health facilities because access to family planning programs and health facilities are so closely connected.

Despite these issues, the size of reduction in child mortality is interesting both in its own right and because it can still provide an indication of the relative importance of health facilities and family planning. Table 5 presents the estimated effects of access to family planning on three measures of child mortality: whether any of a woman's children have died, the number of children who have died, and the share of children who have died.[18] For the sample of women who have had children, close to 30 percent have had at least one child die, the average number of children who died is 0.57 and 10 percent of children children born have died.[19]

None of the average effects are statistically significant, although negative as expected, except for the OLS estimate of any children died. The reductions in whether a woman has had at least one child die by age group are small, statistically insignificant and many are of the wrong sign. For the number of children that have died, there are statistically significant and negative effects of family planning for women younger than 25, whereas the effects for older women are not significantly significant. Likewise for the share of children who have died, where the only statistically significant effect is for women younger than 20. For older women the effects are small and not statistically significant, although negative (except for women age 40 to 45). The small effects on child mortality and that the effects are concentrated among the youngest women indicate that the reduction in fertility is unlikely to come from access to health facilities. A more convincing explanation is that family planning services reduced fertility and that combined with health facilities in turn lead to lower mortality.

A different approach to determining if health facilities or family planning services are responsible for the reduction in fertility is to examine three outcomes that are mainly influenced by family planning rather than health facilities: age at first birth, recent birth or pregnancy, and unwanted births or pregnancies. Even if lower mortality leads to lower desired fertility, it is, for example, harder to avoid unwanted births or pregnancies unless one has regular access to family planning services. To capture the timing of first birth, the dependent variable is coded one for women who

[18]The corresponding results using health facility access are available on request, but lead to qualitatively similar results.

[19]It should be kept in mind that this includes mortality after age 5 and the sample consists solely of women with no schooling.

Table 6: Effect of Family Planning Access on
Timing of First Birth for Women Without Schooling

	Was 18 Years or Older at First Birth			
	OLS		Instrumental Variable[a]	
	Model I	Model II	Model III	Model IV
Family planning	0.049		0.035	
	(0.058)		(0.082)	
Family planning × age 20-24		−0.061		−0.405
		(0.140)		(0.250)
Family planning × age 25-29		0.047		0.099
		(0.086)		(0.148)
Family planning × age 30-34		0.038		0.093
		(0.111)		(0.191)
Family planning × age 35-39		0.120		0.125
		(0.084)		(0.123)

Notes. * sign. at 10%; ** sign. at 5%; *** sign. at 1%. Linear probability model with robust standard errors clustered at PA level in parentheses. Family planning indicates whether there was a family planning within 40 km in 1990. Additional variables not shown are region dummies, ethnic group dummies, five year age group dummies, dummies for religion, area of wereda, rainfall and rainfall squared of wereda, dummy for urban area, dummy for market in area, and dummies for road access all year and road access only during dry season. Number of observations for all models is 1024; sample consists of women without education who are between 20 years and 39 years of age. Complete results including other explanatory variables are available on request. [a] Weighted IV estimation using Stata's svy command with family planning access treated as endogenous. In Model III the predicted probability of a family planning program in the area is the instrument. In Model IV the predicted probability of program in area interacted with age dummies are the instruments.

are either 18 years or older and have not yet had a child or had their first birth after age 18, otherwise the variable is coded zero. We restrict the sample to women 20 to 39 years of age because very few women 40 or older were exposed by the program early enough for it to have an effect on this outcome. Table 6 presents the results of the linear probability model. For OLS the average effect is small and positive but not statistically significant. The OLS results by age groups are mostly negative despite the expected sign being positive, but again not statistically significant. The IV results are positive with the exception of women aged 20 to 24 for whom the effect is negative and unrealistically large. A potential issue is that a relatively large share of women report being 18 or older when they had their first child and there is surprisingly little variation across age groups, which is likely to be the result of recall error. For women 20 to 24, 58 percent report being 18 or older at their first birth and the same is the case for women age 35 to 39.

Table 7 presents the estimated impact of access to family planning on whether a woman has either had a birth within the last 12 months or is currently pregnant. For both OLS and IV the average effect is negative and statistically significant. The IV results indicate that a woman with

Table 7: Effect of Family Planning Access on
Recent Birth or Pregnancy for Women Without Schooling

	Birth within last 12 months or currently pregnant			
	OLS		Instrumental Variable[a]	
	Model I	Model II	Model III	Model IV
Family planning	−0.063*		−0.071	
	(0.034)		(0.059)	
Family planning × age 15-19		0.095		0.173
		(0.151)		(0.170)
Family planning × age 20-24		0.189**		0.304*
		(0.074)		(0.173)
Family planning × age 25-29		−0.110*		−0.030
		(0.065)		(0.156)
Family planning × age 30-34		−0.114		−0.140
		(0.075)		(0.107)
Family planning × age 35-39		−0.168**		−0.301***
		(0.075)		(0.086)
Family planning × age 40-45		−0.029		−0.056
		(0.056)		(0.077)

Notes. * sign. at 10%; ** sign. at 5%; *** sign. at 1%. Linear probability model with robust standard errors clustered at PA level in parentheses. Family planning indicates whether there was a family planning within 40 km in 1990. Additional variables not shown are region dummies, ethnic group dummies, five year age group dummies, dummies for religion, area of wereda, rainfall and rainfall squared of wereda, dummy for urban area, dummy for market in area, and dummies for road access all year and road access only during dry season. Number of observations for all models is 1021; sample consists of women without education who are between 20 years and 39 years of age. Complete results including other explanatory variables are available on request.
[a] Weighted IV estimation using Stata's svy command with family planning access treated as endogenous. In Model III the predicted probability of a family planning program in the area is the instrument. In Model IV the predicted probability of program in area interacted with age dummies are the instruments.

access to family planning is more than 10 percent less likely to have had a birth within the last 12 months or be currently pregnant compared to a woman without access to family planning. The average effect masks substantial differences across age groups. For women younger than 25 access to family planning increases the chance of a recent birth or pregnancy; the OLS effect for women 20 to 24 is statically significant. For older women the effect of access is negative and generally statistically significant. The IV results show large reductions in the probability of a recent birth or pregnancy with women 30 to 34 are 18 percent less likely and women 35 to 39 are 35 percent less likely with access to family planning.

Finally, Table 8 shows the effects of family planning on the last birth or current pregnancy being unwanted. Control over fertility provides possibly the most direct evidence on whether family planning or health facilities are responsible for the reduction in fertility. To capture control over fertility, women without children are coded as not having had a unwanted birth or pregnancy;

Table 8: Effect of Family Planning Access on
Unwanted Fertility for Women Without Schooling

	Last/Current Pregnancy Unwanted			
	OLS		Instrumental Variable[a]	
	Model I	Model II	Model III	Model IV
Family planning	−0.071		−0.051	
	(0.046)		(0.071)	
Family planning × age 15-19		−0.073		−0.104
		(0.104)		(0.130)
Family planning × age 20-24		−0.039		−0.001
		(0.071)		(0.130)
Family planning × age 25-29		−0.079		−0.034
		(0.071)		(0.132)
Family planning × age 30-34		0.034		0.083
		(0.083)		(0.120)
Family planning × age 35-39		−0.142**		−0.124
		(0.061)		(0.094)
Family planning × age 40-45		−0.113		−0.140
		(0.073)		(0.107)

Notes. * sign. at 10%; ** sign. at 5%; *** sign. at 1%. Linear probability model with robust standard errors clustered at PA level in parentheses. Family planning indicates whether there was a family planning within 40 km in 1990. Additional variables not shown are region dummies, ethnic group dummies, five year age group dummies, dummies for religion, area of wereda, rainfall and rainfall squared of wereda, dummy for urban area, dummy for market in area, and dummies for road access all year and road access only during dry season. Number of observations for all models is 1340. Complete results including other explanatory variables are available on request.

[a] Weighted IV estimation using Stata's svy command with family planning access treated as endogenous. In Model III the predicted probability of a family planning program in the area is the instrument. In Model IV the predicted probability of program in area interacted with age dummies are the instruments.

women who have not had any children have presumably been able to avoid a pregnancy exactly because of access to family planning. The average effects indicate that longer expose to family planning reduces the risk of an unwanted birth or pregnancy but the effects are not statically significant. The results by age group show that mainly older women benefit from family planning in terms of avoiding unwanted fertility. For both women aged 35 to 39 and 40 to 45 there is a substantial reduction in the probability of last birth or current pregnancy being unwanted with the effects statistically significant for women 35 to 39 for the OLS results and for women 40 to 45 for the IV model. That there is a reduction in unwanted fertility among older part of the sample indicates that the reductions in fertility is likely due to family planning access and not health facilities.

6 Conclusion

Despite a substantial interest in family planning programs there is relatively little research on their effectiveness. Given the long lag between implementation and effect researchers are generally forced to use survey data instead of ideal experimental data. The reliance on survey data requires methods for dealing with the problem of potentially non-random program placement. This paper uses a novel set of instruments to estimate the effects of access to family planning on fertility and related outcomes in Ethiopia. The advantages of the instruments, ranking of area characteristics, are twofold. First, they are easy to understand and likely to reflect what policy makers care about while not being directly related to fertility. Secondly, they are easy to create from readily available secondary data like a census or even from the primary data set itself.

Access to family planning substantially reduces the number of children born for women without education. Most of the reduction is concentrated among the youngest and the oldest women. Women younger than 20 with long-term access to family planning have one child fewer than those without access to family planning indicating postponements of births. The reduction in completed fertility, captured by the number of children born to women aged 40 to 45, is almost 1.2 children. This effect is more than twice as larger as found in other studies. There are two likely explanations for this. First, Ethiopia's fertility is high compared to other study countries such as Columbia. Secondly, the role of women's educational attainment is important when examining the effectiveness of family planning; family planning significantly reduces fertility for women with no education, but there is no effect of family planning among women who have gone to school and not properly accounting for this will bias downwards the estimated effect. In addition, many of the women who did not have access to family planning in 1990 will subsequently receive access. The results therefore likely underestimate the true effect of exposure to family planning programs.[20]

Although family planning programs are always offered in conjunction with health facilities we argue that the presence of health facilities is unlikely to explain the reduction in fertility. If

[20]There is unfortunately little scope for determining how severe the underestimation is because the Pathfinder data does not collect birth histories.

health facilities explained the reduction we would expect them to work mainly through reductions in child mortality. We do find reductions in child mortality, but the size of the reductions and their distribution across age groups makes the reductions more likely to be the result of reduced fertility than a strong impact of health facilities. Furthermore, women with access to family planning are less like to have their first birth before they turn 18. Women are also less likely to have had a recent birth or pregnancy and the last birth or pregnancy is more likely to be wanted for older women with access to family planning. All of these results indicate it is not the health facility but rather the access to family planning that is the dominant explanation for the reduction in fertility.

The age pattern of the effects on fertility is evidence of a strong compression of the timing of births with access to family planning. One might a priori expect a uniform reduction in births across age or that the cumulative effect would become larger with increasing age. Instead there are large reductions for women younger than 20 and women older than 30. Because the outcome is cumulative births, the women with access to family planning must have had more children in their twenties than those without access to family planning.[21]

The compression of births is important for three reasons. First, it is another indication that family planning rather than health facilities is responsible for the reduction in fertility. Without access to modern contraceptives the main way to reduce fertility is to ensure that the space between births is as long as possible. Only with added control over the timing of births and completed fertility would it be optimal for a woman to have children more closely spaced. With modern contraceptives it is possible to control the timing of births and stop child bearing completely when desired. Secondly, with closer spacing of children women can spend more time in the labour market thereby increasing the amount of resources available to the household. Finally, the compression of births into a shorter period of time allows women to investment more in human capital.

The reduction in births with access to family planning in Ethiopia, although large compared to

[21] Women aged 15 to 19 are estimated to have 1 birth less with access compared to without access. Women aged 20 to 24 with access have only 0.5 births less than those without access. In other words, compared to those without access to family planning, women with access have, on average, given birth to 0.5 children *more* from turning 20 until they turn 25. Say, a woman without family planning have 1 child between 15 and 19 and an additional 1.5 child between 20 and 25, then a woman with access will have 0 children between 15 and 19 and 2 children between 20 and 25.

previous studies, is not sufficient to reduce fertility to replacement level. The results here indicates that the reduction in fertility is mainly due to the avoidance of unwanted births, especially births early and late in a woman's fertile years. Our estimated effect of family planning does not take into account that a potentially important impact of family planning access is that it allows young women to continue going to school longer than they would otherwise be able to. The resulting increase in schooling will in turn lead to a even larger reduction in fertility and better health for both mother and children. Family planning may not be a panacea for rapid population growth but it does substantially reduce fertility and, maybe even more importantly, it provides women with increased control over the timing of births. Clearly providing access to family planning is an effective means of improving the welfare of women.

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A Appendix

Table A-1: Estimated Effect of Family Planning Access on Children Ever Born for Women Without Schooling

	OLS		IV[a]	
	Model I	Model II	Model III	Model IV
Age 20-24	1.054***	1.010***	1.041***	0.953***
	(0.148)	(0.166)	(0.147)	(0.164)
Age 25-29	2.538***	2.473***	2.544***	2.521***
	(0.156)	(0.184)	(0.158)	(0.195)
Age 30-34	3.862***	3.906***	3.867***	3.848***
	(0.164)	(0.176)	(0.164)	(0.193)
Age 35-39	5.112***	5.163***	5.117***	5.054***
	(0.191)	(0.205)	(0.192)	(0.227)
Age 40-45	5.757***	5.819***	5.772***	5.837***
	(0.234)	(0.264)	(0.236)	(0.274)
Orthodox	−0.321	−0.318	−0.320	−0.328
	(0.267)	(0.268)	(0.267)	(0.269)
Muslim	0.131	0.136	0.143	0.127
	(0.240)	(0.240)	(0.238)	(0.240)
Total area	0.008	0.007	0.008	0.007
	(0.009)	(0.009)	(0.010)	(0.010)
Avg. yearly rainfall (mm/100)	−0.180	−0.185	−0.206	−0.213
	(0.132)	(0.130)	(0.132)	(0.132)
Avg. yearly rainfall^2/100	0.716	0.737	0.830	0.852
	(0.537)	(0.529)	(0.546)	(0.546)
Elevation (m/100)	0.030	0.034	0.045	0.046
	(0.136)	(0.136)	(0.139)	(0.139)
Elevation^2/100	−0.023	−0.036	−0.067	−0.073
	(0.332)	(0.330)	(0.336)	(0.337)
Urban	0.343	0.272	0.433	0.396
	(0.328)	(0.300)	(0.352)	(0.338)
Market in area	−0.044	−0.036	−0.023	−0.027
	(0.154)	(0.152)	(0.159)	(0.157)
Road access - all year	0.129	0.141	0.113	0.113
	(0.214)	(0.208)	(0.219)	(0.218)
Road access - dry season	0.269	0.266	0.249	0.241
	(0.219)	(0.216)	(0.219)	(0.218)
Percent with no education in zone	−0.082	−0.082	−0.090	−0.094*
	(0.056)	(0.056)	(0.055)	(0.056)
Percent with 1-6 years of education in zone	−0.088	−0.089	−0.093	−0.098
	(0.069)	(0.069)	(0.068)	(0.070)
Percent with 7-8 years of education in zone	−0.086	−0.072	−0.105	−0.106
	(0.181)	(0.181)	(0.178)	(0.181)
PA/kebele population / 1000	−0.030**	−0.029**	−0.030**	−0.030**
	(0.012)	(0.012)	(0.012)	(0.012)
Family planning	−0.687***		−0.892***	
	(0.215)		(0.323)	
Family planning × age 15-19		−0.656**		−1.052**
		(0.288)		(0.412)
Family planning × age 20-24		−0.219		−0.281
		(0.254)		(0.465)
Family planning × age 25-29		−0.302		−0.899**
		(0.236)		(0.448)
Family planning × age 30-34		−0.919**		−0.925
		(0.395)		(0.590)
Family planning × age 35-39		−0.928***		−0.700*
		(0.339)		(0.418)
Family planning × age 40-45		−0.932*		−1.269**
		(0.487)		(0.604)
Constant	9.689	9.601	10.402*	10.898*
	(6.379)	(6.268)	(6.277)	(6.326)
R^2	0.500	0.502	0.500	0.500
Observations	1326	1326	1326	1326

Notes. * sign. at 10%; ** sign. at 5%; *** sign. at 1%. Robust standard errors clustered at community level in parentheses. Family planning indicates whether there was a family planning within 40 km in 1990. Additional variables not shown are region dummies and ethnic group dummies.

[a] Weighted IV estimation using Stata's svy command with family planning access treated as endogenous. In Model III the predicted probability of a family planning program in the area is the instrument. In Model IV the predicted probability of program in area interacted with age dummies are the instruments.

Table A-2: Estimated Effect of Family Planning Access on Children Ever Born using All Education Groups

	OLS				Instrumental Variable[a]			
	Model I	Model II	Model III	Model IV	Model V	Model VI	Model VII	Model VIII
Family planning	−0.394**	−0.620***			−0.505	−0.739**		
	(0.169)	(0.211)			(0.306)	(0.332)		
Family planning		0.763**				0.883		
× 1-5 years of education		(0.340)				(0.541)		
Family planning		0.505**				0.534		
× 6-12 years of education		(0.237)				(0.345)		
Family planning × age 15-19			−0.257	−0.600**			−0.754**	−0.889**
			(0.277)	(0.291)			(0.337)	(0.389)
Family planning × age 20-24			−0.348	−0.116			−0.479	−0.055
			(0.215)	(0.236)			(0.394)	(0.440)
Family planning × age 25-29			−0.038	−0.255			−0.394	−0.731
			(0.183)	(0.233)			(0.383)	(0.455)
Family planning × age 30-34			−0.700**	−0.829**			−0.369	−0.694
			(0.298)	(0.395)			(0.492)	(0.613)
Family planning × age 35-39			−0.459*	−0.855**			−0.276	−0.535
			(0.272)	(0.342)			(0.357)	(0.424)
Family planning × age 40-45			−0.735	−0.883*			−1.060*	−1.162*
			(0.455)	(0.495)			(0.612)	(0.621)
Family planning × Age 15-19				0.820				0.317
× 1-5 years of education				(0.604)				(0.566)
Family planning × Age 20-24				−0.647*				−0.913*
× 1-5 years of education				(0.343)				(0.487)
Family planning × Age 25-29				0.914				0.728
× 1-5 years of education				(0.558)				(0.953)
Family planning × Age 30-34				0.580				2.069*
× 1-5 years of education				(0.756)				(1.147)
Family planning × Age 35-39				1.256*				1.017
× 1-5 years of education				(0.698)				(0.815)
Family planning × Age 40-45				1.248				6.137
× 1-5 years of education				(1.039)				(5.441)
Family planning × Age 15-19				0.722				0.519
× 6-12 years of education				(0.447)				(0.599)
Family planning × Age 20-24				−0.255				−0.485
× 6-12 years of education				(0.320)				(0.491)
Family planning × Age 25-29				0.067				0.754
× 6-12 years of education				(0.347)				(0.593)
Family planning × Age 30-34				0.221				0.158
× 6-12 years of education				(0.575)				(0.853)
Family planning × Age 35-39				1.639**				1.541
× 6-12 years of education				(0.743)				(1.240)
Family planning × Age 40-45				2.204				−2.325
× 6-12 years of education				(1.390)				(5.092)
Observations	2051	2051	2051	2051	2051	2051	2051	2051

Notes. * sign. at 10%; ** sign. at 5%; *** sign. at 1%. Robust standard errors clustered at PA level in parentheses. Family planning indicates whether there was a family planning within 40 km in 1990. Additional variables not shown are region dummies, ethnic group dummies, five year age group dummies, dummies for religion, area of wereda, rainfall and rainfall squared of wereda, dummy for urban area, dummy for market in area, and dummies for road access all year and road access only during dry season.

[a] Weighted IV estimation using Stata's svy command with family planning access treated as endogenous. Instrument for Model V is the predicted probability of a family planning program in the area. Instruments for Model VI are predicted probability of program in area and its interaction with dummies for the two education levels. Instruments for Model VII are predicted probability of program in area interacted with age dummies. Instruments for Model VIII are predicted probability of program in area interacted with age dummies and age dummies interacted with dummies for the two education levels.

Economic Incentives for Sex-Selective Abortion in India

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Abstract

This paper uses a new technique to estimate the economic gains to households who selectively abort girls. Parents in India do not selectively abort their first pregnancy; hence, the sex outcome of the first pregnancy is a natural experiment. Parents who have first-born girls, who instead had used sex-selective abortion to have a boy, will, absent the costs of selective abortion, have the same outcomes as parents who have first-born boys. The paper uses a detailed economic survey to provide the first estimates of the magnitude of the economic benefits for parents' use of sex-selective abortion including the gains to per capita income, expenditure, household assets, and the probability the household is below the poverty line. The significant benefits of a son relative to a daughter help to explain the high levels of sex discrimination in India. Household labor supply gains from sons and higher labor supply from fathers of sons helps to explain why sons yield such large economic benefits for parents.

Keywords: Sex-Selective Abortion; India; Labor Supply
JEL Classification Numbers: J13, J16, O12.

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Several recent papers estimate the number of sex-selective abortions in India. Arnold et al. (2002) and Jha et al. (2006) recently estimated 100,000 and 500,000 selective abortions respectively per year in India. With annual births around 27 million, between 0.4 and 1.8 percent of pregnancies are terminated because of sex-selection. These numbers combined with excess female mortality are enough to skew the sex ratio of men to women far above the rates seen in developed countries. Whatever the exact number, the increasing male-female sex ratio in India is a major concern for policy makers. Recently the Indian government introduced programs to provide monetary incentives for parents to have daughters and keep their daughters alive. For example, the *Apni Beti Apna Dhan* (Our Daughter, Our Wealth) program in the state of Haryana gives parents cash when a daughter is born plus a long-term savings bond if the daughter survives to age 18 and is unmarried (Sinha and Yoong, 2009).

Although the idea that sex-selection occurs for economic reasons has been hypothesized, this paper is the first to estimate the magnitude of the economic benefits from a son relative to a daughter in India. In the long-run, the net benefits of sons and daughters must equalize for there to be equality between the sexes. As a short-run measure, policy makers are subsidizing the birth of girls, and this paper informs policy makers as to the necessary size of such subsidies if they want to reduce selective abortion. Furthermore, the paper points to specific types of households that have stronger economic gains from a son so that policy makers can target specific population groups that are likely to have the largest incentives to discriminate against girls.

Poor households in India face stark economic realities and having a son instead of a daughter may mean living in or out of poverty. Although this paper will not determine the exact mechanisms which cause sons to be so valuable, it will investigate changes in household labor supply as one plausible mechanism. Sons in India tend to stay with their parents in a joint household even after they marry, bringing in income as well as a daughter-in-law. This means that a son's income will flow to his parents while a daughter's income will flow to her in-laws. Furthermore, because parents know this

they will invest less in their daughters than their sons, causing men to earn more in the labor market. The findings below are consistent with this story. Parents that have a son instead of a daughter have more workers in the household when the parents are older. Parents have a lower labor supply when older, but per capita income is higher, indicating both leisure and economic gains from having a son in old age. Consistent with other research on parents' labor supply reactions to having a son over a daughter, if rural fathers have a son instead of a daughter, the father works and earns more when children are young (Lundberg and Rose, 2002). There is no effect on the mother's labor supply when children are young, which is particularly surprising since having a son in India usually means fewer children and, thus, a high probability of fewer child rearing responsibilities. Understanding this labor supply mechanism can help policy makers to address the underlying economic causes of sex-selective abortion.

Parents who use sex-selective abortion also have fewer children, so per capita income and expenditures could rise for this reason when children are young. (When children marry, parents of sons gain household members, while parents of daughters lose household members.) Appendix A shows that under a wide range of equivalence scales, parents that use sex-selective abortion still have higher income and expenditure.

Another benefit of having a son in India is that it is common for parents to pay for the marriage of their daughter and give a large dowry to the groom's household. This one time transfer of assets may allow households that use sex-selective abortion to avoid having to save (or go into debt) to pay for a daughter's marriage. While households are younger, parents of daughters may be spending less per capita than parents of sons because these parents do not have to save for a dowry. I will not be able to say how important or unimportant dowries are for causing sex-selective abortion, but it is certainly an additional mechanism that could be causing sons to give greater economic benefits than daughters.

There are large economic gains to households from sex-selective abortion, but also heterogeneity across regions and demographic groups. A sex-selective abortion increases per

capita household income on average across India by about 7 percentage points, increases household per capita expenditure by about 2 percentage points, increases household assets by about 2 percentage points, and decreases the probability the household is in poverty by about 1 percentage point. These results change when households are younger or older, parents have high or low education, live in rural or urban areas, or have different religions or castes. The average gain from sons is larger in some regions of India than others (for example the per capita expenditure gain from a son is 5.4 percentage points higher in the North than in the South). However, once household-level controls are added, much of the differences across regions disappear. One explanation is that differences in population characteristics across India that make sons economically valuable is the cause of differences in discrimination against girls in India, rather than any inherent cultural differences across India.

Last, the paper shows that the benefits from sons diminishes as the household has more sons. Conditional on having one son, a household gets no economic benefit from a second son. This result helps to explain why parents generally delay having a sex-selective abortion until the second or third pregnancy because they only get a significant benefit from the first son. This result also explains why parents who already have one son do not appear to use sex-selective abortion for later pregnancies.

1 Background

Although there has been much research that finds bias against girls and estimates the large number of sex-selective abortions and excess female mortality in South Asia (Visaria, 1969; Basu, 1989; Sen, 1990; Coale, 1991; Klasen, 1994; Hazarika, 2000; Asfaw et al., 2007; Anderson and Ray, 2008), there has been little research trying to determine the specific economic benefits of sons in India. Rosenblum (2010), for example, hypothesizes that the future economic benefits of sons and costs of daughters lead to fertility decisions that exacerbate discrimination against girls. However, these potential benefits from sons

are not deeply explored. Rosenzweig and Schultz (1982) argue that the relatively higher future wages of sons cause parents to invest in sons over daughters in India. This is the first paper to estimate the size of the benefits of sons in India and link them to sex-selective abortion.

There has been research in other parts of the world on the benefits of sons over daughters. Ding and Zhang (2009) and Ebenstein (2009) both estimate the value of a son over a daughter in China. Ding and Zhang find that a son increases the amount of investment in household agriculture and business, while Ebenstein estimates that a son is worth an extra 1.42 years of income to a household, although the paper does not determine why a son is worth so much. Qian (2008)'s research is along the same lines as Rosenzweig and Schultz and finds that labor income differences between men and women can explain some of the mortality differences in China. Edlund et al. (2007) examine the larger social externalities of the high male-female sex ratio in China, finding that an increase in men causes an increase in crime.

Son preference is not restricted to developing countries. This paper investigates changes in household labor supply in response to sex-selective abortion, and like Lundberg and Rose (2002)'s investigation of parents' labor supply in the US, finds that fathers work more if they have a son instead of a daughter. This finding is also consistent with Dahl and Moretti (2008) who find that fathers in the US are more likely to remain in a marriage if they have a son. This paper's findings are the opposite of Angrist and Evans (1998) who find that mothers in the US increase their labor supply if they have fewer children, while the father's labor supply does not change.

Although this paper calculates the economic benefit it does not estimate the economic cost of sex-selective abortion. Arnold et al. (2002) cite figures of the cost of sex detection via ultrasound at around US$10-$20 per test and Ganatra and Hirve (2002) finds that in rural Maharashtra the cost of an abortion (in private clinics) costs around US$10 in the first trimester and US$30 in the second trimester. Although these costs seem low, these are still significant costs in a country with a large fraction of the population living on less

than $1.25 a day. Households who live farther away from medical services (rural areas) have larger costs than households who have better access. Another cost is the health risk to the mother, which is larger the lower the quality of the health provider (and there is surely an inverse relationship between health risk and the direct medical cost). Part of the opportunity cost of abortion is giving birth, which has its own economic and health costs and could be very similar to the direct costs of sex selective abortion.[1] This paper cannot estimate the specific costs to households of sex-selective abortion, and thus cannot estimate the net benefit of a son versus a daughter. Nonetheless, determining the gross benefits will give important information about the incentives causing parents to use sex-selective abortion.

2 Estimation Strategy

The goal of this paper is to estimate the economic benefits of having a son versus a daughter. As shown in Rosenblum (2010), Ebenstein (2007), and Portner (2010), in India first births have a biologically normal sex ratio, i.e. parents do not use sex selective abortion for their first pregnancies. Sociological qualitative studies also provide evidence that parents do not mind having a first-born daughter, but they have a strong preference for sons after this first birth (Patel, ed, 2007). The population weighted ratio of male to female first-births in the data used for the economic analysis in this paper is about 1.09 with a 95% confidence interval of 1.05-1.13. 1.09 is larger than the 1.03-1.07 range considered biologically normal (Chahnazarian, 1988). However, even with more than 24,000 obeservations, the sample size is too small to reject the hypothesis that the sex ratio at birth is normal. The reason that a large sample size is needed to accurately estimate proportions can be seen from the formula to calculate 95% confidence intervals:

$p \pm 1.96\sqrt{\frac{p(1-p)}{n}}$

where p is the estimated proportion and n is the number of observations. Assuming

[1] The 2007-2008 Indian DLHS 3 has responses from 217,986 women about the cost of their most recent delivery. The sample-weighted mean cost of delivery is about Rs. 2200.

the estimated proportion was exactly 1.05 males per female (or 51.2% male births), one would need more than 40,000 observations to get a 95% confidence interval within the 1.03-1.07 range. If the estimated proportion was exactly 1.06 males per female, one would need more than 170,000 observations to get a 95% confidence interval within the 1.05-1.07 range. The closer the actual estimated proportion is to 1.07, the more observations are needed to distinguish the estimated proportion from being above 1.07. The larger data sets as used in Rosenblum (2010) and Portner (2010) find that the sex ratio at birth for more recently born first-borns is biologically normal in India, noting the possibility of recall and survival bias pulling up the sex ratio at birth for births many years prior to the survey. For example, Rosenblum finds that in the District Level Household and Facility Survey (DLHS) 2 from 2002-2003[2], with around 500,000 households, the sex ratio at birth for all first borns is 1.089 with a 95% confidence interval of (1.083, 1.095). However, for first-borns born within ten years of the survey, the sex ratio at birth is 1.066 with a 95% confidence interval of (1.056, 1.076). Rosenblum argues that if recall and survival bias is the cause of higher sex ratios for older births (which is likely given the lack of selection technology available when the sex ratios are high), it will tend to make first-born boy households appear worse relative to first-born girl households, dampening any positive effect found from the birth of a son. In particular, parents of first-born girls tend to have a higher level of education. Portner uses the Indian National Family Health Surveys and finds no evidence that there is sex-selection for first births, even for the group mostly likely to use sex selection (highly educated and urban).

Since I do not want to replicate what has been done in other papers, I urge the reader to refer to the previous studies for more detailed evidence of the absence of sex-selection for first-births in India. However, I will provide some new evidence, that again supports this assumption. The DLHS 3 from 2007-2008, which has a sample size of over 600,000 ever-married women, asks about birth outcomes since 2004, including induced abortions of each of these pregnancies. It is representative at the district level and covers all of

[2]This is the only round of the DLHS that contains a full birth history of respondents

India. For first-births, zero women report induced abortion. For second-births, about 0.36 percent of women report induced abortion. The proportion of induced abortions is also about 0.36 percent for third-order births. These numbers are likely an underestimate of the actual number of induced abortions, since abortion is not always legal. However, it is telling that literally zero respondents report induced abortion for first pregnancies and some do report them for higher order births. I show below that there is little difference between parents who have a first-born boy or first-born girl in terms of age and education. I also show below that there is little difference between parents who have a first-born boy or first-born girl in terms of assets or poverty status for younger households, indicating that households who have a first-born boy are not initially wealthier than households with a first-born girl. Ideally, one would like to look at parents who just had their first son and parents who just had their first daughter and test for economic differences. The small sample size of these households makes any difference difficult to detect. For example there are around 300 households whose first-born child is one year old or younger and there is no detectable difference in economic outcomes for those who have a son versus those who have a daughter.

Given the large amount of evidence from several research papers that there is not sex-selection at the first birth parity, we can treat a first birth as a natural experiment and ask what would have happened if parents had selectively aborted their first-born daughters. Households with a first-born girl, if they had used selective abortion (ignoring any direct costs of the selective abortion itself) would on average look exactly like households with a first-born boy. Thus, we can estimate the following equation:

$$Y_i = \beta_0 + \beta_1 FirstBoy_i + e_i \tag{1}$$

where Y_i is the economic variable of interest, $FirstBoy$ is a 0/1 variable equal to 1 if household i has a first-born boy, and e_i is the error term. β_1 is the estimate of the value

of a son compared to a daughter, i.e. the estimated value of using sex-selective abortion to attain a son instead of a daughter. One problem with this estimation strategy, as indicated above, is that there is recall and survival bias in the data that causes first-born girl households to appear to be better off than they really are. Recall bias occurs when parents do not accurately report their birth history. In particular, parents fail to report daughters who died when very young. Survival bias happens because mothers of first-born girls have more children on average than mothers of first-born sons. Since death during childbirth is not uncommon in India, this difference in number of children means that mothers of first-born girls are more likely to die than mothers of first-born boys and, therefore, are not available to be surveyed. In either case, the worst-off mothers of first-born girls are the ones not being surveyed or recording the wrong sex of their first-born child. The bias will cause parents of first-born girls to appear better off on average than they really are. Thus, β_1 is an underestimate of the true economic benefits of sex-selective abortion.

In order to reduce this bias, household characteristics are included in the estimation, expanding the equation to:

$$Y_{ij} = \beta_0 + \beta_1 FirstBoy_{ij} + \alpha X_{ij} + S_j + e_{ij} \tag{2}$$

where X_{ij} are characteristics for households i in state/territory j, including parents' age, years of schooling, religion, caste, and whether the household is in a rural or urban part of India. S_j are state/territory fixed effects.

3 Data Description

To undertake this analysis I use the detailed economic data from the 2005 India Human Development Survey (IHDS). The data is nationally representative of Indian households and covers 41,554 households in 1504 villages and 970 urban neighborhoods in 33 states

and union territories. It contains detailed economic and demographic information. I drop households if the head of household has no children, birth history was not recorded for the head's wife or female head of household, of if they have first-born twins, bringing the sample size down to 24,396 households.

4 Descriptive Statistics

Table 18 shows population-weighted means for IHDS household variables separated into male and female first-birth households. All of these variables should be independent of the sex outcome of a first birth. For these variables there are almost no statistically significant differences between first-born boy and first-born girl households. This table indicates that if there is bias in the dataset, it is likely small. The one exception is the proportion of households who are Muslim, where first-born boy households are 13 percent Muslim and first-born girl households are 11 percent Muslim. One possibility is that Muslim households are using sex-selective abortion for first-pregnancies. Another possibility, given the large number of variables, is that this just represents a false positive.

Table 1: Mean Independent Variables by First-Birth Outcome

Variable	**First-Born Boy**	**First-Born Girl**	**Difference**
Mother's Age (years)	35.2	35.3	-0.1
Father's Age (years)	40.5	40.4	0.1
Mother's Education (years)	3.71	3.66	0.05
Father's Education (years)	5.98	5.95	0.03
Rural (0/1)	0.69	0.69	0.00
Muslim (0/1)	0.13	0.11	0.02*
Christian (0/1)	0.01	0.02	-0.01
Sikh, Jain (0/1)	0.01	0.02	-0.01
Brahmin (0/1)	0.05	0.05	0.00
High Caste (0/1)	0.15	0.15	0.00
Scheduled Caste (0/1)	0.23	0.23	0.00
Scheduled Tribe (0/1)	0.07	0.08	-0.01
Other Backward Classes (0/1)	0.35	0.36	-0.01
N	12752	11644	

Notes: No households with first-born twins. Sampling weights used.
* indicates significant at the 5% level using t-tests for age and education, and Pearson chi-squared tests for the binary variables.

5 The Economic Gains from Sex-Selective Abortion

This paper estimates the effect of a sex-selective abortion on the (natural) log of per capita yearly income (PCI) in Indian rupees, log of per capita monthly expenditure (PCE) in Indian rupees, as well as a household assets index, and whether the household is below or above the poverty line. There is a strong economic argument for using expenditure as a measure of long-term household welfare (Deaton, 1997). I include income as well, even though it may represent a more short-term measure of welfare as well as having potentially more difficult measurement problems. I also use per capita instead of total income or expenditure to account for differences in household composition (Datta and Meerman, 1980). The household assets index is based on the household's ownership of durable items (vehicle, sewing machine, computer, etc.) and the quality of the home (flush toilet, quality of the walls, roof, and floor, electricity, etc.). The index ranges from 0 to 30 with 30 indicating the highest level of assets. This asset index can be thought of as a measure of household wealth. As argued by Filmer and Pritchett (2001), an asset index can also be thought of as a long-term measure of household welfare. The poverty line is determined by the Indian Planning Commission's official per capita expenditure poverty lines for 2005, which varies by location. Thus, the paper not only gives estimates of an improved household economy from a son, but the probability that a household is pushed out of poverty by having one.

Table 2 presents the mean dependent variables of interest, split by the sex of the first-born child. On average, households with a first-born son have higher mean economic outcomes than households with a first-born daughter (although there is a only a statistically significant difference for per capita income). The mean household in the IHDS is very poor with per capita annual income around Rs. 9,700 ($620 in 2005 USD using World Bank PPP), per capita monthly expenditures around Rs. 850 ($54), and around 22 percent of households living below the poverty line.

Table 3 shows initial estimates of the average economic benefit across India of a house-

hold using sex-selective abortion. A selective abortion at the first pregnancy increases per capita yearly income by about 6.9 percentage points, increases per capita monthly expenditure by 2.2 percentage points, reduces the probability that the household is below the poverty line by 0.7 percentage points and increases the household asset index by about 0.2. The results indicate a strong positive effect on the household economy of having a son instead of a daughter.

Table 2: Mean Dependent Variables by First-Birth Outcome

Variable	**First-Born Boy**	**First-Born Girl**	**Difference**
Annual Per Capita Income (Rs.)	9952	9441	511*
Monthly Per Capita Expenditure (Rs.)	853	832	21
Below Poverty Line (0/1)	0.21	0.23	-0.02
Household Asset Index	11.2	11.1	0.1
N	12752	11644	

Notes: No households with first-born twins. Sampling weights used.
* indicates significant at the 5% level using t-tests for PCI, PCE, and the Asset Index, and a Pearson chi-squared test for the Below Poverty Line binary varibale.

Table 3: OLS: Effect of a son versus a daughter on the household economy

	log(PCI)	**log(PCE)**	**Asset Index**	**Below Poverty Line**
First-Born Boy	0.069**	0.022**	0.196**	-0.007†
	(0.013)	(0.007)	(0.044)	(0.004)
Parents' Age and Education	Yes	Yes	Yes	Yes
Urban/Rural Dummy	Yes	Yes	Yes	Yes
Religion Dummies	Yes	Yes	Yes	Yes
Caste Dummies	Yes	Yes	Yes	Yes
State Dummies	Yes	Yes	Yes	Yes
N	22251	22524	22540	22540
Clusters	33	33	33	33
R^2	0.38	0.45	0.66	0.20

Significance levels : † : 10% * : 5% ** : 1%
Notes: Robust standard errors clustered by state in parentheses.

That I use per capita measures instead of total household income and expenditure means that I ignore potential economies of scale in the household.[3] As in Rosenblum (2010), a first-born son predicts more total children born (about 2/5 of a child more in

[3]See Deaton (1997) for a discussion of the various problems of calculating per capita equivalents.

the IHDS data for mothers aged 35 and older). However, once the children grow up, daughters leave the household, while sons remain and bring their wives and children into the household. For example, a household with one son will start out with three family members (mother, father, son) and then increase to four with the addition of the son's wife and then continue to increase as the son has children. By contrast, two parents with four daughters will start off with six household members, but then shrink to two as all the daughters get married and join different households. Thus, when looking at per capita outcomes, the life stage of the household should be taken into account. Table 4 shows the effect of a first-born son on the number of children (aged 0-14), number of teens (15-21), and number of adults (21+). The sample is stratified by the mother's age (under 35, between 35 and 45, and above 45). The change in household structure over time follows the above story. A first-born son reduces the number of children for mothers under 45, but for older mothers, the son has no effect on number of children in the household. Presumably, this is due to the son having children of his own at this point. The first-born son only increases the number of adults in the household as the household gets older. A young son will not affect the number of adults, but as the household ages, the son marries and brings in a daughter-in-law, while the daughters leave, causing a rise in the number of adults. On net, a first-born son reduces the number of household members when the household is young and raises it when the household is old. These changes in household structure mean that if household economies of scale are important, the per capita economic benefits of a son will be overestimated for young households and underestimated for older households. A further discussion on the implications of and the robustness of the estimates to different measures of economies of scales can be found in Appendix A.[4]

[4]Panel data exists connecting a subset of households from the 1993-1994 Human Development Profile of India (HDPI) survey to the IHDS. Using it, I have found that household size for heads of household with a first-born son are smaller than those with a first-born daughter when the household is young and larger when the household is old. The small sample size, splitting of households and difficulty linking individuals between surveys, and inconsistency of economic questions between surveys meant only a rough panel data analysis could be done on economic outcomes. This panel data analysis could not detect economic differences in first-born boy and firstborn-girl households over time at a statistically significant level.

Table 4: OLS: Effect of a son versus a daughter on household size and age structure

	Coefficient on First-Born Boy			
	Children	**Teens**	**Adults**	**Total Persons**
Mother Age < 35	-0.18**	0.01	-0.01	-0.18**
	(0.02)	(0.01)	(0.01)	(0.03)
N	10289	10289	10289	10289
35 ≤ Mother Age < 45	-0.25**	0.08**	0.15**	-0.03
	(0.04)	(0.02)	(0.02)	(0.04)
N	9341	9341	9341	9341
Mother Age ≥ 45	0.03	-0.11*	0.38**	0.30**
	(0.04)	(0.05)	(0.05)	(0.09)
N	2910	2910	2910	2910

Significance levels : † : 10% * : 5% ** : 1%

Notes: Robust standard errors clustered by state in parentheses, all regressions include the following independent variables: parents' age and education, caste/religion, urban/rural and state.

As households age not only does the household structure change, but the economic situation changes as well. In particular, younger households should have fewer assets and less immediate income and wealth gains from sons. However, the birth of a son could cause an immediate increase in household spending because parents have stronger incentives to invest in a son than a daughter and also because parents do not have to worry about saving for a dowry or retirement. I split the sample by mother's age in Table 5. The estimates indicate that per capita expenditure gains occur early in the household's lifecycle. Per capita income rises the most for mothers between 30 and 40. The largest asset gains and reductions in poverty are for older households. These results make particular sense in the context of dowries. Parents with sons can spend more early in life, since they will gain a large asset in the form of a dowry payment when their son marries. Parents with a daughter, on the other hand, must reduce spending and save for their daughter's marriage when they are young. The results are also consistent with the labor supply findings below: fathers work more if they have a son and sons bring extra income to the household when they grow up. Per capita income and expenditure gains fall as the household ages and after the son marries since the new daughter-in-law and children in the joint household dilute economic gains from sons.

Table 5: OLS: Effect of a son versus a daughter, stratified by mother's age

	log(PCI)	**log(PCE)**	**Asset Index**	**Below Poverty Line**
Mother Aged < 30	0.044**	0.019†	0.024	-0.000
	(0.014)	(0.010)	(0.074)	(0.010)
N	5534	5596	5599	5599
30 ≤Mother Aged < 35	0.117**	0.049**	0.189*	-0.025*
	(0.018)	(0.012)	(0.080)	(0.012)
N	4637	4689	4690	4690
35 ≤Mother Aged < 40	0.085**	0.024	0.104	-0.005
	(0.032)	(0.017)	(0.108)	(0.005)
N	5182	5254	5257	5257
40 ≤ Mother Aged < 45	0.047†	-0.001	0.423**	0.010
	(0.023)	(0.018)	(0.136)	(0.008)
N	4032	4079	4084	4084
Mother Aged ≥ 45	0.044	0.013	0.352**	-0.020*
	(0.031)	(0.024)	(0.128)	(0.009)
N	2866	2906	2910	2910

Significance levels : † : 10% * : 5% ** : 1%

Notes: Robust standard errors clustered by state in parentheses. Coefficients not shown for the following independent variables parents' age and education, caste/religion, and state.

6 Heterogeneous Effects of Sex-Selective Abortion

The above estimates show the effect of sex-selective abortion on households on average and over different age structures. However, different types of households likely benefit in different ways from having sons (if they benefit at all). This section divides the households into subsamples based on household characteristics and location. Jha et al. find more sex-selective abortions in households with more education and in some regions of India more than others (e.g. northern India has many more sex-selective abortions than southern India). The analysis below examines whether the economic incentives from sons matches up with the types of households that are using selective abortion.

6.1 Urban and Rural

Table 6 estimates the value of a sex-selective abortion stratified by urban and rural households. The point estimates are larger for rural households and they are statisti-

cally significant at the 5 percent level for all of the household economic outcomes except whether the household is below the poverty line, while for urban households, the coefficient is only statistically significant for per capita income. However, these estimates are possibly misleading because they ignore the different benefits across life stages. To make the analysis as simple as possible I stratify urban and rural households by those with the mother aged under 40 and those where the mother is 40 and older, which is approximately where sons shift from being children to being adults. For mothers aged 40 the mean age of their first-born child is 19.5 years. The estimates are shown in Table 7. The main difference is that now, although rural households appear to be better off from having a son early in life, urban households appear better off later in life, at least in terms of per capita income. This may be because income gains are higher in cities, where, for example, education has more market value. Urban households also see a significant gain to future assets, although not quite as much as in rural households.

Table 6: OLS: Effect of a son versus a daughter, stratified by urban/rural

	log(PCI)	**log(PCE)**	**Assets**	**Below Poverty Line**
Urban	0.061**	0.019	0.124	-0.006
	(0.014)	(0.013)	(0.101)	(0.009)
N	8659	8702	8711	8711
Rural	0.071**	0.021*	0.229**	-0.008
	(0.016)	(0.009)	(0.059)	(0.007)
N	13592	13822	13829	13829

Significance levels : † : 10% ∗ : 5% ∗∗ : 1%

Notes: Robust standard errors clustered by state in parentheses. Coefficients not shown for the following independent variables: parents' age and education, caste/religion, and state.

6.2 Father's Education

In Table 8 the sample is split into education groups based on the father's years of schooling:[5] 0-6 years (low education), 7-12 years (middle education), 13 or more years (high education). Fathers with low education benefit from a son in each economic measure.

[5]The gain from a son is unaffected by the education of the mother.

Table 7: OLS: Effect of a son versus a daughter, stratified by urban/rural and mother's age

	log(PCI)	**log(PCE)**	**Asset Index**	**Below Poverty Line**
Urban and Mother Age < 40	0.061**	0.018	0.012	-0.006
	(0.017)	(0.014)	(0.124)	(0.013)
N	5965	5994	5997	5997
Rural and Mother Age < 40	0.085**	0.034**	0.134*	-0.012
	(0.017)	(0.009)	(0.066)	(0.008)
N	9388	9545	9549	9549
Urban and Mother Age ≥ 40	0.063*	0.024	0.348†	-0.008
	(0.029)	(0.028)	(0.183)	(0.012)
N	2694	2708	2714	2714
Rural and Mother Age ≥ 40	0.038†	-0.006	0.445**	0.002
	(0.021)	(0.019)	(0.103)	(0.012)
N	4204	4277	4280	4280

Significance levels : † : 10% * : 5% ** : 1%

Notes: Robust standard errors clustered by state in parentheses. Coefficients not shown for the following independent variables: parents' age and education, caste/religion, and state.

Fathers with middle education benefit from a son in terms of per capita income and assets. Fathers with high education have no benefit on average. There also appears to be an upside-down U relationship between education and gains to per capita income and assets. This finding is verified in Table 9 which includes the entire sample and interactions of first-born boy with years of father's schooling and years of schooling squared (FB * Father's YOS and FB * Father's YOS^2). Education increases the gains from sons, but only up to a point. Fathers with 5-7 years of education gain the most from a first-born son. This relationship continues to hold even when households are further stratified by mother's age (estimates not shown). These results are particularly interesting given Jha et al.'s and Portner's findings that the more highly educated the household, the greater the number of sex selective abortions. One implication from these results is that cultural incentives may be an important factor for the highly educated (and presumably wealthy) of India, while the less educated have greater economic incentives for sex-selection. Another possibility, since both Jha et al.'s and Portner's tests use Grade 10 and 8 years of education respectively as cutoffs for the highly educated group, that

sex-selective abortion is occurring amongst those with high school level education, but not those with post-secondary education. Further analysis of the subgroup of Indians with post-secondary education would be useful to shed light on this issue, but the small sample size of this group may make accurate estimates difficult.

Table 8: OLS: Effect of a son versus a daughter, stratified by father's education

	log(PCI)	**log(PCE)**	**Asset Index**	**Below Poverty Line**
0-6 Years of School	0.058**	0.016*	0.179**	-0.016*
	(0.016)	(0.006)	(0.059)	(0.007)
N	10783	10918	10924	10924
7-12 Years of School	0.093**	0.025†	0.299**	0.002
	(0.016)	(0.013)	(0.080)	(0.007)
N	9302	9427	9435	9435
13+ Years of School	-0.004	0.020	-0.156	0.003
	(0.041)	(0.021)	(0.125)	(0.007)
N	2166	2179	2181	2181

Significance levels : † : 10% * : 5% ** : 1%

Notes: Robust standard errors clustered by state in parentheses. Coefficients not shown for the following independent variables: parents' age and education, caste/religion, and state.

6.3 Caste and Religion

Next, I look at differences in economic incentives across caste and religious groups. Table 10 shows the coefficient on $FirstBoy$ stratified by these groups. Although many of the groups gain in at least one of the economic categories, other backwards classes and Muslims show consistent benefits from sons across outcomes. Since these are poorer groups, they may have the most to gain. One explanation for why low caste groups are more affected by the birth of a son than high caste groups is that they may not have the assets to respond to economic shocks as easily as richer, high-caste households. One may think that if dowry was driving things, we would expect the highest caste groups to have the largest gains from sons, but here Brahmin and high caste households have only small benefits from sons. However, a large dowry may be more economically manageable for a rich Brahmin household than a small dowry is for a poor low-caste household. If

Table 9: Upside-down U relationship between father's education and the gains from sons

	log(PCI)	**log(PCE)**	**Asset Index**	**Below Poverty Line**
First-Born Boy (FB)	0.046*	0.015	0.152*	-0.020*
	(0.017)	(0.010)	(0.066)	(0.009)
FB * Father's YOS	0.015*	0.010	0.060*	0.003
	(0.006)	(0.005)	(0.028)	(0.002)
FB * Father's YOS^2	-0.001*	-0.000	-0.005**	-0.000
	(0.000)	(0.007)	(0.002)	(0.000)
Father's YOS	-0.022**	0.001	0.189**	-0.017**
	(0.004)	(0.003)	(0.023)	(0.002)
Father's YOS^2	0.005**	0.002**	0.013**	0.000
	(0.000)	(0.000)	(0.002)	(0.000)
Mother's Age and Education	Yes	Yes	Yes	Yes
Urban/Rural Dummy	Yes	Yes	Yes	Yes
Religion Dummies	Yes	Yes	Yes	Yes
Caste Dummies	Yes	Yes	Yes	Yes
State Dummies	Yes	Yes	Yes	Yes
N	22251	22524	22540	22540
Clusters	33	33	33	33
R^2	0.39	0.46	0.66	0.20

Significance levels : † : 10% * : 5% ** : 1%

Notes: Robust standard errors clustered by state in parentheses.

the sample is further stratified by the mother's age (estimates not shown), then older (mother aged 35 or older) Brahmin households gain wealth later in life while high-caste households gain income. For older Brahmin households the asset index rises by 0.432 (significant at the 10 percent level) and older high caste households have 7.6 percent higher PCI (significant at the 5 percent level).

Table 10: OLS: Effect of a son versus a daughter, stratified by caste/religious group

	log(PCI)	**log(PCE)**	**Asset Index**	**Below Poverty Line**
Brahmin	0.048	0.056	0.209	0.002
	(0.049)	(0.036)	(0.200)	(0.017)
N	1185	1197	1198	1198
High Caste	0.062†	0.026	-0.021	-0.003
	(0.030)	(0.020)	(0.103)	(0.011)
N	3697	3744	3747	3747
Other Backward Classes	0.082**	0.026**	0.175*	-0.027**
	(0.025)	(0.007)	(0.073)	(0.007)
N	7553	7677	7681	7681
Scheduled Castes	0.065**	0.011	0.110	0.003
	(0.021)	(0.014)	(0.076)	(0.013)
N	4681	4721	4726	4726
Scheduled Tribes	0.031	0.012	0.281*	-0.002
	(0.028)	(0.022)	(0.112)	(0.017)
N	1802	1818	1820	1820
Muslim	0.075*	0.033*	0.594**	0.004
	(0.030)	(0.014)	(0.117)	(0.011)
N	2659	2687	2687	2687
Sikh or Jain	0.040	-0.002	0.216	0.042**
	(0.066)	(0.052)	(0.415)	(0.012)
N	318	322	323	323
Christian	0.149†	0.024	0.959*	0.004
	(0.080)	(0.025)	(0.373)	(0.031)
N	356	358	358	358

Significance levels : $\dagger$: 10% $*$: 5% $**$: 1%

Notes: Robust standard errors clustered by state in parentheses. Coefficients not shown for the following independent variables: parents' age and education, urban/rural, and state.

6.4 Regional Differences

In India there are large differences in the sex ratio across regions. In particular the richer North has a higher male-female ratio than the poorer South. That the North is richer and has more discrimination against girls may be because the North has larger economic gains from a son compared to the South. To test whether there are large variations in the value of a son versus a daughter across India, the sample is split into six subgroups: North, Central, West, East, South, and Northeast.[6] The male-female sex ratio varies by region, in particular being higher in the North and lower in the South, so presumably the gains from a boy should be larger in the areas with a high male-female sex ratio. For my regional tests I use the following estimation equation:

$$Y_{ik} = \beta_0 + \beta_1 FirstBoy_{ik} + \beta_{2k} FirstBoy_{ik} * R_k + \gamma R_k + \alpha X_{ik} + e_{ik} \tag{3}$$

where the economic outcome for household i in region k depends on the sex outcome of the first-birth, region fixed effects (R_k), region interacted with the sex outcome of the first-birth, and other household characteristics. I estimate Equation (3) with and without X_{ik}. Without household level controls, the estimates show average differences across regions in the economic benefits from sons. The estimations are presented in Table 11. The omitted region is the South which usually shows a lower level of sex discrimination (along with the Northeast). The North, East, Center, and West generally show greater gains from a son compared to the South, while the Northeast shows lower gains than the South. These average differences correlate with the number of sex-selective abortions by region. Using the data from Jha et al., I rank the regions by male-female sex-ratio at birth for second-born children conditional on the first-born being female. A rank of 1 indicates

[6] North: Jammu and Kashmir, Himachal Pradesh, Punjab, Chandigarh, Uttaranchal, Haryana, Delhi, Rajasthan. Center: Uttar Pradesh and Madhya Pradesh. West: Gujarat, Maharashtra, and Goa. East: Bihar, Jharkand, Chhattisgar, Orissa, and West Bengal. South: Andhra Pradesh, Karnataka, Kerala, and Tamil Nadu. Northeast: Sikkim, Arunachal Pradesh, Nagaland, Manipur, Mizoram, Tripura, Meghalaya and Assam.

the highest proportion of sex-selective abortion and 6 indicates the lowest.[7] As shown in Table 12, relative gains in PCE positively correlate with the number of sex-selective abortions.

Table 11: Regional effects of a son versus a daughter on the household economy.

Selective Abortion Rank	**log(PCI)**	**log(PCE)**	**Asset Index**	**Below Poverty Line**
First-Born Boy	0.040**	-0.007	0.027	-0.001
	(0.012)	(0.006)	(0.099)	(0.002)
FB * North	0.014	0.054**	0.302	-0.015*
	(0.034)	(0.016)	(0.199)	(0.007)
FB * Center	0.036	0.028**	0.220†	0.017**
	(0.033)	(0.008)	(0.126)	(0.006)
FB * East	0.048†	0.044†	0.204	-0.029**
	(0.027)	(0.025)	(0.268)	(0.008)
FB * Northeast	-0.091*	-0.076*	-0.557†	0.038**
	(0.044)	(0.032)	(0.274)	(0.011)
FB * West	0.083**	0.051**	0.350**	-0.017†
	(0.026)	(0.017)	(0.100)	(0.010)
Parents' Age and Education	No	No	No	No
Urban/Rural Dummy	No	No	No	No
Religion Dummies	No	No	No	No
Caste Dummies	No	No	No	No
Region Dummies	Yes	Yes	Yes	Yes
N	23905	24228	24247	24247
Clusters	30	30	30	30
R^2	0.06	0.09	0.11	0.05

Significance levels : † : 10% * : 5% ** : 1%

Notes: Robust standard errors clustered by state in parentheses. South is the omitted region. The territories of Dam and Diu, Dadra and Nagar Haveli, and Pondicherry are not included in these estimates.

One underlying hypothesis in this paper is that economic incentives are the cause of sex-selective abortion. If household controls are added to the regional estimates, much of the differences across regions will disappear if the gains across regions are from differences in the composition of the population rather than specific cultural differences across

[7] Since Jha et al. does not include all states, I rank using the following state/groups: North: Himachal Pradesh, Punjab,Haryana, Delhi, Rajasthan. Center: Uttar Pradesh and Madhya Pradesh. West: Gujarat and Maharashtra. East: Bihar, Orissa, and West Bengal. South: Andhra Pradesh, Karnataka, Kerala, and Tamil Nadu. Northeast: Assam.

Table 12: Rankings of Regions by Number of and PCE Gains from Sex-Selective Abortions

	Selective Abortion Rank	**log(PCE)**
North	1	0.054**
West	2	0.051**
East	3	0.044†
Center	4	0.028**
South	5	
Northeast	6	-0.076*

regions. And this is exactly what I find. The estimation results are shown in Table 13. The main conclusion from the estimates is that most of the regions show similar average gains from a sex-selective abortion. If the estimations are further stratified by mother's age, this finding and the general results continue to hold.[8] Thus, there is hope from a policy perspective that economic incentives, which are more easily changed than cultural incentives, could be used to influence the amount of sex selection. There are some exceptions. With household controls, the North and West have PCE gains of about 3 percent from a son compared to the South (significant at the 10 percent level) and the West is the only region with higher gains to income from a son. The Northeast has lower benefits from sons compared to the South with 7.4 percent less of a gain to PCE and a 4.4 percent higher probability of being below the poverty line. These results indicate that much of the regional differences across India in sex-selective abortions and discrimination against girls in general may be explained by the regional differences in population characteristics that make boys relatively better for the household economy.

[8] Although I do not show the stratified estimates here, for households with mothers under the age of 40, there is little difference across regions, even without controls (the Northeast continues to get less from a son and the West gets more), but for households where the mother is 40 or older the point estimates rise significantly. With no controls, the North, with the largest gains from a son, has about 14 percent higher PCI, 15 percent higher PCE, and 1.3 more assets compared to the South all significant at the 1 percent level. Once other controls are added, even these gains from a son are wiped away.

Table 13: Regional effects of a son versus a daughter on the household economy with household controls.

	log(PCI)	**log(PCE)**	**Asset Index**	**Below Poverty Line**
First-Born Boy	0.067**	0.014	0.217**	-0.008*
	(0.019)	(0.011)	(0.068)	(0.004)
FB * North	-0.020	0.029†	0.038	-0.011
	(0.032)	(0.017)	(0.117)	(0.009)
FB * Center	-0.006	0.003	-0.017	0.024**
	(0.035)	(0.011)	(0.088)	(0.004)
FB * East	-0.001	0.007	-0.128	-0.013†
	(0.028)	(0.018)	(0.162)	(0.008)
FB * Northeast	-0.055†	-0.074**	-0.263	0.044**
	(0.032)	(0.027)	(0.198)	(0.010)
FB * West	0.070†	0.033†	0.130	-0.010
	(0.035)	(0.019)	(0.090)	(0.008)
Parents' Age and Education	Yes	Yes	Yes	Yes
Urban/Rural Dummy	Yes	Yes	Yes	Yes
Religion Dummies	Yes	Yes	Yes	Yes
Caste Dummies	Yes	Yes	Yes	Yes
Region Dummies	Yes	Yes	Yes	Yes
N	22110	22382	22398	22398
Clusters	30	30	30	30
R^2	0.37	0.42	0.63	0.37

Significance levels : † : 10% * : 5% ** : 1%

Notes: Robust standard errors clustered by state in parentheses. South is the omitted region. The territories of Dam and Diu, Dadra and Nagar Haveli, and Pondicherry are not included in these estimates.

7 Diminishing returns to sons

Although the number of children that parents have is certainly endogenous to economic outcomes, almost all parents in India eventually have two or more children. In the IHDS 94 percent of mothers aged 35 and older have two or more children. However, as indicated in Jha et al., there is likely sex-selective abortion occurring among second pregnancies if the first child is a daughter. In the IHDS the male-female sex ratio at birth for second-borns conditional on having a first-born male (1.02) is lower than the sex ratio conditional on having a first-born female (1.08), although they are not statistically different due to the sample size of the IHDS. Assuming that the sex of the second birth is exogenous to the household economy conditional on having a first-born son, we can test whether having an additional son has any effect on the household economy. Table 14 shows the linear regression estimates for the value of this second son. There are no statistically significant economic benefits from having a second son, indicating diminishing returns to sons. This finding helps to explain Portner's finding that parents do not use sex-selective abortion in India if they already have one son.

Table 14: OLS: Effect of sex-selective abortion at the second pregnancy, conditional on having a first-born son

	log(PCI)	**log(PCE)**	**Asset Index**	**Below Poverty Line**
Second-Born Boy	0.020	0.004	-0.003	0.000
	(0.018)	(0.014)	(0.100)	(0.008)
Parents Age and Education	Yes	Yes	Yes	Yes
Urban/Rural Dummy	Yes	Yes	Yes	Yes
Religion Dummies	Yes	Yes	Yes	Yes
Caste Dummies	Yes	Yes	Yes	Yes
State Dummies	Yes	Yes	Yes	Yes
N	10190	10301	10310	10310
Clusters	33	33	33	33
R^2	0.38	0.45	0.65	0.20

Significance levels : † : 10% ∗ : 5% ∗∗ : 1%

Notes: Robust standard errors clustered by state in parentheses, no first-parity or second-parity twin households.

Even though there is likely bias from sex-selective abortion, I also present the regressions for the effect of a second-born son on a household with a first-born girl in Table 15. The estimates are similar to those found in Table 3, although with larger point estimates for PCE and Below Poverty Line status. With the bias it is difficult to make anything conclusive of these effects, since they could be coming from richer households being the ones using sex-selective abortion. However, if this bias is small the estimates indicate that having one son provides a large benefit to the household, while a second son does not. If this is true, then it would explain why households do not use sex-selective abortion for the first pregnancy (only one son matters) and there are very few if any sex-selective abortions in a given birth parity if there was one son born previously.

Table 15: OLS: Effect of sex-selective abortion at the second pregnancy, conditional on having a first-born daughter

	log(PCI)	**log(PCE)**	**Asset Index**	**Below Poverty Line**
Second-Born Boy	0.068**	0.045**	0.176*	-0.023**
	(0.021)	(0.013)	(0.076)	(0.008)
Parents Age and Education	Yes	Yes	Yes	Yes
Urban/Rural Dummy	Yes	Yes	Yes	Yes
Religion Dummies	Yes	Yes	Yes	Yes
Caste Dummies	Yes	Yes	Yes	Yes
State Dummies	Yes	Yes	Yes	Yes
N	9364	9490	9495	10310
Clusters	33	33	33	33
R^2	0.37	0.44	0.65	0.21

Significance levels : † : 10% * : 5% ** : 1%

Notes: Robust standard errors clustered by state in parentheses, no first-parity or second-parity twin households.

8 Mechanism: Labor Supply

One possible mechanism that allows households to increase their income and expenditure after having a selective abortion is an increase in parents' labor supply, particularly the mother's labor supply, because of less time needed for child-rearing. The IHDS defines

working as spending at least 240 hours per year in work of some kind. 95 percent of husbands in the sample work, while 57 percent of wives work. Thus, there is a huge labor supply gap between men and women. Working as an agricultural laborer is by far the dominant type of employment. Primary occupation in the IHDS is defined as anything outside of the family farm or business and which can be attributed to an individual's work (as opposed to the family farm where it is difficult to disentangle each individual's productivity). About 60 percent of working women in the sample work in agricultural labor as their primary occupation, while about 30 percent of working men work in agricultural labor as their primary occupation.

Using Estimation Equation (2) with labor supply as the dependent variable, I find that having a first-born son has little effect on whether a parent works. (See Appendix B for tables.) And if anything, there is a negative effect, reducing the probability of 35 to 44 year old mothers working by about 2 percentage points (significant at the 5 percent level). I also find that for households that have a farm, both fathers and mothers each work on average about 9 days less per year on the farm if the mother is aged 45 or older. However, for rural households where the mother is less than 35, fathers work more in their primary occupation by about 2.8 days per year (significant at the 10 percent level). The rural father's income is also about Rs. 1100 higher if he has a first-born son (significant at the 1 percent level). There is no effect on whether the father chooses to work or not (estimates not shown), there is only an effect on how much he works and earns. This increase in father's labor supply and income explains why households with a first-born son are economically better off even when the son is young, at least for rural households.

For households where the parents are older, parents of first-born sons are working less and yet household income is much greater. Thus, there must be some cross-generation labor substitution happening or the son attracts additional family members to the household to help out. As shown in Table 4 above, a household with a first-born son has more adults in it as the household ages. The IHDS asks about each member of the household and whether they are working. The number of non-parents working is higher for older

households with a first-born son. There is no significant difference in workers for households where the mother is under 35. There are about 0.18 more workers in households where the mother is between 35 and 45; there are 0.25 more workers where the mother is 45 or older. The magnitude of the number of extra workers in the household is similar for urban and rural households (estimates not shown). Most income is tied up in the family farm or business, however individual household members report individual income if they have it (under their primary occupation as above). The income from household members who are not the parents make up about 8 percent of total household income (for households with positive yearly income) on average, but it becomes increasingly important as parents age. About 3 percent of total income is individual-non-parent-income for households where the mother is under 35. It rises to 9 percent for mothers between 35 and 45, and rises further to 18 percent for mothers older than 45. There is a statistically significant increase in the household's individual-non-parent-income for older households: 26.5 percent higher for mothers between 35 and 45; 14 percent higher for mothers older than 45. Sons are by far the most common extra worker in the household, although some of the extra workers are the daughter, daughter-in-law, or son-in-law. Thus, it appears that a sex-selective abortion means higher future income (and leisure) for a parent's household because they are able to keep their son in their home.

9 Conclusion

The above estimates paint a multihued picture of the economic gains from sex-selective abortion. A selective abortion improves the household economy in several different ways depending on the type and age of the household. Some households gain income, while others gain assets. Some types of households are pushed out of poverty, but others are unaffected. That almost all of the statistically significant results show positive gains from sons indicates the general robustness of the finding that a son is a large economic gain for a household. Thus, it is quite plausible that these economic gains can explain why sex-

selective abortion is so prevalent in India. Regions of India with the greatest economic gains from sons are those that have the greatest levels of discrimination. However, the finding that when household controls are added, regions are not very different on average, supports the idea that economic differences, rather than cultural differences, across India are the larger cause of sex-selective abortion. This finding should give hope to policy makers that financial incentives can actually improve the situation. Furthermore, the future increased household labor supply from a son is shown to be a likely mechanism for the economic benefits of sons. Interestingly, rural fathers work harder if they have a son instead of a daughter. That fathers do more for sons also holds true in the US, so India is not unique in this respect.

For policy makers, the findings from this paper mean that eliminating the use of sex-selective abortion will be an expensive and difficult task, but this is no reason to sit on our hands. Any marginal increase in the benefit of a daughter relative to a son will decrease the use of sex-selective abortion and increase equality of the sexes in India. In the short-run, trying to directly subsidize daughters may be the most effective tool that policy makers have in tackling India's demographic problems. The existence of joint families in India, which gives parents large gains from sons' future income, is unlikely to be changed through policy. In the long-run, equality in education and labor markets may prove to be the best policy to ameliorate discrimination amongst girls.

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APPENDIX

A Economies of Scale

Following along the lines of Lanjouw and Ravallion (1995) I examine the log of per capita income and expenditure for different size elasticities. For a household of size N, and total expenditure X, the economic welfare of a household is calculated as X/N^{θ}, where θ is the size elasticity and $0 \leq \theta \leq 1$. When $\theta = 0$ welfare is total household expenditure and when $\theta = 1$ it is per capita expenditure. Since there is no agreed upon standard equivalence scale, I test whether the advantages from sons are robust to a range of θ's. Estimating the regressions for income and expenditure in Table 3 and using $\theta = 0.1, 0.2, ..., 0.9, 1$, the coefficient on $FirstBoy$ remains positive and significant, only losing its statistical significance for $\theta = 1$ for expenditure. Table 16 reports the coefficients for $\theta = 0$ and $\theta = 0.5$. Only at the extreme of large economies of scale, a son does not add to average household expenditure. However, the son still contributes significantly to average household income in India. These tests indicate that the benefits from sons are largely robust to choice of θ.

Table 16: OLS: Effect of sex-selective abortion on the log of scaled household income and expenditure

	income/$N^{0.5}$	**expend./$N^{0.5}$**	**income**	**expend.**
First-Born Boy	0.063**	0.016*	0.057**	0.010
	(0.012)	(0.006)	(0.011)	(0.006)
Parents' Age and Education	Yes	Yes	Yes	Yes
Urban/Rural Dummy	Yes	Yes	Yes	Yes
Religion Dummies	Yes	Yes	Yes	Yes
Caste Dummies	Yes	Yes	Yes	Yes
State Dummies	Yes	Yes	Yes	Yes
N	22251	22524	22251	22524
Clusters	33	33	33	33
R^2	0.38	0.45	0.36	0.42

Significance levels : † : 10% ∗ : 5% ∗∗ : 1%

Notes: Robust standard errors clustered by state in parentheses.

Following up on the above discussion of the effects of household structure on the

economic welfare implications of a son versus a daughter, Table 17 further stratifies the population by the mother's age and reports the coefficient on $FirstBoy$. As expected, the gains from sons is larger for older households and smaller for younger households. Also, as noted above, a low value of θ (the more household economies of scale matter) indicates lower benefits for sons when the household is young (since these households are smaller) and a larger benefit for sons when the household is older (since these households are larger). For example when mothers are under the age of 35, a first-born son increases total household income by about 4%, but with $\theta = 0.5$, scaled household income rises by 6%. When mothers are older the opposite holds. When mothers are over the age of 45, a first-born son increases total household income by about 9%, but with $\theta = 0.5$, scaled household income rises by only about 7%. In either case, sons generally have a strong positive effect on the household economy. In Table 5 above (with per capita measures), as households age, PCI and PCE fall. In Table 17 there appears to be an increasing trend in the income and expenditure gains from sons as households age. And whereas in Table 5 the log of per capita income does not improve from having a son for mothers above age 35, in Table 17, expenditures are significantly higher for households where the mother is 45 or older. Thus, as noted, not accounting for equivalence scales in the per capita analysis of economic outcomes may be overestimating the value of sons early in life and underestimating them later in life.

Table 17: OLS: Effect of sex-selective abortion on log income and expenditure, with economies of scale, stratified by mother's age

	Coefficient on First-Born Boy			
	income/$N^{0.5}$	**expend./$N^{0.5}$**	**income**	**expend.**
Mother Age < 35	0.060**	0.017*	0.042**	-0.000
	(0.010)	(0.008)	(0.010)	(0.009)
N	10171	10285	10171	10285
35 ≤ Mother Age < 45	0.067**	0.009	0.063**	0.006
	(0.020)	(0.011)	(0.019)	(0.012)
N	9214	9333	9214	9333
Mother Age ≥ 45	0.068*	0.036†	0.093**	0.060**
	(0.030)	(0.020)	(0.031)	(0.019)
N	2866	2906	2866	2906

Significance levels : † : 10% ∗ : 5% ∗∗ : 1%

Notes: Robust standard errors clustered by state in parentheses, no first-born twins households, all regressions include the following independent variables: parents' age and education, caste/religion, urban/rural and state.

B Labor Supply Tables

Table 18: Mean Dependent Labor Supply Variables

Variable	Mean	Standard Deviation	N
Father Any Work (0/1)	0.952	0.002	22786
Mother Any Work (0/1)	0.571	0.008	24274
Father Any Farm Work (0/1)	0.723	0.008	22786
Mother Any Farm Work (0/1)	0.206	0.007	24274
Father Days/Year on Farm	136	2.3	6856
Mother Days/Year on Farm	115	2.1	6057
Father Days/Year in Primary Occupation	229	1.7	15939
Mother Days/Year in Primary Occupation	170	2.4	6350
Father's Income in Primary Occupation	30271	741	15938
Mother's Income in Primary Occupation	9209	301	6350
Non-parent workers in household	0.56	0.12	24396

Notes: Sampling weights used. Days/year worked are conditional on working at least one day in that job. Primary occupation is asked using the following survey question "Now, [besides work on the household farm or in any of the household's businesses,] what work for pay or goods did [NAME] DO LAST YEAR?" (IHDS Household Questionnaire, p.11.)

Table 19: OLS: Effect of sex-selective abortion on father's labor supply, stratified by mother's age

	Coefficient on First-Born Boy			
	Any Work	**Work on Farm**	**Farm Days/Year**	**Primary Occ. Days/Year**
Mother Age < 35	0.006	-0.012	1.28	1.64
	(0.005)	(0.008)	(2.73)	(1.32)
N	10289	10289	2724	7664
35 ≤ Mother Age < 45	0.001	0.006	0.81	-1.75
	(0.004)	(0.008)	(2.65)	(1.74)
N	9341	9341	3006	6377
Mother Age ≥ 45	0.002	-0.017	-8.84*	-1.35
	(0.011)	(0.015)	(4.23)	(3.85)
N	2910	2910	1029	1761

Significance levels : † : 10% ∗ : 5% ∗∗ : 1%

Notes: Robust standard errors clustered by state in parentheses, no first-born twins households, all regressions include the following independent variables: parents' age and education, caste/religion, urban/rural and state.

Table 20: OLS: Effect of sex-selective abortion on mother's labor supply, stratified by mother's age

	Coefficient on First-Born Boy			
	Any Work	**Work on Farm**	**Farm Days/Year**	**Primary Occ. Days/Year**
Mother Age < 35	-0.003	-0.052	3.31	5.04
	(0.009)	(0.006)	(2.87)	(3.19)
N	10289	10289	2268	2571
35 ≤ Mother Age < 45	-0.021*	-0.004	-1.47	-3.37
	(0.004)	(0.006)	(3.07)	(3.55)
N	9341	9341	2551	2394
Mother Age ≥ 45	-0.019	-0.007	-9.08**	0.37
	(0.013)	(0.016)	(2.98)	(8.37)
N	2910	2910	857	644

Significance levels : † : 10% ∗ : 5% ∗∗ : 1%

Notes: Robust standard errors clustered by state in parentheses, no first-born twins households, all regressions include the following independent variables: parents' age and education, caste/religion, urban/rural and state.

Table 21: OLS: Effect of sex-selective abortion on parents non-farm/non-business income (Rs.), stratified by mother's age and urban/rural

	Coefficient on First-Born Boy			
	Urban		**Rural**	
	Father	**Mother**	**Father**	**Mother**
Mother Age < 35	1094	-2728	1120**	104
	(1825)	(2122)	(400)	(218)
N	2964	507	4700	2064
$35 \leq$ Mother Age < 45	-700	2576	-655	-399
	(1848)	(3681)	(593)	(338)
N	2534	505	3842	1889
Mother Age ≥ 45	-4491	4025	2534	-146
	(3461)	(7604)	(1640)	(613)
N	769	146	992	498

Significance levels : † : 10% ∗ : 5% ∗∗ : 1%

Notes: Robust standard errors clustered by state in parentheses, no first-born twins households, all regressions include the following independent variables: parents' age and education, caste/religion, urban/rural and state.

Table 22: OLS: Effect of sex-selective abortion on fathers' non-farm/non-business labor supply (days/year), stratified by mother's age and urban/rural

	Coefficient on First-Born Boy	
	Urban	**Rural**
Mother Age < 35	-1.11	2.76†
	(2.11)	(1.50)
N	2964	4700
$35 \leq$ Mother Age < 45	-2.34	-0.49
	(2.04)	(2.22)
N	2534	3843
Mother Age ≥ 45	-2.04	1.23
	(4.50)	(6.13)
N	769	992

Significance levels : † : 10% ∗ : 5% ∗∗ : 1%

Notes: Robust standard errors clustered by state in parentheses, no first-born twins households, all regressions include the following independent variables: parents' age and education, caste/religion, urban/rural and state.

Table 23: OLS: Effect of sex-selective abortion on non-parent household labor supply and income, stratified by mother's age

	Coefficient on First-Born Boy	
	workers	**log(income)**
Mother Age < 35	-0.006	0.056
	(0.016)	(0.096)
N	10289	765
$35 \leq$ Mother Age < 45	0.184^{**}	0.265^{**}
	(0.022)	(0.045)
N	9341	1935
Mother Age ≥ 45	0.249^{**}	0.141^{*}
	(0.055)	(0.067)
N	2910	987

Significance levels : † : 10% $*$: 5% $**$: 1%

Notes: Robust standard errors clustered by state in parentheses, no first-born twins households, all regressions include the following independent variables: parents' age and education, caste/religion, urban/rural and state.

Divorce, Abortion and the Sex Ratio at Birth

The Effect of the Amended Divorce Law in China

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Brown University

Jan 25th, 2011

Question and Hypothesis:

- Question:
  How do the relative divorce options affect sex selective abortion decisions within marriages?
- Hypothesis (Story):
  - Increases women's marriage exit options , hence increases women's intra-marriage bargaining position, therefore allows women to avoid having sex-selective abortions.
- Dictatorship (extreme case of bargaining)
- Empirical evidence

Literature (1): Skewed Sex Ratio

- Skewed sex ratio at birth in China:
  120 boys were born for every 100 girls in year 2000.
  Goodkind 1996, Poston et al. 1997, Banister 2004, Li 2005, Das Gupta et.al. 2009;
- Potential social problems caused by skewed sex ratio
  Wei and Zhang 2009, Almond, Edlund and Milligan 2009;
- Controversy
  Foster and Rosenzweig 2001, Almond, Edlund and Milligan, 2009.

Literature (2): Divorce and Intra-household Allocation

Linkage between Divorce and Bargaining

marriage "exit" options are used as a "threat point" in intra-household decision making procedure (Manser and Brown, 1979, 1980; Horney and McElroy, 1981)

Bargaining power and Household outcomes

Household nutrition intake (Thomas 1990); the welfare of female children (Duflo and Udry 2005); gender differential children survival rate (Qian 2008)

Household outcomes of Divorce law

Labor supply (Gray 1998,Stevenson 2007); the welfare of children (Gruber 2004); the divorce rate (Friedberg 1998,Wolfers 2007); household specialization (Stevenson 2007) and domestic violence (Stevenson and Wolfers 2007).

Literature (2): Open Questions?

- Is divorce a relevant "threat"?
  Other "threats" within marriages: Lundberg and Pollack 1993, 1996
- Why can sex selection behavior be affected by bargaining?
  - Why can gender-differential investment in children be affected?
    Women care more about girls? Duflo (2005) and Qian (2008)
  - Why can public good provision (children's welfare or sex composition) be affected under efficiency?

The New Marriage Law ("Divorce Law") in 2001:

- Four new articles to protect property rights.
  - Women's land and property loss upon divorce (Liu and Chan, 1999; Hare, Li and Englander, 1997)
- Unilateral divorce is available under the case of domestic violence and extra-marital relationships.
  - All-China Women's Federation 2002 Survey
- Further clauses stipulating enforcement.
  - Social assistance and Legal Liability Provisions.

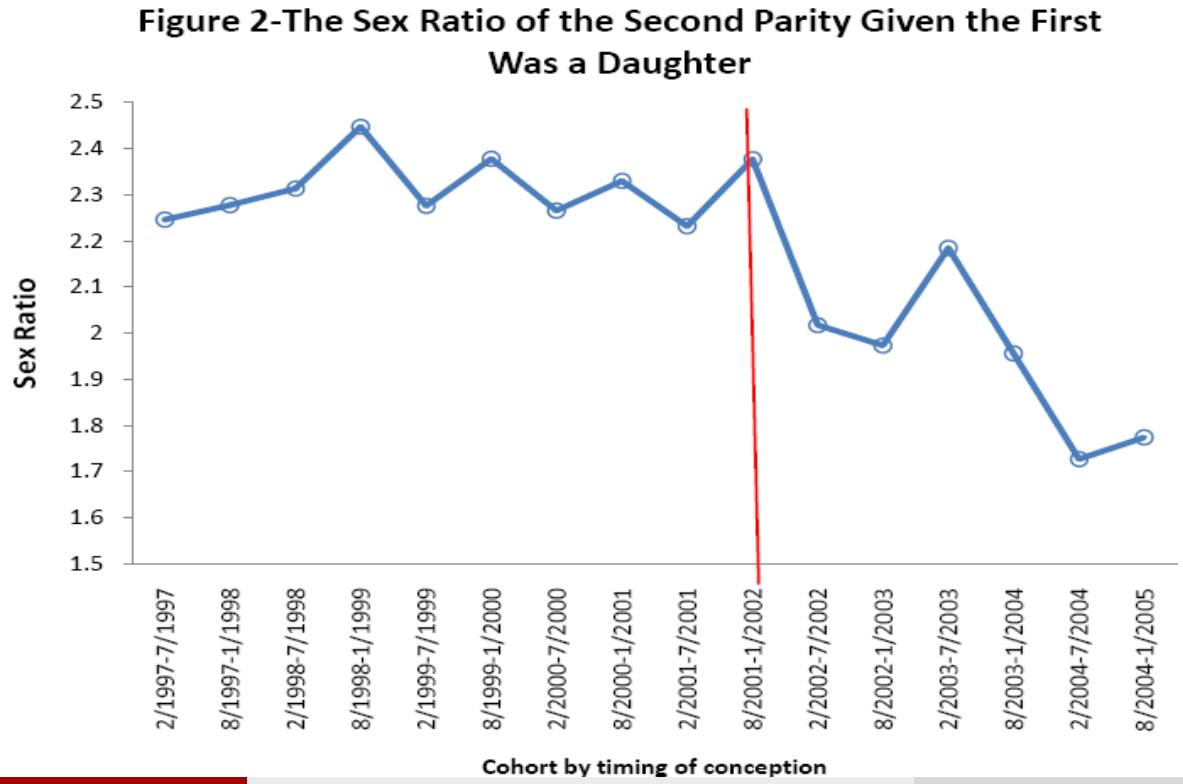
Sex Selection in China

- Son Preference
- Family Planning Policy for Han ethnicity (majority) in rural China
  - (1)One Child Policy (6 provinces); (2) Second child allowed after having a firstborn daughter (24 provinces)
- Sex Selection Performed:
  - Ultra-sound B sex screening and induced abortions (Greenhalgh and Li, 1995; 1996; Chu 2001; Li, 2005; Li and Zheng, 2009)
- Potential discord over sex selective abortions (Chu 2001, 1997 In-depth fertility survey).

The "Turning Point" of the Second Birth Sex Ratio

Definition sex ratio at birth is the ratio of males to females among newborns

- The margin where sex selective abortions are heavily used:
  - Second pregnancy after having a firstborn daughter (Poston et al. 1997).



The Increase in Divorce Propensity Between 2000 and 2005

	2000 Census			*2005 Census*		
	All women	Gave birth to one child (a daughter)	Gave birth to one child (a son)	All women	Gave birth to one child (a daughter)	Gave birth to one child (a son)
In the first marriage	0.969	0.951	0.961	0.949	0.94	0.957
Remarried	0.019	0.024	0.018	0.026	0.026	0.02
Currently divorced	0.008	0.019	0.014	0.013	0.028	0.015
Currently Widowed	0.004	0.006	0.007	0.013	0.006	0.007
Observations	258,445	45,234	60,777	562,197	97,061	145,099

Why a Model

Why is the above empirical evidence not the end of the story?

Rule out alternative explanations

- Rule out a secular trend or other simultaneous policies
  - Difficulty: No ideal control group
- Rule out the effect of channels other than improvement of women's bargaining position

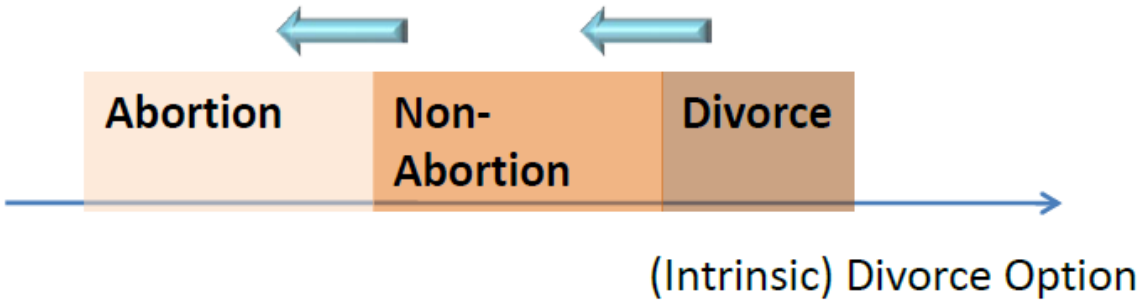
A formal theoretical framework

- Provide a better understanding of the sex selection decision making procedure
- The second-order prediction can help to drop out other time trend
- The supporting evidence on the unique second-order prediction makes the story more convincing.

Two Key Assumptions and Predictions

- Two key features (Assumptions):
  - The health-related costs of fertility and sex selective abortion are hard (very costly) to be compensated by more consumption.
  - Marriage exit options can transfer into bargaining position within marriage.
- First-Order Prediction:
  Sex selective abortions should decrease after the implementation.

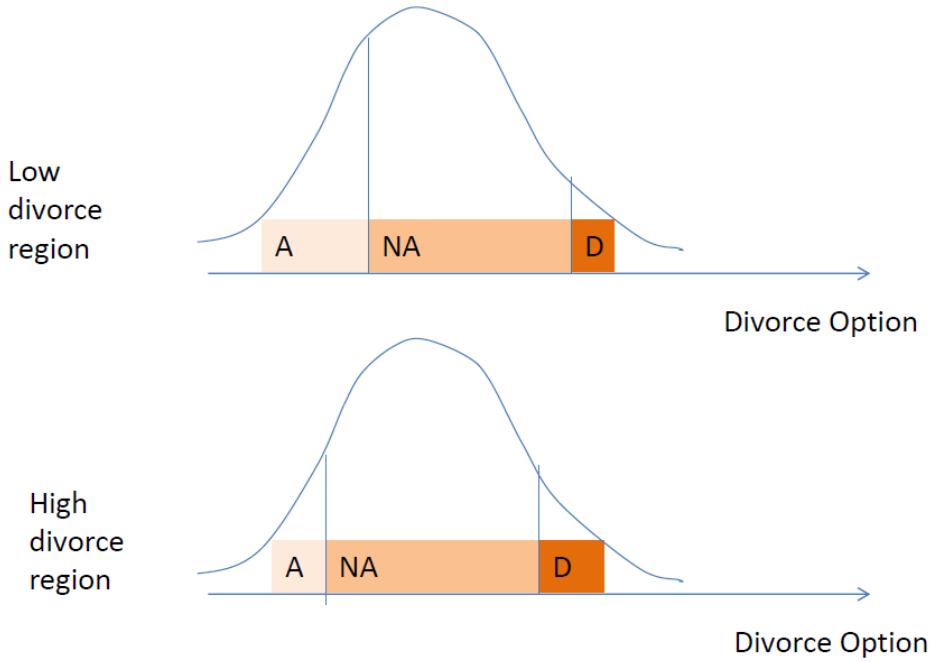
The Intuition (1): First-order Predictions



- The rank:
  - Holding all other factors constant, women with better divorce options can afford to divorce (Liu and Chan 1999; Platte 1988)
  - Under the two assumptions, amongst those staying married, women in abortion households should have worse divorce options.
- Divorce law: women's divorce cost reductions shift both margins

The Intuition (2): Second-order Prediction

- The effect of fewer abortions drives the sex ratio (#boys/#girls) downward
- The effect of more divorces drives the sex ratio (#boys/#girls) upward
  - The restriction of family planning policy on remarried couples: only 37% of women gave birth again
- Under the assumption of (1) Normal distribution and (2) that the initial divorce rate is low enough, sex ratio should decline and historically low divorce provinces should be affected most.



The Model: State Contingent Utilities within Marriage

- Utility

$$u = \sigma_H x_H \ ; \ v = \sigma_W x_W$$

x_H and x_W : private consumption;
σ_H and σ_W : the value of the sex of the second child;

- Son Preference

$$\sigma_H^D = \sigma_W^D = \sigma \ ; \ \sigma_H^S = \sigma_W^S = \gamma\sigma; \ \ \gamma > 1$$

- Costs of fertility and abortion

An additional birth: depreciated by β^b;
Have a sex selective abortion before giving another birth: depreciated by β^a.

$$v = \beta^b \sigma_W x_W; \quad v = \beta^a \sigma_W x_W; \ \ \beta^a < \beta^b < 1$$

- The budget constraint:

$$x_H + x_W \leq M$$

The Model: Decision Making Procedure

An Extreme Case: Dictator Husband

θ is an exogenous parameter in this model, and assumed to be determined by social norms.

- Nash Bargaining: ($0 < \theta < 1$)

$$\max (u - u_r)^{\theta} (v - v_r)^{1-\theta}$$

$$s.t. \quad u \geq u_r \ and \ v \geq v_r$$

- Dictator husband: ($\theta = 1$)

$$\max u - u_r$$

$$s.t. \quad u \geq u_r \ and \ v \geq v_r$$

The Model: Women's Marriage Exit Option

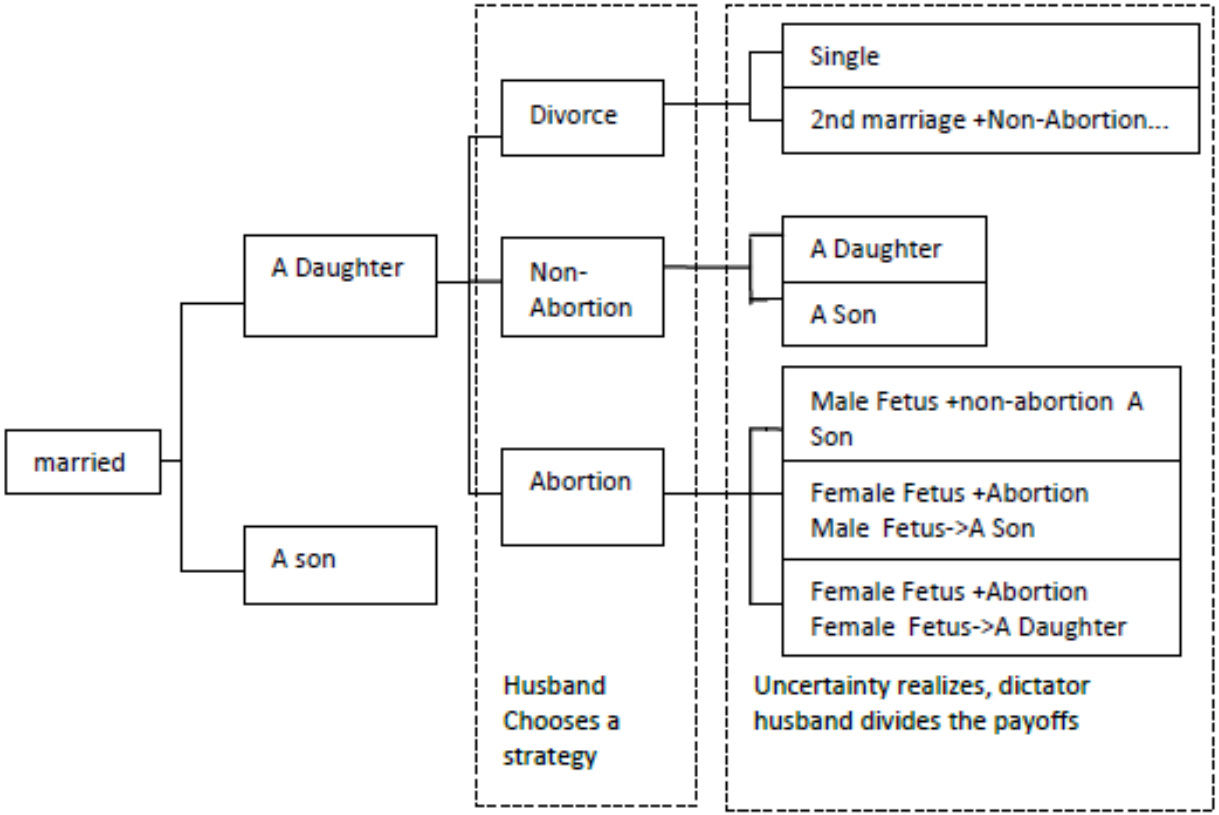
- The utility upon divorce

$$
\begin{aligned}
u_r &= u_d - c_H > \sigma_H x_H \\
v_r &= v_d - c_W > \sigma_W x_W
\end{aligned}
$$

  - v_r is the reservation utility, which is the combination of alimony, child support, property division, etc.;
  - u_d and v_d captures some idiosyncratic factors, such as each party's social networks, etc., and c_W mainly reflects the legal conditions.
  - For each husband, v_d is not clear before the marriage and revealed afterwards.

- The new divorce law reduced women's divorce costs, c_W.

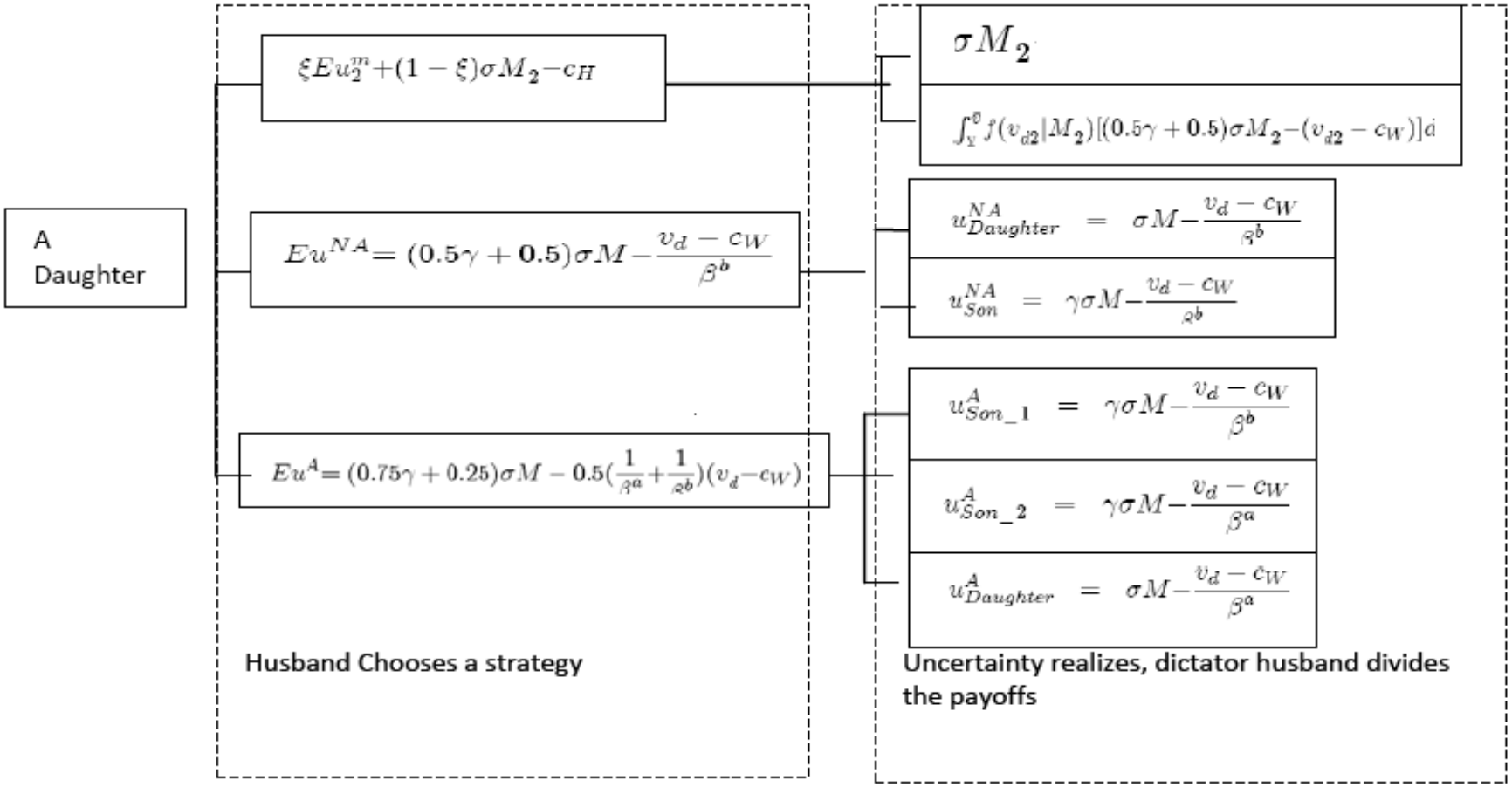
The Optimization Problem



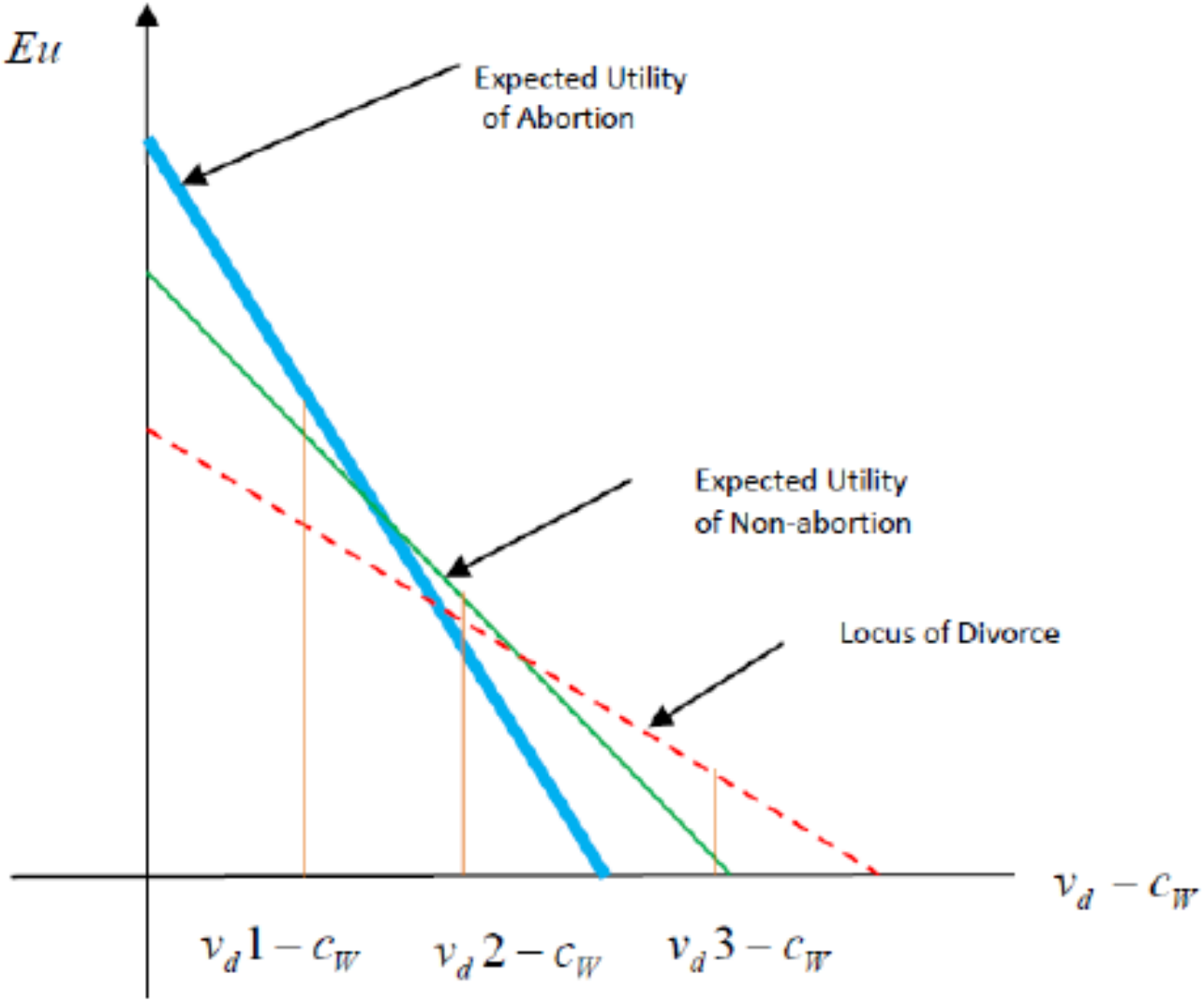
the husband chooses a strategy from $\Theta = \{A, NA, D\}$

$$\max_{s \in \Theta} Eu$$
$$s.t.\ v \geq v_d - c_W$$

The Optimization Problem



The Optimization Problem



The Model: Deriving the margin of abortion and divorce

The cutoff between choosing A and NA,

$$\widetilde{v} = \frac{0.5(\gamma - 1)\sigma M}{\frac{1}{\beta^a} - \frac{1}{\beta^b}} + c_W \equiv a + c_W$$

The cutoff between choosing NA and D,

$$\begin{aligned} \hat{v} &= \frac{0.5(\gamma - 1)(1 - \xi)\sigma M}{\frac{1}{\beta^b} - 1 - 0.5\xi(\gamma - 1)} + \frac{c_H + 0.5\mu}{\frac{1}{\beta^b} - 1 - 0.5\xi(\gamma - 1)} \\ &+ [\frac{1}{\beta^b} - 0.5\xi(\gamma - 1)]c_W \equiv b + \beta c_W \end{aligned}$$

The fraction of women having a second-born son,

$$p = 0.5 + 0.25\frac{\Phi(\widetilde{v})}{\Phi(\hat{v})}$$

Comparative Statics and the Predictions

Under the assumptions that (1) women's divorce options follow normal distribution and (2) the initial divorce rate is low enough:

Proposition 1

$\frac{\partial Divorce\ rate}{\partial c_W} < 0$

The divorce rate should increase amongst couples with firstborn daughters.

Proposition 2

$\frac{\partial p}{\partial c_W} > 0$

The sex ratio of children should converge to the natual level (decrease).

Proposition 3

$\frac{\partial^2 p}{\partial c_W^2} > 0$

The sex ratio in historically low divorce regions should be affected most.

Data source: 2005 Census of 0.2 percent population; 2000 Census of 0.1 percent population.

cohort	#female Birth	#male Birth	SRB	Father's age at this birth	Mother's age at this birth	Father's Education years	Mother's Education years
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
8/1999-1/2000	294	699	2.37	30.286	28.949	8.348	7.650
				(3.900)	(3.453)	(1.976)	(2.313)
2/2000-7/2000	312	710	2.27	31.144	29.727	8.411	7.509
				(4.082)	(3.648)	(1.955)	(2.314)
8/2000-1/2001	294	685	2.32	30.696	29.186	8.362	7.302
				(3.632)	(3.447)	(2.010)	(2.488)
2/2001-7/2001	272	606	2.22	31.473	29.902	8.427	7.691
				(3.598)	(3.397)	(2.002)	(2.310)
8/2001-1/2002	305	724	2.37	31.108	29.648	8.473	7.473
				(4.090)	(3.718)	(1.858)	(2.509)
2/2002-7/2002	336	678	2.01	31.943	30.184	8.389	7.627
				(4.458)	(3.768)	(1.912)	(2.277)
8/2002-1/2003	341	674	1.97	31.379	29.799	8.554	7.692
				(4.243)	(3.983)	(1.948)	(2.356)
2/2003-7/2003	343	748	2.18	31.877	30.423	8.540	7.614
				(4.155)	(3.903)	(1.901)	(2.297)
8/2003-1/2004	364	712	1.95	31.317	29.927	8.516	7.743
				(3.797)	(3.605)	(1.939)	(2.264)
2/2004-7/2004	420	727	1.73	32.280	30.675	8.464	7.745
				(4.339)	(3.869)	(1.812)	(2.194)

Empirical Strategy

Empirical Tests on Divorce Rate and Sex Ratio

$$
\begin{aligned}
Divorce_{ipt} &= \alpha + post_t \cdot \underset{(+)}{\delta} + div2000_p \times post_t \cdot \beta \\
&\quad + div2000_p^2 \times post_t \cdot \gamma + X_{ipt}\eta + Z_{pt}\rho + \lambda_p + \pi_c + \varepsilon_{ipt} \\
male_{ipc} &= \alpha + \Sigma_{l=1}^{15} 1\{c = l\} \cdot \underset{(-)}{\beta_l} + X_{ipc}\gamma + \lambda_p + \varepsilon_{ipc}
\end{aligned}
$$

Sex Ratio declined most in Historically Low Divorce Region

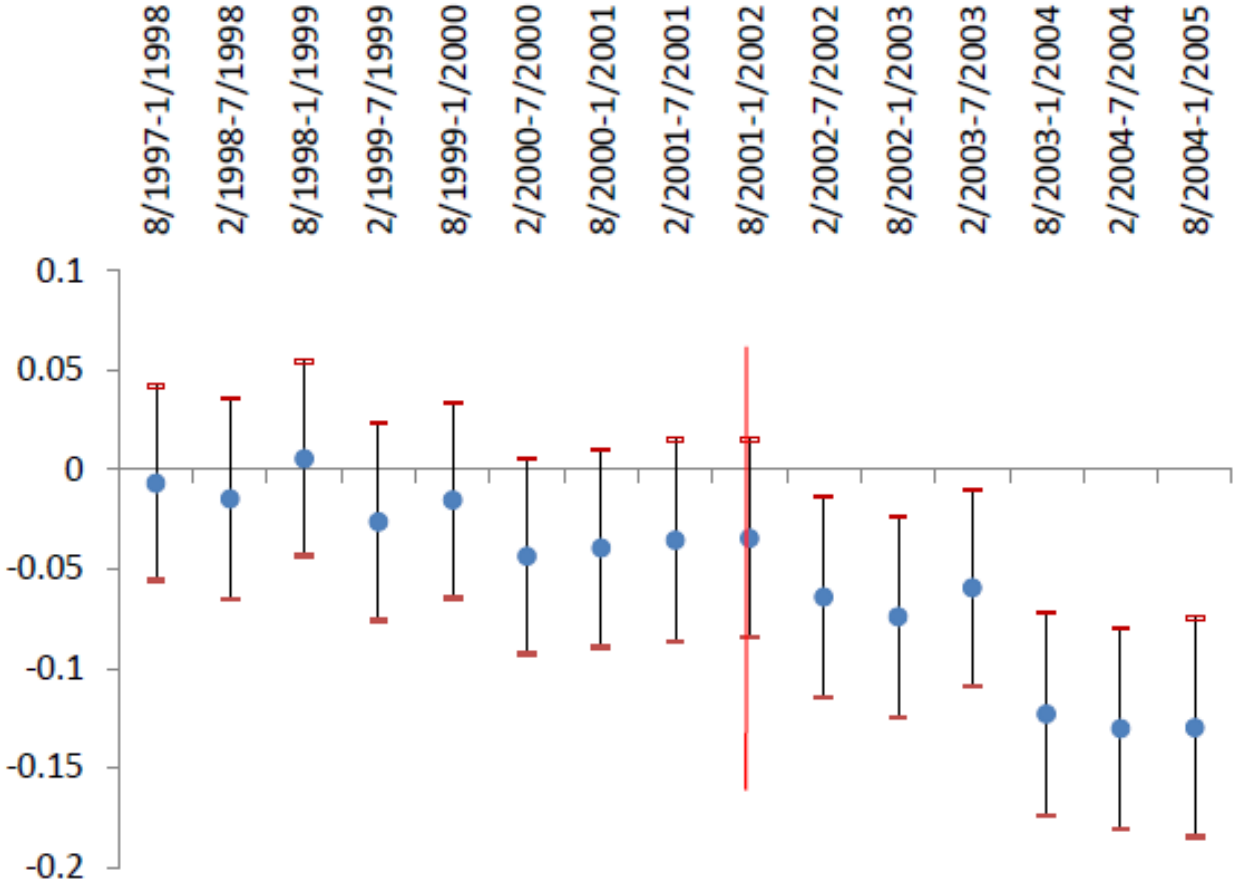
$$
\begin{aligned}
male_{ipc} &= \alpha + \Sigma_{l=1}^{15} 1\{c = l\} \times Divorce_p \cdot \underset{(+)}{\omega_l} \\
&\quad + \Sigma_{l=1}^{15} 1\{c = l\} \cdot \underset{(-)}{\beta_l} + X_{ipc}\gamma + \lambda_p + \varepsilon_{ipc}
\end{aligned}
$$

The Increase in Divorce Propensity

Dependent variable: Indicator of currently divorced				
	Women who gave birth to one daughter		Women who gave birth to one son	
	(1)	(2)	(3)	(4)
"Post the implementation"	.018 (.006)***	.016 (.009)*	.003 (.006)	.009 (.008)
Divorce rate*post	-.027 (.011)**	-.026 (.014)*	-.003 (.011)	-.008 (.012)
Divorce rate_sq*post	.007 (.004)*	.007 (.004)	.003 (.006)	.002 (.004)
Controls	N	Y	N	Y
Provincial F.E.	Y	Y	Y	Y
Cohort F.E.	Y	Y	Y	Y
R square	0.0027	0.0041	0.0028	0.0041
observations	24,119	23,983	24,119	23,983

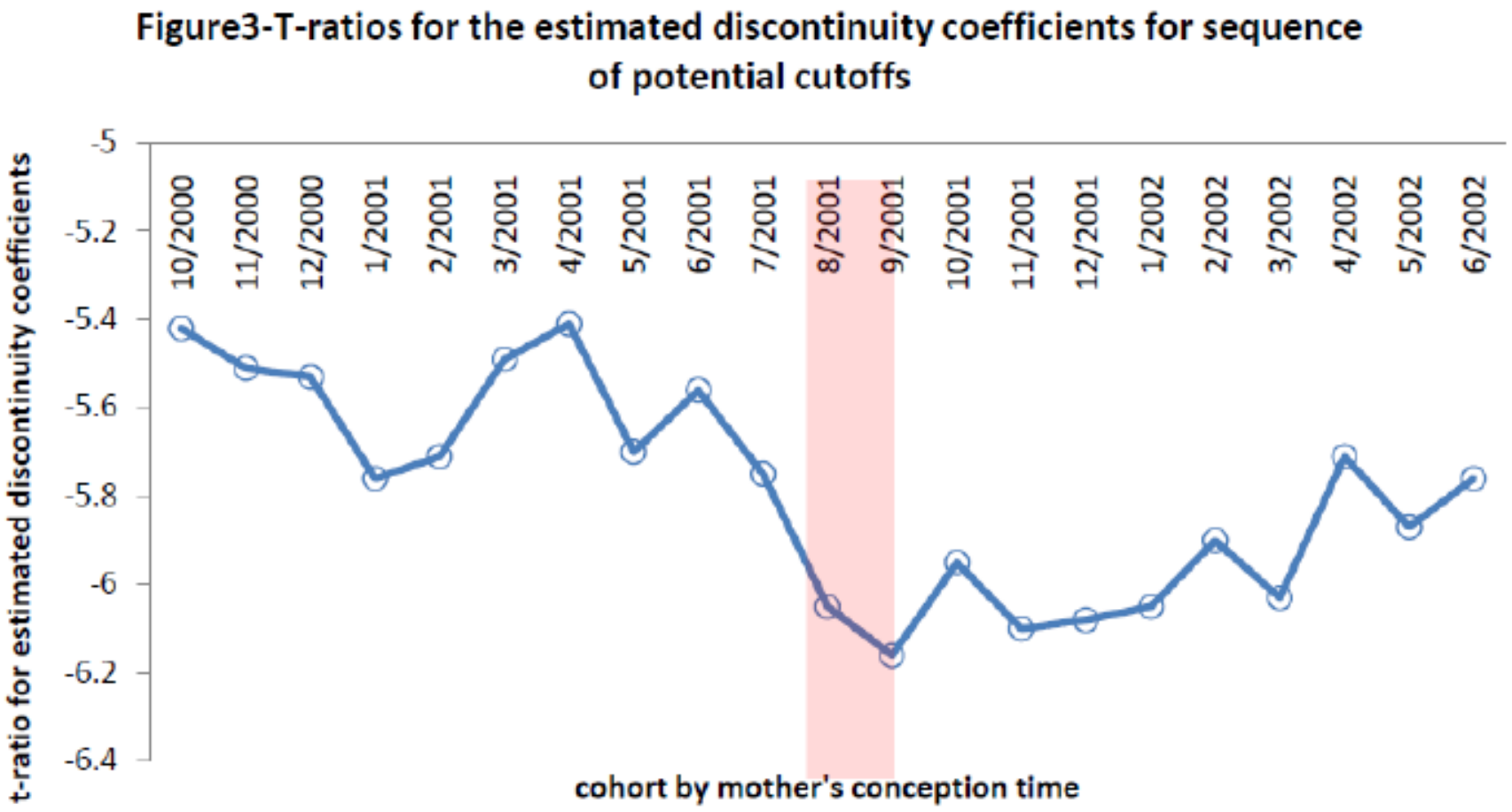
Sex Ratio at Birth Converges to Natural Level

Coefficients of Sex Ratio Trend by Cohort: Each Cohort is Defined as All Babies Conceived in a Certain Six-month Period as Listed in the Table (cohort conceived within 2/1997-7/1997 is the reference group)



The Discontinuity in Time Dimension

$$male_{ipc} = \alpha + d\{c > time\_conception\} \cdot \beta_l + X_{ipc}\gamma + \lambda_p + \varepsilon_{ipc}$$



Sex Ratio Declines Most in Low Divorce Region

Corollary

Historically low divorce region had higher sex ratio.

$$\frac{\partial Divorce\ rate}{\partial c_W} < 0$$

$$\frac{\partial p}{\partial c_W} > 0$$

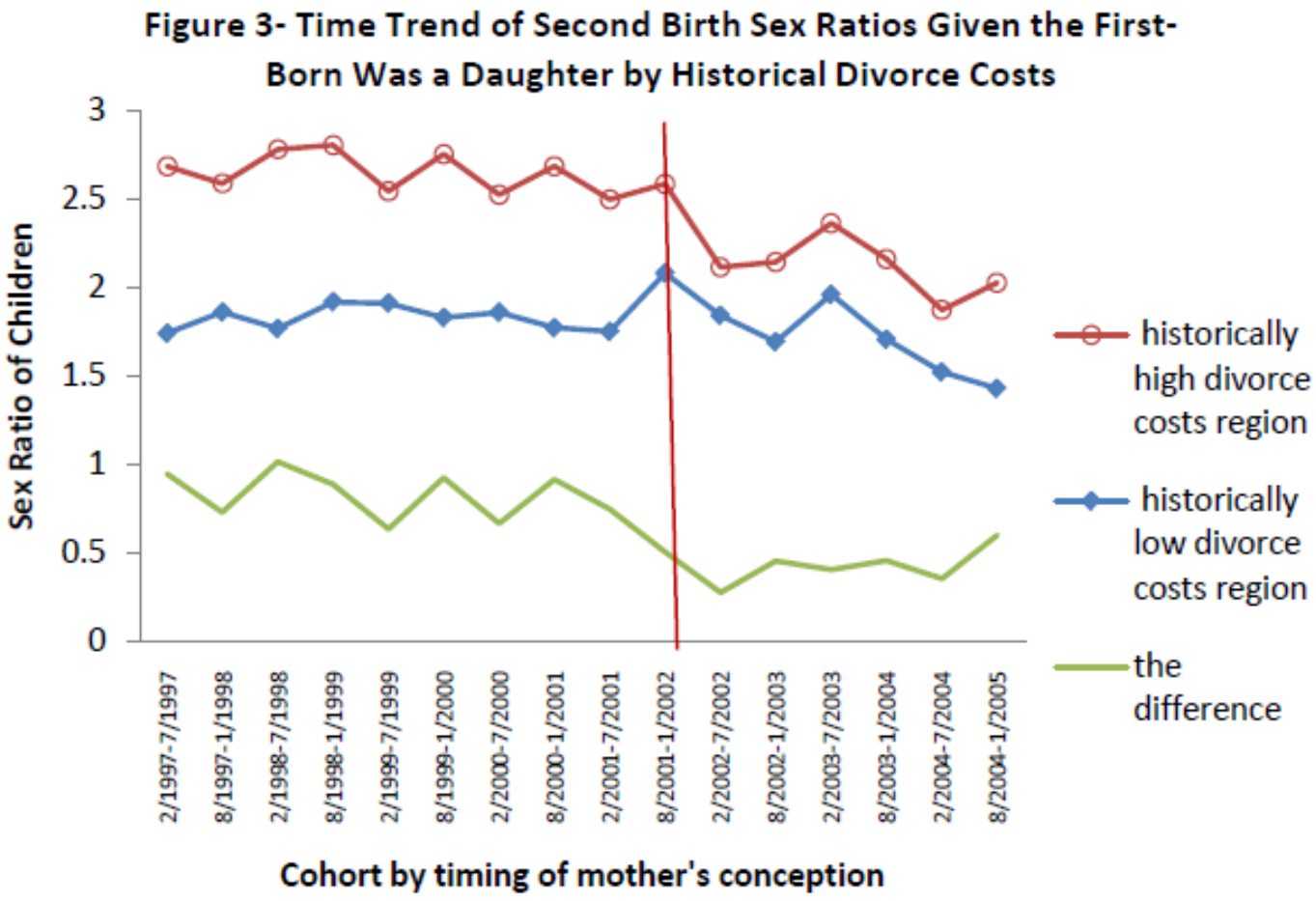
Corollary

The Gap of sex ratio between historically high and low divorce regions should decrease following the divorce law.

$$\frac{\partial^2 p}{\partial c_W^2} > 0$$

Sex Ratio Declines Most in Low Divorce Region

Shrinking Gap in Sex Ratio Between High &Low Divorce Regions



Sex Ratio Declines Most in Low Divorce Region

Shrinking Gap in Sex Ratio Between High &Low Divorce Regions

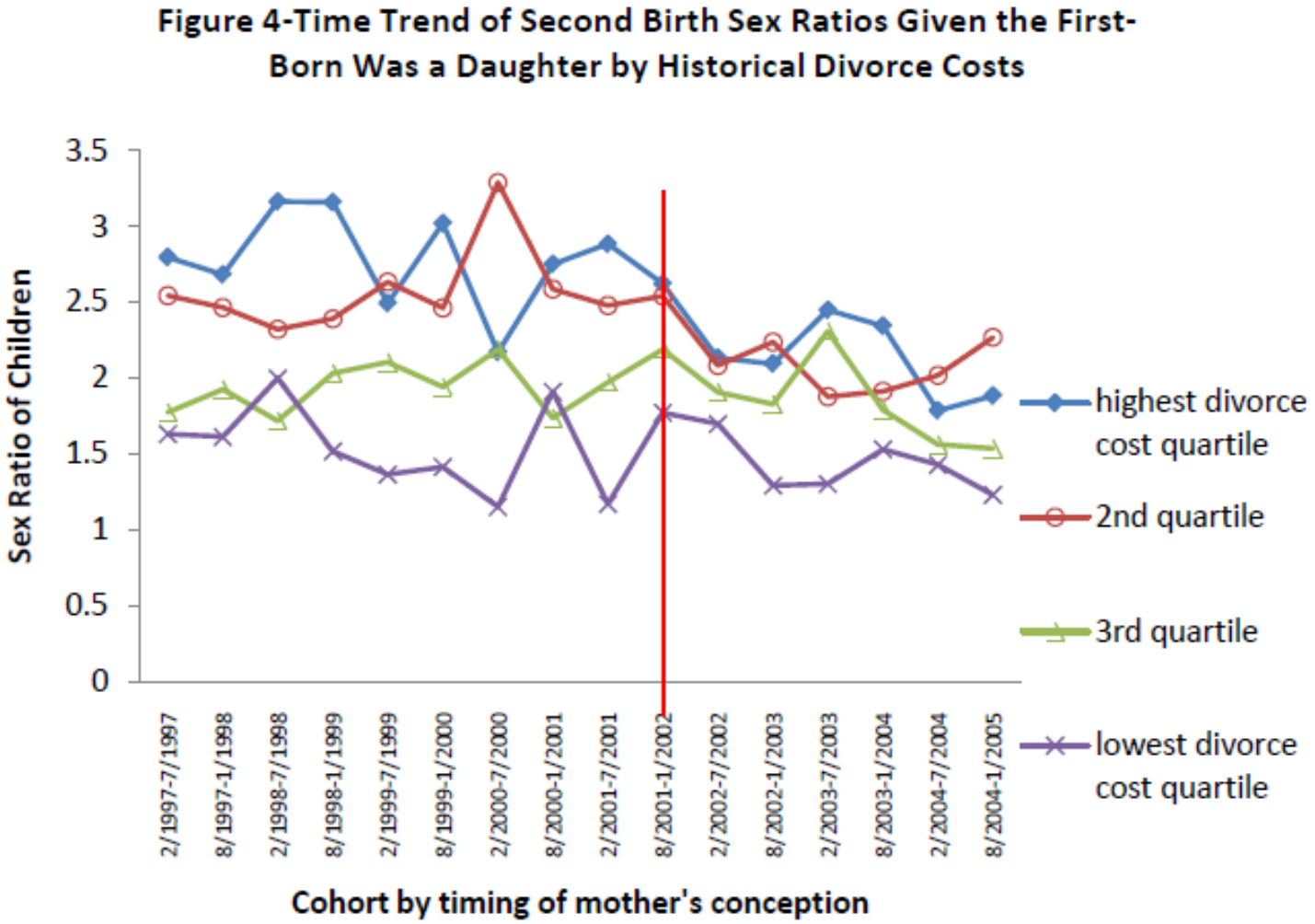


Table 8 The Sex Ratio Gap between Historically High and Low Divorce Rate Regions across Cohorts (cohort conceived within 2/1997-7/1997 as the reference group)

Dependent Variable: dummy variable indicating whether the second child is male								
	(1)		(2)		(3)		(4)	
	High vs Low (low div=1)		1st quartile vs 3rd and 4th quartiles (1st qt=1)		2nd quartile vs 3rd and 4th quartiles (2nd qt=1)		Crude divorce rate In 2000	
8/1997-1/1998*low div	-.068	(.054)	-.077	(.058)	-.059	(.066)	.154	(.083)*
2/1998-7/1998*low div	-.048	(.056)	-.027	(.060)	-.085	(.069)	.089	(.079)
8/1998-1/1999*low div	-.064	(.053)	-.041	(.058)	-.098	(.066)	.075	(.080)
2/1999-7/1999*low div	-.070	(.055)	-.073	(.059)	-.066	(.067)	.125	(.077)
8/1999-1/2000*low div	-.065	(.055)	-.058	(.059)	-.073	(.066)	.039	(.089)
2/2000-7/2000*low div	-.061	(.054)	-.085	(.059)	-.019	(.066)	.090	(.076)
8/2000-1/2001*low div	-.062	(.055)	-.058	(.059)	-.067	(.068)	.095	(.087)
2/2001-7/2001*low div	-.056	(.056)	-.047	(.060)	-.068	(.068)	.081	(.081)
8/2001-1/2002*low div	-.112	(.054)**	-.113	(.058)*	-.110	(.066)*	.159	(.079)**
2/2002-7/2002*low div	-.114	(.054)**	-.102	(.058)*	-.132	(.068)*	.194	(.069)***
8/2002-1/2003*low div	-.144	(.054)***	-.143	(.058)**	-.141	(.068)**	.171	(.081)**
2/2003-7/2003*low div	-.152	(.053)***	-.136	(.057)**	-.175	(.066)***	.127	(.075)*
8/2003-1/2004*low div	-.078	(.054)	-.077	(.059)	-.079	(.067)	.117	(.073)
2/2004-7/2004*low div	-.121	(.053)**	-.120	(.057)**	-.120	(.066)*	.133	(.071)*
8/2004-1/2005*low div	-.054	(.058)	-.064	(.063)	-.036	(.071)	.111	(.076)
Control var	Y		Y		Y		Y	
Provincial Fixed effect	Y		Y		Y		Y	
R-Square	0.0459		0.0533		0.0277		0.0434	
Observations	10,721		7,951		6,208		10,721	

Conclusions

- Better understanding of household decision making: The change in relative divorce option can change the public good provision through the channel of intra-household bargaining under the setup of non-transferable utility.
- The empowerment of women through the new pro-women divorce law causes the sex ratio for the second birth after a first born girl to decrease from 2.3-2.4 boys for one girl to 1.19-1.24 boys for one girl, holding other factors constant.
- Policy implication: the empowerment of women through improving their relative divorce options can decrease sex selection behavior.
- Decreasing sex ratio of children doesn't necessarily imply a high divorce rate. On the contrary, the new divorce law can affect abortion behavior by shifting more power to women **within** marriages.

Thank you!

Competing Interpretations

- Higher return to a daughter?
- Under-reporting of Female births?
- Does the divorce rate capture other heterogeneity?
- Lower marginal utility of public durable goods under higher risk of divorce?

Higher Return of Having a Daughter?

- Divorce rate increased for households with firstborn daughters.
- Evidence from the CHNS data:Thomas (1990) Duflo (2003) Duflo and Udry (2004)

	Dependent Variable		
	Household Protein intake (g/day)	Household Calorie intake (calorie/day)	Husband's Liquor consumption (50g/week)
	(1)	(2)	(3)
Post implementation	5.751	-40.394	-1.523
	(2.196)***	(69.616)	(0.749)**
Control var	Yes	Yes	Yes
Province F.E.	Yes	Yes	Yes
R squared	.1195	.1082	.0788
Observations	823	823	396

Evidence on Fewer Induced Abortions

- Some suggestive evidence using 2005 Census data

The birth spacing between the first and second birth decreases by 3.4 months.

- Using the data of 7 provinces, 1,009 women whose firstborns were daughters in CHNS data, the propensity of abortion decreases by 2.5%.

Does the divorce rate capture other heterogeneity?

First stage:

$$divorce\_rate_p \times post_c = \alpha + (\overrightarrow{court}_p \times post_c)\pi + post_c\rho + X_{pc}\gamma + \lambda_p + \varepsilon_{pc}$$

Second-stage:

$$male_{pc} = \alpha + (divorce\_rate_p \times post_c) \cdot \omega + post_c\rho + X_{pc}\gamma + \lambda_p + \varepsilon_{pc}$$

IV: $\overrightarrow{court}_p$ includes the number of courts per capita and the quadratic term, and the total number of courts in certain provinces.

Does the divorce rate capture other heterogeneity?

First Stage of 2SLS Estimates of the Differential Effect of The New Divorce Law on The Fraction of Male Children (Sex Ratio) across Provinces with Different Divorce Rates

Dependent variables: Divorce rate*post implementation	(1)	(2)
	All cohorts	Conceived before Feb 2003
Court pc*post	330.723	335.72
	(54.056)***	(60.560)***
Court pc squared*post	-14086.32	-14303.1
	(2704.472)***	(3030.066)***
#courts*post	-.001	-.001
	(.000)***	(.000)***
F value	30.83	24.57
Controls of other interaction terms	Yes	Yes
Provincial F.E.	Yes	Yes
Observations	382	310

Does the divorce rate capture other heterogeneity?

OLS and 2SLS Estimates of the Differential Effect of The New Divorce Law on The Fraction of Male Children (Sex Ratio) across Provinces with Different Divorce Rates

	Dependent variables: Fraction of male children			
	(1) OLS	(2) OLS	(5) IV	(6) IV
	All cohorts		All cohorts	
Divorce rate*post	.078	.102	.065	.154
	(.028)***	(.034)**	(.039)*	(.068)**
Post	-.099	-.104	-.104	-.157
	(.038)***	(.039)***	(.047)**	(.078)*
Controls of other interaction terms	Yes	Yes	Yes	Yes
Provincial F.E.	Yes	Yes	Yes	Yes
R-squred	0.2738	0.3414	0.2406	0.2701
Observations	382	382	382	382

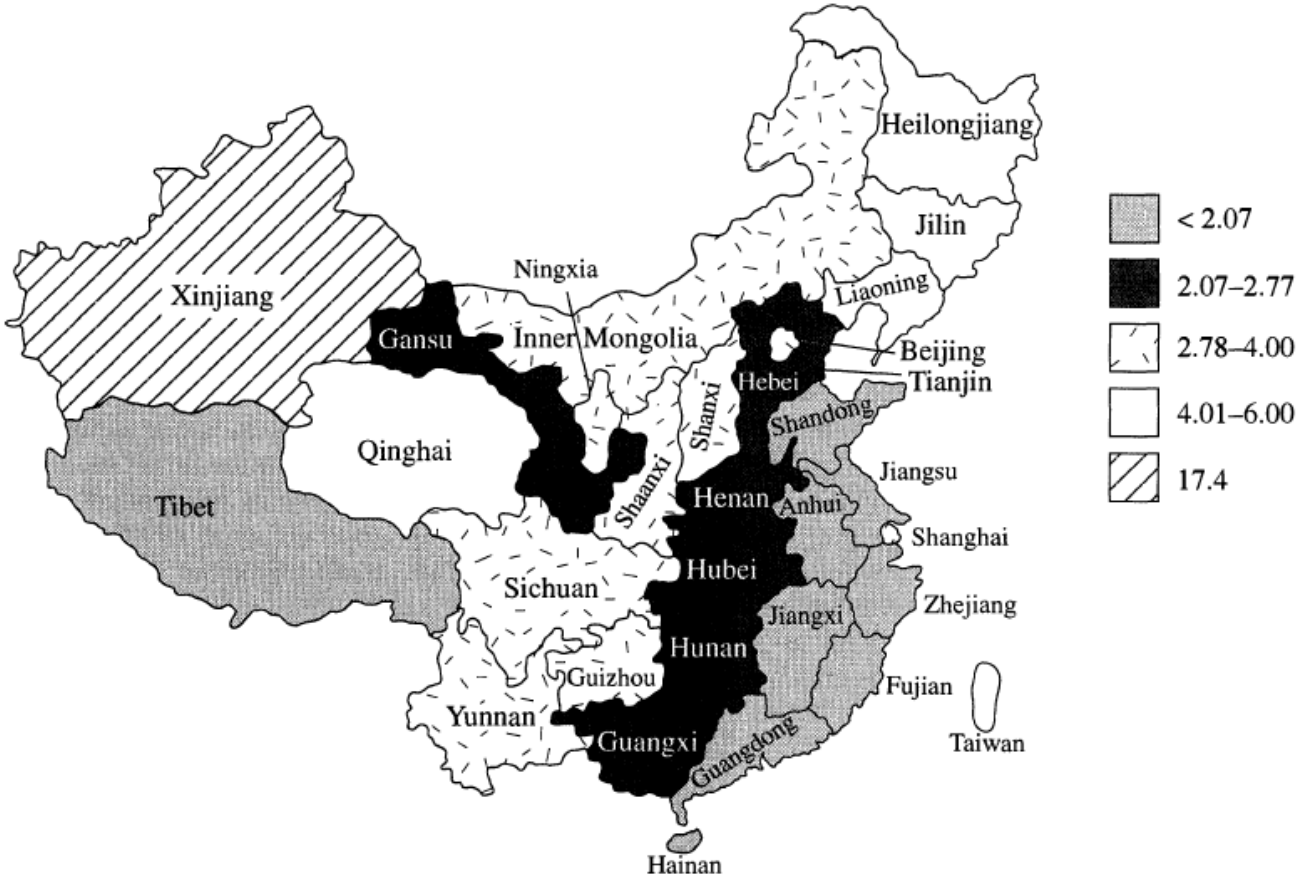
Lower Marginal Utility of Public Durable Goods?

$$public\_durable\_good_{ipt} = \alpha + firstborn\_girl_i \times post_t\beta + firstborn\_girl_i\gamma + X_{ipt}\sigma + \lambda_p + \tau_t + \varepsilon_{ipt}$$

	Dependent Variable			
	# electric Fans owned (1)	#refrigerators owned (2)	#color tv owned (3)	#camera owned (3)
Firstborn girl*Post	-0.031	-0.009	-0.001	-0.031
	(0.062)	(0.027)	(0.023)	(0.071)
Firstborn girl	0.030	-0.012	-0.115	0.007
	(.034)	(0.009)	(0.014)	(0.011)
Control var	Y	Y	Y	Y
Province F.E.	Y	Y	Y	Y
R squared	.5589	.7781	.1585	.0788
Observations	2696	3210	4342	1906

General Divorce Rate across Provinces

FIGURE 1. REGIONAL DIFFERENTIALS OF GENERAL DIVORCE RATES IN MAINLAND CHINA IN 1990



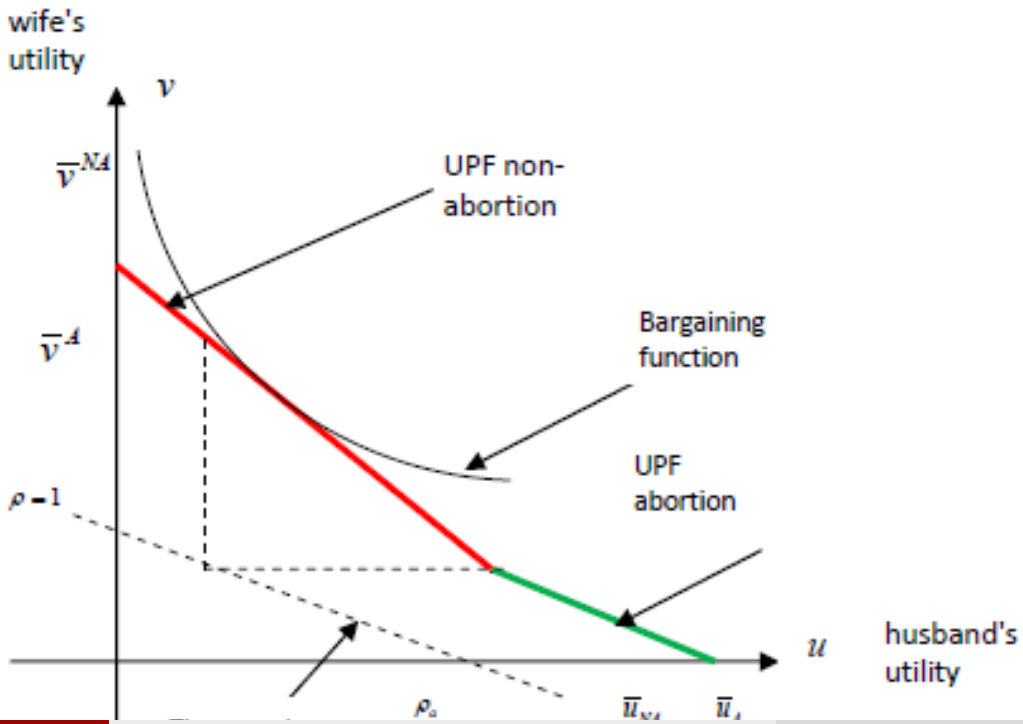
The feature of non-transferable utility

- Generalized transferable utilty

$$u_i = A(\sigma)x_i + B_i(\sigma)$$

- Under Pareto Efficiency

$$\frac{\partial u(x_H, \sigma)}{\partial x_H} = \frac{\partial v(x_W, \sigma)}{\partial x_W}$$



The Difference in Crude Divorce Rate between 2000 and 2005