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## ABSTRACT

# Women's Inheritance Rights Reform and the Preference for Sons in India\*

We investigate whether legislation of equal inheritance rights for women modifies the historic preference for sons in India, and find that it exacerbates it. Children born after the reform in families with a first-born daughter are 3.8–4.3 percentage points less likely to be girls, indicating that the reform encouraged female foeticide. We also find that the reform increased excess female infant mortality and son-biased fertility stopping. This suggests that the inheritance reform raised the costs of having daughters, consistent with which we document an increase in stated son preference in fertility post-reform. We conclude that this is a case where legal reform was frustrated by persistence of cultural norms. We provide some suggestive evidence of slowly changing patrilocality norms.

**JEL Classification:** O12, K11, I21

**Keywords:** inheritance rights, ultrasound, female foeticide, sex selection, son preference, gender, India

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\* We are grateful to Farzana Afridi, Jere Behrman and Prakarsh Singh for their valuable feedback and suggestions, and to the participants of the UPenn-CASI Workshop on Early Childhood Development in India for many helpful comments. We would also like to thank seminar participants at Rimini, CAGE-Warwick, CMPO-Bristol, Sussex for their comments. Brule would like to thank Andrew Foster for kindly providing access to the REDS 2006 dataset.

## 1. Introduction

Two centuries ago, women in Europe and America were considered the property of men (fathers or husbands), having no intrinsic rights of their own. Women's rights have been established by a long, slow process (described for instance in Doepke *et al.* 2012), in which legislation of their property rights was an important milestone. However, daughters still continue to have weaker property inheritance rights than sons in many developing countries today. In all of the Middle East and North Africa, in 50 percent of South Asian countries, 34 percent of sub-Saharan African countries, and 25 percent of East Asian and Pacific countries, inheritance laws either disfavour or altogether exclude women (World Bank 2011). Twenty-one of the 63 countries studied by Htun and Weldon (2011) have unequal property inheritance rights for men and women (WDR 2012).

This is possibly the single most important factor in compromising the position of women. Property, and in particular land, is a critical determinant of economic and social status (Agarwal, 1994). Property rights are associated with increased investment, productivity, access to credit and labour market opportunities (Besley 1995; Banerjee *et al.* 2002; Field 2007; Besley and Ghatak 2009). Land markets in developing countries are scarce and land is typically acquired through inheritance. As a result, even amongst the landowning classes, women in developing countries continue to be asset-poor, and unequal inheritance rights tend to perpetuate the condition in which women find themselves reliant upon men, having limited earning capacity and limited options outside marriage (Field 2007; Goldstein and Udry 2008; Chung and Dasgupta 2007). The equalization of inheritance rights may therefore be expected to be a powerful instrument for the empowerment of women. This has potential impacts on women's labour supply (Jouffaian 2006), fertility (Ashraf *et al.* 2014), health (Jayaraman *et al.* 2013; Calvi *et al.* 2017) and mortality (Milazzo 2014). In addition, several studies show that the financial empowerment of women directly benefits children because women tend to invest more in children (e.g. Lundberg *et al.*, 1997; Duflo and Udry 2004; Bobonis 2009; Baranov *et al.* 2017). There is also some evidence that women invest relatively more in girls, redressing gender inequality in the next generation (Thomas 1990, 1994; Dahl and Moretti 2008; Baranov *et al.* 2017).

However, causal evidence that property rights empower women is limited, and the available evidence is somewhat mixed. In the context of India, inheritance rights for women were equalized with the rights of men by five states that enacted legislative reforms between 1976 and 1994, with federal legislation imposing equal rights for all states in 2005.

A handful of studies has exploited the staggered implementation of these reforms to examine impacts on different aspects of women's status in India. One study documents increased suicide rates among both men and women aged 15–44, with a significantly larger increase among men (Anderson and Genicot 2015). The authors rationalize this in terms of increased intra-household conflict, showing also an increase in domestic violence. Focusing on girls of school-going age, rather than on women of marriageable and married ages at the time of the reform, Roy (2015) and Deininger *et al.* (2013) show that the reform was associated with increased educational investment in girls relative to boys. These results suggest that negotiating sharp changes in women's economic rights may increase marital conflict for some, while expanding marital prospects for others.<sup>1</sup>

In this paper, we ask a very different question. We investigate not whether equal property rights legislation empowers women within marriage or on the marriage market, but whether it modifies the historic tendency for Indian parents to prefer to have sons. There is hardly a more compelling statement of gender inequality in India than its unnaturally male-biased population sex ratio, an important driver of which is the desire to have sons. To what extent is this historic desire for sons driven by their stronger economic position and, in particular, their greater command over ancestral property? Might granting equal inheritance rights to daughters mellow the preference for having sons? Plausible mechanisms are that it would allow daughters to offer the old-age security that sons have traditionally provided to parents (Chung and Dasgupta 2007), or that it would diminish dowry, the burden of which raises the cost of having daughters (Anderson 2003, Bhalotra *et al.* 2016). These potential mechanisms are linked. Although dowry is now often appropriated by the groom or his parents, historically it was a pre-mortem bequest to daughters given at the time of marriage, while post-mortem bequests (typically of family property) went to sons (Goody 1973). This custom, evident in large parts of Europe and Asia, was premised upon married daughters leaving the parental home, in contrast to married sons who tended to co-reside with parents. Botticini and Siow (2003) model the institution of dowry jointly with patrilineal inheritance as emerging from parents' attempt to solve a free riding problem between siblings to create incentives for sons to provide

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<sup>1</sup> The increase in male relative to female suicides reported in Anderson and Genicot (2015) illustrates the difficulty with defining women's empowerment. It suggests that a challenge to the economic power of men generates greater distress or damage among men, but to the extent that the suicides are of *married* men, they create the perilous state of widowhood for women (Chen and Dreze 1992). However, their data are for all suicides among 15–44 year olds at the state-year level and not available by marital status. This may therefore represent suicides of *unmarried* men at the bottom of the socioeconomic distribution who find it harder to match on the marriage market after the inheritance reform.

effort. Therefore, legislation that compels parents to share post-mortem bequests equally with daughters may lead to erosion of dowry, and it may modify the convention of women marrying into households some distance away from their natal homes, while sons co-reside with or live close to parents (and family property).<sup>2</sup> Thus, to the extent that dowry and co-residence with sons determine the preference for having sons, we may expect gender equality in property rights to lower son preference.

In this paper, we investigate this issue by modelling the sex ratio at birth as a function of whether the inheritance reform in the state of birth had been passed before the year of birth. To identify causal effects of equal inheritance rights for women, we leverage the staggered implementation of the inheritance reform across the Indian states, which created state and cohort-level variation in reform exposure. We buttress this double difference using two further sources of variation that determine the degree of exposure to the reform. First, we interact the inheritance reform indicator with a cohort-varying indicator for availability of ultrasound technology, that previous work shows generated a trend break in the sex ratio at birth (Bhalotra and Cochrane 2010), and exploits the fact that prior to the introduction of ultrasound technology that made prenatal detection of the sex of the foetus feasible, no significant manipulation of the sex ratio at birth is discernible in aggregate statistics (evidence of this is provided in Bhalotra and Cochrane 2010). So, for the sex ratio at birth to have responded to the reform, the indicator for ultrasound access will need to have switched on. This aids identification because any omitted variables that drive a spurious relationship between the outcome variables and inheritance reform would have to have exhibited a discontinuous change that not only lines up with the particular state and year variation in reform (i.e. in the state of Kerala in 1976, in Andhra Pradesh in 1986 and so on) but, *in addition*, the omitted variables would have to have jumped in line with the date of introduction of ultrasound scans as well. Moreover, crossing the reform and ultrasound indicators helps us *attribute* any observed effects to parental manipulation of the sex ratio.

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<sup>2</sup> Patrilineality includes passing on the main productive assets through the male line, while women may be given some movable goods in the form of dowry or inheritance. This constrains women's ability to sustain their economic level of welfare without being attached to a man. Patrilocality involves a couple residing at the man's home, which goes hand in hand with inheritance, especially in peasant societies where land is the main productive asset that is inherited. Indeed, Agarwal (1994) posits that persistence in kinship systems and marriage practices (among which the distance of post-marital residence from the natal home is particularly significant) "may help to explain the failure to realise in practice the significantly greater inheritance rights in landed property which South Asian women now enjoy as a result of recent legislative changes".

The final source of variation deployed towards identification is the sex of the first-born child. Previous work shows that this is a) quasi-random and b) that it predicts sex-selective abortion in favour of male children: parents who, by the random hand of nature, are assigned a son at first birth are significantly less likely to engage in female foeticide for subsequent conceptions (see Almond and Edlund 2008, Abrevaya 2009, Bhalotra and Cochrane 2010).<sup>3</sup> We also show that families with first-born sons and families with first-born daughters are balanced on all relevant observable characteristics.

To summarize, the treated group is defined as a cohort born post-reform and post-ultrasound in a family with a first-born daughter. The control group includes families with first-born sons and children born pre-reform or pre-ultrasound. Since the coefficient of interest is now on an interaction between a family level variable and a state-year level variable, we can include state-year fixed effects as controls. These sweep up all potentially relevant state-varying non-linear omitted trends that may be correlated with the reform as well as with the outcome. We also test the robustness of our results to restricting the identifying variation to within-mother variation, as our sample includes pre- and post-reform birth cohorts. We corroborate our findings of changes in sex-selective abortion behaviour using data on reported son preference (i.e. desired share of sons among births). We conclude with an attempt to investigate whether passage of the reform was associated with changes in patriarchal norms, in particular, whether daughters married closer to the natal home, and whether adult sons changed their propensity to co-reside with parents.

We use the three available rounds of nationally representative individual level data from the National Family and Health Survey (NFHS) pertaining to births between 1972 and 2004. We find a significant *decrease* of 3.8-4.3 percentage points in the probability that a girl is born post-reform and post-ultrasound in households where the first-born child is a girl rather than a boy. This magnitude of decline is outside the biologically normal range.<sup>4</sup> This is 61-69% of the potential female births that Bhalotra and Cochrane (2010) estimate did not occur due to ultrasound use between 1995-2005. The most plausible interpretation of this finding is that awarding inheritance rights to women makes parents more averse to having a daughter rather than a son. In line with this, we also find an intensification of stated preferences for sons relative to daughters after the reform of approximately 1

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<sup>3</sup> The premise that the sex of the first birth is random is discussed and investigated using the same data as used in this paper in Bhalotra and Cochrane (2010).

<sup>4</sup> For a discussion of the biologically normal range of the probability that a birth is a girl rather than a boy, and evidence of the size of deviations from this norm that were apparent immediately upon the introduction of ultrasound technology in India, see Bhalotra and Cochrane (2010).

percentage point, compared to a sample mean of 0.57. These findings are consistent with the theoretical framework of Botticini and Siow (2003).<sup>5</sup>

Our finding that female foeticide increased in response to inheritance reform is robust to the inclusion of state-year of birth fixed effects. These mop up variation in state-level pro-female legislation that may have coincided with inheritance reform, for instance the establishment of Women's Commissions (discussed in Andersen and Genicot 2015). This is a relevant concern given that India has a federal system and a number of states have, at different times, implemented laws designed to limit the extent of sex-selective abortion (Sekhar 2012). State-year fixed effects also absorb the effects of economic influences on the desire for sons such as the increase in returns to education for girls relative to boys, as well as account for any differential state-level pre-trends in the outcome. Our results are also robust to controlling for mother's age and mother fixed effects, which absorb all mother-specific time-invariant variables including unobservables such as her preference for sons or her fertility preferences (as well as, of course, observables such as her education, caste and religion). The mother's preferences may determine compliance and they will adjust for any changes in the composition of women giving birth post-reform. In addition, our results are also robust to using an alternative dataset (REDS 2006). Additional robustness checks are discussed in Section 5.2.

In research conducted in parallel with ours, Rosenblum (2015) shows an increase in excess girl mortality (after birth) following the inheritance reform.<sup>6</sup> We focus on female foeticide as it is a particularly clean indicator of parental preferences for boys over girls – it involves an explicit decision to conduct prenatal detection of the sex of the child, and this needs to be followed up by the deliberate act of aborting the foetus if it is a girl. However, we also extend our analysis to study impacts of the reform on excess infant mortality among girls, and on son-biased fertility stopping. We find that the reform exacerbates son preference

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<sup>5</sup> In their model, Botticini and Siow argue that male-biased inheritance emerges from parents' optimizing choices. Forcing parents to share post-mortem bequests between sons and daughters in patrilocal societies, where married daughters leave their parental home and sons do not, will tend to result in lower parental welfare. Hence it follows that equalizing inheritance rights creates incentives for parents to prefer not to have daughters.

<sup>6</sup> Estimates of the impact of the reform on child mortality is likely to be biased by sex-selective abortion, but the bias is downward implying that Rosenblum's conclusions are robust to this selection. The bias arises because child mortality is defined on a selected sample of conceptions that survive the foetal period to the point of birth. There is no similar bias in looking at changes in (sex-selective) survival of the foetus as done in our paper.

on both these margins.<sup>7</sup> In particular, girls born post-reform and post-ultrasound in families with a first-born girl are 2.3-3.2 pp more likely to die before their reaching first birthday (equivalent to 32-42% of the sample mean), relative to boys. Moreover, children in families with first-born girl are approx. 9 pp more likely to have a younger sibling (equivalent to 12% of the sample mean), relative to families with first-born boy. Together with our sex ratio results, these findings provide compelling evidence in favour of our contention that the inheritance reform intensified son preference in fertility.

A natural question at this point is: if what we capture is a sticky social norm, do we find any corroborating evidence in support of this claim? In order to examine changes in patrilocal norms in response to the reform, we investigate the distance of married daughter's homes from their natal home and co-residence of parents with adult sons using a different dataset Rural Economic and Demographic Survey (REDS) 2006. We find an increase of 3-5 percentage points in the probability that a married daughter lives nearer the natal home (up to 10 km away). But we find no significant impact on the extent of co-residence with sons. Together, these results suggest that patriarchal norms change only very slowly if at all, so that a sharp legislative change intended to favour daughters appears to have raised the perceived cost of daughters enough that parents became more averse to having them.<sup>8</sup>

To the best of our knowledge, this paper provides the first evidence regarding the impact of the gender-equalizing inheritance reforms in India on female foeticide and son-biased fertility stopping behaviour. We also contribute to the literature by using a more stringent estimation strategy (permitting the use of state-year fixed effects as well as mother fixed effects) compared to existing studies analysing the impact of these reforms on various other outcomes.<sup>9</sup> Our findings also contribute to existing evidence indicating that slow-moving social norms may frustrate the intentions of legal reform in the first decades after the passage of legislation (Aldashev *et al.* 2011).

<sup>7</sup> All estimates are intent-to-treat estimates. Different types of families may be at the different margins.

<sup>8</sup> Legislative change pertaining to the division of bequests is (in principle if not in compliance terms) immediate. In contrast, the linked institutions of dowry, patrilocality and co-residence with sons are expected to wither gradually, as a function of changes in the agrarian economy (Rosenzweig and Wolpin 1985, Guner 1998, Botticini and Siow 2003). In particular, we may expect that, as the returns to human capital rise, sons (and daughters) will acquire an education, and sons will no longer be tied to the occupations or assets of their fathers. This will spontaneously but gradually reduce co-residence and the attachment of sons' labour to ancestral property.

<sup>9</sup> Results of some of the previous studies are potentially vulnerable to bias from omitted state- and year-varying variables.

The remainder of the paper is organized as follows. Section 2 describes the institutional background of the Hindu inheritance law in India, as well as details on the advent of ultrasound technology in the country. Section 3 describes the identification strategy and while Section 4 outlines the data and descriptive statistics. Section 5 presents the key results while Section 6 concludes.

## 2. Background

### 2a. Inheritance Rights in India

Gender inequality in inheritance rights in India was entrenched in The Hindu Succession Act (HSA) of 1956. Promulgated not long after India gained Independence from British rule, the HSA conferred the legal right to inherit ancestral property upon sons to the exclusion of daughters. Ancestral or “joint family property” consists of property principally inherited by an individual from their father, paternal grandfather or paternal great-grandfather, plus any property that was jointly acquired by them or acquired separately but merged into the joint property. Separate property, on the other hand, includes that which was self-acquired (if acquired without detriment to the ancestral estate) and any property inherited from persons other than the individual’s father, paternal grandfather or paternal great-grandfather (Agarwal 1994). Under the HSA, daughters of a Hindu male dying intestate (i.e. without a will)<sup>10</sup> had equal inheritance rights as sons to their father’s separate property only their father’s separate property and his “notional” portion of joint family property, but had no direct inheritance rights to joint family property itself. Sons, on the other hand, not only inherited their share of the father’s own property and his “notional” portion of joint family property, but also had a *direct* right by birth to a share of the joint family property. In fact, all persons who acquired interest in the joint family property by birth were said to belong to the “Hindu coparcenary”, which is conceptually similar to an exclusive male membership club in relation to the issue of inheritance to which women had no access.

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<sup>10</sup> According to Deininger *et al.* (2013), the proportion of people who die without making a will in India is very high (around 65%, and probably even higher in rural areas). Recent newspaper articles have put the number at a much higher 80% (see <https://timesofindia.indiatimes.com/Avoid-disputes-write-a-Will/articleshow/802650.cms> and <http://www.fpgindia.org/2011/07/writing-a-will-know-some-facts.html>). The main reason cited for such low prevalence of wills in India is that people often find it uncomfortable discussing a will in their lifetime because of the air of fatality and gloom that surrounds it. Thus, all this would suggest that the HSA 1956 is what ultimately determines inheritance patterns within the family.

For millions living in rural India, the most common form of property is land that is typically family-owned, which makes the gender bias in inheritance rights quite a significant phenomenon. Thus the HSA, by excluding the daughter from participating in the coparcenary ownership of ancestral property, discriminated against the daughter. This inequality is, arguably, fundamental to the vulnerability of Indian women (Agarwal 1994), who are passed from father to husband, their economic insecurity contributing to early marriage, high fertility and domestic violence.

Inheritance is one of the topics in the Indian constitution over which both the federal and the state governments have legislative authority. In 1976, the state of Kerala amended the HSA, subsequently followed by the states of Andhra Pradesh in 1986, Tamil Nadu in 1989, and Maharashtra and Karnataka in 1994 (see Figure 1). These amendments granted equal inheritance rights to women to ancestral property provided they were unmarried at the time of the reform.<sup>11</sup> In 2005, the federal Government of India swept all remaining states into the fold in a momentous constitutional reform.<sup>12</sup> Further discussion of the institutional details of the reform is in Roy (2015), Brulé (2017), Andersen and Genicot (2015), Rosenblum (2015).

As pointed out by Andersen and Genicot (2015), a key point to note here is that there does not appear to be any systematic reason for the specific years in which the different states amended the HSA. Nonetheless, our empirical estimations include state fixed effects, year fixed effects and state-year fixed effects. The last soak up variation in any other state-level pro-female legislation that may have coincided with inheritance reform, for instance the establishment of Women's Commissions (as discussed in Andersen and Genicot 2015), or other state-specific time-varying confounding factors.

Previous work has shown that the HSA amendments have been largely unsuccessful in improving inheritance of women (Roy 2015; Brulé 2017; Landesa 2013): the proportion of

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<sup>11</sup> Kerala passed a slightly different amendment in the form of the Kerala Joint Hindu Family System (Abolition) Act that recognized all family members with an interest in the undivided family estate as being independent full owners of their shares from then onwards, i.e. abolished joint family property altogether. But since the spirit of this amendment was similar to those passed by the other reforming states, and could be expected to favorably affect the inheritance of the daughter, we club them together.

<sup>12</sup> We focus only on the state-level amendments to the HSA in this paper, and hence restrict our data to 2004.

women inheriting property from their parents remains woefully small and, importantly, did not increase significantly following the reform. Parents may have been reluctant to give daughters any property due to patrilocality<sup>13</sup> and the risk that the property ends up being controlled by the in-laws of the daughters. It would thus be fair to ask why we may expect any impact of the reform on parental manipulation of the sex composition of their children. We note that the documented impacts in the cited studies refer to girls who were born before the reform. It is conceivable that, for the next generation of girls, the margin of adjustment is different. A large literature provides evidence that Indian parents have, for centuries, manipulated the sex composition of their children through son-biased fertility stopping and through lower investments in daughters (see a brief survey in Bhalotra et al. 2016 for instance). Since the mid-1980s, the availability of affordable prenatal sex detection techniques (alongside pre-existing availability of abortion) has provided a new tool to meet this end (see discussion in the following Section 2.b). The perceived cost of having a daughter is higher post-reform, because she is a potential competitor for the son in inheritance, and may contest her disinheritance in court in future. Even if parents expect that the probability that daughters contest is small, the risk may loom large – under risk-aversion, uncertainty is more heavily weighted for negative outcome. In addition, parents may incur psychic costs associated with non-compliance with the law, and with alienating their daughters. Finally, the risk that daughters contest their rights is rising and parents can probably see this insofar as a number of NGOs (e.g. Marg and SEWA) now pro-actively encourage women to claim their rights and provide legal support.<sup>14</sup> In addition, the returns to contesting rights to property (mostly land) are increasing in line with the trend of rising land prices across India.<sup>15</sup> This trend raises the stakes for contesting land. Unsurprisingly, a recent study of 9,300 litigants across 170 district courts, balanced across civil and criminal cases, found up to two thirds of cases relate to land and property.<sup>16</sup> Indeed, more than half of these disputes occurred within families (*Ibid*). As of September 2017, the *Business*

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<sup>13</sup> Patrilocality (also known as virilocality) is the social custom that requires married women to leave their parents' home and reside with their husband's family post marriage.

<sup>14</sup> See the summary of these NGO initiatives on the UN Women's website here

<http://www.endvawnow.org/en/articles/943-provide-rights-based-education-and-awareness.html>

<sup>15</sup> Rising agricultural land prices have been a noticeable trend across India since the early 2000s. As of 2013, M. Rajshekhar reports a “3 to 100-fold rise in farm land prices” drawn from microstudies and anecdotal information on 68 villages in seven states gathered by the Economic Times <https://economictimes.indiatimes.com/news/economy/agriculture/great-rural-land-rush-3-to-100-fold-rise-in-farm-land-prices-may-not-bode-well/articleshow/25607513.cms>

<sup>16</sup> Source: <http://www.livemint.com/Politics/DIIsfcuswHskm1jkXNdI2M/A-case-to-revisit-Indias-land-laws.html>

*Standard* notes “land conflicts [are] brewing across the country,”<sup>17</sup> as well as creating family feuds across national boundaries,<sup>18</sup> echoing writing over the past decades on “growing conflict about land.”<sup>19</sup>

## *2b. Sex Ratios and Ultrasound Technology in India*

The phenomenon of “missing women” was initially highlighted in Miller (1981) and Sen (1990). Recent estimates suggest that among the stock of women who could potentially be alive in India today, over 25 million are “missing” (Anderson and Ray 2012). The population sex ratio in India has been unnaturally skewed in favour of males since the first recorded census. Historically, parents have manipulated the ratio of surviving girls to boys by the practice of son-biased fertility stopping (Arnold *et al* 1998; Bhalotra and van Soest 2008) and multiple forms of neglect including breastfeeding duration, immunization and nutrition (Jayachandran and Kuziemko 2011; Oster 2009).

Since the availability of prenatal sex detection technology, it has become possible to manipulate the sex ratio by committing sex-selective abortion or female foeticide, the material and psychic costs of which tend to be lower than the costs of eliminating girls after birth. Abortion has been legal in India since 1972 and the costs of scans and abortion are affordable, albeit with prices increasing in the reliability and safety of these procedures. It is estimated that as many as 0.48 million girls or 6% of potential female births during 1995-2005 were selectively aborted annually during 1995-2005, which is more than the number of girls born in the UK each year (Bhalotra and Cochrane 2010). In contrast to the more subtle and uncertain procedure of neglect, foeticide is a conscious and staged act and, for this reason, it provides a clean measure of parents’ preferences for having sons rather than daughters.

The first imports of ultrasound scanners are recorded in the mid-1980s, associated with India’s first attempt at import liberalization (see Figure 2b). There was a sharp increase in availability in the mid-1990s, created by the relaxation of industrial licensing requirements,

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<sup>17</sup> Source: [http://www.business-standard.com/article/economy-policy/conflicts-across-india-as-states-create-land-banks-for-industry-investment-117091900140\\_1.html](http://www.business-standard.com/article/economy-policy/conflicts-across-india-as-states-create-land-banks-for-industry-investment-117091900140_1.html)

<sup>18</sup> Source: <http://www.bbc.com/news/world-asia-21171262>

<sup>19</sup> Source: <https://www.voanews.com/a/india-witnesses-growing-conflict-over-land-101293609/124357.html>

which led to domestic production of ultrasound machines.<sup>20</sup> Bhalotra and Cochrane (2010) show that the sex ratio at birth for birth orders two and higher tracks the availability of ultrasound, and that a disproportionate share of foeticide is conducted in families with a first-born girl. They also show that the sex of the first-born child is quasi-random and Abrevaya (2009) shows this is also the case for Indian and Chinese families in US. Although stated son preference in India is declining over the sample period (Figure 3), Figure 4 shows a sharp drop in the probability of a female birth that appears after ultrasound is introduced and that this is evident only in families with a first-born girl.

A common pattern in India and other patrilineal societies is that daughters take their bequest at marriage as dowry and marry some distance from their natal home (Guner 1998; Rosenzweig & Wolpin 1985), while sons tend to co-reside with parents, work on the land, and subsequently inherit it. Indeed, Botticini and Siow (2003) postulate that a rationale for the origin and persistence of these arrangements is that they incentivize sons to work on the father's land, contributing to wealth creation as well as old-age security.

### **3. Identification Strategy**

Our objective is to obtain causal impacts of the inheritance reform on the preference for sons in India. For reasons discussed earlier, we focus upon son preference expressed in the stark act of sex-selective abortion (or female foeticide). With appropriate caveats, we seek to corroborate this with data on reported son preference. We then extend the analysis to study the impacts of the reform on excess girl mortality after birth, and son-biased fertility stopping, both of which are alternative ways of adjusting the sex composition of surviving births. This allows us to check whether prenatal mortality (foeticide) is simply substituting for postnatal mortality or fertility stopping behaviour, or if, potentially, different women respond to the reform on these different margins.<sup>21</sup>

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<sup>20</sup> For further details on the timing of availability of ultrasound technology etc. please refer to pages 12-13 in Section 5 of Bhalotra and Cochrane (2010).

<sup>21</sup> We do not directly analyse investments after birth because data on investments (including breastfeeding, vaccinations, visits to the doctor etc.) are only gathered for children born 3-5 years before the survey date. The survey dates of the NFHS rounds are such that we do not have sufficient pre- versus post-reform variation in these data. However, other studies show that under-investment in girls (relative to boys) maps into excess girl mortality in India (see Anukriti *et al.* 2016, and references therein).

The reform was implemented by individual state governments, so its incidence was staggered across the states, generating potential discontinuities in the outcome specific to state and birth year. However, as sex-selective abortion was infeasible prior to availability of prenatal sex detection technologies (which we henceforth abbreviate as “ultrasound”), we expect that any impacts of the reform on desired sex ratios will only be evident in the actual sex ratio at birth among post-ultrasound cohorts. We therefore interact an indicator for cohorts born post-reform ( $R_{st}$ ) with an indicator for cohorts born post-ultrasound ( $Post_t$ ), allowing for the main effects of each.

The initial estimated equation for sex ratios is:

$$y_{imst} = \alpha_s + \beta_t + \delta R_{st} + \theta(R_{st} * Post_t) + \varepsilon_{imst} \quad (1)$$

where  $y_{imst}$  is a binary variable that equals 1 if child  $i$  born to mother  $m$  in state  $s$  in year  $t$  is a girl and zero otherwise, multiplied by 100 so that the regression coefficients can be interpreted as percentage point changes in the sex ratio at birth.  $\alpha_s$  and  $\beta_t$  represent state and year of birth fixed effects.  $R_{st}$  is a dummy variable that equals 1 for cohorts born after the reform in a reforming state and zero for cohorts born before the reform or in a non-reforming state. For instance,  $R_{st}$  defines potential births in Kerala in 1970-1975 and in Karnataka in 1970-1993 as unexposed to the reform, and potential births in Kerala in 1976-2005 and in Karnataka in 1994-2005 as exposed.

In the absence of sex-selection, the outcome probability (i.e. the probability that a birth is a girl) is close to a half (see Appendix Table A5). After and only after the introduction of ultrasound scanners, the share of girls at second and higher-order births has declined steadily, and the magnitude of the decline lies outside the range regarded as consistent with biological variation or slow-changing environmental factors (Bhalotra and Cochrane 2010). Following Bhalotra and Cochrane (2010) who test and defend this formulation with reference to the timing of the first ultrasound scanner imports in India, we construct a dummy variable  $Post_t$  which equals 1 if the year of the child’s birth is after 1985 and zero otherwise. Since diffusion of ultrasound scanners at the state level is likely to be correlated with demand, we use the national timing of ultrasound availability determined by exogenous changes in India’s import tariffs. The coefficient of interest in this specification is  $\theta$ , which captures the differential impact of the inheritance reform on sex ratios at birth after the availability of ultrasound technology.

A familiar concern with difference-in-difference specifications such as (1) is that there may be state-specific time-varying omitted variables that are correlated with the introduction of the inheritance reform. Put differently, the identifying assumption is that the dates of introduction of the reform are not correlated with pre-trends in the sex ratio at birth. If the sex ratio at birth was already growing more male-biased in the reforming (southern) states than in the non-reforming states, we may spuriously attribute to inheritance reform a post-ultrasound North-South differential in sex-selection. The empirical significance of this concern is limited *a priori* by the fact that sex-selective abortion initially grew more rapidly in the Northwest. We nevertheless address this concern as follows.

We create *within-state* variation in the incentive to sex-select at the family level by the sex of the first-born child on the premise that this is quasi-random: parents who, by the random hand of nature, are assigned a son at first birth are significantly less likely to engage in female foeticide for subsequent conceptions (see Almond and Edlund 2008, Abrevaya 2009, Bhalotra and Cochrane 2010).<sup>22</sup> This begets us two advantages. First, any omitted variables at the state-year level would now have to vary systematically between families in a state and birth-year with the sex of the first-born child. Second, we can now include state-year fixed effects that flexibly control for any omitted state-year variation that may be correlated with the passage of inheritance reform. This richer, more rigorous specification is:

$$y_{imst} = \alpha_s + \beta_t + \gamma_{st} + \delta R_{st} + \theta_1 firstgirl_m + \theta_2 firstgirl_m * R_{st} + \theta_3 R_{st} * Post_t + \theta_4 firstgirl_m * Post_t + \theta_5 (firstgirl_m * R_{st} * Post_t) + \varepsilon_{imst} \quad (2)$$

where  $firstgirl_m$  is an indicator for the first-born child of the mother  $m$  being female,  $\gamma_{st}$  denotes state-year fixed effects, and the rest of the notation is as defined for equation (1). We consistently control for all of the underlying main effects and secondary interaction terms.<sup>23</sup> Since we include state-year fixed effects, the level effect of the reform ( $\delta$ ) and the coefficient ( $\theta_3$ ) on the interaction term  $R_{st} * Post_t$  are longer identified. However, our confidence that we identify causal effects is substantially increased. The coefficient of

<sup>22</sup> Bhalotra and Cochrane (2010) discuss and investigate the premise that the sex of the first birth is random using the same data as ours. In our sample too, we find no significant change in sex ratio of first births post reform post ultrasound (see Appendix Table A2c).

<sup>23</sup> In other words, we control for the main effect “ $firstgirl$ ”, an indicator for families with a first-born girl vs a first-born boy. As having a son confers “status” on Indian women, it may be that mothers with first-born boys have more say and are allowed to keep a later-born girl. This is accounted for by the main effect.

interest is now  $\theta_5$ , which captures the impact of inheritance reform on the relative chances that a birth is female in households with a first-born daughter relative to households with a first-born son in the post-ultrasound period. We model the sex ratio only for second-order births to mitigate concerns about potentially endogenous continuation of fertility. However, our results are qualitatively similar when we include third and fourth-order births (see discussion in Section 5.2).

As discussed in Section 2.2, although ultrasound scanners were imported into India from the mid-1980s onwards, there was a sharp increase in their supply once local production was initiated in the wake of industrial de-licensing and related economic reforms in the mid-1990s (Figure 2a).<sup>24</sup> To capture this non-linearity in evolution of supply, we divide the post ultrasound period into an early and a late diffusion period and replace  $Post_t$  with  $p_2$  and  $p_3$  in equations (1) and (2), where  $p_2$  equals 1 for 1985-94 (early ultrasound period) and  $p_3$  equals 1 for 1995-2004 (late ultrasound period).  $p_1$  is the omitted category that captures the pre-ultrasound period 1972-1984. The rationale for these choices is detailed in Bhalotra and Cochrane (2010). Our findings are not sensitive to varying the cut-off years by one or two years on each side of the chosen thresholds or to using  $Post_t$  instead of  $p_2$  and  $p_3$  separately (results are available on request).

Since the reform is at the state level and the sex of the first-born child is quasi-random, any differential uptake of ultrasound scans and sex-selective abortion facilities across mothers within a state will affect interpretation of the distribution of impacts on the treated but will not bias interpretation of the average intent-to-treat effects. However, there may be mother-level selection into conception that is correlated with the inheritance reform and the availability of prenatal sex detection facilities. To allow for this, we show estimates conditional upon mother fixed effects. These comprehensively control for time-invariant

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<sup>24</sup> The first private clinic offering sex determination is thought to have appeared in 1982-83 (Sudha and Rajan 1999). After that supply increased at an increasing rate, fuelled by imports and the growth of local production. In the mid-1990s large-scale local production of ultrasound scanners was initiated by General Electric and other multinational firms in joint ventures with Indian firms (Grover and Vijayvergiya 2006, Mahal *et al.* 2006, Murthy *et al.* 2006). Government data show that the number of ultrasound machines manufactured in India increased 15-fold between 1988 and 2003 with especially marked increases after 1994 (Grover and Vijayvergiya 2006, Murthy *et al.* 2006, George 2006). Ultrasound scans dominate prenatal sex-determination, as they are relatively cheap and non-invasive. The technology is continuously improving, providing finer resolution of the fetal image earlier in pregnancy, and the scanners are growing smaller and more mobile.

differences between mothers in preferences, information and local facilities. The estimated equation looks similar to (2) except that  $\alpha_s$  is replaced with  $\alpha_m$ .

Indian states are large regions that, in many cases, have their own languages and inter-state migration is small in general (Topalova 2010). However, we cannot rule out that the reform may have stimulated inter-state migration. Since we measure the incidence of reform in terms of the potential birth year of the child, a specification including mother fixed effects ensures that selective migration will not bias our estimates unless it occurs between births. To investigate this potential bias, we re-estimate the equation restricting the sample to women who had not migrated between births. This can be done because the NFHS surveys ask women how long they have lived in their current place of residence, and we know the birth year of their first and last child. Since the current place of residence is much more local than their state, this is a stringent test. Our results are found to be robust (available on request.)

As discussed earlier, previous studies on India's inheritance reform exploit state and birth cohort variation in exposure to reform, which leaves the estimates potentially vulnerable to bias from omitted state- and year-varying variables. In addition, previous studies either impose an age of exposure of the mother (e.g. Roy 2015) or use aggregate data and thus cannot account for individual variation in exposure (e.g. Anderson and Genicot 2015). In contrast, our paper has a clean measure of exposure defined on the potential birth year of the child, obtains within-mother estimates, as well as shows robustness to using mother-level exposure to the reform (see Section 5.1). Our identification strategy is therefore considerably more stringent.

The specification estimated to investigate changes in reported son preference is:

$$s_{imst} = \alpha_s + \beta_t + \gamma_{st} + \delta R_{st} + \theta_1 firstgirl_m + \theta_2 firstgirl_m * R_{st} + \theta_3 R_{st} * Post_t + \theta_4 firstgirl_m * Post_t + \theta_5 (firstgirl_m * R_{st} * Post_t) + \varepsilon_{imst} \quad (3)$$

The dependent variable  $s_{imst}$  is the desired or ideal number of sons divided by the desired or ideal number of total children reported by the mother. The resulting variable is bounded between 0 and 1. However, this is measured only at the time of survey. We construct a time-varying measure of reported son preference based upon the age of the mother at the time of the survey by exploiting cohort variation in stated preferences. We assume that son

preference at age 20 determines fertility choices, but we confirmed that small variations on this choice of age made no significant difference to the results.<sup>25</sup> Thus, women aged 20 in the survey year 1998 will define son preference for their cohort for the year 1998, while the preferences of women aged 40 who were surveyed in 1998 will define son preference in 1978, the year in which they were age 20. This assumes that reported preferences do not vary within woman as a function of her ages or her fertility experience. This is a strong assumption and so we only regard these findings as auxiliary – as an attempt to look at whether stated preferences are consistent with our evidence on behavioural change. In this specification, the “Reform” variable  $R_{st}$  is a dummy variable that equals 1 if the mother is 20 years old at the time of the reform in the reforming state and zero otherwise.

As discussed earlier, we also estimated the impacts of the inheritance reform on excess girl mortality and fertility. The mortality specification is:

$$c_{imst} = \alpha_s + \beta_t + \gamma_{st} + \delta R_{st} + \theta_1 firstgirl_m + \theta_2 firstgirl_m * R_{st} + \theta_3 R_{st} * Post_t + \theta_4 firstgirl_m * Post_t + \theta_5 (firstgirl_m * R_{st} * Post_t) + \varepsilon_{imst} \quad (4)$$

where the dependent variable  $c_{imst}$  captures whether or not the child died on or before reaching their first birthday, multiplied by 100. The rest of the variables are as in equation (2). We present results differentially by gender of child. The fertility specification is:

$$f_{imst} = \alpha_s + \beta_t + \gamma_{st} + \delta R_{st} + \theta_1 firstgirl_m + \theta_2 firstgirl_m * R_{st} + \theta_3 R_{st} * Post_t + \theta_4 firstgirl_m * Post_t + \theta_5 (firstgirl_m * R_{st} * Post_t) + \varepsilon_{imst} \quad (5)$$

where the dependent variable  $f_{imst}$  is an indicator for whether or not the child has younger siblings, multiplied by 100. The rest of the variables are as in equation (2).

#### 4. Data and Descriptive Statistics

We primarily use the National Family Health Survey (NFHS) which records the complete fertility histories of women aged 15-49 and thus allows us to identify the sex and birth year of every child for a representative and large sample of women. The birth histories of women span the pre- and post-reform periods in every reforming state. In addition, the

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<sup>25</sup> 70% of women in our NFHS sample have had their first child by the age 20. The median age at first birth is 18. Our results for son preference are robust to using 18 instead of 20 years as the cut-off age for mother’s son preference to determine her fertility choices.

birth histories identify biological children of a mother rather than all children in a household. The surveys also ask women their desired number of children, and the desired numbers of sons and daughters. The NFHS is a household survey representative at the state level, covering 26 states of India,<sup>26</sup> and it was conducted in three waves in 1991/2, 1998/9 and 2005/6. We pool the three rounds and use data on birth cohorts 1972 to 2004. As mentioned in Section 3, we restrict the sample for our main analyses to up to second-order births (but also present additional results later including up to third and fourth-order births for robustness). The total number of births is 490,833, born to 169,728 mothers. Of this, 169,728 are first order births and 136,013 are second order births.

Descriptive statistics are presented in Appendix Table A9. The mean sex ratio (females/males) is 0.48 while mean son preference (defined as the ratio of ideal number of sons over ideal number of total children reported by mother) is 0.57. Appendix Table A10 shows a pre-reform balance test between reforming and non-reforming states. The only systematic difference is in reported son preference.

In order to explore the impact of the inheritance reform on co-residence of parents and sons and the distance of daughters' marital home from their natal home, we use the Rural Economic and Demographic Survey (REDS) 2006. The REDS is a nationally representative survey for rural India that covers 17 major states.<sup>27</sup> To maintain comparability with the NFHS sample, we use data on birth cohorts 1972 to 2004. The analysis sample comprises 41,105 individual births born to 8,154 mothers.

## Results

### 5.1 Sex Ratios at Birth

Table 1 presents estimates of equation (2). The reported coefficients are the triple-difference coefficients that capture the differential impact of the reform on the probability that a birth is female among post-ultrasound cohorts in families with a first-born girl

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<sup>26</sup> The HSA (1956) did not apply to Jammu and Kashmir (Agarwal, 1995), hence this state is excluded from the analysis. In addition, Sikkim, one of the smaller states, is not part of the 1992 wave.

<sup>27</sup> The 17 major states are: Punjab, Haryana, Uttar Pradesh, Rajasthan, Bihar, West Bengal, Madhya Pradesh, Gujarat, Maharashtra, Karnataka, Kerala, Tamil Nadu, Andhra Pradesh, Orissa, Chattisgarh, Uttarkhand and Jharkhand, which account for more than 92% of the total population of India (Census of India, 2011).

relative to families with a first-born boy.<sup>28</sup> Our estimates in Column 1 indicate a significant decline in the probability that a birth is a girl by 3.8-4.3 percentage points (pp). These results are robust to inclusion of household level control variables (Column 2), state-specific linear trends (Column 3) and state-year fixed effects (Column 4). After conditioning on state-year fixed effects, we cannot separately identify the double-difference coefficients for  $R_{st} * p_{2t}$  and  $R_{st} * p_{3t}$ . Our estimates are larger though not statistically significantly different conditional on mother fixed effects (Column 5). All throughout, we cannot reject equality of the coefficients for the two periods marking early and late diffusion of ultrasound. This is plausible given that “treatability” was higher in the first period (low hanging fruit) while availability of ultrasound was higher in the second period, and these differences may offset one another.

We also investigate the robustness of our findings to using mother’s exposure rather than child’s exposure to the reform. The estimating equation for this specification is:

$$f_{mst} = \alpha'_s + \beta'_t + \gamma'_s t + \delta' E_{st} + \varepsilon_{mst} \quad (6)$$

where  $f_{mst}$  denotes the proportion of female births to mother  $m$  who was born in year  $t$  living in state  $s$ . This is specifically measured as  $G/(G+B)$  where  $G$  is the number of girl children and  $B$  is the number of boy children born to the mother. The mother’s exposure to the inheritance reform is denoted by a binary variable  $E_{st}$  that equals 1 if the mother was 14 years old or younger at the time of reform and zero otherwise.<sup>29</sup> The coefficient of interest is  $\delta'$  which captures the differential impact of the reform on the proportion of female births to treated mothers, relative to control mothers (i.e. those who were 15 or older at the time of reform in the reforming states or those who lived in non-reforming states). The results are presented in Appendix Table A4. We estimate that the proportion of female births declined by 0.01 in response to the reform (Column 1), consistent with increased female foeticide as discussed based on results for child-level exposure presented in Table 1. The mother-level result is robust to controlling for household level variables (Column 2) and the coefficient is also robust to state-specific linear trends even if it loses statistical significance at conventional levels (Column 3).

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<sup>28</sup> The full specification (including the double and triple-differences and level coefficients) is reported in Appendix Table A1.

<sup>29</sup> The state-level reforms that we investigate here only affected the inheritance potential of women if they were unmarried at the time of the passage of the reform in their state. Data from NFHS indicates that less than 10% women are married before turning 14.

## 5.2 Sex Ratios at Birth: Robustness Checks

We have already discussed robustness of our sex ratio results to some fairly demanding controls, including state-year fixed effects and mother fixed effects. We now present some further checks. First, we control for state-year variation in the establishment of State Commissions for Women which, in principle, may confound the estimated impacts of the inheritance reform on sex ratios at birth. In fact, our inclusion of state-year fixed effects addresses this (and all other state-year varying omitted variables) but we nevertheless do this to identify how the coefficient of interest changes in response to controlling for this identified reform.<sup>30</sup> First we include an additive control but in a second specification we interact the Women's Commission indicator with  $\text{firstgirl}_m * \text{post}$  (see Table 2, Columns 1-2). In both cases, we control for state-specific linear trends instead of state-year fixed effects. Our results stand up to this specification change.

Maharashtra and Karnataka passed the reform after the constitutional amendment mandating a third of seats in village councils for women in local government in India. We test robustness of our results to the exclusion of Maharashtra and Karnataka from the sample (see Table 2, Column 3). The coefficients are not significantly different.

Since the reforming states are all in southern India we need to address the potential concern that we are merely picking up a “southern state effect”. Although inclusion of state fixed effects and state-year fixed effects ought to account for any such time-invariant or time-varying effects, we nevertheless repeat our analysis by restricting our sample to only the reforming states i.e. Kerala, Andhra Pradesh, Tamil Nadu, Maharashtra and Karnataka, allowing us to exploit variation in dates of reform within southern Indian, rather than variation between the southern and the northern parts of the country. With this restricted sample, we are no longer able to identify different coefficients for the early and late diffusion of ultrasound because all the reforming states had passed the reform by 1994. Hence, we use a common *post* period that covers the entire period of ultrasound availability from 1985 onwards. We continue to find evidence for increase in female foeticide following the passage of inheritance reform (see Table 2, Column 4). Note that

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<sup>30</sup> Anderson and Genicot (2015) control for legal changes such as this but their outcome (suicide) is measured at the state-year level, and so their specification cannot accommodate state-year fixed effects.

the southern states have historically exhibited lower son preference<sup>31</sup> and are known to be more gender progressive than states in north India. Thus, it seems reasonable to think that these states would equalize inheritance rights for women earlier. This is likely to be correct, but we address it in our analysis.<sup>32</sup>

Alternative explanations for sex ratios at birth becoming more male-biased in general include differential access to technology across families, differences in returns to education for boys versus girls, etc. However, our strategy effectively looks for systematic differences between first-son and first-daughter families that, we show, are balanced on observable characteristics (see Table 8). This implies that our findings are robust to these macro-level changes. Moreover, our strategy does not pick up general trends in the sex ratio but, rather, sharp changes in the sex ratio coincident with the introduction of inheritance reform.

Another concern may be that sex ratio at birth of second born children is unlikely to reflect the (complete) average effect of reform on a family's total gender composition of children as mother's fertility is unlikely to be complete. To address this concern, we test the robustness of our sex ratio results by restricting our sample to mothers who have completed fertility defined in one of two ways: a) mothers aged 35 or more at the time of survey and b) mothers whose actual fertility is greater than desired fertility. As Table 2, Columns 5-6 indicates, our results remain qualitatively unchanged.

As mentioned in Section 3, we focus upon second births to mitigate the concern that a select group of women progresses to third and higher birth orders, while 93.7% of Indian women have a 2<sup>nd</sup> birth (NFHS data). However, we also test the robustness of our sex ratio results to including up to third births (see Appendix Table A2a) and up to fourth births (see Appendix Table A2b). The pattern of results is similar, indicating increased female foeticide after the reform, but the coefficients are smaller and not consistently statistically significant.

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<sup>31</sup> Appendix Table A10 indicates that reported son preference in our NFHS dataset is 0.56 in the southern states compared to 0.6 in the rest of India.

<sup>32</sup> We do this essentially by controlling for state-year fixed effects and looking to identify sharp changes in the trend in the sex ratio at birth in first-born girl families relative to first-born boy families that are coincident with the state-specific year of the reform. We find increased female foeticide following the reform even though these are states with lower son preference.

Finally, we test robustness of our sex ratio results to using our alternative dataset REDS 2006 (see Appendix Table A3a). We observe a similar pattern of results as with NFHS.<sup>33</sup> We also repeat our array of further robustness checks outlined above using REDS 2006 (see Appendix Table A3b) and observe a similar pattern of results.

### *5.3 Reported Son Preference*

Table 3 reports results for stated son preference. The reported coefficients are the triple-difference coefficients.<sup>34</sup> We find that families with first-born girls are 1 pp more likely to report preferring sons over daughters relative to families with first-born sons, post-reform and post-ultrasound (Column 1). This is robust to inclusion of household level controls (Column 2), state linear trends (Column 3) and state-year fixed effects (Column 4).<sup>35</sup> These results are therefore consistent with evidence of sex selection discussed earlier. Women who are in the early reproductive years (around age 20) after the reform report higher son preference than women of the same age before the reform. This result is particularly striking since overall son preference has been declining in India (see Figure 3). Thus reported preferences for sons move in line with revealed preferences for sons (revealed in manipulation of the sex ratio at birth and, as we shall see below, also in post-birth girl mortality and son-biased fertility stopping).

### *5.4 Excess Girl Mortality After Birth*

Excess girl mortality after birth is an alternative to female foeticide when parents want to adjust the sex composition of surviving births. Our results for the impact of the reform on excess girl mortality is presented Table 4. The reported coefficients are the triple-difference coefficients.<sup>36</sup> The estimates indicate that post-reform and post-ultrasound, girls in families with a first-born girl are 2.3-3.2 pp more likely to die before reaching their first birthday, relative to boys (Column 1). This effect is equivalent to 32-42% of the sample mean for child mortality among girls (which is 7.3%). This result is robust to inclusion of all controls (Columns 2-5). In contrast, boys in families with a first-born girl are 1.2-1.6 pp less likely to die before reaching their first birthday relative to the control group. Averaging across the

<sup>33</sup> The coefficients are larger but the sample is now rural and not matched to the NFHS sample.

<sup>34</sup> The full specification (including the double and triple-differences and level coefficients) is reported in Appendix Table A5.

<sup>35</sup> Since by construction, the dependent variable does not vary within mother, we do not present results from a specification with mother fixed effects for reported son preference.

<sup>36</sup> The full specification (including the double and triple-differences and level coefficients) is reported in Appendix Table A6.

country, there is evidence of some substitution of postnatal mortality with prenatal mortality (foeticide) in the post-ultrasound period (Anukriti et al. 2016). Here we see that in the reforming states, the equalization of inheritance rights led to the elimination of girls in both ways, before and after birth. This is consistent with different women responding to the inheritance reform on these different margins.

### *5.5 Son-Biased Fertility Stopping*

A further margin of adjustment in the sex of surviving births is son-biased fertility stopping. Our estimates for the impact of the reform on such behaviour is presented in Table 5. The reported coefficients are the triple-difference coefficients.<sup>37</sup> We find that, following inheritance reform, in the late ultrasound period ( $\beta_3$ ), children in families with first-born girl are approximately 9 pp more likely to have a younger sibling (equivalent to 12% of the sample mean) relative to the control group (Column 1). The coefficient for the early ultrasound period ( $\beta_2$ ) is also positive, though not statistically significant.<sup>38</sup> These results are robust to controls (Columns 2-4), and increase in magnitude following the addition of mother fixed effects (Column 5).

Taken together, our finding that inheritance reform exacerbates son preference in each of the three domains in which it may be potentially exercised, is compelling. As discussed in Section 5.3 above, this is also corroborated with available data on reported son preference. Given the caveats discussed earlier with the stated preference results, we underline the behavioural evidence of son-preferring behaviours in three different domains, all of which are consistent with stated son preferences.

### *5.4 Investigating (Shifting) Social Norms*

Historically, sons have enjoyed inheritance rights to family property in the form of post-mortem bequests in societies where daughters receive pre-mortem bequests in the form of dowry (Goody 1973). Botticini and Siow (2003) formalize this in terms of parents incentivizing sons to provide effort on family property. A critical premise is that sons stay and co-reside with parents while married daughters leave the parental home (patrilocality).

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<sup>37</sup> The full specification (including the double and triple-differences and level coefficients) is reported in Appendix Table A7.

<sup>38</sup> We cannot reject the equality of the coefficients for the two ultrasound periods at conventional levels.

Forcing parents to share post-mortem bequests equally between daughters and sons may therefore have implications for familial ties. In this section, we explore the impact of the inheritance reform on two specific dimensions of such ties: co-residence of parents and adult sons, and distance of daughters' marital homes from their parental homes.

By requiring parents to give daughters and sons equal portions of ancestral property, the inheritance reform limits parents' ability to reward sons' effort invested in family property that may in turn lower their inclination to co-reside. Hence, we first test whether exposure to the reform impacts the incidence of co-residence between parents and adult sons. Co-residence is the most widespread form of physical, emotional, economic, and social security for parents in contemporary India. 49% of the sample co-reside with adult sons.

We define a parent-offspring pair in a given household as “exposed to reform” if there is at least one daughter in the household who is 14 years or younger at the time of reform (i.e. likely to be unmarried).<sup>39</sup> This is similar to our measure of mother's exposure to reform (Equation 6). We study two sets of parent-offspring pairs: the household head's parents (ie grandparents) residence with the head and his or her siblings, and the household head's residence with his or her children (grandchildren). Adult sons are defined as married men aged eighteen years or older at the time of the survey.<sup>40</sup> We restrict the sample to the subset of families with at least one living parent and one adult son, with any number of adult daughters (including none).

The estimating equation for co-residence of parents and adult sons is:

$$C_{ihgst} = \alpha''_s + \beta''_t + \gamma'' t_{st} + \mu'' E_{hgst} + \varepsilon_{ihgst} \quad (7)$$

where the dependent variable of interest,  $C_{ihgst}$  is a dummy variable capturing whether or not any surviving parent  $i$  (parent with adult children) in a given natal household  $b$  from parent-child pair  $g$  in state  $s$  born in year  $t$  co-reside with an adult son.  $E_{hgst}$  is a dummy variable that captures the pair's exposure to reform as defined above.

The results are presented in Table 6. Conditioning on state and year of birth fixed effects and household level control variables, the coefficient on exposure to reform is negative but not statistically significant (column 1). Restricting to married adult sons only, and adding

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<sup>39</sup> As mentioned earlier, NFHS data indicates that very few (less than 10%) women are married before turning 14.

<sup>40</sup> Restricting the sample to married adult sons also helps us account for children's widespread residence with parents until marriage in India.

either state linear trends or state-year fixed effects, or mother fixed effects does not change the result (columns 2-5). Thus, we find no evidence that reform exposure reduces co-residence of parents with sons.<sup>41</sup> This appears to be a sticky social norm, corroborating previous work on norms supporting gender inequality (Alesina, Giuliano and Nunn, 2013).

So as to examine the impact of exposure to the reform on distance of married women from their natal home, we use a similar specification as equation (7) where the dependent variable is a binary measure that takes the value 1 if the distance between a married woman's marital and natal homes is 10 kilometers or less and zero otherwise.<sup>42</sup> We choose this threshold as it captures the lowest quartile of women's marriage distances in our sample. "Exposure to reform" is a dummy variable that equals 1 if the woman was 14 or younger at the time of reform and zero otherwise. The results are presented in Table 7. Women exposed to the reform are between 3-5 percentage points more likely to marry into homes within 10 kilometers of their natal households (columns 2 and 4). We also find a significant effect in the same direction if we use 20 kilometers rather than 10 (Appendix Table A8). Thus, it appears that there was in fact some norm-shifting in terms of daughters marrying closer to home, although co-residence often coincides with the son working on the land and then inheriting it, so the latter is the more important, and the more rigid measure, of patrilocality.

To elaborate further, our results for co-residence show no erosion in the tradition of parents living with adult sons and these results alone are consistent with sex ratios being biased against girls. We find some adjustment post reform in the distance between daughters' natal and marital homes. While this may indicate that daughters want to move closer to natal property once they acquire rights over it, it may have little impact on parents' incentives if what parents need is effort (labour) on the property (often land), which is historically provided by co-resident sons. In other words, co-residence with sons may dominate parental incentives. Another explanation that would reconcile the result pertaining to married daughters living closer with the result that parents continue to be averse to giving birth to (more than one) daughter(s) is that the compliers driving these two results may be different. In other words, families who commit female foeticide are

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<sup>41</sup> We do not find any impact on co-residence of parents with adult daughters (results available on request).

<sup>42</sup> We use the kilometer distance between the household sampled and each married woman's marital home. This measure includes female household heads, whose distance from their natal home is defined as zero.

different families than those whose daughters marry closer to the natal home.

## 5. Conclusion

This paper shows that legislation that gives women equal rights to inheritance of ancestral property intensifies son preference in fertility. The evidence is stark, showing large increases in parents' proclivity to commit sex-selective abortion in order to manipulate the sex composition of their births in favour of sons. In fact, we find that parents also adjusted the sex composition of their births in other ways: the reform was associated with an increase in girl relative to boy infant mortality and an increase in the tendency for families without a son (or their desired number of sons) to continue fertility. This is corroborated by evidence on increased reported son preference (i.e. stated desired share of sons among births) post reform.

Our findings demonstrate the challenges faced by legal reform. They suggest that support for institutionalizing women's economic rights was not widespread. Pervasive support (among men) has been argued to emerge as the returns to human capital investment rise (Doepke and Tertilt 2009). While a full analysis is beyond the scope of this paper, we observe that average returns to human capital have been rising in India since the 1990s, and that women's education has converged towards that of men. However, there remain barriers to women realizing returns to education on the labour market (Field *et al.* 2016). Moreover, we provide evidence that the convention that sons provide old-age security has not changed and there is no systematically provided state pension.

In Europe and America, the legislation of economic rights for women followed the implementation of women's suffrage. In India, women and men have had equal voting rights since India gained independence from British rule in 1947, but legislation of rights for women has proceeded only slowly since then.<sup>43</sup> The early amendments to the Hindu Succession Act that we analyse in this paper were favoured by women legislators in seats reserved for lower castes, with women legislators from higher castes having no discernible impact on women-friendly laws (Clots-Figueras, 2011). It may be that consistent monitoring of implementation of legal change requires a larger share of women in

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<sup>43</sup> The Dowry Prohibition Act of 1961 was among the first pro-female legislative reforms, followed by The Prevention of Sati (widow burning) Act of 1987 and the National Commission for Women Act 1990. The federal Inheritance Rights Reform and the Protection of Women Domestic Violence Act were passed as recently as 2005, and the Child Marriage Act was passed in 2006.

government than India has (Brulé 2017). During 1980-2007, for instance, 59% of women in contrast with 66% of men turned out to vote, but only 4.4% of electoral candidates for state legislative assemblies were women, and women comprised only 5.5% of state legislatures (Bhalotra, Clots-Figueras and Iyer *forthcoming*).

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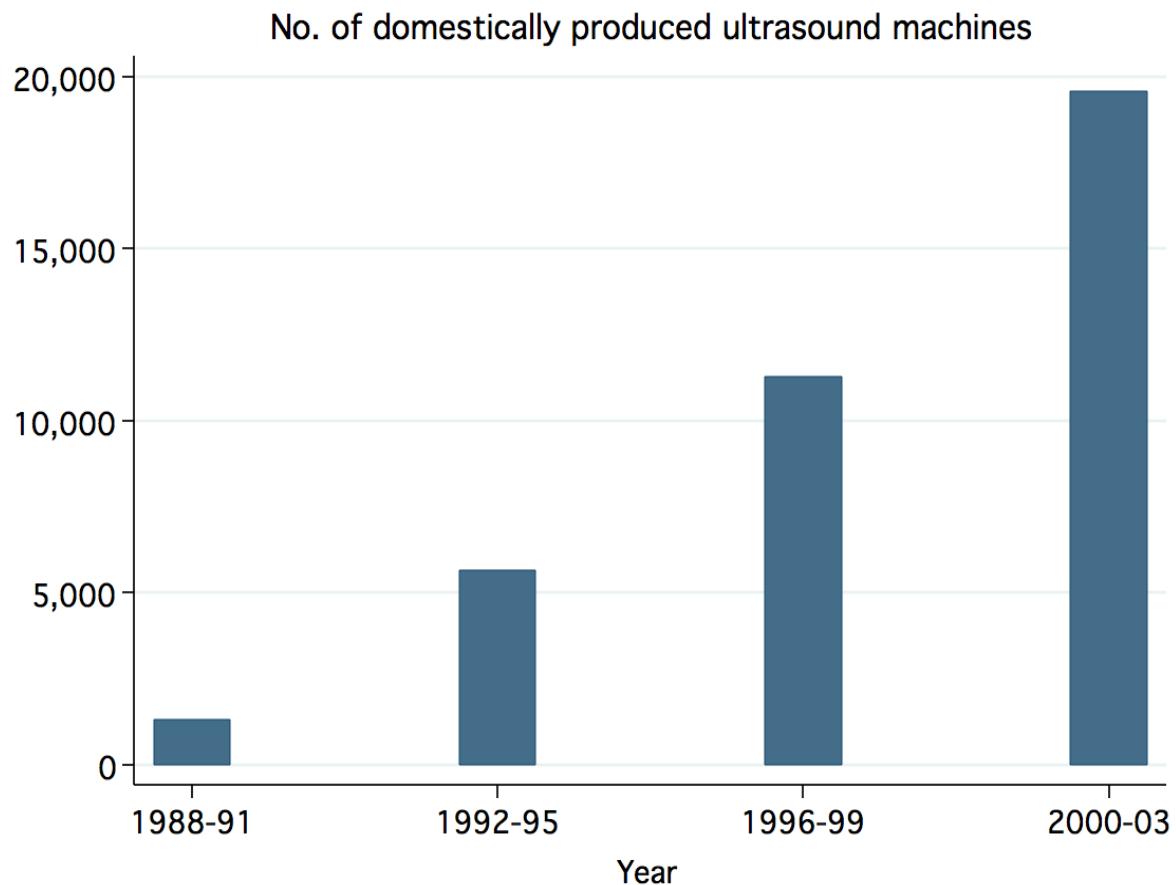
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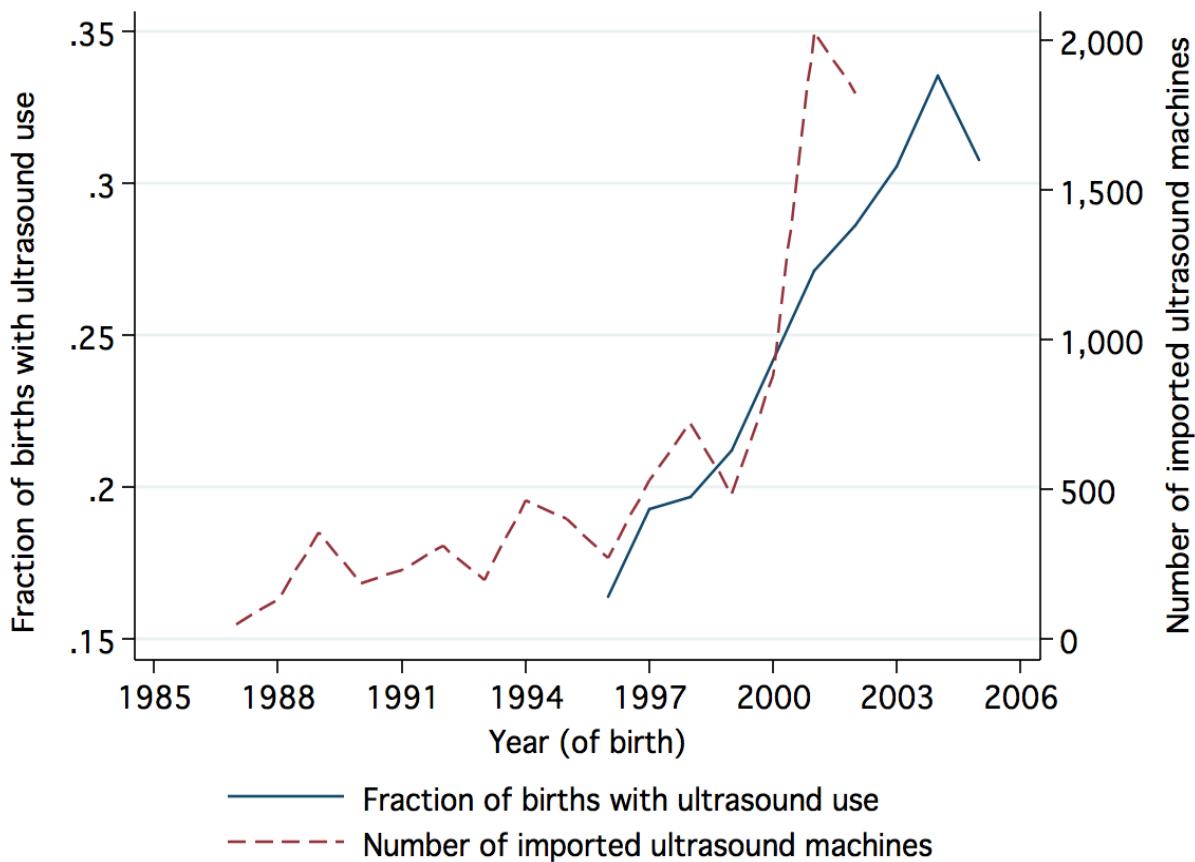


Figure 1: The Five Reforming States (in green) and rest of India (in blue)



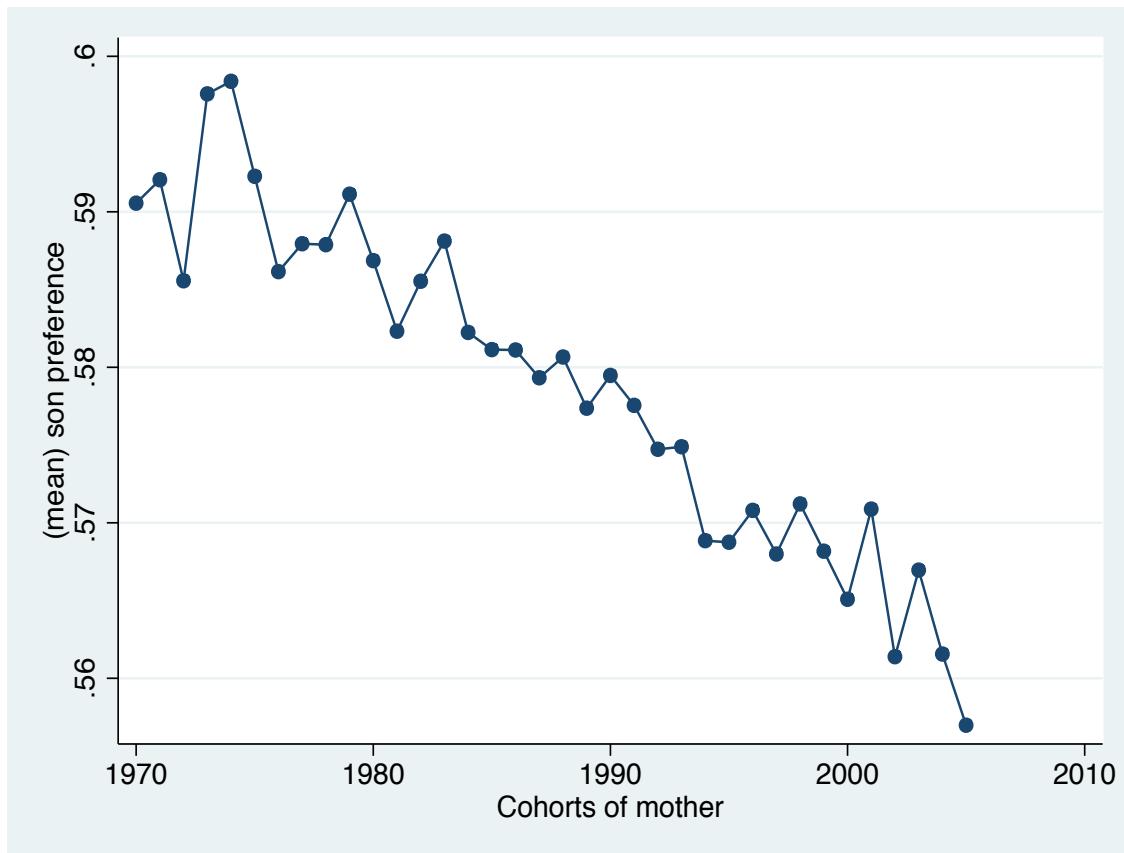
**Figure 2a: Growth in availability of ultrasound machines (domestic)**

Notes: The bars in the bottom graph plot the number of ultrasound machines produced domestically in India.  
Data source: George (2006). Drawn from Anukriti et al. (2016).



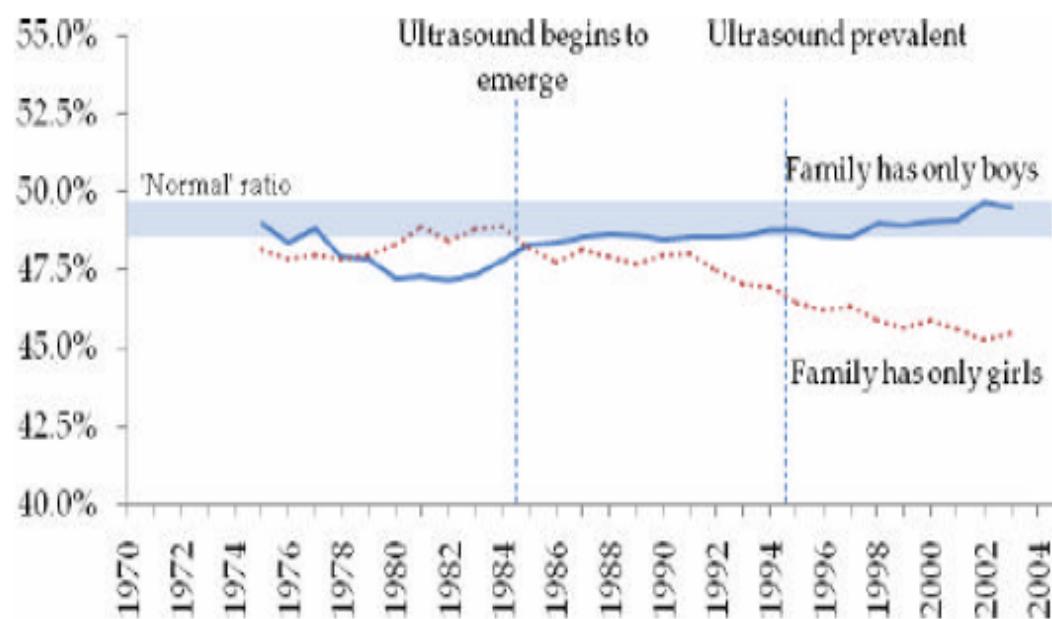
**Figure 2b: Growth in availability of ultrasound machines (imported)**

Notes: (1) The solid line in the top graph plots the fraction of births in a year for which the mother reports getting an ultrasound test at some point during the pregnancy (the denominator equals the number of births with a non-missing response on ultrasound use). The relevant question was not asked in NFHS-1 but in NFHS-2 and NFHS-3, data on ultrasound use was collected for births since January 1995 and January 2001, respectively. The years 1995 and 2000 have been dropped due to extremely small sample sizes. (2) The dashed line in the top graph plots the number of ultrasound scanners imported at the national level, the first records of which appear in the import data in 1987; indeed, there was no category coding these scanners before then. Source: Mahal et al. (2006). Drawn from Anukriti et al. (2016).



**Figure 3: Trends in Reported Son Preference over time**

Notes: NFHS data. Son preference in fertility reported by the mother. Time variation created by assuming that preferences as stated at any age of interview are shaped at age 20. See the text for a discussion.



**Figure 4: Probability of a female birth for second births (5-year moving average)**

Notes: This figure reproduces Figure 2 from Bhalotra and Cochrane (2010). It shows the trend in fraction of female births for the second births separately for families that have one boy and families with no boys. The y-axis shows the 5-year moving average of the fraction of female births. The figure shows that the sex ratio at birth in families without sons diverges from the sex ratio in families with sons after the introduction of ultrasound. It also shows that families with sons have sex ratios in the biologically normal range.

**Table 1: Sex Ratios at Birth**

	(1)	(2)	(3)	(4)	(5)
Child is female					
First-born is girl*Reform*p2	-4.26** (1.54)	-4.22** (1.54)	-4.19** (1.54)	-4.28** (1.57)	-5.60*** (1.64)
First-born is girl*Reform*p3	-3.77** (1.51)	-3.60** (1.46)	-3.59** (1.46)	-3.54** (1.44)	-4.58** (1.91)
Controls	No	Yes	Yes	Yes	No
State fixed effects	Yes	Yes	Yes	Yes	No
Child year of birth fixed effects	Yes	Yes	Yes	Yes	Yes
State linear trends	No	No	Yes	No	Yes
State-year of birth fixed effects	No	No	No	Yes	No
Mother fixed effects	No	No	No	No	Yes
N	305741	301831	301831	301831	305741

Notes: Robust standard errors, clustered at state-level, are in parentheses. \* p<0.10, \*\* p<0.05, \*\*\* p<0.01. This table uses NFHS data, is restricted to up to second births, with birth cohorts from 1972 to 2004. The dependent variable is a dummy that equals 1 if birth is female and zero otherwise, multiplied by 100. "First-born is girl" is a dummy that equals 1 if the first-born child in the family is a girl, and zero otherwise. "Reform" is a dummy that equals 1 if the child is born after the reform in the reforming state and zero if they are born before the reform or in a non-reforming state. The reforming states are Kerala (1976), Andhra Pradesh (1986), Tamil Nadu (1989), Karnataka (1994) and Maharashtra (1994). "p2" and "p3" are dummy variables that capture the early (1985-94) and later (1995-2004) phases of ultrasound availability. Control variables include household wealth, parental education, urban dummy, religion dummies, high-caste dummy and age group categories of mother. With mother fixed effects, most of these controls are not identified since they do not vary within household, hence are not included. All necessary level and double-interaction terms are included but not reported (see Appendix Table A1 instead).

**Table 2: Sex Ratios at Birth: Robustness Checks**

	(1)	(2)	(3)	(4)	(5)	(6)
	Child is female			completed fertility		
	State Women's Commission	Excluding Maha and Karna	Only Southern States	mother age>=35	actual fert.>=desired fert.	
First-born is girl*Reform*p2	-4.19** (1.54)	-4.19** (1.54)	-3.98** (1.67)		-3.11** (1.39)	-2.97* (1.60)
First-born is girl*Reform*p3	-3.59** (1.46)	-3.49** (1.44)	-2.18 (1.37)		-2.93 (2.32)	-1.86 (1.38)
State Commission	0.40 (0.50)	0.53 (0.85)				
First-born is girl*State Commission*p2		(dropped)				
First-born is girl*State Commission*p3		-0.37 (1.47)				
First-born girl*Reform*post				-6.97** (2.00)		
Controls	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Child year of birth fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
State linear trends	Yes	Yes	No	No	Yes	Yes
Female*state linear trends	No	No	No	No	No	No
State-year of birth fixed effects	No	No	Yes	Yes	No	No
N	301831	301831	268594	73298	166561	233633

Notes: Robust standard errors, clustered at state-level, are in parentheses. \* p<0.10, \*\* p<0.05, \*\*\* p<0.01. This table uses NFHS data, is restricted to up to second births, with birth cohorts from 1972 to 2004. We control for state-specific linear trends in Columns 1 and 2 since the relevant control variable (state commission) is not identified with state-year fixed effects. For details on sample and variables, see notes for Table 1. All necessary level and double-interaction terms are included but not reported.

**Table 3: Reported Son Preference**

	(1)	(2)	(3)	(4)
	Son Preference			
First-born is girl*Reform*p2	0.01** (0.00)	0.01** (0.00)	0.01** (0.00)	0.01*** (0.00)
First-born is girl*Reform*p3	0.01* (0.00)	0.01* (0.00)	0.01** (0.00)	0.01** (0.00)
Controls	No	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes
Mother year of birth fixed effects	Yes	Yes	Yes	Yes
State linear trends	No	No	Yes	No
State-year of birth fixed effects	No	No	No	Yes
N	412039	406775	406775	406775

Notes: Robust standard errors, clustered at state-level, are in parentheses. \* p<0.10, \*\* p<0.05, \*\*\* p<0.01. This table uses NFHS data and all births. The dependent variable is the ratio of desired/ideal number of sons over desired/ideal number of total children as reported by the mother. This being measured only at the time of survey, we construct a continuous measure of reported son preference based the age of the mother at the time of survey. We assume that son preferences at age 20 determine fertility (and foeticide) choices (see discussion in text). Thus, women aged 20 in 1998 define son preferences in 1998, while women aged 40 in 1998 defined son preferences in 1978 (since they were 20 years old in 1978). Since by construction, the dependent variable does not vary within mother, we do not present results from a specification with mother fixed effects for reported son preference. For details on other variables, see notes for Table 1. All necessary level and double-interaction terms are included but not reported (see Appendix Table A5 instead).

**Table 4: Female Infant Mortality**

	(1)	(2)	(3)	(4)	(5)
Child dies before turning 1 year old					
First-born is girl*Reform*p2	-1.59*** (0.51)	-1.44** (0.58)	-1.43** (0.57)	-1.37** (0.57)	-3.47*** (1.01)
First-born is girl*Reform*p3	-1.24** (0.57)	-1.15** (0.54)	-1.18** (0.52)	-1.19** (0.52)	-3.61** (1.68)
First-born is girl*Reform*p2*female	3.17*** (0.83)	3.50*** (0.84)	3.47*** (0.83)	3.39*** (0.83)	6.31*** (2.20)
First-born is girl*Reform*p3*female	2.29*** (0.74)	2.57*** (0.69)	2.59*** (0.68)	2.55*** (0.69)	6.31*** (2.15)
Controls	No	Yes	Yes	Yes	No
State fixed effects	Yes	Yes	Yes	Yes	No
Child year of birth fixed effects	Yes	Yes	Yes	Yes	Yes
State linear trends	No	No	Yes	No	Yes
State-year of birth fixed effects	No	No	No	Yes	No
Mother fixed effects	No	No	No	No	Yes
N	305741	301831	301831	301831	301831

Notes: Robust standard errors, clustered at state-level, are in parentheses. \* p<0.10, \*\* p<0.05, \*\*\* p<0.01. This table uses NFHS data, is restricted to up to second births, with birth cohorts from 1972 to 2004. The dependent variable captures whether the child died on or before their first birthday, multiplied by 100. "female" is a dummy that captures if the index child is a girl. For details on sample and variables, see notes for Table 1. All necessary level and double-interaction terms are included but not reported (see Appendix Table A6 instead).

**Table 5: Fertility**

	(1)	(2)	(3)	(4)	(5)
	Has younger sibling				
First-born is girl*Reform*p2	3.30 (3.90)	2.52 (3.79)	2.59 (3.78)	2.45 (3.84)	3.28 (4.68)
First-born is girl*Reform*p3	8.56*** (1.96)	8.10*** (2.00)	8.19*** (1.98)	7.90*** (2.07)	12.33*** (2.94)
Controls	No	Yes	Yes	Yes	No
State fixed effects	Yes	Yes	Yes	Yes	No
Child year of birth fixed effects	Yes	Yes	Yes	Yes	Yes
State linear trends	No	No	Yes	No	Yes
State-year of birth fixed effects	No	No	No	Yes	No
Mother fixed effects	No	No	No	No	Yes
N	305741	301831	301831	301831	305741

Notes: Robust standard errors, clustered at state-level, are in parentheses. \* p<0.10, \*\* p<0.05, \*\*\* p<0.01. This table uses NFHS data, is restricted to up to second births, with birth cohorts from 1972 to 2004. The dependent variable captures whether the child has younger brothers or sisters, multiplied by 100. For details on sample and variables, see notes for Table 1. All necessary level and double-interaction terms are included but not reported (see Appendix Table A7 instead).

**Table 6. Co-residence of Parents and Adult Sons**

	(1)	(2)	(3)	(4)	(5)
	Parent Resides with Adult Son				
Exposure to reform	-0.01 (0.01)	-0.01 (0.01)	-0.01 (0.01)	-0.01 (0.01)	0.15 (0.10)
Controls	Yes	Yes	Yes	Yes	No
State fixed effects	Yes	Yes	Yes	Yes	No
Year of birth fixed effects	Yes	Yes	Yes	Yes	Yes
State linear trends	No	No	Yes	No	Yes
State-year of birth fixed effects	No	No	No	Yes	No
Mother fixed effects	No	No	No	No	Yes
Married sons only	No	Yes	Yes	Yes	Yes
N	16392	13820	13820	6517	13820

Notes: Robust standard errors, clustered at state-level, are in parentheses. \* p<0.10, \*\* p<0.05, \*\*\* p<0.01. This table uses REDS data, includes all adult (aged 18 or older) sons born before 2005 in families with at least one living parent. The dependent variable is a binary indicator of whether or not a given son is residing with a parent. "Exposure to reform" is a dummy variable which equals 1 if there is a sister in the given son's generation who was 14 years or less at the time of reform in the reforming state and zero otherwise. Our measure includes two generations of sons: first, the household head and siblings, and second, the household head's children. Control variables include household size, land ownership, and education.

**Table 7: Patrilocality**

	(1)	(2)	(3)	(4)
Marital & Natal Homes<10km Apart				
Exposure to reform	0.02 (0.02)	0.03* (0.02)	0.00 (0.02)	0.05*** (0.01)
Controls	No	Yes	Yes	No
State fixed effects	Yes	Yes	Yes	No
Year of birth fixed effects	Yes	Yes	Yes	Yes
State linear trends	No	No	Yes	Yes
Mother fixed effects	No	No	No	Yes
N	43035	37283	37283	43035

Notes: Robust standard errors, clustered at state-level, are in parentheses. \* p<0.10, \*\* p<0.05, \*\*\* p<0.01. This table uses REDS data, and includes all married women born before 2005. The dependent variable is a binary indicator of whether or not there are at most 10 km between a given woman's natal and marital homes. "Exposure to reform" is a dummy that equals 1 if the given woman was 14 years or younger at the time of reform in the reforming states and zero otherwise. Control variables include household wealth, parental education, religion dummies, high-caste dummy and age group categories of the mother.

**Table 8: Test of balance in samples by first-born sex**

	first-born is boy	first-born is girl	Difference
	(1)	(2)	(3)
<b>Rural</b>	0.64 (0.48)	0.64 (0.48)	-0.00 [0.00]
<b>Hindu</b>	0.76 (0.43)	0.76 (0.42)	-0.00 [0.00]
<b>Muslim</b>	0.11 (0.31)	0.11 (0.31)	-0.00 [0.00]
<b>High caste Hindu</b>	0.38 (0.48)	0.37 (0.48)	0.00 [0.00]
<b>Parental education</b>			
Mother has secondary or higher education	0.12 (0.32)	0.12 (0.32)	0.00 [0.00]
Father has secondary or higher education	0.21 (0.41)	0.21 (0.41)	0.00 [0.00]
<b>Mother's birth cohort</b>			
1942-1960	0.03 (0.18)	0.03 (0.18)	0.00 [0.00]
1954-1979	0.88 (0.33)	0.88 (0.33)	0.00 [0.00]
1980-1987	0.09 (0.28)	0.09 (0.29)	-0.00 [0.00]
<b>Mother's age at birth</b>			
12-15	0.11 (0.31)	0.10 (0.31)	0.00 [0.00]
16-18	0.32 (0.47)	0.32 (0.46)	0.00 [0.00]
19-24	0.47 (0.50)	0.47 (0.50)	-0.00 [0.00]
25-30	0.09 (0.29)	0.09 (0.29)	-0.00 [0.00]
31-49	0.01 (0.11)	0.01 (0.11)	0.00 [0.00]
<b>Top 20% by wealth</b>	0.24 (0.43)	0.24 (0.43)	0.00 [0.00]
<b>N (1st births)</b>	87760	81968	169728

Notes: \* p<0.10, \*\* p<0.05, \*\*\* p<0.01. Standard deviations are in parentheses. Standard errors are in brackets. This table uses NFHS data.

## **Online Appendix**

**Table A1: Sex Ratios at Birth: Full Specification**

	(1)	(2)	(3)	(4)	(5)	(6)
	Child is female					
First-born is girl	0.88** (0.42)	0.94** (0.42)	0.80* (0.42)	0.80* (0.42)	0.84* (0.42)	-
First-born is girl*p2	-1.46** (0.53)	-1.50*** (0.52)	-1.42** (0.52)	-1.42** (0.52)	-1.47*** (0.52)	-3.33*** (0.90)
First-born is girl*p3	-4.13*** (0.82)	-4.19*** (0.83)	-4.08*** (0.83)	-4.09*** (0.83)	-4.19*** (0.83)	-9.17*** (1.38)
Reform	0.46** (0.22)	-0.76** (0.35)	-0.96** (0.37)	-3.11*** (0.59)	-	1.12 (0.89)
Reform*p2	0.08 (0.26)	0.69 (0.47)	0.90* (0.50)	2.65*** (0.55)	-	-2.08*** (0.74)
Reform*p3	-0.11 (0.44)	0.40 (0.48)	0.66 (0.54)	2.48*** (0.81)	-	-3.42 (2.15)
First-born is girl*Reform	4.93*** (0.42)	4.88*** (0.43)	4.73*** (0.43)	4.71*** (0.43)	4.74*** (0.42)	6.72*** (0.80)
First-born is girl*Reform*p2	-4.30*** (1.51)	-4.26** (1.54)	-4.22** (1.54)	-4.19** (1.54)	-4.28** (1.57)	-5.60*** (1.64)
First-born is girl*Reform*p3	-3.83** (1.50)	-3.77** (1.51)	-3.60** (1.46)	-3.59** (1.46)	-3.54** (1.44)	-4.58** (1.91)
p2	0.57** (0.21)					
p3	0.99*** (0.33)					
Constant	47.76*** (0.22)					
Controls	No	No	Yes	Yes	Yes	No
State fixed effects	No	Yes	Yes	Yes	Yes	No
Child year of birth fixed effec	No	Yes	Yes	Yes	Yes	Yes
State linear trends	No	No	No	Yes	No	Yes
State-year of birth fixed effec	No	No	No	No	Yes	No
Mother fixed effects	No	No	No	No	No	Yes
N	305741	305741	301831	301831	301831	305741

Notes: Robust standard errors, clustered at state-level, are in parentheses. \* p<0.10, \*\* p<0.05, \*\*\* p<0.01. This table uses NFHS data, is restricted to up to second births, with birth cohorts from 1972 to 2004. For details on sample and variables, see notes for Table 1. In column 5, the level effect of the reform as well as its interactions with the p2 (early ultrasound) and p3 (late ultrasound) dummies are no longer identified after the inclusion of state-year of birth fixed effects. In all the columns, the coefficient on *first-born is girl\*reform* is likely to capture a "Kerala effect", since Kerala was the only state to reform pre-ultrasound.

**Table A2a: Sex Ratios at Birth: Including upto Third Births**

	(1)	(2)	(3)	(4)	(5)
	Child is female				
First-born is girl*Reform*p2	-1.85*	-1.88*	-1.86*	-1.82*	-2.55*
	(1.06)	(1.07)	(1.07)	(1.07)	(1.51)
First-born is girl*Reform*p3	-1.86	-1.83	-1.84	-1.80	-0.22
	(1.40)	(1.39)	(1.38)	(1.37)	(1.68)
Controls	No	Yes	Yes	Yes	No
State fixed effects	Yes	Yes	Yes	Yes	No
Child year of birth fixed effects	Yes	Yes	Yes	Yes	Yes
State linear trends	No	No	Yes	No	Yes
State-year of birth fixed effects	No	No	No	Yes	No
Mother fixed effects	No	No	No	No	Yes
N	393518	388505	388505	388505	393518

Notes: Robust standard errors, clustered at state-level, are in parentheses. \* p<0.10, \*\* p<0.05, \*\*\* p<0.01. This table uses NFHS data, is restricted to up to third births, with birth cohorts from 1972 to 2004. For details on sample and variables, see notes for Table 1.

**Table A2b: Sex Ratios at Birth: Including upto Fourth Births**

	(1)	(2)	(3)	(4)	(5)
	Child is female				
First-born is girl*Reform*p2	-1.65*	-1.63	-1.62	-1.63	-1.51
	(0.95)	(0.98)	(0.99)	(0.99)	(1.32)
First-born is girl*Reform*p3	-1.41	-1.36	-1.37	-1.37	2.11
	(1.22)	(1.21)	(1.21)	(1.20)	(1.38)
Controls	No	Yes	Yes	Yes	No
State fixed effects	Yes	Yes	Yes	Yes	No
Child year of birth fixed effects	Yes	Yes	Yes	Yes	Yes
State linear trends	No	No	Yes	No	Yes
State-year of birth fixed effects	No	No	No	Yes	No
Mother fixed effects	No	No	No	No	Yes
N	442923	437274	437274	437274	442923

Notes: Same as above. Sample restricted up to fourth births.

**Table A2c: Sex ratio at birth: first births**

	Child is female
Reform	0.08 (0.20)
Reform*p2	0.50 (0.37)
Reform*p3	0.84 (0.53)
p2	0.46* (0.24)
p3	0.73** (0.30)
Constant	47.81*** (0.20)
N	169728

Notes: Same as above. Sample restricted first births only.

**Table A3a: Sex Ratios at Birth: Using Alternative dataset REDS 2006**

	(1)	(2)	(3)	(4)	(5)
	Child is female				
First-born is girl*Reform*post	-8.25*	-7.78*	-7.66*	-8.86*	-9.13
	(3.77)	(4.27)	(4.25)	(4.69)	(6.60)
Controls	No	Yes	Yes	Yes	No
State fixed effects	Yes	Yes	Yes	Yes	No
Child year of birth fixed effects	Yes	Yes	Yes	Yes	Yes
State linear trends	No	No	Yes	No	Yes
State-year of birth fixed effects	No	No	No	Yes	No
Mother fixed effects	No	No	No	No	Yes
N	11905	10855	10855	10855	11905

Notes: Robust standard errors, clustered at state-level, are in parentheses. \* p<0.10, \*\* p<0.05, \*\*\* p<0.01. This table uses REDS data, and is restricted up to second births. Due to smaller number of observations in REDS 2006 compared to NFHS, we combine the two ultrasound periods into a single “Post” dummy variable that captures the full ultrasound period 1985-2004. Control variables include household wealth, parental education, religion dummies, high-caste dummy and age group categories of mother. For details on the rest of variables, see notes for Table 1.

**Table A3b: Sex Ratios at Birth: Robustness Checks: Using Alternative dataset REDS 2006**

	(1)	(2)	(3)	(4)	(5)
	Child is female				
	State Women's Commission		Excluding Maha and Karna	Only Southern States	completed fertility mother age>=35
First-born is girl*Reform*post	-7.67*	-6.93	-14.28***	-12.57	-12.67**
	(4.26)	(5.14)	(3.45)	(6.73)	(4.40)
State Commission	0.38	-0.41			
	(2.31)	(2.47)			
Controls	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes
Child year of birth fixed effects	Yes	Yes	Yes	Yes	Yes
State linear trends	Yes	Yes	No	No	Yes
Female*state linear trends	No	No	No	No	No
State-year of birth fixed effects	No	No	Yes	Yes	No
N	10855	10855	9126	3759	8300

Notes: Robust standard errors, clustered at state-level, are in parentheses. \* p<0.10, \*\* p<0.05, \*\*\* p<0.01. This table uses REDS data. Due to smaller number of observations in REDS 2006 compared to NFHS, we combine the two ultrasound periods into a single “Post” dummy variable that captures the full ultrasound period 1985-2004. We control for state-specific linear trends in Columns 1 and 2 since the relevant control variable (state commission) is not identified with state-year fixed effects. For details on the sample and rest of variables variables, see notes for Tables 1 and 2. We cannot replicate the final column (6) of Table 2 as REDS does not provide information on mother's ideal number of children.

**Table A4: Proportion of Female Births: Mother Exposure to Reform**

	(1)	(2)	(3)
	Girls/total children		
Mother exposure	-0.01*** (0.00)	-0.01*** (0.00)	-0.01 (0.01)
Controls	No	Yes	Yes
State fixed effects	Yes	Yes	Yes
Mother year of birth fixed effects	Yes	Yes	Yes
State linear trends	No	No	Yes
N	490833	484501	484501

Notes: \* p<0.10, \*\* p<0.05, \*\*\* p<0.01. Robust standard errors, clustered at state-level, are in parentheses. This table uses NFHS data and all births. The dependent variable is the total number of girls born to the mother divided by the total number of children born to her (G/G+B). 'Reform' is a dummy that equals 1 if the mother is 14 years or less at the time of reform in the reforming state and zero otherwise. Control variables include household wealth, parental education, urban dummy, religion dummies and high-caste dummy.

**Table A5: Reported Son Preference: Full Specification**

	(1)	(2)	(3)	(4)
	Son Preference			
First-born is girl	-0.02*** (0.00)	-0.02*** (0.00)	-0.02*** (0.00)	-0.02*** (0.00)
First-born is girl*p2	0.00 (0.00)	-0.00 (0.00)	-0.00 (0.00)	-0.00 (0.00)
First-born is girl*p3	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)
Reform	-0.00 (0.00)	-0.00 (0.00)	-0.00 (0.00)	-
Reform*p2	-0.00 (0.00)	-0.00 (0.00)	-0.00 (0.00)	-
Reform*p3	-0.01** (0.00)	-0.01* (0.00)	-0.00 (0.00)	-
First-born is girl*Reform	-0.01 (0.00)	-0.01* (0.00)	-0.01** (0.00)	-0.01*** (0.00)
First-born is girl*Reform*p2	0.01** (0.00)	0.01** (0.00)	0.01** (0.00)	0.01*** (0.00)
First-born is girl*Reform*p3	0.01* (0.00)	0.01* (0.00)	0.01** (0.00)	0.01** (0.00)
Controls	No	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes
Mother year of birth fixed effects	Yes	Yes	Yes	Yes
State linear trends	No	No	Yes	No
State-year of birth fixed effects	No	No	No	Yes
N	412039	406775	406775	406775

Notes: Robust standard errors, clustered at state-level, are in parentheses. \* p<0.10, \*\* p<0.05, \*\*\* p<0.01. This table uses NFHS data and all births. For details on sample and variables, see notes for Table 3. In column 4, the level effect of the reform as well as its interaction with the p2 (early ultrasound) and p3 (late ultrasound) dummies are no longer identified after the inclusion of state-year of birth fixed effects.

**Table A6: Infant Mortality: Full Specification**

	(1)	(2)	(3)	(4)	(5)
	Child dies before turning 1 year old				
First-born is girl	-2.63*** (0.36)	-1.76*** (0.35)	-1.78*** (0.35)	-1.85*** (0.35)	-
First-born is girl*p2	0.95* (0.52)	0.77 (0.53)	0.79 (0.52)	0.82 (0.52)	0.82 (1.13)
First-born is girl*p3	0.31 (0.42)	-0.06 (0.45)	0.03 (0.42)	0.10 (0.42)	0.49 (1.23)
Reform	-1.63*** (0.25)	-1.45*** (0.27)	-0.31 (0.36)	-	-1.19 (0.89)
Reform*p2	0.88** (0.33)	0.93** (0.35)	0.20 (0.23)	-	0.14 (0.69)
Reform*p3	1.43** (0.67)	1.72** (0.68)	1.21* (0.68)	-	0.48 (1.71)
First-born is girl*Reform	2.70*** (0.35)	2.42*** (0.33)	2.41*** (0.33)	2.37*** (0.35)	4.85*** (0.84)
First-born is girl*Reform*p2	-1.59*** (0.51)	-1.44** (0.58)	-1.43** (0.57)	-1.37** (0.57)	-3.47*** (1.01)
First-born is girl*Reform*p3	-1.24** (0.57)	-1.15** (0.54)	-1.18** (0.52)	-1.19** (0.52)	-3.61** (1.68)
First-born is girl*Reform*p2*female	3.17*** (0.83)	3.50*** (0.84)	3.47*** (0.83)	3.39*** (0.83)	6.31*** (2.20)
First-born is girl*Reform*p3*female	2.29*** (0.74)	2.57*** (0.69)	2.59*** (0.68)	2.55*** (0.69)	6.31*** (2.15)
Reform*female	0.43** (0.20)	0.43** (0.20)	0.42** (0.20)	0.40* (0.19)	1.13** (0.49)
First-born is girl*female	2.70*** (0.51)	2.60*** (0.51)	2.61*** (0.51)	2.62*** (0.52)	4.68*** (1.13)
First-born is girl*p2*female	-1.78** (0.68)	-1.73** (0.71)	-1.73** (0.71)	-1.70** (0.71)	-1.14 (1.53)
First-born is girl*p3*female	-0.84* (0.43)	-0.84* (0.44)	-0.88** (0.42)	-0.87** (0.42)	-1.06 (1.24)
First-born is girl*Reform*female	-3.03*** (0.53)	-3.17*** (0.54)	-3.15*** (0.53)	-3.10*** (0.52)	-6.47*** (0.95)
Reform*p2*female	-0.24 (0.33)	-0.31 (0.34)	-0.30 (0.34)	-0.30 (0.34)	-0.08 (0.84)
Reform*p3*female	0.19 (0.49)	0.17 (0.47)	0.18 (0.47)	0.16 (0.46)	-0.30 (1.36)
p2*female	0.62*** (0.20)	0.60*** (0.20)	0.60*** (0.20)	0.59*** (0.20)	0.26 (0.63)
p3*female	0.47* (0.25)	0.40 (0.26)	0.40 (0.25)	0.41 (0.26)	0.22 (0.70)
female	-1.90*** (0.21)	-1.85*** (0.21)	-1.85*** (0.20)	-1.84*** (0.20)	-3.91*** (0.70)
Controls	No	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	No
Child year of birth fixed effects	Yes	Yes	Yes	Yes	Yes
State linear trends	No	No	Yes	No	Yes
State-year of birth fixed effects	No	No	No	Yes	No
Mother fixed effects	No	No	No	No	Yes
N	305741	301831	301831	301831	301831

Notes: Robust standard errors, clustered at state-level, are in parentheses. \* p<0.10, \*\* p<0.05, \*\*\* p<0.01. This table uses NFHS data, is restricted to up to second births, with birth cohorts from 1972 to 2004. The dependent variable captures whether the child died on or before their first birthday, multiplied by 100. "female" is a dummy that captures if the index child is a girl. For details on sample and variables, see notes for Table 1. In column 4, the level effect of the reform as well as its interactions with the p2 (early ultrasound) and p3 (late ultrasound) dummies are no longer identified after the inclusion of state-year of birth fixed effects.

**Table A7: Fertility: Full Specification**

	(1)	(2)	(3)	(4)	(5)
	Has younger sibling				
First-born is girl	-2.08*** (0.57)	-0.02 (0.49)	0.00 (0.49)	-0.02 (0.49)	-
First-born is girl*p2	-2.28*** (0.54)	-2.23*** (0.55)	-2.27*** (0.55)	-2.25*** (0.54)	-3.24*** (0.98)
First-born is girl*p3	-2.93*** (0.56)	-2.86*** (0.56)	-2.91*** (0.56)	-2.88*** (0.55)	-10.81*** (1.55)
Reform	7.89*** (0.75)	5.70*** (0.69)	5.47*** (1.09)	-	9.19*** (1.33)
Reform*p2	-7.66*** (0.82)	-5.75*** (0.69)	-5.38*** (0.77)	-	-6.95*** (1.76)
Reform*p3	-5.95*** (1.51)	-3.56** (1.55)	-2.88 (2.00)	-	-8.11** (3.00)
First-born is girl*Reform	-16.11*** (0.57)	-15.51*** (0.62)	-15.57*** (0.63)	-15.34*** (0.62)	-14.34*** (1.08)
First-born is girl*Reform*p2	3.30 (3.90)	2.52 (3.79)	2.59 (3.78)	2.45 (3.84)	3.28 (4.68)
First-born is girl*Reform*p3	8.56*** (1.96)	8.10*** (2.00)	8.19*** (1.98)	7.90*** (2.07)	12.33*** (2.94)
Controls	No	Yes	Yes	Yes	No
State fixed effects	Yes	Yes	Yes	Yes	Yes
Child year of birth fixed effects	Yes	Yes	Yes	Yes	Yes
State linear trends	No	No	Yes	No	Yes
State-year of birth fixed effects	No	No	No	Yes	No
Mother fixed effects	No	No	No	No	Yes
N	305741	301831	301831	301831	305741

Notes: Robust standard errors, clustered at state-level, are in parentheses. \* p<0.10, \*\* p<0.05, \*\*\* p<0.01. This table uses NFHS data, is restricted to up to second births, with birth cohorts from 1972 to 2004. The dependent variable captures whether the child has younger brothers or sisters, multiplied by 100. For details on sample and variables, see notes for Table 1. In column 4, the level effect of the reform as well as its interactions with the p2 (early ultrasound) and p3 (late ultrasound) dummies are no longer identified after the inclusion of state-year of birth fixed effects.

**Table A8: Patrilocality: Alternative distance measure**

	(1)	(2)	(3)
Marital & Natal Homes <20km Apart			
Exposure to Reform	0.03 (0.02)	-0.00 (0.02)	0.06*** (0.02)
Controls	Yes	Yes	No
State fixed effects	Yes	Yes	No
Year of birth fixed effects	Yes	Yes	Yes
State linear trends	No	Yes	Yes
Mother fixed effects	No	No	Yes
N	37283	37283	43035

Notes: This table uses REDS data. See further notes in Table 7.

**Table A9: Means of Outcome Variables**

outcome	variable used to measure	mean	sd
<i>NFHS</i>			
sex ratio	prob of female birth (%)	48.07	
son preference	ideal no. of boys/ideal no. of children	0.57	0.14
infant girl mortality	child dies before first birthday (%)	7.3	
infant boy mortality	child dies before first birthday (%)	7.7	
fertility (upto birth order 2)	child has younger siblings (%)	73.2	44.3
<i>REDS</i>			
co-residence	prob that parent resides with adult son	0.24	
marriage distance	prob that daughter's marital and natal homes<10km Apart	0.18	0.39

**Table A10: Pre-Reform Balance Test in Reforming and Non-reforming States, 1970-75**

	Non-Reform	Reform	Difference
	(1)	(2)	(1)-(2)
Child is female	47.82 (49.96)	49.91 (50.01)	-2.09 [1.73]
Son preference [ideal b/(b+g)]	0.60 (0.14)	0.56 (0.14)	0.04** (0.02)
Mother has secondary or higher education	0.02 (0.15)	0.03 (0.16)	0.01 (0.01)
Father has secondary or higher education	0.11 (0.32)	0.09 (0.29)	0.02 (0.02)
First born in family is a boy	0.15 (0.35)	0.14 (0.34)	0.01 (0.01)
Urban	0.30 (0.46)	0.32 (0.47)	-0.02 (0.04)
Hindu	0.75 (0.43)	0.78 (0.41)	-0.03 (0.07)
Richest wealth quintile	0.22 (0.42)	0.24 (0.43)	-0.02 (0.04)
High caste	0.56 (0.49)	0.63 (0.48)	-0.07 (0.05)
N	9381	2839	12220

Notes: \* p<0.10, \*\* p<0.05, \*\*\* p<0.01. Standard deviations are in parentheses. Standard errors, clustered at the state level, are in brackets. This table uses NFHS data.