The Seen and the Unseen: Impact of a Conditional Cash Transfer Program on Prenatal Sex Selection

Sayli Javadekar * Kritika Saxena †

July 1, 2021

Abstract

How is prenatal sex selective behaviour influenced by the presence of cheap fetal gender identification technology and financial incentives? We analyze a conditional cash transfer program in India called Janani Suraksha Yojna. By providing access to prenatal sex detection technology like the ultrasound scans, and simultaneously providing cash incentives to both households and community health workers for every live birth, this program altered existing trends in prenatal sex selection. Using difference-in-differences and triple difference estimators we find that the policy led to an increase in female births. This improvement comes at a cost, as we observe an increase in under-5 mortality for girls born at higher birth orders, indicating a shift in discrimination against girls from pre-natal to post-natal. Our calculations show that the net effect of the policy was that nearly 300,000 more girls survived in treatment households between

*Department of Economics, University of Bath sj2093@bath.ac.uk

†Graduate Institute of International & Development Studies: kritika.saxena@graduateinstitute.ch The authors would like to thank Giacomo de Giorgi and Lore Vandewalle for their supervision and support.This paper benefited from comments by Jean-Louis Arcand, Christelle Dumas, Eliana La Ferrara, Jaya Krishnakumar, Travis Lybbert, Tobias Mueller, Dilip Mukherjee, Michele Pellizzari, Debraj Ray, Alessandro Tarozzi, and Aleksey Tetenov. We are thankful to all the participants of the brown bag seminar at University of Geneva and the Graduate Institute of International and Development Studies, NCDE 2019, SSDev 2019, DENS Switzerland 2019, the SSES 2019 6th DENeB Berlin 2019 and 2019 ISI conference New Delhi. Biplob Biswas's Python expertise was indispensable. We are thankful to Vikram Bahure for sharing the CHIRPS data.

2006 and 2015. Finally, we find that the role played by community health workers in facilitating the program is a key driver of the decline in prenatal sex selection.

JEL Codes: J13, J16, I12, I28

Keywords: sex selection, gender, health, India, missing girls, prenatal sex detection, sex-selection, community health workers

India's long history of son preference has resulted in nearly 63 million women missing from the country's population with nearly 2 million missing across different age groups every year[1]. This phenomenon of 'missing women' has the potential for socioeconomic disruption such as a marriage market squeeze (Hesketh and Xing, 2006), an increase in crime rates (Edlund et al., 2013), social stratification based on gender (Edlund, 1999) and fewer health and educational investments in women (Jayachandran and Kuziemko, 2011). The Indian government has introduced various schemes to reduce discrimination against women, including giving parents financial incentives to have daughters. However, the effects of these policies are ambiguous (Anukriti, 2018; Sekher, 2012; Sinha and Yoong, 2009). At the same time, the literature shows that access to ultrasound technology increases the likelihood of sex selection (Almond, Li and Zhang, 2013; Anukriti, Bhalotra and Tam, 2021). This paper demonstrates how accessible ultrasound technology, along with financial incentives provided under a nationwide safe motherhood intervention program, interacts with the culture of son preference to influence the gender imbalance in India. We examine the causal relationship between the safe motherhood policy and the sex selective behaviour of Indian parents, and investigate the mechanism that expresses this relationship.

The safe motherhood program, known as Janani Suraksha Yojna (JSY) was launched by the Indian Government in 2005 with the objective of reducing maternal and neonatal mortality

[1] Estimates available in Economic Survey of India http://mofapp.nic.in:8080/economicsurvey/pdf/102-118_Chapter_07_ENGLISH_Vol_01_2017-18.pdf

by giving mothers cash payments for a live birth in a health facility. The scheme mandated that beneficiaries undergo at least three antenatal checkups, including ultrasound scans (a prenatal sex-determination technology). The scheme recruited health workers and gave them performance-based financial incentives to register pregnant women into the program and to promote institutional deliveries and access prenatal health services. To estimate the program effect on prenatal sex selection we use difference-in-difference (DID) and triple difference (DDD) estimators that exploit the variation in the timing of program implementation, the geographical location of beneficiary households and the natural experiment created by sex of the first born child.

Prior to implementation of the program, states in India were categorized as low or high performing based on the state-specific institutional delivery rates. The eligibility criteria for program benefits varied by the household's socioeconomic characteristics across this classification. Individuals belonging to a similar socioeconomic and cultural background benefited from this program if they resided in low performing states, but not if they lived in high performing states. Based on this, our treatment group consists of women living above the poverty line and not belonging to Schedule Castes or Schedule Tribes (SC/ST) from the low performing states and the control group consists of their counterparts from the high performing states. In other words, non-BPL (below poverty line), non-SC/ST women from the low and high performing states form the treatment and control group respectively.[2] To overcome the bias that could arise due to differential mother level characteristics in the comparison groups, we include mother fixed effects. We include year fixed effects that control for the unobserved factors that may be invariant for all mothers across different years. Similarly, we include state-year fixed effects to control for year-variant state-specific shocks and policies. We measure sex selective behaviour by the likelihood of female birth at every birth order for a mother. We attribute the difference in the likelihood of female births across the treatment

[2]Caste groups in India are given a *hierarchical* classification: *upper/forward* castes, other backward castes, schedule castes and schedule tribes. Non-SC/ST group includes *upper/forward* castes and other backward castes. *Upper* and *forward* caste used interchangeably

and control groups to the policy.

We create a mother-child panel using reported fertility history of mothers from the Demographic and Health Surveys (DHS) - 2015/16 and find that, the program increased the likelihood of female births by 4.8 percentage points. The triple difference estimates show that the families in the treatment group with a first born daughter see an increase in the likelihood of female births by 12.7 percentage points for birth orders 2 and above. This is a novel result considering the existing evidence on higher prevalence of prenatal sex selection amongst the *forward* caste, non-poor families and families with first born daughters (Borker et al., 2017; Anukriti, 2018; Almond, Li and Zhang, 2013; Rosenblum, 2013). To establish robustness of our results, we verify the identifying assumption that in the absence of the policy, the likelihood of female births evolve in a parallel manner in the treatment and control groups, conditional on mother fixed effects. To further validate our empirical strategy, we perform our analysis on the data collected prior to the launch of the program and find no program effects on the mothers who never received the program benefits. These falsification tests validate our identification strategy and buttress our findings on the causal effect of the policy.

Further, we explore what these results mean for the survival and well-being of these additional girls. We find that though more girls were born in treatment households, they were more likely to die before reaching 5 years of age. Surviving girls are likely to have poorer health and nutritional outcomes, increasing the gender gap in well-being among children. Though these results are not a program effect, they nevertheless provide additional insights into fertility dynamics in India, particularly among mothers in the treatment states. The net effect of the program on female births and mortality is an overall increase of 300,000 girls born in treatment states between 2006 to 2015.

So, how did JSY influence sex-selective preferences of Indian households? We hypothesize that the program worked through four possible channels: First, by mandating at least three antenatal checkups, JSY increased access to ultrasound technology among households who might have had limited or no access. Parents with a strong son preference may use these for sex selection by inducing abortion of unwanted female fetuses. Second, the financial incentives given to households for every live birth lowered the cost of bearing children. This is a strong motivator to not carry out sex selection and to give birth to the child, particularly during periods of economic shock. Third, health worker's remuneration was linked to the number of beneficiaries registered for the policy and delivering at health centers. This is an incentive for health workers to dissuade parents from performing sex selective abortions and to encourage parents to give birth to their female children. Lastly, the health workers maintained a JSY card to track every pregnancy in their neighbourhood. Fetal sex determination and sex selective abortions are illegal in India. So the registration and monitoring done by the health workers could deter the households from sex selecting.[3] JSY thus could influence the willingness of parents to bear daughters by creating an unintentional trade-off along these different dimensions of the program.

In this paper, we use data from the DHS - 2015/16 survey and HMIS data from the Ministry of Women and Child Development, Govt. of India. Using the latter, we create a dataset of all health workers at the district level from 2008 to 2015. We find that neither access to the ultrasound technology nor the financial incentives explain the increased propensity for having girls in the treatment states. Rather, the increase is explained by the presence of health workers. This result has important policy implication. It shows that that intermediary health workers can play a vital role not just in delivering health services but also in fostering desirable outcomes. Another key result is the shift of the discriminatory behaviour directed at girls from prenatal to post-natal as a response to this policy. This is a reversal of prevailing trend where access to ultrasound technology shifted discrimination against girls from post-natal to prenatal (Bhalotra and Cochrane, 2010; Bhaskar, 2007). Although this result is not

[3] Pre-Conception and Pre-Natal Diagnostic Techniques (PCPNDT) Act, 1994 is an Act of the Parliament of India enacted to stop female foeticides and arrest the declining sex ratio in India. The act banned prenatal sex determination.

encouraging, it shows that there is scope for policy to achieve desirable fertility outcomes even in presence of conflicting cultural beliefs. We discuss policy ideas in section VI.

The first contribution of the paper is to the extensive literature on missing women. Several studies and policy reports have documented the increasing shortfall of girls in Asia, mainly due to a growing incidence of sex-selective abortions (Chen, Li and Meng, 2013; Bharadwaj and Lakdawala, 2013; Almond, Li and Zhang, 2013; Valente, 2014). To the best of our knowledge, this paper is one of the first to document a decline in sex-selective abortions in India. The second contribution of this paper is to the growing literature studying the unintended consequences of public policies and programs (Ebenstein, 2010; Buchmann et al., 2019). This literature evaluates how policies can create perverse incentives and have an unintentional impact on other socioeconomic outcomes. By studying sex selection, an outcome the program did not aim to target, this paper is one of the few papers presenting unintended consequences of the JSY (Nandi and Laxminarayan, 2016; Sen et al., 2020). The final contribution of this paper is to the growing literature on the efficacy of offering financial incentives in general and to community health workers specifically, to achieve desirable maternal and child well being objectives (Cohen, Dehejia and Romanov, 2013; Björkman Nyqvist et al., 2019; Celhay et al., 2019; Brenner et al., 2011). To the best of our knowledge, we are the first to document the contribution of health workers in the reduction of prenatal sex selection in the treatment states.

Our paper is closest to Anukriti, Bhalotra and Tam (2021) who look at the impact of prenatal sex determination technologies (PNSDT) on fertility stopping behaviour in Indian parents. We use similar methodology but our paper differs in three ways. First, we study how the simultaneous granting of access to this technology and financial incentives to households and health workers affect the prenatal sex-selective behaviour. The trade off between these dimensions of the policy are an unintended consequence of the intervention designed to tackle low rates of institutional deliveries which is the main analysis of our paper. Second, our analysis focuses on the prenatal sex-selective behaviour of the non-SC/ST and non-poor groups as opposed to their work which studies all the socioeconomic groups. Although our findings are for a specific socioeconomic group, existing evidence shows that the prenatal sex-selective behaviour is more prominent for this group. Lastly, we attempt to explain how the policy mechanisms affect the prenatal sex-selective behaviour. The distinction here is in the mechanisms which explain the respective results. We find the prominent role played by the community health workers in increasing the likelihood of female births, their paper finds the the decline in desired fertility and lower birth spacing as the driving factors of the decrease in female births.

This paper is organized as follows: Section I provides a background on son preference and missing women in India. Section II introduces the empirical strategy used in the paper and section III is a discussion of the results. Section IV presents results of our robustness tests. Section V is a discussion on well-being in surviving children. Section VI discusses and tests various mechanisms that can explain the results and section VII concludes the paper with some policy recommendations.

1 Background and Data

Discrimination against young girls in India is well documented, with formal records available as far back as the First Census of British India in 1871-72. Today this discrimination is reflected in the the skewed sex ratios at birth and child sex ratios (Waterfield, 1875). The natural sex ratio at birth for humans is estimated to be between 104 - 106 boys per 100 girls (Bhaskar, 2007; Anderson and Ray, 2010), however in India, the sex ratio at birth has increased from 108 boys per 100 girls in 1991 to 111 boys per 100 girls in 2011.[4] This increasing shortfall in girls at birth is primarily due to the culture of son preference. This shortfall has also been documented in other Asian societies that are known to share India's

[4]The sex ratio at birth among many species including humans is biased towards males.

preference for boys over girls (Clark, 2000; Almond, Li and Zhang, 2013).

India has some religious and cultural norms that view sons as assets and daughters as liabilities. For instance, in Hinduism, the dominant religion in India, sons are expected to perform funeral rites when their parents die. In the absence of social security, older parents typically live with their sons, while their daughters live with their husband's family. While daughters have a legal right to an equal inheritance of the family wealth, due to sticky social norms around marriage, households prefer to keep wealth in the family by bearing a son instead of bequeathing assets to a daughter who will eventually move to another household (Bhalotra, Chakravarty and Gulesci, 2018; Roy, 2015) [5]. Paying large dowries for daughters (Borker et al., 2017) and concerns about safety also make it more costly for parents to have a daughter (Borker, 2017). Further, there is some evidence that sons benefit from economic advantages in the labor market that daughters do not receive (Rosenblum, 2013).

These norms shape households' fertility preferences and are in turn reflected in the discriminatory behaviour of households towards daughters before and after their birth. Parents adjust the gender composition of their family via prenatal discrimination and postnatal discrimination. Before ultrasound technology was available in India, parents followed a fertility rule called the *stopping rule* , having children until they reached their desired number of boys. As a result, girls were born in larger families with limited resources and therefore received lower investments (Jensen, 2012; Arnold, Choe and Roy, 1998; Das Gupta and Mari Bhat, 1997). This postnatal discrimination resulted in worse health outcomes and excess mortality amongst young girls. With the arrival of prenatal sex determination technology, parents could determine the sex of the fetus within seven weeks of pregnancy.[6] This allowed parents to abort unwanted female fetuses (Chen, Li and Meng, 2013; Bhalotra and Cochrane, 2010). Easy access to ultrasounds since the mid-1980s and an increasing preference for smaller fam-

[5] In 2005, Hindu Inheritance Act was amended to allow women to inherit wealth from their parents. Our results stay robust to this change. See Appendix C for details.

[6] PNSDT or fetal gender identification technology

ilies has led households to change their behaviour from postnatal discrimination to prenatal discrimination (Goodkind, 1996; Kashyap, 2019).

A feature observed since the nineties in India is that the sex ratio at birth is highly skewed towards males, particularly at higher birth orders (Gellatly and Petrie, 2017; Visaria, 2005; Das, 1987). Parents seldom sex select at the first birth since they prefer to have a child of either gender over the possibility of not having a child. However, in the presence of son preference, parents whose first born is a daughter are more likely to have prenatal sex selective abortions from the second birth onwards compared to parents whose first born is a son. Figure 1 plots sex ratio at birth from 2000 to 2016 at various birth orders. The horizontal line at 106 is the reference line for the natural sex ratio at birth. The solid line plots sex ratio at birth for children born at birth order one i.e. the first born children. This line closely follows the reference line indicating a balanced sex ratio for first born children. The dashed line and the dotted line plots the sex ratio at birth for children born at birth order two and birth order three or above, respectively. Both of these lines diverge increasingly from the reference line of natural sex ratio, indicating that the sex ratio at birth for children born at higher birth orders is substantially distorted towards males. This distortion at higher parity suggests that sex selection is more prevalent for pregnancies at a higher order. While sex ratio imbalance for children born at higher birth orders is linked to prenatal sex determination technology like ultrasounds, the literature also discusses other channels that influence sex selective behaviour among Indian households, like the price of gold, dowry and marriage conventions and the religious identity of the political leader (Bhalotra, Clots-Figueras and Iyer, 2018; Bhalotra, Chakravarty and Gulesci, 2018).

1.1 Janani Suraksha Yojna

In 2005, the Government of India launched Janani Suraksha Yojna, a conditional cash transfer program sponsored 100% by the national Government with a dual objective of reducing the number of maternal and neonatal deaths nationwide.[7] This scheme promoted safe motherhood by providing cash incentives to women if they delivered their children either in government hospitals or in an accredited private health institutions or at home under medical supervision.[8] A further condition to receive the full cash incentive was that the mother should undertake at least three prenatal check ups that include ultrasound and amniocentesis, technologies used to determine fetal sex.

Eligibility for the conditional cash transfer was dependent on the place of residence, income level and the caste of the household. The scheme, implemented nationwide in April 2005, classified states as low and high-performing based on the rates of institutional deliveries i.e the proportion of women giving birth at health centers as shown in Figure 2. Low-performing states were states where the institutional delivery rate was less than 25%. These included - Uttar Pradesh, Uttranchal, Bihar, Jharkhand, Madhya Pradesh, Chhattisgarh, Assam, Rajasthan, Orissa and Jammu & Kashmir. The remaining states were classified as high performing states. The objective of this program was to reduce maternal and child mortality rates by increasing the number of women giving birth safely at health facilities (Joshi and Sivaram, 2014). This makes the division of households into treatment and control groups orthogonal to unobserved factors of sex selection, an aspect we exploit for our identification.

In low-performing states, all pregnant women were program beneficiaries and the benefits were paid regardless of whether the women delivered in a government hospital or in a private accredited health center and regardless of the birth order of their children. In high performing states, only women who were classified as living below the poverty line (BPL) or belonging to a scheduled caste or scheduled tribe (SC/ST) were eligible for program benefits. The eligibility in these states was restricted to women who were 19 years of age or older and

[7] JSY is a modified graded version of the National Maternity Benefit Scheme which uniformly provided all below poverty line women throughout the country with Rs 500 per live birth up to two live births. This Scheme was suspended after JSY was launched. Since our comparison groups do not comprise of below poverty line women, our estimates are not affected by the earlier scheme.

[8] This included government health centres such as Sub Centers/Primary Health Centers/Community Health Centers/First Referral Units/general wards of district or state hospitals

were giving birth to their first or second child. The remuneration received by beneficiary women also differed across the states. Eligible women in the low-performing states received Rs. 1400 (20$) in rural areas and Rs. 1000(14$) in urban areas, per live birth. On the other hand, eligible women in high-performing states received Rs. 700(10$) in rural areas and Rs. 600 (9$) in urban areas, per live birth. The payment was made to the woman as a one year cash installment upon discharge from hospital or health center.[9] The structure of program eligibility across states gives us our comparison groups. The treatment group is the non-BPL and non-SC/ST women from the low-performing states. The control group is the non-BPL and non-SC/ST women from the high-performing states.

A novel feature of the program was the introduction of the community health worker or the accredited social health activist (ASHA) who acted as a link between the government and the beneficiaries. Adult women who have a 12th grade certificate and are from the same village as the beneficiaries were chosen as ASHAs. Engaging health workers from within the community was intended foster relationships of trust and a belief that their advice was credible. The role of the ASHA is to facilitate the program in the village by identifying pregnant women, registering them into the scheme and providing them with a JSY card for recording their pregnancy. Her duties include assisting the beneficiary to access prenatal health services, including at least three antenatal checkups, the TT injection and IFA tablets.[10] The ASHA is also supposed to counsel pregnant women to undertake safe deliveries and escort them to the health centers. She is supposed to provide information to the new mother on the benefits of breastfeeding and immunization of the infant. The role of the ASHA is to ensure that the pregnant women in her village have a safe motherhood experience by encouraging institutional deliveries and facilitating access to prenatal and post natal health services.

ASHAs were rewarded with performance based incentives based on the number of institu-

[9] Average monthly per capita consumer expenditure (average MPCE) in 2005-06 was Rs.625 in rural India and Rs.1171 in urban India at 2005-06 prices.

[10] TT injections : Tetanus Toxoid Injection, IFA tablets: iron and folic acid tablets

tional deliveries they facilitated. The ASHA package was Rs 600 for rural areas and Rs 200 for urban areas and was similar across the low and high performing states. ASHAs were paid in two installments, with the first half of the payment disbursed after the beneficiary's ANC and the second half paid upon the discharge from the birth center.

In June 2011, a few additional features were added to the program to eliminate all out of pocket expenditures related to deliveries, and treatment of sick newborns. This included unpaid normal and cesarean deliveries, free supplements and drugs for the newborn and the mother, free transport from home to the health center and free stay at all government health institutions in both rural and urban areas.

The new features further extended access to health facilities for mother and child. This late diffusion program, now called the Janani Shishu Suraksha Karyakram (Mother Child Safety Program) enhanced access to better facilities for women and child health services. Because of this revision to the program, we are able to compare early and later versions of the JSY with pre program years. The early period is from 2006 until 2010 and the later period is from 2011 until 2015, both of which are compared with pre program period 2000 - 2005.

1.2 Data and Descriptive Statistics

We use the Demographic and Health Survey data from the year 2015-2016. The DHS collects detailed information on every child born to women who were ever married and are in the age range of 15 to 49 years. This includes information on the sex of each child, the birth year, whether this child is dead or alive in the year of the survey and whether he/she is a twin or not. Using this information we are able to create a panel of mothers and children for each state of India. While the data has information on all children born between 1980 to 2016, we restrict our analysis to mothers who conceive their first child in or after the year 2000. We do this because India first imported ultrasound machines in 1985, but the technology only became widespread from 1995 when India started manufacturing the machines locally

(Bhalotra and Cochrane, 2010).

We suspect that a full sample analysis might conflate the effects of an earlier ultrasound shock in 1995. Further, we only care about transitioning mothers i.e. mothers whose fertile period coincided with the program. The majority of such mothers become fertile in or after the year 2000. We also restrict the analysis to rural areas, since the first areas to get access to the technology were likely to be urban and including them in the analysis will bias our estimates. Hence the sample we analyze is that of all the children born to rural mothers whose first child was born in or after 2000.

Table 1 records the descriptive statistics for the treatment and control group. The proportion of girls in both comparison groups is similar, however there are substantial differences in socioeconomic characteristics across the two groups. We take these differences into account in our empirical strategy.

To understand the mechanisms driving sex selective behaviour under this program, we use two additional data sources that are merged with DHS. First, we use rainfall data which is obtained from the Climate Hazards Center of the University of California, Santa Barbara. Variability in precipitation has been shown to impact vulnerability of the population, particularity those in the rural areas. This will help in elucidate the wealth/income channel. Climate Hazards Center InfraRed Precipitation with Station (CHIRPS) data has records of monthly precipitation for each district of India from 1981 to 2015.[11] To explore the health worker channel we use the data obtained from the Health Management Information System of the Ministry of Health and Family Welfare, Government of India.[12] The number of health workers receiving JSY incentives for deliveries in public and private institutions are recorded from 2008 to 2015 in each district. A drawback is that the records show ASHAs at district

[11] Climate Hazards Group InfraRed Precipitation with Station data, Funk, C.C., Peterson, P.J., Landsfeld, M.F.,Pedreros, D.H., Verdin, J.P., Rowland, J.D., Romero, B.E., Husak, G.J., Michaelsen, J.C., and Verdin, A.P.,2014, A quasi-global precipitation year series for drought monitoring: U.S. Geological Survey Data Series 832,4 p.http://pubs.usgs.gov/ds/832/

[12] https://nrhm-mis.nic.in

level, without an urban and rural distinction and only for post program years. This limits our interpretation of the effect of health workers on the program but offers evidence that can be explored in future research.

2 Empirical Strategy

The goal of this paper is to estimate the causal effect of the policy on prenatal sex-selective behaviour of the households and its consequences for child well being. We exploit variation in the timing of program implementation, program eligibility based on state of residence of the women and the random variation in the sex of the first child born to a new mother. We compare the female births to mothers in the rural low and high-performing states who belong to the non-SC/ST and non-BPL families and who became fertile in or after the year 2000. These are our treatment and control groups respectively. To estimate the impact we employ a difference-in-differences and a triple difference strategy.

2.1 Difference-in-Differences

To identify the causal effects of the policy on sex-selective behaviour, we first verify whether the classification of states into treatment and control categories is exogenous and not a response to preexisting values of female births in these states. We find no significant differences in the proportion of girls in the two groups of states prior to the implementation of the policy, as seen in Table 1 and Figure 3a. Secondly, JSY was launched with an objective to increasing institutional deliveries and not with the objective of tackling sex selective abortions. However, it could be argued that gender attitudes and other developmental dimensions are dissimilar across the treatment and control groups. States with lower rates of institutional deliveries could have worse gender attitudes or lower development than the states with higher institutional deliveries rates. These unobserved factors could influence the sex-selective behaviour, so to eliminate potential bias we add mother fixed effects to our specification. This removes all observed and unobserved differences between mothers in the treatment and control groups that could be correlated simultaneously with being qualified for program benefits and the outcome variable of female births. Conditional on mother fixed effects, we conclude that the classification into treatment and control is random.

Our first estimation is a standard DID specification. For a child born at birth order b to mother i in year t and state s, we estimate the following:

$$Girl_{bist} = \beta_0 + \beta_1 Treat_{is} \times Post_t + \delta_i + \lambda_t + \theta_b + e_{bist} \tag{1}$$

The dependent variable $Girl_{bist}$ is a dummy for a female birth at birth order b to mother i in state s in year t. $Treat_{is} \times Post_t$ is a dummy variable that specifies whether the child was born to a mother in the treatment group after 2005. λ_t, θ_b are year of birth fixed effects and birth order fixed effects respectively, which eliminate year and birth order invariant factors that could possibly confound the treatment effect.[13]. Mother fixed effects δ_i will eliminate factors that are invariant for each mother. The DID coefficient β_1 captures within-mother differences in the likelihood of female births between treatment and control groups. This includes comparisons of the children of 'transitional' mothers i.e mothers who have at least one child born before and after 2005. As the program was implemented at state level, we cluster by state to account for the serial correlation that could exist within state.

2.2 Triple difference

One of the limitation of the DID estimator is that it cannot account for any changes taking place in the treatment and control groups after the program implementation that could be correlated with the outcome. The strategy fails to identify causal effects of the program if there are other unobserved factors, for example other pro-female laws or schemes that vary by state-year and are correlated with the comparison groups and the likelihood of having

[13]We also ran regressions including state fixed effects and results are similar.

a girl. This could include state-specific child and maternal welfare schemes launched or discontinued after 2005. For example, the Maternity Benefit Scheme implemented in Tamil Nadu in 2006 that aims to provide optimal nutrition for pregnant women and compensates for wage loss during pregnancy by providing a cash transfer to poor mothers, and there was a MAMATA Maternity Scheme implemented in Orissa from 2011 until 2012. To eliminate this kind of unobserved heterogeneity, we implement a triple difference by using the sex of the first born child as an additional source of variation within the state.

The randomness of the sex of the first born child has been used extensively in the literature (Das Gupta and Mari Bhat, 1997; Rosenblum et al., 2013). There is also evidence that families whose first born child is a daughter are more likely to sex select at consequent birth orders than families whose first born is a son in the presence of son preference. In the absence of sex selection the sex ratios at birth is 104 - 106 boys per 100 girls (Ritchie and Roser, 2019). From figure 1 we see that the sex ratio at birth for parity 1 given by the solid line closely follows the natural sex ratio at birth line. The sex ratio at birth for parity 2 and 3 or above are diverging away from the natural sex ratio at birth line. This indicates sex selection from parity 2 onward and no sex selection at parity 1. Next we check whether the first-girl and first-boy families are different across observable characteristics. We plot the coefficients of the regression of an indicator of first girl on socioeconomic variables in figure 3b. The horizontal black line is to indicate that the estimated coefficient is 0. Each coefficient has a 95% confidence interval. All of the estimated coefficients are 0 i.e the first girl families and the first boy families are not different across observables. Both these arguments support the case for the natural experiments created by the sex of the first born. We compare the first girl families with the first boy families across the treatment and control before and after 2005. This allows us to identify the effect of the program across groups that have different incentives for using the features of the policy.

We run the following triple difference specification where $Treat \times Post$ is interacted with an indicator for first girl families given by $First\_Girl$. We include mother fixed effects, birth order fixed effects and year of birth fixed effects. The triple difference specification estimated is:

$$Girl_{bist} = \beta_0 + \beta_1 Treat_{is} \times Post_t \times First\_Girl_i + \beta_2 Post_t \times First\_Girl_i + Stateyear_{st} + \delta_i + \lambda_t + \theta_b + e_{bits} \quad (2)$$

Now there could be a concern that the confounders varying by state-year influence first girl and first boy families differently, and are not eliminated in the triple difference.[14] To address this concern, we include state-year fixed effects in the above specification along with mother, year and birth order fixed effects.

We also estimate the above DID and DDD specifications by classifying the post JSY years into the early and late diffusion periods. This is done for two reasons. First as additional features were added to JSY in 2011, we can see how the impact changed over the two diffusion periods. Second, we have information on the anthropometric outcomes for children born in the late diffusion period. By classifying the effects into diffusion periods we can tie the effect of the program on the sex ratio at birth for this cohort to their average anthropomentic welfare outcomes.

[14] For example the inheritance law. The Hindu Succession Act 1956 was amended in 2005 and applied across all states such that women could inherit an equal share of the family wealth. If parents with a first girl now decide to not sex select because they want their daughters to claim family wealth, then it would be the change in the inheritance law and not the implementation of JSY that would drive our results. To overcome this concern, we include a covariate to indicate the change of inheritance law in our specification. Table 12 shows the result for this regression. The coefficient on inheritance law is not statistically significant which means that the change in this law is not a possible confounder. As a matter of fact, the inclusion of state-year fixed effects will eliminate all the heterogeneity due to unobservables varying across states and over years, however inclusion of the dummy for inheritance law only makes our case for causality stronger.

3 Results

Table 2 presents the results for the DID estimation. In the first column the post-program years 2006 to 2015 are compared with pre-program years 2000 to 2005. In the second column the post-program years are divided into a late diffusion periods (2011-2015) and an early diffusion period (2006-2010) and compared to the reference pre-program years. The key variables of interest are (i) $Treat_{is} \times Post$, (ii) $Treat_{is} \times Post_I$ and (iii) $Treat_{is} \times Post_{II}$.

We see that the likelihood of a female birth increased by 4.8 percentage points in the treatment group. This translates to nearly a 10% increase in the number of girls born the mothers in the Treat group. When we look at the early and late diffusion periods of the policy, we see that in the early diffusion period this likelihood increases by 4 percentage points while in the later period it increases by 8.6 percentage points. This result is interesting as it shows a reduction in sex selective behaviour among the groups that have been known in literature to sex select i.e. non-SC/ST and non-BPL groups.

The key coefficients of our interest are the triple difference estimators. Similar to the DID specification, we first look at the post-policy period from 2006-2015 and then we differentiate between the early and late diffusion period in columns 3 and 4 of Table 2. We see that the program led to an increase in the likelihood of female births from birth order 2 onward for families with a first-born female child in the treatment group by 12.6 percentage points. There was an increase of almost 18.3 percentage points in the later diffusion period and 11.6 percentage points in the early diffusion period. We add state-year fixed effects and state year trends to our specification. After including state-year fixed effects this estimate reduces to 10.5 percentage points with a 9.7 and 15.2 percentage points increase in the likelihood of female births in the earlier and later diffusion periods. This is a more conservative specification as it controls for additional heterogeneity. This suggests that for the families with first born daughters in the treatment group, the increase in the number of girls after 2005 was nearly 23%, compared to families with a first born boy. These results suggest that an unintentional impact of the program is the reduction in sex selective abortions and an increase in probability of girls being born, in families eligible for treatment. We also see that most of the positive result is driven by the larger impacts in the later diffusion periods.

4 Robustness Tests

4.1 Identification Assumptions

A key assumption of a DID estimation is that in the absence of the program the outcome variable in the treatment and control groups has parallel trends i.e. the outcome variable would have evolved in the same way for both the groups. For our analysis, this implies that the probability of having a girl at next birth should not be significantly different across mothers in the treatment and control in the pre program years. To test this we run a specification where the effect of the program is allowed to vary by year as it would in an event study analysis. For us to be confident that the program had a causal impact on the sex-selective behaviour of a mother, we should not observe any significant differences in the probability of having a girl in the comparison groups prior to the program. Significant differences, if any, should only occur after the program if our identification strategy is identifying the program effect. To check this, we estimate the following specification for a DID and a DDD:

$$Girl_{bist} = \beta_0 + \sum_{j=2000}^{2015} \beta_j Treat_{is} \times Year_j + \delta_i + \lambda_t + \theta_b + e_{bist} \tag{3}$$

$$Girl_{bist} = \beta_0 + \sum_{j=2000}^{2015} \beta_j Treat_{is} \times Year_j \times First\_Girl + Stateyear_{st} + \delta_i + \lambda_t + \theta_b + e_{bist} \tag{4}$$

Figure 4a shows that the likelihood of a girl being born to a mother in the treatment or control groups is not significantly different for years prior to 2005. Similarly Figure 4b shows that the likelihood of giving birth to a girl is not significantly different for first girl families between the treatment and control. The differences in both the figures only becomes significant after the year 2009. The joint test of significance of the lead years of the program renders a pvalue of 0.3 and 0.163, implying that prior to the program the girl births evolved in a similar way and our empirical strategy identifies the differences between sex of children resulting from the program.

Another factor that could bias our results is if households anticipated the implementation of JSY before 2005. We would then be conflating our estimate with the households' expectations. If this were the case then households in Treat should have changed their fertility behaviour prior to 2005 and we should see a decrease in the female births. However if households in the treatment group did not change their behaviour prior to 2005 differently than households in the control group i.e the probability of female births was similar in both groups prior to 2005, we can fairly say that households did not anticipate the program and year of implementation is exogeneous. From figures 4, we confirm that the difference in female births prior to 2005 was not significant, indicating that households did not anticipate the program and change fertility behaviour.

4.2 Falsification tests

If our empirical strategy identifies the causal impact of the program on the fertility decisions of mothers, then we should not be able to see any effect on mothers who never received the program. Our first falsification test is to individually assume each year from 1990 to 2004, i.e. years prior to 2005, to be the program year. Assuming each of the years as the year when JSY was implemented, we check the impact of the program on the likelihood of a girl birth across treatment and control mothers. Figure 5 plots the coefficient for each year and we can see that the probability of girl birth across treatment and the control groups is not significantly different for any of the years except 1996 and 1997. The reason we may see some significant difference in these two years could be due to the structural break in 1995, when the ultrasound technology became widely available in India (Bhalotra and Cochrane, 2010). However, the effect of this structural break does not last long and dissipates after

1997. The coefficients for the remaining years are not significant and we the differences in the outcome only appear after 2005 i.e after JSY was implemented, implying that what we are capturing is the causal effect of JSY.

The second falsification test is to run our triple DID specification on DHS-2005/06. Since this survey was completed by 2005-06, women interviewed in this sample never received the program. The idea is similar to the test above. We should not find any effect of the program on women who never received the program. Here we assume 1995 as the year JSY was implemented and compare children born up to 10 years after 1995 with children born up to 5 years prior to 1995. Our sample consists of mothers who make their fertility decisions from 1990 onwards. Since we assume 1995 to be the year that the program was rolled out we compare children born between 1996 and 2000 (our assumed early diffusion period) and 2001 to 2005 (our late diffusion period) with those born between 1990 to 1995. One reason for doing this is that if there are any reporting biases in fertility for children born more than 10 years prior to the survey year, then these biases should be the same in the any DHS sample. Hence, if our main results are driven by reporting bias then we will also see significant differences in the outcome in our DHS-III estimation results.

We estimate the following specification for DHS-III:

$$Girl_{bits} = \beta_0 + \beta_1 Treat_{is} \times Post_{1996\_00,t} \times First\_Girl + \beta_2 Treat_{is} \times Post_{2001\_05,t} \times First\_Girl + Stateyear_{st} + \delta_i + \lambda_t + \theta_b + e_{bits} \quad (5)$$

Table 3 shows the results of our falsification test on mothers whose fertility decisions commenced in 1990.[15] In both columns we see that the likelihood of giving birth to a girl is not significantly different for families whose first child was a girl across the treatment and the

[15] An additional falsification test assuming year 2000 to be the treatment year for the DHS III sample is shown in appendix.

control. Lack of significance will indicate that our empirical strategy is capturing only the program effect.

Both the falsification tests support our claim of causal identification of the program effect on the likelihood of girl births with the empirical strategy we employ.

5 Discussion and Additional Evidence

The previous section showed the causal impact of JSY on sex selective abortions in India. The program caused an increase in number of girls born to families eligible to receive JSY benefits, indicating that the mechanism of access to prenatal sex determination technologies was not dominant. Previous work has shown that in societies with a preference for male children, girls suffer from lower welfare in families that follow the stopping rule and have more girls than they desire. This discrimination is starker for girls at a higher birth orders. In this section we therefore test the hypothesis that girls born in families with son preference will be worse off.

5.1 Impact on infant mortality

We look at the under-5 mortality of children born to women in our sample. Biologically, mortality is higher among boys than girls between the age of 0 to 1 (Kraemer, 2000) so if we observe higher mortality for girls than boys it would indicate that girls are being neglected.

Using our difference in differences estimator, we test whether the the program increased child mortality for girls. We estimate the model:

$$Dead_{ibt} = \beta_0 + \beta_1 Treat_i \times Post_t \times Girl_i + \beta_2 Treat_i + \beta_3 Post_t + Stateyear_{st} + \delta_i + \lambda_t + \theta_b + e_{ibt} \tag{6}$$

The results in table 4 and 5 show whether there are disproportionately more girls amongst infants who died in their first year or before reaching five years of age. For each of these samples, the first two columns show results for all infants in rural India irrespective of their birth order. Columns 3 and 4 show results for all infants who were born at birth order greater than 1. The last two columns show results for all infants born at birth order greater than 2. We do this distinction by birth order because girls at a higher birth order tend to die more than boys, due to neglect and discrimination.

We find that for both age groups, the probability that the dead child is a girl is positive at all birth orders. For girls born at parity higher than 1, the likelihood of a girl dying is 2 percentage points higher in the treatment group. This is more prominent in the earlier diffusion period and for girls born at parity greater than 2. The likelihood of a girl dying before reaching the age of 5, is nearly 6 percentage points more in the treatment group after the program. An interesting observation is that the significant difference in mortality between girls and boys disappears when we look at the late diffusion period. This could be due to the additional feature of providing nutritional supplements to infants that was added to the program in 2011.

5.2 Well-being in surviving children

This paper has shown that the JSY program unintentionally caused more girls to be born in treated families but at the same time increased their probability of dying before they reached 5 years of age. It appears that the program substituted pre-natal gender discrimination with post natal discrimination among families eligible for the program. While more girls are being born in treated groups, infant mortality is also high for girls in these families, suggesting that unwanted girls are neglected. Work by Anukriti, Bhalotra and Tam (2021) shows that access to PNSD technologies leads families to not give birth to unwanted girls, such that as a result post natal gender gaps between wanted girls and boys in a family disappear. However, in our case, the program induced families to defer the discrimination against girls until after they were born. In the previous section we show some evidence that the additional girls being born were unwanted and not cared for, which led to a significantly higher probability of these girls dying before the age of 5.

Given the discrimination against girls, it becomes important to assess the well being of girl children. Child anthopometric indicators are are derived from physical body measurements, such as height or weight (in relation to age and sex) and can be used to assess child nutrition. Weight and height based on age and sex do not indicate malnutrition directly, as they are affected by many factors other than nutrient intake, in particular genetic variation. However, even in the presence of such natural variation, physical measurements do signal the adequacy of diet and growth, in particular in infants and children. This is done by looking at the distribution of an indicator for a "healthy" reference group and identifying "extreme" or "abnormal" departures from this distribution (O'donnell et al., 2008). The new reference population recommended by WHO is based on random samples reflecting ethnic diversity among the US population of mothers who follow prescribed health behaviours eg. breastfeeding, no smoking etc.

The most common way of using these measures is to convert them in to z-scores. The two most commonly used indicators for assessing child level nutrition are (i) Height-for-age z-score (HAZ); and (ii) Weight-for-age z-score (WAZ). WAZ is used to monitor growth and change in malnutrition over a year, HAZ on the other hand reflects cumulative linear growth and indicates past inadequate nutrition or chronic illness. To compute these z-scores, we follow the latest process prescribed by WHO and also used by Jayachandran and Pande (2017). The results in this section further explore the heterogeneity in child anthropometric outcomes for all surviving children in our sample that are born on or after 2010. In other words, this section answers the question: "for the families eligible for the program, are the child level outcomes different for children on the lower end of the distribution than those with average outcomes?"

DHS-2015/16 collects information on these child level anthropometric indicators for all children aged 0 to 5 years who agree to be measured and are present at the year of the survey. Our sample consists of 237,508 children from the rural population. This enables us to create two measures for assessing the well-being of the surviving children in our sample. There are, some drawbacks: first, in our sample many mothers have no more than one child below the age of 5 years, so we can no longer use mother fixed effects in our regression. Second, since the children in the 0 to 5 age range are all born in the late diffusion phase, we do not have a counter-factual. We can only compare the outcomes between our treatment and control groups and hence these results should not be interpreted as the causal effect of the JSY program. This analysis is only meant to provide suggestive evidence for the possible child level outcomes that result from changes in fertility decisions caused by the program. The specification estimated is:

$$Ybits = \beta_0 + \beta_1 Girl_{bi} \times Treat_{is} + \beta_2 Girl_{bit} + X_{its} + \delta_i + \lambda_t + \theta_b + e_{bits} \tag{7}$$

Where the dependent variable is either of the two z-scores for child at birth order b born to mother i in year t in state s. $Girl_{bit}$ is the dummy variable which equals one if the child is a girl. $Treat_{is}$ is the same variable as before that captures whether the child is from a family eligible for treatment or if he/she is from the control group family. Lastly, X_{its} is a vector of mother and household level controls. The estimation uses bootstrapped standard errors since we are unable to control for mother level heterogeneity. The key variable of interest is Girl × Treat that compares the difference in height-for-age outcomes for girls in the treatment group with those in the control group. We find that, in table 11 while the height-for-age for girls in the treatment group is lower than those in the control group at all percentiles, the difference is only statistically significant for girls in twenty fifth and seventy fifth percentiles. So girls at twenty fifth percentile from treatment group have height-for-age 0.087 standard deviation points lower than those in the control group. Similarly, girls at seventy fifth percentile in the treatment group have height-for-age 0.05 standard deviation points lower than those in the control group.

Similar to Table 11, Table 10 shows the results of fixed effects quantile regressions for weight for age for all surviving children aged 0 to 5 years in our sample. Again, the key variable of interest is Girl × Treat that measures the difference in weight for age outcomes for girls born in the treatment group with those born in the control group. We can see that for most of the percentiles, weight for age for girls in the treatment group is lower than those in the control group versus boys in these groups, except for in the ninetieth percentile . These differences are statistically significant and increase in magnitude with each percentile.

These results, indicate that gender gaps in well-being continue to exist among the surviving children, with girls having poorer health outcomes than boys their age in the sample, with the effect being more detrimental for girls in the lower end of the distribution.

6 Mechanisms

6.1 Ultrasound Access Channel

According to the literature, one of the main channels that impacts households' sex-selective fertility decisions is access to pre-natal sex determination technologies like ultrasounds. All program beneficiaries were expected to undergo three ante-natal checkups that included ultrasound scans. Although discovering the gender of the fetus was not the purpose of the scans, parents might use this information and abort unwanted female fetuses. While we cannot observe who uses the technology to determine sex of the fetus and who uses it to satisfy the program condition, we can hypothesize that if more people were using this aspect of the program to sex select, we should see this channel to lead to on average a significantly lower probability of girls being born on average in the treatment group.

Using the DHS- 2015/16 data, we have information on which mothers report having used ultrasound technology. Column 2 in table 6 shows the results using this information.

$Reported\_Ultrasound_{bit}$ is a dummy variable that takes the value 1 if, for a child born at birth order b to mother 1 at year t, the mother received an used ultrasound, and 0 otherwise. One thing to note here is that the reported use of ultrasound technology was only asked for the births in the last five years, so we only have information about ultrasound use for births on or after 2010. Therefore, we cannot compare ultrasound usage before and after the program. Results in column 2 show that there was no significant difference in likelihood of a girl birth as a result of using ultrasound between the treatment and control groups. This result is mostly descriptive, as reported values are are prone to measurement errors and may be biased.

We therefore compute an indicator of the likelihood of ultrasound use by a mother based on use by her neighbours (excluding her own use).[16] We do this because there could be large reporting errors, particularly for mothers who use the technology to sex select and who may choose not to report. With assumption that not all mothers in the neighbourhood will be sex selecting (since some will conceive boys), using their reported usage of this technology we can provide a likelihood of use for all eligible mothers in the neighbourhood. Using this indicator instead of reported values does not completely absolve us of bias, but it provides a better understanding of how ultrasound access might be impacting decisions. Looking at the sample of mothers who gave births in the last five years, so from 2010 onward, this indicator is constructed as:

$$Likelihood_{cip}^{Ultrasound} = 1 - \left(\frac{(\sum_{c=1}^{C} B_{cip}^{U}) - B_{cip}^{U}}{\sum_{c=1}^{C} B_{cip}} \right) \tag{8}$$

Term B_{cip}^{U} indicates whether for birth of child c to mother i in PSU p ultrasound (U) had ever been used. The numerator captures use of ultrasound in the neighbourhood, excluding own mother's use and $\sum_{c=1}^{C} B_{cp}$ captures all the births that happen in a PSU with or without ultrasound. Using this indicator, we are able to generate a likelihood estimate for all eligible

[16]We consider all eligible women surveyed within a primary sampling unit (PSU) as neighbours.

women in the sample irrespective of whether they reported having an ultrasound. Column 1 of Table 6 shows the likelihood of use for treatment and control populations. We can see that there is no significant differences in sex of the children born in these two groups as a result of ultrasound use.

These results show that the use of ultrasound does not explain the differential probabilities of having a girl at every birth order between treatment and control groups, and therefore we can conclude that by providing access to the ultrasound technology, the program did not induce eligible households to sex select.

6.2 Cash Transfer Channel

Wealth Effect

The program provided women with cash benefits for every live birth delivered at a public or private health center. This one year payment reduced the cost of child bearing. The cash transfer was a substantial amount of almost three years of the monthly consumption expenditure of rural families in 2005 and almost 60% of a woman's average monthly rural wage. This cash transfer would be more valuable to parents at the lower end of the wealth distribution among the non-BPL group. Using the information on wealth index for each household available in DHS IV, we examine whether parents belonging to different wealth categories have differential probabilities of having a girl. A significant difference here would indicate that the financial benefit of the cash transfer induced Treat households to have more girls and therefore, not sex select.

In Table 7, we see the results of an interaction of wealth quintiles with the indicator of being in the treatment group and post program years. The results show us that the likelihood of a girl birth at subsequent birth orders does not differ by wealth across the treatment and control groups post 2005. We can therefore conclude that the program did not lead to parents bearing girls for the cash incentive.

Income Effect

The JSY cash incentive could also have been used to smooth consumption if the parents faced an income shock, especially when abortion is still an option. In the literature we see that in the event of weather shocks, households smooth consumption in various ways such as reduced health and human capital investments in children, increased dowry deaths among women and marrying daughters to distant households (Rose, 1999; Sekhri and Storeygard, 2014). Here we want to see if in response to a weather shock and given the availability of a cash transfer under the program, would parents be more likely to have a girl to smooth consumption.

To test this channel we use rainfall shocks that vary across districts and years. We use is Climate Hazards Group InfraRed Precipitation with Station data (CHIRPS) monthly rainfall data for the period 2000 to 2015.[17] As the main agricultural season in India is the monsoons (July - September) and the majority of Indian agriculture depends on rainfall during these months, we construct rainfall shocks for each year as one (two or three) standard deviations below the long run mean. For children born after the month of July in a given year, we lag the rainfall shock faced by parents by one year and for children born before the month of June we lag the rainfall shock by two years.

In Table 8, we record the results of shock interacted with Treat and post indicator. In Column 1 (2 and 3), we say parents faced a rainfall shock if they were residents in district where recorded rainfall in for the given year was 1 (2 and 3) standard deviations below and above the long run mean. The regressions control for mother, year and birth order fixed effects and we see that the rain shock has no effect on the likelihood of girl births. Parents most likely did not use the program to smooth consumption in the case of an income shock.

[17]Climate Hazards Group InfraRed Precipitation with Station data, Funk, C.C., Peterson, P.J., Landsfeld, M.F., Pedreros, D.H., Verdin, J.P., Rowland, J.D., Romero, B.E., Husak, G.J., Michaelsen, J.C., and Verdin, A.P., 2014, A quasi-global precipitation year series for drought monitoring: U.S. Geological Survey Data Series 832, 4 p. http://pubs.usgs.gov/ds/832/

6.3 Health Workers Channel

The last channel we test is the community health workers (ASHA). This channel could have operated in two ways to affect the probability of giving birth to a girl. First, the health workers received financial incentives for assisting women in the program throughout their pregnancies. Their typical duties involved maintaining a record of all pregnancies for each beneficiary, preparing the JSY beneficiary card, assisting women with the antenatal checkups and deliveries at health institutions and delivering postnatal care. Half of the incentive was paid after assisting beneficiaries with antenatal checkups and the other half after a beneficiary's delivery in a health care facility. This gave them an incentive to discourage the women in their care from having abortions. Second, maintaining a record of pregnancies is a further deterrent to sex-selective abortions, as these are prohibited by law. Given how close these two factors are, we are unable to say whether the health worker effect is due to the financial incentives or to the record of pregnancies they maintain. Hence we combine both of these factors into the health worker channel.

To test for this channel we use data on the number of ASHA workers who received JSY incentive for public and private deliveries per district every year since 2008, which is provided by the Government of India's National Rural Health Mission (NRHM). This gives us variation in exposure to ASHA workers over years and by districts which helps us in estimating their effect on births of additional girls in the treatment groups. In the table 9, we have the regression output of the effect going through the number of health workers. Health workers receive JSY benefits upon the delivery of the beneficiary in public and limited private health institutions. Since the number of JSY-accredited private health centers will be lower than public health centers, we run regressions separately for health workers receiving benefits for public and private hospital deliveries. In column 1 (2), we interact treatment variable with the number of health workers receiving incentives for deliveries in public (private) institutions. The number of health workers is scaled by per 10000 women in the district.

The result shows that an increase in the number ASHA workers increases the probability of having a girl among treatment group families by 1.5 percentage points. This increase however is associated only to the ASHA workers who received incentive for the beneficiary's delivery in public health center and not the private health center. This result clearly shows that the unintended effect of the program on improving sex ratios at birth is mostly driven through the role of ASHA workers.

6.4 Net Effect on Missing Women

This paper has so far shown that the JSY led to an increase in the number of girls being born but at the same time increased mortality for girls under the age of 5. To assess the outcome of this result on demographics we use our estimates from DID and mortality results combined with methodology similar to that used by Anderson and Ray (2010) and Anukriti, Bhalotra and Tam (2021). We first compute an estimate of change in the likelihood of birth and death for girls between 0-4 years for each year in our analysis. We then compare our observed estimates with reference estimates and multiply it with the starting population of girls in this age group from Treat (excluding population of SC and ST) as shown below:[18]

$$\text{Excess Births} = (Births_{Estimate} - Births_{Reference})$$

$$\text{Excess Deaths} = (Deaths_{Estimate} - Deaths_{Reference})$$

$$\text{Missing Women} = [\text{Excess Births} - \text{Excess Deaths}] \times Population_{0-4years} \tag{9}$$

Our estimates show that in the Treat after 2005, the program resulted in on average 621,470 additional births of girls, while in the same year average excess mortality in girls ages 0-4 years was 1,046,295. This results in the net effect of 424,825 missing women in the 0-4 years

[18] We use the natural sex ratio of 106 boys per 100 girls as a reference for calculating excess births in our sample. To calculate excess deaths in girls we use the ratio of death rates for girls and boys (0-4 years) in all countries of Europe and North America in 2015. The starting population is taken from the census of 2011 because the census 2001 does not contain information on caste for different age groups.

age group. When we compare this estimate of missing women to that in Treat in pre-program years, we find that prior to the program there were 724,997 missing women in the 0-4 years age group. This shows that while there are 424,825 missing women in our treatment sample, the program contributed to an increase of nearly 300,000 women.

This calculation of the net effect of the program on missing women is particularly important for policy, as it highlights the magnitude of the improvement in the gender balance that can be achieved in a high son-preference society when the right incentives are provided to community health workers. As can be seen from figure 6, most of the improvement in missing women comes additional births of girls due to the program[19].

7 Conclusion and Policy Recommendations

This paper examined the impact JSY conditional cash transfer program had on fertility decisions of mothers in rural India. More specifically, it provides causal evidence of the impact of the JSY on sex-selective behaviour among Indian households. Results show that, contrary to previous work on sex selection, this program led to an increase in the probability of having a girl at each birth order for mothers eligible for program. The magnitude is especially larger in families who according to the literature have a greater incentive to sex select i.e those whose first child is a daughter. While overall in the country there is an increase in the prevalence of sex selective abortions, JSY managed to reduce this practice amongst families who qualified for the program.

Results also show that while there were more girls being born to families in LPS, these girls are also more likely to die before reaching the age of 5 years. Among the surviving children we find that girls on average have lower nutritional status than boys their age and this gender gap is highest for children on the lower end of the distribution. These findings indicate

[19] We see two limitations in this rough calculation. First, we compare a longer post program period to a shorter pre program period. Second, we do not take into account the change in birth and infant mortality rate over the study period.

that though there are improvements in birth outcomes for girls as a result of the program, discrimination against them continues and shifts from prenatal to postnatal discrimination.

Our results show that in the age group 0-4, 424,825 women were missing from the population. However this is an improvement of nearly 300,000 women compared to 724,997 missing women in the same age group a decade prior to the program. While there still is a very large number of missing girls in the country, the policy contributed to reducing this number. The channel that leads to this result is the one driven by community health workers (ASHA) that were appointed as part of the program to assist pregnancies in their neighbourhood. Since these workers record each pregnancy for beneficiaries of the program and get financial incentives for every live birth of beneficiaries at health institution, they act as deterrents for couples to selectively aborting their fetuses. This result supports the emerging evidence on the role that health workers play in efficient public good distribution and in supporting health programs.

The effectiveness of community health workers in reducing the practice of prenatal sex-selective abortions either due to parental fear of being reported if they undergo a sex selective abortion or ASHA's pressure on parents to not abort the child as her payment is conditional on a beneficiary's delivery in a hospital. This is an important piece of evidence in a country that has been unsuccessfully trying to reduce female foeticide through laws against sex selective abortions or financial incentives to bear girls. However our results should be taken with a pinch of salt as we do not claim that the health workers reduced the son preference in India. It merely was substituted by postnatal excess girl mortality.

References

Almond, Douglas, Hongbin Li, and Shuang Zhang. 2013. "Land reform and sex selection in China." National Bureau of Economic Research.

Anderson, Siwan, and Debraj Ray. 2010. "Missing women: age and disease." *The Review of Economic Studies*, 77(4): 1262–1300.

Anukriti, S. 2018. "Financial incentives and the fertility-sex ratio trade-off." *American Economic Journal: Applied Economics*, 10(2): 27–57.

Anukriti, S, Sonia Bhalotra, and Eddy H. F. Tam. 2021. "On the Quantity and Quality of Girls : Fertility, Parental Investments, and Mortality."

Arnold, Fred, Minja Kim Choe, and Tarun K Roy. 1998. "Son preference, the family-building process and child mortality in India." *Population studies*, 52(3): 301–315.

Bhalotra, Sonia, Abhishek Chakravarty, and Selim Gulesci. 2018. "The price of gold: Dowry and death in India."

Bhalotra, Sonia R, and Tom Cochrane. 2010. "Where have all the young girls gone? Identification of sex selection in India."

Bhalotra, Sonia R, Irma Clots-Figueras, and Lakshmi Iyer. 2018. "Religion and Abortion: The Role of Politician Identity."

Bharadwaj, Prashant, and Leah K Lakdawala. 2013. "Discrimination begins in the womb: evidence of sex-selective prenatal investments." *Journal of Human Resources*, 48(1): 71–113.

Bhaskar, V. 2007. "Parental Choice and Gender Balance." *University College London.*

Björkman Nyqvist, Martina, Andrea Guariso, Jakob Svensson, and David Yanagizawa-Drott. 2019. "Reducing child mortality in the last mile: experimental evidence on community health promoters in Uganda." *American Economic Journal: Applied Economics*, 11(3): 155–92.

Borker, Girija. 2017. "Safety first: Perceived risk of street harassment and educational choices of women." *Job Market Paper, Department of Economics, Brown University*, 12–45.

Borker, Girija, Jan Eeckhout, Nancy Luke, Shantidani Minz, Kaivan Munshi, and Soumya Swaminathan. 2017. "Wealth, Marriage, and Sex Selection." Working Paper, Cambridge University.

Brenner, Jennifer L, Jerome Kabakyenga, Teddy Kyomuhangi, Kathryn A Wotton, Carolyn Pim, Moses Ntaro, Fred Norman Bagenda, Ndaruhutse Ruzazaaza Gad, John Godel, James Kayizzi, et al. 2011. "Can volunteer community health workers decrease child morbidity and mortality in southwestern Uganda? An impact evaluation." *PloS one*, 6(12): e27997.

Buchmann, Nina, Erica M Field, Rachel Glennerster, and Reshmaan N Hussam. 2019. "Throwing the baby out with the drinking water: Unintended consequences of arsenic mitigation efforts in Bangladesh." National Bureau of Economic Research.

Celhay, Pablo A, Paul J Gertler, Paula Giovagnoli, and Christel Vermeersch. 2019. "Long-run effects of temporary incentives on medical care productivity." *American Economic Journal: Applied Economics*, 11(3): 92–127.

Chen, Yuyu, Hongbin Li, and Lingsheng Meng. 2013. "Prenatal sex selection and missing girls in China: Evidence from the diffusion of diagnostic ultrasound." *Journal of Human Resources*, 48(1): 36–70.

Clark, Shelley. 2000. "Son preference and sex composition of children: Evidence from India." *Demography*, 37(1): 95–108.

Cohen, Alma, Rajeev Dehejia, and Dmitri Romanov. 2013. "Financial incentives and fertility." *Review of Economics and Statistics*, 95(1): 1–20.

Das Gupta, Monica, and PN Mari Bhat. 1997. "Fertility decline and increased manifestation of sex bias in India." *Population studies*, 51(3): 307–315.

Das, Narayan. 1987. "Sex preference and fertility behavior: A study of recent Indian data." *Demography*, 24(4): 517–530.

Ebenstein, Avraham. 2010. "The "missing girls" of China and the unintended consequences of the one child policy." *Journal of Human Resources*, 45(1): 87–115.

Edlund, Lena. 1999. "Son preference, sex ratios, and marriage patterns." *Journal of political Economy*, 107(6): 1275–1304.

Edlund, Lena, Hongbin Li, Junjian Yi, and Junsen Zhang. 2013. "Sex ratios and crime: Evidence from China." *Review of Economics and Statistics*, 95(5): 1520–1534.

Gellatly, Corry, and Marion Petrie. 2017. "Prenatal sex selection and female infant mortality are more common in India after firstborn and second-born daughters." *J Epidemiol Community Health*, 71(3): 269–274.

Goodkind, Daniel. 1996. "On substituting sex preference strategies in East Asia: Does prenatal sex selection reduce postnatal discrimination?" *Population and Development Review*, 22(1): 111–126.

Hesketh, Therese, and Zhu Wei Xing. 2006. "Abnormal sex ratios in human populations: Causes and consequences." *Proceedings of the National Academy of Sciences*, 103(36): 13271–13275.

Jayachandran, Seema, and Ilyana Kuziemko. 2011. "Why do mothers breastfeed girls less than boys? Evidence and implications for child health in India." *The Quarterly journal of economics*, 126(3): 1485–1538.

Jayachandran, Seema, and Rohini Pande. 2017. "Why are Indian children so short? The role of birth order and son preference." *American Economic Review*, 107(9): 2600–2629.

Jensen, Robert. 2012. "Another mouth to feed? The effects of (in) fertility on malnutrition." *CESifo Economic Studies*, 58(2): 322–347.

Joshi, Shareen, and Anusuya Sivaram. 2014. "Does it pay to deliver? An evaluation of India's safe motherhood program." *World Development*, 64: 434–447.

Kashyap, Ridhi. 2019. "Is prenatal sex selection associated with lower female child mortality?" *Population studies*, 73(1): 57–78.

Kraemer, Sebastian. 2000. "The fragile male." *BMJ*, 321(7276): 1609–1612.

Nandi, Arindam, and Ramanan Laxminarayan. 2016. "The unintended effects of cash transfers on fertility: evidence from the Safe Motherhood Scheme in India." *Journal of Population Economics*, 29(2): 457–491.

O'donnell, Owen, Eddy Van Doorslaer, Wagstaff Wagstaff, and Magnus Lindelow. 2008. "Analyzing health equity using household survey data."

Ritchie, Hannah, and Max Roser. 2019. "Gender Ratio." *Our World in Data*. https://ourworldindata.org/gender-ratio.

Rose, Elaina. 1999. "Consumption smoothing and excess female mortality in rural India." *Review of Economics and statistics*, 81(1): 41–49.

Rosenblum, Daniel. 2013. "The effect of fertility decisions on excess female mortality in India." *Journal of Population Economics*, 26(1): 147–180.

Rosenblum, Dan, et al. 2013. "Economic incentives for sex-selective abortion in India." *Canadian Centre for Health Economics*, 2014–13.

Roy, Sanchari. 2015. "Empowering women? Inheritance rights, female education and dowry payments in India." *Journal of Development Economics*, 114: 233–251.

Sekher, TV. 2012. "Ladlis and Lakshmis: financial incentive schemes for the girl child." *Economic and Political Weekly*, 58–65.

Sekhri, Sheetal, and Adam Storeygard. 2014. "Dowry deaths: Response to weather variability in India." *Journal of development economics*, 111: 212–223.

Sen, Soumendu, Sayantani Chatterjee, Sanjay K Mohanty, et al. 2020. "Unintended effects of Janani Suraksha Yojana on maternal care in India." *SSM-population health*, 100619.

Sinha, Nistha, and Joanne Yoong. 2009. *Long-term financial incentives and investment in daughters: Evidence from conditional cash transfers in North India.* The World Bank.

Valente, Christine. 2014. "Access to abortion, investments in neonatal health, and sex-selection: Evidence from Nepal." *Journal of Development Economics*, 107: 225–243.

Visaria, Leela. 2005. "Female deficit in India: Role of prevention of sex selective abortion act." 5–7, Citeseer.

Waterfield, Henry. 1875. *Memorandum on the Census of British India of 1871-72.* Vol. 1349, HM Stationery Office.

A Tables

Table 1: Descriptive Statistics

	Treat	Control	Diff.	Std Err
Prop of girls	0.443	0.442	-0.001	0.0037
Mother's education	4.607	7.426	2.818***	0.0588
No of women in hh	1.356	1.282	-0.074***	0.0094
Sex of hh head	1.125	1.116	-0.009**	0.0041
Age of hh head	44.452	47.308	2.857***	0.1721
Ultrasound use	0.257	0.376	0.120***	0.0276
Fertility	3.139	2.379	-0.760***	0.0148
SC	0.000	0.000	0.000	0.0000
ST	0.000	0.000	0.000	0.0000
OBC	0.649	0.510	-0.139***	0.0061
Forward Caste	0.280	0.422	0.142***	0.0058
Hindu	0.810	0.758	-0.052***	0.0051
Muslim	0.179	0.100	-0.078***	0.0045
Christians	0.002	0.037	0.034***	0.0015
Other religions	0.009	0.105	0.096***	0.0025
BPL	0.000	0.000	0.000	0.0000
Poorest	0.138	0.132	-0.006	0.0043
Poorer	0.158	0.155	-0.003	0.0046
Middle	0.186	0.198	0.012**	0.0049
Richer	0.218	0.239	0.021***	0.0052
Richest	0.300	0.276	-0.025***	0.0057
Elec	0.981	1.055	0.074***	0.0125
Radio	0.301	0.217	-0.085***	0.0137
TV	0.686	0.935	0.250***	0.0135
Refrigerator	0.377	0.586	0.209***	0.0140
Cycle	0.807	0.648	-0.160***	0.0136
Scooter	0.548	0.612	0.064***	0.0140
Truck	0.249	0.227	-0.021	0.0137
N	45195	28628		

Table 2: Main Results: Estimation Results for Difference in Difference and Triple Difference Estimation

	Dep Var: Girl							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treat×Post	0.0484**		-0.0108		451.9		-0.0804	
	(2.20)		(-0.22)		(0.00)		(-1.54)	
Treat×Post$_I$		0.0407**		-0.0166		620.7		-0.0626
		(2.05)		(-0.36)		(0.00)		(-1.30)
Treat×Post$_{II}$		0.0864**		0.0136		176.5		-0.0793
		(2.42)		(0.23)		(0.00)		(-1.35)
Treat×Post×First_Girl			0.126**		0.105*		0.114**	
			(2.26)		(1.77)		(2.04)	
Treat×Post$_I$×First_Girl				0.116**		0.0971*		0.107*
				(2.14)		(1.71)		(1.97)
Treat×Post$_{II}$×First_Girl				0.183**		0.152**		0.163**
				(2.71)		(2.21)		(2.51)
FE	✓	✓	✓	✓	✓	✓	✓	✓
State Year Trend							✓	✓
State Year FE					✓	✓		
No. of Obs.	150757	150757	63250	63250	63204	63204	63250	63250

Notes: *p<0.1; **p<0.05;***p<0.01
The table reports difference in difference and triple difference coefficient of the impact of JSY on the likelihood of observing the child born to be a girl. *Treat* is the dummy variable that takes the value 1 if the mother is from the treatment group. Similarly, *First_Girl* indicates if the woman's first born child was a girl. *Post* compares post program years (2006-2015) to the pre program years (2000-2005). $Post_I$ and $Post_{II}$ are the early (2006-2010) and late diffusion (2011-2015) periods of the program. *FE* contains mother, birth and year fixed effects. State Year Trend is the state specific time trend and State Year FE is the State Year fixed effect. All triple difference estimates are for children at parity 2 onward. t-statistic in parentheses. Standard errors are clustered at the state level.

Table 3: Falsification Test: Triple Difference Estimation using DHS 2005-06

	Dep Var: Girl		
	(1)	(2)	(3)
Treat×Post1996-00×First_Girl	-0.0362		
	(-0.57)		
Treat×Post2001-05×First_Girl	-0.0921		
	(-1.12)		
Treat×Post1995-05×First_Girl		-0.0480	
		(-0.72)	
Treat×Post2001-05×First_Girl			-0.0517
			(-1.22)
FE	✓	✓	✓
No. of Obs.	15524	15524	11987

Notes: *p<0.1; **p<0.05;***p<0.01
The table reports the triple difference results for the falsification tests using DHS 2005-06 data collected prior to the implementation of the program. In columns (1) and (2), we assume 1995 to be the year of program implementation. In column (1), we consider years 1996 - 2000 and years 2001-2005 as early and late diffusion periods. These are compared to the pre program period 1990-1995. In column (2), we assume years 1996-2005 as post program years. In column (3), we assume 2000 as the year of program implementation. Post program years 2001-2005 are compared to pre program year 1996-2000. $Treat$ is the dummy variable that takes the value 1 if the mother is from the treatment group. Similarly, $First\_Girl$ indicates if the woman's first born child was a girl. FE contains mother, birth and year fixed effects. All triple difference estimates are for children at parity 2 onward. FE contains mother, birth and year fixed effects. t-statistic in parentheses. Standard errors are clustered at the state level.

Table 4: Estimation Results for Mortality for children under 1 year

	Dep Var: Mortality before age1					
	All Births		Parity >1		Parity>2	
	(1)	(2)	(3)	(4)	(5)	(6)
Treat×Post×Girl	0.00743		0.0203*		0.0423	
	(1.15)		(1.77)		(1.59)	
Treat×Post$_I$×Girl		0.00898		0.0253**		0.0708**
		(1.45)		(2.22)		(2.57)
Treat×Post$_{II}$×Girl		0.00539		0.0174		0.00868
		(0.68)		(1.08)		(0.27)
FE	✓	✓	✓	✓	✓	✓
No. of Obs.	150757	150757	63250	63250	23275	23275

Notes: *p<0.1; **p<0.05;***p<0.01
The table reports the likelihood of a girl in treatment group dying before she reaches age 1 and age 5. Columns 1 and 2 record the likelihood of girls dying before reaching age 1 and 5.Columns 3 and 4 record the likelihood of girls born at parity 2 and above dying before reaching age 1 and 5. Columns 5 and 6 record the likelihood of girls born at parity 3 and above dying before reaching age 1 and 5. $Treat$ that takes the value 1 if the mother is from our treatment group. $Post$ compares post program years (2006-2015) to the pre program years (2000-2005). $Post_I$ and $Post_{II}$ are the early (2006-2010) and late diffusion (2011-2015) periods of the program. FE contains mother, birth and year fixed effects. t-statistic in parentheses. Standard errors are clustered at the state level.

Table 5: Estimation Results for Mortality for children under 5 year

	Dep Var: Mortality before age 5					
	All Births		Parity >1		Parity>2	
	(1)	(2)	(3)	(4)	(5)	(6)
Treat×Post×Girl	0.0134		0.0134		0.0578*	
	(1.08)		(1.08)		(2.02)	
Treat×Post$_I$×Girl		0.0200		0.0200		0.0911***
		(1.56)		(1.56)		(3.03)
Treat×Post$_{II}$×Girl		0.00854		0.00854		0.0182
		(0.55)		(0.55)		(0.56)
FE	✓	✓	✓	✓	✓	✓
No. of Obs.	63250	63250	63250	63250	23275	23275

Notes: *p<0.1; **p<0.05;***p<0.01
The table reports the likelihood of a girl in treatment group dying before she reaches age 5. Columns 1 and 2 record the likelihood of girls dying before reaching age 5. Columns 3 and 4 record the likelihood of girls born at parity 2 and above dying before reaching age 5. Columns 5 and 6 record the likelihood of girls born at parity 3 and above dying before reaching age 5. $Treat$ that takes the value 1 if the mother is from our treatment group. $Post$ compares post program years (2006-2015) to the pre program years (2000-2005). $Post_I$ and $Post_{II}$ are the early (2006-2010) and late diffusion (2011-2015) periods of the program. FE contains mother, birth and year fixed effects. t-statistic in parentheses. Standard errors are clustered at the state level.

Table 6: Mechanism: Ultrasound

	Dep Var: Girl	
	(1)	(2)
Treat×First_Girl×Likelihood^{Ultrasound}	-0.237	
	(-0.76)	
Treat×First_Girl	-458884.5	
	(-0.00)	
First_Girl×Likelihood^{Ultrasound}	1.546***	
	(5.50)	
Likelihood^{Ultrasound}	-1.566***	
	(-4.66)	
Treat×Reported_Ultrasound		-0.0166
		(-0.50)
Reported_Ultrasound		-0.0526*
		(-1.76)
FE	✓	✓
No. of Obs.	64248	40240

Notes: *p<0.1; **p<0.05;***p<0.01
The table reports coefficients of the impact of the likelihood of ultrasound availability in the neighbourhood on the likelihood of observing the child born to be a girl. The likelihood of ultrasound availability data is observed from 2010 on wards. $Likelihood^{Ultrasound}$ is obtained using equation 8. $Treat$ that takes the value 0 if the mother is from our treatment group.FE contains mother, birth and year fixed effects. t-statistic in parentheses. Standard errors are clustered at the state level

Table 7: Mechanism: Wealth Effect

	Dep Var: Girl
	(1)
Treat×Post×Poorer	0.0353
	(0.92)
Treat×Post×Middle	-0.0215
	(-0.62)
Treat×Post×Richer	0.0420
	(1.39)
Treat×Post×Richest	-0.0154
	(-0.43)
FE	✓
No. of Obs.	150757

Notes: *p<0.1; **p<0.05; ***p<0.01
The table reports coefficients of wealth category likelihood of observing the child born to be a girl. The reference is the poorest category given in the DHS data. *Treat* is the dummy variable that takes the value 0 if the mother is from our treatment group. *Post* compares post program years (2006-2015) to the pre program years (2000-2005). *FE* contains mother, birth and year fixed effects. t-statistic in parentheses. Standard errors are clustered at the state level.

Table 8: Mechanism: Income Effect

	Dep Var: Girl		
	(1)	(2)	(3)
Treat×Post×Rain_Shock	-0.00612	0.0247	0.0149
	(-0.32)	(0.84)	(0.30)
Treat×Rain_Shock	0.0214	-0.00509	-0.000966
	(1.20)	(-0.18)	(-0.02)
Post×Rain_Shock	0.00792	-0.0157	0.00927
	(0.48)	(-0.57)	(0.20)
Treat×Post	0.0531*	0.0448*	0.0470**
	(1.94)	(1.85)	(2.08)
FE	✓	✓	✓
No. of Obs.	150757	150757	150757

Notes: *p<0.1; **p<0.05;***p<0.01
The table reports coefficients of an income shock on the likelihood of observing the child born to be a girl. The income shock is proxied by rainfall below long run mean. Columns (1), (2) and (3) record the effect of rainfall below long run mean, rainfall 1 and 2 standard deviations below long run mean respectively . *Treat* that takes the value 0 if the mother is from our treatment group. *Post* compares post program years (2006-2015) to the pre program years (2000-2005). *FE* contains mother, birth and year fixed effects. t-statistic in parentheses. Standard errors are clustered at the state level.

Table 9: Mechanism: Health Workers Effect

	Dep Var: Girl	
	(1) Public	(2) Private
Treat×Health_Worker	0.0148**	-0.0119
	(2.09)	(-0.98)
Health_Worker	-0.00868	0.00357
	(-1.55)	(0.36)
FE	✓	✓
No. of Obs.	56614	56614

Notes: *p<0.1; **p<0.05;***p<0.01
The table reports coefficients of the effect of health workers per 10,000 women on the likelihood of observing the child born to be a girl. The data on health workers is available from 2008 on wards. In column (1) and (2), we use the data on the number of health workers who were paid JSY incentives for deliveries in public institutions and private institutions, respectively. *Treat* is the dummy variable that takes the value 1 if the mother is from our treatment group. *FE* contains mother, birth and year fixed effects. State Year Trend is the state specific time trend. t-statistic in parentheses. Standard errors are clustered at the state level.

Table 10: Quantile level HAZ outcomes for children aged 0-5 years

	0.1 (1)	0.25 (2)	0.5 (3)	0.75 (4)	0.9 (5)
Girl × Treat	-0.0351	-0.0871**	-0.0441	-0.0513*	0.0052
	(0.0499)	(0.0381)	(0.0299)	(0.0289)	(0.0667)
Girl	0.1045**	0.1202***	0.0490*	0.0083	0.0176
	(0.0453)	(0.0296)	(0.0266)	(0.0260)	(0.0618)
Treat	-0.0870**	-0.1044***	-0.1425***	-0.1812***	-0.3220***
	(0.0365)	(0.0237)	(0.0217)	(0.0212)	(0.0375)
Mom Age	0.1036***	0.0878***	0.0754***	0.0483**	0.0943***
	(0.0189)	(0.0163)	(0.0185)	(0.0196)	(0.0270)
Mom Age Sq	-0.0010***	-0.0007***	-0.0007***	-0.0004	-0.0011**
	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0004)
Mom Education	0.0439***	0.0372***	0.0323***	0.0260***	0.0199***
	(0.0025)	(0.0026)	(0.0012)	(0.0021)	(0.0040)
Age at First Birth	-0.0370***	-0.0313***	-0.0170***	-0.0093*	-0.0112*
	(0.0048)	(0.0045)	(0.0030)	(0.0054)	(0.0062)
Total Eligible Women	-0.0316**	-0.0205**	-0.0011	0.0037	0.0051
	(0.0137)	(0.0101)	(0.0075)	(0.0078)	(0.0163)
Wealth	0.1748***	0.1725***	0.1618***	0.1433***	0.1186***
	(0.0118)	(0.0091)	(0.0051)	(0.0082)	(0.0106)
Birth Order FE	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓
N	64209	64209	64209	64209	64209

The table reports height for age outcomes for all quantiles for children aged 0-5 years in the sample. $Treat$ is the dummy variable that takes the value 0 if the mother is from our treatment group. Similarly, $Girl$ is an indicator for if the child is a girl. Since the results are only for children aged between 0-5 years, we cannot use mother and state fixed effects. Bootstrapped standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 11: Quantile level WAZ outcomes for children aged 0-5 years

	0.1 (1)	0.25 (2)	0.5 (3)	0.75 (4)	0.9 (5)
Girl × Treat	-0.0815**	-0.0797***	-0.0595***	-0.0522*	0.0106
	(0.0349)	(0.0247)	(0.0231)	(0.0283)	(0.0387)
Girl	0.1395***	0.0937***	0.0650***	0.0419*	-0.0274
	(0.0269)	(0.0253)	(0.0172)	(0.0240)	(0.0313)
Treat	0.0460*	-0.0049	-0.0506***	-0.0916***	-0.1557***
	(0.0253)	(0.0109)	(0.0172)	(0.0176)	(0.0240)
Mom Age	0.0832***	0.0652***	0.0607***	0.0633***	0.0544***
	(0.0188)	(0.0186)	(0.0136)	(0.0131)	(0.0202)
Mom Age Sq	-0.0008**	-0.0006*	-0.0005**	-0.0005**	-0.0004
	(0.0003)	(0.0003)	(0.0002)	(0.0002)	(0.0003)
Mom Education	0.0295***	0.0280***	0.0263***	0.0227***	0.0217***
	(0.0023)	(0.0018)	(0.0013)	(0.0017)	(0.0019)
Age at First Birth	-0.0275***	-0.0206***	-0.0161***	-0.0135***	-0.0113*
	(0.0063)	(0.0038)	(0.0038)	(0.0037)	(0.0067)
Total Eligible Women	0.0089	0.0029	-0.0005	-0.0011	0.0065
	(0.0073)	(0.0086)	(0.0085)	(0.0060)	(0.0099)
Wealth	0.1518***	0.1523***	0.1490***	0.1417***	0.1358***
	(0.0058)	(0.0044)	(0.0063)	(0.0056)	(0.0103)
Birth Order FE	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓
N	65240	65240	65240	65240	65240

The table reports weight for age outcomes for all quantiles for children aged 0-5 years in the sample. $Treat$ is the dummy variable that takes the value 0 if the mother is from our treatment group. Similarly, $Girl$ is an indicator for if the child is a girl. Since the results are only for children aged between 0-5 years, we cannot use mother and state fixed effects. Bootstrap standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

B Figures

Figure 1: Sex Ratio at Birth by Birth Order

Figure 2: Comparison Groups

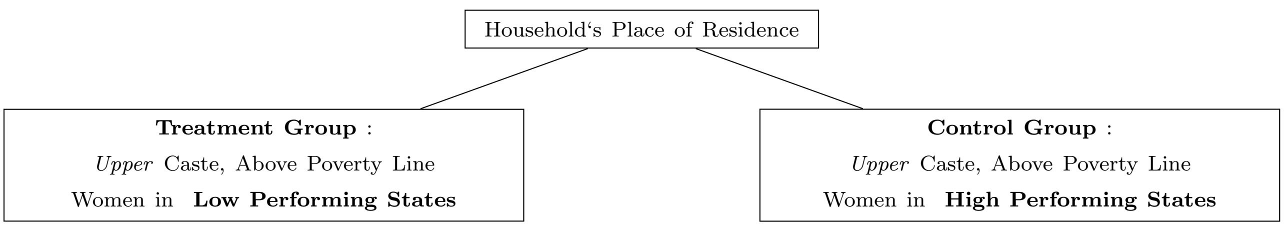
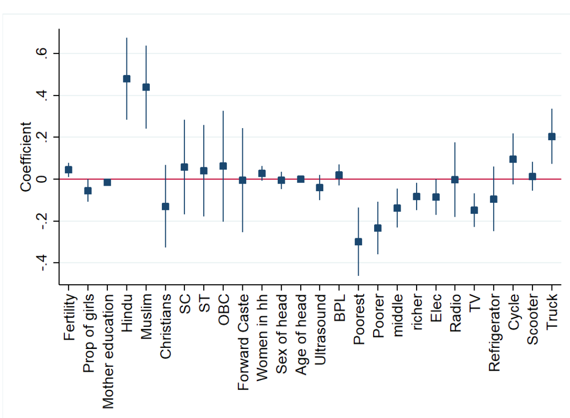
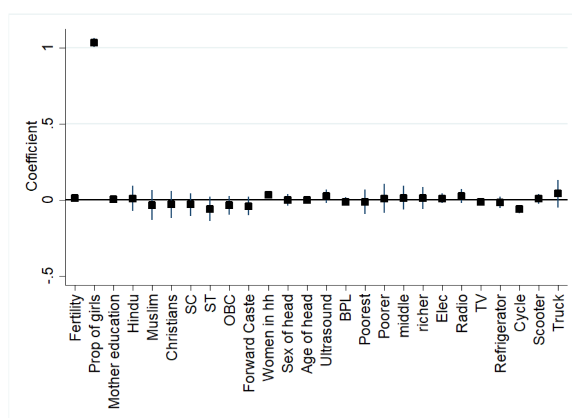


Figure 3: Test of balance

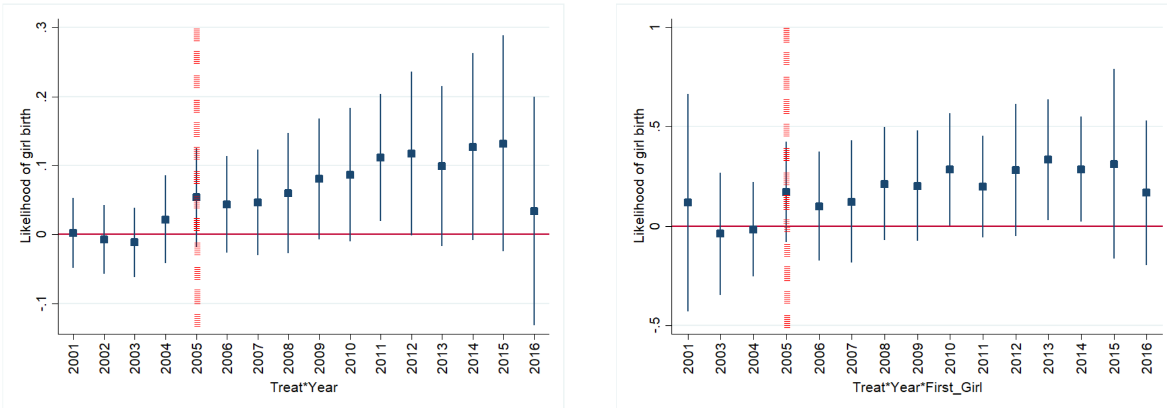


(a) Treatment and Control



(b) First girl and First boy families

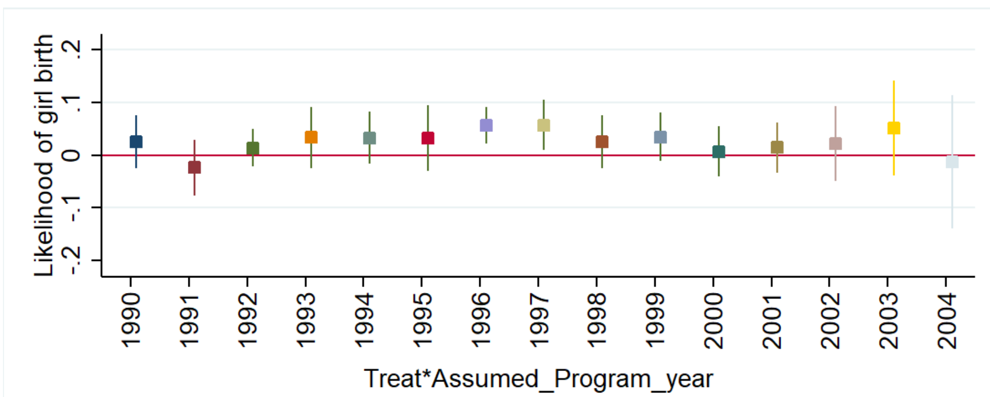
Figure 4: Test of non differential pretrends



(a) Difference in Difference

(b) Triple Difference

Figure 5: Falsification Tests using DHS - 2015/16



This figure reports coefficients of the difference-in-difference analysis assuming years from 1990 to 2004 as program years and check if the likelihood of female birth is difference across the treatment and control group. All regressions contain mother, birth and year fixed effects. Standard errors are clustered at the state level.

Figure 6: Changes in Missing Women over year based on author's estimates.

Population data for India from Census 2011 and mortality data for reference group from UM World Population Prospects 2019.

C Hindu Inheritance Law

One of the possible concerns with our results is that there could be other factors or other government programs that changed concurrently which may simultaneously change household's preference for girls. One such policy is the Hindu Succession Act 1956, which came into force in September 2005. This act allows women to inherit property of their fathers and have legal rights on properties of their husbands. Prior to 2005, implementation of this law was voluntary for states but in 2005, The central government of India mandated all states to impose this law. We suspect that this will impact household's preference for female children and its effect could be confounded in our results. Though any changes that impact the propensity of households to prefer girls is controlled by the state year fixed effects in our model, we still include a dummy variable that takes the value of 1 if a state in the given year had implemented the Inheritance Law and 0 other wise. Since some states had introduced this law prior to 2005, there is substantial variation in this variable to capture the program effect. We find that inclusion of inheritance law dummy does not change our results and is in fact not significant in the regression. Out main coefficients also do not change with the inclusion of this variable.

Table 12: Main Results: Estimation Results for Triple Difference with Inheritance Law

	Dep Var: Girl	
	(1)	(2)
Treat*Post*First_Girl	0.114*	
	(2.03)	
Treat*Post$_I$ $*$ $First\_Girl$		0.107*
		(1.96)
Treat*Post$_{II}$ $*$ $First\_Girl$		0.162**
		(2.49)
Inheritance Law	0.0168	0.0183
	(0.28)	(0.30)
FE	✓	✓
State Year FE	✓	✓
No. of Obs.	63250	63250

Notes: *p<0.1; **p<0.05;***p<0.01
The table reports triple difference coefficient of the impact of JSY on the likelihood of observing the child born to be a girl controlling for the change in inheritance law. $Treat$ is the dummy variable that takes the value 1 if the mother is from our treatment group. Similarly, $First_Girl$ is an indicator for if the woman's first born child was a girl. $Post$ compares post program years (2006-2015) to the pre program years (2000-2005). $Post_I$ and $Post_{II}$ are the early (2006-2010) and late diffusion (2011-2015) periods of the program. FE contains mother, birth and year fixed effects. State Year Trend is the state specific time trend and State Year FE is the State Year fixed effect. All triple difference estimates are for the sex of the child born at birth order 2 or higher. Standard errors in parentheses are clustered at the state level.