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

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Abstract

This study examines the unintended consequences of the Janani Suraksha Yojana, a conditional cash transfer program in India, on prenatal sex-selective behavior within a son-preference culture. This program unintentionally altered existing trends in prenatal sex selection through its simultaneous provision of cash incentives to households and community health workers as well as access to prenatal sex detection technology such as ultrasound scans. Using difference-in-differences and triple difference estimators we find that the program causes an increase in the likelihood of female births. Furthermore, we observe a rise in under-5 mortality for girls born at higher birth orders, suggesting a shift in discrimination against girls from prenatal to postnatal. Our calculations suggest that the net impact was approximately 300,000 girls surviving in treated states between 2006 and 2015. Finally, we find suggestive evidence that the involvement of community health workers in facilitating the program is a key driver of this trend. Overall, this study sheds light on the complex interplay between policy interventions, cultural norms, and gender disparities in shaping demographic outcomes

JEL Codes: J13, J16, J18

Keywords: prenatal sex selection, missing girls, sex-selective abortions, community health workers, Janani Suraksha Yojana

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Conflict of Interest

The authors declare that they have no conflict of interest.

Data availability

The data and do file will be made available upon request.

1 Introduction

The long history of son preference in India has resulted in nearly 63 million women missing from the country's population with almost 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 providing parents with 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 et al., 2013; Anukriti et al., 2022). This paper demonstrates how accessible ultrasound technology, along with financial incentives provided under a nationwide safe motherhood program, interacts with the culture of son preference to influence the gender imbalance in India. We examine the causal relationship between a safe motherhood program and sex-selective behaviour among Indian parents and investigate the underlying mechanism that explains this relationship.

The safe motherhood program, known as Janani Suraksha Yojna (JSY) was launched by the Indian Government in 2005 to reduce maternal and neonatal mortality. Mothers were given cash payments for every live birth in a health facility. The program also mandated that beneficiaries undergo at least three antenatal checkups, including ultrasound scans (a prenatal sex-determination technology). To facilitate the program, the government recruited health workers. They received financial incentives for every institutional delivery by registered mothers in their neighbourhood. The scheme, thus, provided simultaneous access to prenatal sex detection technology and cash incentives for institutional births. Therefore, the program had the potential to influence sex-selective behaviour among Indian parents unintentionally. To estimate the impact of the program on prenatal sex selection we use difference-in-differences (DID) and triple difference (DDD) estimators that exploit the variation in the timing of program implementation, eligibility of beneficiary households based on their socioeconomic status and geographic location, and the natural experiment created by sex of the firstborn child.

Before the implementation of the program, states in India were categorized as low or high-performing based on their state-specific institutional delivery rates. The eligibility criteria for program benefits varied by household socioeconomic characteristics across this classification. All women residing in low-performing states were eligible for the program, however, only those living below the poverty line (BPL) and belonging to the Schedule Castes or Schedule Tribes (SC/ST) in high-performing states could participate. Taking advantage of this variation in program access, we compare women living above the poverty line and not belonging to SC/ST from low-performing states with their counterparts from the high-performing states who were excluded from the program. In other words, non-BPL, non-SC/ST women from low and high-performing states composed the treatment and control group respectively [2]. For our analysis, we created a mother-child panel using the reported fertility history of mothers from the Demographic and Health Survey of India (DHS) - 2015/16.

The identification of our estimates is conditional on the inclusion of mother-fixed effects which accounts for the systematic differences in the characteristics of mothers in our two comparison groups. We include the child's year of birth fixed effects that account for the unobserved time-varying factors that may influence the propensity of births to a mother over time. We also include the child's birth order fixed effect to account for the unobserved heterogeneity in the propensity to sex-select dissimilarly for different birth orders. To ensure the robustness of our estimator to any time-variant state-specific shocks and policies that could conflate with the program's impact, we include state-year fixed effects in our DDD estimation. We measure sex-selective

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

[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

behaviour as the likelihood of female birth at every birth order for a mother.

Our DID estimator shows that the program increased the likelihood of female births by 4.8 percentage points. The DDD estimates support this finding. We show that the families in the treatment group with a firstborn 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 the greater prevalence of prenatal sex selection among the *forward* caste, non-poor families and families with firstborn daughters (Borker et al., 2018; Anukriti, 2018; Almond et al., 2019; Rosenblum, 2013). To establish the robustness of our results, we verify the identifying assumption. Our identifying assumption is that in the absence of the policy, the likelihood of female births evolves 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 before the launch of the program and find no program effects on 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.

Furthermore, we explore what these results mean for the survival and well-being of these additional girls. We found suggestive evidence 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. Although these results are not causal effects of the program, they provide additional insights into fertility dynamics in India, particularly among mothers in the treatment states. Our broad calculations indicate that the net effect of the program on female births is an overall increase of 300,000 girls born in treatment states between 2006 and 2015.

How did JSY influence the 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 strong son preferences may use these for sex selection by inducing abortions of unwanted female fetuses. Second, the financial incentives given to households for every live birth lowered the cost of bearing children. This is a motivator to not carry out sex selection and to give birth to their child, particularly during periods of economic shock. Third, health workers' remuneration was linked to the number of beneficiaries registered for the policy and their deliveries at health centres. This is an incentive for health workers to dissuade parents from performing sex-selective abortions and to encourage them to give birth to their female children. Finally, 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. Hence, 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.[4]

We use the Health Management Information System data obtained from the Ministry of Women and Child Development, Govt. of India. Using this approach, we created a dataset of all health workers at the district level from 2008 to 2015. We find suggestive evidence that neither access to ultrasound technology nor the financial incentives given to parents explain the increased propensity for having girls in the treatment states. We find descriptive evidence that the increase is explained by the presence of health workers. This result has important policy implications. This shows 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 postnatal as a response to this policy. This is a reversal of the prevailing trend where access to ultrasound technology shifted discrimination against girls from postnatal to prenatal (Bhalotra and Cochrane, 2010; Bhaskar, 2007). Although this result is not encouraging, it shows that there is scope for policy to achieve desirable fertility outcomes even in the presence of conflicting cultural beliefs. We discuss policy ideas in section VI.

[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.

[4]We test these various mechanisms, however given the availability of data and restrictions on empirical estimations, we can only provide supportive evidence supporting or rejecting these mechanisms.

This paper contributes to the extensive literature on missing women. Several existing papers examine the effect of ultrasound technology on sex ratios at birth and the relative well-being of female children (Chen et al. (2013); Anukriti et al. (2022); Lin et al. (2014); Hu and Schlosser (2015); Almond et al. (2019); Bharadwaj and Lakdawala (2013); Valente (2014)). All of these studies show that the increased availability of fetal gender identification technology induces parents to abort unwanted female fetuses. The surviving girls, therefore, are wanted and acquire health investments. The structure of the program creates a tradeoff between access to technology for prenatal sex selection and health workers' performance-based benefits and beneficiaries' own cash transfers for live deliveries at health centres. We fill the gap in the literature by documenting the impact of the program on sex-selective behaviour for the population amongst whom the practice is most prevalent, the upper-caste and wealthy families in India (Bhalotra and Cochrane, 2010). We find that the program causes a decline in sex-selective abortions in India.

The second contribution of this paper is to the growing literature on 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. JSY was implemented to reduce maternal and neonatal deaths during deliveries. The scheme did not target improving gender equality at birth. Existing literature assessing the impact of JSY has studied its impact on the uptake of maternity services and maternal mortality (Powell-Jackson et al., 2015), fertility (Nandi and Laxminarayan, 2016), maternal care (Sen et al., 2020) and the academic performance of children (Chatterjee and Poddar, 2021). This paper is the first to study the impact of JSY on sex selection, an outcome it did not target, and understand the underlying mechanism. The final contribution of this paper is to the growing literature on the importance of community health workers, to achieve desirable maternal and child well-being objectives. Several studies have documented the impact of financial incentives given to community health workers on a reduction in child mortality and morbidity (Cohen et al., 2013; Björkman Nyqvist et al., 2019; Celhay et al., 2019; Brenner et al., 2011; Singh and Masters, 2017). We add to this growing literature by showing suggestive evidence of the contribution of health workers in the reduction of prenatal sex selection in the treatment states.

In terms of methodology, our paper is closest to Anukriti et al. (2022), but our paper differs in three ways. First, we study how simultaneous access to prenatal sex detection technology and financial incentives to households and health workers affect prenatal sex-selective behaviour. The trade-off between these dimensions of the policy is an unintended consequence of the intervention designed to tackle low rates of institutional deliveries. This 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 prenatal sex-selective behaviour is more prominent for this group. Finally, we attempt to explain how policy mechanisms affect prenatal sex-selective behaviour. The mechanisms that explain these respective results are distinct. We found that community health workers played a prominent role in increasing the likelihood of female births. Their paper finds the decline in desired fertility and lower birth spacing as the driving factors of the decrease in the number of female births.

This paper is organized as follows: Section II provides background on son preference in India and discusses the data and descriptive statistics. Section III introduces the empirical strategy used in the paper. Section IV is a discussion of the results. Section V presents the robustness tests. Section VI is a discussion on mortality and additional evidence. Section VII discusses and tests various mechanisms that explain the results and section VIII concludes the paper with some policy recommendations.

2 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 (Waterfield, 1875). Today this discrimination is reflected in skewed sex ratios at birth and child sex ratios. The natural sex ratio at birth for humans is estimated to be between 104 and 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[5]. 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 preference for boys over girls (Clark, 2000; Almond et al., 2019).

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 husbands' families. Although 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 et al., 2020; Roy, 2015)[6]. Paying large dowries for daughters (Borker et al., 2018) and safety concerns also make it more costly for parents to have a daughter (Alfano, 2017; Borker, 2021; Anukriti et al., 2022). Furthermore, there is some evidence that sons benefit from economic advantages in the labour 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*, of having children until they reached their desired number of boys. As a result, girls were born into larger families with limited resources and therefore received lower investments (Jensen, 2012; Arnold et al., 1998; Das Gupta and Mari Bhat, 1997). This postnatal discrimination resulted in worse health outcomes and excess mortality among young girls. With the advent of prenatal sex determination technology, parents can determine the sex of the fetus within seven weeks of pregnancy[7]. This allowed parents to abort unwanted female fetuses (Chen et al., 2013; Bhalotra and Cochrane, 2010). Easy access to ultrasounds since the mid-1980s and an increasing preference for smaller families have led households to change their behaviour from postnatal discrimination to prenatal discrimination (Goodkind, 1996; Kashyap, 2019).

A feature observed since the 1990s 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 firstborn is a daughter are more likely to have prenatal sex-selective abortions from the second birth onwards than are parents whose firstborn is a son. Figure 1 plots the 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 the 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 firstborn children. The dashed line and the dotted line plot 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 the 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. Although the 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, such as the price of gold, dowry and marriage conventions and the religious identity of the political leader (Bhalotra et al., 2018, 2020).

2.1 Janani Suraksha Yojna

In 2005, the Government of India launched Janani Suraksha Yojana, a conditional cash transfer program sponsored 100% by the national Government with a dual objective of reducing the number of maternal and

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

[6]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.

[7]PNSDT or fetal gender identification technology.

Figure 1: Sex Ratio at Birth by Birth Order

Figure Notes: Sex ratio is measured as the number of males per 100 females.

neonatal deaths nationwide[8]. This scheme promoted safe motherhood by providing cash incentives to women if they delivered their children either in government hospitals or in accredited private health institutions or at home under medical supervision[9]. A further condition to receive the full cash incentive was that the mother should undergo at least three prenatal checkups that include ultrasound and amniocentesis, technologies used to determine fetal sex. By mandating ante-natal checkups, JSY enabled higher access and use of ultrasound technology even in areas that previously did not have access to it.

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 who give birth at health centres as shown in Figure 1. 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 who gave birth safely at health facilities (Joshi and Sivaram, 2014).

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 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. Eligibility in these states was restricted to women who were 19 years of age or older and who were giving birth to their first or second child. The remuneration received by beneficiaries also differed across the states. Eligible women in the low-performing states received Rs. 1400 (20$) in rural areas and Rs. 1000(14$)

[8]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 women below the poverty line, our estimates are not affected by the earlier scheme.

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

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-time cash instalment upon discharge from the hospital or health centre[10]. The structure of program eligibility across states gives us our comparison groups. The treatment group includes the non-BPL and non-SC/ST women from the low-performing states. The control group includes the non-BPL and non-SC/ST women from the high-performing states.

A novel feature of the program was the introduction of community health workers or the accredited social health activist (ASHA) who acted as a link between the government and the beneficiaries. Adult women who had a 12th-grade certificate and were from the same village as the beneficiaries were chosen as ASHAs. Engaging health workers from within the community was intended to foster relationships of trust and a belief that their advice was credible. The role of the ASHA was 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 included assisting the beneficiary in accessing prenatal health services, including at least three antenatal checkups, the TT injections and IFA tablets[11]. The ASHA was also supposed to counsel pregnant women to undertake safe deliveries and escort them to health centres. She was to provide information to the new mother on the benefits of breastfeeding and immunization of the infant. The role of the ASHAs was to ensure that the pregnant women in their villages had a safe motherhood experience by encouraging institutional deliveries and facilitating access to prenatal and postnatal health services.

ASHAs were rewarded with performance-based incentives based on the number of institutional 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 instalments, with the first half of the payment disbursed after the beneficiary's ANC and the second half paid upon discharge from the birth centre.

In June 2011, a few additional features were added to the program to eliminate all out-of-pocket expenditures related to deliveries, and the 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 centre and free stay at all government health institutions in both rural and urban areas.

The new features further extended access to health facilities for mothers and children. 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 can compare early and later versions of the JSY with preprogram years. The early period is from 2006 until 2010 and the later period is from 2011 until 2015, both of which are compared with preprogram period from 2000 to 2005.

2.2 Data and descriptive statistics

We use the Demographic and Health Survey data from 2015-2016. The DHS collects detailed information on every child born to women who were ever married in the age range of 15 to 49 years. This includes information on the sex of each child, the birth year, whether this child died or was alive in the year of the survey and whether he/she is a twin or not. Using this information we can create a panel of mothers and children for each state of India. While the data includes information on all children born between 1980 to 2016, we restrict our analysis to mothers who conceived their first child in or after the year 2000. By restricting our sample we can include mothers for whom the program occurred in between their fertility plans and estimate any changes in their fertility as a result of the exposure to the program.

Another reason to restrict our sample to mothers whose fertility started from 2000 onwards is that India

[10] 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.

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

first imported ultrasound machines in 1985 and the technology became widespread in 1995 when India also started manufacturing the machines locally (Bhalotra and Cochrane, 2010). Access to ultrasounds is one of the channels through which the program could affect sex-selective abortions. The supply shock could spread unevenly in the treatment and control groups. There are two reasons why this structural break can influence our estimates:

1. The enforcement of the Act takes time and hence areas that received the technology first (mostly big cities) were able to use it for sex-selection, till the enforcement was strengthened.
2. The diffusion of technology takes time, areas that first received the technology would have higher sex-selection rates than other areas till the time rest of the country received the technology as well. However, we do not think that this diffusion was complete by 2000 but we do expect that most urban areas got access to it early on.

While figure 3 shows that the effect of this shock dissipates over time, we restrict our estimations to five years after the shock so that its effect stabilises in the comparison groups. In table 15 column 7, we show our estimates for the whole sample and find that our choice of time restriction does not impact our main results.

We also restrict the analysis to rural areas, since the first areas to obtain 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 shows the descriptive statistics for the treatment and control groups. The proportion of girls overall as well as the proportion of girls born at different birth orders in both comparison groups is similar before 2005. However, there are a few maternal and household characteristics that differ across the two groups. We, therefore, account for these differences in our empirical strategy by including mother-fixed effects in the specification.

To understand the mechanisms driving sex-selective behaviour under this program, we used two additional data sources that were merged with DHS. First, we use rainfall data obtained from the Climate Hazards Centre of the University of California, Santa Barbara. Variability in precipitation has been shown to impact the vulnerability of the population, particularly in rural areas. This will help in elucidating the wealth/income channel. Climate Hazards Centre InfraRed Precipitation with Station (CHIRPS) data include records of monthly precipitation for each district of India from 1981 to 2015.[12] 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.[13] The number of health workers receiving JSY incentives for deliveries in public and private institutions were recorded from 2008 to 2015 in each district. A drawback is that the records show ASHAs at the district level, without an urban and rural distinction and only for postprogram years. This limits our interpretation of the effect of health workers on the program but offers suggestive evidence that can be explored in future research.

[12] 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/

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

Table 1: Descriptive Statistics

	Treatment			Control			
	N	Mean	SD	N	Mean	SD	Difference
Proportion of girls parity 1	18065	0.48	0.50	9874	0.48	0.50	0.005
Proportion of girls parity 2	9138	0.48	0.50	4480	0.48	0.50	0.002
Proportion of girls parity 3	2543	0.47	0.50	691	0.44	0.50	0.032
Proportion of girls parity 4 & above	327	0.46	0.50	65	0.51	0.50	-0.049
Total childern	18065	3.14	1.29	9874	2.38	0.97	0.760***
Hindu	18065	0.81	0.39	9874	0.76	0.43	0.052
Muslim	18065	0.18	0.38	9874	0.10	0.30	0.078
Forward Caste	18065	0.28	0.45	9874	0.42	0.49	-0.142*
OBC	18065	0.65	0.48	9874	0.51	0.50	0.139
Mother's education	18065	4.61	4.79	9874	7.43	4.51	-2.818***
Sex of household head	18065	1.13	0.33	9874	1.12	0.32	0.009
Age of household head	18065	44.45	13.52	9874	47.31	14.16	-2.857***
Self reported ultrasound use	1110	.26	.44	348	0.37	0.48	-.112
Wealth Indicators							
Poorest	18065	0.14	0.34	9874	0.13	0.34	0.006
Poorer	18065	0.16	0.36	9874	0.16	0.36	0.003
Middle	18065	0.19	0.39	9874	0.20	0.40	-0.012
Richer	18065	0.22	0.41	9874	0.24	0.43	-0.021**
Richest	18065	0.30	0.46	9874	0.28	0.45	0.025
Electricity	18065	0.98	1.11	9874	1.05	0.76	-0.074*
Truck	18065	0.25	1.18	9874	0.23	0.90	0.021
Fridge	18065	0.38	1.20	9874	0.59	0.94	-0.209**
Cycle	18065	0.81	1.17	9874	0.65	0.93	0.160
TV	18065	0.69	1.19	9874	0.94	0.84	-0.250***
Radio	18065	0.30	1.19	9874	0.22	0.90	0.085*

Notes: *p<0.1; **p<0.05;***p<0.01The baseline descriptive statistics are for women in rural areas who had their first birth during pre-program years 2000 - 2005. In the last column, we have the coefficients for the regression of the respective variable on the indicator $Treat$. Standard error clustered at the state level. The variable self-reported ultrasound use has missing values and is only reported in DHS for the last born child to the mother after 2010 hence the number of observations is different than the rest of the variables.

3 Empirical Strategy

The goal of this paper is to estimate the unintended causal effect of the policy on the prenatal sex-selective behaviour of households and its consequences for child well-being. We exploit the timing of the program, eligibility for the program based on the state of residence of the woman along with her socioeconomic status and the random variation in the sex of the first child born to a new mother. Using the eligibility criteria for the program as discussed in section 2.1, we compare women from high-performing states who were not eligible for the program with their counterparts in low-performing states who were eligible for the program. Therefore, to estimate the program effects, our treatment group comprises all births to mothers from upper caste and richer households in the rural areas of the low-performing states. Our control group includes births to mothers from upper caste and richer households in the rural areas of the high-performing states. Additionally, to account for changes over time and at the individual level, we also include additional comparisons in the propensity to sex selection by exploiting the sex of the firstborn child within our treatment and control groups. To estimate the impact of the program we employ a difference-in-differences and a triple-difference strategy which is discussed in detail below.

3.1 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 before the implementation of the policy, as shown in Table 1. We also find no significant difference in the proportion of girls at different birth orders in the comparison groups before 2005. Second, JSY was launched to increase institutional deliveries and not to tackle sex-selective abortions. However, it could be argued that gender attitudes and other development 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 delivery rates. These unobserved factors could influence sex-selective behaviour. Including mother-fixed effects in our estimation accounts for the potential bias arising from such unobserved heterogeneity at the household and mother levels.

In our canonical DID model, we have two time periods (pre and post-intervention), a treated group that receives the program at the beginning of the post-period, and a control group that does not receive the program in either period. In this setting, the key identifying assumption is that in the absence of the program, both the treatment and the control group would have evolved in a parallel manner. Section 5.1 discusses in detail and tests this identifying assumption for our DID estimator.

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 and θ_b are year of birth fixed effects and birth order fixed effects respectively, which eliminate year and birth order invariant factors that could confound the treatment effect[14]. Mother fixed effects δ_i eliminates factors that are invariant for each mother. As the program was implemented at the state level, we cluster by state to account for the serial correlation that could exist within these states Bertrand et al. (2004); Roth et al. (2023).

The DID coefficient β_1 captures within-mother differences in the likelihood of female births between the 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. Including the year of birth fixed effects through λ_t and birth order fixed effects through θ_b ensures that our estimation also accounts for any differences that could arise due to the year of birth or the birth order of the child. A positive and significant β_1 will indicate that the mothers in the treatment group who started their fertility before the program, are more likely to have girls after the program than mothers in the control group. That is, mothers in the treatment group are less likely to sex-select at every birth order compared to the control group mothers. Similarly, a negative β_1 will indicate the opposite effect confirming an increase in sex-selective behaviour due to the program.

3.2 Triple difference

One of the limitations 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 and year, and are correlated with the comparison groups and the likelihood of having 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 aims to provide optimal nutrition for pregnant women and compensate for wage loss during pregnancy by providing a cash transfer to poor mothers, and there was a MAMATA Maternity Scheme implemented in Orissa in 2011 until 2012.

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

To account for such confounders we employ a triple difference approach. Intuitively, for this to work we need to find two comparable groups within the treatment and control groups where the following two conditions are met:

1. The two groups are statistically similar to each other such that there are no factors that can differentially impact them other than through the program.
2. And only one of these two groups can stand to benefit from the program and the other does not.

The triple difference estimator takes the difference over time of these two groups within the treatment and control groups, and then the difference between the treatment and control estimates. By first taking the difference between the additional two groups over time any state specific policy or scheme that shouldn't differentially impact the groups is removed. Then taking the difference of these two groups within the treatment and control groups provides an unbiased estimate of the program.

The randomness of the sex of the firstborn 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 firstborn child is a daughter are more likely to sex select at consequent birth orders than families whose firstborn child is a son in the presence of son preference. In the absence of sex selection, the sex ratio at birth is 104 - 106 boys per 100 girls (Ritchie and Roser, 2019) [15]. Figure 1 shows that the sex ratio at birth for parity 1 given by the solid line closely follows the natural sex ratio at the birth line. The sex ratio at birth for parity 2 and 3 or above diverges away from the natural sex ratio at the 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 differ across observable characteristics. In table 2, we see that the first girl and first boy families are similar on most of the observable covariates. Both of these arguments support the case for natural experiments created by the sex of the firstborn and satify the first condition mentioned above.

Further, while having a firstborn girl or boy does not inherently confer additional benefits for accessing the JSY program, families with firstborn girls may have an incentive to utilize prenatal sex-selection technologies offered through the program for sex-selection (Akbulut-Yuksel and Rosenblum, 2023; Anukriti et al., 2022; Bhalotra and Cochrane, 2010). Therefore, the group with firstborn girls the group potentially benefits from the program in both LPS and HPS states compared to the firstborn boys group and hence satisfies the second condition.

The triple difference estimator is equivalent to taking the difference between two difference-in-difference estimators. (Olden and Møen, 2022). It first takes the difference between first girl and first boy families over time in treatment group and control group separately and then takes the difference if these two differences. Since the first girl families and the first boy families in the treatment and control states experience the same state-specific time varying effects. By doing this the triple difference estimator allows us to account for state-specific confounding effects which we couldn't in our difference in difference estimation.

Like the DID estimation, the DDD estimator also requires a parallel trend assumption for the estimated effect to have a causal interpretation. Although DDD is the difference between two difference-in-differences (difference between first girl and first boy families over time and difference between treatment and control groups over time), it does not require two parallel trends assumption (Olden and Møen, 2022). We discuss this identifying assumption in section 5.1.

We run the following triple difference specification where $Treat \times Post$ interacts with an indicator for first

[15] It could be argued that the costs of raising same-sex children could be lower than raising children of different genders. However, we believe that consumption goods for children are unisex eg medicines, food etc. In a son preferring country, the benefits of having children of different genders (at least one son) outweigh the costs of raising only daughters. Even if daughters could share clothes, parents may have to pay for multiple dowries raising the costs in the long term

Table 2: Balance Test

	First Girl Families			First Boy Families			
	N	Mean	SD	N	Mean	SD	Diff
Hindu	35539	0.76	0.43	38284	0.76	0.43	-0.002
Muslim	35539	0.11	0.31	38284	0.11	0.31	0.002
Forward Caste	35539	0.18	0.38	38284	0.17	0.38	0.003
OBC	35539	0.38	0.49	38284	0.39	0.49	-0.006**
Mother's education	35539	4.47	4.59	38284	4.49	4.58	-0.017
Sex of household head	35539	1.12	0.33	38284	1.12	0.33	0.000
Age of household head	35539	44.16	13.07	38284	44.13	13.08	0.032
Self reported ultrasound use	1771	0.29	0.45	1519	0.23	0.42	0.058***
Poorest	35539	0.22	0.42	38284	0.22	0.41	0.004
Poorer	35539	0.21	0.40	38284	0.21	0.41	-0.001
Middle	35539	0.20	0.40	38284	0.20	0.40	0.004
Richer	35539	0.19	0.39	38284	0.19	0.39	-0.003
Richest	35539	0.18	0.39	38284	0.19	0.39	-0.004
Electricity	35539	0.96	0.94	38284	0.96	0.93	0.002
Truck	35539	0.18	0.99	38284	0.18	0.99	0.000
Fridge	35539	0.32	1.03	38284	0.32	1.02	-0.005
Cycle	35539	0.66	1.03	38284	0.70	1.02	-0.040***
TV	35539	0.66	1.03	38284	0.67	1.02	-0.004
Radio	35539	0.22	1.01	38284	0.22	1.00	0.002

Notes: *p<0.1; **p<0.05;***p<0.01The baseline descriptive statistics are for families with firstborn girl and firstborn boy in rural areas during pre-program years 2000 - 2005. In the last column, we have the coefficients for the regression of the respective variable on the indicator $FirstGirl$. Standard error clustered at the state level. The variable self-reported ultrasound use has missing values hence the number of observations is different than the rest of the variables.

girl families given by $First\_Girl$. Triple difference specification estimated is:

$$\begin{aligned} Girl_{bist} = \beta_0 + \ &\beta_1 Treat_{is} \times Post_t \times First\_Girl_i + \beta_2 Post_t \times First\_Girl_i + \\ &\beta_3 Treat_{is} \times Post_t + \beta_4 First\_Girl_i \times Treat_{is} + \beta_5 Treat_{is} + \\ &\beta_6 Post_t + \beta_7 First\_Girl_i + Stateyear_{st} + \delta_i + \lambda_t + \theta_b + e_{bits} \end{aligned} \tag{2}$$

The DDD coefficient β_1 captures the difference in the likelihood of female births between families with firstborn daughters in the treatment and control group. We include mother-fixed effects, birth order fixed effects, year-of-birth fixed effects and state-year-of-birth fixed effects. Furthermore, we 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 anthropometric welfare outcomes.

4 Results

Table 3 presents the results for the DID estimation and table 4 shows results for our triple difference estimator. In the first column of table 3, the post-program years 2006 to 2015 are compared with the pre program years 2000 to 2005. In the second column, the post program years are divided into a late diffusion period (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) $Treat_{is} \times Post_{II}$.

Columns 1, 3, and 5 of table 3 show that the likelihood of a female birth increased by 4.8 percentage points in the treatment group. This translates to a nearly 10% increase in the number of girls born to mothers in the treatment 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 because it shows a reduction in sex-selective behaviour among the groups that have been known in the literature to sex select i.e. non-SC/ST and non-BPL groups.

The key coefficients of interest are the triple difference estimators. Similar to the DID specification, we first look at the post policy period from 2006-2015 in table 4 in columns 1,3,5,7,and 9 and then we differentiate between the early and late diffusion periods in columns 2,4,6, and 8 of Table 4. We see that the program led to an increase in the likelihood of female births from birth order 2 onwards 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 (column 2). We add state-year fixed effects and state-year trends to our specifications. After including state-year fixed effects this estimate reduces to 10.5 percentage points (column 3) with increases of 9.7 and 15.2 percentage points in the likelihood of female births in the earlier and later diffusion periods (column 4). This is a more conservative specification as it controls for state specific time varying confounders. This suggests that for 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. In columns 7 and 8 we include month of birth fixed effect and in column 9 we additionally include birth month-year specific fixed effects to account for seasonality of births (Boland et al., 2020; Krombholz, 2023).

Though our triple difference estimate suggests increased likelihood of birth of girls, the coefficient on $Post * FirstGirl$ in all our specifications shows that in the control group first girl families after the year 2005 were significantly less likely to have second birth of a girl compared to first boy families before 2005. Overall, our results suggest that an unintentional impact of the program is the reduction in sex selective abortions and an increase in the probability of girls being born, in families eligible for treatment. We also see that most of the positive results are driven by the larger impacts in the later diffusion periods.

Table 3: Main Results: Estimation Results for Difference in Difference Estimation

	(1)	(2)	(3) Dep Var: Girl	(4)	(5)
Treat*Post	0.048**		0.048**		0.048**
	(0.023)		(0.023)		(0.023)
Treat*Post2006-10		0.041**		0.041*	
		(0.020)		(0.021)	
Treat*Post2011-15		0.086**		0.086**	
		(0.036)		(0.038)	
No. of Obs.	150757	150757	150757	150757	150757
Main FE	✓	✓	✓	✓	✓
Season FE			✓	✓	
Season and Year FE					✓

Notes: *p<0.1; **p<0.05;***p<0.01
The table reports the difference-in-difference estimation coefficient of the impact of the 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 our treatment group. $Post$ compares post-program years (2006-2015) to the pre-program years (2000-2005). $Post2006-10$ and $Post2011-15$ are the early (2006-2010) and late diffusion (2011-2015) periods of the program. The main FEs include mother, birth order, and year of birth fixed effects as indicated. The state-year trend is the state-specific time trend and the state-year FE is the State Year specific fixed effect. Season FEs are birth month fixed effects and Season Year FEs are month and year-specific fixed effects. All standard errors are clustered bootstrapped (with 1000 reps) at the state level and reported in parentheses.

Table 4: Main Results: Estimation Results for Triple Difference Estimation

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
					Dep Var: Girl				
Treat*Post*FirstGirl	0.126**		0.105*		0.114**		0.127**		0.126**
	(0.056)		(0.058)		(0.057)		(0.055)		(0.056)
Treat*Post2006-10*FirstGirl		0.116**		0.097*		0.107*		0.116**	
		(0.056)		(0.058)		(0.056)		(0.056)	
Treat*Post2011-15*First_Girl		0.183***		0.152**		0.163**		0.184***	
		(0.069)		(0.069)		(0.067)		(0.070)	
Treat*Post	-0.011		0.000		-0.080		-0.012		-0.013
	(0.050)		(0.000)		(0.053)		(0.051)		(0.053)
Post*FirstGirl	-0.181***		-0.147***		-0.157***		-0.181***		-0.180***
	(0.053)		(0.056)		(0.055)		(0.053)		(0.054)
Treat*Post2006-10		-0.017		0.000		-0.063		-0.018	
		(0.049)		(0.000)		(0.048)		(0.048)	
Treat*Post2011-15		0.014		0.000		-0.079		0.012	
		(0.064)		(0.000)		(0.060)		(0.064)	
No. of Obs.	63250	63250	63204	63204	63250	63250	63232	63232	63250
Main FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
State Year FE			✓	✓					
State Year Trend					✓	✓			
Season FE							✓	✓	
Season Year FE									✓

Notes: *p<0.1; **p<0.05;***p<0.01
The table reports triple difference estimation coefficient of the impact of the JSY on the likelihood of observing the child born to first girl families is a girl. $Treat$ is the dummy variable that takes the value 1 if the mother is from our treatment group. Similarly, $FirstGirl$ is an indicator for if the woman's firstborn child was a girl. $Post$ compares post program years (2006-2015) to the pre-program years (2000-2005). $Post2006-10$ and $Post2011-15$ are the early (2006-2010) and late diffusion (2011-2015) periods of the program. The main FEs include mother, birth order, and year of birth fixed effects as indicated. The state-year trend is the state specific time trend and the state year FE is the State Year specific fixed effect. Season FEs are birth month fixed effects and Season Year FEs are birth month and year specific fixed effects. All standard errors are clustered bootstrapped (with 1000 reps) at the state level and reported in parentheses.

5 Robustness tests

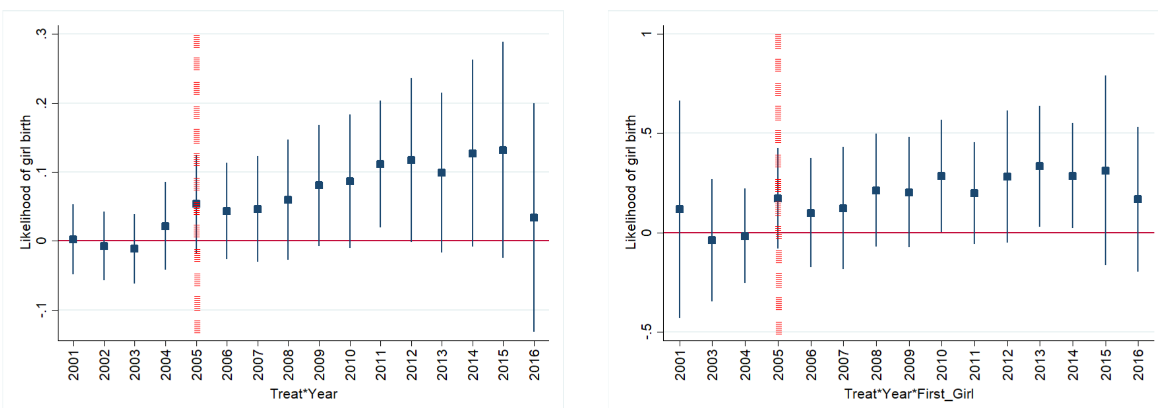
5.1 Identification assumption

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 groups. For validity of our analysis, the probability of having a girl at the next birth should not be significantly different across mothers in the treatment and control groups during the pre-program years. To test this we run a specification where the effect of the program is allowed to vary by year, as in an event study analysis. This approach is recommended and widely used for detecting pretrends (Roth et al., 2023). For us to be confident that the program had a causal impact on the sex-selective behaviour mothers, 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 the program has any effect on sex selective abortions. To check this, we estimate the following specification for a DID and a DDD:

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

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

Figure 2: Test for parallel trends



(a) Difference in Difference (b) Triple Difference

Panel (a) plots estimated difference in the likelihood of girl births between treatment and control groups, conditional on mother fixed effects. Panel (b) plots the estimated differences in the likelihood of girl births to mothers with first girls in treatment groups with their counterparts in the control group. The dashed red line represents the year of the JSY program. The joint test of the significance of the lead years of the program yielded a F-Statistic of 1.26 and 1.74 respectively, implying that before the program, the number of girl births evolved similarly in the two groups.

Figure 2a shows 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 2b shows the likelihood of giving birth to a girl is not significantly different for first-girl families between the treatment and control groups. Conditional on mother fixed effects, we find no significant differences in the likelihood of birth of girls between the treatment and control groups before program implementation in 2005. The differences in outcomes become significant only after 2009. The joint test of the significance of the lead years of the program yielded p-value of 0.3 and 0.163, implying that prior to the program, the number of girl births evolved similarly in the two groups.

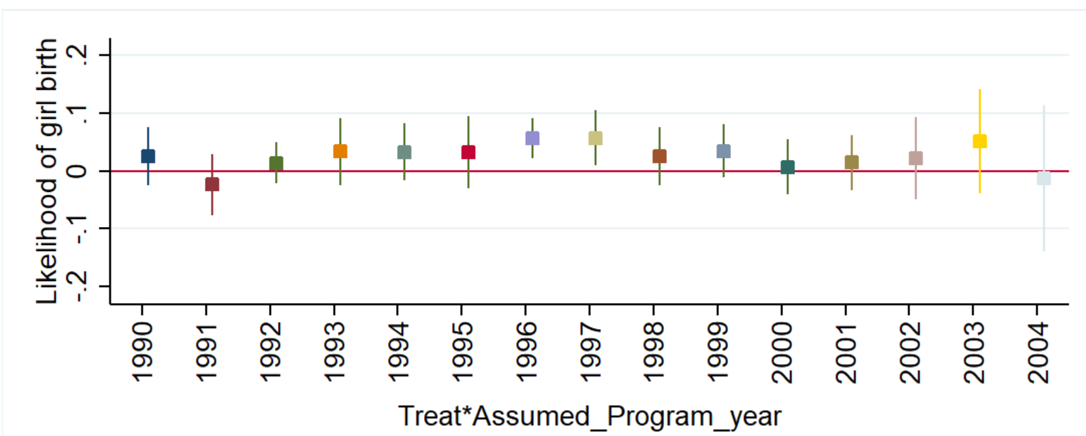
While testing for pretrends alone is not sufficient we also support our results with the placebo tests discussed in the next subsection.

Another factor that could bias our results and threaten our identification strategy is whether the implementation of the JSY was anticipated before before 2005. We would then be conflating our estimate with the households' expectations. If this were the case then households in the treatment group should have changed their fertility behaviour prior to 2005 and we should see a decrease in female births. However, if households in the treatment group did not change their behaviour prior to 2005 differently than did households in the control group i.e., the probability of female births was similar in both groups prior to 2005, we can say that households did not anticipate the programme and that the year of implementation was exogenous. In figures 2, we show that the difference in the number of female births prior to 2005 was not significant, indicating that households did not anticipate the program and change their fertility behavior.

5.2 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 involves individual assumptions for each year from 1990 to 2004, i.e. years prior to 2005, to be the program year. Assuming that each of the years was the year when the JSY was implemented, we checked the impact of the program on the likelihood of girl births across treatment and control mothers. Figure 3 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 significant difference in these two years could be due to the structural break in 1995 when ultrasound technology became widely available in India (Bhalotra and Cochrane, 2010). However, the effect of this structural break did not last long and dissipated after 1997 as can be seen in figure 3. The coefficients for the remaining years are not significant and the differences in the outcome only appear after 2005 i.e after the JSY was implemented, suggesting that what we are capturing is the causal effect of the JSY.

Figure 3: 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 checks if the likelihood of female birth is different across the treatment and control group. All regressions contain mother, birth and year fixed effects. Standard errors are clustered at the state level. The F-statistics of the pre-treatment years jointly is 1.04.

The second falsification test is to run our triple DID specification on DHS-2005/06. Since this survey was completed by 2005-06, the women interviewed in this sample never received the program. This 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 the 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 made 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 between 2001 and 2005 (our late diffusion period) with those born between 1990 and 1995. One reason for 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 any DHS sample. Hence, if our main results are driven by reporting bias, then we will also see significant differences in the outcome of our DHS-III estimation results.

We estimate the following specification for DHS-III:

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

Table 5 shows the results of our falsification test on mothers whose fertility decisions were made in 1990.[16] 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 control groups. A lack of significance will indicate that our empirical strategy is to capture only the program effect.

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

Table 5: 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.

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

6 Discussion and Additional Evidence

The previous section described the causal impact of the JSY on sex-selective abortions in India. The program caused an increase in the number of girls born to families eligible to receive the 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 higher birth orders. In this section, we therefore test the hypothesis that girls born in families with son preference will be worse off.

6.1 Impact on infant mortality

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

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

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

The results in table 6 and 7 show whether there are disproportionately more girls among infants who died in their first year or before reaching five years of age. For each of these samples, the first two columns show the results for all infants in rural India irrespective of their birth order. Columns 3 and 4 show the 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 make 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 deceased child is a girl is positive for all birth orders. For girls born at a parity greater than 1, the likelihood of a girl dying is 2 percentage points greater in the treatment group. This is more prominent in the earlier diffusion period and for girls born at a parity greater than 2. The likelihood of a girl dying before reaching the age of 5 is nearly 6 percentage points greater 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 were added to the program in 2011.

7 Mechanisms

7.1 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 such as ultrasounds. All program beneficiaries were expected to undergo three ante-natal checkups that included ultrasound scans. Although discovering the gender of the foetus was not the purpose of the scans, parents might use this information and abort unwanted female foetuses. Since we cannot observe who uses the technology to determine the sex of the foetus and who uses it to satisfy the programme condition, we can hypothesize that if more people were using this aspect of the programme 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 obtained information on which mothers reported having used ultrasound technology. Column 2 in table 8 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 at year t, the mother

Table 6: 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	0.007		0.020		0.042	
	(0.007)		(0.013)		(0.030)	
Treat*Post2006-10*Girl		0.009		0.025**		0.071**
		(0.007)		(0.012)		(0.033)
Treat*Post2011-15*Girl		0.005		0.017		0.009
		(0.008)		(0.018)		(0.038)
No. of Obs.	150757	150757	63250	63250	23275	23275
Main FE	✓	✓	✓	✓	✓	✓

Notes: *p<0.1; **p<0.05;***p<0.01
The table reports the mortality outcomes for children below age 1. Columns 1 and 2 record the likelihood of girls dying before reaching age 1.Columns 3 and 4 record the likelihood of girls born at parity 2 and above dying before reaching age 1. Columns 5 and 6 record the likelihood of girls born at parity 3 and above dying before reaching age 1. 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). Post2006-10 and Post2011-15 are the early (2006-2010) and late diffusion (2011-2015) periods of the program. The main FEs include mother, birth and year fixed effects. Standard errors in parentheses are clustered bootstrapped (1000 reps) at the state level.

used an ultrasound, and 0 otherwise. One thing to note here is that the reported use of ultrasound technology was only asked for 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. This implies that the results are only suggestive of the trend rather than a causal implication of the program. The results in column 2 show that there was no significant difference in the likelihood of girl births resulting from the use of ultrasound between the treatment and control groups.

It is important to note that the measure of reported ultrasound usage has many missing values which create a bias in our estimation. Further, this result is mostly descriptive, as reported values are prone to measurement errors and may be biased as respondents may not want to be identified as using ultrasound due to the fear of legal punishment. We, therefore, compute an indicator of the likelihood of ultrasound use by a mother based on use by her neighbours (excluding her own use).[17] 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 the 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 determine the likelihood of use for all eligible mothers in the neighbourhood. Using this indicator instead of reported values does not completely eliminate the bias, but it provides a better understanding of how ultrasound access might impact decisions. Considering the sample of mothers who gave birth in the last five years, from 2010 onwards, this indicator is constructed as follows:

$$Likelihood_{cip}^{Ultrasound} = 1 - \left(\frac{(\sum_{c=1}^{C} B_{cip}^{U}) - B_{cip}^{U}}{\sum_{c=1}^{C} B_{cip}} \right) \tag{7}$$

Term B_{cip}^{U} indicates whether for the birth of child c to mother i in PSU p ultrasound (U) had ever been used. The numerator captures the use of ultrasound in the neighbourhood, excluding own mother's use and

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

Table 7: 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	0.005		0.013			0.058*
	(0.007)		(0.014)			(0.034)
Treat*Post2006-10*Girl		0.008		0.020	0.091**	
		(0.007)		(0.015)	(0.036)	
Treat*Post2011-15*Girl		0.002		0.009	0.018	
		(0.009)		(0.018)	(0.039)	
No. of Obs.	150757	150757	63250	63250	23275	23275
Main FE	✓	✓	✓	✓	✓	✓

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 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). Post2006-10 and Post2011-15 are the early (2006-2010) and late diffusion (2011-2015) periods of the program. The main FEs include mother, birth and year fixed effects. Standard errors in parentheses are clustered bootstrapped (1000 reps) at the state level.

$\sum_{c=1}^{C} B_{cp}$ captures all the births that occur in a PSU with or without ultrasound. Using this indicator, we are able to generate a likelihood estimate for all eligible women in the sample irrespective of whether they reported having an ultrasound. Column 1 of Table 8 shows the likelihood of use for the treatment and control populations. We can see that there are no significant differences in the sex of the children born in these two groups as a result of ultrasound use.

Table 8: Mechanism: Ultrasound

	(1)	(2)
	Dep Var: Girl	
$Treat * Likelihood^{Ultrasound}$	0.138	
	(0.322)	
Treat*Reported_Ultrasound		-0.017
		(0.035)
$Likelihood^{Ultrasound}$	-1.353***	
	(0.207)	
Reported_Ultrasound		-0.053*
		(0.030)
No. of Obs.	64248	40240
Main FE	✓	✓

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 onwards. 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). The main FEs include mother, birth order and year of birth fixed effects. Column 1 reports results using likelihood of ultrasound and column 2 reports results from reported ultrasound usage. Standard errors in parentheses are clustered Bootstrapped (1000 reps) at the state level.

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

7.2 Cash Transfer Channel

Wealth Effect

The program provided women with cash benefits for every live birth delivered at a public or private health centre. This one year payment reduced the cost of childbearing. 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 the 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-selection.

Table 9 shows the results of the interaction of wealth quintiles with the indicator of being in the treatment group and post program years. The results show that the likelihood of girl births occurring at subsequent birth orders does not differ by wealth across the treatment and control groups after 2005. We can therefore conclude that the program did not lead to parents bearing girls for the cash incentive.

Table 9: Mechanism: Wealth Effect

	(1)
	Dep Var: Girl
Treat*Post*Poorest	0.021
	(0.057)
Treat*Post*Poorer	-0.007
	(0.056)
Treat*Post*Middle	-0.011
	(0.048)
Treat*Post*Richer	0.016
	(0.036)
No. of Obs.	150757
Main FE	✓

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 richest category given in the DHS data. Treat is the dummy variable 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). The main FEs include mother and year fixed effects. Standard errors in parentheses are clustered bootstrapped (1000 reps) at the state level.

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 reducing health and human capital investments in children, increasing dowry deaths among women and marrying daughters to distant households (Rose, 1999; Sekhri and Storeygard, 2014). Under the program, households could receive a substantial one-time cash payment which was nearly double the monthly per capita consumption expenditure. Hence, we want to determine whether in response to a weather shock and given the availability of a cash transfer under the program, parents would 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 the Climate Hazards Group InfraRed Precipitation with Station data (CHIRPS) monthly rainfall data for the period 2000 to

2015.[18] As the main agricultural season in India is monsoons (July - September) and the majority of Indian agriculture depends on rainfall during these months, we construct rainfall shocks for each year as below mean, one, or two standard deviations below the long term 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 10, we record the results of shock interacted with Treat and post indicator. In Columns 1 (2 and 3), we say parents faced a rainfall shock if they were residents in the district where recorded rainfall for the given year was 0 (1 and 2) standard deviations below and 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.

Table 10: Mechanism: Income Effect

	(1)	(2)	(3)
		Dep Var: Girl	
Treat*Post*Rain_Shock	-0.001	0.001	0.049
	(0.022)	(0.025)	(0.049)
Treat*Post	0.050	0.049**	0.045*
	(0.032)	(0.024)	(0.024)
Treat*Rain_Shock	0.005	0.008	-0.059
	(0.018)	(0.025)	(0.043)
Post*Rain_Shock	0.003	0.016	-0.026
	(0.017)	(0.018)	(0.042)
No. of Obs.	149855	149855	149855
Main FE	✓	✓	✓
Rainfall below	Mean	1 SD	2 SD

Notes: *p<0.1; **p<0.05;***p<0.01
The table reports coefficients of an income shock represented by rainfall below long run mean on the likelihood of observing the child born to be a girl. 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 is the dummy variable 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). The main FEs include mother, birth order and year fixed effects. $Rainfall_shock$ is a dummy variable if the rainfall was below long run mean in column 1, below 1 standard deviation in columns 2; and below 2 standard deviations in column 3. Standard errors in parentheses are clustered bootstrapped (1000 resp) at the state level.

[18] 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/

7.3 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 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 was paid after a beneficiary's delivery in a health care facility. This gave them the 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 financial incentives or to the records of pregnancies they maintain. Hence we combined 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 the JSY incentives 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 the births of additional girls in the treatment groups. It is important to note here that since our data on ASHA workers is only available after 2008, our estimates cannot not be interpreted as causal effect of ASHA workers. Rather they provide suggestive evidence on the possible effect of this channel on our main outcome. In table 11, 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 centres will be lower than public health centres, we run regressions separately for health workers receiving benefits for public and private hospital deliveries. In column 1 (2), we interact the treatment variable with the number of health workers receiving incentives for deliveries in public (private) institutions. The number of health workers is scaled by 10000 women in the district.

Table 11: Mechanism: Health Workers Effect

	Dep Var: Girl	
	(1) Public	(2) Private
Treat*Health_Worker	0.015*	-0.011
	(0.008)	(0.016)
Health_Worker	-0.009	0.003
	(0.006)	(0.013)
No. of Obs.	56614	56614
FE	✓	✓

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. Main FE contains mother, birth order and year fixed effects. Standard errors are clustered bootstrapped (1000 reps) at the state level.

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

7.4 Net Effect on Missing Women

This paper has so far shown that the JSY led to an increase in the number of girls born but also 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 a methodology similar to that used by Anderson and Ray (2010) and Anukriti et al. (2022). We first compute an estimate of the 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 the population of SC and ST) as shown below:[19]

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

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

[19]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 of 2001 does not contain information on caste for different age groups.

$$\text{Missing Women} = [\text{Excess Births} \; - \text{Excess Deaths} \;] \times Population_{0-4years} \tag{8}$$

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 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 5 in appendix A.2, most of the improvement in missing women comes from additional births of girls due to the program[20].

8 Conclusion and Policy Recommendations

This paper examined the impact of the JSY conditional cash transfer program on the fertility decisions of mothers in rural India. More specifically, this study provides causal evidence of the impact of the JSY on sex-selective behaviour among Indian households. The 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 the 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. Although overall in the country, there has been an increase in the prevalence of sex-selective abortions, the JSY managed to reduce this practice among families who qualified for the program.

The results also showed 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. These findings indicate that though there are improvements in birth outcomes for girls as a result of the program, it may be the case that discrimination against them continues and shifts from prenatal to postnatal discrimination.

Our results show that in the 0-4 age group, 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 institutions, they act as deterrents for couples to selectively abort 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 caution as we do not claim that the intervention of health workers shifted parental son preference in India. Our results point to the fact that parental son preference was merely substituted by postnatal excess girl mortality between the ages of 24 and 59 months.

[20] 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.

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A Appendix

A.1 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 the household's preference for girls. One such policy is the Hindu Succession Act of 1956, which came into force in September 2005. This act allows women to inherit the property of their fathers and have legal rights on the 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 the 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 are 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 otherwise. 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 the inclusion of the inheritance law dummy does not change our results and is in fact not significant in the regression. Our main coefficients also do not change with the inclusion of this variable.

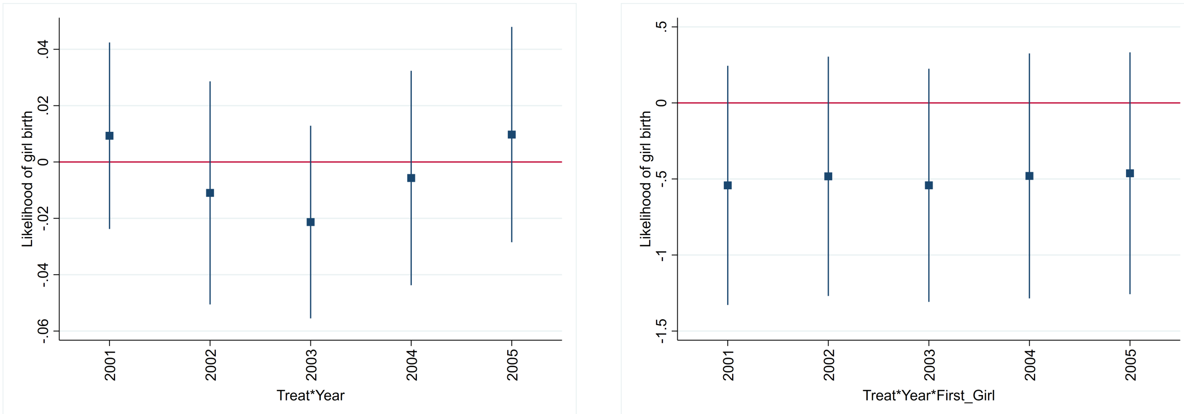
Table A.1: Main Results: Estimation Results for Triple Difference with Inheritance Law

	(1)	(2)	(3)	(4)
			Dep Var: Girl	
Treat*Post*First_Girl	0.114**			0.126**
	(0.054)			(0.056)
Inheritance Law	0.017	0.018		0.025
	(0.092)	(0.093)		(0.106)
Treat*Post2006-10*First_Girl		0.107**	0.104*	
		(0.054)	(0.057)	
Treat*Post2011-15*First_Girl		0.162**	0.160**	
		(0.065)	(0.068)	
Inheritance Law*First_Girl			0.019	
			(0.025)	
No. of Obs.	63250	63250	63250	63249
Main FE	✓	✓	✓	✓
State Year Trend	✓	✓	✓	
Season Year FE				✓

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, FirstGirl 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). Post2006-10 and Post2011-15 are the early (2006-2010) and late diffusion (2011-2015) periods of the program. The main FEs include mother, birth and year fixed effects. State Year Trend is the state specific time trend and State Year FE is the State Year specific fixed effect. Season Year FE is birth month and year fixed effects All triple difference estimates are for the sex of the child born at birth order 2 or higher. Standard errors in parentheses are clustered bootstrapped (1000 reps) at the state level.

A.2 Additional Figures

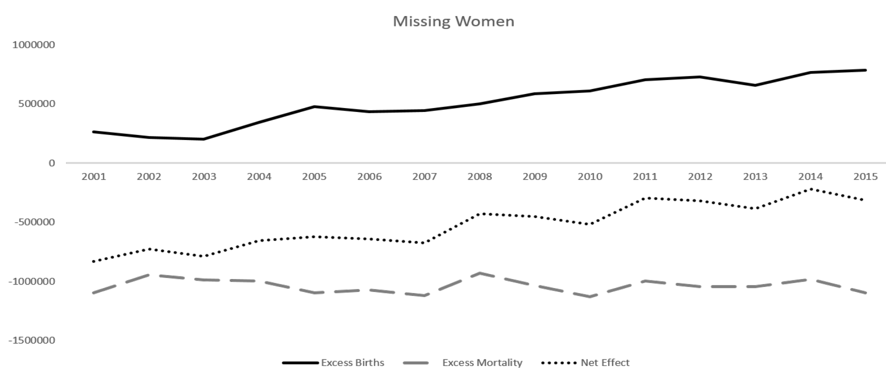
Figure 4: Parallel Trends



(a) Treatment and Control

(b) First girl and First boy families

Figure 5: 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.

A.3 Additional Tables

Table 13: Robustness: Unmet Need for Contraception

	(1) Unmet need for Contraception
Treat*First_Girl	-0.005
	(0.007)
First_Girl	-0.020***
	(0.006)
No. of Obs.	523412
District FE	✓

Notes: *p<0.1; **p<0.05;***p<0.01
The table reports coefficients for proxy for supply side differences. 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. Dependent variable is unmet need for contraception which is women who do not want more children or get pregnant but are not using any form of contraception or family planning. District FE are district level fixed effects. Standard errors in parentheses are clustered bootstrapped (1000 reps) at the state level.

Table 14: Mechanism: Ultrasound with first girl families

	(1) Dep Var:	(2) Girl
Treat* FirstGirl*$Likelihood^{Ultrasound}$	-0.237	
	(0.312)	
Treat*FirstGirl*Reported_Ultrasound		0.046
		(0.117)
$Likelihood^{Ultrasound}$	-0.276	
	(0.291)	
Treat*$Likelihood^{Ultrasound}$	0.255	
	(0.363)	
FirstGirl*$Likelihood^{Ultrasound}$	-1.546***	
	(0.281)	
Reported_Ultrasound		-0.001
		(0.106)
Treat*Reported_Ultrasound		-0.033
		(0.111)
FirstGirl*Reported_Ultrasound		-0.106
		(0.113)
No. of Obs.	64248	18835
Main FE	✓	✓

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 onwards. Treat is the dummy variable that takes the value 0 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). Post2006-10 and Post2011-15 are the early (2006-2010) and late diffusion (2011-2015) periods of the program. Main FE contains mother, birth and year fixed effects. All estimates with FirstGirl are for the sex of the child born at birth order 2 or higher. Column 1 reports results using likelihood of ultrasound and column 2 reports results from reported ultrasound usage. Standard errors in parentheses are clustered at the state level.

Table 15: Robustness: Urban Sample

	Full	Rural	Urban	Full Dep Var: Girl	Rural	Urban	All mothers
Treat*Post*FirstGirl				0.097*** (0.034)	0.105* (0.059)	0.077 (0.063)	
Treat*Post	0.039** (0.020)	0.048** (0.023)	0.029 (0.020)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.044** (0.021)
Post*FirstGirl				-0.147*** (0.030)	-0.147** (0.058)	-0.146*** (0.048)	
No. of Obs.	223283	150757	72525	86631	63204	23342	192112

Notes: *p<0.1; **p<0.05;***p<0.01

The table reports coefficients of the difference in difference and triple difference estimators 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 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). Main FE contains mother, birth and year fixed effects. State Year FE includes State year fixed effects. All triple difference estimates are for the sex of the child born at birth order 2 or higher. Column 1 and 4 report results for full sample that includes all urban and rural regions. Column 2 and 5 report results for rural sample and columns 3 and 6 report results only for the urban sample. Column 7 reports the difference in difference estimation including rural women who statrted their fertility before the year 2000. Standard errors in parentheses are clustered bootstrapped (1000 reps) at the state level.