National Cash Transfer, Deteriorated Aggregate Sex Ratio and Neonatal Mortality in India

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Abstract

The national conditional cash transfer program (JSY) incentivizes women to use institutional delivery and antenatal care to improve maternal and neonatal mortality in India. We test the hypothesis if this additional exposure to ultrasound scan deteriorated the sex ratio because of son preference culture. We use the staggered adoption design with randomization inference to exploit the incremental rollout feature of this national program. We find that JSY has significantly reduced the proportion of girls born. In addition, the improvement in neonatal mortality is only significant for boys. We highlight that, with son preference culture, cash transfer programs aimed at improving health-related behavior with ultrasound scans may change the aggregate sex ratio which might affect the marriage market and social stratification in the long run.

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Keywords: conditional cash transfer, sex ratio, ultrasound scan, neonatal mortality, India.

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1.Introduction

High maternal mortality has long been a public health issue in India. In 2017, India has the second highest amount of maternal death in 2017, accounting for 12 percent of global maternal death[2]. In response, the Indian central government has launched the Janani Suraksha Yojana (JSY) program in 2005, the world's largest conditional cash transfer program to pay for women to give birth at registered health institutions to reduce mortality of both mother and infant.

In addition, JSY encouraged pregnant women to take up three antenatal cares. Specifically, health assistants were hired to personally incentivise pregnant women to take up antenatal visits and to help them plan expected dates for these visits. Besides, vouchers were supplied to cover any logistical expenses, because transportation costs were previously identified as a major barrier to visiting antenatal care. In some cases, pregnant women were additionally paid for visiting antenatal care.

However, it may be worrying to see a national promotion in India to increase exposure to ultrasound scan which Arnold et al. (2002) find to have contributed to the sharp decline in the share of female infants in India, by enabling in-utero sex determination and inducing more female fetus abortion. Therefore, JSY may have unintendedly deteriorated the sex ratio, by subsidizing the households who would otherwise be constrained by financial cost or information, to access ultrasound scan to determine the sex of the foetus.

The answer is urgently required, as more efforts to promote universal access to antenatal care is expected over the globe. In India, health promotion to access antenatal check-ups is highly praised by World Health Organisation (WHO) in 2018, and further identified as a strategic means to reach WHO target of a maternal mortality ratio below 70 by 2030 in India[3], Furthermore, to reduce global child mortality, United Nations has recently called global effort to reaching high coverage of quality antenatal care, jointly supported by UNICEF, the World

[2] A joint estimate by WHO, UNICEF, UNFPA, the World Bank Group and the United Nations Population Division (UNPD). https://www.who.int/reproductivehealth/publications/maternal-mortality-2000-2017/en/

[3] https://www.who.int/southeastasia/news/detail/10-06-2018-india-has-achieved-groundbreaking-success-in-reducing-maternal-mortality

Health Organization (WHO), the World Bank Group and the Population Division of the United Nations Department of Economic and Social Affairs (UN DESA) [4].

Therefore, this paper aims to understand whether promotion to universal access to antenatal care services with ultrasound scan is harmful in areas where son preference is strong is required, by skewing the sex child ratio. We exploit the incremental roll-out feature of the program. JSY scheme was announced in 2005 by the Indian central government and the adoption of JSY was compulsory for all districts. While the program was financed entirely by central government budget, it was implemented at district level. In term of the rollout, it is state authority who made the decision over which districts to launce JSY earlier and which districts later.

We follow Athey and Imbens (2018) to use a standard difference-in-differences estimator. Importantly, we take a design-based perspective where the stochastic nature and properties of the estimators arises from the stochastic nature of the assignment of the treatments. In potential outcome framework, we consider potential sex child ratio to be deterministic, and realised sex child ratio (what we observe) to be stochastic, as randomisation occur at adoption time. Under the crucial assumption that adoption year is random across districts in the same state, our estimates are be interpreted as causal effect, which is an average of different timing causal effect, including the effect of changing from never adopting to adopting in the first period, or changing from never adopting to adopting later.

Despite crucial assumption being untestable, the common trend assumption follows the crucial assumption, as a necessary condition (Athey and Imbens, 2018). We first show that there is no pre-trend in changes in sex ratio before the policy implementation and also we show that timing of program adoption is uncorrelated with changes in outcomes prior to policy implementation.

We find that JSY has significant reduced the share of girls born, especially among girls who are not the first child. Second, we find that neonatal mortality is declined only for boys but not for girls. The result is robust when we use randomisation inference or take into account multiple hypotheses testing.

[4] https://www.un.org/development/desa/pd/sites/www.un.org.development.desa.pd/files/unpd_2020_levels-and-trends-in-child-mortality-igme-.pdf

This paper contributes in 3 ways. First, it contributes to the understanding of declining in sex ratio in recent decades in India and concluded that JSY, the national health cash transfer program, has further deteriorated the skewed sex ratio. Second, it contributed to the current research stream to evaluate JSY, finding unintended consequence. Lastly and most importantly, it provides robust result for an urgent call to modify current policy design to be more sensitive to local son preference culture, in the face of forthcoming global promotion to universal access to antenatal care service. The result is worrying because sex imbalance is found to have long-lasting negative impacts on the marriage market (Angrist, 2002), crime (Edlund et al., 2013), social stratification, and welfare more generally (Bhaskar, 2010; Edlund, 1999).

This paper is organised as follows. Section 2 documents institutional details. Second 3 presents the data, sample, and variable construction. Section 4 explains the methodology and assumption. Second 5 presents the main result. Second 6 shows robustness check. Second 7 concludes.

2. Institutional Details

2.1 maternal and child mortality in India and JSY

Every year, India accounts for a considerable number of deaths of both mothers and infants. For maternal death, although India has reduced the mortality rate by 77 percent from 1990 to 2016[5], this country still has the second highest amount of maternal death in 2017, accounting for 12 percent of global maternal death[6]. For infant mortality, India and Nigeria account for nearly a third of global infant deaths in 2019, although the under-five mortality (deaths per 1,000 live births) in India has already declined in India to 34 in 2019 from 126 in 1990[7].

It is acknowledged that almost half of these deaths can be prevented by reaching high coverage of quality antenatal care, skilled care at birth, and postnatal care for mother and baby (UN,

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[5] https://www.who.int/southeastasia/news/detail/10-06-2018-india-has-achieved-groundbreaking-success-in-reducing-maternal-mortality

[6] A joint estimate by WHO, UNICEF, UNFPA, the World Bank Group and the United Nations Population Division (UNPD). https://www.who.int/reproductivehealth/publications/maternal-mortality-2000-2017/en/

[7] The mortality estimates released by UNICEF, the World Health Organization (WHO), the Population Division of the United Nations Department of Economic and Social Affairs and the World Bank Group. https://www.un.org/development/desa/pd/sites/www.un.org.development.desa.pd/files/unpd_2020_levels-and-trends-in-child-mortality-igme-.pdf

2020). Following similar guidance to reduce mortality rate by promoting universal access to institutional health services, the central government in India introduced a large-scale conditional cash transfer programme – the Janani Suraksha Yojana scheme (JSY henceforth) in 2005, to use cash incentive to promote institutional delivery, antenatal and postnatal care.

First, for women who give birth in registered health institutions, JSY pays substantial cash incentives – around 28 times the average rural daily wage for casual labour. Furthermore, JSY encourages them to attend at least three antenatal care visits. First, health assistants were hired in some districts[8] to personally incentivise pregnant women to attend antenatal care visits and also to help them plan expected dates for these visits. Second, vouchers were supplied to cover any logistical expenses for both pregnant women and health assistants, because transportation costs have been identified as a major barrier to antenatal care visits (Titaley et al., 2010). In some cases, beneficiaries received payments for registering for JSY and for antenatal care visits (Commissioner of Family Welfare, AP, 2010). Despite ultimately implemented at district level, JSY is entirely financed by Indian central government budget[9].

2.2 JSY rollout

Across the whole country, the adoption of JSY was compulsory for all districts which constitute the smallest local authority in India[10], at which JSY was ultimately implemented. However, in terms of the rollout across districts within the state, it is the state-level authority who made the decision over which districts to implement JSY first and which districts later. Figure 1 shows the JSY rollout across India.

Hence, it is reasonable to suspect that districts which adopted JSY at different time to have different level of capacity of district authority. For instance, districts chosen by state authority to implement JSY in 2005, same year when central government announced the scheme, may be these districts with the largest local capacity to guarantee a successful implementation, whereas other districts without such capacity may have been given more time to prepare.

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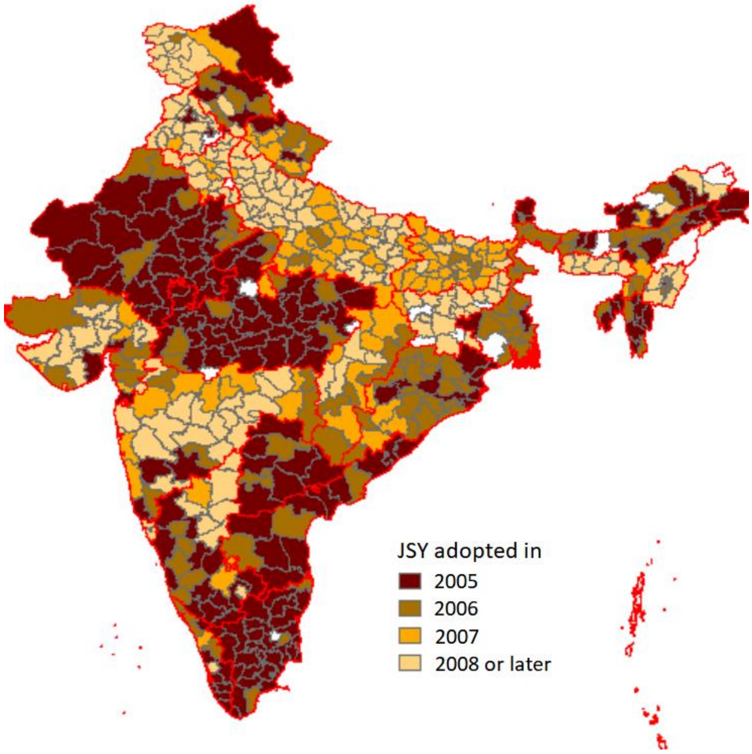
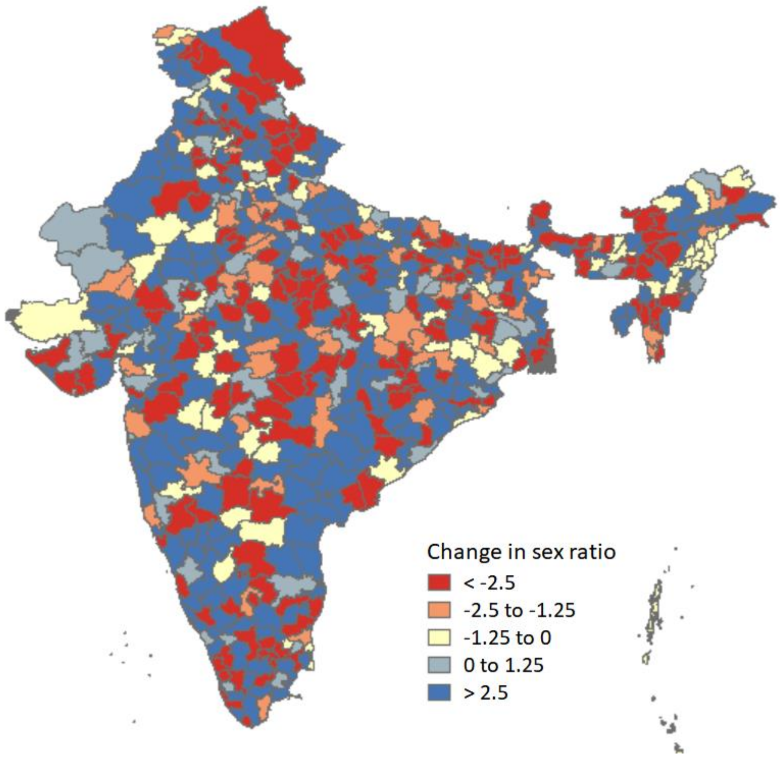
[8] Health assistant were hired only in low performing states and north-east states and in tribal districts of all states (World Bank, 2009). After registration to JSY, health assistants draw up a micro-birth plan for each expecting mother, which details, among other things, the expected dates for antenatal visits, delivery and post-partum care.
[9] https://www.nhp.gov.in/janani-suraksha-yojana-jsy-_pg (accessed 06Sep2020).
[10] In 2005, the number of districts in India was 594.

Figure 1. JSY Adoption in India



Source: Authors' calculation using DLHS 3, India.

Figure 2. Trend in Sex Ratio in India



Source: Authors' calculation using DLHS 2, India.

Therefore, for studies comparing the outcome measure between districts having adopted JSY and those having not, results are contaminated by differences at district level. For this reason, Lim et al. (2010) which uses matching method to evaluate the impact of JSY on mortality is criticised for the credibility of their results. Figure 2 shows the changes in sex ratio before JSY implementation across India.

For other more rigorous study tries to use difference-in-differences methods which requires a weaker assumption that trend in outcome measure must be parallel between districts having adopted JSY and those having not. In other words, the level of mortality can be different between the JSY-adopted districts and others, but the trend of mortality must be the same. For outcome measures in institutional delivery, perinatal mortality and vacations, Andrew and Vera-Hernández (2020) do not find any evidence that districts where JSY rolled out at different time were on different trends prior to the program.

2.3 JSY effect

First, most research find that JSY leads to increased use of institutional delivery (Lim et al., 2010; Ng et al., 2014; Powell-Jackson et al.,2015; Andrew and Vera-Hernández, 2020) and increased take-up of antenatal care (Lim et al., 2010; Powell-Jackson et al., 2015).

However, JSY is found to have no overall effect on mortality. Most research concludes that no effect of JSY is detected on mortality of mothers (Lim et al., 2010; Ng et al. (2014) or of infants (Powell-Jackson et al., 2015), with one exception which however has been criticised for using method with implausible assumption[11].

In addition, Powell-Jackson et al. (2015) find that JSY has resulted in more pregnancies. Therefore, later in our specification, we need to control for pregnant population change induced by JSY, so our result is not confounded by a changed mother population.

Andrew and Vera-Hernández (2022) further criticises JSY that, as a demand-side policy, it has been harmful in areas where health system capacity was low. Specifically, they find that,

[11]Lim et al. (2010) finds reduced perinatal and neonatal death by JSY but is criticised for the credibility of their result, as the assumption of their matching methods is unlikely to hold. The result is likely driven by unobserved local-level factors between districts. The violation is very likely as JSY adopted districts and others are significantly different, as found by Andrew and Vera-Hernández (2022).

as JSY doubled the number of deliveries for which the average facility was responsible, JSY increased perinatal mortality, in areas where health institution capacity is below-median. In addition, they also conclude that JSY caused adverse effects spilled over onto lower rates of childhood vaccinations, suggesting a diversion of resources from routine services.

Previous findings on JSY have been disappointing. Furthermore, it is worrying to see such a national promotion to universal access to antenatal care service of which ultrasound scan is a standard component. The use of ultrasound scan is found to have significantly contributed to the sharp decline in the sex child ratio in India in recent decades (Arnold et al, 2002), by having induced more abortion for female fetus. This is because ultrasound scan reveals the sex of fetus for Indian households who strongly prefer sons to daughters.

2.4 son preference in India

Son preference in India is well-documented. For children under six, the country's most recent Census (2011) reports a ratio of 914 girls to 1,000 boys,[12] which lies considerably below the natural rate (see Bhaskar and Gupta, 2007; for an overview). Such aggregate sex ratio is a reflection of preference and decisions made at family level where households strongly prefer sons to daughters. Pande and Malhotra (2006) find that only 2.5 percent of women in rural households want more girls than boys in a national survey, whereas over 45 percent of women want more boys than girls. In addition, they find that the son preference does not vary alongside economic development of villages or wealth of households. They find that village-level economic development variables show no statistically significant relationship with son preference.

This finding is important for our crucial assumption that JSY adoption timing within state is random in terms of potential sex ratio, explained in detail in Section 4. As Pande and Malhotra (2006) find that son preference is independent of village-level economic development and household wealth. It is plausible that sex ratio, as an aggregate reflection of son preference at

[12] The Census only provides sex ratios for the age group of 0 to 6 year olds (Indian Census, 2011). The Sample Registration System (2012) reports that the sex ratio at Birth for India increased from 906 in 2009-11 to 908 in 2010-12. Chhattisgarh has reported the highest Sex Ratio at Birth (979); Haryana, the lowest (857). See Figure 1.

household level, is independent of local economic development, whereas the adoption timing of JSY is determined by local authority capacity contributed by local economic development.

Second, Pande and Malhotra (2006) find that at household level, maternal education is the single significant predictor for son preference. As Powell-Jackson et al. (2015) find that JSY has resulted in more pregnancies and JSY subsidies to give birth so more pregnancies occurred to household who otherwise be financially constrained. Therefore, we expect that JSY leads to pregnant cohorts with lower maternal education and stronger son preference, assuming that maternal education and household wealth is positively correlated.

Controlling for maternal education first ensures that our result is not confounded by a changed pregnant cohort, induced by JSY incentives, with stronger son preference. Second, it also makes our crucial assumption more plausible that JSY adoption timing across districts is random on sex ratio, after controlling for average maternal education of pregnant cohort.

Such persistent preference that sons are valued more than daughters is partly driven by economic reasons and social norms.

First of all, daughters are considered to drain family wealth, because a transfer of wealth is required at the marriage from the daughter’s parents to her husband. Second, only sons are expected by parents to expand labour force, wealth and property of their household, as a daughter will move into her husband’s household, contributing her lifetime adult labour to the production of her husband’s household. Lastly, living with his parents and wife, only son is expected by parents to provide financial and social insurance when they are old, instead of their daughter who has moved out of their household since her marriage.

Consequently, preference and decisions made by families result in the aggregate sex gap that men outnumber women by 4% in Indian population in 2019[13]. Such imbalance is found to have long-lasting negative impacts on the marriage market (Angrist, 2002), crime (Edlund et al., 2013), social stratification, and welfare more generally (Bhaskar, 2010; Edlund, 1999).

[13] World Bank staff estimates based on age/sex distributions of United Nations Population Division's World Population. https://data.worldbank.org/indicator/SP.POP.TOTL.FE.ZS?most_recent_year_desc=false

Worse, it is puzzling that the share of female children in India is declining, despite all advances made in education, literacy, healthcare and income attainment (Mitra, 2014). One reason found by rigorous research is that modern technology to detect the sex of fetus has partly contributed to such deterioration in sex child ratio, by inducing sex selective abortion when the sex of fetus is revealed as female (Arnold et al, 2002).

2.5 the effect of ultrasound scan and global promotion to universal antenatal care

Ultrasonography is a commonly used technique for ascertaining a foetus' sex during pregnancy.[14] Although it is illegal in India to use ultrasound scans for the purpose of determining the sex of a foetus, there are genuine medical reasons to have scans during which it is not difficult to determine the sex of the foetus. Despite sex selective abortions are illegal (Pre-Conception and Pre-Natal Diagnostic Techniques (PCPNDT) Act, 1994), this law is hard to enforce (Sudha and Rajan, 1999), as households can first obtain the sex of fetus during the antenatal care service, and then carry out abortion separately somewhere else, such as in private sector.

Importantly, first antenatal care visit enables detection of the sex of the fetus at the pregnant period when abortion is still legal. This is because, from 16 to 18 weeks onwards, the determination of sex by ultrasound scan become reliable, and abortion is only illegal after the first 20 weeks of pregnancy (Medical Termination of Pregnancy Act, 1972)[15].

Therefore, we investigate whether a national promotion to universal antenatal care services has further skewed the sex child ratio. Households who would otherwise have been constrained by information or financial costs to use antenatal care service to detect the sex of fetus are now subsidised with fees and transportation cost by JSY and encouraged by health assistants to take up anal care where the sex of fetus is revealed to the family by the ultrasound scan.

[14] The two alternatives Chorionic Villus Sampling and Amniocentesis can determine the gender of the foetus from 10 to 12 and 15 to 18 weeks respectively. These methods are considerably more expensive than ultrasound.

[15] Pregnancies not exceeding 12 weeks may be terminated based on a single opinion formed in good faith. In case of pregnancies exceeding 12 weeks but less than 20 weeks, termination needs opinion of two doctors.

It is crucial and urgent to know whether JSY, with promotion to antenatal service, led to decline in the share of girls born, as a global effort is on its way to promote universal access to antenatal care service.

First, in India, further push to promote universal access is expected. Increasing antenatal check-up coverage is identified by World Health Organisation (WHO) as a strategic means to reach WHO target of a maternal mortality ratio below 70 by 2030 in India[16], and the Indian government is highly praised in 2018 for its effort to increase antenatal check-ups.

Furthermore, a global call to reach high coverage of quality antenatal care to prevent child mortality is led United Nations (2020)[17], jointly supported by UNICEF, the World Health Organization (WHO), the World Bank Group and the Population Division of the United Nations Department of Economic and Social Affairs (UN DESA).

Without modification in the policy design, universal antenatal care will translate into universal detection of the sex of the fetus. This could be harmful in areas where son preference is strong and sex selective abortion is not difficult to implement. The purpose of this paper is to answer this question. In the worse scenario, the sex child ratio could drop due to global effort to universal antenatal care service.

3. Data, sample and measures

3.1 Data and sample

We use two waves of the District Level Household Survey in India (DLHS 2004, DLHS 2008) which cover periods before and after the introduction of JSY.

The DLHS2 (DLHS3) interviewed 620,107 (720,320) women, during March 2002 to December 2004 (December 2007 to December 2008). The sample of each round is representative at the district level, for which, on average 1,000 women were interviewed.

[16] https://www.who.int/southeastasia/news/detail/10-06-2018-india-has-achieved-groundbreaking-success-in-reducing-maternal-mortality

[17] https://www.un.org/development/desa/pd/sites/www.un.org.development.desa.pd/files/unpd_2020_levels-and-trends-in-child-mortality-igme-.pdf

Further, for the most recent birth within 5 years preceding the survey, the DLHS collects information on a number of antenatal care services aspects, such as, for instance, which tests were carried out and whether the woman received an ultrasound scan during pregnancy. DLHS-2 interviewed women aged 15-44, but DLHS-3 interviewed women aged 15-49. We therefore focus on women age 15-44 across the two surveys.

Next, the dataset (in which unit is mother) is then further reshaped into the dataset in which unit is each child. Because DLHS-3 only collects childbirths that occurred since 2004, we do not keep births in DLHS-2 after 2004, to avoid overlapping observations with DLHS-3. As the amount of birth cohort each year evolves smoothly as shown in appendix table A1, we argue that memory recall is reliable for a mother to recall which year her child was born. As most fiscal year sees the amount of newborns to be above 70,000, we therefore keep all children with detailed birthday recorded from 1994, the first year when the aggregate number is above 70,000.

In summary, we keep all children from 1994 to 2004 in DLHS-2 and all from 2004 to 2008 in DLHS-3. The final sample consists of 982,566 children in 582 districts. However, not all districts are surveyed in 2008 in DLHS-3. Therefore, only 307 of 582 districts have observations in 2008.

Importantly, 2002 and 2003 in DLHS appear to have unusually low amount of births. Therefore, we drop 2003 and 2003, as the first robust check, in case pre-trend in sex children ratio is contaminated by DLHS survey design involving these 2 years.

3.2 Treatment variable

Importantly, we define treatment at district level, as the district has adopted JSY. So, treatment in our study is the exposure to JSY, rather than that individual mother has been confirmed to be treated. Individual take-up is likely endogenous. But it has not been possible to obtain exact information on the precise timing of JSY adoption by districts, although we do know that JSY was launched in 2005 and was rolled out incrementally across the country,

Therefore, we follow the approach of previous studies (Lim et al., 2010; Powell-Jackson, et al., 2015) to define the treatment status of a district. We consider a district to have adopted JSY, if the share of mothers in the district in the DLHS survey claiming having received government financial assistance for delivery care is higher than a certain threshold. The variable used comes from the self-reported information on JSY receipt regarding the last live birth contained in the DLHS3. The question is: *"Did you receive any government financial assistance for delivery care under the Janani Suraksha Yojana or state-specific scheme?"*

Table 1. the number of districts under different cutoffs

Group		cutoff 10%	cutoff 15%	cutoff 20%	cutoff 25%
1	Adopters in 2005	136	76	40	19
2	Adopters in 2006	145	134	111	68
3	Adopters in 2007	87	82	78	98
4	Adopters in 2008	17	14	10	7
5	Never treated	197	276	343	390
	total	582	582	582	582

Source: Authors' calculation using DLHS 3, India.

For each district and fiscal year, we calculate the weighted proportion of mothers reporting to have received pecuniary assistance under JSY conditional on having given birth in that year. When this proportion exceeds a pre-determined threshold, we consider this district has adopted JSY from that year onwards.

Among previous studies, Andrew and Vera-Hernández (2022) uses 20% as the cutoff, while Powell-Jackson, et al. (2015) have three binary dummies for 10-25%, 25-50%, 50% - 100%, leaving 0-10% as baseline in their specification. Due to the arbitrariness of the choice of cut-offs, we experiment with four different thresholds: 10%, 15%, 20%, and 25%, and always report results under all cutoffs.

Table 1 shows the number of districts, when JSY adoption is defined by different cutoffs. Importantly, when a higher cutoff is used, fewer districts are defined as treated, as a higher percentage of JSY recipients is required for a district to pass the threshold and defined as having adopted JSY.

It is importantly that neither direction is preferred. On one hand, by using higher threshold, we are more selective and the risk increases that treated districts differ from the untreated districts. On the other hand, by using a lower threshold, it is easier for a district to be defined as treated district. So the risk increases that we consider a district to be treated even if it has not adopted JSY. In that case, if we do not detect any effect of JSY, it can be a result that many districts we define as being treated, has not adopted JSY in reality.

3.3 Main outcome variable – sex ratio

We construct sex ratio variable at district level. We calculate the share of girls at birth in each district for the fiscal years 1994 to 2008, weighted by the weights for population estimate provided in DLHS survey. As discussed earlier, the data come from retrospective birth histories in DLHS2 and DLHS3. Details for the construction on other outcomes are available upon request.

3.4 Control variables

We construct the maternal education variable, using weighted average of mother's year of schooling in each fiscal year in each year from 1994 to 2008. As discussed earlier in section 2, maternal education is the single predictor for son preference and JSY has induced more pregnancies with cash incentives. In theory, by paying for additional incentives and the cost of delivery and transportation, JSY induces more pregnancies with less-affluent women which are also less-educated and therefore have stronger son preference. We must control for maternal education of each birth cohort at district level, in order to isolate the estimated effect of JSY on sex ratio through access to ultrasound scan, from the effect of JSY on sex ratio through a changed population of pregnant women with stronger son preference.

The other control variables, such as living standard, religion and caste, are less crucial for this reason, as they are found independent of son preference. But we still include them, in order to relax our crucial but untestable assumption that random adoption is completely random. Specifically, we use the weighted share of mothers being scheduled caste, the weighted share of mothers being Hindu and the weighted share of households being in top third standard of living index, in all women giving birth in a certain district in a certain year in DLHS.

In table 2, we present the characteristics between JSY adopted districts and the rest, by using 25% as the cutoff which the most selective threshold we use, leading to largest difference between treated districts and the results.

Table 2. Differences between Treatment and Control in year at baseline variables of 2004 cutoff 25%

Adoption status	2005.000		2006.000		2007.000		2008.000	
	C	T	C	T	C	T	C	T
scheduled caste	0.190	0.216	0.190	0.200	0.194	0.186	0.191	0.146
scheduled tribe	0.149	0.142	0.147	0.154	0.132	0.185	0.164	0.307
other-backward caste	0.376	0.547	0.367	0.463	0.366	0.414	0.277	0.317
hindu	0.760	0.814	0.748	0.843	0.722	0.848	0.689	0.760
muslim	0.130	0.039	0.139	0.060	0.154	0.068	0.130	0.080
bottom third std of living index	0.355	0.197	0.358	0.302	0.327	0.400	0.252	0.460
middle third std of living index	0.340	0.447	0.335	0.390	0.341	0.349	0.359	0.317
top third std of living index	0.305	0.356	0.307	0.308	0.333	0.251	0.390	0.223
mother year schooling	4.813	6.590	4.721	5.725	4.931	4.741	5.501	4.392
father year schooling	6.826	7.499	6.813	7.048	6.963	6.603	7.373	6.095
mother cannot read	0.458	0.343	0.465	0.390	0.451	0.461	0.395	0.475
mother age	26.756	25.938	26.807	26.286	26.838	26.496	27.044	26.785
father age	33.026	32.365	33.073	32.614	33.242	32.494	33.362	33.021
districts	563	19	495	87	397	185	230	77

Sources: DLHS 2 and 3, India.

Importantly, our crucial assumption is only on potential sex ratio in the absence of JSY, so it does not require any of these covariates to be similar, as long as they do not affect potential sex ratio in the absence of JSY. Previous finding is supportive as son preference is found persistent across the entire society, regardless of religious, race, household wealth or local economic development.

That said, it appears that difference between adopted districts and untreated districted has reduced from 2005 to 2007, as more districts adopted JSY. However, the difference becomes the largest between adopted districts and non-adopted districts in 2008 when DLHS only surveyed 307 districts, instead of the entire India. However, it is reassuring that later we find no differential in pre-trend in sex ratio is detected across different cutoffs, by treatment status defined in 2008.

4. Methodology estimation and inference

Given JSY's incremental rollout feature, we use the same methodology of Athey and Imbens (2018) in which units adopt the policy or treatment at a particular point in time, and remain exposed to the treatment at all times afterwards. Furthermore, we use randomisation inference by following Athey and Imbens (2018) to take a design-based perspective where the stochastic nature and properties of the estimators arises from the random assignment of adoption process, as Young (2019) and Heß (2017) conclude that conventional econometric tests using asymptotically accurate clustered/robust covariance estimates often leads to over-rejection and are often driven by extreme values by a few observations. In addition, we always present joint tests of multiple treatment effect, also to avoid over-rejection. Furthermore, for the event-study analysis we follow Borusyak, Javarvel, and Spiess (2022).

4.1 Set up

We set up the problem with the adoption date of a district, rather than the actual exposure to the intervention of individual pregnant woman, as individual take-up of JSY is likely to be endogenous. Treatment is reduced to a binary one, defined as the dummy indicator whether or not the treatment has already been adopted in this district.

The treatment indicator will always be 1 from the year onwards once the district is identified as JSY adopter. Similarly, no treatment indicator will be valued 1 before 2005, but there are only 3 years from 2005 to 2007. As the second robustness check, we only use balanced panel by excluding observations in 2008.

We follow Athey and Imbens (2018) to use a potential outcome framework for causal inference. We observe for each district the adoption year (or never adopted from 2005 to 2008) and the sequence of realised sex child ratio. Importantly, here we view the potential sex ratio of a district as deterministic, and only the adoption year as scholastic, as well as the realised sex ratio. Distributions of estimators will be fully determined by the adoption year distribution, with the number of units and the number of time periods fixed.

The crucial assumption is that adoption year is completely random across districts within the same state. As this assumption is on potential outcomes, it has no testable implication as we only observe realised sex ratio.

But we relax the assumption. First, as Athey and Imbens (2018) suggests, some variables are known not to be affected by the treatment, the assumption can be relaxed by requiring only that the adoption year is completely random within subpopulations with the same values for the pre-treatment variables. Therefore, by controling for maternal education, religious, scheduled caste and living standards for each birth cohort, we have a weaker assumption that adoption year is random within districts with same level of maternal education and other local factors.

Under the random assignment of adoption date within each state, the stochastic properties of these averages are well-defined because the share of adopters in each state is fixed over the randomization distribution. The averages are stochastic because the realised outcomes depend on the adoption date. The standard difference-in-differences estimator has causal interpretation as a weighted casual effect (Athey and Imbens, 2018), in terms of average causal effects of different adoption year, for example, the effect of changing from never adopting to adopting in the first year, or changing from never adopting to adopting later in 2008.

The main specification is as follows.

$$Y_{it} = \alpha_i + \beta_t + \tau W_{it} + X_{it} + \varepsilon_{it}, \qquad (1)$$

In this model there are district effects and time effects while we also control for covariates of each birth cohort, such as mother year of schooling. The effect of the treatment is implicitly assumed to be additive and constant across units and time periods. In addition, SUTVA assumption is required that districts are not affected by the JSY adoption of other districts (Rubin, 1978; Imbens and Rubin, 2015).

4.2 Tests for pre-differential trends in sex child ratio

Although common trend assumption is not a starting point of our assumption, it does follow from the random assignment assumption (Athey and Imbens, 2018). Therefore, no-rejection of common trend assumption is a necessary condition for our untestable assumption on random assignment not to be violated. In other words, if differential pre-trend in sex ratio is detected, our untestable assumption is violated.

We first examine whether there is differential linear trend in sex child ratio in pre-reform years from 1994 to 2004. As shown in Table 3, differential pre-trend in sex ratio is not detected across any threshold or any treatment status defined in each post-reform year. We control for state-fixed effect, as our resampling occurs within state.

Furthermore, we allow for non-linear year trend to check differential trends. We interact pre-reform year dummies for 2002, 2003, and 2004, with treatment indicator defined in each post-reform year, in turn by each cutoff. As shown in Table A1 in the appendix, most results conclude non-detectable differential trend in sex ratio, except for sex child ratio in year 2003 among treated districts in 2007 or 2008, and the rest, when cutoff is 10 percent. As in section 3, we already find 2003 has an exceptional low amount of childbirth records in DLHS 2, it may arise as a sample selection issue. As the first robustness check, we will exclude 2002 and 2003.

4.3 Tests for the correlation in changes in outcomes and JSY rollout timing

In addition, we show in Table 4 that the start year of JSY is uncorrelated with long-run changes in outcome variables, confirming the findings of Andrew and Vera-Hernandez (2022) and Powell-Jackson et al. (2015) that the rollout appears uncorrelated with prior trends.

4.4 Inference

This design-based approach views the potential outcome as deterministic, and only the adoption dates and realized outcomes as stochastic. Distributions of estimators will be fully determined by the adoption date distribution, with the number of units N and the number of time periods T fixed. During the repeated sampling, the fraction of real treated districts in a state is fixed. Consequently, the known universe of potential treatment allocations determines the statistical distribution of the estimated coefficients or the distribution of T statistics, and can be used to test sharp hypotheses that precisely specify the treatment effect for each district, allowing the calculation of what sex ratio would have been for any potential random allocation (Young, 2019) of JSY adoption.

Table 3. Pre-JSY Linear Trend Differences in Sex Ratio

	Treatment status defined in each year			
	2005	2006	2007	2008
JSY cutoff 10%	(1)	(2)	(3)	(4)
Treatment X Time trend	-0.001	-0.000	-0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)
Linear Time trend	-0.000	-0.000	-0.000	-0.001
	(0.000)	(0.000)	(0.000)	(0.001)
Treatment dummy	1.590	0.256	0.094	-0.641
	(1.237)	(1.047)	(1.084)	(1.531)
Observations	6393	6393	6393	3368
JSY cutoff 15%	(1)	(2)	(3)	(4)
Treatment X Time trend	-0.001	0.000	-0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)
Linear Time trend	-0.000	-0.000	-0.000	-0.001
	(0.000)	(0.000)	(0.000)	(0.001)
Treatment dummy	1.096	-0.549	0.193	-0.332
	(1.562)	(1.092)	(1.047)	(1.507)
Observations	6393	6393	6393	3368
JSY cutoff 20%	(1)	(2)	(3)	(4)
Treatment X Time trend	-0.001	0.000	-0.000	0.281
	(0.001)	(0.001)	(0.001)	(1.072)
Linear Time trend	-0.000	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Treatment dummy	1.573	-0.768	0.281	0.786
	(2.068)	(1.197)	(1.072)	(1.613)
Observations	6393	6393	6393	3368
JSY cutoff 25%	(1)	(2)	(3)	(4)
Treatment X Time trend	-0.001	0.000	-0.000	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Linear Time trend	-0.000	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Treatment dummy	1.554	-0.777	0.731	1.064
	(2.945)	(1.468)	(1.124)	(1.730)
Observations	6393	6393	6393	3368

Note: State fixed effect is included. Standard error in parenthese.

Source: DLHS 2, 3, India. * p<0.1   ** p<0.05 ** p<0.05.

Table 4. Relation between year of JSY's rollout and prior changes in outcomes of interests

	(1) JSY5 Rollout	(2) JSY10 Rollout	(3) JSY15 Rollout	(4) JSY20 Rollout
Change in the Share of girls born	-0.036	-0.058	-0.060	-0.061
	(0.048)	(0.050)	(0.046)	(0.041)
Observations	581	581	581	581
Change in the Share of girls born among first child	0.009	-0.020	-0.069	-0.041
	(0.048)	(0.050)	(0.046)	(0.041)
Observations	581	581	581	581
Change in the Share of girls born among non first child	-0.053	-0.062	-0.031	-0.046
	(0.048)	(0.050)	(0.046)	(0.041)
Observations	581	581	581	581
Change in the neonatal mortality	0.047	0.036	0.017	0.031
	(0.048)	(0.050)	(0.046)	(0.041)
Observations	581	581	581	581
Change in the boy neonatal mortality	0.017	0.019	0.012	0.041
	(0.048)	(0.050)	(0.046)	(0.041)
Observations	581	581	581	581
Change in the girl neonatal mortality	0.042	0.025	0.005	-0.007
	(0.048)	(0.050)	(0.046)	(0.041)
Observations	581	581	581	581

Note: * p<0.1 ** p<0.05 ** p<0.05. Estimates from district level regression of the yearly that JSY started on standardized (mean 0, variance 1 across districts in changes in outcome variables from 1990 to 2001). Standard error in parenthese. *Source*: DLHS 2, 3, India

An exact test of a sharp null is constructed by calculating possible realizations of a test statistic and rejecting if the observed realization in the realized adoption is extreme enough. We follow Young (2019) to use T-statistics based randomization test as it is more accurate in the face of heterogeneous treatment effects. On average, randomisation inference reduces the number of significant results relative to those use using asymptotically accurate clustered/robust covariance estimates, and randomisation inference is more robust to extreme observations (Young, 2019) Furthermore, multiple hypothesis testing is also used to further reduce over-rejection.

5. Main result

First, we show that the treatment effect of JSY. As shown in Table 5, the national cash transfer program has significantly reduced the share of girls born. Consistent with previous findings that son preference is much weaker among first child, we do not find treatment effect among first child. In addition, we find the neonatal mortality has only improved for boys. The result holds either we use asymptotic p-values or use randomisation inference p-values.

In addition, as we control for the time-varying composition of mothers' characteristics such as wealth, rural, maternal year of schooling. Therefore, the treatment effect is not induced by the demographic changes in motherhood.

Table 5. Treatment effect of JSY

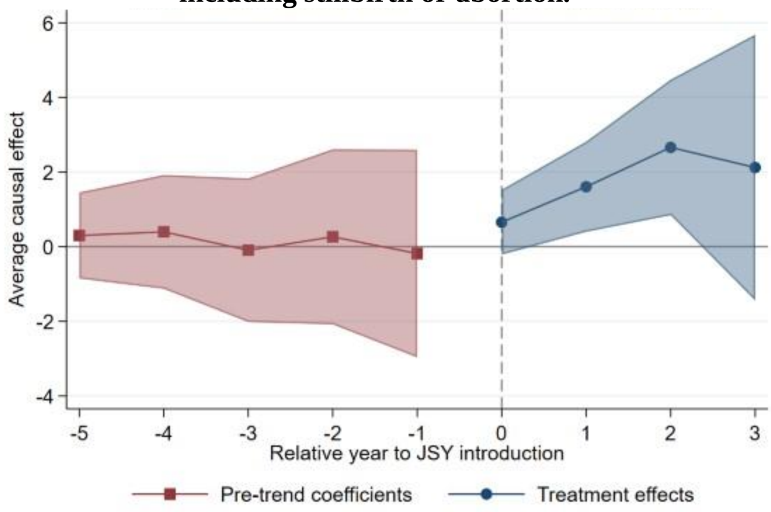
Outcome variable	Control mean in 2005	treatment effect	asym. p-value	p-value ritest	N	n
share of girls	47.776	-0.961	**0.040**	**0.033**	581	5536
share of girls among 1st kid	47.664	0.050	0.942	0.936	581	5487
share of girls among non-1st kid	47.891	-1.257	**0.022**	**0.011**	581	5526
neonatal mortality girls	2.116	0.228	0.250	0.273	581	5508
neonatal mortality boys	2.685	-0.520	**0.008**	**0.003**	581	5517

Notes: All regressions contain time-varying covariates at district level including average maternal year of schooling, share of top third living standard households, and share of rural households. JSY adoption is defined by using all births including public deliveries and non-public deliveries. Threshold is 5%. Randomisation is conducted at district level by year with each state, to keep the number of treated districts fixed as reality. Randomisation is repeated 2000 times to form a distribution of T statistics. *Source*: DLHS 2, 3, India

6. Dynamic treatment effect

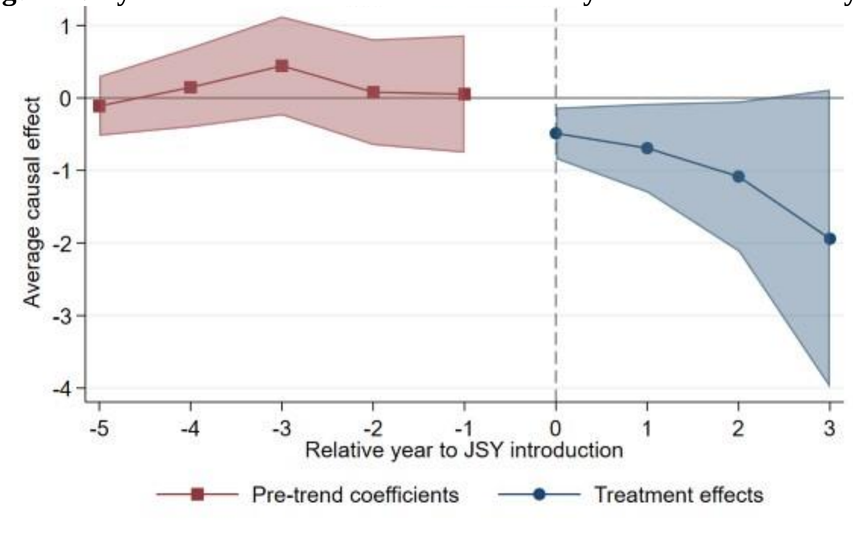
Next, we decompose the treatment effect estimates from a standard difference-in-differences estimator to an event study decomposition to investigate the dynamic treatment effect that we find for sex ratio and gendered neonatal mortality. We follow Borusyak, Jaravel and Spiess (2022) for the event-study estimation. As shown in Figure 3 and 4, there is no differential pre-trend in either share of boys born or neonatal mortality of boys. Furthermore, there is a significant impact of JSY on the share of boys born and on the improvement of boys' neonatal mortality. The treatment effect on sex ratio even becomes larger after 2 years a district having adopted JSY.

Figure 3. Dynamic treatment effect of JSY on share of boys born among all birth outcomes including stillbirth or abortion.



Notes: All regressions contain time-varying covariates at district level including average maternal year of schooling, share of top third living standard households, and share of rural households. JSY adoption is defined by using all births including public deliveries and non-public deliveries. Threshold is 5%. *Source*: DLHS 2, 3, India

Figure 4. Dynamic treatment effect of JSY on boys' neonatal mortality



Notes: All regressions contain time-varying covariates at district level including average maternal year of schooling, share of top third living standard households, and share of rural households. JSY adoption is defined by using all births including public deliveries and non-public deliveries. Threshold is 5%. *Source*: DLHS 2, 3, India

7. conclusion

Quality antenatal care services is identified as a strategic means to reduce global mortality of mother and infant. Health promotion to universal access to antenatal care services is on the agenda of United Nations, World Health Organisations, and The World Bank Group.

However, across various assumption and specification, robust results suggest that such health promotion unintendedly reduced sex ratio, as its ultrasound scan component reveals the sex of fetus for households who strongly prefer sons to daughters. In the face of forthcoming global effort to promote universal access to antenatal care service to reduce mortality, we call for urgent modification of current practice to be more sensitive to local culture to be non-harmful.

Future research can explore if JSY have long-lasting impact on the marriage market, crime, social stratification and welfare which are found to be affected by the aggregate sex ratio in other context.

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Appendix

Table 4. Prereform Non-Linear Trend Differences in sex ratio

	Treatment status defined in each year			
	2005	2006	2007	2008
cutoff JSY 10%	(1)	(2)	(3)	(4)
Treatment X Year 2002	-0.001	0.000	-0.001	-0.002
	(0.007)	(0.006)	(0.006)	(0.008)
Treatment X Year 2003	-0.011	0.005	**0.014****	**0.025*****
	(0.007)	(0.006)	(0.006)	(0.008)
Treatment X Year 2004	-0.007	-0.010*	-0.010	-0.014*
	(0.007)	(0.006)	(0.006)	(0.008)
Treatment	0.003	0.009***	0.006**	0.009**

	(0.003)	(0.003)	(0.003)	(0.004)
Year 2002	0.001	0.001	0.002	0.004
	(0.003)	(0.004)	(0.005)	(0.007)
Year 2003	-0.009***	-0.014***	-0.020***	-0.031***
	(0.003)	(0.004)	(0.005)	(0.007)
Year 2004	0.000	0.003	0.005	0.007
	(0.003)	(0.004)	(0.005)	(0.007)
Observations	6393	6393	6393	3368
cutoff JSY 15%	(1)	(2)	(3)	(4)
Treatment X Year 2002	-0.002	-0.002	0.003	0.009
	(0.009)	(0.006)	(0.006)	(0.008)
Treatment X Year 2003	0.006	0.012*	0.007	0.007
	(0.009)	(0.006)	(0.006)	(0.008)
Treatment X Year 2004	-0.011	-0.005	-0.005	-0.010
	(0.009)	(0.006)	(0.006)	(0.008)
Treatment	0.002	0.003	0.007**	0.012***
	(0.003)	(0.003)	(0.003)	(0.004)
Year 2002	0.001	0.002	-0.000	-0.002
	(0.003)	(0.004)	(0.004)	(0.006)
Year 2003	-0.012***	-0.015***	-0.015***	-0.019***
	(0.003)	(0.004)	(0.004)	(0.006)
Year 2004	0.000	0.001	0.001	0.004
	(0.003)	(0.004)	(0.004)	(0.006)
Observations	6393	6393	6393	3368
cutoff JSY 20%	(1)	(2)	(3)	(4)
Treatment X Year 2002	-0.004	-0.002	-0.003	0.009
	(0.012)	(0.007)	(0.006)	(0.009)
Treatment X Year 2003	-0.018	0.007	0.003	0.000
	(0.012)	(0.007)	(0.006)	(0.009)
Treatment X Year 2004	-0.003	-0.004	-0.005	-0.013
	(0.012)	(0.007)	(0.006)	(0.009)
Treatment	-0.000	0.002	0.002	0.003
	(0.004)	(0.003)	(0.003)	(0.005)
Year 2002	0.002	0.002	0.002	-0.001
	(0.003)	(0.003)	(0.004)	(0.005)
Year 2003	-0.010***	-0.013***	-0.012***	-0.016***
	(0.003)	(0.003)	(0.004)	(0.005)
Year 2004	-0.001	-0.000	0.001	0.003
	(0.003)	(0.003)	(0.004)	(0.005)
Observations	6393	6393	6393	3368
cutoff JSY 25%	(1)	(2)	(3)	(4)

Treatment X Year 2002	-0.018	-0.002	-0.008	-0.003
	(0.016)	(0.008)	(0.006)	(0.010)
Treatment X Year 2003	0.012	0.007	0.001	-0.004
	(0.016)	(0.008)	(0.006)	(0.010)
Treatment X Year 2004	-0.005	-0.005	-0.008	-0.011
	(0.016)	(0.008)	(0.006)	(0.010)
Treatment	0.002	0.004	0.003	0.001
	(0.006)	(0.003)	(0.003)	(0.005)
Year 2002	0.002	0.001	0.004	0.003
	(0.003)	(0.003)	(0.004)	(0.005)
Year 2003	-0.011***	-0.012***	-0.011***	-0.015***
	(0.003)	(0.003)	(0.004)	(0.005)
Year 2004	-0.001	-0.000	0.001	0.002
	(0.003)	(0.003)	(0.004)	(0.005)
Observations	6393	6393	6393	3368

Note: State fixed effect is included. Standard error in parenthese.

Source: DLHS 2, 3, India. * p<0.1 ** p<0.05 ** p<0.05.

Table A2. Children born each fiscal year in DLHS sample

	DLHS		
Fiscal year	2	3	Total
1989	15,927	0	15,927
1990	62,249	0	62,249
1991	65,805	0	65,805
1992	67,476	0	67,476
1993	69,455	0	69,455
1994	70,958	0	70,958
1995	71,158	0	71,158
1996	76,673	0	76,673
1997	73,587	0	73,587
1998	77,711	0	77,711
1999	75,444	0	75,444
2000	71,960	0	71,960
2001	71,432	0	71,432
2002	44,931	0	44,931
2003	35,113	12,824	47,937
2004	8,348	62,097	70,445
2005	0	71,894	71,894

2006	0	74,966	74,966
2007	0	75,623	75,623
2008	0	7,847	7,847
Total	958,227	305,251	1,263,478

Source: DLHS 2 and 3, India.