When Free Healthcare Meets Gender Norms: the Impact of Public Healthcare on Sex Selection*

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Abstract

This paper estimates the effect of expanding free healthcare in a context with a strong son preference on fertility and sex-selection. I examine a government program which provided free healthcare to low-income households in the state of Rajasthan, India. Using a difference-in-differences, I show that the policy reduced fertility among eligible women. This decline is accompanied by a disproportionate reduction in female births, consistent with increased sex-selective behavior. The policy also led to a decline in female infant mortality. Exploiting spatial variation in access to newly enrolled facilities, I show that reductions in travel distance to free hospitals constitute an important channel. I further provide evidence of changes in healthcare seeking behavior, contraceptive use, increase reliance on sex-selective abortions and stopping rules.

Keywords: fertility, son preference, sex selection, health insurance, India, free healthcare

JEL Codes: I18, J13, J16, O15

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1 Introduction

In 1992 Amartya Sen warned that "*more than 100 million women are missing*", mostly in Asian countries (Sen, 1992). It soon became clear the phenomenon of missing women was driven not only by excess female infant mortality due to post-natal discrimination – through neglect in infant female nutrition and healthcare, and in some extreme cases female infanticide – but also by prenatal sex-selection. The underlying cause of this discrimination is a deeply entrenched cultural preference for sons observed in many countries, particularly in Asia. In India alone, an estimated 460,000 women were missing between 2013 and 2017 (United Nations Population Fund, 2020). Indeed, India's sex ratio at birth (henceforth SRB) has become increasingly male-biased over time, deviating from the natural SRB of 105.[1]

This paper addresses the question of what are the fertility consequences of expanding free healthcare in a context where son preference is strong. Despite extensive research documenting the persistence of son preference, less is known about how public health policies may influence it, reducing it or unintentionally increasing the incidence of sex selection. One policy particularly relevant in this context is Bhamashah Swasthya Bima Yojana (BSBY), a government funded insurance scheme that made healthcare effectively free for low-income families in the state of Rajasthan.

Launched in 2015, BSBY expanded free access to primary, secondary, and tertiary healthcare services for low income households in the state. Approximately 46 million people stood to benefit from this program in one of India's most populous and economically disadvantaged states. Moreover, the relevance of this analysis extends beyond Rajasthan. BSBY was expanded in 2018 to the rest of India under Ayushman Bharat Pradham Mantri Jan Arogya Yojan (AB-PMJAY), the world's largest national health insurance scheme. Understanding how reductions in the cost of healthcare affect fertility behavior and sex-selection in the Indian context is critical for anticipating the broader demographic consequences of this program.

The effects of large-scale health policies on fertility behavior, and especially on sex selection, are not obvious a priori. On one hand, by reducing the financial burden of healthcare, this program makes raising an unwanted daughter less costly. This could decrease both sex selective abortions and female infant mortality. While reducing the financial burden of raising daughters can weaken son preference (Bhalotra, Chakravarty, and Gulesci, 2020), entrenched patriarchal norms can cause even well-intentioned policies to backfire, sometimes reinforcing gender biases and increasing sex-selective behavior (Bhalotra, Brulé, and Roy,

[1]The SRB is the number of male births per 100 female births. The natural SRB is 105 as male fetuses have a slighter higher advantage in survival in the womb.

2020; Rosenblum, 2015). On the other hand, by making abortion more accessible and eliminating its financial cost, the policy could increase the reliance on sex-selective abortion. Additionally, if the policy reduces fertility, through a combination of increased gynecological visits and access to abortion, it may push a greater share of the population to resort to sex-selective abortions as families planning fewer births face a higher risk of not obtaining a son naturally.

The policy came into effect in December of 2015, when all public hospitals in the state were automatically enrolled in the insurance scheme. Private hospitals that met certain eligibility requirements were also allowed to participate, resulting in a network of covered providers that gradually expanded over time. Eligibility was determined at the household level: households belonging to the bottom 40% of the income distribution automatically qualified for the insurance scheme. Effectively, after December 2015 every eligible woman in Rajasthan was able to obtain free healthcare, including abortion services, depending on how far she was willing to travel to obtain it. Not wanting to make any assumptions on how far women would be willing to travel to obtain prenatal screenings as well as safe and free abortions, I first estimate the effect of the policy using a difference-in-differences. I use the National Family and Health Survey (NFHS) from 2019/2020 to create a panel of women that includes the fertility history of over 27,000 women in Rajasthan. I consider all eligible women, i.e. low-income women, in the state as treated, and compare eligible women to non eligible women before and after the policy introduction to estimate the causal effect of the policy on fertility and on sex selection.

I find that BSBY led to a significant reduction in fertility among eligible women. Event-study estimates show that this decline is gradual and becomes statistically significant approximately two years after implementation. The effects extend beyond birth timing: women who have completed fertility exhibit a lower total number of children, indicating a reduction in completed fertility. Importantly, I find evidence that the expansion of free healthcare increased sex selection. While both female and male births decline, natural differences in the probability of having a male vs a female child cannot account for the observed decrease in female births. Back-of-the-envelope calculations show that the drop in female births is too large to be explained by a fertility decline alone, even when conditioning on baseline prevalence of sex-selection. Instead, the results point to an increase in son-targeting fertility behavior, including both stopping rules and sex-selective abortion.

Looking at changes in fertility behavior by birth parity, I find that the decrease in female births is driven by first births. This finding has two key implications. First, this documents a behavioral shift regarding sex-selection: the gender of the first child is not longer random. The policy induced families to start resorting to sex-selective abortions at their first child,

consistent with the idea that when desired fertility declines, the likelihood of satisfying son preference without resorting to sex selection is reduced. This finding is notable because most of the literature finds no evidence of sex selection at first birth. An exception is Anukriti (2018), who shows that a conditional cash transfer in Haryana increased sex ratios at first birth by incentivizing families to stop childbearing after one child or after two daughters. In contrast, BSBY does not directly target fertility or sex ratios. Any increase in sex selection therefore reflects lower costs of acting on son preference, reinforced by the policy-induced decline in fertility. Second, this finding helps disentangle the two mechanisms behind sex selection: stopping rules and sex-selective abortion. While both contribute, the increase in sex-ratio at first birth points to sex-selective abortion as the dominant channel.

The gradual expansion of the policy allows me to further explore the effect of not only providing free healthcare, but also the role of *proximity* to care as a mechanism. To asses this channel, I obtain the geolocation of all hospitals under BSBY from December 2015 to October 2019, along with the date each hospital began filing insurance claims under BSBY.[2] I match the location of NFHS-5 clusters to the locations of empaneled hospitals and calculate the driving distance to the nearest operating hospital in each month for all women in the sample. By combining this information with the fertility panel, I investigate how reductions in distance to the nearest free healthcare facility affect fertility and sex selection, highlighting proximity as a key mechanism through which the policy operates. Importantly, the hospitals were already present at the same locations before the policy; the key change is that after their empanelment they became *free* for eligible women. Because the hospitals' locations are fixed over the time horizon I investigate, variation in which hospitals became free over time allows for a causal interpretation, akin to a difference-in-differences design. I find that being closer to a free hospital reduces the overall probability of giving birth, with the effect driven primarily by a larger decrease in the probability of having a daughter.

Next, I investigate other channels through which the policy might have decreased fertility and increased sex-selection. One such channel is changes in healthcare seeking behavior. By expanding access to free care, BSBY likely increased the use of gynecological consultations and prenatal screenings, enabling women to monitor pregnancies more closely, make informed fertility decisions, and potentially act on son preference. Indeed, I show that the policy increased prenatal care, changed the places in which women seek it, and increased the use of ultrasounds. Closely related, I provide further evidence of both channels behind sex-selection: abortion and stopping rules. Lastly, I show that the policy increased deliveries at healthcare facilities and reduced daughters' mortality at birth. Overall, the results highlight that the

[2] I thank Pascaline Dupas and Radika Jain for sharing with me the geolocalization of hospitals empaneled in BSBY used in their work (Dupas and Jain, 2024).

policy changed healthcare seeking behavior around prenatal screenings and family planning. At the same time, they reveal an unintended consequence of free healthcare expansion: while it improves access and healthcare usage, it can also amplify gender-biased fertility behavior in contexts where son preference remains strong.

This paper contributes to several strands of literature. Son preference in India is a well-documented phenomenon, reflecting not only cultural bias but also economic considerations, as parents may perceive higher returns on investment in sons due to their expected socio-economic roles. Dimri, Gille, and Ketz (2024) document widespread repeated sex-selective abortions among women with two or more children, estimating between 47 and 143 aborted female fetuses per 1,000 women. This practice has historically been concentrated in Northern India and among more educated, upper-caste, wealthier, urban households (Bhalotra and Cochrane, 2010). I extend this literature by showing that when healthcare becomes free and more accessible, lower-income women also engage in sex selection, suggesting that financial and geographic barriers previously limited access to these practices . This study joins other studies showing that as fertility falls in India, sex ratios worsen (Jayachandran, 2017). By documenting a shift in sex-selective behavior toward parity one, it highlights how policies that reduce the cost of healthcare can potentially lower fertility and unintentionally reshape the timing of sex selection.

Several studies have explored the effects of sex-selective technology on neonatal female mortality and sex selective practices in context with son preference. Lin, Liu, and Qian (2014) show that following the legalization of abortion in Taiwan, where ultrasounds were widely available, sex-selective abortions increased and relative female infant mortality decreased for higher parity births. Interestingly, by lowering the cost of sex-selection, access to abortion also led to an increase in fertility for older women with strong son preference. In other words, decreasing the cost of sex-selective abortions pushed women who might otherwise have stopped childbearing to enter a pregnancy, knowing that they could ensure the birth of a son. Similarly, in India – where abortion has been legal since the 1971 Medical Termination of Pregnancy Act – the widespread diffusion of sex detection technology, i.e. ultrasound machines, in the 1980s and 1990s, led to a significant increase in prenatal sex-selection (Arnold, Kishor, and Roy, 2002; Bhalotra and Cochrane, 2010). Concerns over rising SRBs led to the 1994 Pre-Natal Diagnostic Techniques Act, which banned fetal sex determination. Despite this prohibition, sex-selective abortions remain widespread, with an estimated 20.7 million missing women in India between 1970 and 2017 (Chao et al., 2019). Bhalotra and Cochrane (2010) estimate that between 1995 and 2005 0.48 million girls were aborted in India and that most of those sex-selective abortions were concentrated among wealthier and relatively educated women. As in Taiwan, the availability of sex-selection technology

(ultrasound and abortion) in India decreased relative female infant mortality (Anukriti, Bhalotra, and Tam, 2022). This project makes a novel contribution to this literature by shifting the focus to the role of overall healthcare access, in a context where abortion is legal and ultrasound technology is widespread.[3]

This paper also contributes to the growing literature examining the effects of healthcare expansions on fertility across different institutional and economic contexts. Existing evidence is mixed. Studies in the United States have found heterogeneous responses to healthcare expansion. Apostolova-Mihaylova and Yelowitz (2018) find that health insurance coverage increased fertility among married women while decreasing it for unmarried women, whereas Kearney and Levine (2009) find a decline in fertility following Medicaid expansion. In the Turkish context, Cesur et al. (2023) find that socialized healthcare caused a drop in fertility driven by reduced costs in family planning services. Research in developing countries remains scarce but generally finds that free healthcare causes fertility declines, as documented in Brazil and Iran (Rocha and Soares, 2010; Salehi-Isfahani, Abbasi-Shavazi, and Hosseini-Chavoshi, 2010). This is the first study to provide causal evidence on how free healthcare shapes fertility behavior while specifically exploring the role of sex selection.

The findings in this paper carry important policy implications. Health insurance expansion in contexts with strong cultural norms should be complemented with norm-changing interventions that directly address son preference and gender differences. More broadly, policies aimed at reducing fertility or expanding healthcare access should be designed with a specific gender lens, anticipating potential behavioral responses in settings where son preference persists.

The remainder of the paper is organized as follows. Section 2 provides useful background information on BSBB, prenatal healthcare in Rajasthan before the policy, and abortion. Section 3 and Section 4 describe the data and the empirical strategy. Section 5 presents the results and heterogeneity and Section 6 discusses the mechanisms driving the results. Section 7 provides additional results for infant health and mortality, and Section 8 concludes.

2 Background

2.1 BSBY

BSBY is a government funded insurance scheme launched in December of 2015 that grants free access to empaneled public and private healthcare facilities for the bottom 40% of the

[3]Abortion has been legal in India since the 1971 Medical Termination of Pregnancy Act, which permits termination up to 20 weeks of gestation.

income distribution in the state of Rajasthan. The insurance scheme is cashless, requiring no copay or upfront charges when seeking healthcare at empaneled hospitals. The policy entitles 46 million low-income residents, representing more than half of the state's population, to free secondary and tertiary education.[4] While all public hospitals were admitted to the program by default at the time of the launch, private hospitals that met basic eligibility criteria had to register and follow the empanelment process to join.[5] All members of a household that meet the eligibility criteria are eligible to cashless care and automatically enrolled in BSBY upon verification of their Bhamashah card at the healthcare facility.[6] However, evidence shows that often patients are charged for services that are supposed to be free for them, mostly due to patient's unawareness that all hospital fees, tests and medicines were meant to be fully covered by the insurance (Dupas and Jain, 2023; Jain, 2021).

Eligible households were entitled to up to ₹30,000 in secondary care and ₹100,000 in tertiary care per year. This limit appears to be non binding as less than 1% of families reach it (Jain, 2021). Recent work by Dupas and Jain (2024) shows that the policy did not reduce gender gaps in healthcare utilization. Although BSBY increased healthcare usage for both men and women, households continued to allocate fewer resources to female health, resulting in men benefiting more from the policy overall.

In December of 2017, the program was renewed, some redundant services were eliminated, hospital reimbursement rates were changed, and the annual limit for eligible households was raised to ₹300,000 for tertiary care. Requirements for private hospitals to join the insurance scheme were relaxed, allowing smaller private facilities to join the scheme, significantly increasing the supply of free healthcare. Moreover, all public hospitals were no longer reimbursed for child deliveries as these services were already provided under other government health programs targeting pregnant women. Finally, in 2018, when the scheme was expanded to the rest of India under ABPM-JAY, the annual limit was raised to ₹500,000.

2.2 Prenatal Healthcare

Prior to BSBY, basic preventive maternal and child care was available through a network of community based services. In rural areas, auxiliary nurses and midwives at sub-Primary Health Centers provided antenatal care either at home or in clinics. These sub-Primary Health Centers are the most peripheral contact point for basic primary healthcare, immu-

[4]Primary healthcare was already available for free and comprises basic preventive and curative care. Secondary care comprises basic hospital stays and uncomplicated child deliveries, and tertiary care includes major surgeries, in-patient care and specialized visits.

[5]Eligibility criteria for private hospitals included formal registration, minimum bed capacity, equipped and staffed to provide key services.

[6]A biometrically lined card that provides access to social programs in the state, including food rationing.

nization, and reproductive health. Subcenters are staffed with Auxiliary Nurse Midwifes (ANMs), which provide maternal and child care. In addition, India's Anganwadi Centers, operating under the Integrated Child Development Services (ICDS) program, are the first point of contact for healthcare for low-income mothers in rural areas. These centers provide immunization and health checkups for children under the age of 6 and pregnant women, as well as supplementary nutrition and referral services when doctors are needed.

Although these systems offer broad access to basic prenatal monitoring and health education, they lack medical doctors, as well as hospital-based obstetric and abortion services. Patients living in rural areas can also access care at the nearest Primary Health Center (PHC), which are staffed with medical doctors; however, for many, these facilities remain prohibitively distant. When PHCs are out of reach, rural inhabitants frequently turn to private healthcare facilities for care. The introduction of BSBY therefore represents a significant expansion in access to women's healthcare, extending coverage from basic community services to comprehensive hospital care.

2.3 Abortion

Abortion has been legal in India since 1971, and is permitted up to 20 weeks gestation under specific circumstances, with services delivered through certified government facilities and approved private clinics. Access to abortion was further expanded in 2002 allowing for use of medical abortion pills outside certified facilities, provided by certified providers (Stillman et al., 2014). A detailed abortion study across six northern India states reports that the majority of the facilities providing abortions are in the private sector, even though the Ministry of Health and Family allows all public sector facilities and primary health centers to provide abortions (Singh et al., 2018). For this reason, the majority of abortions reported in healthcare facilities are provided in private facilities and many are provided by informal sector providers, posing significant health threats for women.

While abortion is supposed to be free in public healthcare facilities, the average cost incurred by women in Rajasthan in 2005 varied between ₹200 to ₹1500 at public facilities, while reaching over ₹2000 in private facilities (Iyengar, Iyengar, and Suhalka, 2005). More recently, the cost is estimated to be between ₹500 and ₹5,000. Abortion costs vary depending on how far along women are in the pregnancy, and whether an abortion is a sex-selective abortion, for which clinics charge more (Duggal, 2004). Many women in India report barriers to abortion that include costs, distance, and difficulties to reach a health facility (Singh et al., 2018).

3 Data

3.1 Dataset

To examine fertility, sex selection and mortality, I use National Family and Health Survey (NFHS) waves conducted in 2015/2016 and 2019/2021. NFHS waves for the years 2015/16 (NFHS-4) and 2019/2021 (NFHS-5) are geolocated and sub-nationally representative household surveys.[7] The NFHS is a large scale survey of health and fertility of women aged 15-49. It is ideal to compute fertility measures as it provides the full fertility history of the women interviewed, including the date of birth of their children, their gender, the date of death of the children that are not alive at the time of the survey, and a detailed contraceptive use calendar. The surveys also report socioeconomic characteristics of the mothers and the household they are born into. NFHS also incudes data on children's prenatal and postnatal care and various measures of the child's health. These questions are however asked only for the children born between the time of the survey and five years prior to the survey.

I construct three separate datasets. First, to estimate the effects of the policy on fertility and sex-selection, I build a retrospective woman-quarter panel covering the period 2010-2019 using NFHS-5. For each quarter of the year the panel includes information on whether the woman gave birth to a child, its gender, and its birth order. I restrict the sample to women in Rajasthan that by the interview date are not childless, excluding women who have had twins or multiple births in the same date since these births are unplanned and do not reflect parent's fertility preferences. I also exclude women that are not residents of the household they are interviewed at, since there is no information on their residence location.

While after December 2015 all eligible women gained access to free healthcare, the proximity to their closest free hospital depends on the date each hospital joined this scheme. Figure 1(a) and 1(b) plot the spatial distribution of the NFHS-5 clusters and the hospitals under the insurance scheme in December 2015 and October 2019. To leverage this variation, I construct a time-varying measure of access to free healthcare for each NFHS-5 cluster. Specifically, for each month between December 2015 (the start of the BSBY rollout) and October 2019, I calculate the driving distance from the centroid of each NFHS-5 cluster to the nearest hospital empaneled under BSBY at that point in time.[8]

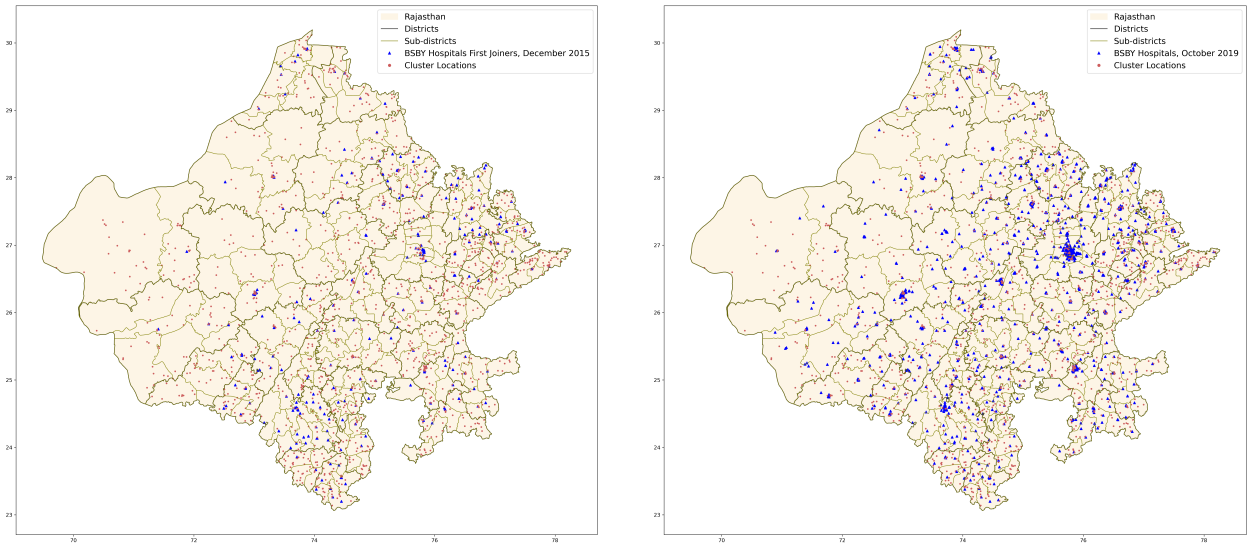
This measure is time-varying because it updates as new hospitals join the scheme, a cluster's distance to the nearest free hospital shrinks whenever a closer hospital becomes

[7] The previous waves are not geolocated nor representative at the sub-national level, therefore are excluded from this analysis.

[8] Driving distances between cluster centroids and empaneled hospitals are computed using the `OSMnx` Python library, which constructs road networks from OpenStreetMap data and calculates shortest-path travel distances based on local road type and speed.

empaneled. I then collapse this monthly measure to an end-of-quarter measure and merge it with the woman-quarter panel. This allows me to leverage the spatial and temporal variation in proximity to a free hospital to understand how reductions in travel times, associated with monetary and time costs, shape fertility and sex-selection after the policy implementation. The final sample comprises 27,317 women observed between 2010 and the end of 2019.

Figure 1: Geographic Distribution of BSBY Hospitals and NFHS-5 Clusters



(a) Map of BSBY Hospitals (2015)

(b) Map of BSBY Hospitals (2019)

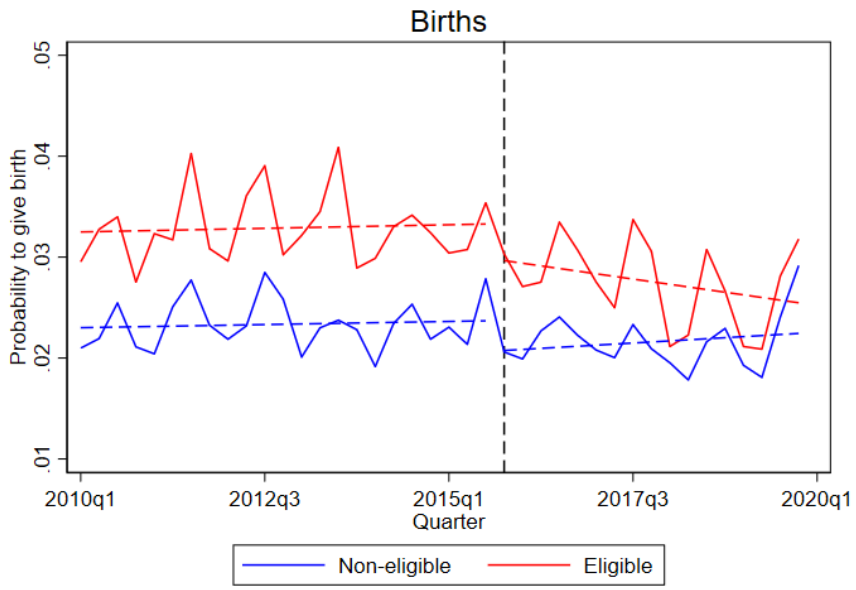
Second, I use all births in Rajasthan from NFHS-4 (2015/2016) and NFHS-5 (2019/2021) to construct a child-level dataset. This dataset allows me to explore the effects of the policy on the probability that a child is female (conditional on being born), prenatal care, and child mortality. The final sample of births comprises 152,954 births from 54,418 women. Lastly, I poll all the selected women interviewed in Rajasthan in NFHS-4 and NFHS-5 to construct a cross-sectional dataset at the woman level. I use this dataset to assess the effect of the policy on completed fertility.

3.2 Descriptive Statistics

Since BSBY was the precursor to AB-PMJAY, which was rolled out in the rest of India in August 2018, other Indian states cannot serve as a valid control group. Fertility responds slowly, making it unfeasible to restrict the analysis to the pre-2018 period. Instead, I use ineligible women – those above the bottom 40% of the income distribution – in Rajasthan as a control group. NFHS does not ask about the income of the households interviewed, but provides a wealth index, classified in five quintiles, which I use as as a proxi for eligibility to

the program. All women with a wealth index in the bottom 40% of the distribution in the state are treated as eligible. Table A1 presents sample means separately for the treatment and control groups. Women in the treatment and control groups are similar in terms of age, age at first birth, and marital status. For both groups, the majority is of Hindu religion. Eligible women have, on average, more children and they report a higher fertility desire, and both groups report a higher ideal number of sons than daughters. Eligible women disproportionally live in rural areas.

Figure 2: Probability to give birth by quarter (raw data)



Specifically, I leverage within-cluster variation in eligibility of women and the timing of the policy to estimate the causal effect of BSBY on fertility. This approach accounts for all time-invariant cluster-specific factors that may affect the outcome variables, including baseline differences in rural versus urban composition, socioeconomic characteristics, and access to local health infrastructure. Nevertheless, to ensure that my estimates are not confounded by differences in individual characteristics between eligible and non-eligible women, I gradually include time invariant individual characteristics (caste and religion), woman's age, and the number of children at baseline. I omit controls for other socio-economic characteristics as they are measured at the time of the survey and could induce a "bad control" problem as characteristics such as education and marital status might be affected by the policy.

To ensure that non-eligible women serve as an adequate control group, I plot the raw data for the probability of giving birth in each quarter for women in the sample by eligibility status in Figure 2. The raw data suggests parallel trends prior to the policy introduction and

a convergence after the policy, with eligible women reducing their likelihood of giving birth. Figure A1 in the Appendix shows the same pattern when the probabilities are separated by gender of the children born.

4 Empirical Strategy

The goal of this paper is to estimate the causal effect of BSBY on fertility and sex-selection. To estimate the impact of free healthcare provision, I exploit the quasi-experimental nature of BSBY. Specifically, I compare eligible women (those belonging to low-income households) to non-eligible women within Rajasthan, exploiting within-cluster variation over time in a differences-in-differences framework.

4.1 Fertility

BSBY was implemented in December 2015, at which point all public hospitals were automatically empaneled and thus became free for eligible households, while private hospitals could join the scheme upon an application, creating a network of free healthcare providers that gradually expanded over time. I begin comparing eligible to non-eligible individuals before and after the policy introduction. Because eligibility is proxied using the NFHS wealth index, the estimates should be interpreted as intention-to-treat (ITT) estimates. I estimate the following specification using the retrospective woman-quarter panel I construct from NFHS-5 to identify the causal effect of the policy on fertility and sex selection.

$$Y_{ict} = \beta Eligible_i * Post_t + \gamma Eligible_i + \alpha_c + \delta_t + \epsilon_{idt} \quad (1)$$

Y_{idt} is the probability for woman i in cluster c to give (i) birth, (ii) birth to a son, or (iii) birth to a daughter, respectively, in the quarter of the year t. $Eligible_i$ is a dummy equal to 1 if the woman's wealth index corresponds to the bottom 40% of the income distribution of the state of Rajasthan. $Post_t$ is a dummy equal to 1 for all the quarters after the fourth quarter of 2015, which is omitted as it is absorbed by the quarter-year fixed effects. Finally, I account for any cluster-specific time-invariant factor including cluster fixed effects (α_c), and for any time-specific shocks, including seasonal birth variation, by including quarter-year fixed effects (δ_t). Identification therefore relies on within-cluster variation in eligibility status, comparing outcomes between eligible and non-eligible women residing in the same cluster, net of any cluster-level characteristics and aggregate time trends.[9] In alternative specifica-

[9]The woman-quarter dataset comprises 27,317 women across 1,485 clusters. The panel is balanced, and the large majority of clusters contain both eligible and ineligible women. On average, clusters include 7.9

tions, I include district-quarter-year fixed effects to account for any time-varying unobserved factors at the district level that could affect fertility trends, as well as time-invariant woman characteristics (caste and religion), age, and number of children in the quarter before the policy was introduced. Standard errors are clustered at the cluster×eligible level.

The β coefficient captures the average treatment effect on the treated (ATT) using a TWFE estimation approach, given that the treatment adoption is not staggered (Roth et al., 2023). Identification of β relies on the assumption that, in the absence of BSBY, the fertility trajectory of eligible women would have evolved following the same trend as the control group. Because the policy specifically targets low-income families, differential fertility trends by income could be a concern. Adding to the evidence provided by the raw data shown in Figure 2, I provide additional empirical support of lack of pre-trends through an event study specification, where I allow the $Post_t$ coefficient to vary by quarter of the year.[10] This specification also allows to observe the dynamic effects of the policy.

Additionally, to explore heterogeneous effects by parity, I implement a triple-differences (DDD) approach that exploits heterogeneity by parity, comparing changes in fertility across parity groups among eligible women after the policy, relative to the same parity differences among non-eligible women. This strategy differences out any income-specific shocks common across income groups, as well as any parity-specific shock common across all income groups, thereby strengthening identification. The identifying assumption in this case is that, absent the reform, parity-specific fertility trends would have evolved similarly for eligible and non eligible-women.

4.2 Completed Fertility

To investigate changes in total fertility, I employ the woman cross-section dataset created from NFHS-4 and NFHS-5. I restrict the sample to women aged 35 and older at the time of the interview and implement the following specification:

$$Children_{iw} = \alpha + \beta Eligible_{iw} * Post_w + \gamma Eligible_{iw} + X_i'\lambda + \rho_d + \theta_w + \epsilon_i \quad (2)$$

eligible and 11.3 ineligible women. The number of clusters composed entirely of eligible women is 66, and entirely of ineligible women is 178; together, these single-type clusters represent 16% of the sample, meaning that 84% of clusters contribute within-cluster variation to the identification.

[10] The event-study specification is:

$$Y_{ict} = \sum_{\substack{k=\text{Q1,2010},\\ k\neq\text{Q4,2015}}}^{\text{Q4,2019}} \beta_k \left(Eligible_i \times 1\{t=k\}\right) + \gamma\, Eligible_i + \alpha_c + \delta_t + \epsilon_{ict}$$

where $1\{t=k\}$ is an indicator for k quarters

Where $Children_i$ is the "stock" of children of woman i, Post is a dummy equal to one for the women in the NFHS-5 interview wave, and $X_i'\lambda$ is a vector of the woman's individual characteristics. Because the clusters of the two NFHS waves are independent, I am unable to include cluster fixed effects in this specification. Therefore, I firs include district fixed effects (ρ_d) and wave fixed effects. Additionally, to resemble the specification controlling for clusters fixed effect, I geocode the position of the clusters and create a *municipality* variable that provides a finer level of aggregation than the district variable present in the NFHS (Figure A2 in Appendix shows the spatial distribution of the clusters from the two NFHS waves).[11] Taken together, these specifications capture the effect of BSBY on the "stock" and "flow" of children among the eligible population, i.e., low-income women.

4.3 Child Mortality and Health

Beyond its effects on fertility and sex selection, BSBY could also influence child health in multiple ways. By expanding access to free healthcare, the program may have increased the use of prenatal screenings which can improve birth outcomes through timely interventions. Postnatal care may also have improved, as the policy reduced the financial and logistical barriers to hospital visits for infants, enabling faster treatment of conditions such as infections, respiratory distress, or health complications. These improvements in access and utilization of healthcare services could ultimately affect infant and early-childhood mortality, as well as the overall health and survival of children.

Child mortality may also decline as a result of increased access to abortion services, particularly female infant mortality. If the policy facilitated access to prenatal diagnostics and the termination of unwanted female pregnancies, the girls who are carried to term may be more likely to be wanted and consequently receive greater parental investment, consistent with prior evidence on sex selection (Anukriti, Bhalotra, and Tam, 2022; Lin, Liu, and Qian, 2014). In this case, reductions in female infant mortality would not only reflect improvements in healthcare access but also a compositional effect: selection into birth leads to a positively selected group of surviving girls who receive more equal treatment.

I estimate the effects of the policy on infant mortality using the following specification:

$$Y_{imdt} = \alpha + \beta Eligible_i * Post_t + \gamma Eligible_i + X'_{mdt}\lambda + Z'_{imdt}\varphi + \rho_d + \omega_t + \epsilon_{imdt} \quad (3)$$

where Y_{imdt} is the health outcome of child c, born from mother m, captured by infant mortality and other health outcomes. Infant mortality at birth and under one year is mea-

[11] I obtain 207 municipalities out of 34 districts across Rajasthan.

sured using dummy variables equal to one if the child died at the corresponding age interval and 0 otherwise. X'_{mdt} is a vector of mother characteristics as in the above specifications, and Z'_{mdt} is a vector of children's characteristics (birth order and gender). The rest of the specification is as in specification (2).

4.4 Mechanism: distance to the nearest free clinic

BSBY is an interesting policy to explore because its design first made hospitals free, but over time made free hospitals geographically closer to some people than others. While all public hospitals were automatically empaneled in December 2015, private hospitals gradually joined the scheme, expanding the network of free facilities and reducing travel distances for eligible households. Accessing healthcare in India entails both monetary and non-monetary costs, including transport, foregone wages, time, and potential safety risks, barriers that are especially binding for low-income women in rural areas. To capture the role that reducing distances played in facilitating access to healthcare that is now costless, I exploit changes in the distance to the nearest healthcare facility. Importantly, these are pre-existing facilities that were available before the policy, but were not *free*. What this specification is capturing is the availability of healthcare that is both free and locally available. I implement the following specification:

$$Y_{ict} = \beta Eligible_i * Distance_{ict} + \gamma Eligible_i + \theta Distance_{ict} + \alpha_c + \delta_t + \epsilon_{idt} \quad (4)$$

where *Distance* is equal to the driving distance in kilometers, divided by 10 for ease of interpretation, to the nearest free healthcare facility from the centroid of cluster c. For all the quarters before the policy introduction the distance to a free healthcare facility is imputed to 210 km (which is slightly higher than the maximum distance in the first treated quarter $\sim$ 205 km), as there was no full healthcare coverage for eligible women before the policy introduction. The rest of the specification is as described above for Equation (1).

5 Results

In this section, I present the main results on the impact of BSBY on fertility and sex-selective behavior. I begin by examining the effects on overall fertility and completed fertility, and then turn to the policy's impact on sex selection, robustness checks, and heterogeneity analysis. I conclude with results on facility deliveries and mortality at birth and under age one.

5.1 Fertility

This section presents the main results on the effect of BSBY on fertility and traces the dynamic effects of the policy over time. Column 1 of Table 1 reports estimates from the baseline specification (Eq. 1), which includes cluster and quarter fixed effects. The coefficient of interest, Eligible×Post, captures the differential change in fertility among eligible women following the introduction of free healthcare, relative to ineligible women in the same cluster. Cluster fixed effects absorb time-invariant differences across communities ensuring that the estimate is identified from within-cluster variation in eligibility status. Quarter of the year fixed effects control for common seasonal patterns in fertility that affect all women equally. Together, these fixed effects ensure that the estimated effect is not confounded by systematic differences across clusters or common seasonal fluctuations in fertility, and instead isolate the differential behavioral response of eligible women to the introduction of the program. After the policy introduction, eligible women experience a reduction in the likelihood of giving birth in an given quarter by 0.38 percentage points, which corresponds to a 15% decrease.

The estimated coefficient is robust to the inclusion of controls (Column 2), the addition of district-by-quarter fixed effects (Columns 3-4), and the inclusion of individual fixed effects (Column 5). The latter represents a particularly demanding alternative specification: since individual fixed effects absorb all individual time-invariant characteristics, the stability of the coefficient confirms that the estimated effect is not driven by systematic fixed differences between eligible and ineligible women, but rather reflects a behavioral change among eligible women following the introduction of the free healthcare program.

To further provide evidence of parallel trends and analyze the evolution of the outcome over time, Figure 3 reports results for an event-study specification, where the post coefficient is allowed to vary quarter-by-quarter. Prior to the policy, no notable difference between eligible and non eligible women can be observed, further providing evidence of parallel trends. Following the policy introduction, a gradual decrease in fertility can be observed over time. The drop in fertility becomes pronounced and statistically significant starting in 2018, two years following the implementation of the policy. This timing is consistent with the inherently slow adjustment of fertility outcomes.

5.2 Completed Fertility

Next, I estimate the effect of BSBY on the "stock" of children. Table 2 displays estimates for the specification in equation (2) on the sub-sample of women that were over 35 at the time of their interview. Columns 1-2 report results using district fixed effects and wave fixed effect, while columns 3-4 report results using municipality fixed effect, which is the finest

Table 1: Probability to give birth

	Any birth of a child				
	(1)	(2)	(3)	(4)	(5)
Eligible=1	0.0071***	0.0046***	0.0071***	0.0046***	
	(0.000)	(0.000)	(0.000)	(0.000)	
Eligible=1 × Post=1	-0.0038***	-0.0038***	-0.0039***	-0.0039***	-0.0039***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Controls		x		x	
Cluster FE	x	x	x	x	
Quarter-Year FE	x	x			
DistrictxQuarterxYear FE			x	x	x
Woman FE					x
R-squared	0.004	0.014	0.005	0.016	0.032
Mean Dep. Variable	0.026	0.026	0.026	0.026	0.026
Mean Dep. Variable (Eligible, Pre)	0.033	0.033	0.033	0.033	0.033
Observations	1092680	1092680	1092680	1092680	1092680

Notes: This table reports the coefficients from specification (1), the unit of observation is woman-quarter. Covariates include woman's age, caste, religion, and the number of children in 2015. *Post* equals one for all quarters after Q42015, and zero otherwise. *Eligible* equals one for women in the bottom 40% of the income distribution. Standard errors clustered at the eligible × cluster level are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Figure 3: Event study estimates of the probability to give birth by quarter



Notes: This figure provides estimates for the dynamic effect of BSBY on the probability to give birth at each quarter from 2014Q1 to 2019Q4. Coefficients shown are β_k Eligible×quarter. Standard errors are clustered at the eligible × cluster level level. Bars represent 95% confidence intervals.

level of aggregation that can be obtained when NFHS-4 and NFHS-5 are merged, as clusters between the waves are independent. The policy led to a reduction in the number of children among women over 35, those most likely to have completed their fertility. The magnitude of the coefficients ranges from -0.20 to -0.24 depending on the specification, corresponding to a decline in total number of children of about 7%. This suggests that the observed decline in fertility is not driven by women postponing childbearing as a results of the policy, but rather reflects a reduction in overall fertility. As an robustness check, I repeat the analysis limiting the sample to women sterilized at the time of the interview, and find that total fertility also declines for this group (see Appendix Table A3). Overall, the results align with the patterns observable in the raw data (Figure 2). BSBY appears to have narrowed the gap in fertility behavior between low-income households and the rest of Rajasthan, suggesting that the program accelerated the fertility transition in the state.

Table 2: Total children, sample restricted to women older than 35

	Total Children			
	(1)	(2)	(3)	(4)
Eligible=1	0.810***	0.810***	0.729***	0.729***
	(0.039)	(0.039)	(0.038)	(0.038)
Eligible=1 × Post=1	-0.234***	-0.234***	-0.198***	-0.198***
	(0.049)	(0.049)	(0.048)	(0.048)
Controls		x		x
Wave FE	x	x	x	x
District FE	x	x		
Municipality FE			x	x
R-squared	0.102	0.102	0.126	0.126
Mean Dep. Var.	3.418	3.418	3.418	3.418
Mean Dep. Var. (Pre, Eligible)	4.134	4.134	4.134	4.134
Observations	25949	25949	25874	25874

Notes: This table reports the coefficients from specification (2), using the cross-section of women from NFHS-4 and NFHS-5. Sample is limited to women older than 35 at the time of the interview. Covariates include woman's age, caste, religion, and the number of children in 2015. *Post* is equal to 1 for all women in NFHS-5, and zero otherwise. *Eligible* equals one for women in the bottom 40% of the income distribution. Standard errors clustered at the eligible × cluster level are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

5.3 Fertility & Son Preference

Having shown that the policy decreased fertility, it is essential to understand whether this decrease in fertility affected stopping rules and sex selective abortions. Following Jayachan-

dran (2017), we would anticipate an increase in sex-selection. Table 3 reports specification (1) estimated separately for the probability to give birth to a boy and a to girl. The estimated coefficient for the probability to give birth to a female following the policy is about twice as large as that for the birth of a son. The policy led to a reduction in the probability of a male birth by 9% and that of a female birth by 22% relative to the overall mean. Relative to the baseline mean of the eligible group, the corresponding reductions are 8% and 16%, respectively. These results are robust to the inclusion of control variables, district-quarter-year fixed effects, and woman's fixed effects. A Wald test of the Eligible×Post coefficients for birth of male and birth of female confirms that the coefficients are statistically different from each other at the 10% level.

It is well established that the probability of giving birth to a male child is naturally, without any sex selection, slightly higher at 51.5%.[12] However, the baseline mean of the dependent variable for the eligible group suggests that sex selection was widespread at baseline, resulting in a probability of male birth exceeding the natural rate (Table 3). While the comparison with baseline birth probabilities by gender already provides evidence of behavioral change, I provide further evidence of this shift in sex-selective behavior following the policy by computing the expected coefficients for male and female births implied by the observed fertility decline of 0.38 percentage points, given the baseline probability of a birth by gender. Given the estimated fertility decline, the expected coefficient for a male birth is -0.0020 (-0.0038×0.526), and for a female birth it is -0.0018.[13] This simple back-of-the-envelope calculation highlights two key points. First, the much larger decline in female births cannot be explained by the fertility reduction alone. Second, the gap also cannot be explained by baseline sex-selection among eligible women. This therefore implies that there is an increase in in sex-selective behavior due to the policy.

[]

Figure 4 shows the evolution over time of the probability of giving birth separately by child's gender. The results reveal a sustained decline in the probability of a female birth among eligible women from 2018 onward, while the probability to give birth to a son fluctuates and significantly drops only a year later, in 2019. This timing is consistent with natural and logistical constraints: it takes nine months to carry a pregnancy to term, and if families engage in sex-selective abortions, the process requires at least one year.[14]

[12] This is due to the fact that male fetuses are naturally more likely to survive in the womb and be carried to term.

[13] The probability to give birth to a son for eligible women at baseline is 0.528, which is calculated as $\frac{P(birthmale|pre,eligible)}{P(birth|pre,eligible)} = \frac{0.0327}{0.0172}$

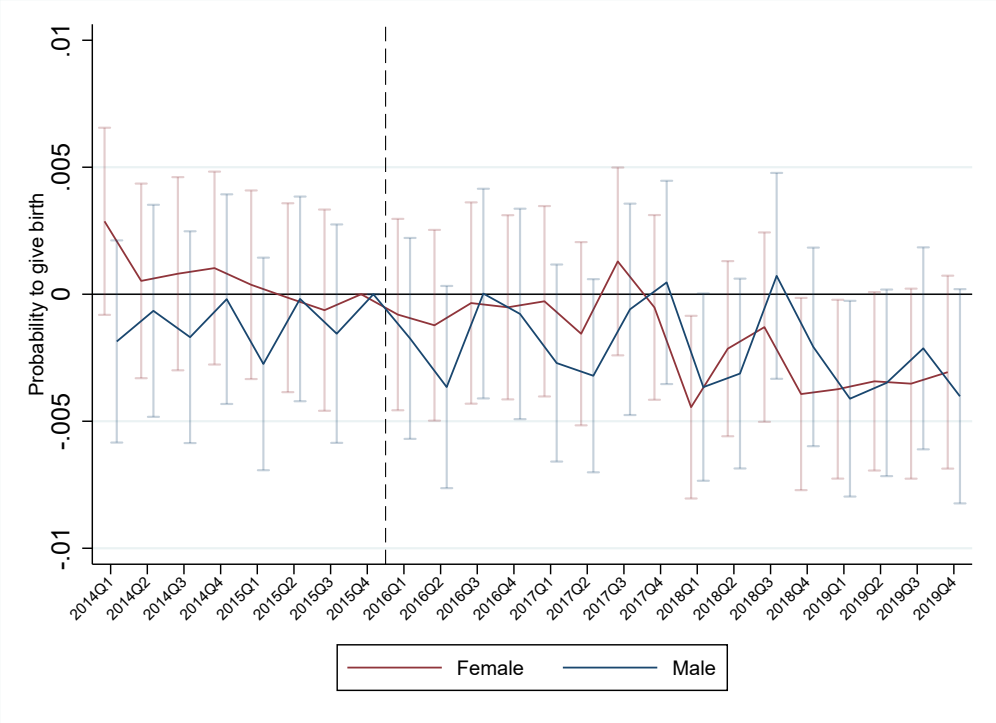
[14] It takes about 12 weeks to detect the sex of the fetus and make a decision regarding abortion. After the abortions, 2-3 months of recovery time are needed before attempting a new pregnancy which, if successful, results in a subsequent pregnancy.

Table 3: Probability to give birth by gender

	Any birth of a son				Any birth of a daughter			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Eligible=1	0.0034***	0.0023***	0.0023***		0.0037***	0.0023***	0.0023***	
	(0.000)	(0.000)	(0.000)		(0.000)	(0.000)	(0.000)	
Eligible=1 × Post=1	-0.0013***	-0.0013***	-0.0013***	-0.0013***	-0.0025***	-0.0025***	-0.0026***	-0.0026***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Controls		x	x			x	x	
Cluster FE	x	x	x		x	x	x	
Quarter-Year FE	x	x			x	x		
DistrictxQuarterxYear FE			x	x			x	x
Woman FE				x				x
R-squared	0.002	0.008	0.009	0.025	0.003	0.008	0.009	0.032
Mean Dep. Var.	0.014	0.014	0.014	0.014	0.012	0.012	0.012	0.012
Mean Dep. Var. (Eligible, Pre)	0.017	0.017	0.017	0.017	0.016	0.016	0.016	0.016
Observations	1092680	1092680	1092680	1092680	1092680	1092680	1092680	1092680

Notes: This table reports the coefficients from specification (1), the unit of observation is woman-quarter. Columns (1)-(4) report the likelihood to give birth to a male in a quarter, columns (5)-(8) report the likelihood to give birth to a female. Covariates include woman's age, caste, religion, and the number of children in 2015. *Post* equals one for all years quarters after Q42015, and zero otherwise. *Eligible* equals one for women in the bottom 40% of the income distribution. Standard errors clustered at the eligible × cluster level are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Figure 4: Event study: probability of birth by gender



Notes: This figure provides estimates for the dynamic effect of BSBY on the probability of giving birth to a male and a female separately. Coefficients shown are β_k Eligible×quarter. Standard errors are clustered at the eligible × cluster level level. Bars represent 95% confidence intervals.

5.4 Robustness

The baseline results are robust to a range of alternative specifications and robustness checks. First, I aggregate the data at the yearly level to reduce noise due to seasonal patterns in fertility; the results are unchanged (see Appendix Figure A3 and Table A4). Second, I perform a placebo test, extending the yearly panel going back to 2006 and focusing on pre-2016 years, assigning treatment in 2011; consistent with expectations, this exercise yields no detectable effects (see Appendix Figure A4 and Table A6). Third, I estimate the probability that a child is female using the children dataset, which pools all births recorded in NFHS-4 and NFHS-5 and therefore spans a longer time horizon than the main analysis. This allows me to assess whether the decline in the probability of a female birth is detectable across a broader time window, rather than being specific to the sample years used in the baseline specification. The results confirm the main findings: following the introduction of BSBY, the probability that a birth is female among eligible women declined by 3.5 percentage points relative to ineligible women, a result that is statistically significant (Appendix Table A7).

Lastly, since I proxi eligibility to BSBY with NFHS wealth quintiles, a potential concern is that this could be an imperfect proxy for program eligibility. Measurement error in the wealth index near the threshold may mean that some of the women that are attributed a middle wealth index were instead able to qualify for coverage, contaminating the control group and attenuating my estimates toward zero. The main estimates should therefore be interpreted as a conservative lower bound on the true treatment effects. To assess the extent of this attenuation, I re-estimate the main specification restricting the sample to households clearly on either side of the threshold, dropping all middle-wealth households. The estimated effects grow slightly in magnitude, consistent with mild downward bias in the main specification (Appendix Table A5). When excluding women in the middle wealth index, the probability of a male birth falls by 12% and that of a female birth by 24% relative to the overall mean, with corresponding declines of 11% (male births) and 20% (female births) relative to the baseline mean for eligible women.

5.4.1 Heterogeneity: Birth Order

Next, I consider a key source of heterogeneity in the impact of free healthcare on sex-selection: birth order. I leverage the children dataset with all births from NFHS-4 and NFHS-5 to look at the probability that a child is a girl. I test for birth-order-specific effects by interacting the treatment variable with three indicators for the birth order of the children: (i) parity 1, (ii) parity 2, and (iii) parity three and up. The coefficients for $Eligible \times Post$ capture the effect of the policy for first births. The results indicate that the decrease in female births is

Table 4: Child is a girl, heterogeneity by birth order

	All births			
	(1)	(2)	(3)	(4)
Eligible=1 × Post=1	-0.030**	-0.030**	-0.033**	-0.033**
	(0.015)	(0.015)	(0.016)	(0.016)
Eligible=1 × Post=1 × Bord=2	-0.005	-0.005	-0.004	-0.004
	(0.024)	(0.024)	(0.025)	(0.025)
Eligible=1 × Post=1 × Bord=3	-0.005	-0.005	-0.004	-0.004
	(0.023)	(0.023)	(0.024)	(0.024)
Controls		x		x
District FE	x	x		
Year FE	x	x		
District-Year FE			x	x
Municipality FE			x	x
R-squared	0.001	0.001	0.011	0.011
Mean Dep. Variable	0.468	0.468	0.468	0.468
Mean Dep. Variable (Pre,Eligible)	0.476	0.476	0.476	0.476
Observations	151795	151795	151769	151769

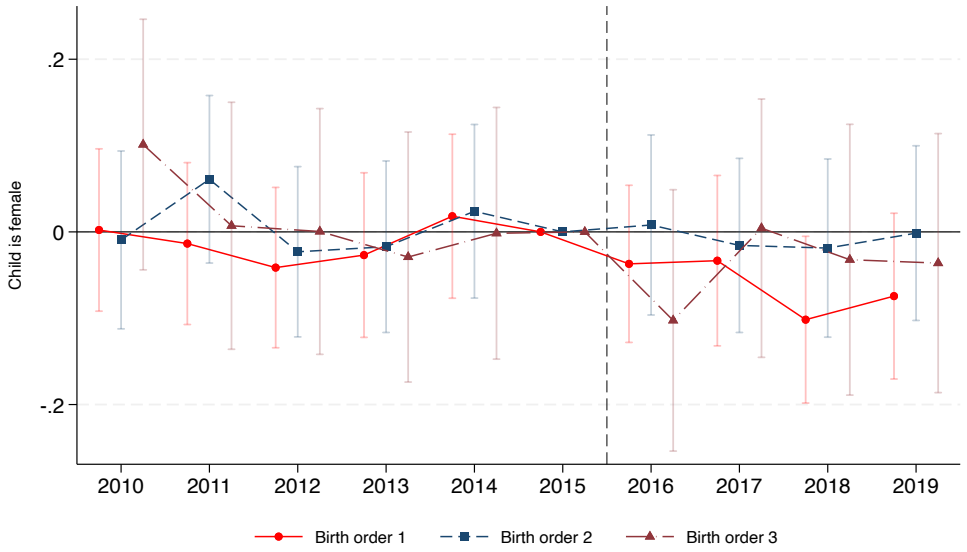
Notes: the outcome variable equals one if the child is female, 0 otherwise. Sample is all births from NFHS-4 and NFHS-5. Covariates include woman's age, caste, religion, and the number of children in 2015. *Post* equals one for all years quarters after 2015, and zero otherwise. *Bord* is a categorical variable equal to 1 for parity one, 2 for parity two, and 3 for parity three and up. *Eligible* equals one for women in the bottom 40% of the income distribution. Standard errors clustered at the eligible × cluster level are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

uniform across all births (Table 4). To avoid potential bias arising from inaccurate recall of births further away in time from the time of the interview, Table A8 in Appendix replicates the analysis restricting the sample to births occurring after 1990, the results are unchanged.

To further examine the decline in female births by parity, I estimate an event study separately for first, second, and births of parity three and up. Figure 5 shows the dynamic of the drop in births by parity. The negative and statistically significant effect of eligibility on fertility is concentrated among first births. For second and third births, the estimated coefficients are negative but imprecisely estimated, suggesting no meaningful policy-induced reduction in higher-order births. This pattern is consistent with sex-selective abortions being practiced primary at parity one as a direct consequence of the policy.

These results carry two important implications. First, while disentangling sex-selective abortions from stopping rules is empirically challenging, the observed decline in the probability that a firstborn child is female points clearly to sex-selective abortion as the operative mechanism. Stopping rules, by definition, operate through the decision to continue having

Figure 5: Child is a girl, by parity



Notes: This figure provides estimates for the dynamic effect of BSBY on the probability that a birth is female, by parity. Coefficients shown are β_k Eligible×year. Standard errors are clustered at the eligible × cluster level level. Bars represent 95% confidence intervals.

children conditional on the sex of previous births, and therefore cannot account for distortions in the sex ratio at first birth. Second, this paper provides the first evidence that a large-scale policy intervention not explicitly aimed at reducing fertility or sex selection can distort sex ratios at first birth. This stands in contrast to existing work, such as Anukriti (2018), which examines policies that directly targeted both fertility decline and sex selection simultaneously. More broadly, these findings challenge a foundational assumption in the son preference literature: that first births are effectively random with respect to sex. This assumption has been widely relied upon to identify causal effects of economic and policy shocks on son preference, given that sex selection has traditionally been observed only at higher parities. The results suggest that this assumption may no longer hold in India, particularly as the program expanded nationwide through AB-PMJAY in 2018, potentially amplifying the scope for sex-selective behavior at first birth.[15]

[15]Unfortunately, verifying this for the whole of India will not be possible until a new round of NFHS data becomes available.

6 Mechanisms

This section explores several potential mechanisms through which BSBY could have affected fertility and sex-selection. Understanding the mechanisms behind the effect of the policy on sex-selection is essential to design interventions and policies to mitigate the unintended effects of universal healthcare in contexts where son preference remains a strong cultural norm.

6.1 Distance

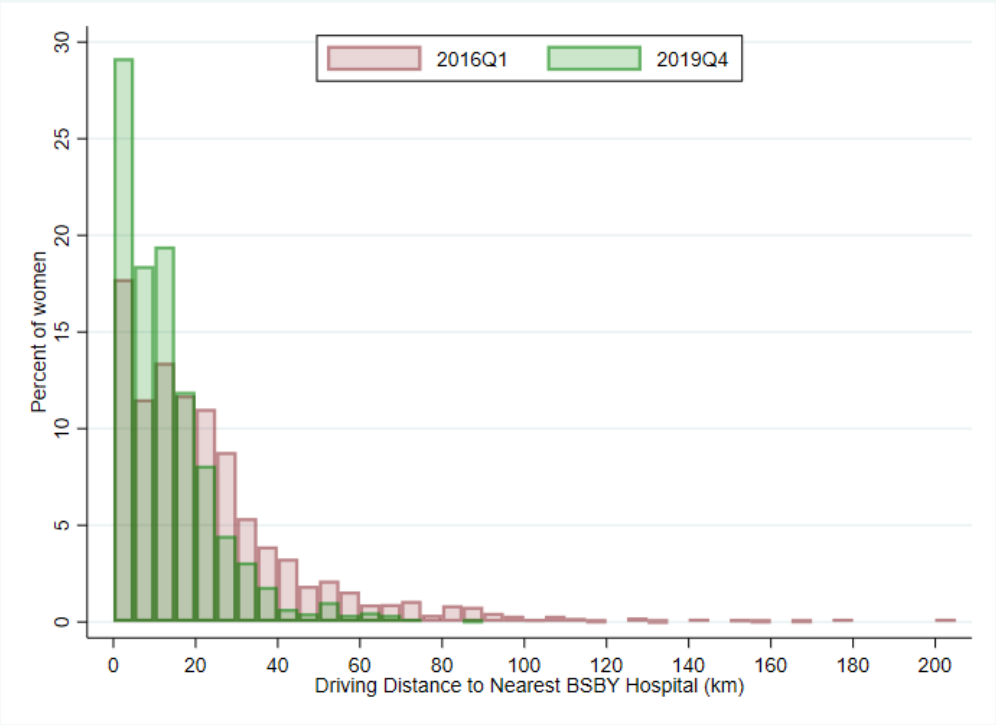
Seeking healthcare entails multiple costs, particularly for low-income women in India. In addition to direct medical expenses, women often face transportation costs and lost wages. Distance also generates substantial time costs and can expose women to safety risks. Longer travel distances can therefore act as a barrier to accessing healthcare, particularly in rural areas where transport infrastructure is limited and mobility constraints are more binding. By making healthcare free at selected facilities, BSBY expanded access in two ways: it removed the financial cost of care and increased effective accessibility for women living closer to these hospitals relative to those farther away.

The policy took effect in December 2015, with all public hospitals automatically enrolled from the start and private hospitals gradually joining over time, progressively expanding geographic coverage. This rollout generates temporal variation in the distance to the nearest free healthcare facility, which differs across women depending on their residence, making some women more exposed to the policy than others, at different points in time. Importantly, the variation in distance that I exploit reflects the distance to the nearest *free* hospital, not the presence of the hospital itself. The hospitals themselves were already present before the policy, but only became free for eligible women starting December 2015. Because hospitals locations did not change over time, the physical distance from a woman's residence to the nearest hospital is fixed over time. However, in this paper, I define *distance* as the distance to the nearest *free* hospital from the time the hospital joins BSBY. To capture the temporal variation from the pre period, I impute a large distance for all the quarters before the policy roll-out in December of 2015, when those hospitals were not free.

Figure 6 compares the distribution of driving distances to the nearest free healthcare facility at the onset of the policy, in December 2015, and in October 2019, corresponding to the final month covered by the hospital enrollment data. The policy significantly reduced the distance to the nearest free clinic over time, with the majority of the reduction happening within the first year (see Figure A5 in Appendix).

Using this variation, I compare fertility outcomes of eligible and non-eligible women

Figure 6: Distribution of driving distances



Notes: This figure shows the distribution of driving distances to the nearest fee healthcare facility for the women in the sample.

within the same cluster, before and after hospitals join BSBY, across women located at different distances from the nearest free hospital. This strategy is analogous to a difference-in-differences design, where the treatment intensity is given by proximity to a hospital that becomes free, which varies across women and over time as private hospitals progressively enroll. Women living closer to a hospital that joins BSBY experience a larger reduction in the effective cost of care than those living farther away, but only among those eligible for the program. The comparison between eligible and non-eligible women within the same cluster absorbs any time-invariant location-specific characteristics since both groups experience the same change in distance but only eligible women benefit from it. The identifying assumption is therefore that, absent the policy, fertility trends would have been similar for eligible and non-eligible women within the same cluster, regardless of their distance to the nearest hospital. This identifying assumption is supported by the results from the event study of the main specification (Figure 3), which compares eligible and non-eligible women within the same cluster before and after the policy and finds no evidence of differential pre-trends in fertility outcomes.

Table 5 shows that as distance to the nearest free hospital increases – that is, as proximity to free care decreases – the likelihood of giving birth in a given quarter increases. A 10

Table 5: Distance to the nearest BSBY Hospital and likelihood of giving birth

	Any birth			Any birth of a son			Any birth of a daughter		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Eligible=1	0.00277***	0.00272***		0.00183***	0.00189***		0.00094**	0.00083**	
	(0.001)	(0.001)		(0.000)	(0.000)		(0.000)	(0.000)	
Distance (10km)	0.00041*	0.00042*	0.00042	0.00026*	0.00021	0.00020	0.00015	0.00022	0.00021
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Eligible=1 × Distance (10km)	0.00021***	0.00021***	0.00020***	0.00008***	0.00008***	0.00007***	0.00013***	0.00013***	0.00013***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Controls		x			x			x	
Cluster FE	x	x	x	x	x	x	x	x	x
Quarter-Year FE	x			x			x		
DistrictxQuarterxYear FE		x	x		x	x		x	x
Woman FE			x			x			x
R-squared	0.004	0.005	0.034	0.002	0.004	0.027	0.003	0.004	0.034
Mean Dep. Variable	0.026	0.026	0.026	0.014	0.014	0.014	0.012	0.012	0.012
Mean Dep. Variable (Eligible, Pre)	0.033	0.033	0.033	0.017	0.017	0.016	0.016	0.016	0.016
Observations	1037318	1037318	1037318	1037318	1037318	1037318	1037318	1037318	1037318

Notes: This table reports the relationship between distance to the closest free healthcare facility (in tens of km) and the likelihood of giving birth to a child (columns 1-3), to a son (columns 4-6), and to a daughter (columns 7-8) separately. The unit of observation is a woman-quarter, observed from 2010Q1 to 2019Q4. All distances before the policy are imputed to be 210km. Covariates include woman's age, caste, religion, and the number of children in 2015. *Eligible* equals one for women in the bottom 40% of the income distribution, and *Distance (10km)* is the driving distance to the nearest hospital (in tens of km) two quarters before the birth of the child. Standard errors clustered at the eligible × cluster level are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

kilometer increase in distance raises the probability of giving birth by 0.8% relative to the mean. Consistent with the main results, the effect differs markedly by sex of the child: a 10 kilometer increase in distance raises the probability of having a son by 0.6% and that of having a daughter by 1%, both relative to their respective means. Women living farther from free care are therefore disproportionately more likely to have a daughter, reflecting reduced access to sex-selective services. The results are robust to the inclusion of controls, district-quarter fixed effects, and woman fixed effects (columns 3, 6 and 9). Similarly to other papers that have exploited changes in distances to health clinics on abortions and fertility (Lindo et al., 2020; Lu and Slusky, 2019), in Appendix I also present results for the so called "as the crow flies" distance; the results are unchanged (Appendix Table A9).[16]

These results point to the decrease in distance as a contributing factor to the observed fertility decline and increase in sex-selection. These results are consistent with Dupas and Jain (2024) that find that households are willing to travel less to obtain female care than for male care, even within the same household. Gender norms, which affect women's mobility and agency, concerns about travel safety for women, and limited resources are all possible obstacles in women's access to healthcare and family planning. By lowering the distance to an a free healthcare facility, this policy not only lowered the monetary cost associated with healthcare – in this context especially prenatal care, family planning, and abortion services – but also lowered the cost associated with traveling to seek healthcare. Proximity to accessible healthcare likely improved timely access to reproductive health services, and enabled women to attend gynecological consultation and visits and return to a facility for an abortion, if needed. By lowering both financial and geographic barriers, improved access made it easier for women to act on fertility preferences.

6.2 Healthcare seeking behavior

In addition to reducing effective distance to care, BSBY may have influenced fertility through a second channel: changes in healthcare-seeking behavior. The decline in overall fertility, together with the increase in sex-selective behavior, is consistent with greater utilization of reproductive health services following the policy. Expanded access to gynecological consultations and prenatal screenings likely enabled women to better monitor pregnancies and make informed decisions, including terminating pregnancies if desired. At the same time, these visits may have increased the opportunity for women to learn the gender of the fetus and act on sex preferences. Although disclosing fetal gender has long been illegal in India – a law was introduced in the early 1990s to curb sex-selective abortions – persistently high sex

[16] "As the crow flies" distance is calculated as geodesic distances using the `geopy` library in Python.

ratios at birth suggest that women are still able to obtain this information, likely due to limited enforcement (Manhoff, 2005).

While the NFHS lacks comprehensive data on overall healthcare utilization, it provides detailed information on prenatal visits for all children under five born to women in the sample. Therefore, I explore whether the policy changed prenatal visits attendance for eligible women. Table 6 shows that the policy substantially increased the number of prenatal visits. The number of prenatal visits rose by 17% compared to the overall mean and by 26% compared to the pre-policy mean for eligible women. This result is particularly noteworthy given that prior to the policy, low-income women were already entitled to certain free prenatal services at Anganwadi centers and public hospitals. Despite the availability of these services, the introduction of BSBY appears to have substantially altered prenatal healthcare seeking behavior. While prenatal visits are observed only for live births and therefore cannot capture the full extent of healthcare utilization – in particular, pregnancies that ended in abortion are unobserved – this result suggests that the policy successfully increased contact between eligible women and formal healthcare facilities, consistent with the hypothesis that greater access to care also expanded access to sex-selective services.

The rise in prenatal screenings could suggest a change not only in the frequency of visits but also in the nature of the services women are accessing. Indeed, looking at the type of healthcare facility where women report having had prenatal visits paints a clear picture of the change in health seeking behavior that BSBY caused. The policy reduced women's reliance on Anganwadi centers, which typically provide lower quality prenatal services delivered by nurses and midwives, and increased their reliance on private sector facilities for prenatal visits by a comparable magnitude (Table 7). No change can be observed for public hospitals. Since Anganwadi centers generally lack physicians, which are generally present at private clinics, the observed reallocation points to a shift toward higher-quality care once it becomes accessible.

This change in healthcare seeking behavior also extends to the kind of prenatal screenings that women report taking advantage of. Figure 7 shows the increase of the reporting of ultrasound use during prenatal visits. There is a clear increase in ultrasound use for pregnancies that were carried to term in the years after the policy came into effect. The policy caused a 24% increase in ultrasound usage with respect to the mean, and a 38% increase with respect to the baseline mean for eligible women (Table A10). The increase in ultrasound visits likely raised the probability that women learned the sex of the fetus and, conditional on strong son preference, engaged in sex-selective behavior. In a context characterized by persistent son preference and a documented decline in female births, improved access to sex-determination technologies is consistent with the observed rise in sex selection. By expanding access to

Table 6: Prenatal Visits

	Number of prenatal visits			
	(1)	(2)	(3)	(4)
Eligible=1	-1.307***	-1.162***	-1.158***	-1.019***
	(0.084)	(0.083)	(0.086)	(0.085)
Eligible=1 × Post=1	0.643***	0.610***	0.577***	0.547***
	(0.113)	(0.113)	(0.123)	(0.124)
Controls		x		x
District FE	x	x		
Year FE	x	x		
District-Year FE			x	x
Municipality FE			x	x
R-squared	0.066	0.072	0.112	0.118
Mean Dep. Variable	3.731	3.731	3.731	3.731
Mean Dep. Variable (Pre,Eligible)	2.483	2.483	2.483	2.483
Observations	19723	19723	19643	19643

Notes: Sample includes all births in the last 5 years of women interviews in NFHS-4 and NFHS-5 . Covariates are woman's age, caste, religion, and child birth order. *Post* is equal to 1 for all years after 2014, and zero otherwise. *Eligible* equals one for all women in the bottom 40% of the income distribution. Standard errors clustered at the eligible × cluster level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10.p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

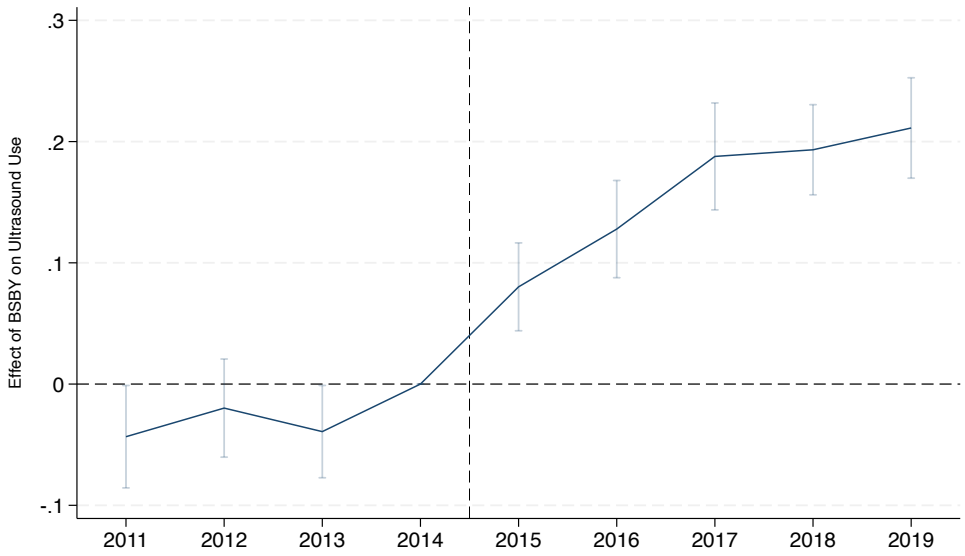
Table 7: Location of Prenatal Visits

	Public Facility		Anganwadi Center		Private Facility	
	(1)	(2)	(3)	(4)	(5)	(6)
Eligible=1	0.014	0.013	0.165***	0.166***	-0.179***	-0.179***
	(0.016)	(0.017)	(0.015)	(0.015)	(0.014)	(0.016)
Eligible=1 × Post=1	0.000	-0.003	-0.044**	-0.053***	0.044***	0.056***
	(0.019)	(0.021)	(0.018)	(0.018)	(0.016)	(0.020)
Controls		x		x		x
District FE	x		x		x	
Year FE	x		x		x	
District-Year FE		x		x		x
Municipality FE		x		x		x
R-squared	0.023	0.075	0.073	0.120	0.057	0.102
Mean Dep. Variable	0.373	0.373	0.325	0.325	0.303	0.303
Mean Dep. Variable (Pre, Eligible)	0.387	0.387	0.400	0.400	0.213	0.213
Observations	17160	17097	17160	17097	17160	17097

Notes: Sample includes all births in the last 5 years of women interviews in NFHS-4 and NFHS-5 . Covariates include woman's age, caste, religion, and number of children. Post equals one for all years after 2014, and zero otherwise. Eligible equals one for women in the bottom 40% of the income distribution. Standard errors clustered at the eligible × cluster level are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

prenatal care, the policy may have inadvertently strengthened families' ability to act on pre-existing gender preferences, thereby amplifying sex-selective practices.

Figure 7: Ultrasound use



Notes: This figure provides estimates for the dynamic effects of BSBY on ultrasound use during pregnancy. Sample includes all births in the last 5 years of women interviews in NFHS-4 and NFHS-5 . Coefficients shown are β_k Eligible×year. Standard errors are clustered at the eligible × cluster level level. Bars represent 95% confidence intervals.

6.3 Contraceptives

An additional mechanism through which free healthcare can reduce births operates via contraceptive use. Barriers to healthcare access constrain the set of contraceptive methods available to women, independent of preferences, as many modern contraceptive methods require repeated clinical visits for initiation, follow-up, or resupply. By removing these barriers, the policy may have shifted women away from traditional methods or non-use toward more effective modern contraception, enabling greater fertility control. Similarly, the increased exposure to prenatal screenings may itself have raised contraceptive uptake, as greater contact with the healthcare system expands access to and awareness of family planning services.

To explore this mechanism, I rely on the same cross-section of women from NFHS-4 and NFHS-5 used to estimate the effect of the policy on completed fertility. One limitation specific to the contraceptive use analysis concerns the timing of the survey waves. The identification relies on a difference-in-differences between two rounds of the NFHS – NFHS 2015/16 and NFHS 2019/21. Unlike birth outcomes and child health indicators, including

prenatal visits, which capture realized events over time through a retrospective panel, offering significant time variation before and after the policy, contraceptive use is a stock variable measured contemporaneously at the time of interview. Since the pre-policy wave was conducted between 2015 and 2016, women interviewed after the policy introduction in 2016 may already report contraceptive behavior reflecting the new healthcare environment, contaminating the pre-period and attenuating the estimates toward zero. Any estimated effects on contraceptive use should therefore be interpreted as a lower bound on the true policy effect. Nevertheless, exploring contraceptive take-up remains informative as healthcare utilization is plausibly a slow-moving margin as habit formation, information diffusion, and provider capacity constraints may mean that contraceptive behavior adjusts gradually, limiting the extent of pre-period contamination.

Sterilization is the dominant method of fertility control in India, historically promoted and at times coercively enforced through mass sterilization campaigns. It has been shown to negatively affect women's health, with harms falling disproportionately on those who are uneducated, from lower castes, in remote areas, or with limited access to quality healthcare (De La Rupelle and Dumas, 2025). Reliance on female sterilization in Rajasthan is above the national level (Jayakumar, 2024). Nearly half of all contraceptive use in the sample is attributed to sterilization: 48 percent of women report it as their current method. All remaining contraceptive methods are considerably less prevalent, with no single alternative accounting for more than 11 percent of the sample. Contraceptive use is especially low among eligible women for both modern methods (excluding sterilization) and condoms. Fewer than 5% of women in the full sample report using modern contraception, and this share falls to 2% among eligible women prior to the policy. Condom use and traditional method use show no change following the policy implementation.

Table 8 presents the effect of the policy on contraceptive use. Panel A covers modern reversible contraception, defined as current use of the pill, IUD, or injectables, Panel B covers female sterilization, Panel C covers condom use, and Panel D covers traditional methods, comprising periodic abstinence, withdrawal, and lactational amenorrhea. The baseline coefficient on eligibility captures pre-policy differences in contraceptive use between eligible and ineligible women. Prior to the policy, eligible women were significantly less likely to rely on modern reversible contraception and condoms relative to ineligible women, while significantly more likely to rely on sterilization. The policy induced an increase in reliance on modern contraceptives and sterilization for eligible women. The increase in reliance on modern contraceptive use is consistent with the policy relaxing the healthcare access constraint that could have previously prevented women from adopting methods requiring multiple facility visits. Sterilization, on the other hand, has long been incentivized and subsidized.

Table 8: Contraceptive methods

	(1)	(2)	(3)	(4)
Panel A: Contraceptives (Pill, IUD, injections)				
Eligible=1	-0.026***	-0.026***	-0.024***	-0.024***
	(0.003)	(0.003)	(0.003)	(0.003)
Eligible=1 × Post=1	0.016***	0.016***	0.016***	0.016***
	(0.004)	(0.004)	(0.004)	(0.004)
R-squared	0.01	0.01	0.01	0.01
Mean Dep. Variable	0.05	0.05	0.05	0.05
Mean Dep. Variable (Pre, Eligible)	0.02	0.02	0.02	0.02
Observations	54418	54418	54240	54240
Panel B: Sterlization				
Eligible=1	0.038***	0.038***	0.033***	0.033***
	(0.009)	(0.009)	(0.008)	(0.008)
Eligible=1 × Post=1	0.038***	0.038***	0.038***	0.038***
	(0.012)	(0.012)	(0.011)	(0.011)
R-squared	0.01	0.01	0.03	0.03
Mean Dep. Variable	0.48	0.48	0.48	0.48
Mean Dep. Variable (Pre, Eligible)	0.49	0.49	0.49	0.49
Observations	54418	54418	54240	54240
Panel C: Condoms				
Eligible=1	-0.074***	-0.074***	-0.067***	-0.067***
	(0.004)	(0.004)	(0.004)	(0.004)
Eligible=1 × Post=1	-0.001	-0.001	-0.004	-0.004
	(0.006)	(0.006)	(0.006)	(0.006)
R-squared	0.03	0.03	0.04	0.04
Mean Dep. Variable	0.11	0.11	0.11	0.11
Mean Dep. Variable (Pre, Eligible)	0.04	0.04	0.04	0.04
Observations	54418	54418	54240	54240
Panel D: Traditional Methods				
Eligible=1	-0.002	-0.002	-0.003	-0.003
	(0.004)	(0.004)	(0.004)	(0.004)
Eligible=1 × Post=1	0.005	0.005	0.006	0.006
	(0.006)	(0.006)	(0.006)	(0.006)
R-squared	0.01	0.01	0.02	0.02
Mean Dep. Variable	0.09	0.09	0.09	0.09
Mean Dep. Variable (Pre, Eligible)	0.07	0.07	0.07	0.07
Observations	54418	54418	54240	54240

Notes: this table reports contraceptive use for all women in the sample from the NFHS-4 and NFHS-5. All regressions include wave fixed effects. Columns (1)-(2) include district fixed effects and columns (3)-(4) include municipality fixed effects. Covariates include woman's age, caste, and religion. *Post* equals 1 for all women interviewed in NFHS-5. *Eligible* equals 1 for women in the bottom 40% of the income distribution. Standard errors clustered at the eligible × municipality level are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

6.4 Abortion & Stopping Rules

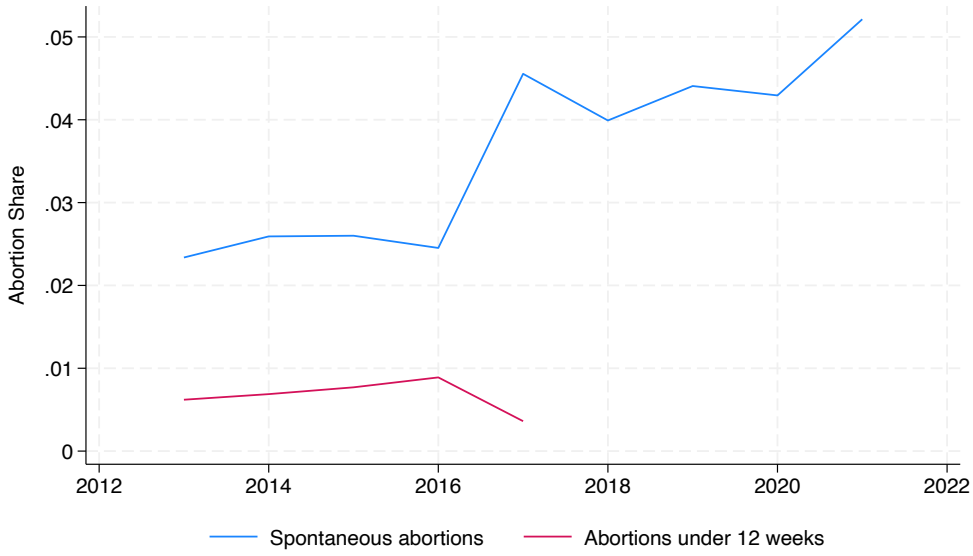
By making abortion free of charge at all empaneled public and private facilities, the policy eliminates the direct monetary cost of terminating a pregnancy and expands access, particularly for women living far away from public hospitals. Although public facilities are intended to provide abortions free of charge, it is often the case that they impose informal fees or do not provide abortions. The inclusion of private hospitals in the scheme both reduces the financial barriers and shortens the distance to a facility where abortion services can be obtained at no cost. This expansion of effective access is likely to alter abortion behavior, especially for women residing in areas where free services were previously unavailable, difficult to access, or unreliable. By lowering both monetary and geographic barriers, the policy increases the feasibility of acting on fertility preferences, including son preference.

Additionally, households face a tradeoff between achieving their desired fertility and their desired number of sons. As desired fertility declines, families have a lower probability of naturally obtaining a son with fewer children, making it more difficult to achieve their desired number sons. In this context, access to sex-selective technologies becomes more consequential. BSBY facilitates women's ability to act on these preferences by expanding access to prenatal care, providing free hospital based abortion services, and reducing the expected healthcare costs associated with abortion-related complications. By lowering both the financial and health risks of terminating a pregnancy, the policy makes it easier for households to reconcile low fertility with persistent son preference.

In Section 5.4.1 I argue that the concentration of the decline in female births among first births points to increased reliance on sex-selective abortions as a direct consequence of the policy. To reinforce this argument, I present additional evidence from the Health Management Information System (HMIS) data on reported abortions, which helps corroborate the patterns observed in the NFHS data. HMIS data are compiled from information uploaded to a national online portal by hospitals across India, where participating facilities report births, deaths, and other health indicators (Asher et al., 2020). The dataset, however, has important limitations: it does not cover the universe of hospitals in India, nor does it constitute a representative sample. The National Health System Resource Center cautions that reporting from private sector facilities might be incomplete and data might be of poor quality (National Health Systems Resource Centre, 2026). Despite these limitations, HMIS data can still be informative for detecting broad changes in health service utilization around the time of the policy.

This additional source allows me to observe trends in reported abortions and pregnancy outcomes, offering complementary evidence on how BSBY influenced reproductive behavior. Figure 8 reports the share of spontaneous abortions, i.e. miscarriages, and medical abortions

Figure 8: Reported Abortions from Healthcare Facilities



Notes: This figure shows raw data from HMIS. Abortion is reported as the share of all reported births, still births, spontaneous and medical abortions.

reported by public and private hospitals as a share of reported pregnancies, which include abortions, miscarriages, still births, and live births. Although the share of medical abortions under 12 weeks accounts for less than 1% of all pregnancies recorded and data is scarce, a decline is visible from 2016 to 2017. Strikingly, this decline coincides with an increase in the share of reported spontaneous abortions. The timing of this increase in registered abortions is consistent with the introduction of the policy, and may reflect changes in reporting practices, with some abortions potentially being recorded as "spontaneous". Such behavior would be consistent with the findings of Jain (2021), who documents that hospitals enrolled in BSBY engaged in coding manipulation following changes in reimbursement rates, pointing to strategic misreporting. These patterns are purely descriptive and rely on incomplete data; they should therefore be interpreted with caution. Nonetheless, they remain consistent with the broader findings of this paper.

An increased reliance on sex-selective abortion does not preclude the role of differential stopping behavior. To test this, I estimate the effect of the policy on the likelihood that the last birth is male. Table 9 shows that after the policy, the likelihood that the last birth is a boy increased by 2.3 percentage points. This indicates that while families are relying on sex-selective abortions to ensure that their first child is a female, they are also adjusting their behavior at subsequent births, stopping with a male at higher rates than before.

Table 9: Stopping rules: likelihood that the last birth is male

	Last Child is a Boy			
	(1)	(2)	(3)	(4)
Eligible=1	0.002	0.002	-0.003	-0.003
	(0.005)	(0.005)	(0.004)	(0.004)
Eligible=1 × Post=1	0.023**	0.023**	0.027**	0.027**
	(0.012)	(0.012)	(0.012)	(0.012)
Controls		x		x
District FE	x	x		
Year FE	x	x		
District-Year FE			x	x
Municipality FE			x	x
R-squared	0.013	0.013	0.034	0.034
Mean Dep. Variable	0.621	0.621	0.621	0.621
Mean Dep. Variable (Pre,Eligible)	0.633	0.633	0.633	0.633
Observations	53478	53478	53468	53468

Notes: the outcome variable equals one if the last child is a male, 0 otherwise. The sample includes all last births from 1990s onwards from NFHS-4 and NFHS-5. Covariates include woman's age, caste, religion, and number of children. *Post* equals one for all years after 2015, and zero otherwise. *Eligible* equals one for women in the bottom 40% of the income distribution. Standard errors clustered at the eligible × cluster level are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

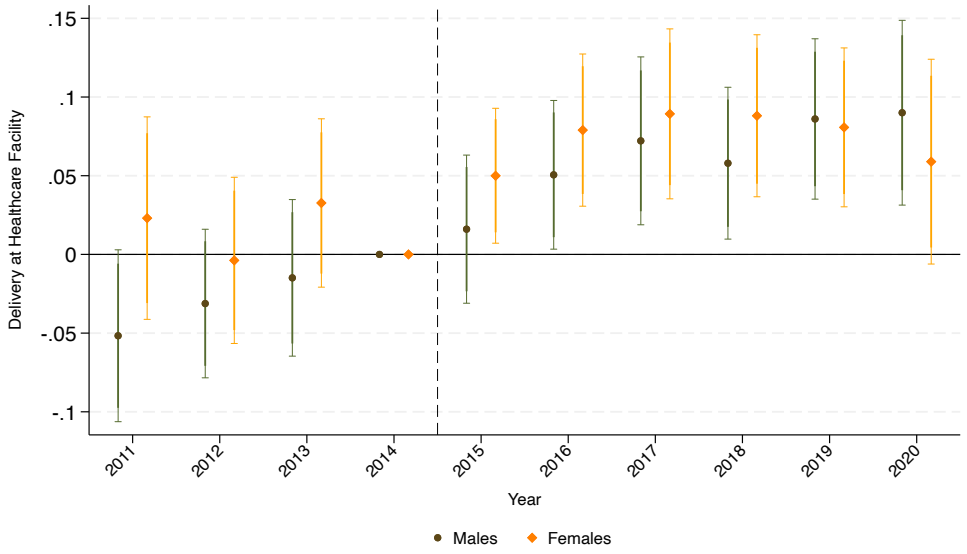
7 Additional Results: Infant Health & Mortality

7.1 Deliveries at Healthcare Facility

Through increased prenatal care, the policy could also have resulted in higher hospital deliveries. Greater access to skilled birth attendants, timely cesarean sections, and better-equipped facilities can directly lower perinatal and neonatal mortality. The state of Rajasthan already provided free deliveries at healthcare facilities before the policy. [17] However, through an overall increase in healthcare seeking and prenatal visits, the program could have further reduced financial and logistical barriers to institutional delivery. Figure 9 illustrates that, following the implementation of BSBY, deliveries at healthcare facilities increased for both male and female births (see Table A11 in Appendix for aggregated deliveries at healthcare facility). While a modest upward pre-trend is observable for male deliveries (not statistically significant) no such pre-trend is detected for female births. Table 10 further details this change in delivery behavior: following the policy, home deliveries fell by 6.5 percentage points, deliveries at public facilities rose by 4.2 percentage points, and those at private facilities increased by 2.3 percentage points. This represents a 26% decline in home deliveries for eligible women compared to their baseline mean of 25%, a 6% increase for public facility deliveries and a 25% increase for private facility deliveries, which were initially used by less than 10% of eligible women. These results highlight the effectiveness of BSBY in shifting childbirth away from home settings and toward both public and private healthcare facilities, reflecting improved access to institutional care and the removal of financial and logistical barriers.

[17] Janani Suraksha Yojna (JSY), launched in 2005, provided cash incentives to encourage deliveries at healthcare facilities. In 2011, the program was expanded to cover all out-of-pocket delivery expenses as well as the treatment of sick newborns.

Figure 9: Delivery at healthcare facility



Notes: This figure provides estimates for the dynamic effect of BSBY on the likelihood that a child is born at a healthcare facility, sperately by gender. The sample includes all births from NFHS-4 and NFHS-5. Coefficients shown are β_k Eligible×year. Standard errors are clustered at the eligible × cluster level level. Bars represent 95% and 90% confidence intervals.

Table 10: Place of delivery

	Home Delivery		Public Facility		Private Facility	
	(1)	(2)	(3)	(4)	(5)	(6)
Eligible=1	0.133***	0.120***	0.004	0.009	-0.138***	-0.130***
	(0.011)	(0.010)	(0.013)	(0.012)	(0.010)	(0.009)
Eligible=1 × Post=1	-0.067***	-0.065***	0.049***	0.042***	0.019*	0.024**
	(0.011)	(0.012)	(0.014)	(0.015)	(0.011)	(0.011)
Controls		x		x		x
District FE	x		x		x	
Year FE	x		x		x	
District-Year FE		x		x		x
Municipality FE		x		x		x
R-squared	0.104	0.150	0.069	0.119	0.083	0.130
Mean Dep. Variable	0.113	0.113	0.714	0.714	0.171	0.171
Mean Dep. Variable (Pre, Eligible)	0.248	0.248	0.654	0.654	0.095	0.095
Observations	27583	27463	27583	27463	27583	27463

Notes: the outcome variable equals one if the child was delivered at home (columns 1-2), at a public facility (columns 3-4), at a private facility (columns 5-6), and zero otherwise. The sample includes all births from 1990s onwards from NFHS-4 and NFHS-5. Covariates include woman's age, caste, religion, and child's birth order. *Post* equals one for all years after 2014, and zero otherwise. *Eligible* equals one for women in the bottom 40% of the income distribution. Standard errors clustered at the eligible × cluster level are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 11: Infant Mortality

	(1)	(2)	(3)	(4)
Panel A: Infant Mortality at Birth				
Eligible=1	0.002***	0.002***	0.002***	0.002***
	(0.001)	(0.001)	(0.001)	(0.001)
Eligible=1 × Post=1	-0.001	-0.001	-0.002	-0.002
	(0.002)	(0.002)	(0.002)	(0.002)
R^2	0.00	0.00	0.01	0.01
Mean Dep. Variable	0.01	0.01	0.01	0.01
St. Dep. Variable	0.10	0.10	0.10	0.10
Observations	148434	148434	148434	148434
Panel B: Mortality Under 1				
Eligible=1	0.015***	0.015***	0.015***	0.015***
	(0.002)	(0.002)	(0.001)	(0.001)
Eligible=1 × Post=1	-0.003	-0.003	-0.002	-0.002
	(0.003)	(0.003)	(0.003)	(0.003)
R^2	0.01	0.01	0.02	0.02
Mean Dep. Variable	0.05	0.05	0.05	0.05
St. Dep. Variable	0.21	0.21	0.21	0.21
Observations	148434	148434	148434	148434

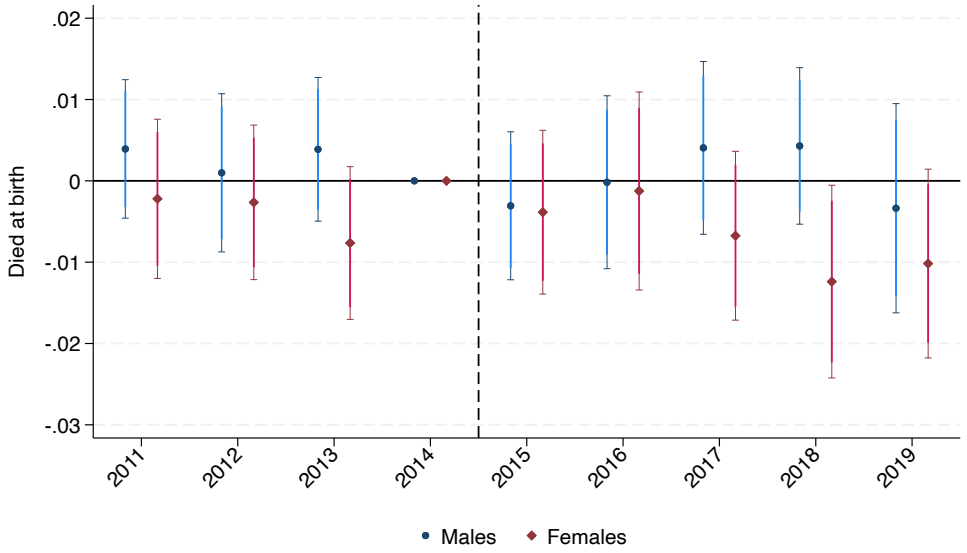
Notes: Sample includes all births from 1990 onward from NFHS-4 and NFHS-5. Column (1) includes year of birth and district fixed effects, column (2) adds controls, column (3) includes year of birth × district fixed effects and municipality fixed effects, and column (4) adds controls. Covariates include woman's age, caste, religion, and child's birth order. *Post* equals 1 for all births after 2014. *Eligible* equals 1 for women in the bottom 40% of the income distribution. Standard errors clustered at the eligible × municipality level are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

7.2 Infant Mortality

Lastly, by expanding access to healthcare, BSBY could have influenced infant mortality in several ways. First, the increase in hospital deliveries could lower infant neonatal mortality. As more women give birth at healthcare facilities, the risk of complications at delivery decreases, leading to lower mortality at birth. Second, the increase in prenatal screenings may have increased the treatment of pregnancy-related complications and high-risk births, thereby reducing infant mortality at birth. For infants, coverage of hospitalization costs could facilitate timely care for conditions such as infections, respiratory distress, or complications associated with low birth weight, which are common drivers of early-life mortality. Closely related, the policy could have altered parental investment incentives by reducing the expected cost of child health, influencing postnatal care-seeking behavior, immunization uptake, and treatment intensity, all of which are closely linked to infant survival.

Table 11 shows that the policy did not lead to a statistically significant reduction in overall child mortality at birth or under one. Given that NFHS-5 was conducted in 2020 in Rajasthan, it is not possible to reliably estimate effects on under-five mortality. Interacting eligibility with a female indicator reveals no significant differential effect on female infant mortality, suggesting the policy did not on average improve survival outcomes for girls relative to boys (Table A12). However, the event study reveals a decline in female mortality at birth from 2018 onwards, significant at the 90% level (Figure 10), with a similar pattern for mortality under one (Appendix Figure A6). While the average post-policy effect remains statistically insignificant, the event study suggests the effect materialized with a lag. This delayed effect is consistent with two reinforcing mechanisms: improved healthcare access among eligible women and a compositional shift toward wanted births as prenatal sex selection increased.

Figure 10: Infant mortality at birth



Notes: This figure provides estimates for the dynamic effect of BSBY on mortality at birth for male and females separately. The sample includes all births from NFHS-4 and NFHS-5. Coefficients shown are β_k Eligible×year. Standard errors are clustered at the eligible × cluster level level. Bars represent 95% and 90% confidence intervals.

These patterns are consistent with the broader evidence on sex selection. The literature shows that when parents can detect the gender of the fetus and selectively terminate unwanted female pregnancies, the daughters who are carried to term tend to be wanted and receive better care, which can reduce relative female infant mortality (Anukriti, Bhalotra, and Tam, 2022; Lin, Liu, and Qian, 2014). In the context of BSBY, expanded access to prenatal care and free hospital-based abortion services likely allowed families to act on son

preference, resulting in a higher likelihood that female births are wanted and receive adequate care. This provides a plausible explanation for the observed decline in female mortality from 2018 onwards.

8 Conclusion

This paper explores the unintended consequences of expanding access to free healthcare in contexts with strong son preference. Focusing on BSBY in Rajasthan, I show that the policy led to an overall decline in fertility and an increase in sex-selective behavior. At the same time, the policy had important positive effects: it increased deliveries at healthcare facilities and shows suggestive evidence of a delayed decline in female infant mortality in 2018-2019, consistent with prior evidence that access to prenatal sex selection can reduce female infant mortality by shifting selection from postnatal to prenatal methods.

I identify several mechanisms underlying these effects. First, reductions in the distance to the nearest free healthcare facility amplified women's use of care, as the policy effectively lowered both monetary and non-monetary costs of accessing care. Second, the policy altered healthcare-seeking behavior: eligible women increased prenatal visits, shifted prenatal care toward facilities providing physician-led care instead of nurse-only care. Third, I provide evidence of an increase use of modern contraceptives, including sterilization, following the policy implementation. Lastly, I show that the policy increased sex selection through both abortions at first births and stopping rules at higher birth order.

These results carry important policy implications. They show that even interventions that do not directly target fertility can influence sex selection and distort sex ratios in contexts where son preference is strong. As BSBY expands into the nationwide AB-PMJAY, similar dynamics may arise across other parts of Northern India, where son preference remains prevalent. These findings do not suggest that developing countries should reduce investments in subsidized healthcare. Rather, they highlight the need for policymakers to anticipate how deeply rooted cultural norms may shape the use of health services. More broadly, this study underscores the importance of incorporating monitoring mechanisms and safeguards into health policy design, particularly in contexts where gender-biased behavior persists.

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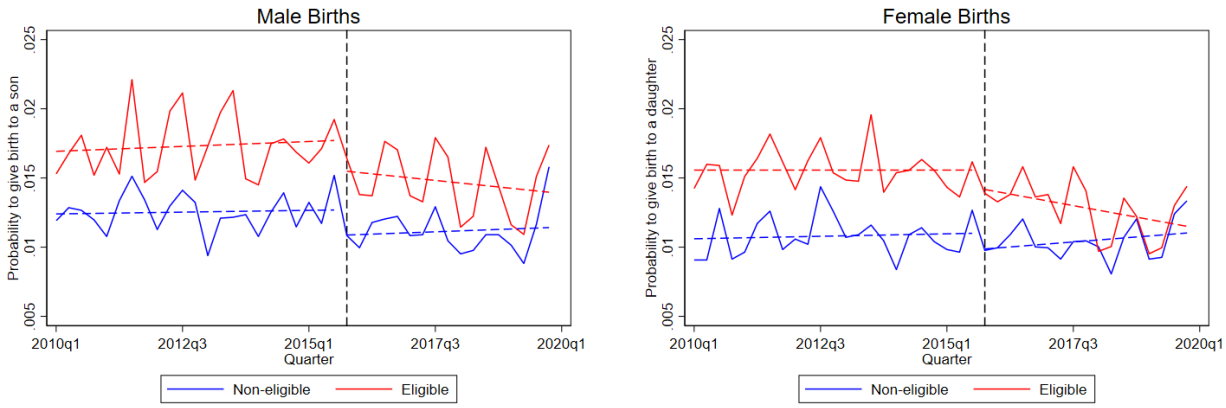
Appendix

Table A1: Balancing Table (NFHS-5)

Variable	(1) Not Eligible	(2) Eligible	(3) Diff	(4) Norm. Diff.
Woman's age at interview	35.24	34.43	-0.80***	-0.07
	(8.14)	(8.08)	(0.10)	
Age at frist birth	20.65	20.23	-0.43***	-0.09
	(3.29)	(3.21)	(0.04)	
Schedule Caste	0.18	0.25	0.08***	0.13
	(0.38)	(0.43)	(0.00)	
Schedule Tribe	0.06	0.28	0.22***	0.42
	(0.25)	(0.45)	(0.00)	
OBC	0.51	0.39	-0.13***	-0.18
	(0.50)	(0.49)	(0.01)	
Other caste/don't know	0.25	0.08	-0.17***	-0.33
	(0.43)	(0.27)	(0.00)	
Hindu	0.88	0.94	0.06***	0.15
	(0.32)	(0.24)	(0.00)	
Muslim	0.10	0.05	-0.04***	-0.11
	(0.30)	(0.23)	(0.00)	
Sik/Other	0.02	0.01	-0.02***	-0.09
	(0.15)	(0.08)	(0.00)	
Education	1.32	0.55	-0.77***	-0.58
	(1.05)	(0.82)	(0.01)	
Husband's Education	1.97	1.34	-0.63***	-0.48
	(0.86)	(0.98)	(0.03)	
Married	0.97	0.95	-0.01***	-0.05
	(0.18)	(0.21)	(0.00)	
Urban residence	0.33	0.04	-0.29***	-0.57
	(0.47)	(0.20)	(0.00)	
Ideal number of children	2.24	2.42	0.17***	0.15
	(0.74)	(0.85)	(0.01)	
Children	2.51	2.98	0.47***	0.25
	(1.22)	(1.47)	(0.02)	
Ideal number of daughters	0.94	1.00	0.07***	0.10
	(0.45)	(0.50)	(0.01)	
Ideal number of sons	1.12	1.27	0.15***	0.17
	(0.58)	(0.66)	(0.01)	
Observations	15,871	11,446	27,317	

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

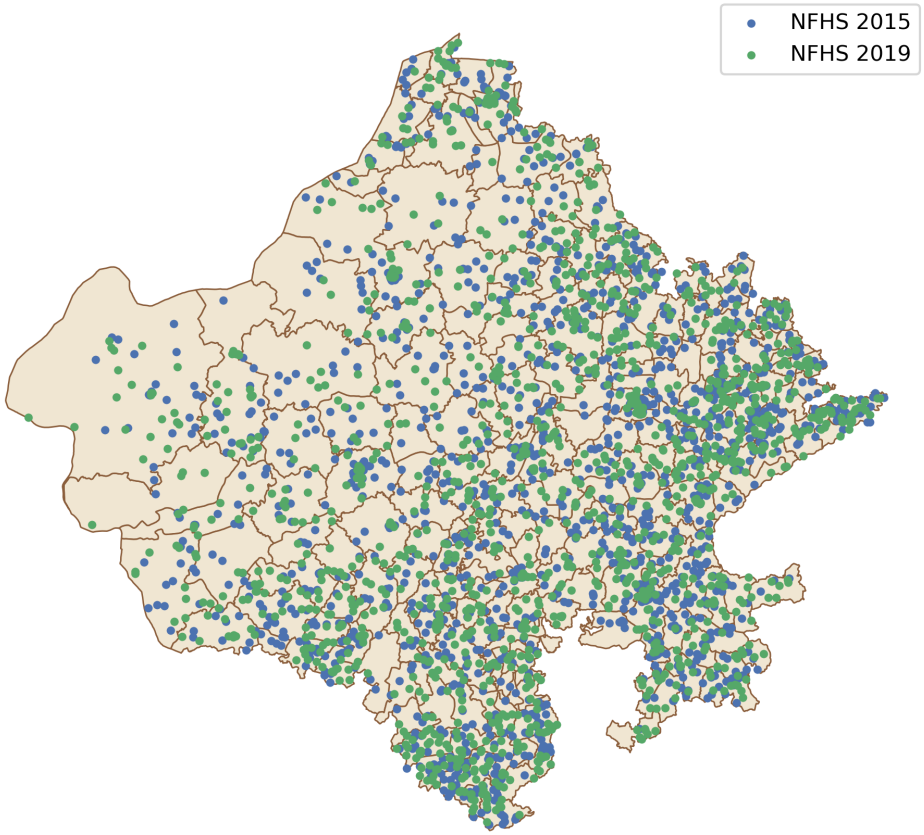
Figure A1: Probability to give birth by quarter, by gender (raw data)



(a) Male

(b) Female

Figure A2: NFHS clusters



Notes: This figure shows the boundaries from the *municipality* variable I created and the clusters spacial distribution.

Table A3: Total children, sample restricted to women sterilized

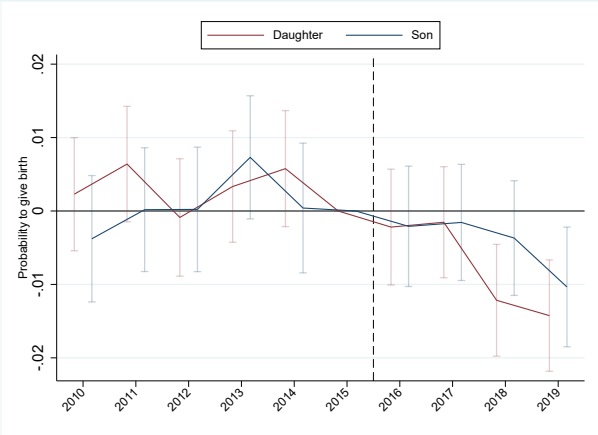
	Total Children			
	(1)	(2)	(3)	(4)
Eligible=1	0.490***	0.490***	0.460***	0.460***
	(0.030)	(0.030)	(0.029)	(0.029)
Eligible=1 × Post=1	-0.084**	-0.084**	-0.066*	-0.066*
	(0.038)	(0.038)	(0.037)	(0.037)
Controls		x		x
Wave FE	x	x	x	x
District FE	x	x		
Municipality FE			x	x
R-squared	0.070	0.070	0.087	0.087
Mean Dep. Var.	3.214	3.214	3.214	3.214
Observations	26181	26181	26093	26093

Notes: Sample of women from NFHS-4 and NFHS-5, restricted to women that at the time of the survey declared to have been sterilized. Covariates are woman's age, caste and religion. *Post* is equal to 1 for all women in NFHS-5, and zero otherwise. *Eligible* equals one for all women in the bottom 40% of the income distribution. Standard errors clustered at the eligible × cluster level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Figure A3: Probability to give birth by year



(a) Birth of a child



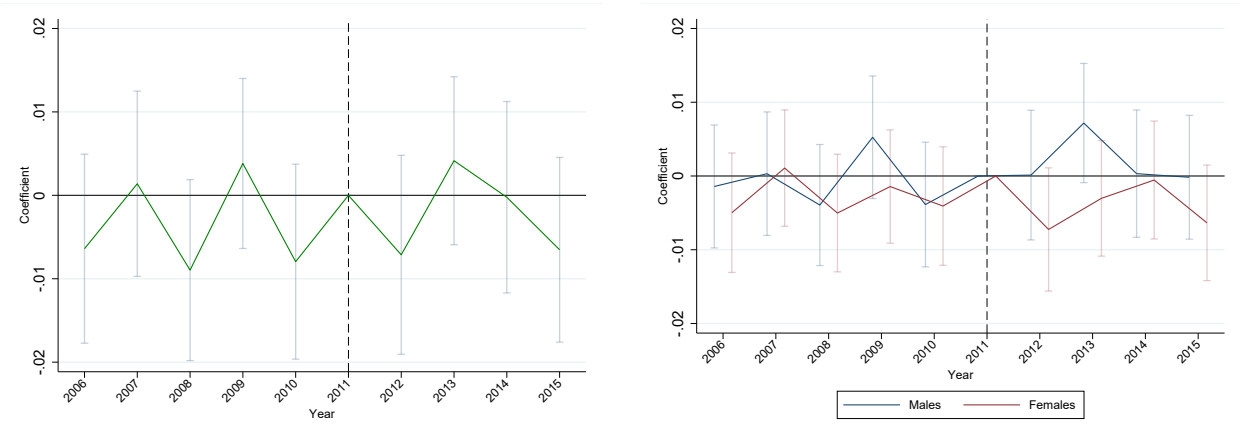
(b) Birth by gender

Table A4: Robustness: Probability to give birth

	Any birth		Any birth of a son		Any birth of a daughter	
	(1)	(2)	(3)	(4)	(5)	(6)
eligible=1	0.0283***	0.0184***	0.0136***	0.0092***	0.0147***	0.0093***
	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
eligible=1 × Post=1	-0.0151***	-0.0155***	-0.0053***	-0.0051**	-0.0098***	-0.0103***
	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)
Controls		x		x		x
Cluster FE	x	x	x	x	x	x
Year FE	x		x		x	
DistrictxYear FE		x		x		x
R-squared	0.015	0.063	0.009	0.032	0.011	0.034
Mean Dep. Variable	0.104	0.104	0.055	0.055	0.049	0.049
Mean Dep. Variable (Eligible, Pre)	0.131	0.131	0.069	0.069	0.062	0.062
Observations	273170	273170	273170	273170	273170	273170

Notes: This table reports coefficients from a specification similar to Equation (1), including year fixed effects. The unit of observation is a woman–year. *Post* equals one for all years after 2015, and zero otherwise. *Eligible* equals one for women in the bottom 40% of the income distribution. Standard errors clustered at the eligible × cluster level are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

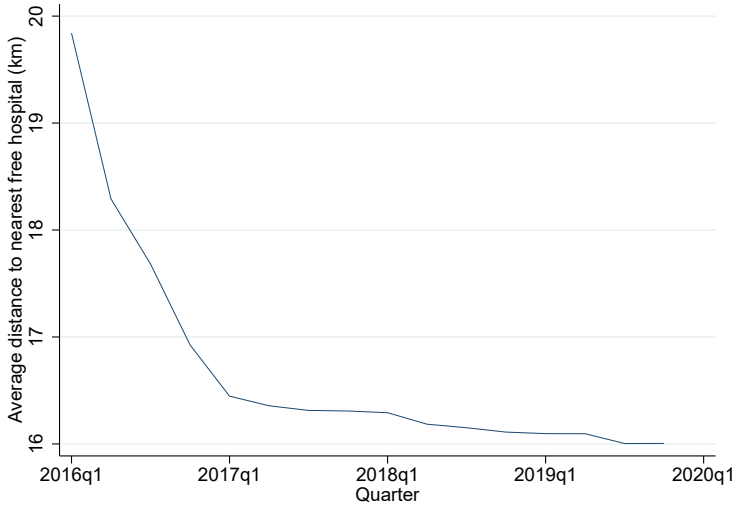
Figure A4: Placebo Test: probability to give birth by year



(a) Birth of a child

(b) Birth by gender

Figure A5: Average Distance



Notes: This figure shows average distances to the closest healthcare facility over time for the women in the sample.

Table A5: Robustness: Probability to give birth by gender, omitting mid wealth index

	Any birth			Any birth of a son			Any birth of a daughter		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Eligible=1	0.0085***	0.0053***	0.0055***	0.0040***	0.0027***	0.0027***	0.0045***	0.0027***	0.0028***
	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Eligible=1 × Post=1	-0.0046***	-0.0046***	-0.0050***	-0.0017***	-0.0017***	-0.0018***	-0.0029***	-0.0029***	-0.0032***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)	(0.001)
Controls		x	x		x	x		x	x
Cluster FE	x	x	x	x	x	x	x	x	x
Quarter-Year FE	x	x		x	x		x	x	
DistrictxQuarterxYear FE			x			x			x
R-squared	0.004	0.015	0.016	0.003	0.008	0.009	0.003	0.008	0.010
Mean Dep. Variable	0.026	0.026	0.026	0.014	0.014	0.014	0.012	0.012	0.012
Mean Dep. Variable (Eligible, Pre)	0.033	0.033	0.033	0.017	0.017	0.017	0.016	0.016	0.016
Observations	870760	870760	870760	870760	870760	870760	870760	870760	870760

Notes: This table reports the coefficients from specification (1) excluding women with a mid wealth index, the unit of observation is woman-quarter. Columns (1)-(3) report the likelihood to give birth to a male in a quarter, columns (4)-(6) report the likelihood to give birth to a female. Covariates include woman's age, caste, religion, and the number of children in 2015. *Post* equals one for all years quarters after Q42015, and zero otherwise. *Eligible* equals one for women in the bottom 40% of the income distribution. Standard errors clustered at the eligible × cluster level are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A6: Placebo: Probability to give birth

	Any birth		Any birth of a son		Any birth of a daughter	
	(1)	(2)	(3)	(4)	(5)	(6)
eligible=1	0.0312***	0.0097***	0.0137***	0.0042***	0.0175***	0.0055***
	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
eligible=1 × post_placebo=1	0.0023	0.0017	0.0026	0.0022	-0.0003	-0.0005
	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)
Controls		x		x		x
Cluster FE	x	x	x	x	x	x
Year FE	x		x		x	
DistrictxYear FE		x		x		x
R-squared	0.015	0.072	0.009	0.030	0.011	0.046
Mean Dep. Variable	0.109	0.109	0.058	0.058	0.051	0.051
Observations	274420	274420	274420	274420	274420	274420

Notes: This table reports coefficients from specification similar to Equation (1), including year fixed effects. The unit of observation is a woman–year. *Post* equals one for all years after 2011, and zero otherwise. *Eligible* equals one for women in the bottom 40% of the income distribution. Covariates include woman's age, caste, and religion. Standard errors clustered at the eligible × cluster level are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A7: Child is a girl

	Child is a Girl			
	(1)	(2)	(3)	(4)
Eligible=1	0.017***	0.017***	0.018***	0.018***
	(0.003)	(0.003)	(0.002)	(0.002)
Eligible=1 × Post=1	-0.033***	-0.033***	-0.035***	-0.035***
	(0.010)	(0.010)	(0.010)	(0.010)
Controls		x		x
District FE	x	x		
Year FE	x	x		
District-Year FE			x	x
Municipality FE			x	x
R-squared	0.001	0.001	0.011	0.011
Probability of a Female Birth	0.468	0.468	0.468	0.468
Probability of a Female Birth (Pre, Eligible)	0.476	0.476	0.476	0.476
Observations	151795	151795	151769	151769

Notes: Sample includes all children from NFHS-4 and NFHS-5. Covariates include woman's age, caste, and religion. *Post* equals 1 for all births after 2015. *Eligible* equals 1 for women in the bottom 40% of the income distribution. Standard errors clustered at the eligible × municipality level are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A8: Child is a girl, heterogeneity by birth order, sample restricted

	Births 1990 onwards			
	(1)	(2)	(3)	(4)
Eligible=1 × Post=1	-0.031**	-0.031**	-0.033**	-0.033**
	(0.015)	(0.015)	(0.016)	(0.016)
Eligible=1 × Post=1 × Bord=2	-0.004	-0.004	-0.003	-0.003
	(0.025)	(0.025)	(0.025)	(0.025)
Eligible=1 × Post=1 × Bord=3	-0.005	-0.005	-0.004	-0.004
	(0.024)	(0.024)	(0.024)	(0.024)
Controls		x		x
District FE	x	x		
Year FE	x	x		
District-Year FE			x	x
Municipality FE			x	x
R-squared	0.001	0.001	0.009	0.009
Mean Dep. Variable	0.468	0.468	0.468	0.468
Mean Dep. Variable (Pre,Eligible)	0.476	0.476	0.476	0.476
Observations	148434	148434	148434	148434

Notes: the outcome variable equals one if the child is female, 0 otherwise. Sample is all births from NFHS-4 and NFHS-5 from 1990s onward. Covariates include woman's age, caste, religion, and the number of children in 2015. *Post* equals one for all years quarters after 2015, and zero otherwise. *Bord* is a categorical variable equal to 1 for parity one, 2 for parity two, and 3 for parity three and up. *Eligible* equals one for women in the bottom 40% of the income distribution. Standard errors clustered at the eligible × cluster level are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A9: Distance to the nearest BSBY Hospital and likelihood of giving birth

	Any birth			Any birth of a son			Any birth of a daughter		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Eligible=1	0.00286***	0.00281***		0.00187***	0.00192***		0.00099***	0.00089**	
	(0.001)	(0.001)		(0.000)	(0.000)		(0.000)	(0.000)	
Distance (10km)	0.00048	0.00049	0.00048	0.00040*	0.00035	0.00035	0.00009	0.00014	0.00014
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Eligible=1 × Distance (10km)	0.00021***	0.00021***	0.00021***	0.00008***	0.00008***	0.00008***	0.00013***	0.00014***	0.00013***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Controls		x			x			x	
Cluster FE	x	x	x	x	x	x	x	x	x
Quarter-Year FE	x			x			x		
DistrictxQuarterxYear FE		x	x		x	x		x	x
Woman FE			x			x			x
R-squared	0.004	0.005	0.034	0.002	0.004	0.027	0.003	0.004	0.034
Mean Dep. Variable	0.026	0.026	0.026	0.014	0.014	0.014	0.012	0.012	0.012
Mean Dep. Variable (Eligible, Pre)	0.033	0.033	0.033	0.017	0.017	0.016	0.016	0.016	0.016
Observations	1038046	1038046	1038046	1038046	1038046	1038046	1038046	1038046	1038046

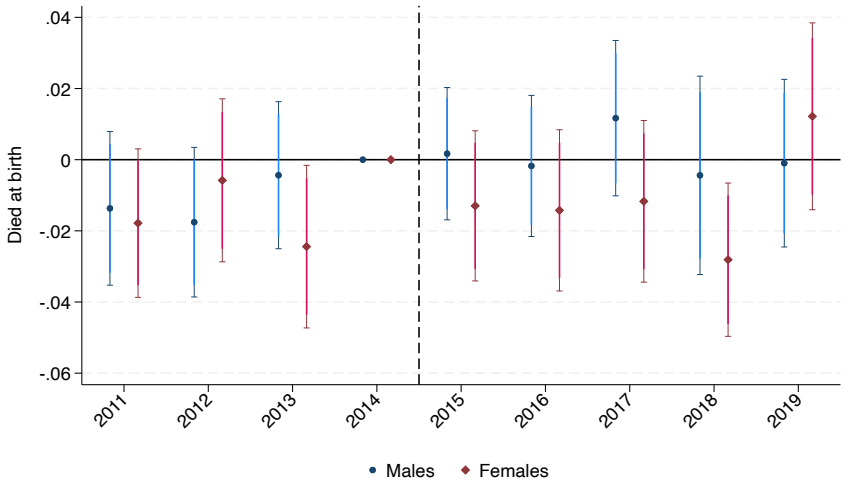
Notes: This table reports the relationship between "as the crow flies" distance to the closest free healthcare facility (in tens of km) and the likelihood of giving birth to a child (columns 1-3), to a son (columns 4-6), and to a daughter (columns 7-8) separately. The unit of observation is a woman-quarter, observed from 2010Q1 to 2019Q4. All distances before the policy are imputed to be 200km. Covariates include woman's age, caste, religion, and the number of children in 2015. *Eligible* equals one for women in the bottom 40% of the income distribution, and *Distance (10km)* is the distance to the nearest hospital (in tens of km) calculated "as the crow flies" two quarters before the birth of the child. Standard errors clustered at the eligible × cluster level are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A10: Ultrasound Reported Use

	Ultrasound Use			
	(1)	(2)	(3)	(4)
Eligible=1	-0.309***	-0.276***	-0.279***	-0.249***
	(0.011)	(0.011)	(0.012)	(0.011)
Eligible=1 × Post=1	0.193***	0.186***	0.167***	0.162***
	(0.012)	(0.011)	(0.013)	(0.013)
Controls		x		x
District FE	x	x		
Year FE	x	x		
District-Year FE			x	x
Municipality FE			x	x
R-squared	0.186	0.216	0.226	0.252
Mean Dep. Variable	0.803	0.803	0.803	0.803
Mean Dep. Variable (Pre, Eligible)	0.514	0.514	0.514	0.514
Observations	29434	29434	29311	29311

Notes: Sample of births in the last 5 years of women interviews in NFHS-4 and NFHS-5 . Covariates are woman's age, caste and religion. *Post* is equal to 1 for all years after 2014, and zero otherwise. *Eligible* equals one for all women in the bottom 40% of the income distribution. Standard errors clustered at the eligible × cluster level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Figure A6: Mortality under one



Notes: This figure provides estimates for the dynamic effect of BSBY on mortality under one for children born in year t, for male and females separately. Mortality under one for children born in 2019 should be interpreted with caution as it includes mortality in 2020 during Covid-19. The sample includes all births from NFHS-4 and NFHS-5. Coefficients shown are β_k Eligible×year. Standard errors are clustered at the eligible × cluster level level. Bars represent 95% and 90% confidence intervals.

Table A11: Delivery at healthcare facility

	Child was born at healthcare facility			
	(1)	(2)	(3)	(4)
Eligible=1	-0.133***	-0.111***	-0.120***	-0.099***
	(0.011)	(0.010)	(0.010)	(0.010)
Eligible=1 × Post=1	0.067***	0.062***	0.065***	0.061***
	(0.011)	(0.010)	(0.012)	(0.012)
Controls		x		x
District FE	x	x		
Year FE	x	x		
District-Year FE			x	x
Municipality FE			x	x
R-squared	0.103	0.124	0.149	0.168
Mean Dep. Variable	0.886	0.886	0.886	0.886
Std. Dep. Variable	0.318	0.318	0.318	0.318
Observations	27583	27583	27463	27463

Notes: the outcome variable equals one if the child was born at a healthcare facility and 0 otherwise. The sample includes all births from 1990s onwards from NFHS-4 and NFHS-5. Covariates include woman's age, caste, religion, and child's birth order. *Post* equals one for all years after 2014, and zero otherwise. *Eligible* equals one for women in the bottom 40% of the income distribution. Standard errors clustered at the eligible × cluster level are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A12: Relative female infant mortality

	(1)	(2)	(3)	(4)
Panel A: Panel A: Infant Mortality at Birth				
Child is a girl=1 × Eligible=1	0.000	0.000	0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)
Child is a girl=1 × Eligible=1 × Post=1	-0.002	-0.002	-0.002	-0.002
	(0.003)	(0.003)	(0.003)	(0.003)
R^2	0.00	0.00	0.01	0.01
Mean Dep. Variable	0.01	0.01	0.01	0.01
St. Dep. Variable	0.10	0.10	0.10	0.10
Observations	148434	148434	148434	148434
Panel B: Panel B: Mortality Under 1				
Child is a girl=1 × Eligible=1	-0.000	-0.000	0.000	0.000
	(0.003)	(0.003)	(0.003)	(0.003)
Child is a girl=1 × Eligible=1 × Post=1	-0.005	-0.006	-0.006	-0.006
	(0.006)	(0.006)	(0.006)	(0.006)
R^2	0.01	0.01	0.02	0.02
Mean Dep. Variable	0.05	0.05	0.05	0.05
St. Dep. Variable	0.21	0.21	0.21	0.21
Observations	148434	148434	148434	148434

Notes: Sample includes all births from 1990 onward from NFHS-4 and NFHS-5. Column (1) includes year of birth and district fixed effects, column (2) adds controls, column (3) includes year of birth × district fixed effects and municipality fixed effects, and column (4) adds controls. Covariates include woman's age, caste, religion, and child's birth order. *Post* equals 1 for all births after 2014. *Eligible* equals 1 for women in the bottom 40% of the income distribution. Standard errors clustered at the eligible × municipality level are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.