

Maternal Health Programs and the Continuation of Unintended Pregnancies

Hyunji Kim *

November 5, 2025

[Click here for the latest version](#)

Abstract

Maternal and Child Health (MCH) programs promote safe motherhood by linking financial incentives for institutional delivery with health-worker outreach and family-planning education. I study how MCH programs influence reproductive behavior through pre-conception margin, by affecting contraceptive use and fertility, and post-conception margin by affecting whether unintended pregnancies continue to birth. Using a difference-in-differences design exploiting India's national MCH program, I find that it increased modern contraceptive use by 12 percent and reduced fertility among older women. In contrast, the program increased unintended pregnancies ending in births among younger women by four percentage points. Evidence from a later phase of the program that expanded health-worker incentives shows that greater contact with community health workers contributed to this rise. These findings reveal an unintended consequence of maternal health interventions: by simultaneously promoting planned and protected pregnancy, they can inadvertently constrain women's reproductive autonomy.

Keywords: Maternal and Child Health (MCH), Unplanned Fertility, Reproductive Behavior, Community Health Workers, Difference-in-Differences, Regression Discontinuity

*University of Washington, Seattle, WA. Email: hjkim26@uw.edu

1 Introduction

An unintended pregnancy ending in a live birth, often referred to as an unplanned birth, is a substantial public health and development concern. Between 2015 and 2019, an estimated 121 million unintended pregnancies occurred annually worldwide, and nearly 40 million of these were carried to term ([Bearak et al., 2020](#)). The burden is particularly high in low-income settings such as India, which accounted for over 14 percent of all unintended pregnancies during this period, with roughly one-third ending in live births ([McFarlane et al., 2022](#)). These births are associated with lower utilization of maternal health services, worse maternal mental health, and poorer health and developmental outcomes for children ([Wado et al., 2013](#); [Bahk et al., 2015](#); [Nelson et al., 2022](#); [Singh et al., 2012](#); [Marston and Cleland, 2003](#); [Shapiro-Mendoza et al., 2006](#); [Foster et al., 2018](#)). Considering their wide-ranging impact on maternal and child well-being, understanding the determinants of unplanned births is essential for designing policies that improve maternal and child health.

Maternal and Child Health (MCH) programs are a central policy instrument for improving outcomes around childbirth and advancing reproductive goals. These initiatives combine messaging on safe motherhood and institutional delivery with financial incentives and outreach by community health workers ([Neelsen et al., 2021](#)). Their typical activities emphasize family planning, antenatal care, skilled attendance at delivery, and postnatal follow-up, often framed around the importance of a planned and protected pregnancy. Community health workers also provide counseling on nutrition, birth preparedness, and birth spacing, and encourage women to seek timely care during pregnancy ([Gebremedhin et al., 2022](#)).

While these interventions have improved maternal and neonatal care, promoting safe motherhood can influence reproductive behavior more broadly when it encourages pregnancies that are both planned and safely carried. First, counseling on birth preparedness and family planning provided by health workers may increase awareness and access to contraception, lowering fertility. Second, these interactions may alter how women

perceive fertility preferences within their households. Specifically, such counseling can shift women's own preferences toward limiting childbearing, while their perception of their husbands' preferences may not change. As a result, women may become more likely to perceive that they do not want additional children whereas their husbands do. In male-dominated contexts, social norms and intra-household power dynamics constrain women's reproductive autonomy, so pregnancy outcomes tend to align with their husbands' preferences (Komura, 2013; Doepke and Tertilt, 2018; Mishra and Parasnis, 2021; Thomson, 1997). Thus, such perceived discordance can limit women's ability to follow their own preferences and instead lead them to conform to what they believe their husbands want. At the same time, repeated contact with health workers and sustained messaging about safe pregnancy and institutional delivery may sensitize pregnancy—heightening its cognitive, emotional, and social salience—and thereby affect how women and families respond once conception occurs.

Maternal health programs create an inherent tension in predicting their impact on fertility: by expanding access to contraception they may reduce pregnancies, yet by sensitizing pregnancy they may also increase the likelihood that unintended pregnancies are carried to term. Despite these opposing forces, little is known about how such interventions affect overall fertility and the continuation of unintended pregnancies. Existing studies of MCH programs have focused primarily on service utilization and maternal and infant mortality (Lim et al., 2010; Powell-Jackson et al., 2015) or on aggregate fertility levels (Nandi and Laxminarayan, 2016). This paper addresses this gap by studying India's nationwide maternal and child health program, the Janani Suraksha Yojana (JSY), launched in 2005 to foster safe motherhood. JSY promotes both planned and protected pregnancies through two main components: financial incentives that lower the cost of institutional delivery and sustained engagement with community health workers (Accredited Social Health Activists, or ASHAs). These features create conditions that can shape not only the timing and number of pregnancies through contraceptive use but also the likelihood that unintended ones are carried to term.

I estimate the causal effect of JSY on these reproductive behaviors using a difference-in-differences (DiD) strategy that leverages variation in program eligibility across In-

dian states. Indian states were classified as either Low-Performing States (LPS) or High-Performing States (HPS) based on baseline institutional delivery rates.¹ In LPS, where institutional delivery rates were historically low, JSY benefits were universally available to all pregnant women for every delivery. In HPS, eligibility was restricted to women below the poverty line, Scheduled Castes or Tribes, and those with up to two live births. The DiD design compares changes in outcomes between two groups with similar socioeconomic characteristics: ineligible women in HPS (those above the poverty line and not from Scheduled Castes or Tribes) serve as the control group, while eligible women with the same characteristics in LPS form the treatment group.

The analysis yields two main findings. First, JSY increased the likelihood of using modern contraceptive methods among all women and reduced fertility among older women. The likelihood of using a modern contraceptive method rose by 6–7 percentage points overall, and among women aged 30–40, the number of births over the past five years fell by about 0.10, a decline of nearly 28 percent relative to baseline. This pattern suggests that improved access to family planning services through health programs enabled women to lower their total fertility. Second, the program increased the likelihood of unplanned births, with the effect concentrated among younger women aged 19–29: the probability of an unplanned birth rose by about 4 percentage points, or 20 percent relative to baseline. These results indicate that MCH programs can influence reproductive decisions along both the pre-conception margin, by affecting contraceptive use and fertility levels, and the post-conception margin, by shaping whether unintended pregnancies are carried to term.

To investigate a key channel underlying the increase in unplanned births, I exploit a later-phase reform of the program that expanded ASHA incentives in High-Performing States.² Using a regression discontinuity design, I show that the reform sharply increased women's interactions with community health workers. Building on this evidence, I then

¹LPS include Uttar Pradesh, Uttarakhand, Bihar, Jharkhand, Madhya Pradesh, Chhattisgarh, Assam, Rajasthan, Odisha, and Jammu and Kashmir.

²Four years after JSY began, the program was reformed to extend cash incentives to ASHAs in High-Performing States, where previously only mothers received payments.

estimate a second difference-in-differences model to assess how these expanded interactions affected reproductive behavior. The reform raised the likelihood of an unplanned birth among younger women by 5 percentage points, about 15 percent relative to baseline, with no significant effect among older women. These findings suggest that increased engagement with community health workers, driven by their new cash incentives, contributed to the rise in unplanned births.

I provide suggestive evidence for two behavioral mechanisms through which greater engagement with community health workers contributed to the rise in unplanned births. First, I present evidence consistent with the perceived discordance mechanism discussed above. Using panel data from the India Human Development Survey, I examine how exposure to community health workers affected women's fertility preferences and their perceptions of spousal alignment. Exposure to ASHAs increased the likelihood that women reported not wanting additional children. More importantly, it also raised the share of women who perceived that they did not want more children whereas their husbands did. This widening perception gap suggests that greater engagement with health workers reshaped women's own fertility preferences and heightened perceived discordance within couples. In male-dominated contexts, such belief gaps can translate into higher rates of unplanned births.

Second, more engagement with health workers leads to lower acceptance of abortion. This finding aligns with previous literature showing that community health workers often promote pronatalist norms and discourage abortion. They may do so through repeated counseling, financial incentives that pay ASHAs for each institutional delivery and thereby encourage them to promote delivery over abortion, or fears that abortion intentions disclosed to ASHAs will not remain confidential in village settings, making abortion appear less acceptable or feasible ([Nandagiri, 2019](#); [Glenton et al., 2017](#); [Gupta et al., 2017](#); [Javadekar et al., 2025](#)). When abortion is perceived as less acceptable within households and communities, unintended pregnancies are more likely to be carried to term, contributing to the rise in unplanned births.

Taken together, the findings demonstrate that MCH programs affect not only whether and when to conceive but also the likelihood that unintended pregnancies result in live

births. While these programs aim to improve maternal and neonatal health, their incentive structures and delivery mechanisms can generate unintended effects on post-conception behavior. Understanding these broader impacts is essential for designing MCH interventions that support both health improvements and reproductive autonomy.

This paper contributes to four strands of literature at the intersection of maternal health policy, fertility behavior, community health workers, and incentive design in public service delivery.

First, it expands the literature on the effects of maternal and child health (MCH) programs, which has largely focused on service uptake and clinical outcomes such as institutional delivery rates and maternal or neonatal mortality (e.g., [Lim et al., 2010](#); [Powell-Jackson et al., 2015](#)). Recent work has begun to examine behavioral outcomes such as sex-selective abortion (e.g., [Javadekar et al., 2025](#)). By further shifting attention to whether unintended pregnancies are carried to term, this paper highlights that MCH interventions can influence a broader set of reproductive intentions and birth composition. These outcomes are central to maternal well-being but are often overlooked in program evaluations.

Second, the paper contributes to research on the determinants of unintended fertility. Much of this work emphasizes pre-conception constraints, such as limited access to contraception or safe abortion services (e.g., [Bongaarts, 1990](#); [Bailey, 2010](#); [Miller, 2010](#)). I show that maternal health programs can also shape post-conception behavior. By expanding interactions with health workers who promote institutional delivery and safe motherhood, such programs may increase the likelihood that unintended pregnancies are carried to term. This mechanism helps explain how unplanned births may rise even in settings where contraceptive access is improving.

Third, this study adds to the literature on community health workers (CHWs), which has emphasized their role in expanding access to care and improving health outcomes (e.g., [Björkman and Svensson, 2009](#); [Dupas, 2011](#)). I provide new evidence that CHWs can also influence fertility preferences and attitudes. Exposure to India's community health workers influences women's own fertility preferences without altering how they perceive their husbands' preferences, leading to greater reports of discordance within

couples. It also affects abortion attitudes, particularly reducing acceptance of abortion among men. Together, these findings suggest household- and community-level channels through which CHWs may influence reproductive decisions beyond the delivery of clinical services.

Finally, this paper contributes to the literature on incentive design in public service delivery. Prior work has shown that performance-based pay can increase provider effort and improve the delivery of targeted services. For example, [Gertler and Vermeersch \(2013\)](#) and [Singh and Masters \(2017\)](#) show that provider incentives can improve child health and service delivery. Moreover, [Mendelson et al. \(2017\)](#) and [Gadsden et al. \(2021\)](#) find in systematic reviews that pay-for-performance programs often raise health service use and quality. I extend this literature by showing that incentives tied to institutional delivery affected not only service uptake but also behavioral outcomes. Specifically, the expansion of ASHA incentives increased their engagement with pregnant women, which in turn influenced decisions about whether to continue unintended pregnancies. This suggests that pay-for-performance programs can affect sensitive outcomes such as reproductive autonomy, with implications for how such incentives are designed and targeted.

The rest of the paper proceeds as follows. Section 2 provides institutional background on unplanned fertility and the JSY program. Section 3 outlines the conceptual motivation for how MCH programs can influence fertility behavior along both pre- and post-conception margins. Section 4 describes the data and variable construction. Sections 5 and 6 present the empirical strategy and main results on overall fertility and unplanned births. Section 7 validates the empirical design using pre-trend tests. Section 8 examines mechanisms operating through exposure to community health workers. Section 9 investigates behavioral responses underlying this health worker channel, focusing on changes in perceived fertility preferences and abortion attitudes. Section 10 reports robustness checks, and Section 11 concludes.

2 Background

2.1 Unintended Fertility Ending in Births

Between 2015 and 2019, an estimated 121 million unintended pregnancies occurred annually worldwide, accounting for nearly half of all pregnancies ([Bearak et al., 2020](#); [Mcfarlane et al., 2022](#)). These pregnancies had various outcomes: approximately 61% ended in abortion, while the remainder resulted in live births or miscarriages. When unintended pregnancies are carried to term, the resulting live births are termed unplanned births.

India accounts for a substantial share of global unplanned births. The country experienced over 14% of all unintended pregnancies during 2015-2019 ([Mcfarlane et al., 2022](#)). In 2015, India's unintended pregnancy rate was 70 per 1,000 women aged 15-49. Of these unintended pregnancies, approximately 33% were carried to term as unplanned births or ended in miscarriage, while 67% ended in abortion ([Singh et al., 2018](#)).³

Unplanned births have important implications for maternal and child health. Women with unplanned births are associated with lower utilization of maternal health services, including fewer antenatal care visits and inadequate prenatal follow-up, particularly in low-income settings ([Wado et al., 2013](#)). [Bahk et al. \(2015\)](#) find a causal relationship between unplanned births and poorer maternal mental health, including higher risks of postpartum depression and elevated stress levels. A recent meta-analysis further shows that unintended pregnancies carried to term are strongly associated with increased odds of maternal depression during and after pregnancy ([Nelson et al., 2022](#)).

Children born from unplanned births also face significant health challenges. [Singh et al. \(2012\)](#) find a causal effect of unplanned births on poorer child health outcomes,

³Abortion has been legal in India under the Medical Termination of Pregnancy (MTP) Act since 1971. The law permits abortion up to 20 weeks of gestation under a broad range of conditions, including risk to the woman's physical or mental health, contraceptive failure, and fetal abnormalities ([Stillman et al., 2014](#)). In 2021, amendments extended the gestational limit to 24 weeks for certain categories of women ([Kumari and Kishore, 2021](#)).

including lower vaccination rates, higher risk of stunting (impaired growth and development), and greater mortality during the neonatal and early childhood periods. [Marston and Cleland \(2003\)](#) review evidence showing that children from unintended pregnancies are more likely to experience stunting than those from intended pregnancies. The effects extend to siblings as well: [Foster et al. \(2018\)](#) find that when women were unable to terminate unintended pregnancies, their existing children were associated with lower cognitive development scores and higher poverty rates compared to children whose mothers obtained wanted abortions.

Given their prevalence and wide-ranging health implications, understanding the factors that influence whether unintended pregnancies are carried to term is essential for health policy. They connect immediate reproductive decisions to long-term maternal and child well-being, shaping pathways of health, human capital, and household welfare over time. As countries expand maternal health programs, recognizing their broader effects on fertility decisions becomes increasingly important for policy design.

2.2 Janani Suraksha Yojana (JSY): A Safe Motherhood Initiative

The Janani Suraksha Yojana (JSY) is a conditional cash transfer program launched by the Government of India in 2005 with the primary objective of reducing maternal and neonatal mortality by encouraging institutional deliveries. The program operates under the National Health Mission (NHM) and targets economically vulnerable pregnant women, aiming to increase access to healthcare facilities and reduce home births, which are often associated with higher risks of maternal and neonatal complications.

A key feature of JSY is its cash incentive structure, designed to promote institutional deliveries. Under the scheme, pregnant women receive monetary support when they give birth in a health facility, reducing financial barriers to accessing medical care. Additionally, Accredited Social Health Activists (ASHAs), who are trained community health workers, are compensated for assisting expectant mothers. ASHAs provide guidance on maternal health, facilitate transportation to health facilities, and help with registration

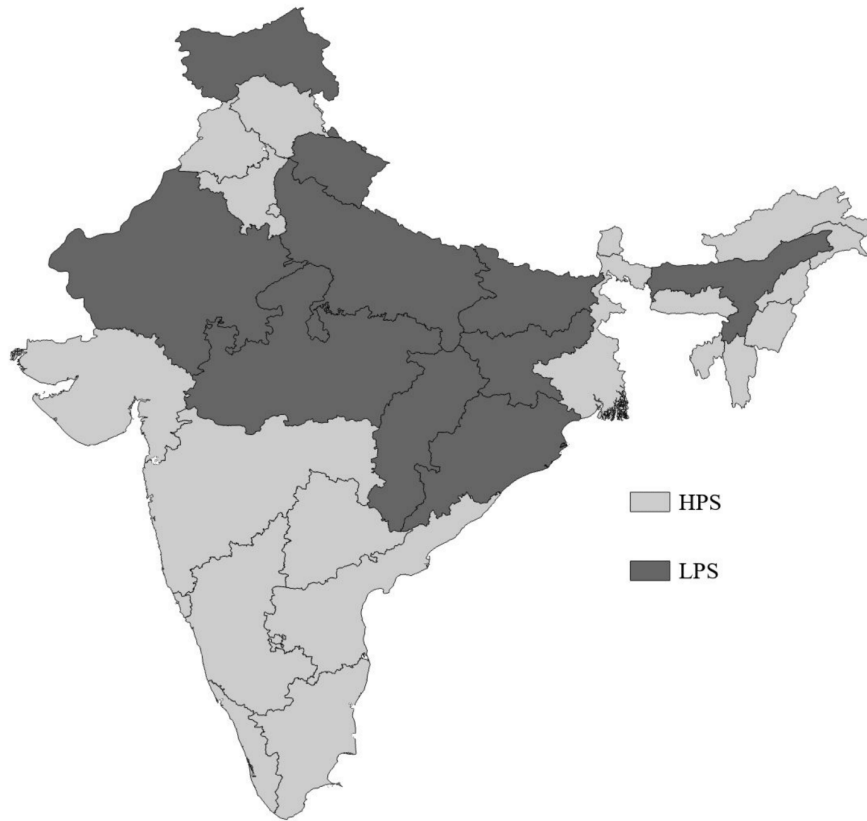
and documentation required to access JSY benefits. By offering incentives to both mothers and ASHAs, the program aims to promote broader outreach and greater uptake of maternal healthcare services.

The program distinguishes between Low-Performing States (LPS) and High-Performing States (HPS) based on institutional delivery rates. In LPS, where institutional delivery rates were initially low, all pregnant women are eligible for JSY benefits. In contrast, in HPS, where institutional deliveries were already relatively high, eligibility is limited to women classified as Below the Poverty Line (BPL) or belonging to Scheduled Castes (SC) or Scheduled Tribes (ST), and only for up to two live births. Figure 1 shows how Indian states are classified as Low- or High-Performing under the JSY program.

The financial incentives provided under JSY have changed over time. In 2005, mothers in low-performing states (LPS) received 1,400 Rps (about \$32 USD, or 1,000 Rps /\$23 USD in urban areas) for giving birth in a health facility, and ASHAs were paid 600 Rps (around \$14 USD) for each delivery they supported. In high-performing states (HPS), mothers received 700 Rps (\$16 USD; 600 Rps/\$14USD in urban areas), and ASHAs did not receive any payment initially. From April 2009, the program expanded to offer ASHAs in HPS a payment of 200Rps (about \$5 USD) per delivery while keeping the same payments for mothers. To give a sense of scale, the 1,400 Rps payment to mothers in 2005 amounted to about 51–52 percent of India’s average monthly per-capita income (the annual income was approximately \$740 USD in 2005, [World Bank 2023](#)), making it a significant amount for low-income households. Similarly, the 600Rps payment to ASHAs was a meaningful performance-based incentive in areas where other earning opportunities were limited.⁴ Table 1 summarizes how JSY incentives varied over time and by state classification.

⁴Based on a 2017 survey of ASHAs in Karnataka, [Shet et al. \(2018\)](#) report that most earned only about Rps 1,200–2,000 per month from activity-based incentives, without any fixed salary, and many considered the pay scale unjust relative to their workload. While incomes varied somewhat across states, these figures highlight how modest ASHAs’ earnings were in practice.

Figure 1: Low- and High-Performing States under the JSY Program



3 Conceptual Motivation

Following [Ananat et al. \(2009\)](#), reproductive behavior can be viewed as involving two related but distinct choices: whether to conceive and whether to continue a pregnancy once it has begun. Together, these choices determine both overall fertility and the incidence of unplanned births.

Both decisions are shaped by financial, informational, and social constraints. The decision to conceive depends on access to contraception and family-planning counseling, which can reduce fertility ([Ashraf et al., 2014](#); [Dupas, 2011](#)), as well as on the expected costs of childbearing. The decision to continue a pregnancy reflects the financial and social costs of delivery, the cost and availability of abortion, interactions with health work-

Table 1: Variation in JSY Incentive Structure

State Type	Recipient	2005–2009 Payment (Rps)	From April 2009 Payment (Rps)
LPS	Mother	1,400 (Rural)	1,400 (Rural)
		1,000 (Urban)	1,000 (Urban)
	ASHA	600 per delivery	600 per delivery
HPS	Mother	700 (Rural)	700 (Rural)
		600 (Urban)	600 (Urban)
	ASHA	None	200 per delivery

ers, and prevailing social norms surrounding termination ([Ananat et al., 2009](#); [Valente, 2014](#); [Antón et al., 2018](#)).

India’s Janani Suraksha Yojana (JSY) provides a useful setting to examine how a maternal and child health (MCH) intervention influences these decisions. JSY is a nationwide safe-motherhood program that provides conditional cash transfers to promote institutional delivery and mobilizes community health workers (ASHAs) to support women through pregnancy and childbirth. While the program’s explicit goal is to reduce maternal and neonatal mortality, its broader emphasis on safe motherhood, encouraging pregnancies that are both planned and protected, may also shape reproductive behavior in unintended ways.

Planned Pregnancy Emphasizing planned pregnancy within JSY may affect fertility behavior through both contraceptive use and fertility preferences. Family-planning counseling provided by ASHAs increases awareness and access to modern contraceptive methods and promotes the benefits of smaller families and adequate birth spacing ([Ministry of Health and Family Welfare, Government of India, 2006](#)). These activities could lower informational and access barriers to contraception, thereby reducing fertility. At the same time, repeated exposure to messages emphasizing planning and control over childbearing may shift household fertility preferences toward smaller family sizes and reduced additional childbearing. When these changes are not matched by what women perceive

to be their husbands' preferences, a perception gap may emerge within couples. In male-dominated settings, where men hold greater influence over pregnancy outcomes (Komura, 2013; Doepke and Tertilt, 2018; Mishra and Parasnis, 2021; Thomson, 1997), such perceived discordance can constrain women's ability to act on their reproductive intentions, increasing the likelihood that unintended pregnancies are carried to term.

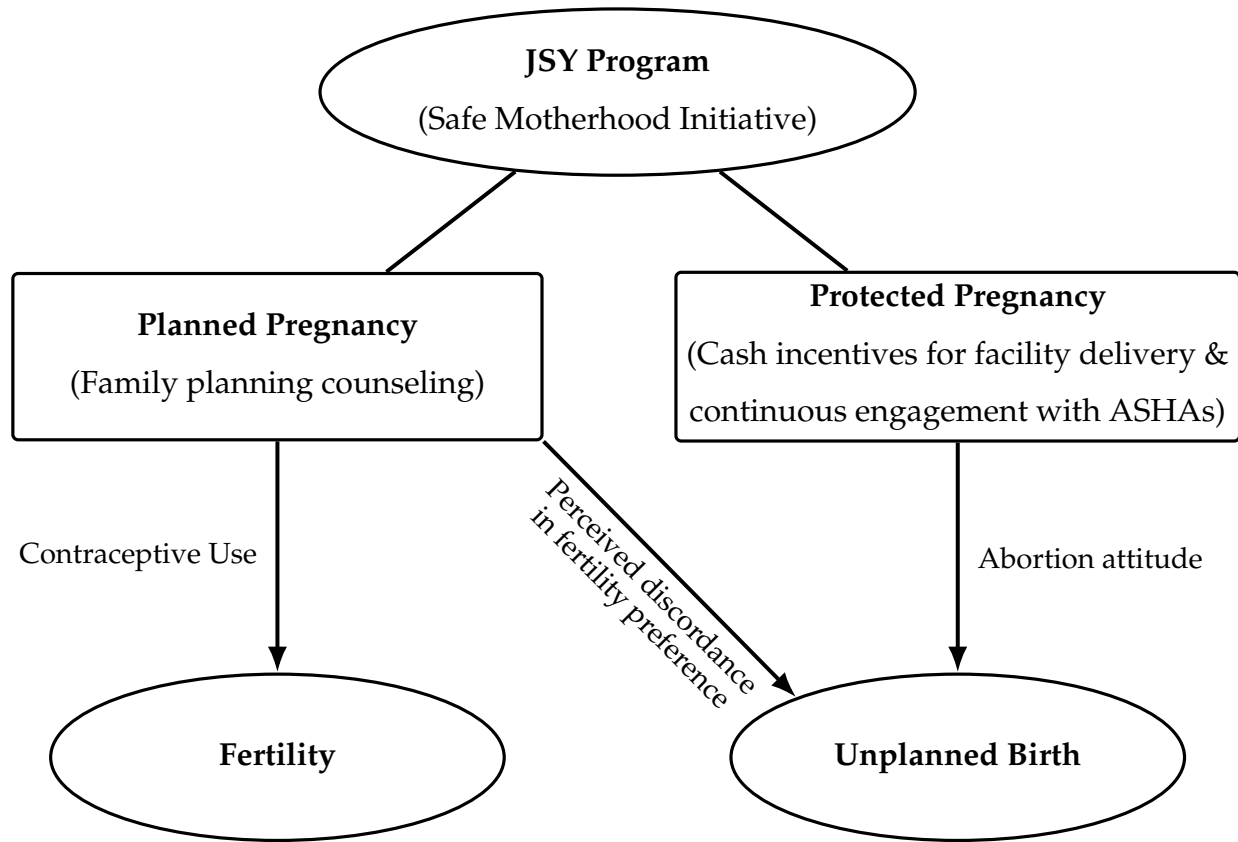
Protected Pregnancy Emphasizing protected pregnancy may plausibly increase the likelihood that unintended pregnancies are carried to term. By combining incentives for institutional delivery with performance-based payments that encourage ASHAs to maintain regular contact with pregnant women and promote safe motherhood, the program could lower the perceived financial and medical risks of childbirth and, in doing so, frame pregnancy as something to be safeguarded and successfully completed under supervision. This framing may sensitize pregnancy—heightening its emotional, moral, and social salience—and could make termination appear less acceptable, leading to an increase in the number of unintended pregnancies to be carried to term.

Figure 2 illustrates the pathways through which JSY may influence reproductive outcomes, highlighting how its emphasis on planned and protected pregnancy links program design to changes in fertility and unplanned births.

Taken together, this study examines three primary research questions:

- **Hypothesis 1:** How does fertility respond to the availability of maternal health services under the JSY program?
- **Hypothesis 2:** Does the provision of maternal health services under the JSY program lead to an increase in unplanned births?
- **Hypothesis 3:** If so, is this effect primarily driven by health worker engagement?

Figure 2: Conceptual Framework Linking JSY Components to Reproductive Decisions



4 Data and variable construction

4.1 Data and Variables for Fertility Analysis (H1) and Unplanned Births Analysis (H2)

The primary data source is the National Family Health Survey (NFHS) of India, specifically waves 2 (1998–99), 3 (2005–06), and 4 (2015–16). The NFHS is a nationally representative household survey that provides detailed information on fertility, reproductive health, and maternal and child health outcomes. These three survey rounds span both the periods before and after the introduction of India’s Janani Suraksha Yojana (JSY) program in 2005, allowing for an examination of changes in reproductive behavior associated with this program.

The analysis focuses on rural areas, which were the primary target of JSY and received higher cash incentives for both mothers and Accredited Social Health Activists (ASHAs). The main sample for the H1 fertility analysis consists of women aged 19 to 40, capturing the key childbearing years, who live in rural areas, are above the poverty threshold, and do not belong to Scheduled Castes or Scheduled Tribes⁵.

For the H2 unplanned birth analysis, I focus on women directly eligible for JSY benefits. Because the program provides cash transfers only to those who give birth, exposure can be observed only among women with at least one live birth during the post-treatment period. Limiting the analysis to women with recent births ensures the sample includes those for whom the program was operationally relevant, allowing for a more precise estimate of JSY's impact.⁶ To capture this, I define a recent birth cohort as women who gave birth within the five years preceding the survey. This sample, like that used in the H1 fertility analysis, is restricted to rural women who are above the poverty line and not from Scheduled Castes or Tribes.

The Total Number of Birth The main variable for the H1 fertility analysis is the total number of births a woman had in the five years preceding the survey interview. For NFHS-3 (2005–06), special care is needed because the survey period itself overlaps with the program's launch in April 2005 although most fertility behavior it records predates JSY. To isolate pre-JSY fertility behavior, births resulting from conceptions after April 2005 are excluded. Assuming a nine-month gestation period, this window includes births conceived between April 2000 and March 2005.

Modern contraceptive use To examine the channel of fertility change, I construct a binary indicator for modern contraceptive use. The variable equals one if a woman reports using a modern method of contraception at the time of the survey, and zero otherwise.

⁵In NFHS-2, where data on Below Poverty Line (BPL) card ownership are unavailable, the bottom 20% of the household wealth index is used as a proxy for BPL status.

⁶Potential selection bias from conditioning on recent births is addressed in the robustness checks section, where I examine maternal compositional changes after treatment and also replicate the main results using a broader sample that includes all women, regardless of birth status.

The Unplanned Birth For the unplanned birth analysis using the recent birth cohort, I construct a binary variable indicating whether a woman experienced an unplanned birth within the five years preceding the survey interview. The measure compares a woman's reported ideal number of children to the birth order of her most recent child born during this period. The ideal number is based on her response to the question: *"If you could go back to the time you did not have any children and could choose exactly the number of children to have in your whole life, how many would that be?"* If the birth order of that child exceeds her stated ideal, she is coded as having had an unplanned birth (unplanned birth = 1); otherwise, the variable is coded as zero. In a robustness check using the full sample of women, including those who have never given birth, women with no births during this period are also coded as zero, consistent with defining unplanned birth conditional on having given birth.

Table A.1 presents summary statistics disaggregated by treatment and control groups in the pre- and post-JSY periods. Panel A reports statistics for the H1 fertility analysis sample, and Panel B for the H2 unplanned birth analysis sample, which is limited to women who gave birth within the past five years.

4.2 Data and variables for mechanism (H3. Health worker access)

To test the health worker channel (H3) through which unplanned births may increase, I use data from the second round (2011–12) of the India Human Development Survey (IHDS). IHDS-II is a nationally representative panel survey of over 42,000 households across India, collecting detailed household- and individual-level information on health, education, economic conditions, and fertility behavior, including ideal family size and gender preferences, similar to the NFHS.

Assuming the 2009 JSY reform increased women's access to health workers, I construct a birth-level sample of rural births conceived in or after January 2005, as these conceptions could plausibly have been influenced by the program. The unit of observation is a birth, and the dataset includes multiple births per woman. Following the approach used in

the main analysis, a birth is defined as unplanned if its birth order exceeds the mother's stated ideal number of children.

Panel C of Table A.1 provides summary statistics for the sample used in the health worker channel analysis (H3). The treatment group consists of births from rural women residing in high-performing states (HPS), while the control group includes those in low-performing states (LPS).

Next, to verify that the 2009 reform increased pregnant women's access to health workers, I use data from the District Level Household and Facility Survey (DLHS) round 4 (2012–13). This survey includes births spanning both the period immediately after JSY's initial launch and the years following the 2009 reform, providing a natural setting to assess changes in exposure to health workers.

DLHS-4 collects retrospective information on ASHA interactions related to pregnancy, delivery, and postnatal care. I define a woman as exposed to an ASHA if she reports any of the following⁷: (i) receiving antenatal care from an ASHA, (ii) being assisted by an ASHA during facility delivery, (iii) receiving transport support from an ASHA, or (iv) being informed by an ASHA about danger signs related to diarrhea or pneumonia. This composite measure captures a broad range of ASHA engagement within the maternal and child health services. The sample is restricted to younger women aged 19 to 29 who meet JSY eligibility criteria in high-performing states (HPS). Eligibility in HPS is defined as being below the poverty line or belonging to a Scheduled Caste or Tribe, and having no more than two previous live births.

DLHS-4 covers only High-Performing States (HPS), where the reform applied. I use this variation in timing to implement a Regression Discontinuity (RD) design, which is described in detail in Section 8.2, to estimate its causal impact. Appendix Table A.2 presents summary statistics for this sample.

⁷While the measure is described in terms of ASHA workers, it also includes interactions with Anganwadi workers, consistent with JSY program guidelines that permit either type of frontline worker to provide maternal and child health services.

4.3 Data and Variables for Suggestive Evidence for the Mechanism

To explore suggestive evidence on the mechanism through which access to health workers influences unplanned fertility, I use panel data from rounds 1 and 2 of the India Human Development Survey (IHDS). This analysis is conducted at the woman level, using the subset of women from the IHDS round 2 birth sample (used in the analysis of the H3 health worker channel) who were also surveyed in round 1. I restrict the sample to women who were interviewed after April 2005 in IHDS1, ensuring that all observations were collected after the launch of JSY, and again in IHDS2 (2011–12), after the program’s expansion in 2009. This panel allows me to examine within-woman changes in fertility preferences during a period of increasing access to health workers.

Both rounds of IHDS include questions on fertility intentions, asking whether the woman wants to have more children and whether she believes her husband wants more children than they currently have. These questions make it possible to see how women’s fertility preferences changed and how their views of their husbands’ preferences shifted as health worker access expanded. Based on these responses, I create three binary variables: whether the woman wants more children, whether she thinks her husband wants more, and whether she thinks her husband wants more while she does not, capturing perceived discordance in fertility preferences.

Additionally, to explore whether access to ASHAs influenced attitudes toward abortion, I use data from the World Values Survey (WVS) waves 4 (2001), 5 (2006), and 6 (2012). These waves span the key policy periods: before the launch of JSY, after JSY but before the 2009 reform, and after the reform, respectively. The WVS includes a question on abortion attitudes: *“How justifiable do you consider abortion to be?”* with responses ranging from 1 (Never justifiable) to 10 (Always justifiable). Based on this question, I construct a standardized (z-score) measure of respondents’ attitudes toward abortion.

Table A.3 provides an overview of the datasets and main outcome variables used in the analyses.

5 Empirical Strategy

5.1 Empirical Strategy for H1 Fertility Analysis

To estimate the impact of the JSY program on fertility, I implement a difference in differences (DiD) design that exploits variation in program eligibility across states and socio-economic groups, along with the timing of the program's launch. As discussed in Section 2.2, the JSY program classifies Indian states into Low Performing States (LPS) and High Performing States (HPS). In LPS, all pregnant women are eligible for JSY benefits, whereas in HPS, eligibility is restricted to women who are below the poverty line (BPL) or belong to Scheduled Castes (SC) or Scheduled Tribes (ST).

The treatment group consists of women in LPS who are not eligible for JSY in HPS. Specifically, these are women who are above the poverty line and do not belong to SC or ST groups. The control group includes women with the same socio economic characteristics residing in HPS. The DiD strategy compares fertility outcomes between these two groups before and after the program was introduced. I do not rely on within-state comparisons between eligible and non-eligible women in HPS, since in HPS eligibility is defined by poverty and caste status, which are systematically related to fertility behavior.

To test H1, the outcome is the number of births a woman reports in the five years preceding the survey. I define the pre-treatment period as observations for which the entire recall window precedes JSY's introduction in April 2005, and the post-treatment period as those for which the entire recall window falls after the program was introduced. This ensures that treatment assignment reflects exposure to JSY during the period when fertility decisions were made.

I estimate the following equation:

$$Num_Births_{isy} = \alpha_0 + \alpha_1 Treat_{is} \times Post_y + X'_{iy} \alpha + \zeta_y + \lambda_s + \epsilon_{isy} \quad (1)$$

where the dependent variable, Num_Birth_{ist} , represents the number of births over the

past five years for woman in state s from the interview year y . The key independent variable, $\text{Treat}_{is} \times \text{Post}_y$, is a dummy indicating whether the woman resides in an LPS and her most recent conception within the five-year recall window occurred after the launch of the JSY program. The vector X_{iy} includes woman i 's age, years of education, partner's education level, number of children over age five, religion (indicators for Muslim and Hindu), household head's age, and an indicator for whether the household is female-headed. Fixed effects for year of interview (ζ_y) and state (λ_s) are included. Standard errors are clustered at the state level, given that the JSY program was implemented at the state level.

5.2 Empirical Strategy for H2 Unplanned Birth Analysis

To assess whether improved access to maternal and child health (MCH) services increased the likelihood of unplanned births, I implement the difference-in-differences (DiD) strategy introduced in Section 5.1 using the recent-birth cohort. The analysis compares the most recent birth within the past five years for women who are above the poverty line and from upper-caste households in Low-Performing States (LPS) to those of comparable women in High-Performing States (HPS). I estimate the following specification to test Hypothesis 2:

$$\text{Unplanned_bir}_{ibsty} = \beta_0 + \beta_1 \text{Treat}_{is} \times \text{Post}_t + X'_{iy} \boldsymbol{\beta} + \theta_b + \eta_t + \lambda_s + \zeta_y + u_{ibsty} \quad (2)$$

Here, $\text{Unplanned_bir}_{ibsty}$ is an indicator for whether the woman i 's most recent birth was unplanned, where b is the order of that birth and t is its year, s indexes the state, and y is the interview year. The key explanatory variable, $\text{Treat}_{is} \times \text{Post}_t$, is an interaction term equal to one for women in the treatment group whose most recent birth was conceived after the launch of JSY in 2005. The regression includes fixed effects for birth order (θ_b), birth year (η_t), state (λ_s), and interview year (ζ_y), with standard errors clustered at the state level.

6 Main Results

6.1 Results for H1 Fertility Analysis

Table 2 reports estimates from Equation 1. Column (1) presents results for all women aged 19 to 40, while Columns (2) and (3) report separate estimates for younger women aged 19 to 29 and older women aged 30 to 40.

Table 2: The Impact of JSY on the Number of Births

	Number of births over the past 5 years		
	(1) All women	(2) Young women (19-29)	(3) Older women (30-40)
Treat $\times$ Post	-0.034 (0.029)	0.007 (0.048)	-0.100*** (0.029)
Control mean (pre)	0.666	0.975	0.351
Observations	140832	70821	70011
Fixed effects	✓	✓	✓
Controls	✓	✓	✓

Note: The sample includes higher-caste women above the poverty line in rural areas from NFHS waves 2 (1998-99), waves 3 (2005-06) and 4 (2015-16). Regressions control for the woman's age, the household head's age and an indicator for whether the head's age is missing, the woman's years of education, her partner's education level, the number of children over age five, indicators for Muslim and Hindu religion, and whether the household is female-headed. All models include fixed effects for year of interview and state. Standard errors are clustered at the state level and are presented in parentheses. Significance is indicated as follows: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

The results suggest that access to maternal and child health (MCH) services under JSY had different effects across age groups. Among older women, program exposure is associated with a reduction of 0.10 births over the past five years, significant at the 1 percent level. This decline corresponds to a 28 percent reduction relative to the pre-treatment mean in high-performing states (HPS). In contrast, there is no statistically significant change in fertility among younger women.

The observed fertility decline among older women is consistent with the intended role

of JSY health workers in promoting family planning. As discussed in section 3, ASHAs, while primarily tasked with encouraging institutional delivery, also distribute contraceptives and provide counseling on birth spacing and fertility limitation (Saxena et al., 2018, 2021; Moughalian et al., 2024). These responsibilities position them to reduce informational and access barriers to contraception, particularly for women who have completed or nearly completed their desired fertility.

If this mechanism is at play, we would expect to see an increase in contraceptive use following JSY exposure, particularly among older women who exhibit fertility reductions. To test this, I estimate JSY's effect on current use of modern contraceptive methods, using the same difference-in-differences strategy as in the fertility analysis (Equation 1). The outcome is a binary indicator equal to one if the woman reports using any modern method of contraception at the time of the survey.⁸

Table 3 presents results separately by age group. Among older women, JSY exposure leads to a significant increase in modern contraceptive use. This pattern supports the interpretation that health worker engagement under JSY enabled women nearing their desired family size to adopt fertility-limiting methods, contributing to the observed decline in recent births.

Among younger women, contraceptive use also increases, yet fertility remains unchanged. One explanation is that younger women primarily adopt contraception to space rather than to limit childbearing (Westoff and Koffman, 2010; Timæus and Moultrie, 2020). Another possibility is that JSY influenced post-conception decisions by heightening the salience of pregnancy. These changes may have increased the likelihood that unintended pregnancies were carried to term, offsetting any fertility-reducing effects of contraception. The next section investigates this channel by examining whether JSY affects the probability that unintended pregnancies result in live births.

⁸This analysis uses data from NFHS waves 2 and 4. Wave 3 is excluded because its interview period overlaps with the rollout of JSY, making it difficult to consistently classify treatment status for contemporaneous outcomes, unlike in the fertility analysis based on retrospective birth histories.

Table 3: The Impact of JSY on Modern Contraceptive Use

	Currently using modern contraceptive method		
	(1) All women	(2) Younger (19-29)	(3) Older (30-40)
Treat $\times$ Post	0.063** (0.031)	0.070** (0.032)	0.069* (0.036)
Control mean (pre)	0.505	0.375	0.644
Observations	122002	61151	60851
Fixed effects	✓	✓	✓
Controls	✓	✓	✓

Note: The sample includes higher-caste women above the poverty line in rural areas from NFHS waves 2 (1998-99) and 4 (2015-16). Regressions control for the woman's age, the household head's age and an indicator for whether the head's age is missing, the woman's years of education, her partner's education level, the number of children, indicators for Muslim and Hindu religion, and whether the household is female-headed. All models include fixed effects for year of interview and state. Standard errors are clustered at the state level and are presented in parentheses. Significance is indicated as follows: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

6.2 Results for H2 Unplanned Births Analysis

Table 4 reports the estimated effects of JSY on unplanned births. Columns (1)–(3) compare the post-program period to the pre-program period using a single post-treatment indicator. Columns (4)–(6) divide the post-program period into an early diffusion phase (2005–2008), before the 2009 reform that introduced ASHA cash incentives in HPS, and a later diffusion phase (2009–2016). Thus, the key variables of interest are $Treat \times Post$, $Treat \times Post(2005–2008)$, and $Treat \times Post(2009–2016)$.⁹

The estimates indicate that JSY increased unplanned births. In Column (1), program exposure raises the probability of having an unplanned birth by 3.2 percentage points, significant at the 10 percent level. Column (4) shows that exposure during the early diffusion period increases this probability by 4.3 percentage points (significant at the 1 percent level), while the estimate for the later period is 3.2 percentage points (significant at the 10

⁹Note that ASHA incentives were part of JSY from the start in LPS, and the 2009 reform mainly extended them to HPS. Accordingly, there is no strong expectation that late-period effects should be larger for the treatment group defined in LPS.

percent level).

The heterogeneity analysis by age indicates that this increase is concentrated among younger women. Columns (2) and (3) report that program exposure raises the probability of an unplanned birth among younger women by 4.2 percentage points, significant at the 1 percent level, with no statistically significant effect observed among older women. In Columns (5) and (6), both the early and later diffusion phases of the program significantly increase unplanned births among younger women, but not among older women.

Table 4: The Impact of JSY on Unplanned Births

	Unplanned birth					
	(1) All	(2) Younger	(3) Older	(4) All	(5) Younger	(6) Older
Treat $\times$ Post	0.032* (0.017)	0.042*** (0.015)	0.020 (0.029)			
Treat $\times$ Post (2005-2008)				0.043*** (0.015)	0.056*** (0.016)	-0.001 (0.032)
Treat $\times$ Post (2009-2016)				0.032* (0.017)	0.042** (0.015)	0.021 (0.030)
Control mean (pre)	0.278	0.205	0.507	0.278	0.205	0.507
Observations	63213	46465	16748	63213	46465	16748
Fixed effects	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓

Note: The dependent variable is an indicator for whether a woman's most recent birth in the past five years was unplanned. The sample includes higher-caste women above the poverty line in rural areas from NFHS waves 2 (1998-99), waves 3 (2005-06) and 4 (2015-16). Regressions control for the woman's age, the household head's age and an indicator for whether the head's age is missing, the woman's years of education, her partner's education level, indicators for Muslim and Hindu religion, and whether the household is female-headed. All models include fixed effects for birth year, birth order, interview year, and state. Standard errors are clustered at the state level and reported in parentheses. Significance is indicated as follows: * $p < 0.1$, ** $p < 0.05$, *** $p < .001$.

The rise in unplanned births among younger women, together with the null effect on fertility in Section 6.1, helps explain why fertility did not decline in this group despite increased contraceptive use. These results are consistent with more unintended pregnancies resulting in births. Among older women, fertility declined and unplanned births did

not rise, consistent with improved fertility control in this group.

A potential concern is that this analysis conditions on having given birth in the past five years, which could be problematic if the composition of younger women who give birth changes in treated states. Differences at the extensive margin are addressed in Table 2, which shows no change in the number of births among younger mothers. However, there may still be changes in the composition of mothers in treated areas. This concern is partially addressed in Section 10.3 through a robustness check that examines shifts in maternal characteristics, following [Bossavie et al. \(2023\)](#), and by replicating the analysis using the full sample of ever-married women, including those who have never given birth.

In sum, these results suggest that maternal and child health (MCH) services under JSY affected fertility behavior differently by age, increasing unplanned births among younger women while improving fertility control among older women. In the next section, I test for pre-trends to assess the validity of the difference-in-differences design. I then isolate the health worker channel to better understand the mechanism underlying these effects.

7 Validation of Empirical Design

The empirical strategy in this paper relies on the Difference-in-Differences (DiD) approach. A key identifying assumption is that, in the absence of the program, the treatment and control groups would have followed parallel trends. In this section, I assess this assumption by examining whether the number of births and the probability of unplanned births evolved similarly between treatment and control groups in the pre-treatment period.

7.1 Test for Parallel Pre-Trends in Fertility Analysis (H1)

I begin by testing for parallel pre-trends in fertility outcomes. Following [Griffith and Noonan \(2022\)](#), I estimate a regression in Equation 3 using data from the pre-treatment period. The dependent variable is the number of births a woman reports in the five years prior to the interview. $Year_y$ denotes the year of interview and accounts for common time trends across states. The interaction term between treatment status and interview year ($Treat_{is} \times Year_y$) captures any differential trends between the treated and control groups before the program. If the parallel trend holds, the interaction should be statistically insignificant ($\tilde{\alpha}_2 = 0$). The estimating equation is as follows:

$$Num_Births_{isy} = \tilde{\alpha}_0 + \tilde{\alpha}_1 Year_y + \tilde{\alpha}_2 Treat_{is} \times Year_y + X'_{iy} \tilde{\alpha} + \lambda_s + \epsilon_{isy}, \quad (3)$$

In Table A.4, estimates of Equation 3 indicate that the null of parallel pretrends is rejected at the 10% level across specifications.

The rejection of parallel pretrends appears driven by Uttar Pradesh (UP). As India's most populous state, UP exhibited fertility rates substantially above the national average in the pre-JSY period. The state's total fertility rate stood at 4.0 in NFHS-2 (1998–99) and 3.8 in NFHS-3 (2005–06)—the highest or near-highest among major states ([Halli et al., 2019](#); [Ministry of Finance, 2025](#)). This elevated baseline, combined with UP's substantial weight in the sample, could have made the linear pre-trend specification particularly sensitive to this state's trajectory. To assess robustness, I re-estimate Equation 3 excluding UP. The pre-trend test no longer rejects at conventional significance levels for the remaining states (Table A.5).

As an additional robustness check for violations of the parallel trends assumption, I implement the sensitivity check proposed by [Kahn-Lang and Lang \(2020\)](#), following the approach of [Griffith and Noonan \(2022\)](#). The method allows for differential linear trends between treatment and control groups in the pre-period and examines whether a treatment effect remains after extrapolating these trends into the post-period. Specifically, I estimate:

$$Num_Births_{isy} = \check{\alpha}_0 + \check{\alpha}_1 Year_y + \check{\alpha}_2 Treat_{is} \times Year_y + \check{\alpha}_3 Treat_{is} \times Post_y + X'_{iy} \check{\alpha} + \lambda_s + \epsilon_{isy} \quad (4)$$

In this specification, $(\check{\alpha}_1 + \check{\alpha}_2)$ captures the trends in treated states. This trend is then extended into the post-treatment period to estimate what would have happened in treated states had the program not been implemented. The coefficient $\check{\alpha}_3$ measures how much actual outcomes in treated states differ from this projected trend. A statistically significant $\check{\alpha}_3$ indicates that the treatment effect still exist.

The results from the sensitivity analysis in Table 5 remain qualitatively consistent with the main DiD estimates reported in Table 2. Once differential pre-treatment trends are extrapolated into the post-period, the estimated effect of JSY on fertility among older women remains large and statistically significant. Specifically, the magnitude increases from a 0.10 to a 0.372 reduction in births, suggesting that the original estimate is not driven by modest trend differences. In contrast, for younger women, the treatment effect remains close to zero and statistically insignificant across both specifications. These findings reinforce the interpretation that JSY reduced fertility among older women, while having no measurable effect on younger women's childbearing.

7.2 Test for Parallel Pre-Trends in Unplanned Birth Analysis (H2)

Next, I test for pre-treatment trends in unplanned births using the specification in Equation 5 on the pre-treatment sample. In this regression, the time variable is the year of birth, and treatment status is interacted with birth year to assess whether treated and control groups exhibited differential trends prior to JSY. The model includes standard controls, along with fixed effects for birth order (θ_b), state (λ_s), and interview year (ζ_y), as in the main analysis.

The estimating equation is:

Table 5: Regressions with Non-Parallel Trends: Following Kahn-Lang and Lang

	Number of births over the past 5 years		
	(1) All women	(2) Younger (19-29)	(3) Older (30-40)
Year of Interview	-0.007*** (0.001)	-0.010*** (0.002)	-0.005*** (0.001)
Year of Interview $\times$ Treat	0.011** (0.004)	0.003 (0.006)	0.019*** (0.003)
Treat $\times$ Post	-0.184*** (0.053)	-0.019 (0.075)	-0.372*** (0.040)
Control mean (pre)	0.666	0.975	0.351
Observations	140832	70821	70011
Fixed effects	✓	✓	✓
Controls	✓	✓	✓

Note: The sample includes higher-caste women above the poverty line in rural areas from NFHS waves 2 (1998-99), waves 3 (2005-06) and 4 (2015-16). Regressions control for the woman's age, the household head's age and an indicator for whether the head's age is missing, the woman's years of education, her partner's education level, the number of children over age five, indicators for Muslim and Hindu religion, and whether the household is female-headed. All models include state fixed effects. Standard errors are clustered at the state level and are presented in parentheses. Significance is indicated as follows: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

$$Unplanned_bir_{ibsty} = \tilde{\beta}_0 + \tilde{\beta}_1 Birth_year_t + \tilde{\beta}_2 (Treat_s \times Birth_year_t) + X'_{iy} \tilde{\beta} + \theta_b + \zeta_y + \lambda_s + u_{ibsty} \quad (5)$$

Table 6 reports the estimates. Across all groups, the interaction terms are statistically insignificant, consistent with the parallel trends assumption in unplanned birth outcomes during the pre-treatment period.

Event-Study for Nonlinear Pre-Trends. To complement the linear trend test, I also examine potential non-linear pre-treatment dynamics. I estimate an event-study specification restricted to pre-treatment birth years, following Autor (2003), Borusyak et al. (2024), Bossavie et al. (2023), and Salemi (2021). The model interacts treatment status with year-of-birth indicators, using 2004 as the omitted reference year. If trends were parallel, the coefficients on pre-treatment interactions (1994–2003) should be small and statistically in-

significant.

The estimating equation is:

$$Unplanned_bir_{ibsty} = \check{\beta}_0 + \sum_{\tau \neq 2004} \check{\beta}_\tau (\text{Treat}_{is} \times \text{Birth_year}_{t=\tau}) + X'_{it} \check{\beta} + \theta_b + \lambda_s + \zeta_y + u_{ibsty} \quad (6)$$

where $\check{\beta}_\tau$ captures the difference in outcomes between treated and control groups for each birth cohort τ relative to 2004.

Table 6: Pre-Treatment Trends in Unplanned Births

	Unplanned birth		
	(1)	(2)	(3)
	All	Younger	Older
Year of Birth	0.001 (0.003)	-0.003 (0.003)	0.010 (0.007)
Year of Birth $\times$ Treat	0.003 (0.003)	0.002 (0.002)	0.004 (0.005)
Control mean	0.278	0.205	0.507
Observations	19026	14161	4865
Fixed effects	✓	✓	✓
Controls	✓	✓	✓

Note: The sample includes higher-caste women above the poverty line in rural areas from NFHS waves 2 (1998-99), waves 3 (2005-06) and 4 (2015-16). Regressions control for the woman's age, the household head's age and an indicator for whether the head's age is missing, the woman's years of education, her partner's education level, indicators for Muslim and Hindu religion, and whether the household is female-headed. All models include fixed effects for birth order, interview year, and state. Standard errors are clustered at the state level and reported in parentheses. Significance is indicated as follows: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Panels A–C of Figure B.1 plot the estimated coefficients with 90% confidence intervals separately for all women, younger women, and older women. Pre-treatment estimates are small in magnitude and statistically indistinguishable from zero across most birth-year cohorts, providing visual support for the parallel trends assumption.

To formally test this, I follow Borusyak et al. (2024) and Salemi (2021) in assessing whether the pre-treatment coefficients are jointly significant. Table A.6 reports the corre-

sponding F-statistics and p -values. The results fail to reject parallel pretrends for the full sample and the younger cohort. However, the test performs less well in the older cohort, likely due to smaller sample size and limited variation across pre-policy cohorts in that subgroup.

8 Mechanism Analysis: Health Worker Channel

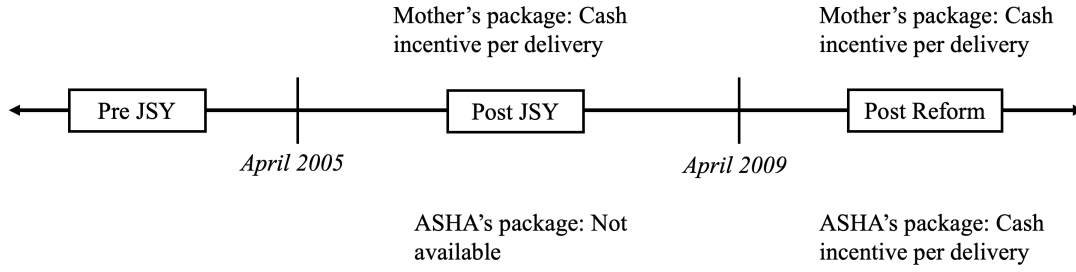
This section examines whether the engagement of health workers helps explain the rise in unplanned births among young women. I use the 2009 reform of the JSY program, which extended performance-based incentives to ASHAs in high-performing states (HPS). The reform provides variation that allows me to study how incentivizing ASHAs affected reproductive outcomes. I first examine the impact of the incentive introduction on unplanned births, under the assumption that the incentives increased women's access to health workers. I then test this assumption directly, using a regression discontinuity approach to assess whether the reform expanded women's exposure to ASHAs.

8.1 Effect of Health-Worker Incentives on Unplanned Births

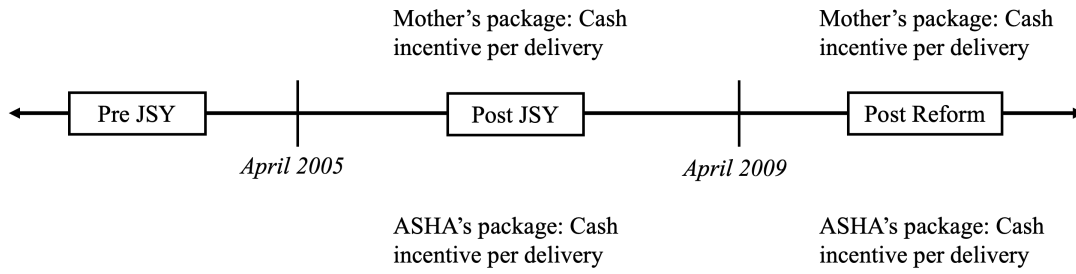
To identify this effect, I implement a secondary difference-in-differences (DiD) strategy. This approach compares JSY-eligible women—those below the poverty line (BPL) or from Scheduled Castes or Tribes (SC/ST)—across high-performing states and low-performing states.

Figure 3 presents the timeline of incentive changes under JSY in HPS and LPS. As shown in the figure, cash incentives to mothers were introduced in April 2005, while performance-based incentives for ASHAs were extended to HPS only in April 2009. Before this reform, ASHAs in LPS were already incentivized, but those in HPS were not. After 2009, ASHAs in HPS became eligible for payments per institutional delivery, likely increasing their interactions with pregnant women. This variation in ASHA incentives

Figure 3: Timeline of Incentive Changes under JSY



Panel A: High Performing States



Panel B: Low Performing States

forms the basis for identifying the causal effect of increased access to health workers on reproductive outcomes.¹⁰

The treatment group consists of JSY-eligible women in HPS who gained exposure to incentivized ASHAs after April 2009. The control group includes comparable women in LPS, who were exposed to incentivized ASHAs both before and after the reform. Because the only policy change affecting the treatment group was the introduction of ASHA incentives, this comparison isolates the incentive-driven effect of health workers.

The estimating equation is as follows:

$$Unplanned_Bir_{cbmst} = \gamma_0 + \gamma_1 HPS_{mst} \times Post_t + X'_{cbhst} \gamma + \eta_t + \theta_b + \mu_m + \omega_{cbmst} \quad (7)$$

Here, $Unplanned_Bir_{cbhst}$ is an indicator for whether birth c of birth order b from mother m in state s at time t was unplanned. The interaction term $HPS_{mst} \times Post_t$ cap-

¹⁰Section 8.2 provides evidence that the reform increased women's exposure to ASHAs.

tures exposure to incentivized ASHAs. The specification includes fixed effects for birth year (η_t), birth order (θ_b), and mother (μ_m). Standard errors are clustered at the state level.

The estimation results show that performance-based incentives for health workers increased unplanned births, with effects concentrated among younger women. Table 7 reports the estimates from regression 7. Column (1) shows that the likelihood of an unplanned birth significantly rises by 4.1 percentage points following the introduction of ASHA incentives. Columns (2) and (3) show that this effect is driven by younger women, among whom the probability increases by 5 percentage points (significant at the 5% level). No statistically significant effect is observed for older women.

These results closely mirror the main findings in Table 4, where the overall JSY program also increased unplanned births, particularly among younger women. Under the assumption that the new incentives expanded women's access to ASHAs, the evidence suggests that increased health worker engagement is an important channel through which the JSY program affected reproductive outcomes. The following section tests directly whether the reform increased women's exposure to ASHAs.

Table 7: The Impact of Health Worker Access on Unplanned Births

	Unplanned birth		
	(1) All	(2) Younger	(3) Older
HPS $\times$ Post	0.041** (0.018)	0.050** (0.019)	0.010 (0.045)
Control mean (pre)	0.329	0.329	0.329
Observations	4726	3192	1532
Fixed effects	✓	✓	✓

Note: The dependent variable is an indicator for whether the birth was unplanned. The data come from IHDS-II (2011–12). The sample is restricted to births to rural women from Scheduled Castes or Tribes or those below the poverty line. All regressions include fixed effects for birth year, birth order, state, and mother. Standard errors, clustered at the state level, are reported in parentheses. Significance is indicated as follows: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

8.2 Testing the Mechanism: Did the Reform Increase Exposure to ASHAs?

This section tests whether the 2009 reform increased women's access to ASHAs in high-performing states. Verifying this first-stage relationship is important for interpreting the main results in the previous section, which attribute changes in unplanned births to increased exposure to health workers.

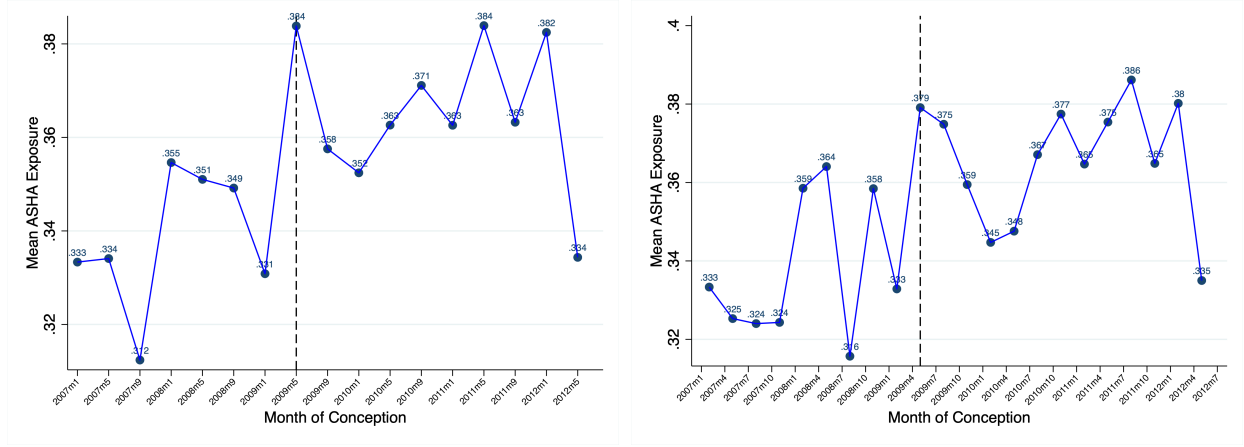
The analysis uses data from DLHS-4 (2012–13), which includes retrospective information on ASHA involvement during pregnancy, delivery, and postnatal care. As detailed in the data section, I construct a composite indicator for ASHA exposure based on reported interactions related to antenatal care, delivery support, transport assistance, and health education. The estimation sample includes JSY-eligible younger women aged 19 to 29 in HPS.

Figure 4 presents average ASHA exposure by estimated conception cohort, using four-month intervals (Panel A) and three-month intervals (Panel B). Conception dates are calculated as nine months prior to the reported birth month to approximate gestational timing. All panels in Figure 4 shows a clear increase in ASHA exposure beginning in May 2009, coinciding closely with the April 2009 policy implementation. In Panel A, Between January 2007 and April 2009, average exposure fluctuated between 31.2% and 35.5%. After the reform, it rose to between 33.4% and 38.4%, with a notable increase from 33.1% in Jan 2009 to 38.4% in May 2009. A similar upward shift is evident in Panel B. Based on this visual evidence, I define May 2009 as the empirical cutoff in the analysis.

Figure 5 illustrates this discontinuity graphically. Panel A divides the sample into four-month bins on each side of the cutoff, while Panel B uses three-month bins. Each dot represents the average exposure within a bin, the solid lines show local linear fits estimated separately before and after the cutoff, and the vertical bars indicate 90 percent confidence intervals. Both panels reveal a visible upward shift in exposure at the May 2009 threshold.

To estimate the effect of the reform on ASHA exposure, I implement a sharp regression discontinuity (RD) design centered at the May 2009 conception threshold, following the

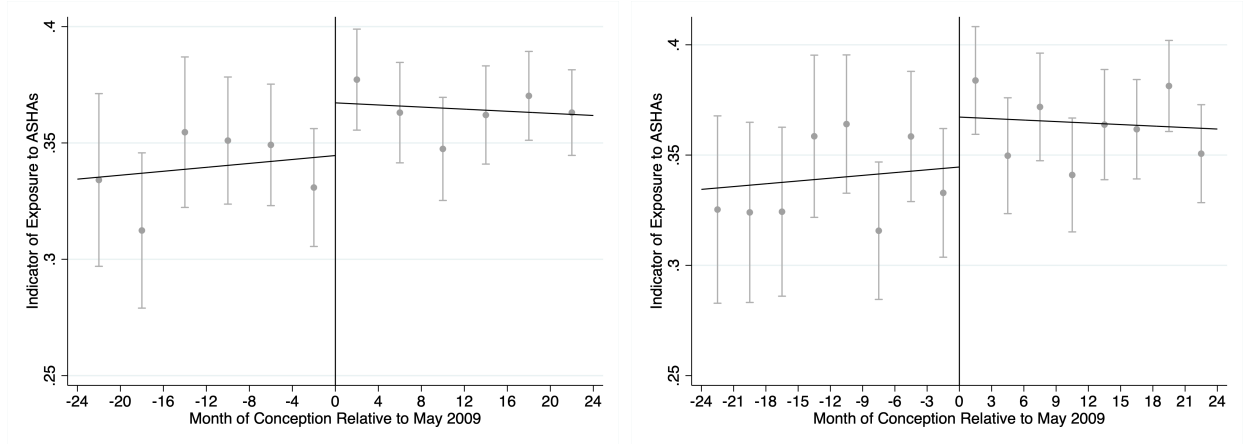
Figure 4: ASHA Exposure by Conception Cohort



Panel A: 4-Month Intervals

Panel B: 3-Month Intervals

Figure 5: ASHA Exposure around the 2009 Reform



Panel A: 4-Month Bins around Cutoff

Panel B: 3-Month Bins around Cutoff

approach in [Sun and Zhao \(2016\)](#). The running variable is the month of conception, and the outcome is the ASHA exposure indicator. The sharp RD framework is appropriate because the policy was implemented simultaneously across all HPS around the threshold.

The RD parameter is defined as:

$$\tau_{RD} = \lim_{c \uparrow l^*} E[\text{exposure}_i \mid c_i = l^*] - \lim_{c \downarrow l^*} E[\text{exposure}_i \mid c_i = l^*] \quad (8)$$

where c_i denotes the conception date and l^* represents the reform threshold in May 2009. I estimate this parameter using local linear regressions, implementing multiple specifications that vary by bandwidth selection (MSERD and CERRD)¹¹ and kernel function (uniform and triangular), as recommended by [Calonico et al. \(2014\)](#) and [Cattaneo et al. \(2020\)](#). These alternative specifications allow for sensitivity checks around the choice of smoothing and weighting.

As an additional sensitivity check, I estimate a pooled OLS specification with an RD structure using the full sample, without restricting the bandwidth:

$$\text{Exposure}_i = \beta_0 + \beta_1 \mathbb{1}(c_i \geq l^*) + f(c_i - l^*) + \mathbb{1}(c_i \geq l^*) \times f(c_i - l^*) + \varepsilon_i,$$

where $\mathbb{1}(c_i \geq l^*)$ is an indicator for conception at or after the May 2009 threshold, and $f(c_i - l^*)$ denotes a linear polynomial in the number of months relative to the cutoff.

Table 8 reports estimates from the regression discontinuity (RD) analysis. Columns (1) through (4) present RD estimates using local linear regressions with different bandwidth selection methods and kernel functions. Specifically, Columns (1) and (2) use the MSERD criterion with uniform and triangular kernels, while Columns (3) and (4) use the CERRD criterion. Column (5) reports a pooled OLS estimate using the full sample as a robustness check. All standard errors are clustered by month of conception.

Across all RD specifications, the results consistently indicate a significant increase in ASHA exposure following the 2009 reform. The estimated effects range from 5.0 to 7.2 percentage points and are statistically significant at the 1 percent level. The OLS estimate in Column (5) also shows a significant increase of 4.0 percentage points, significant at the 5 percent level, providing further support for the main findings.

These results confirm that the reform substantially increased access to health workers among younger women in high-performing states. This validates the use of the reform as a source of exogenous variation in previous analysis of unplanned births in Section 8.1.

¹¹The mean squared error–optimal selector (MSERD) minimizes mean squared error and is well suited for point estimation. The coverage error–optimal selector (CERRD) is designed for robust bias-corrected confidence intervals, delivering faster coverage error decay rates ([Calonico et al., 2017](#)).

Table 8: The Impact of the 2009 Reform on Health Worker Access

	Exposure to ASHAs (d)				
	(1)	(2)	(3)	(4)	(5)
RD estimate	0.050*** (0.015)	0.067*** (0.013)	0.063*** (0.022)	0.072*** (0.011)	
OLS estimate					0.040** (0.012)
Bandwidth (months)	8	8	7	7	
BW selection	MSERD	MSERD	CERRD	CERRD	
Kernel	Uniform	Trinangular	Uniform	Triangular	
Observations	4541	4541	3927	3927	21500

Note: The sample includes younger rural women who are either from Scheduled Castes or Tribes (SC/ST) or below the poverty line (BPL), and who had a pregnancy in the past five years. It is further restricted to those with fewer than three births, reflecting JSY eligibility criteria in high-performing states (HPS). Standard errors, clustered at the month of conception level, are reported in parentheses. Significance is indicated as follows: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

9 Behavioral Responses Underlying the Health Worker Channel

This section explores potential mechanisms through which access to health workers may have contributed to the observed increase in unplanned births, with a specific focus on younger women. Specifically, I test whether increased exposure to health workers affected (i) women's perceived alignment of fertility preferences within couples and (ii) attitudes toward abortion.

9.1 Perceived Discordance in Fertility Preferences

This subsection investigates whether increased access to community health workers heightened women's perception of discordance in fertility preferences within their households. Perceived discordance arises when a woman believes that her own fertility intentions differ from those of her husband, specifically when she wants to stop childbearing but

thinks her husband wants more children. In male-dominated contexts, including India, reproductive outcomes tend to align with men’s preferences (Komura, 2013; Doepke and Tertilt, 2018; Mishra and Parasnis, 2021; Thomson, 1997). As a result, women who perceive such misalignment may be unable to act on their own fertility intentions, resulting in outcomes that reflect their husbands’ preferences rather than their own.

This mechanism is particularly relevant in the context of JSY, which relies on ASHAs to bridge the public health system and households. While ASHAs are primarily tasked with promoting institutional delivery, they also counsel women on family planning and birth spacing (Ministry of Health and Family Welfare, Government of India, 2006). Through repeated contact, these interactions may lead women to update their own fertility preferences and become more inclined to stop childbearing, while continuing to believe that their husbands still want additional children.

Such asymmetric belief updating can increase the share of women who perceive that they do not want more children whereas their husbands do, reinforcing perceived discordance within couples. When women have limited autonomy over reproductive decisions, this belief gap can constrain their ability to act on their own intentions once conception occurs, increasing the likelihood that unintended pregnancies are carried to term.

To empirically assess this mechanism, I use the same JSY-eligible sample described in Section 8, restricting it to women observed in both Waves 1 (2004–05) and 2 (2011–12) of the India Human Development Survey (IHDS). This panel structure enables comparison of reported fertility preferences before and after the 2009 reform.¹² Fertility preferences are measured using two survey questions: whether the woman herself wants more children and whether she thinks her husband wants more children. The key outcome is a binary variable, *husb_want_more*, coded as one if the woman reports that she does not want additional children but believes her husband does, capturing perceived discordance in fertility preferences.

The empirical specification is as follows:

$$y_{mst} = \delta_0 + \delta_1 \text{HPS}_{ms} \times \text{Post}_t + X'_{mst} \delta + \lambda_s + \mu_m + \epsilon_{mst} \quad (9)$$

¹²The sample is restricted to women interviewed after the 2005 launch of JSY so that their fertility preferences reflect the post-program period.

where y_{mst} denotes outcomes for individual m in state s at time t , including the woman's desire for more children, her perceived husband's desire, and the perceived-discordance indicator *husb_want_more*. $Post_t$ equals one for interviews conducted after April 2009, when ASHA incentives were expanded to High-Performing States (HPS). HPS_{ms} equals one if the respondent resides in an HPS. The interaction term captures the differential change in outcomes in HPS following the reform. The model includes woman and state fixed effects, and standard errors are clustered at the state level.

Table 9 reports the estimated effects of health-worker access on perceived fertility preferences. Among younger women (Panel B), exposure to the JSY reform leads to a significant decline in women's own stated desire for more children, by 19.6 percentage points (significant at the 10% level), and a significant rise of 7.3 percentage points in the share of women who report that they do not want more children but believe their husbands do. No comparable effects are observed among older women (Panel C), further underscoring that the effects of JSY on unplanned births are concentrated among younger women.

These results are consistent with the perceived-discordance mechanism. Exposure to ASHAs appears to have shifted women's own fertility preferences without changing how they perceive their husbands' preferences, thereby widening the perceived gap within couples. In male-dominated contexts, such belief gaps can constrain women's ability to act on their own reproductive intentions and help explain the rise in unplanned births among younger women documented in Section 8.

Table 9: The Impact of Health Worker Access on Perceived Discordance in Fertility Preferences

	Husband–Wife desire for more children		
	(1)	(2)	(3)
	Wife wants	Husband wants	Husband wants more
Panel A: All women			
HPS $\times$ Post	-0.169* (0.086)	-0.115 (0.085)	0.027 (0.036)
Control mean (pre)	0.551	0.564	0.037
Observations	690	690	690
Panel B: Younger women			
HPS $\times$ Post	-0.196* (0.112)	-0.082 (0.112)	0.073* (0.040)
Control mean (pre)	0.725	0.754	0.049
Observations	276	276	276
Panel C: Older women			
HPS $\times$ Post	-0.124 (0.105)	-0.121 (0.115)	-0.023 (0.035)
Control mean (pre)	0.459	0.462	0.030
Observations	414	414	414
Fixed effects	✓	✓	✓
Controls	✓	✓	✓

Note: The dependent variable in Columns (1) and (2) indicates whether the wife or her husband (as perceived by the wife) reports a desire for more children. Column (3) is an indicator for perceived spousal discordance in fertility preferences, equal to one if the wife reports not wanting more children but believes her husband does. The data come from the balanced panel of IHDS-I (2005–06) and IHDS-II (2011–12). Panel A includes the sample of rural women from Scheduled Castes or Tribes, or those below the poverty line. Panel B restricts the sample to younger women aged 19–29, and Panel C to older women aged 30–40. All regressions control for the number of children, an indicator for current pregnancy, the woman’s age and education, and her husband’s education. State and individual fixed effects are included. Standard errors, clustered at the state level, are reported in parentheses. Significance is indicated as follows: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

9.2 Attitudes Toward Abortion

Another potential mechanism linking increased access to health workers with higher unplanned fertility is a shift in abortion-related attitudes. This channel may operate through several pathways. First, repeated engagement with community health workers may increase the perceived moral or social cost of abortion. Studies show that health workers often hold conservative views on abortion and can influence perceptions through sustained interactions that reinforce stigma, in India ([Nandagiri, 2019](#); [Sunil, 2022](#)) as well as in other low- and middle-income countries ([Glenton et al., 2017](#)).

Even when they do not personally hold such conservative views, the performance-based incentive structure of JSY may create financial motives for ASHAs to discourage abortion. According to [Javadekar et al. \(2025\)](#), ASHAs who receive payments conditional on institutional delivery may dissuade households from terminating pregnancies.

Additionally, qualitative evidence from India documents women's concerns about maintaining confidentiality regarding abortion when interacting with community health workers ([Gupta et al., 2017](#)). Under JSY, ASHAs are expected to know about women's pregnancies. The very fact that health workers are aware of a pregnancy may make households reluctant to consider abortion, as the decision may no longer remain private.

To examine whether abortion attitudes shifted following the JSY reform, I use data from Waves 4 (2001), 5 (2006), and 6 (2012) of the World Values Survey (WVS). Respondents were asked, "*How justifiable do you consider abortion?*" on a scale from 1 ("*never justifiable*") to 10 ("*always justifiable*"). I construct a standardized measure reflecting respondents' justification of abortion. The sample includes both men and women and is not restricted by age, as spousal age gaps are common in India, and broader shifts in abortion attitudes may influence fertility outcomes among younger women.

I exploit the phased rollout of JSY, which introduced cash incentives to mothers beginning in 2005 and expanded payments to health workers in 2009 in high-performing states (HPS). To estimate changes in abortion attitudes associated with program exposure, I implement a triple-differences (DDD) strategy that compares outcomes across both states

(HPS versus LPS) and three time periods: before JSY (pre-2005), during the initial rollout (2005–2008), and after the expansion of ASHA incentives (2009 onward).

The estimating equation is as follows:

$$\begin{aligned} Z_JustAbortion_{ist} = & \gamma_0 + \gamma_1 HPS_{is} \times Post(2005-2008)_t + \gamma_2 HPS_{is} \times Post(2009-)_t \\ & + X_{ist} + \delta_t + \lambda_s + \epsilon_{ist} \end{aligned} \quad (10)$$

In this equation, $Z_JustAbortion_{ist}$ is the standardized measure of abortion justification for individual i in state s at time t . The variable HPS_{is} is an indicator for whether the individual resides in a high-performing state. $Post(2005-2008)_t$ is an indicator for the early JSY diffusion period (2005–2008), while $Post(2009-)_t$ represents the later diffusion phase following the 2009 reform. The term X_{ist} represents a vector of individual-level control variables. δ_t and λ_s denote year and state fixed effects, respectively. Standard errors are clustered at the state level and denoted by ϵ_{ist} . The coefficient of interest, γ_2 , captures the change in attitudes toward abortion associated with increased access to health workers during the later diffusion period in high-performing states.

Table 10 presents the estimated effects of JSY on attitudes toward abortion. The results indicate that increased exposure to health workers following the 2009 reform is consistently associated with more conservative views on abortion. The effect is particularly pronounced among men. The estimates suggest that the reform reduced abortion acceptability by 0.43 standard deviations among men, with smaller and statistically insignificant effects for women. In contrast, the earlier phase of the program (2005–2008), which introduced cash incentives for mothers but did not directly incentivize health workers, shows no significant association with abortion attitudes.

This pattern implies that the messaging and interactions facilitated by ASHAs may have diffused through households and communities, shaping broader social norms around abortion. The results further suggest that when abortion is framed as morally or socially unacceptable, men may internalize these messages more strongly in patriarchal settings. Since men often hold greater influence over fertility decisions in such settings, such shifts in male attitudes could help explain the rise in unplanned births documented in Section 8.

These findings should be interpreted with caution because of limitations in the WVS data. The sample is not restricted to individuals who recently experienced a pregnancy or birth, which may weaken the link between stated attitudes and actual fertility behavior. As a result, the estimates may capture attitudes at a broader level rather than directly reflecting behavioral responses to the program.

Table 10: The Impact of JSY on Abortion Attitudes Among Women and Men

	Abortion Acceptability (z-score)		
	(1) All	(2) Women	(3) Men
HPS $\times$ Post (2005-2008)	0.277 (0.570)	0.006 (0.512)	0.421 (0.595)
HPS $\times$ Post (2009-)	-0.377 (0.236)	-0.325 (0.289)	-0.425* (0.220)
HPS	0.189 (0.205)	0.165 (0.220)	0.213 (0.206)
Control mean (pre)	-0.000	-0.000	0.000
Observations	4161	1901	2260
Fixed effects	✓	✓	✓
Controls	✓	✓	✓

Note: The dependent variable is a standardized index measuring attitudes toward abortion. The data come from the World Values Survey (WVS), waves 4 (2001), 5 (2006), and 6 (2012). The sample includes rural women and men from lower socioeconomic backgrounds, defined based on indicators of lower caste status, low income, and residence in areas with fewer than 10,000 residents. Column (2) restricts the sample to women, and Column (3) to men. All regressions control for age, education, and number of children. Interview year and state fixed effects are included. Standard errors, clustered at the state level, are reported in parentheses. Significance is indicated as follows: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

10 Robustness Checks

10.1 Testing JSY Effects Among APL Women with Similar Characteristics to BPL

A potential concern in linking the main finding that JSY increased unplanned births to the mechanism analysis involving access to community health workers is that the two results are estimated on different samples. The main analysis focuses on women above the poverty line and not from Scheduled Castes or Tribes, while the mechanism analysis uses data on births to women who are either below the poverty line or from SC or ST groups. If these populations differ systematically in how they interact with health workers or respond to JSY incentives, the interpretation that increased access to ASHAs explains the rise in unplanned births may be weakened.

To bridge the sample difference between the main and mechanism analyses, I restrict the main analysis sample to above-poverty-line (APL) women in the bottom two quintiles of the wealth index, representing the poorest segment of the APL population. Although these women are technically above the poverty line, their socioeconomic characteristics more closely resemble those of below-poverty-line (BPL) women included in the mechanism analysis. This subgroup is more likely to have comparable exposure to ASHAs and to face similar constraints in exercising fertility preferences.

Table A.7 presents the results from this subsample analysis. The estimated effect of JSY on unplanned births remains positive and statistically significant among poorer APL women, with magnitudes similar to those in the main specification. The effect is again concentrated among younger women, significant at the 1% level. These results show that JSY increased unplanned births even among women whose socioeconomic status is closer to that of the BPL and SC/ST populations used in the mechanism analysis. While this test does not directly evaluate the mechanism, it supports the use of the BPL/SCST sample to examine how increased access to community health workers may have contributed to the observed rise in unplanned births.

10.2 Placebo Test: Ineligible Births Above Second Parity

To assess the internal validity of the mechanism analysis, I conduct a placebo test using births that were not eligible for JSY benefits. Specifically, I restrict the sample to third and higher-order births among rural women below the poverty line or from Scheduled Castes or Tribes. These higher-parity births were excluded from the main mechanism analysis in Section 8, which focused on JSY-eligible first and second births, in line with the program's parity-based eligibility rule in high-performing states (HPS). Since JSY incentives do not apply beyond the second birth, these women should not have experienced increased engagement with ASHAs following the 2009 reform. Although the sample size is limited, this exercise provides a useful robustness check.

Table A.8 reports the results. The interaction term between HPS status and the post-reform period is statistically insignificant in all columns, with point estimates close to zero. Among younger women, the coefficient is positive but not statistically distinguishable from zero, while the estimates for the full sample and older women are slightly negative and also insignificant.

These null results lend support to the interpretation that the rise in unplanned births observed in the main analysis is specific to JSY-eligible women and is not driven by broader trends affecting all women. The placebo test helps rule out alternative explanations and reinforces the claim that increased health worker engagement contributed to the rise in unplanned births among younger women.

10.3 Testing for Selection Bias in Unplanned Birth Estimates

A potential concern with the main analysis in Table 4 is that it conditions on women who gave birth in the past five years. If the Janani Suraksha Yojana (JSY) influenced fertility behavior, this restriction may introduce selection bias. Specifically, if the program affected who gives birth, comparisons limited to ever-mothers may conflate treatment effects with shifts in the composition of mothers.

As discussed in Section 6.2, this concern is partly mitigated by the earlier finding in Table 2, which shows no statistically significant change in overall fertility among younger women—the group where the increase in unplanned births occurred. This suggests that the rise in unplanned births is unlikely to be driven by an increase in the number of births. Nonetheless, it remains important to assess whether changes in the composition of women who give birth, that is, selection on the intensive margin, could bias the estimates.

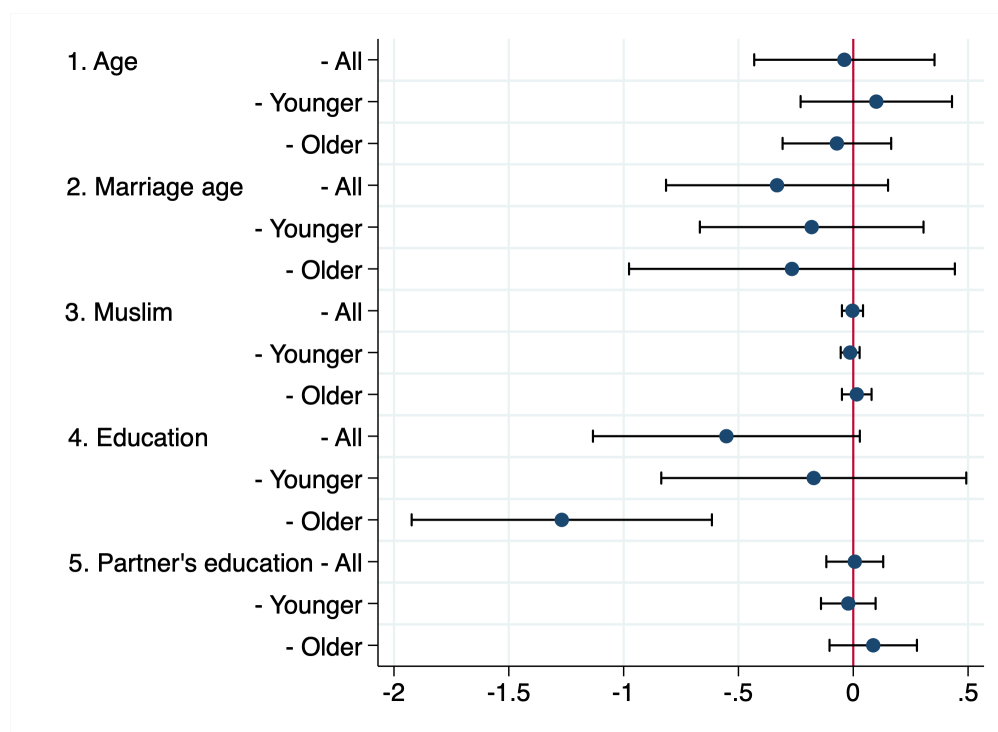
As a first robustness check, I examine changes in observable maternal characteristics. I follow the approach in [Bossavie et al. \(2023\)](#), estimating DiD regressions for five key covariates: age, age at marriage, religion, education, and partner’s education. This strategy tests whether the observable profile of mothers changed across treatment and control groups after JSY implementation.¹³

Figure 6 plots the estimated effects of JSY on key maternal characteristics using a DiD specification. Across most outcomes—age, age at marriage, religion, education, and partner’s education—estimates are small and statistically insignificant. Notably, point estimates are near zero for religion and partner’s education across all age groups. Education shows a slight decline among older women, but this group had largely completed schooling well before JSY was introduced, making it unlikely that the program directly affected their education. By contrast, no such shifts are evident among younger women, for whom education levels remain stable. Overall, there is no evidence of systematic changes in observable characteristics among younger women, suggesting that selection on observables is unlikely to bias the main results.

However, changes in unobserved characteristics may still bias the estimates. To address this concern, I conduct a second robustness check using the full sample of ever-married women. Specifically, I replicate Equation 2 on all ever-married women, regardless of whether they gave birth in the past five years. The dependent variable is an indicator for whether the respondent experienced an unplanned birth during that period, and women who did not give birth are coded as having zero unplanned births. This approach avoids conditioning on childbirth and allows estimation of the average treatment effect

¹³The premise is based on the assumption articulated by [Altonji et al. \(2005\)](#) and [Oster \(2019\)](#), that changes in unobservable characteristics are likely to be correlated with changes in observable ones.

Figure 6: Compositional changes in mothers, post JSY



Note: This figure plots estimates of the effect of JSY exposure on maternal characteristics, by age group. Each coefficient corresponds to the interaction between treatment status and the post-treatment period. All regressions include birth year, birth order, interview year, and state fixed effects as in the main analysis. Standard errors are clustered at the state level.

across the entire population of ever-married women.¹⁴

One complication arises with the use of DHS Wave 3 (2005–06), which was fielded shortly after JSY was introduced. In the birth-only sample, exposure can be assigned based on the timing of the birth. However, for women who did not give birth, treatment status cannot be clearly assigned, because although their date of interview falls after the launch of JSY, most of the five-year recall period still falls in the pre-JSY era. As a result, it is difficult to consistently define pre- and post-treatment periods for these women.

To ensure a clean comparison, I restrict the sample to DHS Wave 2 (1998–99), which

¹⁴As in the main analysis, these regressions include birth order fixed effects. For women with no live births (i.e., never-mothers), birth order is coded as zero so that these cases are consistently incorporated in the fixed effect structure.

predates JSY, and Wave 4 (2015–16), where the entire five-year exposure window falls within the post-JSY period.

Table A.9 reports the results. The estimates confirm the robustness of the main findings in Table 4. For the full sample in Column (1), the estimated effect is small and statistically indistinguishable from zero. In contrast, Column (2) shows that JSY significantly increases the likelihood of unplanned births among younger women. These results underscore that the findings are not an artifact of conditioning on childbirth.

To probe this further, Columns (3) and (4) restrict the sample to younger women with plausibly lower intrahousehold bargaining power—those who married before age 16 and those with more daughters than sons.¹⁵ The estimated effects in these subgroups are larger than those in Column (2), suggesting that JSY had an especially pronounced impact where women’s ability to assert their fertility preferences was most limited. This pattern directly aligns with the mechanism in Section 9.1, which documents that JSY increased discordance in fertility preferences between husbands and wives. When such discordance arises, women with weaker bargaining power are less able to act on their own preferences, making them more likely to continue unintended pregnancies. The stronger effects in these mechanism-relevant subgroups therefore provide additional evidence that the main results are driven by spousal preference divergence rather than by conditioning on childbirth.

11 Conclusion

This paper documents an unintended consequence of expanding maternal health services on unplanned fertility, using evidence from India’s Janani Suraksha Yojana (JSY). While the program reduced fertility among older women, it simultaneously increased unplanned births among younger women in rural areas. Mechanism analysis shows that improved access to community health workers in the program is a key channel. These in-

¹⁵Evidence indicates that early marriage and having fewer sons reduce women’s bargaining power within households (Li and Wu, 2011; Tauseef and Sufian, 2024).

teractions make women more likely to believe that they wish to stop childbearing while their husbands do not. In male-dominated settings, this perceived discordance can limit women's ability to act on their reproductive intentions. They also shift attitudes against abortion, increasing the likelihood that unintended pregnancies are carried to term. These findings show that health system interventions that expand institutional deliveries and antenatal care can unintentionally change reproductive behavior.

These findings have important implications for policy design. Maternal and child health interventions aim to improve maternal survival by promoting institutional delivery and antenatal care. However, their incentive structure can unintentionally increase unplanned births. Shifting from payment-per-delivery incentives for health workers to incentive designs that also support women's reproductive autonomy may help limit these unintended effects while maintaining health gains. In addition, providing services such as maternal mental health care and early childhood support may help mitigate the negative impacts of unplanned births.

Future research should aim to disentangle the separate effects of conditional cash transfers to mothers and improved access to health workers on fertility intentions and unplanned births. It should also examine longer-term consequences of unplanned births, including maternal mental health, child health, and human capital accumulation. This would help clarify the full welfare implications of maternal health interventions in low-income settings.

References

- Altonji, J. G., Elder, T. E., and Taber, C. R. (2005). Selection on observed and unobserved variables: Assessing the effectiveness of catholic schools. *Journal of Political Economy*, 113.
- Ananat, E. O., Gruber, J., Levine, P. B., and Staiger, D. (2009). Abortion and selection. *Review of Economics and Statistics*, 91.
- Antón, J. I., Ferre, Z., and Triunfo, P. (2018). The impact of the legalisation of abortion on birth outcomes in uruguay. *Health Economics (United Kingdom)*, 27.
- Ashraf, N., Field, E., and Lee, J. (2014). Household bargaining and excess fertility: An experimental study in zambia. *American Economic Review*, 104.
- Autor, D. H. (2003). Outsourcing at will: The contribution of unjust dismissal doctrine to the growth of employment outsourcing.
- Bahk, J., Yun, S. C., mi Kim, Y., and Khang, Y. H. (2015). Impact of unintended pregnancy on maternal mental health: A causal analysis using follow up data of the panel study on korean children (pskc). *BMC Pregnancy and Childbirth*, 15.
- Bailey, M. J. (2010). "momma's got the pill": How anthony comstock and griswold v. connecticut shaped us childbearing. *American Economic Review*, 100:98–129.
- Bearak, J., Popinchalk, A., Ganatra, B., Moller, A. B., Özge Tunçalp, Beavin, C., Kwok, L., and Alkema, L. (2020). Unintended pregnancy and abortion by income, region, and the legal status of abortion: estimates from a comprehensive model for 1990–2019. *The Lancet Global Health*, 8.
- Björkman, M. and Svensson, J. (2009). Power to the people: Evidence from a randomized field experiment on community-based monitoring in uganda. *The Quarterly Journal of Economics*, 124(2):735–769.
- Bongaarts, J. (1990). The measurement of wanted fertility. Technical report.

- Borusyak, K., Jaravel, X., and Spiess, J. (2024). Revisiting event-study designs: Robust and efficient estimation. *Review of Economic Studies*, 91.
- Bossavie, L., Cho, Y., and Heath, R. (2023). The effects of international scrutiny on manufacturing workers: Evidence from the rana plaza collapse in bangladesh. *Journal of Development Economics*, 163.
- Calonico, S., Cattaneo, M. D., Farrell, M. H., and Titiunik, R. (2017). Rdrobust: Software for regression-discontinuity designs. *Stata Journal*, 17.
- Calonico, S., Cattaneo, M. D., and Titiunik, R. (2014). Robust nonparametric confidence intervals for regression-discontinuity designs. *Econometrica*, 82.
- Cattaneo, M. D., Titiunik, R., and Vazquez-Bare, G. (2020). *The Regression Discontinuity Design*, pages 835–857. SAGE Publications Ltd.
- Doepke, M. and Tertilt, M. (2018). Women’s empowerment, the gender gap in desired fertility, and fertility outcomes in developing countries. *AEA Papers and Proceedings*, 108.
- Dupas, P. (2011). Health behavior in developing countries. Technical report.
- Foster, D. G., Raifman, S. E., Gipson, J. D., Rocca, C. H., and Biggs, M. A. (2018). Effects of carrying an unwanted pregnancy to term on women’s existing children the journal of pediatrics • www.jpeds.com.
- Gadsden, T., Mabunda, S. A., Palagyi, A., Maharani, A., Sujarwoto, S., Baddeley, M., and Jan, S. (2021). Performance-based incentives and community health workers’ outputs, a systematic review.
- Gebremedhin, A. F., Dawson, A., and Hayen, A. (2022). Evaluations of effective coverage of maternal and child health services: A systematic review.
- Gertler, P. and Vermeersch, C. (2013). Using performance incentives to improve medical care productivity and health outcomes. *NBER Working Paper Series*.

- Glenton, C., Sorhaindo, A. M., Ganatra, B., and Lewin, S. (2017). Implementation considerations when expanding health worker roles to include safe abortion care: A five-country case study synthesis. *BMC Public Health*, 17.
- Griffith, A. and Noonan, T. (2022). The effects of public campaign funding: Evidence from seattle's democracy voucher program. *Journal of Public Economics*, 211.
- Gupta, P., Iyengar, S. D., Ganatra, B., Johnston, H. B., and Iyengar, K. (2017). Can community health workers play a greater role in increasing access to medical abortion services? a qualitative study. *BMC Women's Health*, 17.
- Halli, S. S., Ashwini, D., Dehury, B., Isac, S., Joseph, A., Anand, P., Gothwal, V., Prakash, R., Ramesh, B. M., Blanchard, J., and Boerma, T. (2019). Fertility and family planning in uttar pradesh, india: Major progress and persistent gaps. *Reproductive Health*, 16.
- Javadekar, S., Saxena, K., Dumas, C., Ferrara, E. L., Krishnakumar, J., Lybbert, T., Mueller, T., Mukherjee, D., Pellizzari, M., Ray, D., Tarozzi, A., and Tetenov, A. (2025). The seen and unseen: The unintended impact of a conditional cash transfer program on prenatal sex selection. Technical report.
- Kahn-Lang, A. and Lang, K. (2020). The promise and pitfalls of differences-in-differences: Reflections on 16 and pregnant and other applications. *Journal of Business and Economic Statistics*, 38:613–620.
- Komura, M. (2013). Fertility and endogenous gender bargaining power. *Journal of Population Economics*, 26.
- Kumari, S. and Kishore, J. (2021). Medical termination of pregnancy (amendment bill, 2021): Is it enough for indian women regarding comprehensive abortion care.
- Li, L. and Wu, X. (2011). Gender of children, bargaining power, and intrahousehold resource allocation in china. *Journal of Human Resources*, 46.
- Lim, S. S., Dandona, L., Hoisington, J. A., James, S. L., Hogan, M. C., and Gakidou, E. (2010). India's janani suraksha yojana, a conditional cash transfer programme to increase births in health facilities: an impact evaluation. *The Lancet*, 375:2009–2023.

- Marston, C. and Cleland, J. (2003). Relationships between contraception and abortion: A review of the evidence. *International Family Planning Perspectives*, 29.
- Mcfarlane, I., Zerzan, R., Baker, D., Keogh, S., Luchsinger, G., Roseman, M., Sedgh, G., and Solo, J. (2022). State of the world population: Seeing the unseen. the case for action in the neglected crisis of unintended pregnancy. *Unfpa*.
- Mendelson, A., Kondo, K., Damberg, C., Low, A., Motuapuaka, M., Freeman, M., O’Neil, M., Relevo, R., and Kansagara, D. (2017). The effects of pay-for-performance programs on health, health care use, and processes of care: A systematic review. *Annals of Internal Medicine*, 166.
- Miller, G. (2010). Contraception as development? new evidence from family planning in colombia. *Economic Journal*, 120:709–736.
- Ministry of Finance (2025). Economic survey 2024–25. Accessed September 29, 2025.
- Ministry of Health and Family Welfare, Government of India (2006). *Reading Material for ASHA, Book No. 3: Family Planning, RTI/STIs & HIV/AIDS and ARSH (2005–2012)*. Ministry of Health and Family Welfare, New Delhi. July 2006 edition.
- Mishra, A. and Parasnis, J. (2021). Husband, sons and the fertility gap: evidence from india. *Journal of Population Research*, 38.
- Moughalian, C., Almansa, J., Vogt, T., Biesma, R., Täuber, S., Rao, A., Srivastava, A., and Stekelenburg, J. (2024). The impact of accredited social health activists in india on uptake of modern contraception: A nationally representative multilevel modelling study. *Global Public Health*, 19.
- Nandagiri, R. (2019). “like a mother-daughter relationship”: Community health intermediaries’ knowledge of and attitudes to abortion in karnataka, india. *Social Science and Medicine*, 239.
- Nandi, A. and Laxminarayan, R. (2016). The unintended effects of cash transfers on fertility: evidence from the safe motherhood scheme in india. *Journal of Population Economics*, 29.

- Neelsen, S., de Walque, D., Friedman, J., and Wagstaff, A. (2021). *Financial Incentives to Increase Utilization of Reproductive, Maternal, and Child Health Services in Low- and Middle-Income Countries: A Systematic Review and Meta-Analysis*.
- Nelson, H. D., Darney, B. G., Ahrens, K., Burgess, A., Jungbauer, R. M., Cantor, A., Atchison, C., Eden, K. B., Goueth, R., and Fu, R. (2022). Associations of unintended pregnancy with maternal and infant health outcomes: A systematic review and meta-analysis.
- Oster, E. (2019). Unobservable selection and coefficient stability: Theory and evidence. *Journal of Business and Economic Statistics*, 37.
- Powell-Jackson, T., Mazumdar, S., and Mills, A. (2015). Financial incentives in health: New evidence from india's janani suraksha yojana. *Journal of Health Economics*, 43:154–169.
- Salemi, C. (2021). Refugee camps and deforestation in sub-saharan africa. *Journal of Development Economics*, 152.
- Saxena, S., Maheshwari, S., and Saxena, A. (2018). Evaluation of knowledge of asha regarding family planning services provided under nrhm in bhojipura block, district bareilly. *Religion*, 3.
- Saxena, S., Maheshwari, S., Saxena, A., and Statistician, C. (2021). Evaluation of knowledge of asha regarding family planning services provided under nrhm in bhojipura block, district bareilly. *Journal of Preventive Medicine and Holistic Health*, 3:56–61.
- Shapiro-Mendoza, C. K., Tomashek, K. M., Anderson, R. N., and Wingo, J. (2006). Recent national trends in sudden, unexpected infant deaths: More evidence supporting a change in classification or reporting. *American Journal of Epidemiology*, 163.
- Shet, S., Sumit, K., and Phadnis, S. (2018). A study on assessment of asha's work profile in the context of udupi taluk, karnataka, india. *Clinical Epidemiology and Global Health*, 6.

- Singh, A., Chalasani, S., Koenig, M. A., and Mahapatra, B. (2012). The consequences of unintended births for maternal and child health in india. *Population Studies*, 66:223–239.
- Singh, P. and Masters, W. A. (2017). Impact of caregiver incentives on child health: Evidence from an experiment with anganwadi workers in india. *Journal of Health Economics*, 55.
- Singh, S., Shekhar, C., Acharya, R., Moore, A. M., Stillman, M., Pradhan, M. R., Frost, J. J., Sahoo, H., Alagarajan, M., Hussain, R., Sundaram, A., Vlassoff, M., Kalyanwala, S., and Browne, A. (2018). The incidence of abortion and unintended pregnancy in india, 2015. *The Lancet Global Health*, 6:e111–e120.
- Stillman, M., Frost, J. J., Singh, S., Kalyanwala, S., and Moore, A. (2014). Abortion in india: A literature review. *Guttmacher Institute*.
- Sun, A. and Zhao, Y. (2016). Divorce, abortion, and the child sex ratio: The impact of divorce reform in china. *Journal of Development Economics*, 120:53–69.
- Sunil, B. (2022). Running an obstacle-course: a qualitative study of women’s experiences with abortion-seeking in tamil nadu, india. *Sexual and Reproductive Health Matters*, 29.
- Tauseef, S. and Sufian, F. D. (2024). The causal effect of early marriage on women’s bargaining power: Evidence from bangladesh. *World Bank Economic Review*, 38.
- Thomson, E. (1997). Couple childbearing desires, intentions, and births. *Demography*, 34.
- Timæus, I. M. and Moultrie, T. A. (2020). Pathways to low fertility: 50 years of limitation, curtailment, and postponement of childbearing. *Demography*, 57.
- Valente, C. (2014). Access to abortion, investments in neonatal health, and sex-selection: Evidence from nepal. *Journal of Development Economics*, 107.
- Wado, Y. D., Afework, M. F., and Hindin, M. J. (2013). Unintended pregnancies and the use of maternal health services in southwestern ethiopia. *BMC International Health and Human Rights*, 13.

Westoff, C. F. and Koffman, D. (2010). Birth spacing and limiting connections. Technical report.

World Bank (2023). World development indicators: Gdp per capita (current us\$) for india. <https://data.worldbank.org/indicator/NY.GDP.PCAP.CD?locations=IN>. Accessed: 2024-07-09.

Appendix A Tables

Table A.1: Summary Statistics

	Treatment			Control			
Variable	N	Mean	SD	N	Mean	SD	Diff
A. Fertility analysis (H1)							
Age (years)	18426	29.4	5.83	20453	30.1	5.76	−0.702***
Number of births in past 5 years	18426	.963	.876	20453	.727	.812	0.236***
Number of children	18426	2.84	1.53	20453	2.38	1.2	0.464***
Number of children above age 5	18426	1.88	1.69	20453	1.63	1.43	0.253***
Birth order of the last child	18426	3.21	1.78	20453	2.57	1.34	0.638***
Household head - Hindu	18426	.82	.384	20453	.779	.415	0.041
Household head - Muslim	18426	.165	.371	20453	.111	.315	0.053
Years of schooling (imp)	18426	3.13	4.2	20453	5.21	4.54	−2.075***
Any contraceptive use	18426	.451	.498	20453	.645	.479	−0.194***
Modern contraceptive use	18426	.375	.484	20453	.567	.495	−0.192***
B. Unplanned births analysis (H2)							
Age (years)	10137	26.7	4.93	8890	26.5	4.6	0.287
Number of children	10137	2.7	1.59	8890	2.17	1.23	0.525***
Household head - Hindu	10137	.793	.405	8890	.753	.431	0.040
Household head - Muslim	10137	.195	.396	8890	.132	.338	0.063
Years of schooling (imp)	10137	3.2	4.24	8890	5.85	4.66	−2.656***
Unplanned birth	10137	.357	.479	8890	.278	.448	0.079***
Birth order of the last child	10137	3.04	1.86	8890	2.34	1.37	0.708***
C. Mechanism analysis (H3)							
Age (years)	2102	28.1	4.41	2870	29.2	4.83	−1.164***
No. children	2102	2.64	1.1	2870	3.27	1.48	−0.636***
Below poverty line	2102	.525	.499	2870	.675	.469	−0.149**
Scheduled caste/tribe	2102	.74	.439	2870	.651	.477	0.089
Woman’s years of education	2102	5.09	4.29	2870	2.6	3.69	2.498***
Unplanned Birth	2102	.223	.416	2870	.329	.47	−0.105***

Note: Panel A includes the sample for the fertility analysis (H1), and Panel B includes the sample for the unplanned births analysis (H2). In both panels, the treatment group consists of rural women in low-performing states (LPS) who are above the poverty line and do not belong to Scheduled Castes or Tribes (SC/ST). Panel B further restricts the sample to women who had a birth within the past five years. Panel C presents the sample for the mechanism analysis (H3), focusing on the health worker channel. This sample includes births from rural women who are either below the poverty line or belong to SC/ST groups. The treatment group in Panel C consists of births from rural women residing in high-performing states (HPS).

Table A.2: Summary Statistics for Regression Discontinuity Sample from DLHS-4

Variable	N	Mean	SD
Total live births	21315	1.74	.897
Mother's age in completed years	21614	24.3	2.82
Years of Education	21307	6.47	4.6
Has fridge	21614	.176	.38
Own house	21614	.939	.238
ASHA encouraged facility delivery	21614	.225	.417
Any ASHA engagement	21614	.354	.478

The sample is from DLHS-4 (2012–13) and includes JSY-eligible women aged 19–40 residing in high-performing states (HPS), with no more than two previous live births. The sample includes births conceived before and after the 2009 reform, and is used in the health worker channel analysis (H3)

Table A.3: Summary of Analyses, Data, and Empirical Strategies

Analysis	Main Outcome(s)	Data Source	Empirical Strategy
H1 Fertility and Contraceptive Use	Number of Births; Contraceptive use	NFHS Rounds 2–4	Difference-in-Differences (LPS vs. HPS)
H2 Unplanned Births	Indicator for unplanned birth	NFHS Rounds 2–4	Difference-in-Differences (LPS vs. HPS)
H3 Health Worker Channel	Unplanned births; ASHA contact	IHDS (2011–12); DLHS 4	DiD exploiting 2009 reform; Regression Discontinuity
Perceived Discordance Mechanism	Perceived fertility preferences	IHDS Panel (2005–06, 2011–12)	DiD exploiting 2009 reform
Abortion Attitude Mechanism	Standardized (z-score) abortion attitude	WVS Waves 4–6 (2001–2012)	Triple Differences

Table A.4: Pre-Treatment Trends in Fertility

	Number of births over the past 5 years		
	(1) All women	(2) Younger (19-29)	(3) Older (30-40)
Year of Interview	-0.005* (0.003)	-0.018*** (0.004)	0.008*** (0.003)
Year of Interview $\times$ Treat	0.009* (0.005)	0.011* (0.006)	0.007* (0.004)
Control mean (pre)	0.666	0.975	0.351
Observations	43997	23299	20697
Fixed effects	✓	✓	✓
Controls	✓	✓	✓

Note: The sample includes higher-caste women above the poverty line in rural areas from NFHS waves 2 (1998-99), waves 3 (2005-06) and 4 (2015-16). Regressions control for the woman's age, the household head's age and an indicator for whether the head's age is missing, the woman's years of education, her partner's education level, the number of children over age five, indicators for Muslim and Hindu religion, and whether the household is female-headed. All models include state fixed effects. Standard errors are clustered at the state level and are presented in parentheses. Significance is indicated as follows: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.5: Pre-Treatment Trends in Fertility (Excluding Uttar Pradesh)

	Number of births over the past 5 years		
	(1) All women	(2) Younger (19-29)	(3) Older (30-40)
Year of Interview	-0.005** (0.003)	-0.018*** (0.004)	0.007*** (0.003)
Year of Interview $\times$ Treat	0.005 (0.005)	0.008 (0.008)	0.004 (0.004)
Control mean (pre)	0.666	0.975	0.351
Observations	38156	19962	18193
Fixed effects	✓	✓	✓
Controls	✓	✓	✓

Note: The sample includes higher-caste women above the poverty line in rural areas from NFHS waves 2 (1998-99), waves 3 (2005-06) and 4 (2015-16), excluding Uttar Pradesh. Regressions control for the woman's age, the household head's age and an indicator for whether the head's age is missing, the woman's years of education, her partner's education level, the number of children over age five, indicators for Muslim and Hindu religion, and whether the household is female-headed. All models include state fixed effects. Standard errors are clustered at the state level and are presented in parentheses. Significance is indicated as follows: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.6: F-test Results of Pre-Trends in Event Study Specifications

	(1) F-value	(2) P-value
Unplanned birth (All)	1.3697	.2483
Unplanned birth (Younger)	.8905	.5462
Unplanned birth (Older)	4.591	.0009

Note: The sample includes higher-caste women above the poverty line in rural areas from NFHS waves 2 (1998-99), waves 3 (2005-06) and 4 (2015-16). Regressions control for the woman's age, the household head's age and an indicator for whether the head's age is missing, the woman's years of education, her partner's education level, indicators for Muslim and Hindu religion, and whether the household is female-headed. All models include state fixed effects for birth order, interview year, and state. Standard errors are clustered at the state level and reported in parentheses.

Table A.7: Impact of JSY on Unplanned Births Among Poorer APL Women

	Unplanned births					
	(1) All	(2) Younger	(3) Older	(4) All	(5) Younger	(6) Older
Treat $\times$ Post	0.040* (0.020)	0.049*** (0.018)	0.022 (0.046)			
Treat $\times$ Post (2005-2008)				0.026 (0.023)	0.054** (0.022)	-0.067 (0.061)
Treat $\times$ Post (2009-2016)				0.041* (0.022)	0.049** (0.019)	0.028 (0.047)
Control mean (pre)	0.278	0.205	0.507	0.278	0.205	0.507
Observations	25295	17923	7370	25295	17923	7370
Fixed effects	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓

Note: The dependent variable is an indicator for whether a woman's most recent birth in the past five years was unplanned. The data come from the NFHS of India, waves 2–4. The sample is restricted to the poorest subgroup within the main sample—rural women from higher castes and above the poverty line. Regressions control for the woman's age, the household head's age and an indicator for whether the head's age is missing, the woman's years of education, her partner's education level, indicators for Muslim and Hindu religion, and whether the household is female-headed. All models include fixed effects for birth year, birth order, interview year, and state. Standard errors are clustered at the state level and reported in parentheses. Significance is indicated as follows: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.8: The Impact of Health Worker Access on Unplanned Births Beyond Second Parity

	Unplanned birth		
	(1) All	(2) Younger	(3) Older
HPS $\times$ Post	-0.012 (0.108)	0.114 (0.137)	-0.031 (0.190)
Control mean (pre)	0.728	0.651	0.796
Observations	177	103	70
Fixed effects	✓	✓	✓

Note: The dependent variable is an indicator for whether the birth was unplanned. The data come from IHDS-II (2011–12). The sample is restricted to births to rural women from Scheduled Castes or Tribes or those below the poverty line. All regressions include fixed effects for birth year, birth order, state, and mother. Standard errors, clustered at the state level, are reported in parentheses. Significance is indicated as follows: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

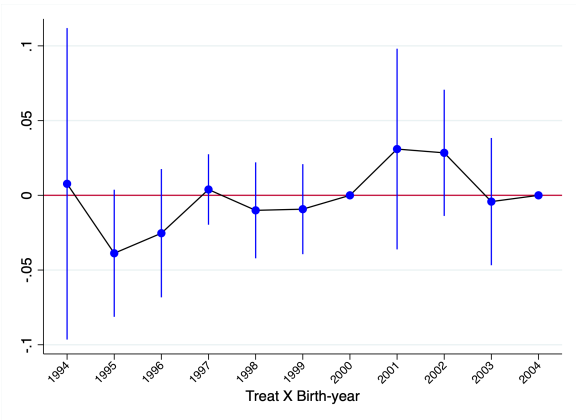
Table A.9: The Impact of JSY on Unplanned Births: Results from Full Sample

	Unplanned birth			
	(1)	(2)	(3)	(4)
	All	Younger	Younger & Early marriage	Younger & More daughters
Treat $\times$ Post	-0.008 (0.009)	0.031*** (0.011)	0.044** (0.021)	0.057*** (0.018)
Control mean (pre)	0.139	0.149	0.243	0.252
Observations	122002	61150	11093	17465
Fixed effects	✓	✓	✓	✓
Controls	✓	✓	✓	✓

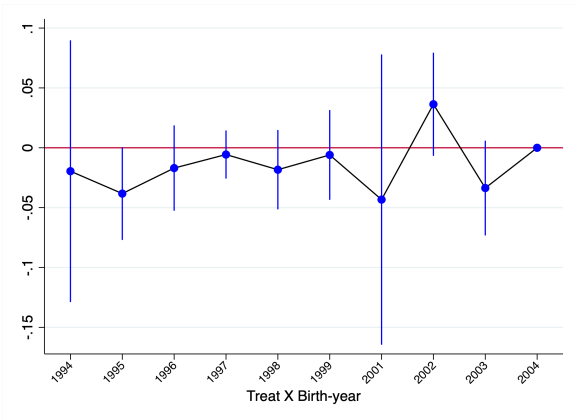
Note: The dependent variable is an indicator for whether a woman's most recent birth in the past five years was unplanned. The sample includes all ever-married women from NFHS Waves 2 (1998–99) and 4 (2015–16), including both ever-mothers and never-mothers. Regressions control for the woman's age, the household head's age and an indicator for whether the head's age is missing, the woman's years of education, her partner's education level, the number of children, indicators for Muslim and Hindu religion, and whether the household is female-headed. All models include fixed effects for interview year, state, and birth order (with never-mothers and childless women coded as birth order 0). Column (1) reports estimates for the full sample; column (2) restricts to younger women; column (3) further restricts to younger women who married before age 16; and column (4) restricts to younger women whose daughters outnumber sons. Standard errors are clustered at the state level and reported in parentheses. Significance is indicated as follows: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Appendix B Figures

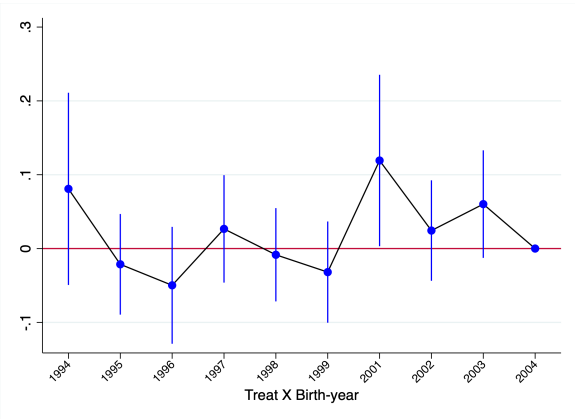
Figure B.1: Event-study for pre-treatment trends



Panel A: PTA - All women



Panel B: PTA - Younger women



Panel C: PTA - Older women