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September 14, 2026

Professors Emily Breza, Arun Chandrasekhar, and Davide Viviano
National Bureau of Economic Research
1050 Massachusetts Avenue
Cambridge, MA 02138

Re: Application to the Workshop on Development Economics and Networks for Early-Career Researchers

Dear Professors Breza, Chandrasekhar, and Viviano,

I am writing to apply to the Workshop on Development Economics and Networks for Early-Career Researchers, to be held in Cambridge on December 5, 2026. I am a fourth-year PhD student in economics at the University of Chicago. My research is in development and behavioral economics, with a focus on gender and health in India.

This application contains my curriculum vitae, including all completed and current PhD coursework, a one-page motivation statement, and my working paper "Female Sterilization and India's Fertility Decline," which I would be glad to present at the workshop.

My faculty recommender, should I be shortlisted, is my advisor Professor Anne Karing (University of Chicago, akaring@uchicago.edu).

Thank you for your consideration.

Sincerely,

Ragini Jain

Ragini Jain

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EDUCATION

University of Chicago **Sep 2023 - Present**
PhD in Economics, Expected Graduation: 2029 *Chicago, IL*

- Fields: Development, Behavioral, Labor Economics
- M.A. in Economics conferred December 2024

University of British Columbia **Sep 2016 - May 2020**
Bachelor of International Economics (BIE) *Vancouver, BC*

- Graduated Class I with Honors

PHD COURSEWORK (UNIVERSITY OF CHICAGO)

2023 - 24 Price Theory I, II, III; Empirical Analysis I, II, III; Theory of Income I, II, III

2024 - 25 Development Economics; Behavioral Economics: Development and Observational Data; Advanced Topics in Political Economy: Comparing Societies; Topics in Labor Markets: Earnings and Employment; A Course in Experimental Economics; Political Economy of Development; Behavioral Development Economics; Behavioral Economics: Theory and the Lab;

2025 - 26 Applied Microeconometrics; Research Seminar (year-long); dissertation research

WORKING PAPER

Female Sterilization and India's Fertility Decline
Quantifies the role of female sterilization in India's fertility decline using five waves of the National Family Health Survey (1992–2019) and uses sterilization-conditional girl-child cash transfer schemes to estimate the causal effect of sterilization incentives on completed fertility and on women's work, health, autonomy, and children's outcomes.

RESEARCH IN PROGRESS

Female Seclusion and Women's Mobility *(with Kalyani Chaudhuri and Prerna Kundu)*
Conducts a randomized intervention to address female seclusion norms, evaluated with a three-round panel survey of women in Haryana; pilot completed.

Fairness in Gig Work *(with Varun Kapoor, Chinmaya Kumar, and Shivani Pandey)*

Regulation in Reproductive Markets *(with Meera Mody)*

RESEARCH EXPERIENCE

Princeton University **Sep 2020 - May 2023**
Pre-Doctoral Research Assistant, Economics Department *Princeton, NJ*

- For Professors Henrik Kleven and Ilyana Kuziemko; conducted empirical research in labor and public economics (child penalties, the EITC, defense spending and income inequality) using event studies, synthetic controls, instrumental variables, and labor supply simulations on large administrative and survey datasets

University of British Columbia **Jan 2018 - Aug 2020**
Research Assistant, Economics Department *Vancouver, BC*

- For Professors Siwan Anderson, Jamie McCasland, Viktoria Hnatkovska, and Cesi Cruz (full-time and part-time appointments)

TEACHING EXPERIENCE

Spring 2026 Consumers, Firms, and the Environment (Undergraduate), UChicago (Matthew Brown)
Winter 2026 Behavioral Economics and Welfare Analysis (Undergraduate), UChicago (Matthew Brown)
Fall 2025 Causal Inference (Master's), UChicago (Melissa Tartari)
Winter 2025 Empirical Analysis II (PhD), UChicago (Stephane Bonhomme)
Winter 2019 Understanding Globalization (Undergraduate), UBC (Angela Redish)

GRANTS AND AWARDS

2026 - 27 Lloyd and Susanne Rudolph Field Research Grant, CISSR, UChicago
2025 - 27 Development Economics Research Fund (DERF) Grant, Becker Friedman Institute
2024 Martin and Margaret Lee Prize in Empirical Analysis, UChicago
(highest score on the PhD Empirical Analysis qualifying exam)
2020 BIE Award for Best Research Paper in Applied Development Economics, UBC

OTHER ACTIVITIES

President and Co-Founder, UBC Women in Economics and Policy (WEP) **2019 - 2020**

ADDITIONAL INFORMATION

Technical Skills Stata, R, LaTeX
Data & Methods Analysis of large administrative and survey microdata; RCT and survey design
Languages English (Native), Spanish (Advanced), Hindi (Intermediate)

Motivation Statement

Ragini Jain, University of Chicago

Workshop on Development Economics and Networks for Early-Career Researchers

I am a fourth-year PhD student in economics at the University of Chicago working on research in development, labor, and behavioral economics, with a particular focus on gender and health in India. The working paper included with this application studies the role of female sterilization in India's fertility decline using five waves of the National Family Health Survey (1992–2019). I first show that the decline came from falling completed fertility within every social group, with rising sterilization as the dominant proximate channel. I then use quasi-experimental variation in cash transfer schemes that condition payment on a parent's sterilization to estimate the causal effect of sterilization incentives on completed fertility and on women's work, health, and autonomy.

My current experimental work runs directly into the questions this workshop covers. With coauthors, I am running a randomized intervention in Haryana that addresses social norms around women's mobility. We have completed the pilot and are now designing the main rollout. Norms that restrict the time women spend outside the home are sustained by a woman's reference network in this context. Whose behavior she observes, who sanctions her, and who carries information about what other households are doing all determine whether an intervention can shift her mobility, and whether any shift spreads beyond treated households. The design decisions in front of us are network questions, such as how to measure the relevant reference groups with survey instruments we can afford, how to randomize when effects are likely to spill over within villages, and how to conduct inference in that setting. Getting frontier guidance on these choices now, before the main wave is locked, is the most concrete reason I want to attend.

A second experimental project raises the same issues in an urban labor market. With coauthors, I am piloting a study of fairness in gig work in Bangalore, centered on platforms banning worker accounts (temporarily or permanently). We document that workers rarely observe the platform's rules directly, so what a worker believes about why accounts get banned comes largely from other workers. Beliefs about the algorithm form and travel through networks of gig workers, and those perceptions in turn shape how workers perform on the platform. Studying this well requires measuring information diffusion in a worker network and separating what the platform does from what workers believe it does, and I want to learn from researchers who have confronted exactly these measurement and identification problems.

The sterilization paper raises diffusion questions of its own that I want to develop. Family planning in India spread through village-level camps and community health workers, and one woman's adoption of an irreversible method plausibly changes the information and the norms facing her neighbors and kin. Whether sterilization diffused through social ties, and whether incentive payments amplified that diffusion, is a natural next step for the project.

More broadly, I am at the stage where my dissertation is taking shape and the methods I invest in now will define my research program. The chance to present my work to, and learn from, the researchers who built the empirical networks literature, alongside a cohort of early-career researchers facing the same design and inference problems, would be valuable to me well beyond a single project. I would be glad to present my working paper, and I would be equally glad to attend and discuss.

Female Sterilization and India's Fertility Decline

Ragini Jain

September 15, 2026

Abstract

This paper studies the role of female sterilization in India's fertility decline and its consequences for women's labor force participation, health, autonomy, and their children's outcomes using five waves of the National Family Health Survey (1992–2019). I first show that the decline came from falling completed fertility within every social group, with rising sterilization as the dominant proximate channel. Then I turn to state girl-child cash transfer schemes, which reward families for having daughters and, in several states, pay only on proof of a parent's sterilization. Eligibility for a sterilization-conditional transfer lowers completed fertility by about 0.2 children relative to cohorts the schemes did not reach, and by about 0.4 relative to eligibility for an unconditional transfer. The channel is an 8 percentage-point rise, relative to the unconditional arm, in the probability the woman is ever sterilized, and there is no evidence that couples sex-select to become eligible. A mechanical accounting exercise caps sterilization's contribution to the fertility decline at two-thirds, with the remainder coming from falling desired family size. The condition also raises women's work and nutritional health, leaving autonomy, attitudes, and children's outcomes unchanged.

1 Introduction

Governments spent much of the twentieth century trying to lower fertility. Beginning with India in 1952, dozens of developing countries adopted population programs, set fertility targets, and built bureaucracies to meet them (de Silva and Tenreyro, 2017). The decline they sought has arrived, with the world's total fertility rate falling from about five children per woman in the 1960s to 2.2 in 2024 (World Bank, 2024). Existing literature documents that fertility fell where and when desired family size fell (Pritchett, 1994; Vogl, 2026; Fang and Liu, 2026) and access-and-outreach programs helped couples implement those desires (Miller, 2010; Barham et al., 2021). The programs this evidence covers offered improved

access to contraceptive services; however, the era's most aggressive programs went further and attached money to particular methods, paying adopters and providers against numerical targets (de Silva and Tenreyro, 2017). Most countries have since prohibited such payment, with India one of the remaining few (Pandey, 2014). Whether the payments lowered fertility beyond what women already wanted, and what the paid-for method did to the women who adopted it, has no causal answer.

The Indian state pays women who sterilize, pays the providers who perform the procedure, sets worker-level expectations, and delivers the method through mass camps. Female sterilization, or tubectomy, is a surgical procedure that blocks the fallopian tubes. It ends childbearing outright and is irreversible in practice. In the survey data this paper uses, a third of reproductive-age Indian women are sterilized, and sterilization is the method behind 57 percent of contraceptive use. India's decline also took a route that matches the promoted method. The total fertility rate (TFR) fell from about six children per woman in the 1960s to below the replacement level of 2.1[1] by 2019. But where women elsewhere reached low fertility by starting later, marrying and bearing first children at older ages alongside rising schooling and work, Indian women continue to start early and instead stop childbearing about five years earlier (Figure 2). Demographers link this unusual reproductive life cycle to the contraceptive regime (Park et al., 2023), but no causal relationship has been established. Did the promoted method drive the decline, or did it ratify stopping decisions women would have carried out anyway? The demand-side consensus implies the second answer, but it is based on evidence from settings without India's sterilization push.

The second open question is what a decline achieved through earlier stopping rather than later childbearing delivers for women. Economists have documented substantial benefits of contraception for women, but they are all focused on reversible methods and the benefits are directly linked to starting later. The pill and its successors, reaching college-aged American women, raised schooling, careers, and, through them, earnings (Goldin and Katz, 2002; Bailey, 2006). Motherhood is associated with career penalties (Kleven, Landais and Søgaard, 2019; Kleven et al., 2019; Cortés and Pan, 2023) and these penalties are higher for women earlier in life (Gallen et al., 2023). A decline driven by a terminal method, rather than by temporary methods that delay early childbearing, may not confer the same benefits. Sterilization frees a woman's time at ages when she can still work (in my data the median Indian woman sterilizes at 25.7, younger than the average American woman is at her *first* birth (Osterman et al., 2024)), but after she may already have left school and early-career opportunities for marriage and children. It also may change her position in the household. In much of India a married woman's position depends on her childbearing, and on sons

[1]This is the rate required to sustain current population levels.

in particular, who carry the family line and support their parents in old age (Dyson and Moore, 1983; Jayachandran, 2017). Couples bargain over fertility, and spouses in developing countries often disagree about having another child (Manser and Brown, 1980; Doepke and Kindermann, 2019; Doepke and Tertilt, 2018). A woman who can still bear a son, particularly in settings where child mortality remains a concern, holds a different position in that bargain than one who cannot. Sterilization can therefore strengthen a woman's position, by providing certainty that she will not return to child-rearing and by sparing her health. It can also weaken her position, by removing the leverage that the ability to bear future children provides.

In this paper, I first provide descriptive evidence on the pattern of India's fertility decline, using five rounds of the National Family Health Survey (1992–2019). Demographers have documented the stopping pattern and ranked its proximate channels (Park et al., 2023; Singh et al., 2022). What the record lacks is an accounting that isolates sterilization within contraception and follows actual cohorts of women to completed fertility. The record also cannot say who drove the decline, and the question matters because population programs have historically concentrated on marginalized groups (León-Ciliotta, Zejcirovic and Fernandez, 2025; Visaria, Acharya and Raj, 2006; Patel, 2017). Using Oaxaca–Blinder decompositions (Oaxaca, 1973; Blinder, 1973), I find that completed fertility fell within every social group. The decline did not come from particular groups (lower caste, lower income, less-educated women), nor from the population shifting toward low-fertility groups. I then decompose the decline into its proximate determinants (Bongaarts, 1978). The driver is rising female sterilization prevalence rather than declining marriage, rising abortion, rising infecundity, or other forms of contraception.

I then turn to causation, exploiting quasi-experimental variation from state cash transfer schemes that counter son preference by raising the financial value of a daughter, through transfers paid across her childhood. The programs began in 1992 in Tamil Nadu and Rajasthan, and by 2019 thirteen states had run one, with the sterilization-conditional schemes concentrated in the south. Some states conditioned the transfer on a parent's sterilization certificate or a particular number of children, while others did not. I implement a triple-difference design that compares outcomes for women eligible for a sterilization-conditional transfer with outcomes for women eligible for an unconditional one. The design exploits the fact that eligibility is determined by the sex of a woman's first one or two births, which is fixed before any scheme could act on her choices, and thus provides a quasi-exogenous source of variation in sterilization. I find that eligibility for a sterilization-conditional transfer lowers a woman's completed fertility by about 0.2 children relative to cohorts the schemes did not reach, and by about 0.4 relative to eligibility for an unconditional girl-child transfer. The

channel is an 8 percentage-point rise, relative to the unconditional arm, in the probability she is ever sterilized. One limitation of this design is that the first-stage response is concentrated in the southern states: in the north, where son preference is stronger, a first daughter leads women to keep childbearing rather than sterilize, and the design finds no response.

Sterilization, by guaranteeing that a woman bears no more children,[2] may change her life along several dimensions. Ending childbearing frees her time at ages when she can still work and spares her the nutritional cost of further pregnancies. It may also move her standing in the household, in either direction, through the bargaining channel above. And for her children, the quantity-quality logic predicts gains: with fewer children, each could receive more (Rosenzweig and Zhang, 2009). I find that the probability a woman works for pay rises about four percentage points relative to the unconditional arm, an eighth of the 0.37 base. The detectable gains are in manual and agricultural work rather than white-collar jobs, consistent with sterilization freeing time in mid-life without creating the human capital that starting later builds. Anemia falls and hemoglobin rises, consistent with the nutritional channel above. De La Rupelle and Dumas (2025), instrumenting sterilization with disease ecology in an earlier period, find gynecological harms and no nutritional change. Their design identifies a different complier population, so the two sets of estimates answer related but distinct questions. Women's say in household decisions and their attitudes toward wife-beating do not move, and those estimates are precise: an effect as large as the labor-supply gain would have been detected. Children's health and schooling do not move either, so the quantity-quality gains the smaller families might have delivered do not appear.

Finally, I ask how much of the aggregate decline the rise of sterilization can explain. The paper offers two answers of different kinds, and neither is a causal share. The first is an accounting ceiling. Freezing sterilization at its 1992 level inside the proximate-determinants model, while crediting every sterilization with preventing every birth that could have followed it, removes at most 67 percent of the decline; sterilization cannot account for more than that even on the most generous reckoning. The second is the causal estimate from the transfer schemes, and it answers a narrower question: what the policies did to the women they reached. Eligibility lowered their completed fertility by about 0.2 children. That number does not say what sterilization did to India as a whole, because the women a scheme moved need not resemble the tens of millions who sterilized without one, so I do not scale it up to a national share. Together the two answers support a qualitative conclusion. A sustained supply-side push delivered a large part of a national fertility decline. The rest came from families wanting fewer children, visible in the falling childbearing of women who

[2]Fewer than one in a hundred sterilized women ever conceive again (Peterson et al., 1996; Trussell, 2011), and reversal is rarely feasible in practice (van Seeters et al., 2017).

never sterilized (Pritchett, 1994; Vogl, 2026).

The paper contributes to three literatures. The first asks where fertility declines come from and what population programs do. This literature attributes fertility declines to desired family size declines rather than to contraceptive access improvements (Pritchett, 1994; Vogl, 2026). The program evaluations point the same way, finding fertility effects with few lasting gains for women or children (Barham et al., 2021; Rosenzweig and Zhang, 2009) and lasting institutional damage where programs coerced (León-Ciliotta, Zejcirovic and Fernandez, 2025). For India, demographers have mapped the decline's geography and correlates descriptively (Dyson and Moore, 1983; Drèze and Murthi, 2001; Singh et al., 2022; Bansal, Dwivedi and Ali, 2022), but cannot say whether sterilization caused the fertility decline it accompanied. I contribute on both fronts. The decompositions deliver new facts about the transition itself: fertility fell inside every social group, and changes in population composition explain almost none of the decline. Women without schooling cut their fertility by nearly the size of the aggregate fall, and the sterilized and never-sterilized converged to the same completed family size, so the method spread without sorting women into a low-fertility group. The causal design then does what no decomposition can. It estimates what attaching the sterilization condition did to eligible women's fertility, and, under the exclusion restriction stated in Section 6.5, what the promoted method did to the women it moved.

The second is the economics of contraception, which is almost entirely about reversible methods and the starting margin. The pill let American women delay first births, invest in careers, and work more (Goldin and Katz, 2002; Bailey, 2006; Bailey and Lindo, 2018), and family-planning access in developing settings raises schooling and employment by lowering the cost of postponing early births (Miller, 2010; Herrera, Sahn and Villa, 2019; Jensen, 2012). Although sterilization is the most prevalent form of contraception in the world currently, the closest antecedent is Fung (2024), who shows that female sterilization in the United States lowered the age at last birth and raised labor supply. There, sterilization is a late private choice, taken after years of reversible use, while in India the method is dominant, adopted early, and rarely preceded by temporary methods, suggesting that the labor-supply effects in Fung (2024) may not present here. India-specific work so far studies access constraints on reversible methods (Anukriti et al., 2022; Anukriti, Herrera-Almanza and Karra, 2024), with De La Rupelle and Dumas (2025) on the health effects of sterilization the only causal antecedent. This paper provides the first causal estimates of what the world's most used contraceptive method does to fertility and to the women who adopt it.

The third is the evaluation of girl-child transfers and fertility incentives. Existing work evaluates single schemes against their stated goals, finding that daughter-rewarding incentives can lower fertility while worsening the sex ratio (Anukriti, 2018; Anukriti and

Chakravarty, 2017; Jain, 2023; Biswas, Cornwell and Zimmermann, 2025), and that a transfer's fertility response follows what it conditions on (Stecklov et al., 2007; Nandi and Laxminarayan, 2016). I add three things to this literature. I pool the full set of schemes, nineteen programs across thirteen states over nearly three decades, rather than evaluating one. I split them by the margin they condition on, comparing the schemes that require a sterilization certificate with those that pay for daughters unconditionally, so the comparison speaks to what attaching the condition buys a policymaker. And I follow the eligible women to completed fertility, connecting scheme-level evaluation to the population-scale decline the first part of the paper documents. Anukriti (2018), whose analysis of Devi Rupak in Haryana documents the take-up response this design builds on, is the closest antecedent.

The paper proceeds as follows. Section 2 describes the sterilization program and the cash-transfer schemes, Section 3 the data. Section 4 documents the decline and the rise of sterilization, Section 5 decomposes it, Section 6 sets out the design and the causal results, and Section 8 translates them into a share of the aggregate fertility decline. The last section concludes.

2 Background and Institutional Setting

India was the first country to adopt an official family-planning program, in 1952, and its program remains among the world's largest, with sterilization at its center throughout (Srinivasan, 2017). The program was defined by a coercive mass-vasectomy campaign of the 1975–77 Emergency, which peaked at about six million male sterilizations in a single year (Appendix Figure A4). This period triggered widespread backlash, with male sterilization falling to zero in the following years and female sterilization rising to become the near-universal terminal method (Singh and Vincent, 2024; Pelras and Renk, 2023). This section sets out what the procedure is, how the program delivers and promotes it, and the state cash-transfer schemes the design exploits.

The majority of India's post-Emergency family-planning budget is devoted to female sterilizations, also known as tubectomies (De La Rupelle and Dumas, 2025; Pelras and Renk, 2023). A tubectomy blocks or cuts the fallopian tubes so that eggs cannot reach the uterus. It is minor surgery, typically done in under an hour with the woman home the same day, and it prevents conception without affecting the menstrual cycle.[3] Fewer than one in a hundred sterilized women ever conceive again (Peterson et al., 1996; Trussell, 2011), so the method

[3]Surgeons in India typically perform it by minilaparotomy, through a small abdominal incision, or by laparoscopy, usually under local anesthesia with sedation and followed by a few hours of observation. Unlike hysterectomy, it removes no organ.

operates at essentially full use-effectiveness. Reversal requires microsurgical reconstruction of the tubes, costly, scarce in practice, and far from certain to restore fertility (van Seeters et al., 2017), so the decision is permanent in practice. Vasectomy, the male equivalent, is simpler, safer, and cheaper, but as described above, India's program largely abandoned it after the Emergency (Singh and Vincent, 2024).[4]

The public health system delivers most sterilizations, at a scale of several million tubectomies a year (Appendix Figure A4). A woman can undergo the procedure free of charge at a public district hospital, community health center, or primary health center, or at an accredited private facility on the same terms, with the same government compensation. The public health system uses the "camp" method for various services, including sterilizations. Camps are designated days on which a visiting team provides specific health services at a primary or community health center or at a temporary site. Camps have historically performed tubectomies in bulk, often dozens and in documented cases more than eighty in a day. They are how the program reaches rural women where staff surgeons are scarce (Pelras and Renk, 2023; De La Rupelle and Dumas, 2025).[5] The program is centrally financed and state-delivered. The central health ministry sets the compensation schedule and pays for it through the National Health Mission, while state health departments run the camps and employ the front-line workers. For decades the center also handed each state annual method-specific sterilization targets. The targets were formally abolished in 1996 in favor of a "target-free approach," but states have continued to set worker-level expectations that function as targets in practice (Srinivasan, 2017).

The program pays both sides of the market. The woman receives a compensation payment for undergoing sterilization, and the front-line health worker who "motivates" her, an ASHA (Accredited Social Health Activist), receives a per-case fee.[6] The compensation schedule

[4]Abandoned in practice, not in law. Vasectomy remains on the public method menu, and its compensation payments are set higher than tubectomy's to counter its unpopularity. Few men take it up, and provider availability is far more limited, with many facilities offering no vasectomy services at all.

[5]Camps have also been the program's flashpoint. After thirteen women died following a mass tubectomy camp in Bilaspur in 2014 (Sharma, 2014), the Supreme Court ordered camps phased out within three years (*Devika Biswas v. Union of India*, 2016). The same structure has continued in the form of "Fixed Day Services", designated sterilization days at approved facilities, typically held weekly or monthly. The episode belongs to a longer controversy over informed consent, in which compensation payments, provider targets, and camp-scale throughput raise the concern that some women consent without full information. In the recent survey rounds, 16 percent of sterilized women report not having been told the method is permanent, and Karra and Canning (2024) show that counting such cases multiplies India's measured rate of unwanted family planning several-fold.

[6]Under the national compensation schedule, a woman in the high-fertility "high-focus" states receives Rs. 1,400 for undergoing tubectomy (about $65 at purchasing-power parity) and the ASHA who motivates the case Rs. 200 ($9); the remaining states pay less than half these rates, and the highest-fertility districts are topped up further from 2016 under Mission Parivar Vikas. PPP conversions here and throughout use the World Bank consumption factor of roughly Rs. 21 per international dollar (2019).

tracks fertility. The high-fertility "high-focus" states pay more than double the other states' rates, and the Mission Parivar Vikas districts, selected for the highest fertility, pay the most,[7] so a woman's incentive to sterilize is strongest where fertility is highest. Several states have layered further inducements on top, such as lotteries for consumer durables among sterilized women.

The variation this paper exploits comes from a separate policy layer, state cash-transfer schemes that pay families for having daughters. These schemes were introduced with the intention of improving sex ratios and addressing son-preference rather than reducing fertility (Sekher, 2010; Srinivasan and Bedi, 2009). Unlike the compensation schedule for sterilizations above and other programs that induced variation in sterilization, the placement of these policies is not directly correlated with fertility. They are decided state-by-state rather than centrally, so exposure varies across states and years. They also split on one key design choice. *Sterilization-conditional* schemes release the money only against a parent's sterilization certificate, while *unconditional* girl-child schemes reward comparable daughter compositions with no family planning strings attached. One concern with a difference-in-differences design comparing states with and without sterilization-conditional schemes is that the income effect of the transfer itself may affect fertility, labor force and health outcomes directly. Comparing sterilization-conditional and unconditional schemes allows me to account for this income effect, albeit imperfectly, which I discuss in more detail in Section 6.8. Section 6 describes each scheme, its rules, and its payout (Table 6).

3 Data

I pool the 1992, 1998, 2005, 2015, and 2019 rounds of the National Family Health Survey (the Indian DHS), harmonized by IPUMS-DHS (Boyle et al., 2026). The survey includes retrospective birth histories for each woman which I use to build a woman-year discrete-time hazard panel to estimate the first stage of conditional policy eligibility on sterilization hazard. I date the sterilization year exactly from the date the woman began her current method, which the 2005, 2015, and 2019 rounds record for essentially all sterilized women. The 1992 and 1998 rounds record only banded years since sterilization, so I date those events to the most recent year their band allows; the scheme years the design uses fall almost entirely within the exactly dated rounds. The sample runs from about ninety thousand women in each early round to roughly seven hundred thousand in 2015 and 2019. Every round

[7] Mission Parivar Vikas is the central government's family-planning intensification drive, launched in 2016 and targeted at the highest-fertility districts (initially about 146 districts with a TFR of 3 or more, concentrated in the seven high-focus states). It expands service delivery, broadens the method basket, and pays enhanced sterilization compensation in the covered districts.

is representative at the state level, the level the analysis uses. Appendix A.1 details the design changes behind the growth in sample size. Table 1 describes the sample by round. The analysis is restricted to ever-married women, the population at risk of sterilization; all statistics use the DHS person weight multiplied by the survey's all-women factor, which makes the 1992 and 1998 rounds, sampled from ever-married women only, comparable with the later all-women rounds. Appendix A.1 details the sampling frames and the reweighting.

The second data input is a policy dataset I compile by hand, covering every state girl-child cash-transfer scheme. I record each scheme's launch and closure dates, eligibility rules (the rewarded child composition, birthdate cutoffs, income criteria), whether and when it required sterilization, and its payment schedule, assembled from government orders, scheme portals, and secondary reviews. Section 6 describes the schemes and Table 6 summarizes them.

4 Descriptive Analysis

This section documents key descriptive facts about India's fertility decline and the rise of sterilization.

4.1 Fertility Declined Faster Than Desired Family Size

I focus on two main measures of fertility in the paper, both of which declined steeply over time. The period total fertility rate (TFR) is the number of children the average woman would bear if she lived her reproductive life at the current year's birth rates.[8] It fell from 3.45 to 2.01 across the 1992–2019 survey rounds. Cohort completed fertility (CFR) is the number of children the women of a birth cohort actually end up with, measured at ages 40–49 when childbearing is over. CFR fell from about 5.0 for the 1945 birth cohort to 2.9 for the 1975 cohort, the last cohort I observe at completed fertility. Figure 1 plots the two on a common birth-cohort axis. The period rate's weakness is that it mixes the number of children with the timing of births. Nonetheless, the two series track each other closely where they overlap, a first sign that the decline reflects a decline in the number of children rather than just delays in childbearing. Section 8 discusses this in further detail.

Desired fertility fell alongside actual fertility, but more slowly. The figure's third series is the ideal number of children women report: across the same cohorts it fell from about 3.2 to 2.3 while completed fertility fell from 5.0 to 2.9, so the long-standing excess of births

[8] The TFR sums the age-specific fertility rates, the births per woman-year observed at each age in the 36 months before the interview. Appendix A.2 states the formulas for both measures.

over stated wants narrowed sharply. The decline is thus partly a fall in the demand for children and partly a closing of that excess. How much belongs to each is the question that remains. Appendix Figure A5a shows the completed and ideal series separately for sterilized and never-sterilized women, highlighting that the two fall in parallel for both groups.

4.2 Age at Last Birth Has Declined

India's fertility transition moved the age at which women stop having children, not the age at which they begin. Figure 2 plots the ages at marriage, first birth, and last birth by cohort in the pooled surveys. Across the 1943–1975 birth cohorts, the mean age at first birth rose by less than two years, from about 19 to 21. The mean age at marriage rose from about 16.5 to 18.5. In contrast, the mean age at last birth fell by about five years, from roughly 32 to 27. Rather than postponing the start of childbearing, the standard historical global pattern, they end it earlier and at lower parities. The median age at first birth is low relative to developing countries at similar fertility, and age-specific fertility rates collapse after the late twenties relative to other countries (Figure 3).[9]

4.3 Sterilization Dominates the Method Mix

Among currently married women using any method, sterilization is the method for 57 percent in 2019, and 62 percent pooled across the survey rounds (Figure 4). Among countries at comparable total fertility, India has the highest rate of female sterilization after the Dominican Republic (Figure 5).[10] Reversible methods remain a small and frequently discontinued share. For most women who use any modern method, that method is sterilization. It is important to note that sterilization is not just the endpoint of women's fertility journey, as only 14 percent of sterilized women report having ever previously used a modern reversible method. For most, the procedure is the first and only method they adopt. The sterilized share falls off for the youngest cohorts partially because the survey observes them at ages before most sterilizations occur (younger cohorts are less likely to be sterilized at any given

[9] Appendix A.2 defines the age-specific fertility rates and the total fertility rate built from them.

[10] The Dominican Republic, the heaviest user of female sterilization in Latin America (Leite, Gupta and Rodrigues, 2004), has slightly higher prevalence than India, and its system is not free of India's problems either. Quality-of-care studies document high regret among women sterilized young (Loaiza, 1995), and human-rights work documents coerced sterilizations of marginalized women there too (Patel, 2017). Despite the similar prevalence, sterilization policy differs sharply between the two countries, and the difference is what makes India stand out globally. The Dominican Republic, a country of eleven million, does not pay women to sterilize, while India promotes the method with acceptor and provider payments through the largest program in the world, in a population of reproductive-age women more than a hundred times larger. India is where sterilization operates as an instrument of national population policy, and that program is the object of interest here.

age than their older counterparts, Figure 6a). The demographic literature has documented the fertility schedule patterns above and linked them to sterilization (Bansal, Dwivedi and Ali, 2022; Singh et al., 2022), but the same patterns could arise from women compressing childbearing on their own, with sterilization the instrument of a decision already made rather than its cause. Separating the two is the task of Section 6.

4.4 Indian Women Sterilize Early and at Increasingly Lower Parity

Most Indian women who sterilize do so in their mid-twenties, with two or three children. Among sterilized women with exact procedure dates (the 2005–2019 rounds), the mean age at sterilization is 26 (median 25.7, interquartile range 23 to 29). Half had the procedure within four months of their last birth, a quarter within the same month as the birth. Table 2 compares sterilized with never-sterilized women at ages 40–49, where childbearing is complete and a never-sterilized woman will almost surely remain so. In this pooled sample the sterilized end with slightly fewer children (3.3 versus 3.5), but the gap is small and its sign is not stable. Within single birth cohorts observed at completed fertility the two groups have nearly the same family size, with the sterilized bearing slightly more children in the earliest and most recent cohorts and slightly fewer in between (Appendix Figure A5a). The sterilized are more likely to have at least one son (93 versus 83 percent with at least one son). They marry and begin childbearing about a year earlier and stop about two and a half years earlier (age at last birth 26.4 versus 29.1). They are also somewhat less educated and more often Hindu (Appendix Figures A5a and A5b show the cohort profiles). These differences are statistically significant, highlighting the selection problem that Section 6 addresses.

Figure 6 shows the profile across cohorts. The age curve of sterilization shifts left for later cohorts, and the mean parity at sterilization falls from 5.1 in the 1945 cohort to 2.6 in the 1985 cohort, with the share sterilizing at two children or fewer rising from 6 to 57 percent.

4.5 Female Sterilization Is Highest and Fertility Is Lowest in South India

The decline displays spatial heterogeneity, which matters later for identification. Figure 7 maps sterilization, the period TFR, and completed fertility by state in 1992 and 2019. Sterilization is highest in the south in both years and rose almost everywhere. The maps measure the share of all women aged 40–49 who are sterilized, at the ages where childbearing,

and hence sterilization, is essentially complete. In 2019 that share runs from 6 percent in Manipur and under 10 percent in Meghalaya to 72 percent in Tamil Nadu, 76 percent in Telangana, and 81 percent in Andhra Pradesh. The gradient is not only about whether women use contraception at all. Among currently married women aged 40–49 using any method, sterilization is the method for 94 percent of users in Tamil Nadu and 99 percent in Andhra Pradesh, against 47 percent in Uttar Pradesh and 28 percent in Assam, so southern users overwhelmingly choose the terminal method while northern and northeastern users rely on reversible and traditional ones (Appendix Figure A3). Fertility, in both the period rate and completed fertility, is the mirror image, lowest where sterilization is most common, falling everywhere over the period, and highest throughout in the northern belt (Bihar, Uttar Pradesh) where sterilizations are less prevalent. Appendix Figure A2 shows the same geography cohort by cohort. Within every state, later-born cohorts are more likely to be sterilized than earlier ones, and the states with the highest shares in one cohort are the highest in every cohort. The south's lead is therefore not a snapshot particular to the 1992 or 2019 survey; it holds cohort after cohort throughout the transition. The geographic patterns are important for the cash-transfer design of Section 6, and Section 7.4 further discusses what these patterns mean for identification.

5 Decomposition of the Fertility Decline

This section decomposes the decline in two steps. The first is demographic: which groups did the fertility decline and the rise of sterilization come from? I ask whether each changed within every education, religion, caste, wealth, and residence group, or through the population shifting across groups. The second step connects fertility to sterilization: a model-based accounting of which behaviors delivered the TFR decline, with sterilization one explicit channel, and a decomposition of completed fertility by sterilization status.

5.1 Fertility Fell within Every Social Group

To answer whether the population composition shifted towards women with lower fertility or whether fertility declined within-groups, I use an Oaxaca–Blinder decomposition (Oaxaca, 1973; Blinder, 1973), with the symmetric midpoint reference weights of Kitagawa (1955). For this decomposition, I focus on the changes across birth cohorts rather than periods and decompose the fertility decline across cohorts into a "within" part (groups changing their own behavior at fixed composition) and a "between" part (the population shifting across groups at fixed behavior). (Appendix A.6 states the identity and the symmetric treatment of the

cross-term). Groups are defined by education, religion, caste, and wealth, the characteristics that both predict fertility strongly and shifted most in composition. The intuition is that the demographic composition of the population may have shifted towards those with lower fertility, or groups may have changed their own behavior. For example, Hindu women born in 1945 tend to have lower fertility than Muslim women in 1945, and lower fertility may have occurred due to the proportion of Hindu families rising or due to Hindu or Muslim women simply reducing their fertility. The comparison runs from the 1945 to the 1970 five-year cohort, the earliest bin with substantial coverage at ages 45–49 and the last observed at completed fertility by 2019. Table 3 reports each group's two terms and the totals, for completed fertility and for lifetime sterilization.

The change is largely within-group. CFR fell by 1.87 children between the 1945 and 1970 cohorts, of which within-education behavior accounts for −1.61 (86 percent) and the shift in educational composition for only −0.26 (14 percent). Caste and religion composition matter even less. The pattern differs from Latin America's cohort record, where rising women's education is the dominant force behind completed-fertility decline, accounting for 39 percent in Calles and Vogl (2026)'s region-cohort panel design. I run their regression on Indian state-cohorts instead, which only credits education expansion with about 20 percent (half the Latin American share).[11] The Indian transition was not primarily a schooling story. Women with no schooling also cut their CFR from 5.4 to 3.7, nearly the size of the aggregate fall.

5.2 Sterilization Rose within Every Social Group

Who adopted the promoted method is also an important question. Population programs have historically concentrated on marginalized groups (León-Ciliotta, Zejcirovic and Fernandez, 2025; Visaria, Acharya and Raj, 2006), and India's program in particular has faced documented charges of coercion directed at poor and lower-caste women (Patel, 2017). If the rise of sterilization were concentrated among scheduled-caste, poor, or uneducated women, the fertility decline it delivered would reflect that targeting. The lifetime-sterilization panel of Table 3 runs the same decomposition on the share ever sterilized to ask where the method's spread was concentrated.

The rise was broad, rather than concentrated among specific demographic groups. The share ever sterilized rose by 0.27 between the 1945 and 1970 cohorts, and 97 percent of that rise is within-group. Every education, religion, caste, wealth, and residence group sterilized more over time, and the composition terms are essentially zero. By the 1970 cohort the sterilized share is nearly identical for SC/ST and other women (0.58 versus 0.55) and for

[11] Appendix A.7 states the regression and reports the estimates.

rural and urban women (0.57 versus 0.55), and it is actually lowest among the poorest wealth quintile (0.45 against 0.55 for the richest) and among women with secondary schooling or more (0.43). These are population aggregates. They speak to where sterilization spread, not to consent or quality of care in any individual procedure, which the outcome set of Section 6.10 also cannot reach.

5.3 The Decline Came from Contraception, above All Sterilization

To ask which behaviors delivered the decline, I use a standard model-based accounting of TFR from demography (Bongaarts, 1978). Rather than explicitly using births to women in the data, the model writes the rate as a biological maximum scaled down by one index for each of four main behaviors that stand between potential and observed fertility: marriage, contraception, breastfeeding, and abortion. Each index lies between zero and one, and a lower value means the behavior suppresses more fertility, which allows me to decompose how much of the fertility decline can be explained by sterilization (Appendix A.2 and Appendix A.4 discuss the model, indices and the origins of the biological maximum used in the model further). The intuition is that there is some maximum number of births a woman can have throughout her lifetime, but remaining unmarried or marrying later, using contraception, or having abortions will all reduce the number of births she has relative to this maximum. I exploit this standard methodology within demography to decompose changes in fertility in India to various factors, including sterilization. The accounting is purely descriptive and cannot say what a sterilized woman would otherwise have done.

In Table 4, I compute each index that makes up Bongaarts TFR separately by survey round. I also go one step further and separate the contraceptive index into different methods, a departure from the literature. The contraception index is $C_c = 1 - 1.08\,ue$: u is the share of currently married women using any method, e is the average use-effectiveness of the methods in use, and the constant corrects for natural sterility (Appendix A.4). Assigning each woman her most effective current method splits the product exactly into one term per method, $ue = \sum_m u_m e_m$, the share of women using method m times that method's effectiveness. These terms are the indented rows of the table: they sum to ue, and a rise in any of them, which indicates more women are covered by a more effective method, pushes C_c down. I find that the contraceptive index in particular falls from 0.64 in 1992 (contraception as a whole lowers fertility 36 percent relative to the biological maximum) to 0.33 (67 percent) in 2019.

To state what each behavior contributed, the table reports a counterfactual TFR for every row, that is, what the 2019 rate would have been had that one behavior stayed at its 1992 level

while everything else changed as observed. The model's own TFR, $\text{TF} \cdot C_m C_c C_i C_a$, falls from 5.10 to 2.15 across the period. Because the model multiplies the indices, holding behavior x's index at 1992 rescales that rate by the index's ratio, $\text{TFR}^{\text{cf}}_{2019} = \text{TFR}^{\text{model}}_{2019} \times C_x^{1992}/C_x^{2019}$. To hold a single method fixed instead, C_c is recomputed with that method's $u_m e_m$ at its 1992 value and every other method's at its observed 2019 value. The "% of decline" column states each behavior's share of the model's log decline, $100\,\Delta \ln C_x / \Delta \ln \text{TFR}^{\text{model}}$. Because the model multiplies the indices, its log decline is exactly the sum of the four behaviors' log changes, so these shares are additive and sum to one hundred.

This exercise shows that mechanically, contraception drove the decline, and within contraception the change is primarily sterilization, although an increase in the use of modern reversible methods is also important. Had contraception's prevalence and mix stayed at their 1992 levels, the model's 2019 TFR would have been 4.2 rather than 2.15, and contraception accounts for 76 percent of the model's decline. Freezing female sterilization alone puts the counterfactual at 3.2, and its share of the decline is 39 percent. Modern reversible methods account for 29 percent, and traditional methods 15. Vasectomy accounts for −7 percent, meaning that the decline in vasectomy use raised fertility relative to 1992. Holding marriage patterns at 1992 yields 2.4, 14 percent of the decline, with a rising age at marriage lowering its index from 0.74 to 0.66.[12] Holding postpartum infecundability at 1992 accounts for 7 percent and abortion for 3, the latter only indicative because the DHS severely underreports induced abortion (Singh et al., 2018).

The full-period accounting also averages over quite different sub-periods. Appendix Table A2 runs the same additive accounting on each pair of consecutive survey rounds. Sterilization drove the early decline, accounting for 64 percent of the model's 1992–98 decline and 55 percent of its 1998–2005 decline, against 13 and 22 percent for modern reversible methods. The ranking reverses at the end. Over 2015–19, modern reversible methods account for 38 percent and traditional methods for 29, with female sterilization at 14. The 2005–15 column is blank because the model implies essentially no decline over that decade. Measured

[12] C_m averages the proportion of women currently married at each age, weighted by the marital fertility schedule. The marital rates come from the survey files. The married proportions cannot come from the survey files for 1992 and 1998, because those rounds interviewed only ever-married women, and recovering the proportions through the all-women reweighting (Section 3) returns a 1992 index of 0.97, an artifact of the sampling frame rather than a measure of marriage. I instead take the 1992 and 1998 proportions from the same surveys' published household-roster tables, which enumerated never-married women (International Institute for Population Sciences , IIPS; International Institute for Population Sciences (IIPS) and ORC Macro, 2000). The 1991 Census independently supports the roster, classifying 35.3 percent of women aged 15–19 and 81.8 percent aged 20–24 as married, against 38.2 and 79.5 in the roster, and puts the female mean age at marriage at 19.3 (Vijayanunni, 1999). In the 2005–2019 rounds the survey itself interviews all women, so there C_m can be computed both ways: directly from the survey data, and anchored to the published tables as in the early rounds. The two versions agree to within 0.01, which validates using the published tables in the rounds where the survey data cannot supply the married shares.

contraceptive prevalence dipped in the 2015 round even as the observed TFR kept falling, so the model's inputs and the observed rate move apart there. The sub-period shares are computed from round-to-round changes in survey-measured inputs, so they are noisier than the endpoint accounting.

A cohort version of the accounting, which the data allow for contraception alone, tells the same story and is reported in Appendix A.5: the cohort contraception index falls from 0.47 for the 1945 birth cohort to 0.23 for the 1975 cohort, and female sterilization alone accounts for 94 percent of that fall (Appendix Table A1).

5.4 The Sterilized and Never-Sterilized Converged

"How much of the decline is sterilization" combines two questions: how much of the fall is the rising share of sterilized women, a decomposition, and how much lower fertility is because sterilization exists at all, a counterfactual that requires knowing what a sterilized woman would otherwise have done. This subsection answers the first with the data alone, and it complements rather than repeats the Bongaarts accounting of Section 5.3. The model-based accounting asks which *behaviors* moved the rate, treating contraception as one channel among four. The decomposition here uses no model at all and instead asks which *women* moved, splitting the observed fall in completed fertility between the two groups' own behavior and the shift of women from one group to the other, analogous to the decomposition by demographic group in Section 5.1. The counterfactual needs the variation of Sections 6 and 8.

Grouping women by sterilization itself asks: did fertility fall because more women got sterilized, or because fertility fell within both the sterilized and the non-sterilized? Table 5 reports the answer for the 1945-to-1970 cohorts, over which CFR fell by 1.87 children, using the same midpoint weighting as Table 3. Almost all of the decline is within-group, and the contribution rows come straight from the level rows above them. Each contribution is the group's Change entry times its Average-share entry. The never-sterilized cut their own completed fertility by 1.82 children, which their average population share of 0.58 turns into 1.05 children, 56 percent of the total decline. The sterilized cut theirs by 2.06, which their average share of 0.42 turns into 0.87 children, 47 percent. The composition term, the rise in the sterilized share (0.29 to 0.56) times the average CFR gap between the two groups, contributes essentially nothing (-3 percent), even though the sterilized share nearly doubled. The composition term is zero because the two groups' completed fertility is nearly the same, about 3.1 children each by the 1970 cohort, down from 5.2 and 4.9. The small gap between the groups even changes sign across cohorts, with the sterilized bearing slightly more children in the earliest and most recent cohorts and slightly fewer in between, so the salient fact is

similarity, not an ordering. Sterilization is the dominant instrument of the decline, as the contraception accounting showed, but the mechanism is lower-parity stopping, not sorting women into a low-fertility category.

Appendix Figures A5a and A5b show the convergence directly, and two of their facts matter later. Completed and ideal family size fall in parallel for both groups, so desired fertility fell for both alike and the convergence reflects a shared fall in demand. Age at first birth is also nearly identical across the groups while the age at last birth is several years earlier for the sterilized, so the difference is in timing, not in count.

6 The Sterilization-Conditional Cash Transfers

6.1 The Schemes

Beginning in the early 1990s, Indian states began paying parents cash transfers through their daughters' childhoods, programs I call girl-child cash transfers. Their stated aim was to counter son preference by raising a daughter's financial value. Tamil Nadu and Rajasthan launched the first schemes in 1992, a second wave followed in the 2000s, and by 2019 thirteen of India's eighteen large states had run at least one. The feature I exploit is that some schemes required that one parent provide a sterilization certificate while others did not. I call these schemes conditional and the rest unconditional.

The condition names either parent, but vasectomy had all but vanished in the exposed cohorts, covering half a percent of couples born 1961–1980 compared to a female-sterilization share of 44 percent (Figure 4). Therefore, a conditional scheme is a female-sterilization scheme in practice, and the first stage accordingly measures the woman's own sterilization. Table 6 lists both arms and Figure 8 maps where each operated. Several states ran both kinds at once, variation I also use.

The conditional arm includes seven schemes across seven states, launched between 1992 and 2007. Around that common core, eligibility varies on three margins: the sex mix (some schemes pay only families with no sons, while others also pay daughters with brothers), a family-size cap (at most two or three children in total, counting children of either sex born by the time the qualifying daughter arrives, with some schemes setting no cap), and a means test (a below-poverty-line card or an income ceiling). Every scheme in either arm pays for at most two daughters per family, with two exceptions on the unconditional side: Aapki Beti Hamari Beti added a third girl from August 2015, and Bebe Nanki pays for every girl.

The conditional schemes differ mainly in how they pay. Most buy a deposit or bond in the girl's name at enrollment, worth about Rs. 20,000 to over a lakh at maturity (roughly

$950 to $4,800 at purchasing-power parity), and release it at her majority. Haryana's Devi Rupak (2002) instead pays about Rs. 500 a month ($24 PPP) for twenty years. One scheme switches arms: Madhya Pradesh's Ladli Laxmi required the certificate at its 2007 launch and dropped it for the first daughter a year later, a within-state change I use for robustness.

Most conditional schemes cover only children born on or after launch, with cutoffs that differ scheme by scheme (Table 6).[13] What matters for the design is that eligibility in the estimation never depends on these rules, or on any choice a woman makes. I use the predetermined firstborn-girl margin throughout, for the reason Section 6.4 develops, and the exact date rules, which the hazard panel records, enter only as a robustness check (Appendix Table A8).

The unconditional arm is larger and more varied: the central Balika Samridhi Yojana adopted by Gujarat and Himachal Pradesh in 1997, the Ladli schemes of Haryana (2005) and Delhi (2008), Bihar's Mukhyamantri Kanya Suraksha Yojana (2008), Punjab's Bebe Nanki (2011–15), Andhra Pradesh's Bangaru Talli (2013–16), the most generous at over two lakh (about $10,300 PPP), and successor schemes of the 2010s in Rajasthan, Gujarat, and Uttar Pradesh. The two arms share the payout form, long-dated child-linked transfers maturing at the daughter's majority and frequently restricted to poor households, so their income content is comparable by construction and the difference between them is the sterilization certificate. Generosity still varies widely, from grants of a few thousand rupees to payouts above a lakh, which the income-netting variants of Section 6.8 account for.

Panel C of Table 6 lists three schemes of the right kind that had negligible take-up: Punjab's Balri Rakshak (about 306 beneficiaries), Himachal's IGBSY (about 700), and Dhan Lakshmi (effective in one district). Their state-years have a scheme on paper but essentially no treated women, so coding them as treated only dilutes the first stage. I set them aside, but using them leaves the estimates essentially unchanged (Appendix Table A3).

6.2 Empirical Strategy

The object of interest is the effect of the sterilization condition on fertility and on the downstream outcomes discussed below. Sterilization itself is not randomly assigned, and sterilized women differ from never-sterilized women on observables (Table 2) and, most likely, on unobservables too. Comparing them directly would certainly be confounded, but the state girl-child transfers supply variation in the incentive to sterilize independent of

[13] Devi Rupak requires the qualifying child born in 2002 or later, and Bhagyalakshmi and Ladli Laxmi cover girls born from 2006, so a Haryana couple whose daughters arrived in the 1990s gained nothing from Devi Rupak and acquired no new reason to sterilize. Andhra Pradesh instead credits sterilizations dated on or after its 2005 launch, Rajalakshmi enrolled girls up to age five, and Tamil Nadu has no birthdate cutoff at all, requiring only that the sterilized parent be under 35.

the woman's own choices. That variation has three confounds of its own. The schemes were placed in particular states, so exposure picks up everything distinctive about adopting states, such as son preference in particular states. They turned on in particular years, so exposure also picks up fertility trends that may coincide with launch. Third, each conditional scheme bundles the certificate requirement with an income transfer that could affect fertility on its own. The design addresses the three confounds with a triple-difference approach:

- comparing eligible to ineligible women within the same state and cohort nets out the fixed differences between adopting and other states;
- comparing cohorts a scheme covered with cohorts it did not nets out trends common to all women around launch, such as the national fertility decline; and
- comparing sterilization-conditional to unconditional transfers differences out the part of the transfer's income effect that the two arms share.

I instrument for sterilization with the interaction between a woman's predetermined eligibility and her exposure to a conditional scheme, controlling for her eligibility interacted with exposure to any girl-child scheme. The identifying variation is therefore the triple difference, and the design is a fuzzy difference-in-differences (de Chaisemartin and D'Haultfœuille, 2018). The first stage is a difference-in-differences on sterilization, the reduced form is the same difference-in-differences on completed fertility, and the effect of sterilization on fertility is their ratio, a Wald difference-in-differences. The pooled coefficient therefore compares changes in the eligible-minus-ineligible gradient under conditional schemes with changes in that gradient under unconditional schemes. It does not compare otherwise identical programs that differ only in the certificate. Throughout, I base the paper's claims on the intent-to-treat estimates and read the instrumented ratio as a second step with its weak-instrument caveats (Section 6.7).

One potential confounder survives all three differences: scheme type lines up with region, with the conditional schemes sitting mostly in the south and the unconditional ones mostly in the north, where son preference differs, although some southern and northern states did implement both types during this period. Section 7.4 confronts this potential confound with region-by-cohort trends and with within-state comparisons using states that ran both scheme types, which avoid the cross-region comparison entirely. The within-state estimates are weak at ages 40–49 only because few exposed cohorts have reached those ages, and they strengthen when lowering the sample's minimum age lets recently exposed cohorts enter.

6.3 Estimating Equations

The estimation sample I use is women observed at ages 40–49, when childbearing is essentially complete, in the thirteen states that ever ran a girl-child scheme. The conditional-versus-unconditional contrast, therefore, compares states that all chose to run some daughter transfer. Within it I keep mothers with at least one birth, because the sex of the first birth defines eligibility and a childless woman is never at the stopping margin the schemes target.[14] The eligibility comparison is therefore between firstborn-girl and firstborn-boy mothers. An all-India version that adds never-scheme states as zero-exposure cells gives nearly identical estimates, and keeping childless women in the sample with composition controls changes nothing (Appendix Table A4).

For woman i in state s, let exposure $\bar{C}_{is}$ be the share of her stopping-decision window, ages 25–39,[15] during which a conditional transfer was active in her state, and $\bar{U}_{is}$ the same share for unconditional transfers. Eligibility E_i comes from the sex of her earliest births, which is fixed before any scheme could act on her choices. E_i^1 indicates that her first birth is a girl, and E_i^2 that her first two births are girls. Section 6.4 explains why I use these predetermined births rather than the exact composition a scheme rewards.[16] The first stage regresses an indicator S_{is} for being ever sterilized by ages 40–49 on eligibility interacted with the two doses,

$$S_{is} = \phi_1 \left(E_i \times \bar{C}_{is}\right) + \phi_2 \left(E_i \times \bar{U}_{is}\right) + \delta\, E_i + \mu_1 \bar{C}_{is} + \mu_2 \bar{U}_{is} + \alpha_s + \alpha_{c(i)} + h(X_{is}) + v_{is}, \quad (1)$$

with state fixed effects α_s, cohort fixed effects $\alpha_{c(i)}$, and covariate controls $h(X_{is})$. The reduced form is the same regression, same sample and same regressors, with completed fertility F_{is}, her children ever born, as the outcome:

$$F_{is} = \pi_1 \left(E_i \times \bar{C}_{is}\right) + \pi_2 \left(E_i \times \bar{U}_{is}\right) + \delta\, E_i + \mu_1 \bar{C}_{is} + \mu_2 \bar{U}_{is} + \alpha_s + \alpha_{c(i)} + h(X_{is}) + u_{is}. \quad (2)$$

[14] The Indian government requires that a woman be married with at least one child before the public system sterilizes her.

[15] The window brackets the central mass of stopping decisions. The median age at sterilization is 25.7, with an interquartile range of 23 to 29, and the procedure directly follows the last birth. The estimates do not depend on this choice. Rebuilding exposure on shorter windows (ages 25–37 or 25–35) or earlier-starting ones (20–39 or 22–39) moves the one-daughter reduced form only between −0.37 and −0.44 (Appendix Table A13) and the first stage between 0.08 and 0.09 in the all-India specification.

[16] Several of the schemes are formally restricted to households below the poverty line. I do not condition eligibility on poverty status. The only available measure, a below-poverty-line ration card, is recorded at the survey, after the transfer window, and may itself respond to the transfer or to a woman's fertility. Using it would therefore risk reintroducing the very endogeneity the predetermined design avoids. Exposure is therefore scheme presence in the woman's state and cohort, an intent-to-treat measure that does not require her poverty status. Restricting the auxiliary per-scheme first stage to a below-poverty-line proxy leaves the conditionality estimate in the same range.

I estimate each equation separately, reusing δ and the μ's here to keep the notation simple. I also estimate each eligibility margin in its own regression. At the one-daughter margin I set $E_i = E_i^1$. At the two-daughter margin I keep every E^1 term and add the E^2 term, so the two-daughter coefficients measure what the second daughter adds beyond the first. I do not take the one-daughter estimates from that same regression, because holding the E^2 terms fixed would identify them from births that come after the first, which the schemes can move (Appendix A.8 writes this specification out).

Each coefficient of interest is a difference-in-differences estimate built from the same object, the gap in the outcome within a state-cohort between eligible and ineligible women. The comparison group is composed of the cohorts within these same states that are already past their stopping window when their state's scheme launched. π_1 is the difference-in-differences estimate for conditional coverage, the change in the completed-fertility gap between state-cohorts a conditional scheme covered and the comparison group. It is an intent-to-treat effect of the offer as a whole, condition and payment together, which I call the conditional package. π_2 is the analogous estimate for unconditional coverage, a payment with no condition attached. The difference $\pi_1 - \pi_2$ is the triple-difference estimate. It compares the response of eligible women offered the conditional package with the response of eligible women offered only the payment. If the two payments were equal, the money would raise both gaps by the same amount and drop out of the difference, leaving the effect of the condition itself. In practice the two arms also differ in payment size and structure, in the compositions they reward, and in where and how they were implemented, so I read $\pi_1 - \pi_2$ as a conditional-minus-unconditional contrast in which the certificate requirement is the most salient difference but not the only one. Appendix Table A7 tests which of the other differences matter, and Section 6.8 further discusses whether the subtraction is able to completely difference the income effect of the transfer.

Analogous interpretations apply to the first stage coefficients. ϕ_1 is the effect of the conditional package on sterilization, ϕ_2 that of the unconditional one, and the ratio of the two netted differences, $(\pi_1 - \pi_2)/(\phi_1 - \phi_2)$, is births per induced sterilization. The result tables use two column labels for these objects. "Package" columns report π_1 or ϕ_1, the effect of conditional coverage against no scheme. "Cond. − uncond." columns report the triple difference $\pi_1 - \pi_2$. Appendix Table A6 reports the results with per-coefficient wild-cluster inference, and Table 11 re-estimates the same specifications purely within the seven states that adopted sterilization-conditional schemes, eligible against ineligible women across covered and uncovered cohorts, with no unconditional arm in the comparison. I obtain that specification by dropping the unconditional interactions from equation (2), keeping the $\bar{U}_{is}$

main effect, and restricting the sample to those states:

$$F_{is} = \pi^{w}\,(E_i \times \bar{C}_{is}) + \delta\,E_i + \mu_1\,\bar{C}_{is} + \mu_2\,\bar{U}_{is} + \alpha_s + \alpha_{c(i)} + h(X_{is}) + u_{is}. \tag{3}$$

Here π^{w} is the intent-to-treat effect of the conditional package against no scheme, a policy estimate in its own right: the effect of offering the sterilization-conditional transfer on eligible women's fertility. It requires no assumption about the two arms' comparability, because no unconditional scheme enters the comparison.

Exposure is a continuous treatment dose rather than a binary indicator. A woman whose scheme was active for six of her fifteen window years has $\bar{C}_{is} = 0.4$, and π_1 is the slope per unit of exposure. I quote magnitudes at full exposure, the outcome change for a woman with the qualifying composition, relative to one without it in the same state and cohort, as exposure goes from zero to one. In Appendix A.8 I describe where the exposure variation comes from, what the $\bar{C}_{is}$ and $\bar{U}_{is}$ main effects absorb, and the checks for linearity in the dose and for adding never-scheme states.

Treatment varies at the state-year level and few states are treated, so I report wild-cluster bootstrap p-values rather than conventional clustered standard errors (Cameron, Gelbach and Miller, 2008). The triple-difference structure follows Gruber (1994).

6.4 Predetermined Eligibility

A scheme rewards a particular child composition, one or two daughters, but her completed family is the outcome of interest itself. The transfers reward stopping at daughters, so the composition a woman ends up with is a consequence of the treatment, and conditioning on it would condition on the outcome itself. Realized composition also selects on preferences even absent the policy. A woman with two daughters, no son, and no sterilization is disproportionately a woman still trying for a son, which biases the first stage toward zero or the wrong sign. This is the sequence-of-treatments problem that Anukriti (2018) confronts in the same setting. Following her and Bhalotra and Cochrane (2010), I instead define eligibility from the sex of the early births, fixed before any scheme could act on the family's choices: E_i^1 indicates that the first birth is a girl (the "one-daughter margin"), E_i^2 that the first two births are girls (the "two-daughter margin"), each switching on from the year of the qualifying birth. For E^1 predetermination is exact, since the first birth's sex is set before any scheme can matter. E^2 is weaker, because it requires a second birth, and whether a woman has a second birth can itself respond to a scheme if she was exposed after the first. The one-daughter margin is therefore the paper's primary margin, and I read the two-girl margin

as supporting evidence.[17] Conditional on parity these indicators are as good as random. Firstborn-girl and firstborn-boy women are balanced on predetermined covariates (Table 7), and the first-birth sex ratio is close to the natural rate, with son-targeting concentrated at later births (Table 8). Section 7.4 examines both further. Throughout, "eligibility" means these predetermined birth-sex indicators, not a scheme's full statutory rule. Where an indicator and a rule mismatch, the interaction averages in women the rule does not cover, which will attenuate the effect to zero rather than skew it upwards. The schemes' birthdate cutoffs are likewise not imposed. Enforcing them directly nearly halves the average conditional dose yet leaves both stages essentially unchanged, because the identifying exposure at ages 40–49 comes from the long-running schemes without cutoffs (Appendix A.8; Appendix Table A8).

6.5 Identifying Assumptions

A causal interpretation of the results requires three broad conditions. First, that absent the programs, the eligible-minus-ineligible fertility gap would have evolved similarly across the relevant states and cohorts. The conditional-versus-unconditional comparison must pick up no other regional change correlated with daughter composition. And for the two-stage least squares step only, which divides the reduced form by the first stage to obtain a per-sterilization effect, the schemes must move fertility only through sterilization. Formally, identification requires six assumptions:

- *Common trends:* one condition, imposed on the eligibility gap rather than on levels (Olden and Møen, 2022). It does not require that eligible and ineligible women, or southern and northern states, be comparable, only that the firstborn-sex gradient in sterilization and fertility would have followed the same cohort path across conditional, unconditional, and untreated state-years absent the schemes. Figure 9 shows the test visually, plotting the two difference-in-differences that form the triple difference with the eligibility gap flat before each launch, and Section 7.2 reports the joint pre-trend tests and the pre-cohort placebo.

- *Exogeneity of eligibility:* the sex of the early births is as good as random given parity (Bhalotra and Cochrane, 2010). Tables 7 and 8 provide suggestive evidence.

- *Exclusion:* the netted instrument affects completed fertility only through sterilization. The threat is that the programs change how many children an eligible family wants, or

[17] Restricting eligibility to births that predate the first girl-child scheme in the woman's state, where predetermination is airtight at both margins, leaves the estimates close to the baseline (Appendix Table A9). The restricted intervals are wider, since a woman whose births all predate the launch is older when the scheme arrives and has less exposure.

how the family reaches that number, through some channel other than the sterilization itself. The money is the clearest such channel. A transfer that pays for a daughter's upbringing lowers the cost of raising her and could make the family willing to have more children, or, arriving at her majority, could substitute for sons as old-age support and lower desired fertility. The netting differences out the part of this response the two arms share, and the payouts are mostly distant and illiquid, a weak lever on current births for credit-constrained households. Second, a woman whose desired fertility the program changed could reach the new number with reversible methods rather than a sterilization, and fertility would then move with no induced sterilization. In the data the transfers raise sterilization specifically and, if anything, lower other modern-method use, so the response runs through the rewarded method (Section 7.7). Third, the schemes reward daughters and could therefore reduce sex-selective abortion, and less son-targeting means fewer repeat pregnancies after girl births and lower fertility with no sterilization involved. The marginal-birth sex test of Section 7.7 finds no such response on average. For downstream outcomes the payment can move the outcome directly, so exclusion additionally requires the netting to remove it outcome by outcome.

- *Common income response:* the income effect of a girl-child transfer on the eligible-minus-ineligible gap is the same under conditional and unconditional schemes. The netting subtracts the unconditional arm's eligibility gradient from the conditional arm's, so it removes the income channel only if the money moves eligible relative to ineligible women equally in the two arms. The compiled payment schedules (Table 6) support it. Both arms mostly pay long-dated transfers tied to the daughter and maturing at her majority, with comparable expected value, so equal income effects are plausible, but this is an assumption of the design, not a mechanical fact.

- *Monotonicity:* every woman's sterilization is weakly increasing in the instrument. No eligible woman becomes less likely to sterilize when conditional coverage of her window rises, so there are no defiers. With the continuous exposure dose, the two-stage estimate is then a first-stage-weighted average of complier effects across exposure margins (Angrist and Imbens, 1995).

- *Fuzzy difference-in-differences stability:* sterilization was rising everywhere independently of the transfers, so the two-stage ratio identifies the compliers' effect only if the per-sterilization effect is stable across cohorts and the same for transfer-induced sterilizations as for the background ones (de Chaisemartin and D'Haultfœuille, 2018). Both conditions are strained here in a known direction, so I quote magnitudes from the

lower ends of the weak-instrument-robust confidence sets rather than from the point estimates (Section 8).

6.6 First Stage: Conditionality Raises Sterilization

Panel A of Table 10 reports the first stage at the primary one-daughter margin. Full exposure to a conditional rather than an unconditional transfer raises the probability that an eligible woman is ever sterilized by ages 40–49 by 8.2 percentage points, with a first-stage F of 26. Against a base of 26 percent ever sterilized among the oldest cohorts, this is a large response. The two-girl margin, which is less cleanly predetermined (Section 6.4), gives 11.1 points with $F = 29$ (Appendix Table A5).

6.7 Reduced Form and IV: Effect on Completed Fertility

Attaching the sterilization condition lowers an eligible woman's completed fertility by about 0.40 children at the one-daughter margin in the conditional-minus-unconditional contrast (Table 10, Panel B), and by 0.39 at the two-girl margin (Appendix Table A5). This reduced form, an intent-to-treat effect, is the basis for the paper's conclusions. The two-stage least squares step below converts it into a per-sterilization effect, but that ratio requires the exclusion restriction and inherits the first stage's imprecision, so I use it for scale rather than for the headline magnitude. The reduced form itself is robust across various sample and coding choices. I can restrict to mothers or keep childless women in the sample, adding a childlessness indicator and its interactions with the two doses so that childless women contribute no eligibility variation. I can also extend use the thirteen scheme states or extend the sample to all of India without the estimates moving. Across these choices the estimate stays between -0.34 and -0.41 and remains significant at the one percent level in every variant (Appendix Table A4), and the two margins, which use different eligibility variation, agree closely. Since completed fertility is measured at ages 40–49, the effect is fewer children ever born, not a re-timing.

Since the unconditional and conditional schemes differ in their payments, I also estimate the package effect with no cross-arm comparison at all, running equation (3) in the seven adopting states alone. Full exposure to the conditional package raises eligible women's sterilization by 4.1 percentage points and lowers their completed fertility by about 0.24 children (Table 11). With seven clusters the wild-cluster p-values are 0.24 and 0.14, and they tighten to 0.02 and 0.06 once I add a control for the woman's exposure to unconditional schemes, which some of the seven states also ran. Lowering the age floor to 35 to admit more recently covered cohorts gives 3.7 points and -0.27 children. This estimate nets out

none of the payment's income effect, so I read it as the effect of the whole conditional offer, money included.

Two-stage least squares: To turn the two stages into a per-sterilization effect, I instrument sterilization with the conditional-exposure interaction $E_i \times \bar{C}_{is}$ while controlling for the *any-scheme* interaction $E_i \times (\bar{C}_{is} + \bar{U}_{is})$. This is ordinary two-stage least squares with one endogenous variable and one excluded instrument. The estimate equals the netted ratio $(\pi_1 - \pi_2)/(\phi_1 - \phi_2)$, the effect of sterilization for the women whom the condition itself moves (Imbens and Angrist, 1994; de Chaisemartin and D'Haultfœuille, 2018).[18] Controlling only the unconditional dose instead would leave the payment inside the instrument. The payment can affect outcomes directly, so that version does not satisfy the exclusion restriction for sterilization alone; it scales the effect of the conditional package, condition and cash together, and I report it only as a comparison. The netted instrument avoids this problem because the differencing removes the payment's direct effect before the instrument reaches the outcome.

Intent-to-treat and LATE: The first stage and the reduced form are intent-to-treat effects. They measure what eligibility under a conditional rather than an unconditional scheme does to the average eligible woman, whether or not she responds to it. The two-stage ratio is a local average treatment effect, births per policy-induced sterilization, for the women the condition moves. I report both for every outcome. For completed fertility the exclusion restriction is credible but not automatic. The netting removes the income channel the two arms share, the payouts mostly mature after childbearing, and the two remaining channels, stopping through reversible methods and sex selection, are tested and found near zero (Section 6.5 states the argument). For the downstream outcomes the ratio requires more, which Section 6.10 spells out, so there I base the interpretation on the intent-to-treat estimates and report the ratio only for scale.

Table 12 reports the instrumented per-sterilization effect. Two-stage least squares with the netted instrument gives about -5.0 births per induced sterilization at the one-daughter margin. Inverting the Anderson–Rubin test gives a weak-instrument-robust 95 percent confidence set of $[-7.7, -3.4]$, wider to the left than the conventional interval but with nearly the same floor. The two-girl margin gives -3.5 with a set of $[-5.3, -2.4]$ (Appendix Table A5).

The point estimates are too large to take literally. Scaled by the rise in sterilization, they would credit sterilization with more of the fertility decline than it could explain even if every sterilization had prevented every remaining birth. Section 8 computes that ceiling at about

[18] Partialling the included any-scheme interaction out of the excluded instrument leaves only variation in which type of scheme covered an eligible woman's window, holding the total covered share fixed. The 2SLS coefficient is the reduced form on that residual variation over the first stage on it, which is the netted ratio.

67 percent, and no causal share can exceed it. The reduced form barely moves across codings, but the first stage ranges from 0.04 to 0.11, and the ratio inherits that variation, which is responsible for the high ceiling. The Wald ratio also requires stronger assumptions here than in a standard IV setting. It equals the compliers' effect only if the per-sterilization effect is stable over time and the same for compliers as for everyone else (de Chaisemartin and D'Haultfœuille, 2018). Neither holds here. Sterilization was rising everywhere for reasons unrelated to the transfers, from 0.26 to 0.57 of women by the end of childbearing, and women sterilized at ever lower parities, so the births each sterilization averted grew across cohorts. The two-stage estimate mixes the compliers' effect with that drifting background effect and can land outside the range of either.[19]

I therefore take the lower ends of the two margins' Anderson–Rubin sets, about 2.4 to 3.4 births per induced sterilization, as the defensible magnitudes and treat the point estimates as upper bounds. The paper's quantitative claims are based on the reduced form and on the share-of-decline bracket of Section 8.

6.8 The Income Channel and Transfer Value

Any daughter transfer, conditional or not, moves outcomes through the money it pays and through the message that daughters are valued, fertility included. Unconditional transfers indeed raise completed fertility at the two-girl margin. That result is direct evidence that the money moves fertility on its own, so a comparison of conditional schemes against no scheme would confound the condition with the cash, which is why the design differences against the unconditional arm. Differencing the conditional against the unconditional gradient removes the part of this confound the two arms share, identically in both stages, but only if the two arms have equal income effects. That is an assumption, not something the design guarantees, though the compiled payment schedules make it plausible. Both arms are predominantly long-dated child-linked payouts maturing at the daughter's majority, the comparison uses expected receipt (the offer times the claim rate) at a common rupee vintage, and the government's standing sterilization compensation, common to every state and group, differences out. The complier's own payment is a separate matter. For completed fertility it matters little, because the money mostly arrives at the daughter's majority, after childbearing has ended, and any anticipation effect before then is shared with the unconditional arm and differenced out. For her later-life outcomes the payment could matter directly, since a large transfer can change what a woman does for work, what her household eats, and her position

[19] A complier characterization in the style of Abadie (2003) would pin down the compliers' counterfactual fertility directly, but the instrument has power only as a difference-in-differences interaction, not at any single binary margin, so it is not available here.

in it. I therefore report each downstream outcome both as the package and as the netted contrast, and the gap between the two measures the payment's own role. The results in Section 6.10 show both cases. Work and anemia move more in the netted contrast than as the package, so the payment is not what moves them, while the sexual-violence and birth-spacing responses appear as the package and vanish once netted, the signature of an income effect.

The two arms' payouts differ widely in size, from a few thousand rupees to over two lakh, and coding exposure as scheme presence treats them all alike, so the raw contrast can misattribute differences in generosity to the condition. As a sensitivity analysis, I rebuild the contrast on the transfers' present value instead of scheme presence. The conditionality effect in the hazard specification stays in a narrow band, and the coefficient on the transfer's value itself, the change in sterilization per additional lakh of payout, is negligible (Table 9).

6.9 Age at Last Birth and Parity Progression

Completed fertility is not the only fertility outcome the conditionality should move. If the transfer works by making women stop childbearing earlier, it should lower the age at last birth and cut progression to higher parities. Table 13 reports the package, netted, and per-sterilization readings on these margins, as for every outcome in the paper. In the netted contrast, the age at last birth falls by about 0.85 years on the eligible margin, roughly a sixth of the five-year fall across cohorts (Figure 2). Progression to higher parities falls by about 8 percentage points at each of the third, fourth, and fifth births. The conditionality moves the stopping margin, not the starting one. The per-sterilization column divides by the same first stage as the completed-fertility ratio and inherits its upward bias, implying an implausible ten years of earlier stopping per induced sterilization. Here too the point estimates are upper bounds (Section 6.7) and I base the interpretation on the intent-to-treat columns.

6.10 Effects beyond Fertility

If the conditional transfers brought women's childbearing to an earlier stop, they may also have changed what those women do afterward, which the completed-fertility estimate cannot show. I estimate equation (2) at the one-daughter margin with each downstream outcome in place of completed fertility, on mothers aged 30 and older, so the netted contrast is again $\pi_1 - \pi_2$. The wider age window is possible because these outcomes are current status at the survey rather than a lifetime count, so they require only that the woman's sterilization decision is largely behind her, and most sterilizations occur before age 30. It also enlarges

the samples for the module-based outcomes, the violence and biomarker measures collected for subsamples in the later rounds. These are measured outcomes, not a welfare accounting: the outcome set omits consent and coercion in the sterilization decision itself, regret, surgical complications, and mental health, costs that could dominate a welfare reckoning in this setting (Patel, 2017). I go through the outcome families in turn: economic empowerment, her own health, decision-making autonomy, experienced domestic violence, gender-role attitudes, and effects on her children, in the next subsection. For the economic, autonomy, and attitude families I also build a summary index, the equally weighted average of the standardized components rather than a principal component, restricting each to components asked consistently across waves so its composition does not change. Table 14 reports three numbers for each outcome. The package column is the effect of the whole conditional scheme, payment included (π_1). The netted column differences it against the unconditional transfers ($\pi_1 - \pi_2$). The per-sterilization column instruments the woman's sterilization with the same netted interaction used for fertility. In this sample the first stage is 0.10, so this column is roughly the netted effect scaled up by ten. The discussion below focuses on the first two columns, and the end of this section explains what the third does and does not measure.

Economic activity: The economic outcomes are working for pay, the type of work (white-collar, agricultural, manual), and controlling her own money; the health outcomes are body mass index, anemia and hemoglobin, and reported high blood pressure and diabetes, from the biomarker modules of the later rounds. The package and netted columns separate what the stopping does from what the payment does. Economic empowerment and the woman's own health respond to the sterilization condition itself, and the netted contrast is larger than the package effect rather than smaller. The economic index rises about 0.03 of a standard deviation as the raw package and 0.06 once income is netted. The chance a woman works for pay is up about two percentage points as the package and four in the netted contrast, a roughly one-eighth increase on a base of 0.37. The kind of work points the same way, though with less power. In the netted contrast, the detectable gains at 30–49 are in manual labor, about 1.3 points on a base of 0.08, with a marginal 0.8-point rise in white-collar work on a base of 0.04, and at 40–49 in agriculture, about 4.6 points on a base of 0.25 (Table 14; Appendix Table A11). The occupation bases are small, so these rows show where the gains are concentrated rather than ruling out changes elsewhere.[20] The pattern fits a human-capital reading. Sterilization ends childbearing in a woman's late twenties, when her schooling is already fixed, so the condition can raise her participation in the labor force but

[20]Occupation is recorded for all women in the 1998 and 2005 rounds and for a random subsample (the state module) in 2015 and 2019, so the occupation rows use a fraction of the works-for-pay sample and have correspondingly less power.

not her qualifications, and the work she adds is the kind that requires no additional human capital.

Health: The probability she is anemic falls 1.4 points due to the package and 2.7 points once netted, and her hemoglobin rises in step.[21] That the netted effect is the larger one is intuitive here. A girl-child income transfer that does not end childbearing neither frees a woman to work nor spares her the iron cost of further pregnancies, so subtracting its effect sharpens rather than dilutes the contribution of the stopping. Anemia is the outcome most directly relieved by ending pregnancies earlier, and the nutritional gain is the channel De La Rupelle and Dumas (2025) also find for sterilization. Body mass index, blood pressure, and diabetes are unchanged in both the package and the netted columns.

Domestic violence, autonomy, and attitudes: The violence outcomes are whether she has ever experienced physical, emotional, or sexual partner violence; the autonomy outcomes are whether she has a say, alone or jointly, in her own health care, large purchases, and contraception, with the summary index built from the three consistently asked decision items (health care, large purchases, and family visits). The attitudes outcome is an index of nine survey items that ask whether a husband is justified in beating his wife in specific situations, each coded one when she rejects the justification. The domestic-violence outcomes show the opposite pattern, and they illustrate why the package-versus-netted distinction matters. Experienced sexual violence falls about 1.3 points as the package but is indistinguishable from zero once income is netted, so the response likely comes from the transfer alone. Emotional violence falls in both columns, about three points as the package and two in the netted contrast. Physical violence is unchanged throughout.[22] Neither the autonomy index nor any decision component shifts in either column, and the wife-beating-attitudes index is flat. The program changed women's circumstances, not their stated norms nor their reported say in household decisions. Among her children, child mortality is unchanged, and the post-scheme birth spacing that looks like a one-and-a-half-month package effect is likewise the payment's, vanishing once income is netted. The share of her children who are sons falls by about fourteen points, but for a mechanical reason. A woman who qualifies through a firstborn daughter and stops sooner has fewer subsequent births and a more daughter-heavy completed family. The result comes from stopping, not from the marginal-birth sex selection Anukriti (2018) finds for Devi Rupak. The direct test of her channel is the sex of the marginal birth,

[21] The DHS cash-earnings variable changes definition and universe across rounds (its non-response rate ranges from near zero to over ninety percent across waves), so I drop it and build the economic index from current work and control over money only.

[22] The survey administers the domestic-violence module to only one selected woman per household, from 2005 on, so I estimate these outcomes on that subsample (roughly nine percent of the 2015 and 2019 women) rather than the full sample. The measures refer to violence by the partner.

which does not respond on average across the pooled schemes (Section 7.7), though Devi-Rupak-style selection in a particular scheme cannot be ruled out. The 40–49 sample shows the same pattern (Appendix Table A11).

The per-sterilization column: Dividing by the first stage converts "the effect of being offered the condition" into "the effect per woman who took it up," so the column shows the magnitudes on the scale of the fertility estimate. The economic index, for example, moves about 0.06 of a standard deviation per eligible woman and about 0.6 of a standard deviation per induced sterilization. I report the number but do not rely on it, for three reasons. The payment can move a woman's work, nutrition, or say in the household directly, so the netting must remove its effect outcome by outcome for exclusion to hold. Under the mostly daughter-only schemes the complier both sterilizes and completes her family without a son, so the per-sterilization number is really the effect of sterilization plus a sonless family, and in this setting sons shape old-age support, status, and bargaining power. And the denominator inherits the fertility ratio's first-stage fragility and fuzzy difference-in-differences caveats unchanged (Section 6.7). The intent-to-treat columns have none of these problems, and the conclusions are based on them.

6.11 Effects on Children

Lowering family size is the classic lever for the quantity-quality trade-off, since with fewer children each might receive more (Becker, 1981; Rosenzweig and Zhang, 2009). I test this directly by running the conditionality reduced form at the child level. For child j of mother i in state s,

$$Y_{jis} = \pi_1 \left(E_i^1 \times \bar{C}_{is}\right) + \pi_2 \left(E_i^1 \times \bar{U}_{is}\right) + \delta E_i^1 + \mu_1 \bar{C}_{is} + \mu_2 \bar{U}_{is} + \alpha_s + z'_{jis}\theta + u_{jis}, \qquad (4)$$

where each child inherits its mother's predetermined eligibility E_i^1 and state-by-cohort exposure, z_{jis} contains the child's birth order and sex together with cohort, wealth, urban, and religion controls, and $\pi_1 - \pi_2$ is again the netted contrast. Table 15 reports the same three readings as the woman-level tables. The per-sterilization column has one more problem beyond those of Section 6.10. A fertility intervention changes which children are born at all. The children a sterilization prevented are absent from the sample by construction, so the child-level first stage is estimated on a set of children the treatment itself selected, and the ratio has no per-child causal interpretation. I report that column only for scale. I run it on three families of child outcomes: anthropometry (stunting, underweight, wasting), immunization (DPT3, measles, and the full schedule), and survival and anemia (Table 15). The anthropometric and anemia measures exist for living under-fives, while survival is defined

over all children ever born. No human-capital gain appears. In the conditional-minus-unconditional contrast, immunization, stunting, wasting, and anemia are unchanged. The two estimates that move are small and adverse, a rise of just under two points in the share underweight and a marginal rise in child mortality, and neither survives scrutiny as a harm from the condition.

The underweight and mortality coefficients: The package and netted columns of Table 15 distinguish their sources. The underweight rise is essentially the same whether read as the raw package or in the netted contrast, so it is not a result of the income transfer. It is also a single small coefficient among eight outcomes, and the conditional schemes operate in poorer areas, a level difference that differencing against unconditional schemes in other states does not fully remove. The mortality rise appears only in the netted contrast and is zero as the package, so it is an artifact of differencing against an unconditional arm that is if anything protective. Against the six flat outcomes, the estimates read as a null, consistent with the long-run Matlab follow-up (Barham et al., 2021), the modest quantity-quality trade-off under China's one-child policy (Rosenzweig and Zhang, 2009), and the Latin American cohort record (Calles and Vogl, 2026). One caveat is intrinsic to the exercise. By changing family size the conditionality changes which children are observed, so the estimate is the net effect on the children who exist, not a pure resources-per-child channel.

7 Validation, Robustness, and Mechanisms

The causal reading depends on the assumptions of Section 6.5. This section takes the main threats in turn, stating each issue and then the evidence on it.

7.1 Timing: The Response Follows the Condition

Eligible women's sterilization moves when and where a conditional scheme is active, and not otherwise. That timing is a validation test the woman-level equations cannot run, because they collapse each woman's history into a single observation at ages 40–49 and so cannot show that the extra sterilizations happened while the transfers were on offer. This subsection tests the timing in four ways.

If the condition is what moves behavior, the eligible-minus-ineligible sterilization gap should widen in the years a conditional scheme is active, in the states running one, and nowhere else. I therefore estimate a discrete-time hazard. I build a panel with one observation per woman for each year of marriage using the birth histories provided in the NFHS, following

her until sterilization or censoring at her interview, and run

$$S_{ist} = \gamma\,(E_i \times \text{Cond}_{st}) + \lambda\,(E_i \times \text{Any}_{st}) + \delta\,E_i + \alpha_{st} + g(\text{age}_{it}) + \varepsilon_{ist}, \tag{5}$$

where S_{ist} indicates that woman i sterilizes in year t, Cond_{st} indicates that a sterilization-conditional transfer is active in state s in year t, and Any_{st} that a girl-child transfer of either type is. The coefficient γ is the annual analogue of the triple difference $\phi_1 - \phi_2$, the differential sterilization hazard of an eligible woman in a state-year with a conditional transfer, over and above her differential hazard under an unconditional one. Because treatment now varies by year, the regression can include state-by-year fixed effects α_{st}, which absorb the scheme indicators and every other state-year shock. The interaction with Any_{st} provides a placebo test. A transfer that pays whether or not the woman sterilizes gives an eligible woman no extra reason to sterilize, so λ should be near zero if the condition, and not the cash, moves sterilization.

Appendix Table A10 reports the estimates. The one-daughter contrast is sensitive to when women enter the risk set, so the year-level evidence comes from the two-daughter margin.[23] The unconditional coefficient λ is close to zero and precisely estimated, as it should be if the condition, and not the cash, drives the response. The event study below estimates the same gap year by year around each scheme's launch.

7.2 Pre-Trends: The Event Study and the Pre-Cohort Placebo

The common-trends assumption is that the eligibility gap would have followed the same path across scheme regimes absent the transfers. Its direct test is to trace the gap year by year around each launch and check that nothing happens before one. On the same woman-year panel I estimate

$$S_{ist} = \sum_{k \neq -1} \theta_k \left(E_i^1 \times \mathbf{1}\{t - t_s^* = k\}\right) + \delta E_i^1 + \alpha_s + \alpha_t + g(\text{age}_{it}) + \varepsilon_{ist}, \tag{6}$$

where t_s^* is the year a conditional scheme became active in state s, k indexes years since launch, the year before launch ($k = -1$) is the omitted reference, and α_s and α_t are state and calendar-year fixed effects. The pre-launch coefficients ($k < 0$) are the direct test of common trends and the post-launch ones trace take-up. Figure 9 shows the triple difference

[23] In the baseline panel a woman is at risk from her first marriage, so the one-daughter comparison group mixes firstborn-boy mothers with women who have no children yet. Starting the risk window at the first birth instead reduces the one-daughter contrast to 0.4 percentage points and insignificance, while the two-daughter contrast stays at about 4 points (wild-cluster $p = 0.035$). The woman-level first stage always compares firstborn-girl with firstborn-boy mothers, so it is unaffected.

as the two difference-in-differences it is built from. Panel (a) traces the eligible gap around conditional launches. There is no systematic pre-trend, then a gradual rise that peaks three to five years out, consistent with take-up accruing as eligible women reach the ages at which they sterilize. Panel (b) repeats the estimation around unconditional launches, with t_s^* redefined as the first unconditional-scheme year. The gap is flat before launch and does not rise after, drifting if anything slightly negative at long horizons, with confidence bands that include zero throughout. The jump follows the condition, not the transfer. Inference uses the wild-cluster bootstrap, which matters here. The joint pre-launch test has a bootstrap p-value of 0.40 in the conditional panel and 0.19 in the unconditional panel, comfortably failing to reject parallel pre-trends in either, even though the conventional clustered tests spuriously reject because so few states are treated.

A pre-cohort placebo makes the same point at the cohort level. A placebo run on women too old to have been exposed returns a clean null, so eligible and ineligible women in scheme states were not already on different fertility paths before the transfers existed.

7.3 Is Eligibility as Good as Random?

E^1 is the sex of the first birth, so it should be unrelated to anything the woman chooses. Table 7 compares predetermined covariates for firstborn-girl and firstborn-boy women. At a sample of over a million women even trivial gaps are statistically distinguishable, and several differences have stars, so the question is their size. The largest is 0.2 of a year in birth cohort, the marriage- and first-birth-age gaps are about a month, and firstborn-girl women are less than one percentage point less wealthy and less urban, consistent with limited first-birth sex selection among wealthier urban families. All of these covariates enter the regressions as controls. The imbalances are too small to move the estimates, so treating the sex of the first birth as random, conditional on parity, is justified in this sample.

That the early births precede any response to the schemes does not by itself make eligibility as good as random. If couples sex-select the early births, eligibility is chosen rather than given. Sex selection in India, however, tends to intensify at higher parities, among families that still lack a son, and leaves the earliest births closest to chance. The first-birth ratio is about 110 boys per 100 girls against a biological benchmark near 105 (Jayachandran, 2017) (Table 8, Panel A), a modest elevation that is stable over time (Figure 10a) even as sex-selective abortion spread after 1990, and the marginal ratio stays in a narrow band across birth orders (Figure 10b). The targeting appears at the second birth, where the ratio is 106 after a first son against 113 after a first daughter, so son-targeting sharpens once a couple already has a girl. The covariate balance above bounds how much this matters. The

differences it leaves in the woman's predetermined characteristics are trivially small, so the firstborn-girl margin E^1 is close to random, and the two-girl margin E^2 only mildly selected, one reason the estimates rely on E^1.

7.4 The Geographic Confound

The most serious threat to identification is geographic. The conditional schemes are mostly southern and the unconditional ones mostly northern, so the contrast $\pi_1 - \pi_2$ sets a southern eligible-minus-ineligible gap against a northern one, and son preference differs across the two. Figure 11 maps the sex ratio at birth, whose male skew concentrates in the northwest. The firstborn-girl stopping gradient is therefore plausibly steeper in the north for reasons unrelated to any scheme, a difference in the eligibility gradient rather than in levels, which state fixed effects do not remove. I address it with a series of checks, and the results are mixed. The effect survives regional-trend controls and appears within states that ran both scheme types, but single-region designs are underpowered at completed fertility, the north because its conditional schemes had little take-up, the south because its exposed cohorts are mostly still short of completed fertility.

The pooled estimate uses three sources of variation, which the checks isolate in turn, and they behave differently.

- *Cross-state:* comparing the firstborn-girl gradient in conditional-scheme states with the gradient elsewhere. This is where the long-running schemes contribute, and it is the source the geographic confound threatens.
- *Within-state cohort timing:* comparing cohorts a scheme reached with cohorts it did not inside the same state.
- *Within-state conditional versus unconditional:* in the states that ran both kinds of scheme at once.

A timing constraint sets how much of each the two equations can use. The first stage is a woman-year hazard and uses every exposed woman-year, but completed fertility needs women at 40–49, and a cohort is fully exposed to a scheme starting in 2005 only if born from 1980, who do not reach 40 until 2020. Only Tamil Nadu and Rajasthan (1992) and marginally Haryana (2002) have fully-exposed cohorts already at completed fertility. The reduced form's full-dose variation therefore comes mostly from the long-running schemes and the cross-state contrast, while the within-state evidence comes from the first stage. This is not a design choice but a consequence of the schemes being recent and completed fertility requiring old cohorts, and it resolves as future waves observe the exposed cohorts.

7.5 Regional-Trend and Catalogue Robustness

The reduced form is unmoved by absorbing regional trends. Adding region-by-cohort fixed effects, which remove any region-specific cohort trend in the gradient, leaves the conditionality effect at −0.40, from −0.40 pooled (Table 16). It is equally stable across every catalogue judgment call. Folding the low-exposure schemes back in, moving the de-facto-unenforced Karnataka scheme to the unconditional arm, restricting to the no-son schemes, dating Tamil Nadu's treatment from its 2001 restructuring, and dropping Madhya Pradesh all leave the one-daughter estimate between −0.34 and −0.44 (Appendix Table A3).

7.6 Within-State Estimates

The timing constraint shows up in the within-state evidence. Restricting to states that ran a conditional and an unconditional scheme at once, so the contrast is purely within-state, the first stage is +0.09 at the one-daughter margin and +0.19 at the two-daughter margin, both excluding zero. The reduced form is negative and significant (−0.71 and −0.68). A regional difference in norms cannot generate this, since both sides come from the same state-years. The most demanding within-state specification, however, adding fixed effects for eligibility interacted with state and with cohort so that only within-state cohort timing identifies, leaves the first stage significant (+0.04) while the reduced form becomes imprecise (−0.09, standard error 0.08). This is the timing constraint, not a refutation, and lowering the age floor shows it. As the floor drops from 40 to 35, pulling the recent fully-exposed cohorts into the sample, the pooled reduced form stays fixed at about −0.41 while the within-state reduced form strengthens monotonically, from −0.09 (insignificant) at 40–49 to −0.14 (standard error 0.06, significant at the ten percent level) at 35–49 (Appendix Table A12). The within-state fertility effect emerges just as the exposed cohorts arrive. The timing constraint alone hid it at 40–49. The pooled estimate is also a non-convex weighted average of cell-level effects, and a binarized-exposure version that restores convex weights gives a smaller reduced form, two further reasons to rely on the intent-to-treat estimates rather than the raw per-sterilization number.[24]

The design finds no response in the north: restricted to northern states, the first stage is near zero or slightly negative at both margins (Appendix Table A3). The failure is substantive rather than statistical. Son preference is strong in the north, so a firstborn-girl woman there keeps childbearing in the hope of a son rather than sterilize, and the low-take-up northern schemes did not overcome that. The effect is a southern, lower-son-preference

[24] 15 of 31 cell weights are negative in the heterogeneity-robust diagnostic, and the binarized-exposure reduced form is −0.29.

phenomenon, and I do not claim it generalizes northward.

7.7 Mechanisms: Sterilization, Not Substitution or Sex Selection

The transfers raise sterilization specifically, by about 8 percentage points on the eligible margin, while if anything lowering other modern-method use, so they work by moving women into the rewarded method rather than raising contraception in general.

The exclusion restriction would fail if the transfers changed fertility through sex selection rather than sterilization, the channel Anukriti (2018) documents for Devi Rupak. Testing it directly, at the birth level with the sex of marginal births (orders two through four, birth-order and state-by-year fixed effects) as the outcome, the conditional-minus-unconditional coefficient is -0.007 and statistically insignificant. On average, across the schemes I pool, the conditionality works through stopping, not sex selection.

7.8 Composition Rules

A further caveat concerns what the two arms reward, beyond whether they condition on sterilization. Three of the seven conditional schemes, the Girl Child Protection Schemes of Tamil Nadu, Andhra Pradesh, and Telangana, pay only families with one or two girls and no son, while the other conditional schemes allow sons. Most unconditional schemes reward daughters regardless of the family's sons, and most set no cap on total children. A no-son scheme creates a stopping incentive of its own, since a firstborn-girl woman has reason to stop at daughters to stay in the rewarded no-son group even absent any sterilization condition. The conditional-minus-unconditional contrast then partly reflects this difference in rewarded composition rather than the condition alone, which would bias it upward. I address it by re-estimating with the conditional arm restricted to the no-son schemes, and, where feasible, by contrasting a child-cap conditional scheme with the child-cap controls so the composition rule is held fixed. The no-son restriction is among the catalogue checks of Appendix Table A3 and leaves the estimate in the same range.

7.9 Limitations

Two limits remain. In the south, conditional and unconditional schemes overlap only briefly (Andhra Pradesh and Telangana ran Bangaru Talli alongside their Girl Child Protection Schemes in 2013–16, years whose exposed cohorts are still far from completed fertility), so at completed fertility the baseline contrast still spans regions and depends on the cross-scheme comparison made credible by the region-by-cohort trends. And the within-state variation

that avoids the north-south comparison altogether comes from three northern and central states, each with a complication. Haryana ran Devi Rupak (conditional, 2002 through at least 2018) and the Ladli scheme, later Aapki Beti Hamari Beti (unconditional, 2005 on), simultaneously, both rewarding two daughters. The conditionality contrast is therefore available entirely within the state, though at the two-girl composition where Devi Rupak's sex selection runs (Anukriti, 2018). Madhya Pradesh's Ladli Laxmi dropped its sterilization requirement for the first girl in 2008 while keeping it for the second, a conditional-to-unconditional switch in time. Rajasthan ran a conditional scheme through 2000 and an unconditional one from 2016 that never overlap but bracket a long span. The pooled both-scheme estimate reported above draws on these three states.

Single-region designs run into the same timing constraint: Haryana alone is one cluster with reduced-form standard errors near 0.7. The southern bundled design has a clear first stage but a censored reduced form, because outside Tamil Nadu the southern schemes start in 2005 or later and their exposed cohorts are still short of completed fertility (Appendix Table A3). Dating the northern conditional-to-unconditional switches from government orders, and comparing border districts across scheme lines, remain for future work. Figure 12 shows the raw cohort patterns behind the contrast, including the sex composition of births, which moves in parallel across the three scheme groups.

8 From CFR to the TFR Decline

The reduced form and the decomposition are about CFR, the number of children a woman has by the end of her reproductive life. The number policymakers track is the period TFR of Section 4.1. The two coincide only in steady state, and in a transition they can diverge. A shift toward stopping earlier compresses childbearing into younger ages and can pull the period TFR down faster than completed cohort fertility falls, a tempo effect on top of the quantum. Whether a CFR effect maps into the TFR therefore depends on how large that tempo wedge is, which is an empirical question I take up before translating.

Here the wedge is close to zero: the decline is almost entirely in the number of children, not their timing. First, the period TFR and completed cohort fertility fell by nearly the same proportion. The period TFR fell 42 percent, from 3.45 to 2.01; CFR for women observed at ages 45–49 fell 39 percent, from 5.11 children for those interviewed in 1992 to 3.09 for those interviewed in 2019. A large timing distortion would drive a wedge between these two proportional declines, and there is almost none. Second, the shape of the change is the signature of stopping, not postponement. Figure 13 plots the age-specific fertility schedule by survey year. A postponement decline shifts the schedule to the right, lowering fertility at

young ages and raising it at older ones; a stopping decline collapses the older-age tail with no compensating rise. The Indian schedule is the latter. Rates at ages 30 and above fall away while the young-age rates barely move, consistent with a stable age at first birth and a falling age at last birth. The decline being quantum, the paper's CFR results carry over to the headline period TFR, and the translation below is a matter of scaling rather than of tempo adjustment.

A first, mechanical way to ask how much of the 1.44-point decline (Table 17) is sterilization applies the period indices of Section 5 (Table 4) through a single counterfactual in three steps. In 2019 the observed contraception index is $C_c \approx 0.33$, the counterpart of the observed TFR of 2.01. Freezing the prevalence of sterilization at its 1992 level, with the other methods left as observed, raises the index to $C_c \approx 0.49$, because the near-perfect method then covers fewer women. Since the model makes the TFR proportional to C_c, the counterfactual TFR is

$$\mathrm{TFR}^{\mathrm{cf}}_{2019} = \mathrm{TFR}_{2019} \times \frac{C_c^{\mathrm{cf}}}{C_c} = 2.01 \times \frac{0.49}{0.33} \approx 2.98, \tag{7}$$

so without the rise in sterilization the 2019 TFR would be near 2.98 rather than 2.01. That rise accounts for $2.98 - 2.01 \approx 0.97$ of the 1.44 decline, about 67 percent; Table 4's female-sterilization row applies the identical freeze within the model's own accounting, where it is 39 percent of the model's larger decline. This is the demographic accounting answer, and it is an upper bound. It credits every sterilized woman with the full birth-averting power of the method, as if she would otherwise have borne children at the non-sterilized rate, which the next step shows is too strong.

That accounting assumes each sterilization prevents every birth the woman would otherwise have had, which overstates the causal role: much of measured sterilization comes from women who would have limited their fertility anyway. Replacing the assumption with the causal births-per-sterilization requires matching units, and the LATE and the rise in sterilization are both CFR objects, so the denominator must be CFR too. Among women observed at 45–49, the share ever sterilized rose from 0.26 in 1992 to 0.57 in 2019 while CFR fell by 2.02 children, so the causal share is

$$\text{share} = \frac{\text{LATE} \times \Delta\text{Ster}_{45\text{–}49}}{\Delta\text{CFR}_{45\text{–}49}} = \frac{\text{LATE} \times 0.31}{2.02}. \tag{8}$$

The two-stage points (3.5 to 5.0 births per sterilization) would put that share at 54 to 76 percent, the top of which exceeds the 67 percent mechanical ceiling, confirming that the points overstate (Section 6.7). I therefore do not convert the design-based estimates into

a causal share at all. The ratio's confidence boundary is a local complier quantity, not a lower bound on sterilization's contribution to the national decline, and an earlier draft's use of it as a floor was a mistake. Two numbers of different kinds survive. The mechanical calculation credits every sterilization with full birth-averting power and caps the contribution at 67 percent of the CFR decline; it is an accounting exercise and labeled as such. Since the decline is quantum (Figure 13), the same ceiling applies to the headline period TFR. The design-based number is the policy contrast, about 0.2 children per eligible woman against the no-scheme benchmark, local to the covered cohorts of thirteen states. The pairing is consistent with the near-zero composition term of Section 5. Sterilization lowered fertility within groups, the women who sterilize doing so at ever-lower parities, not by reweighting the population toward a sterilized category, which is the only thing a between-group composition term can see.

The gap between the policy-contrast estimates and the mechanical bound is the endogeneity the supply-versus-demand literature has long stressed. A sterilization often ratifies a stopping decision the woman had already made (Pritchett, 1994; Vogl, 2026), just as Chinese births barely responded once quotas were lifted because desires already bound (Fang and Liu, 2026). The residual is the broad fall in desired fertility, visible in the lower childbearing of the never-sterilized too. One caveat remains. The calculation applies the policy-induced margin's effect to the whole rise in sterilization, and the average sterilization, much of it after a woman has completed her family, plausibly averts fewer births than the marginal one.

This share is not the contribution of the cash-transfer schemes themselves. The CCT is an instrument, not the treatment: under the exclusion restriction it has no fertility channel of its own to quantify, and its aggregate reach was in any case small. The broader program, the camps, provider incentives, and the supply of services, drove the rise in sterilization. The girl-child transfers serve here only to identify what a sterilization does, and applying that effect to the actual rise gives the bracket above.

9 Conclusion

India reached replacement fertility through women stopping childbearing earlier, not starting later, and sterilization was the instrument. Fertility fell within every social group rather than through population change, and the sterilized and never-sterilized converged to the same completed family size. Attaching a sterilization condition to a girl-child transfer raises an eligible woman's probability of sterilization by 8 percentage points and lowers her completed fertility by about 0.2 children against uncovered cohorts, 0.4 against the unconditional arm. A mechanical accounting exercise caps sterilization's contribution to the decline at about

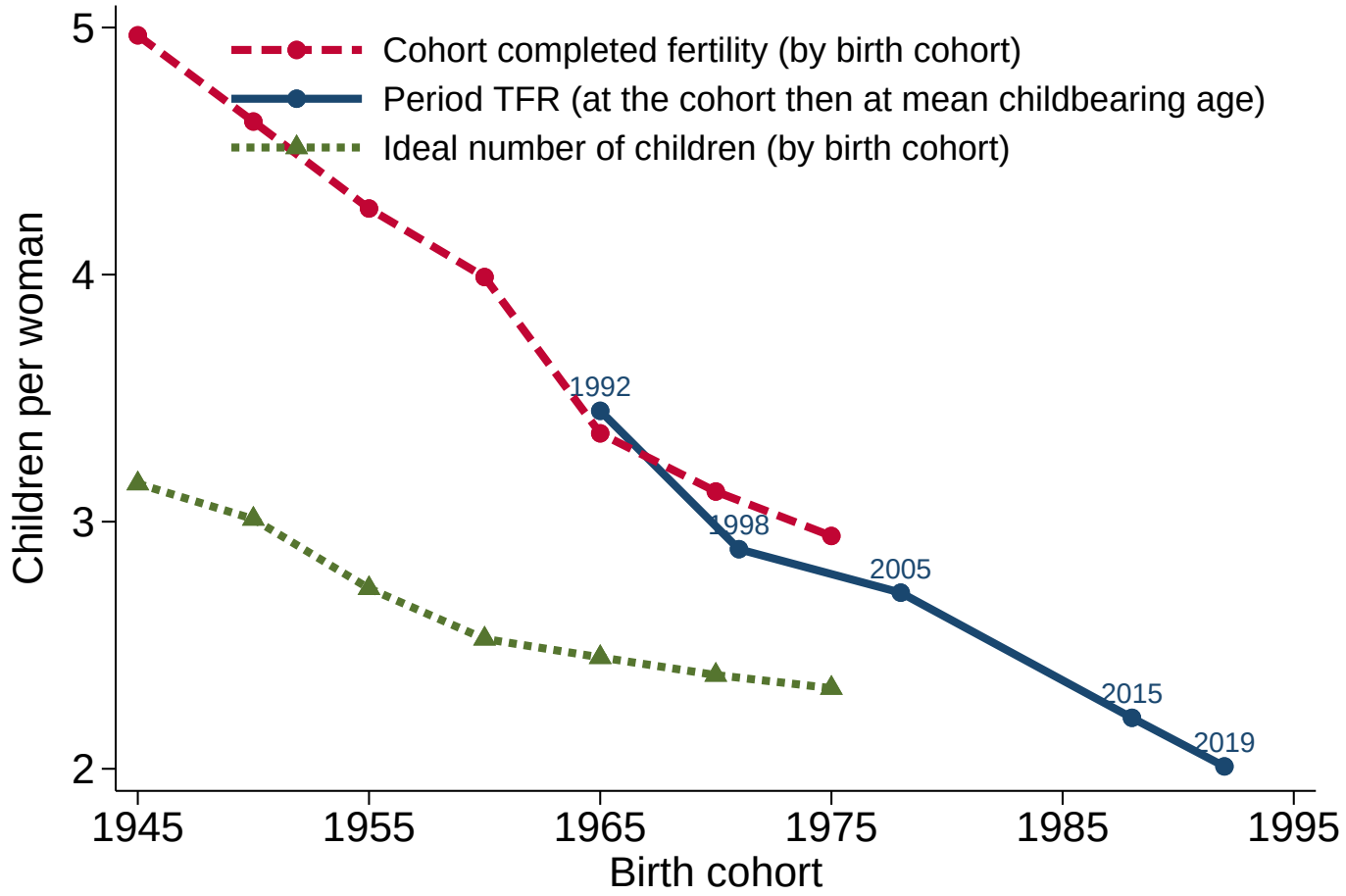
two-thirds. The design-based estimates are local policy contrasts and are not converted into a causal share. Beneath the ceiling is the broad fall in desired family size. The earlier stopping raises women's paid work and improves their nutrition, and it leaves their stated norms, their say in household decisions, and their children's outcomes unchanged.

Three limits bound these answers. The identification comes from the southern scheme states. In the north, son preference blunts the condition, and the estimates do not describe it. The per-sterilization ratio is fragile in the way fuzzy difference-in-differences theory predicts, so I use only the lower ends of its confidence sets. And the within-state completed-fertility evidence is censored by the schemes' recency, a constraint each future survey round relaxes.

The paper's contribution is causal evidence on the world's most used contraceptive method, on what a sterilization condition does to fertility and to the woman who responds, and on how much of a national fertility decline the promoted method can account for. For policy, the operative margin is the condition, not the cash. Unconditional daughter transfers do not raise sterilization, so a state that attaches a certificate is buying stopping, not subsidizing daughters.

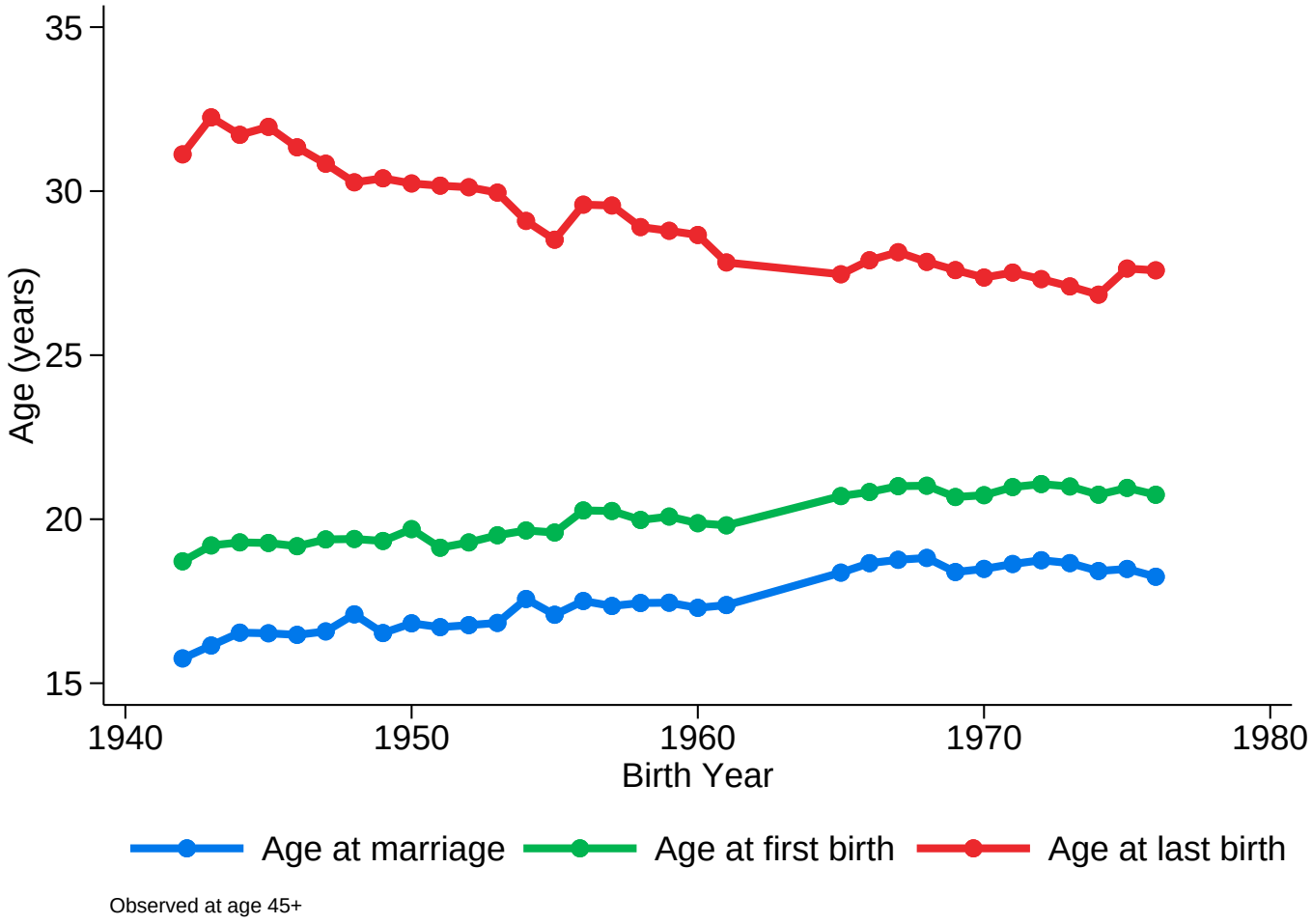
Figures and Tables

Figure 1: Period TFR and Cohort CFR



Cohort CFR and the ideal number of children are plotted by birth cohort (all women observed at ages 40–49). Period TFR is translated to the birth cohort at its mean age of childbearing (about 27 years) in each survey year. Appendix Figure A5a splits the completed and ideal series by sterilization status.

Figure 2: Age at Marriage, First Birth, and Last Birth, by Birth Cohort



Means by birth cohort, restricted to women observed at ages 45–49, so every cohort shown has essentially completed childbearing.

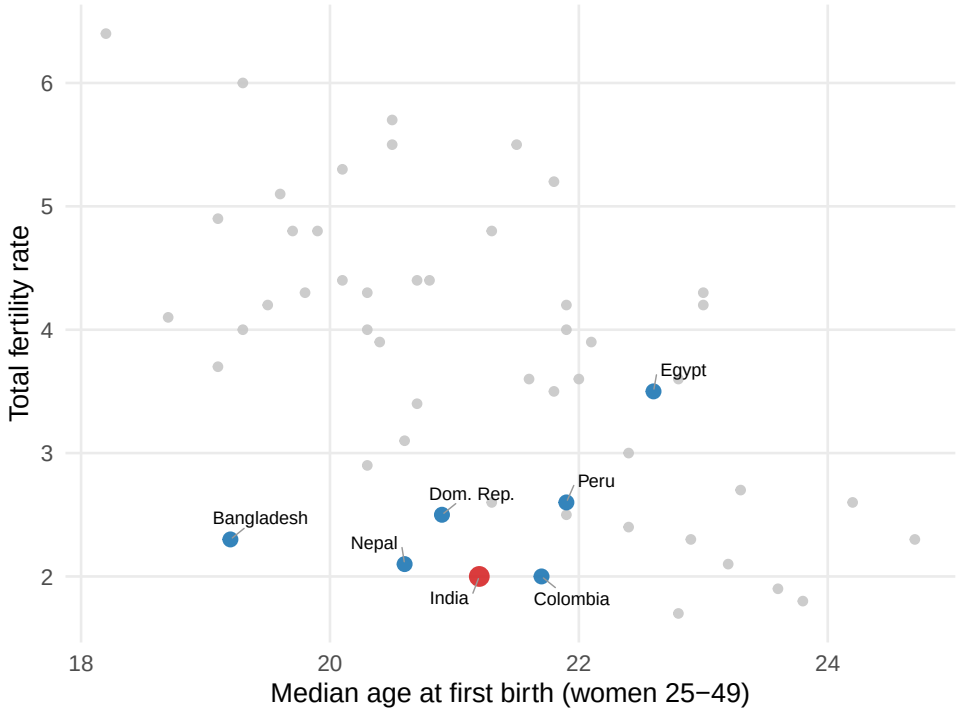
Table 1: Sample Characteristics by Survey Round

	1992	1998	2005	2015	2019
Demographics					
Age	28.4	28.6	29.0	30.0	30.4
Urban	0.243	0.241	0.328	0.346	0.325
Education					
No education	0.624	0.536	0.406	0.275	0.224
Secondary or higher	0.030	0.130	0.120	0.215	0.191
Caste					
SC/ST	0.217	0.280	0.268	0.295	0.318
OBC (1998+)	0.000	0.330	0.396	0.434	0.419
Religion					
Hindu	0.821	0.820	0.806	0.806	0.814
Muslim	0.122	0.129	0.136	0.138	0.135
Wealth					
Top 2 wealth quintiles	0.383	0.371	0.434	0.422	0.410
Fertility and marriage					
Sterilized	0.220	0.268	0.296	0.279	0.290
Children ever born	2.7	2.6	2.3	1.8	1.8
Age at first birth	18.5	18.5	19.2	20.3	20.6
Age at first marriage	16.4	16.6	17.2	18.4	18.6
Women	89,502	90,303	124,385	699,686	724,115

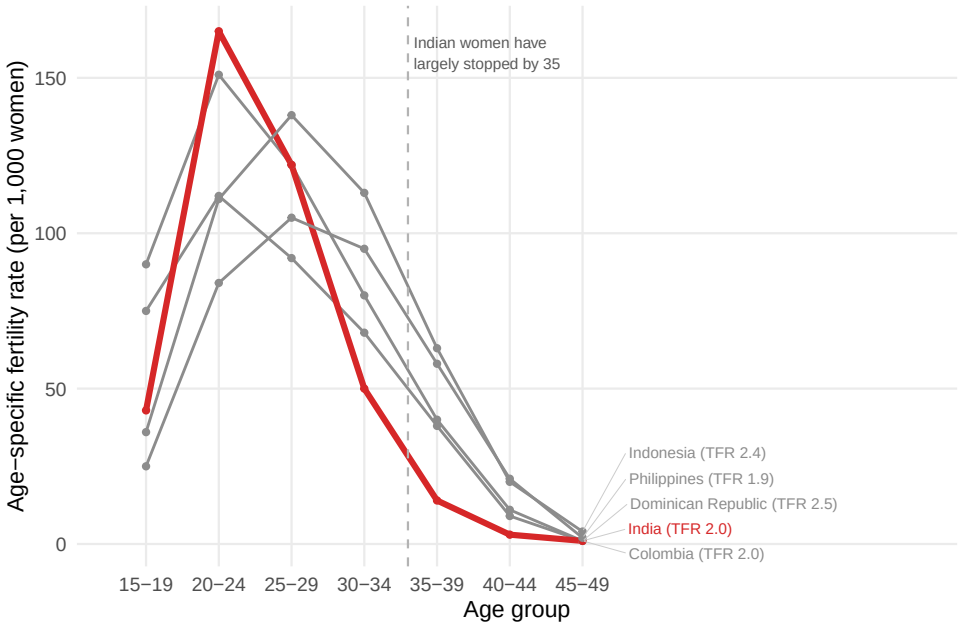
OBC is asked only from 1998 on. CEB is children ever born, and ages are in years. The 1992 and 1998 rounds interviewed ever-married women only, rescaled to all women by the all-women factor, so their estimates for characteristics correlated with marriage timing are approximate.

Figure 3: Timing of Childbearing in India and Comparison Countries

(a) Median Age at First Birth against Total Fertility

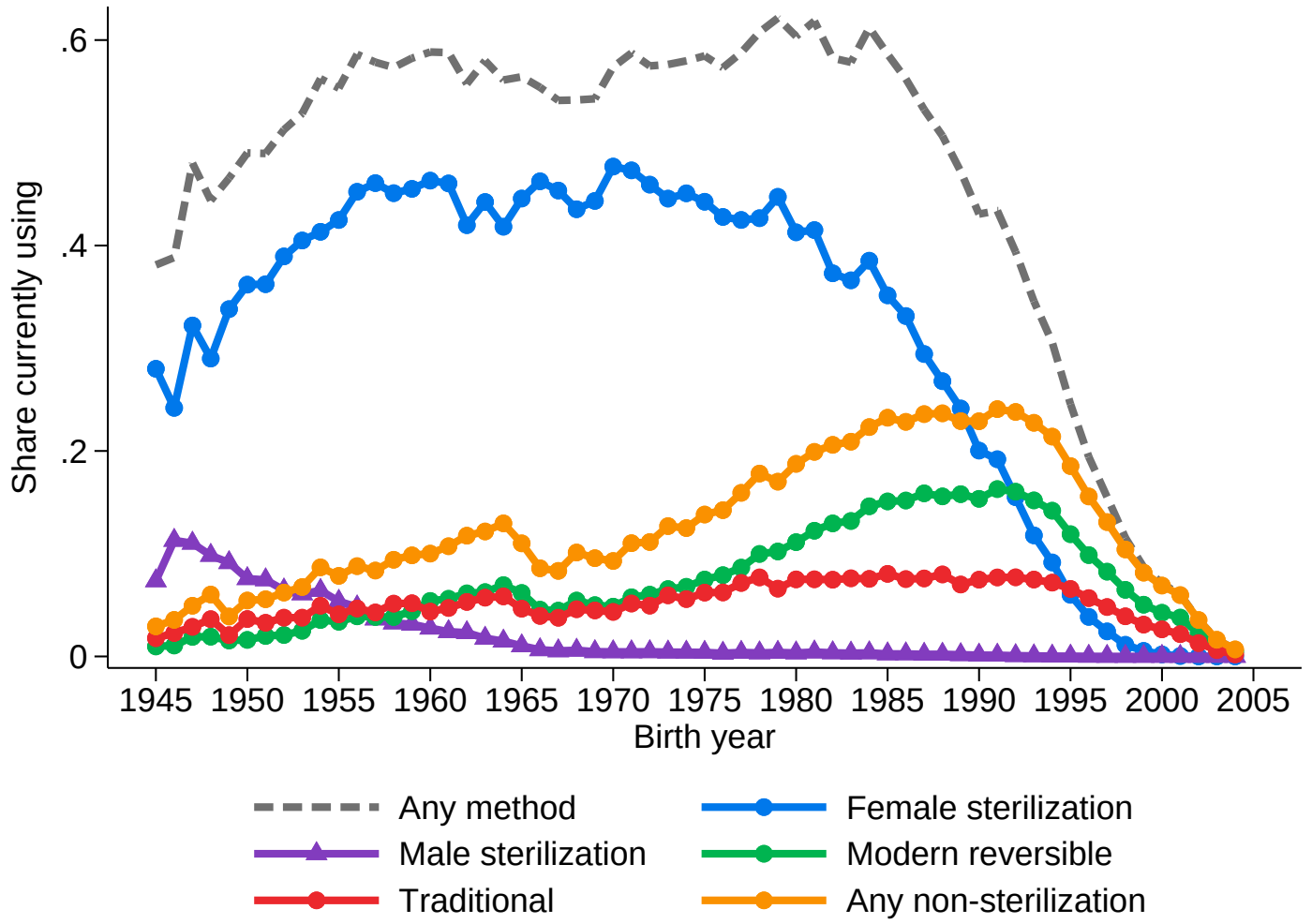


(b) Age-Specific Fertility Schedules



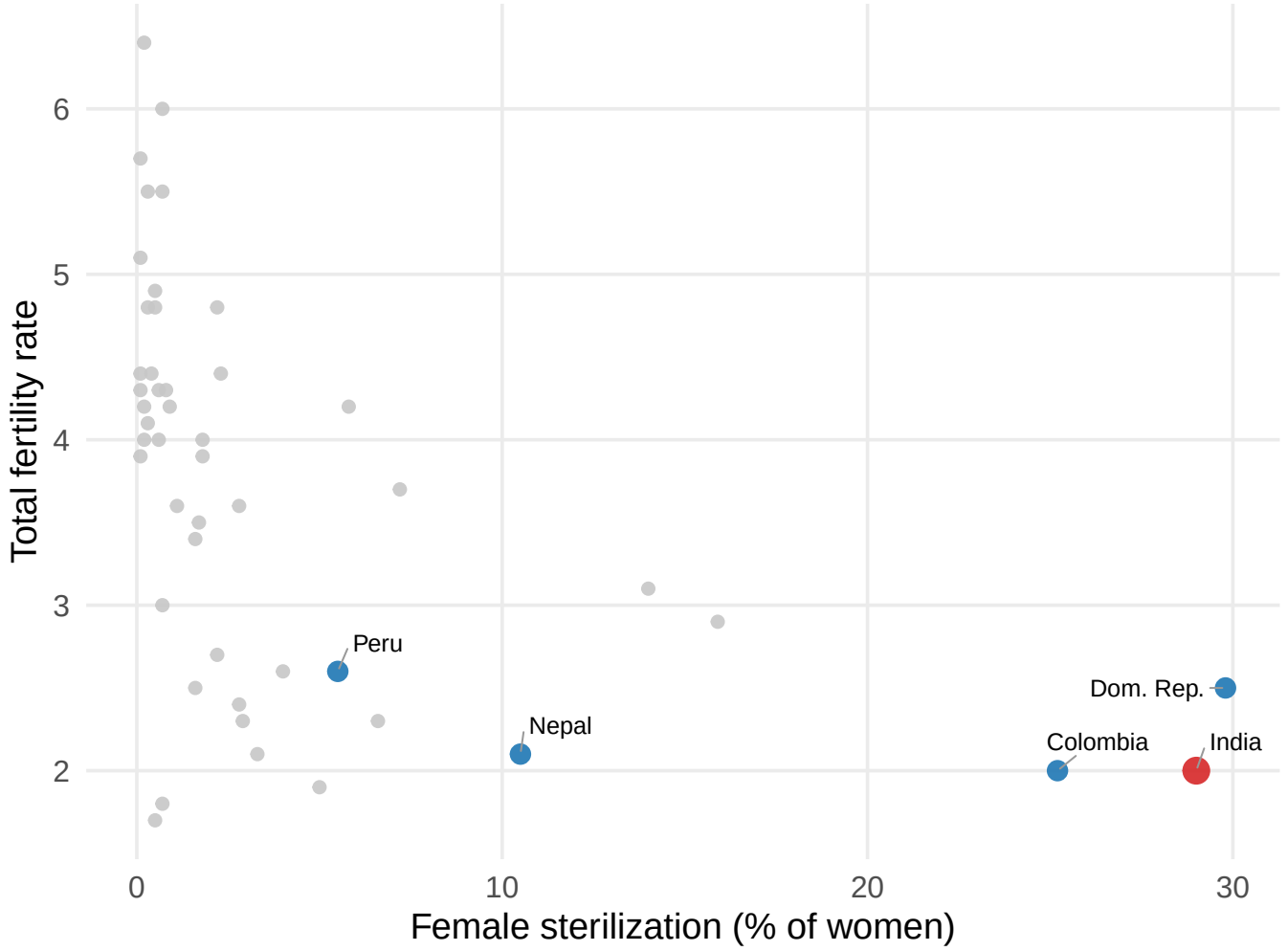
India highlighted. Panel (a): each point is a country, and median age at first birth is for women 25–49. Panel (b): age-specific fertility rates (births per 1,000 women) by five-year age group, India against Colombia, the Dominican Republic, Indonesia, and the Philippines. Source: DHS StatCompiler (, 2012), most recent survey per country (2010 or later).

Figure 4: Contraceptive Method Mix by Birth Cohort



Share of women currently using each method group, by birth year (1945–2004), pooled across survey rounds. The dashed gray line is use of any method. Modern reversible covers pill, IUD, implant, condom, injectable, and other modern methods, while traditional covers abstinence, withdrawal, and other traditional methods. Any non-sterilization is any current method other than sterilization. Each cohort pools all the ages (15–49) at which the survey observes it, so cohorts born after about 1975 are observed mostly at ages before most sterilizations occur.

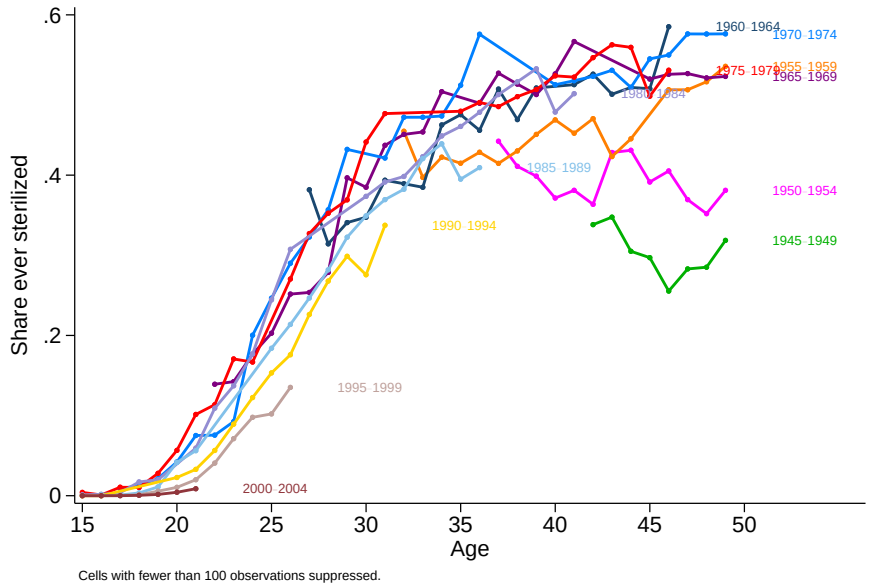
Figure 5: Female Sterilization against Total Fertility across Countries



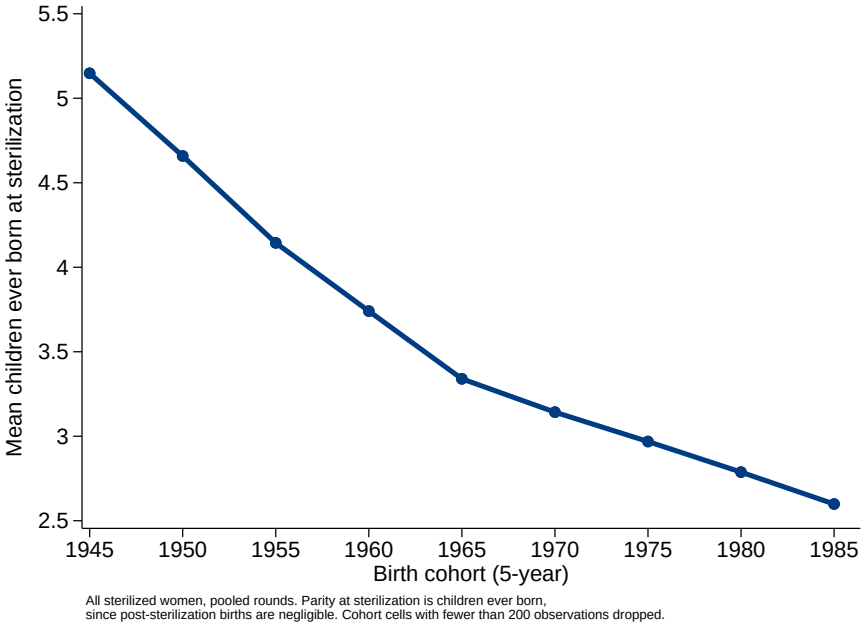
Each point is a country covered by the Demographic and Health Surveys, with India highlighted. Female sterilization is the percent of women currently sterilized. Source: DHS StatCompiler (, 2012), most recent survey per country (2010 or later).

Figure 6: Sterilization by Age and by Parity, across Cohorts

(a) Share Ever Sterilized, by Age and Cohort



(b) Mean Parity at Sterilization, by Cohort



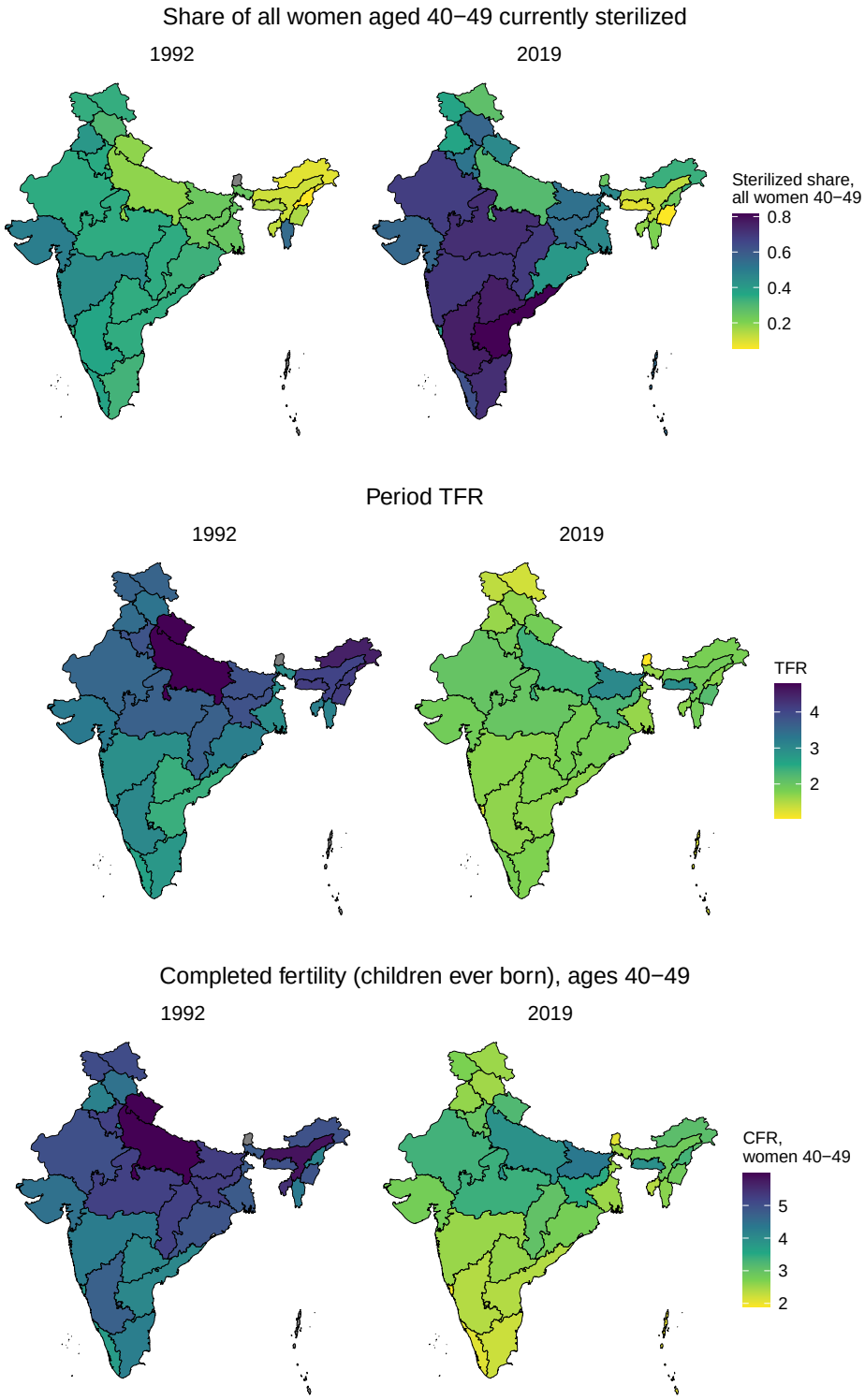
Panel (a): share of women ever sterilized by age, one line per 5-year birth cohort, all women, pooled rounds. Each cohort's line ends at the oldest age the surveys observe it, and cells with fewer than 100 observations are suppressed. Panel (b): mean children ever born at sterilization among sterilized women with exact procedure dates (2005–2019 rounds), by cohort.

Table 2: Sterilized versus Non-Sterilized Women

Characteristic	Sterilized	Not sterilized	Difference
Fertility			
Children ever born	3.3	3.5	-0.2***
Number of sons	1.7	1.6	0.1***
Fraction of children who are sons	0.574	0.537	0.037***
Has at least one son	0.928	0.832	0.096***
Timing			
Age at first birth	20.1	21.3	-1.2***
Age at last birth	26.4	29.1	-2.6***
Age at first marriage	17.9	18.7	-0.9***
Demographics			
Age	44.1	44.1	0.1***
Urban	0.339	0.350	-0.011***
Education			
No education	0.478	0.487	-0.010***
Secondary or higher	0.072	0.115	-0.043***
Caste			
SC/ST	0.291	0.258	0.033***
Religion			
Hindu	0.868	0.774	0.094***
Muslim	0.073	0.166	-0.094***
Wealth			
Top 2 wealth quintiles	0.474	0.435	0.039***
Women	163,991	167,301	

Currently married women aged 40–49, pooled across survey rounds. The difference column pools cohorts (Table 5 gives the by-cohort comparison) and is starred from a z-test on the difference. $^{*}\,p < 0.10$, $^{**}\,p < 0.05$, $^{***}\,p < 0.01$.

Figure 7: Sterilization and Fertility across States, 1992 and 2019



By state, in the 1992 and 2019 rounds. Top row: share of all women aged 40–49 currently sterilized, at completed fertility. Middle row: period TFR from the 36-month birth window. Bottom row: completed fertility, the mean children ever born of women aged 40–49. Each outcome uses a single color scale across the two years. Modern state boundaries throughout. In the 1992 panels the post-2000 successor states are assigned their parent state's value.

Table 3: Decomposition of CFR and Lifetime Sterilization by Social Group

	Group	Share 1945	Share 1970	r_g 1945	r_g 1970	Within	Between
Children ever born							
Education	None	0.70	0.49	5.40	3.68	-1.022	-0.931
	Below secondary	0.27	0.44	4.27	2.72	-0.551	0.580
	Secondary+	0.03	0.07	2.61	1.91	-0.036	0.088
	Total					**-1.608 (86%)**	**-0.263 (14%)**
Religion	Hindu	0.83	0.82	4.94	3.06	-1.552	-0.022
	Muslim	0.10	0.12	6.04	3.85	-0.237	0.084
	Christian	0.03	0.03	4.12	2.68	-0.044	-0.020
	Other	0.04	0.03	4.49	2.71	-0.060	-0.020
	Total					**-1.894 (101%)**	**0.022 (-1%)**
Caste (SC/ST)	Non-SC/ST	0.78	0.70	4.89	3.02	-1.386	-0.335
	SC/ST	0.22	0.30	5.43	3.39	-0.524	0.373
	Total					**-1.910 (102%)**	**0.039 (-2%)**
Urban/rural	Rural	0.72	0.66	5.27	3.35	-1.324	-0.265
	Urban	0.28	0.34	4.31	2.71	-0.499	0.217
	Total					**-1.823 (97%)**	**-0.049 (3%)**
Wealth quintile	Q1 poorest	0.17	0.16	5.84	3.97	-0.311	-0.024
	Q2	0.18	0.19	5.40	3.50	-0.348	0.035
	Q3	0.20	0.21	5.18	3.09	-0.426	0.068
	Q4	0.22	0.21	4.97	2.86	-0.458	-0.018
	Q5 richest	0.24	0.22	3.99	2.50	-0.341	-0.048
	Total					**-1.885 (101%)**	**0.013 (-1%)**
Ever sterilized							
Education	None	0.70	0.49	0.26	0.57	0.183	-0.085
	Below secondary	0.27	0.44	0.35	0.58	0.079	0.077
	Secondary+	0.03	0.07	0.33	0.43	0.005	0.015
	Total					**0.267 (97%)**	**0.007 (3%)**
Religion	Hindu	0.83	0.82	0.30	0.59	0.239	-0.002
	Muslim	0.10	0.12	0.16	0.36	0.022	0.004
	Christian	0.03	0.03	0.28	0.56	0.009	-0.002
	Other	0.04	0.03	0.33	0.54	0.007	-0.002
	Total					**0.277 (101%)**	**-0.003 (-1%)**
Caste (SC/ST)	Non-SC/ST	0.78	0.70	0.29	0.55	0.193	-0.036
	SC/ST	0.22	0.30	0.27	0.58	0.081	0.036
	Total					**0.274 (100%)**	**0.000 (0%)**
Urban/rural	Rural	0.72	0.66	0.28	0.57	0.197	-0.026
	Urban	0.28	0.34	0.30	0.55	0.077	0.026
	Total					**0.275 (100%)**	**-0.000 (-0%)**
Wealth quintile	Q1 poorest	0.17	0.16	0.19	0.45	0.043	-0.002
	Q2	0.18	0.19	0.22	0.54	0.060	0.003
	Q3	0.20	0.21	0.31	0.61	0.062	0.008
	Q4	0.22	0.21	0.33	0.62	0.062	-0.002
	Q5 richest	0.24	0.22	0.34	0.55	0.048	-0.007
	Total					**0.274 (100%)**	**0.000 (0%)**

Change between the 1945 and 1970 birth cohorts, women observed at ages 45–49. The Share columns give the group's population share in each cohort and the r_g columns its own mean outcome. Within is $\bar{w}_g \, \Delta r_g$ (midpoint-weighted) and Between is $\bar{r}_g \, \Delta w_g$. Each characteristic's Total row sums its groups' terms, also stated as shares of the aggregate change ΔR.

Table 4: Decomposition of the TFR Decline into Proximate Determinants

	1992	2019	Counterfactual 2019 TFR	% of decline
Model-based TFR ($\text{TF} \cdot C_m C_c C_i C_a$)	5.10	2.15		100
Marriage C_m	0.74	0.66	2.4	14
Contraception $C_c = 1 - 1.08\,ue$	0.64	0.33	4.2	76
any method, $ue = \sum_m u_m e_m$	0.34	0.62		
female sterilization $u_m e_m$	0.23	0.38	3.2	39
male sterilization	0.03	0.00	2.0	-7
modern reversible	0.05	0.16	2.9	29
traditional & withdrawal	0.03	0.08	2.5	15
Infecundability C_i	0.72	0.68	2.3	7
Abortion C_a	0.97	0.95	2.2	3

Currently married women, pooled rounds. The anchor row is the model's TFR, total fecundity TF = 15.3 scaled down by the four indices. Method rows report $u_m e_m$, the share of women using the method times its use-effectiveness. They sum to the "any method" row, and $C_c = 1 - 1.08\,ue$. The married proportions inside C_m for 1992 and 1998 come from the surveys' published household-roster tables (text footnote). C_a is indicative only, since the DHS underreports induced abortion. "Counterfactual 2019 TFR": the model's 2019 rate with the row's behavior held at its 1992 level and everything else at its observed 2019 value, so $\text{TFR}^{\text{cf}} = 2.15 \times C_x^{1992}/C_x^{2019}$. For a single method, C_c is recomputed with that method's $u_m e_m$ at 1992. "% of decline": the behavior's share of the model's log decline, $100\,\Delta \ln C_x / \Delta \ln \text{TFR}^{\text{model}}$, which is exactly additive, so the four index rows sum to 100 and the method rows allocate the contraception row. The counterfactual TFRs themselves cannot be summed.

Table 5: Decomposition of CFR by Sterilization Status

	1945 cohort	1970 cohort	Change	Average share
Completed fertility, all women	5.00	3.13	-1.87	
never sterilized	4.92	3.09	-1.82	0.58
ever sterilized	5.22	3.16	-2.06	0.42
Share ever sterilized	0.29	0.56	0.27	
Contribution to the 1.87-child decline (average share × own change)				
never-sterilized women's own decline			1.05 (56%)	
sterilized women's own decline			0.87 (47%)	
shift of women into sterilization			-0.05 (-3%)	

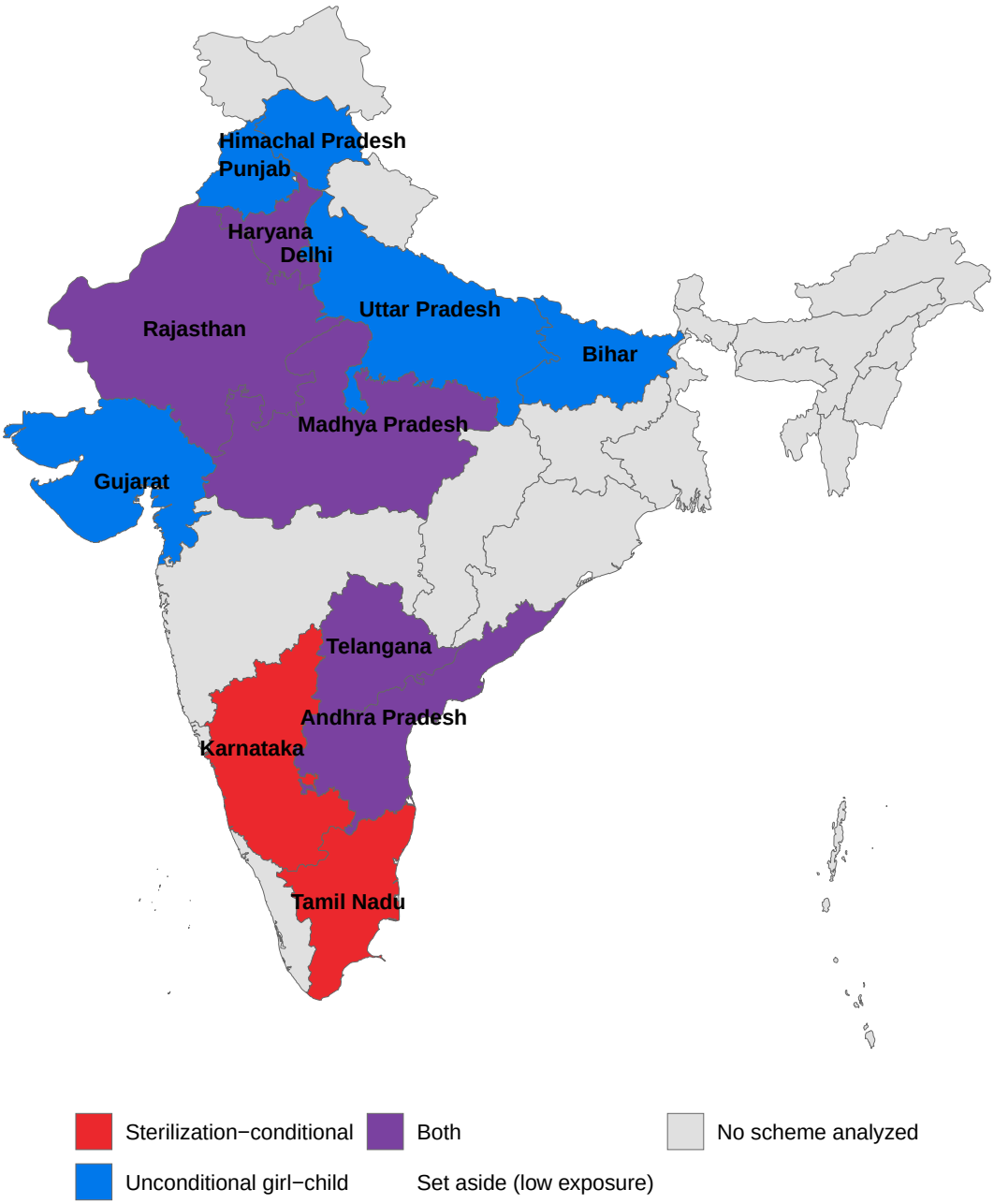
Women observed at ages 45–49, 1945 versus 1970 birth cohorts. Sterilization is current status at 45–49 (the method is terminal, so this approximates ever-sterilized). The bottom panel decomposes the decline with the same midpoint weighting as Table 3. Each group's contribution is its Change entry times its Average-share entry, and the composition term is the change in the sterilized share times the average CFR gap between the groups.

Table 6: State Cash-Transfer Schemes for Girl Children, 1992–2019

Scheme	State	Years	Max children	Sex mix	Transfer (Rs)	
					One daughter	Two daughters
A. Sterilization-conditional CCTs (analyzed: the conditional arm)						
Rajalakshmi	Rajasthan	1992–2000	2	Sons allowed	21,000	–
Girl Child Protection Scheme	Tamil Nadu	1992–	2	Daughters only	80,000	80,000
Devi Rupak	Haryana	2002–	2	One child of either sex, or two daughters	120,000	48,000
Girl Child Protection Scheme	Telangana	2005–	2	Daughters only	100,000	60,000
Girl Child Protection Scheme	Andhra Pradesh	2005–	2	Daughters only	100,000	60,000
Bhagyalakshmi	Karnataka	2006–	3	Sons allowed	100,097	200,149
Ladli Laxmi Yojana	Madhya Pradesh	2007–2008	2	Sons allowed	118,300	236,600
B. Unconditional girl-child CCTs (analyzed: the income comparison)						
Apni Beti Apna Dhan	Haryana	1994–1998	3	Sons allowed	25,000	50,000
Balika Samridhi Yojana	Himachal Pradesh	1997–2010	None	Sons allowed	6,700	13,400
Balika Samridhi Yojana	Gujarat	1997–	None	Sons allowed	6,700	13,400
Ladli Scheme	Haryana	2005–2015	None	Sons allowed	0	96,000
Ladli Laxmi Yojana (unconditional)	Madhya Pradesh	2008–	2	Sons allowed	118,300	236,600
Ladli Scheme	Delhi	2008–	None	Sons allowed	100,000	200,000
Mukhyamantri Kanya Suraksha Yojana	Bihar	2008–	None	Sons allowed	18,000	36,000
Beti Hai Anmol	Himachal Pradesh	2010–	None	Sons allowed	5,100	10,200
Bebe Nanki Laadli Beti Kalyan Scheme	Punjab	2011–2015	None	Sons allowed	61,000	122,000
Bangaru Talli	Andhra Pradesh	2013–2016	None	Sons allowed	216,000	432,000
Bangaru Talli	Telangana	2014–2016	None	Sons allowed	216,000	432,000
Aapki Beti Hamari Beti	Haryana	2015–	None	Sons allowed	0	21,000
Mukhyamantri Rajshri Yojana	Rajasthan	2016–	None	Sons allowed	50,000	100,000
Vahali Dikri Yojana	Gujarat	2019–	None	Sons allowed	110,000	–
Mukhyamantri Kanya Sumangala Yojana	Uttar Pradesh	2019–	2	Sons allowed	15,000	30,000
C. Relevant schemes set aside (negligible exposure)						
Balri Rakshak Yojana	Punjab	2005–2013	Negligible uptake (≈306 beneficiaries by 2010) (Sekher, 2010)			
Indira Gandhi Balika Suraksha Yojana	Himachal Pradesh	2007–	Negligible uptake (≈700 beneficiaries) (Sekher, 2010)			
Dhan Lakshmi	Punjab	2008–2013	Operated in effectively one district (Sirhind, Punjab) (Biswas, Cornwell and Zimmermann, 2025)			

Every scheme pays for at most two daughters per family, except that Aapki Beti Hamari Beti adds a third girl from August 2015 and Bebe Nanki pays for every girl, so the transfer columns stop at two daughters. Max children is the largest family size an eligible family may have, counting all children of either sex born by the time the qualifying daughter arrives, and None means the scheme sets no limit. The transfers pay for daughters throughout, except that Devi Rupak's one-child tier also pays for a single son. Sons allowed

Figure 8: States Operating Sterilization-Conditional and Unconditional Girl-Child CCTs



A state is "Both" if it ran at least one of each analyzed type over the sample period, and a state whose only relevant scheme had negligible take-up would be shown as set aside. Marriage and pension schemes are not childbearing incentives and are not shown.

Table 7: Balance of Predetermined Covariates by Sex of the First Birth

Predetermined covariate	Firstborn girl	Firstborn boy	Difference
Education			
Secondary or higher	0.129	0.129	-0.000
Wealth			
Top 2 wealth quintiles	0.401	0.410	-0.009***
Demographics			
Urban	0.312	0.316	-0.004***
Birth cohort	1977.6	1977.4	0.2***
Caste			
SC/ST	0.295	0.294	0.002
Religion			
Hindu	0.818	0.816	0.002*
Muslim	0.129	0.130	-0.002**
Marriage and first birth			
Age at first marriage	18.0	17.9	0.0***
Age at first birth	20.1	20.1	0.1***

Mothers (women with at least one observed birth), pooled across survey rounds. The difference column is starred from a z-test on the difference. $^{*}\,p < 0.10$, $^{**}\,p < 0.05$, $^{***}\,p < 0.01$.

Table 8: Sex of Births at Low Parity

	Fraction male	Boys per 100 girls
Panel A. By birth order (all women)		
Birth 1	0.525	110.5
Birth 2	0.522	109.2
Birth 3	0.527	111.5
Birth 4	0.523	109.5
Birth 5	0.522	109.4
Panel B. Second birth, by sex of the first		
After a first boy	0.515	106.0
After a first girl	0.530	112.8

Births in the pooled birth panel. The natural fraction male at birth is about 0.512 (105 boys per 100 girls). Panel B is the sex of the second birth, split by whether the first birth was a girl.

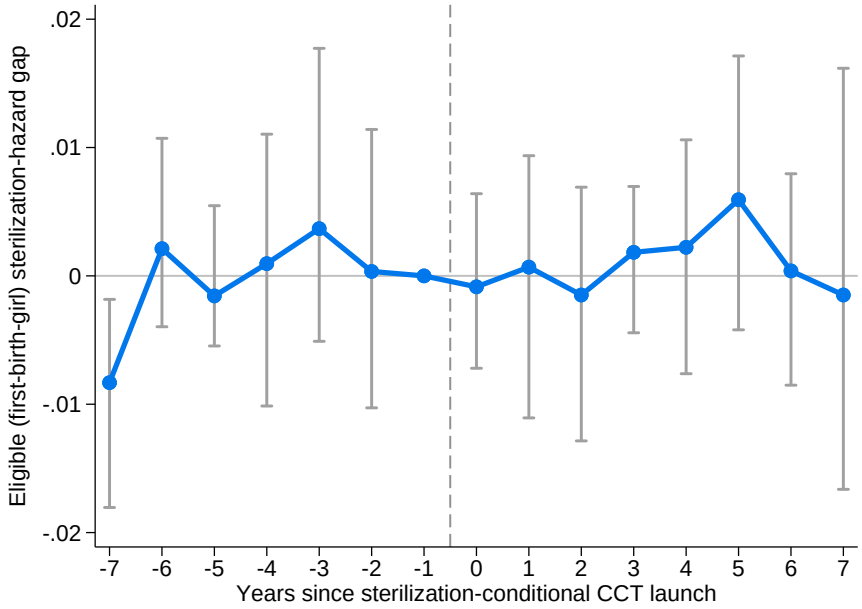
Table 9: Sensitivity of the Netted Contrast to Transfer Value

	(1) FS raw	(2) FS PV	(3) FS PV-receipt	(4) RF PV	(5) RF PV-receipt
Conditional scheme active	0.0161***	0.0177***	0.0155***		
	(0.0038)	(0.0056)	(0.0045)		
Unconditional scheme active	-0.0033*				
	(0.0019)				
Transfer PV (2015 Rs lakh, offer)		-0.0021			
		(0.0056)			
Transfer PV (receipt-scaled)			0.0050		
			(0.0079)		
Conditional exposure				-0.4000**	-0.2103**
				(0.1711)	(0.0689)
PV exposure (offer)				0.1889	
				(0.1639)	
PV exposure (receipt)					-0.0329
					(0.4812)
Obs	15,796,037	15,796,037	15,796,037	197,450	197,450

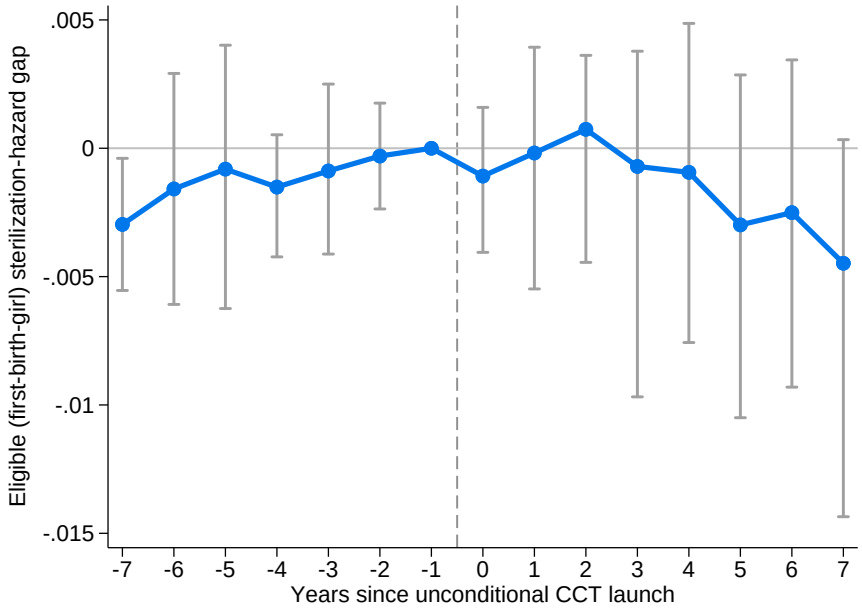
Columns 1–3 are first stages on the woman-year sterilization indicator for firstborn-girl-eligible women, with state-by-year fixed effects. Column 1 uses scheme indicators (conditional, unconditional), while columns 2 and 3 replace the unconditional indicator with the transfer's real discounted value (2015 rupees, as offered and scaled by claim rates), so the transfer-value slope captures the common per-rupee income response and the conditional indicator is the discrete jump from the sterilization requirement. Payouts differ by daughter composition, so the values are the one-girl-tier family totals. Columns 4 and 5 are the corresponding reduced forms on completed fertility (women 40–49, with state, cohort, and covariate fixed effects). Transfer values are in lakhs of 2015 rupees. Standard errors, clustered by state, in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Figure 9: Event Studies around Conditional and Unconditional CCT Launches

(a) Around Sterilization-Conditional CCT Launches



(b) Around Unconditional CCT Launches



Coefficients θ_k from equation (6): the gap in the annual sterilization hazard between firstborn-girl and firstborn-boy women, with state and calendar-year fixed effects, plotted by event time k, years relative to the year before launch ($k = -1$). Panel (a) is estimated in states that ever ran a sterilization-conditional scheme, with launch the first conditional-scheme year, and Panel (b) repeats the same specification in states that ever ran an unconditional scheme, with launch the first unconditional-scheme year. A positive coefficient means eligible women were sterilizing more than ineligible women in that year, beyond their pre-launch gap, and the triple difference is the contrast between the two panels. Woman-year hazard panel. Bands are 95 percent wild-cluster bootstrap confidence intervals (Webb weights), clustered by state.

Table 10: Effect of the Sterilization Condition on Sterilization and Completed Fertility

	Firstborn a girl (E^1)
Panel A. Ever sterilized by ages 40–49	
Eligible × conditional exposure (ϕ_1)	0.037*** (0.012)
Eligible × unconditional exposure (ϕ_2)	-0.044*** (0.013)
Sterilization condition, net of income ($\phi_1 - \phi_2$)	0.082** (0.016)
First-stage F (excluded instrument)	26.4
Panel B. Children ever born	
Eligible × conditional exposure (π_1)	-0.182*** (0.029)
Eligible × unconditional exposure (π_2)	0.222*** (0.064)
Sterilization condition, net of income ($\pi_1 - \pi_2$)	-0.404*** (0.067)
Women	197,450

Both panels estimate the same regression, equations (1) and (2), with only the outcome changing. Panel A uses an indicator for being ever sterilized by ages 40–49, and Panel B children ever born, which is complete at these ages. Sample: mothers (women with at least one birth) aged 40–49 in the thirteen ever-scheme states. The omitted eligibility category is a firstborn boy. Exposure is the share of ages 25–39 with a conditional (or unconditional) transfer active in the woman's state. State, cohort, and covariate fixed effects, with standard errors clustered by state (13 clusters) in parentheses. Stars on the $\phi_1 - \phi_2$ rows are from wild-cluster bootstrap p-values (Webb weights), the paper's inference standard for the first stage, and all other rows use $t(12)$. $^{*}\,p < 0.10$, $^{**}\,p < 0.05$, $^{***}\,p < 0.01$.

Table 11: The Policy Contrast within Adopting States

	(1) Ster. 40–49	(2) CEB 40–49	(3) Ster. +U	(4) CEB +U	(5) Ster. 35–49	(6) CEB 35–49
$E^1\times$ Conditional dose	0.0412**	-0.2389**	0.0389***	-0.2298**	0.0369*	-0.2665**
	(0.0146)	(0.0744)	(0.0089)	(0.0720)	(0.0171)	(0.0770)
$E^1\times$ Unconditional dose (control)			-0.0915***	0.3639**		
			(0.0218)	(0.1118)		
Wild-cluster p (Webb)	0.240	0.140	0.019	0.055	0.198	0.065
Women	104,466	104,466	104,466	104,466	167,801	167,801

Equation (3) (sterilization columns: the same equation with the ever-sterilized indicator as the outcome) estimated only in the seven states that ever ran a sterilization-conditional scheme (Tamil Nadu, Rajasthan, Haryana, Andhra Pradesh, Telangana, Karnataka, Madhya Pradesh). The comparison is eligible (firstborn-girl) against ineligible (firstborn-boy) mothers, across cohorts a conditional scheme covered against cohorts it did not, with no unconditional arm and no never-scheme states in the sample. Columns 3–4 control the unconditional-transfer dose (Haryana and Madhya Pradesh also ran unconditional schemes), and columns 5–6 lower the age floor to 35 to admit recently covered cohorts. The sample is mothers, with state and cohort fixed effects and the full controls. Standard errors are clustered by state (7 clusters), in parentheses, with wild-cluster bootstrap p-values (Webb weights) below. $^{*}\,p < 0.10$, $^{**}\,p < 0.05$, $^{***}\,p < 0.01$.

Table 12: Births per Induced Sterilization

	Firstborn a girl (E^1)
Births per induced sterilization (2SLS)	-4.95*** (0.83)
Anderson–Rubin 95% set	[-7.75, -3.35]
First-stage F	26.4
Women	197,450

Two-stage least squares on the sample of Table 10, instrumenting the ever-sterilized indicator with the conditional-exposure interaction while controlling the any-scheme interaction, so the estimate equals the ratio of Panel B to Panel A of Table 10, $(\pi_1 - \pi_2)/(\phi_1 - \phi_2)$. The Anderson–Rubin sets invert the weak-instrument-robust test. State, cohort, and covariate fixed effects. Standard errors, clustered by state, in parentheses, and stars from t-statistics with 12 degrees of freedom ($^{***}\,p < 0.01$).

Table 13: The Conditionality's Effect on Fertility Timing and Parity Progression

Outcome	Package (π_1)	Cond. − uncond. ($\pi_1 - \pi_2$)	Per sterilization (2SLS)	Baseline mean	Women
Age at last birth	-0.176* (0.087)	-0.845*** (0.169)	-10.527*** (1.250)	27.56	183,293
Progresses to 3rd birth (given 2)	0.029 (0.018)	-0.077** (0.027)	-0.807*** (0.185)	0.72	185,503
Progresses to 4th birth (given 3)	0.008 (0.017)	-0.078*** (0.024)	-0.883*** (0.260)	0.64	135,821
Progresses to 5th birth (given 4)	-0.034** (0.015)	-0.078*** (0.023)	-1.269** (0.529)	0.61	86,409

Estimates of equation (2) with each row's outcome in place of completed fertility, at the one-daughter margin (E^1), on the main estimation sample (mothers aged 40–49 in the thirteen ever-scheme states, as in Table 10). The parity-progression rows condition on reaching the relevant parity. "Package" is the conditional-scheme effect with its payment (π_1), "Cond. − uncond." the netted difference $\pi_1 - \pi_2$, and "Per sterilization" the 2SLS with the netted instrument, as for completed fertility. State, cohort, and covariate fixed effects. Standard errors, clustered by state, in parentheses. $^{*}\,p < 0.10$, $^{**}\,p < 0.05$, $^{***}\,p < 0.01$ (2SLS stars from $t(12)$).

Table 14: Effect of the Sterilization Condition on Outcomes beyond Fertility (Mothers 30–49)

Outcome	Package (π_1)	Cond. − uncond. ($\pi_1 - \pi_2$)	Per sterilization (2SLS)	Baseline mean	Women
Economic empowerment					
Summary index (SD units)	0.025** (0.010)	0.061* (0.030)	0.582* (0.298)	-0.02	159,084
Works for pay	0.019* (0.010)	0.042** (0.017)	0.405** (0.183)	0.37	251,658
Works: white-collar job	0.005* (0.003)	0.008* (0.005)	0.077 (0.045)	0.04	205,862
Works: agriculture	-0.002 (0.007)	0.012 (0.011)	0.115 (0.108)	0.24	205,862
Works: manual labor	0.012** (0.004)	0.013* (0.007)	0.123* (0.067)	0.08	205,862
Owns money she controls	-0.002 (0.010)	0.018 (0.019)	0.173 (0.178)	0.51	159,171
Health (biomarkers, 2015–19)					
Body mass index	0.047 (0.037)	-0.002 (0.065)	-0.023 (0.676)	23.21	730,780
Anemic	-0.014** (0.006)	-0.027*** (0.009)	-0.279*** (0.086)	0.55	677,700
Hemoglobin (g/dL)	0.058*** (0.018)	0.106*** (0.021)	1.109*** (0.246)	11.61	677,707
Reports high blood pressure	-0.001 (0.004)	-0.001 (0.008)	-0.009 (0.073)	0.08	342,392
Reports diabetes	0.002 (0.002)	0.001 (0.003)	0.007 (0.026)	0.03	692,835
Decision-making autonomy (any say)					
Summary index (SD units)	-0.011 (0.009)	0.007 (0.017)	0.079 (0.181)	0.00	197,319
Any say: her own health care	-0.004 (0.008)	0.006 (0.011)	0.060 (0.118)	0.72	197,336
Any say: large purchases	-0.009 (0.006)	-0.007 (0.012)	-0.080 (0.126)	0.70	197,335
Any say: contraceptive use	0.006 (0.005)	-0.001 (0.009)	-0.011 (0.097)	0.92	218,696
Experienced domestic violence (DV subsample, 2005–19)					
Ever experienced physical violence	-0.005 (0.015)	0.005 (0.018)	0.042 (0.152)	0.33	119,252
Ever experienced emotional violence	-0.032*** (0.008)	-0.021** (0.008)	-0.184*** (0.059)	0.15	119,249
Ever experienced sexual violence	-0.013*** (0.003)	-0.004 (0.005)	-0.031 (0.048)	0.07	119,248
Gender-role attitudes					
Rejects wife-beating (index, SD units)	0.008 (0.017)	0.002 (0.025)	0.022 (0.242)	-0.02	206,580
Children's survival and spacing					
Any of her children has died	-0.005 (0.004)	-0.007 (0.005)	-0.068 (0.054)	0.19	796,160
Share of her children who died	-0.001 (0.001)	-0.000 (0.002)	-0.000 (0.017)	0.06	796,160
Birth spacing, post-scheme births (mo.)	1.406** (0.624)	0.249 (0.510)	2.275 (4.796)	38.94	153,923
Mechanically tied to the fertility effect					
Fraction of children who are sons	-0.109*** (0.033)	-0.138*** (0.043)	-1.444** (0.529)	0.55	793,308

Estimates of equation (2) with each outcome in place of completed fertility, on all-India mothers aged 30–49 with never-scheme states as zero-exposure cells, and each row is a separate regression. "Package" is the coefficient on firstborn-girl eligibility interacted with conditional exposure alone, the effect of the whole conditional-scheme package (payment included) on the outcome. "Cond. − uncond." is the difference-in-differences $\pi_1 - \pi_2$, the package effect minus the same quantity under an unconditional girl-child transfer. "Per sterilization" instruments the woman's sterilization with the netted interaction, as for fertility (first stage 0.10 in this sample, discussed in the text). Standard errors (clustered by state) in parentheses under each estimate, and the baseline mean is the sample mean. The violence measures refer to violence by the woman's husband or partner, from the DHS domestic-violence module, asked of one randomly selected woman per household from 2005 onward. Each summary index averages standardized components and is restricted to the components asked consistently across waves, so its composition is fixed. The indices and the wife-beating measure are in standard-deviation units, the mortality and son shares are fractions, birth spacing is in months, body mass index and hemoglobin are in natural units (kg/m^2, g/dL) and come from the biomarker subsample of the later rounds, and the rest are indicators. State, cohort, and covariate fixed effects. $^{*}\,p < 0.10$, $^{**}\,p < 0.05$, $^{***}\,p < 0.01$.

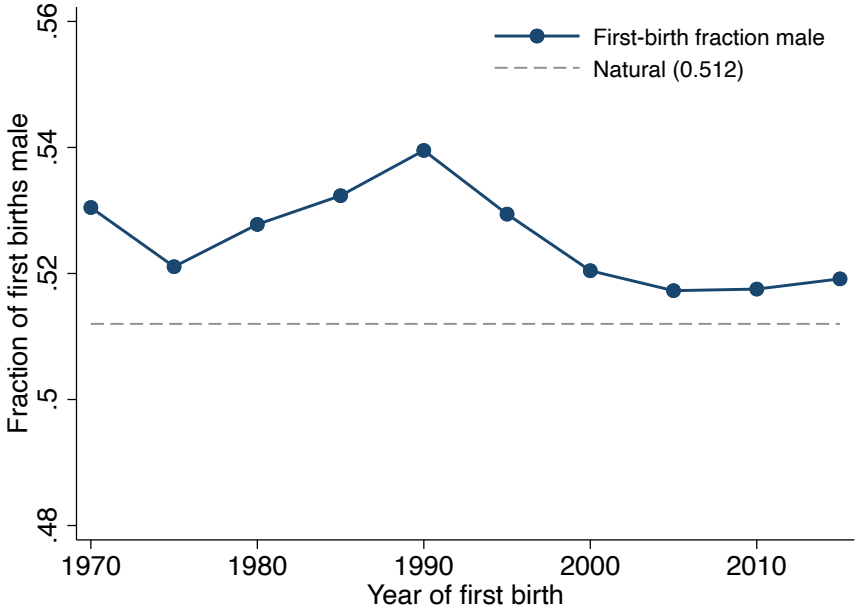
Table 15: Effect of the Sterilization Condition on Children's Outcomes (Under-Fives)

Child outcome	Package (π_1)	Cond. − uncond. ($\pi_1 - \pi_2$)	Per sterilization (2SLS)	Baseline mean	Children
Anthropometry (under-5)					
Stunted (height-for-age < -2)	0.015** (0.007)	0.016 (0.011)	0.200* (0.098)	0.34	511,300
Underweight (weight-for-age < -2)	0.018** (0.007)	0.017** (0.008)	0.208* (0.112)	0.41	520,111
Wasted (weight-for-height < -2)	0.006 (0.007)	-0.004 (0.009)	-0.054 (0.099)	0.18	512,354
Immunization					
Received DPT3	0.005 (0.007)	0.002 (0.011)	0.029 (0.136)	0.66	502,005
Received measles	0.012* (0.007)	-0.002 (0.009)	-0.031 (0.110)	0.58	499,239
Fully immunized	0.012* (0.007)	0.001 (0.010)	0.006 (0.122)	0.52	497,189
Mortality and health					
Has died	0.002 (0.003)	0.007* (0.004)	0.085 (0.060)	0.05	621,989
Anemic	-0.008 (0.009)	-0.009 (0.015)	-0.104 (0.180)	0.64	426,073

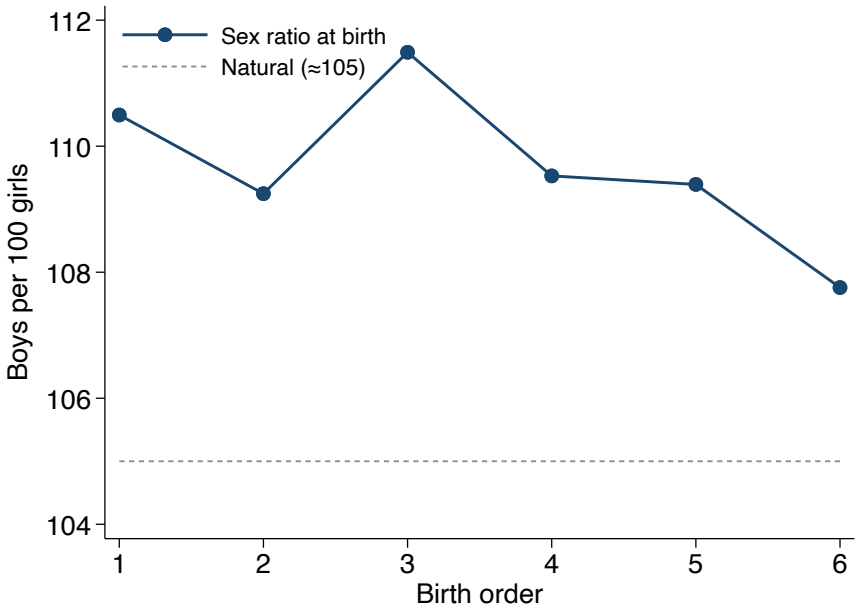
Estimates of equation (4), child-level regressions in which each child is linked to its mother's firstborn-girl eligibility and state-by-cohort CCT exposure. Columns as in Table 14: "Package" is the conditional-exposure coefficient alone (income not netted), "Cond. − uncond." the conditionality contrast $\pi_1 - \pi_2$, and "Per sterilization" the 2SLS instrumenting the mother's sterilization with the netted interaction (see text). Anthropometry and anemia are measured for living under-fives in the biomarker subsample (mostly 2005, 2015, 2019), while survival is defined over all children ever born. Regressions weighted by the DHS person weight. Controls: mother cohort, child birth order and sex, wealth, urban, and religion. Standard errors are clustered by state. $^{*}\,p < 0.10$, $^{**}\,p < 0.05$, $^{***}\,p < 0.01$.

Figure 10: Sex of Births over Time and by Birth Order

(a) Fraction of First Births Male, by Year of First Birth

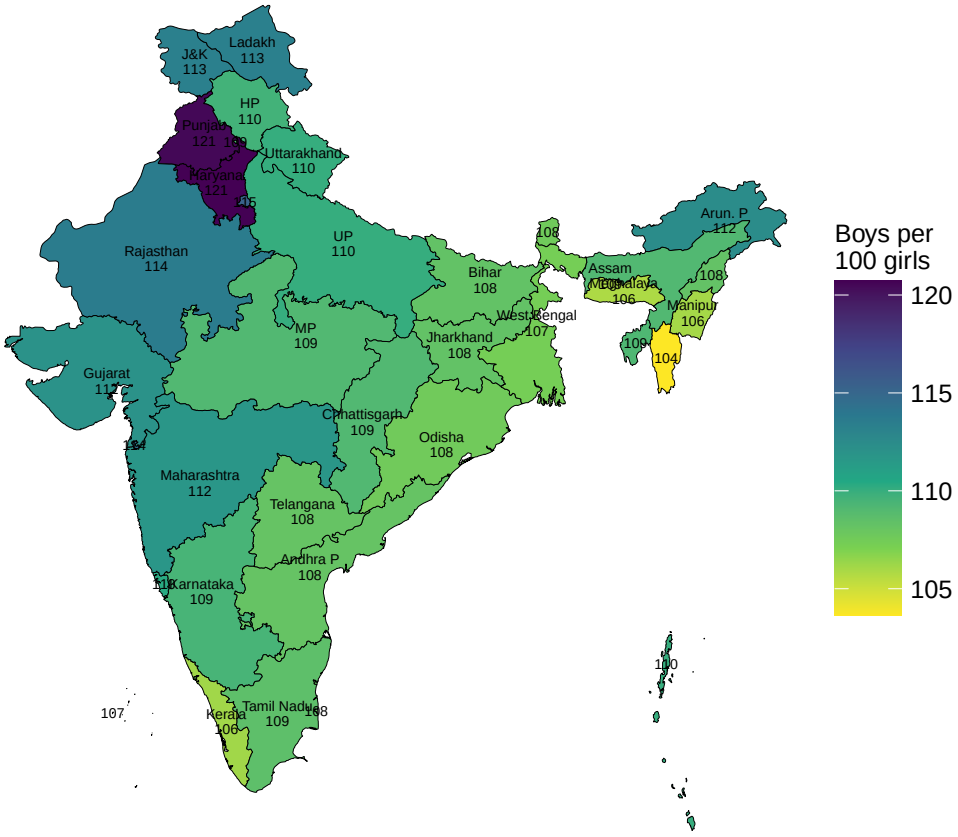


(b) Sex Ratio at Birth by Birth Order



Pooled birth panel. Panel (a): fraction of first births that are male, in five-year bins of the year of first birth. The natural fraction is about 0.512 (105 boys per 100 girls). Panel (b): boys per 100 girls by birth order, all women.

Figure 11: Sex Ratio at Birth by State



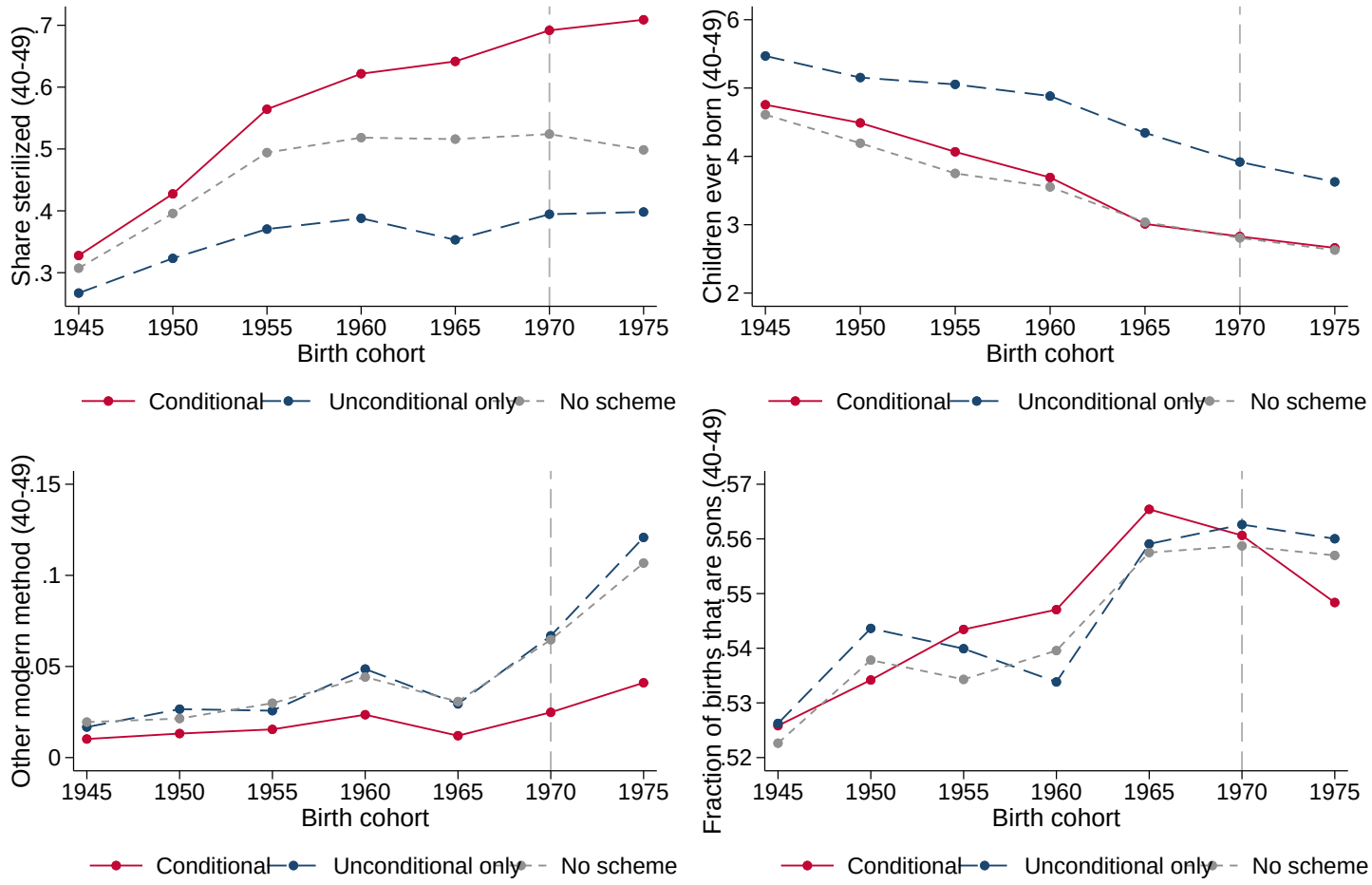
Boys per 100 girls among all births reported by sampled women, pooled across rounds, with state boundaries harmonized to 1992. The natural ratio at birth is about 105.

Table 16: The Conditionality Effect under Region Trends

Reduced form (completed fertility)	Conditionality $\pi_1 - \pi_2$
Baseline (state and cohort fixed effects)	-0.398*** (0.064)
+ South × cohort fixed effects	-0.405*** (0.065)
+ 6-region × cohort fixed effects	-0.403*** (0.064)

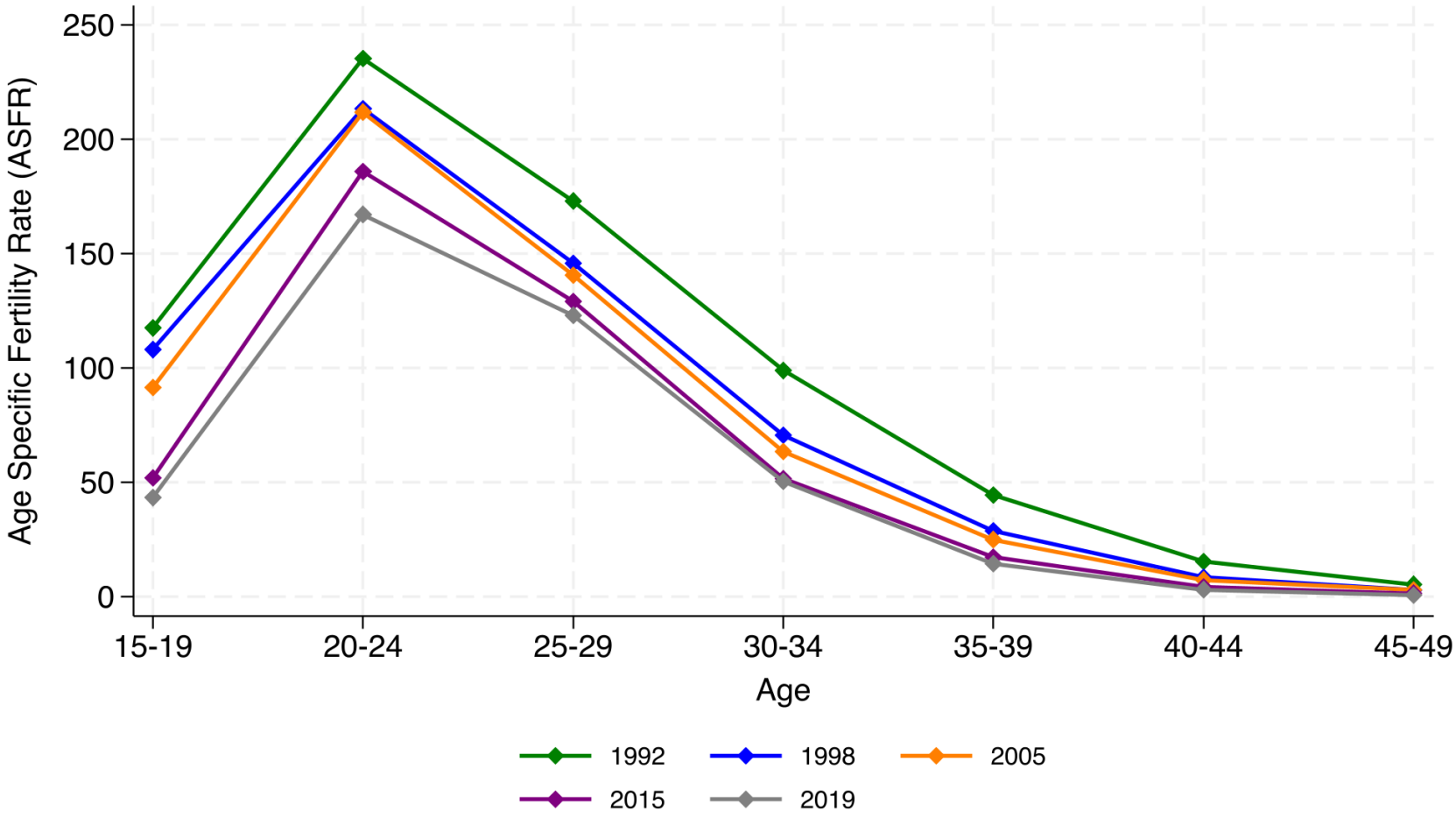
Reduced-form conditionality effect $\pi_1 - \pi_2$ on completed fertility at the one-daughter margin (E^1), mothers 40–49, as the specification progressively allows regions their own cohort paths. The first row is the baseline specification of Table 10, Panel B, re-estimated on the geographic-robustness sample. The second adds South-versus-rest × cohort fixed effects, and the third allows each of six regions its own cohort path. Standard errors, clustered by state, in parentheses, and stars from t-statistics with 12 degrees of freedom (*** $p < 0.01$).

Figure 12: Completed Sterilization, Fertility, Method Mix, and Sex Composition by Cohort and Scheme-State Group



Women aged 40–49 by birth cohort, in sterilization-conditional, unconditional-only, and no-scheme states: share sterilized, children ever born, other modern-method use, and the fraction of children born who are sons. The dashed line marks cohort 1970, the last observed through ages 45–49 by 2019. Later cohorts are not yet complete.

Figure 13: Age-Specific Fertility Rate by Age, Survey Years 1992–2019



Age-specific fertility rates (births per 1,000 women) by five-year age group, one line per survey round, from the 36-month birth window. Exposure is weighted by the combined person weight and births by the standard sampling weight.

Table 17: Sterilization's Mechanical Share of the TFR Decline

Period TFR, 1992 → 2019	3.45 → 2.01
Completed fertility (CEB 45–49), 1992 → 2019	5.11 → 3.09
Ever sterilized (45–49), 1992 → 2019	0.26 → 0.57
Sterilization's share of the (quantum) fertility decline	
Mechanical upper bound (Bongaarts, freeze prevalence)	67%

Period TFR from the 36-month survey windows, completed fertility and completed sterilization at ages 45–49. The mechanical upper bound freezes sterilization prevalence at its 1992 level in the Bongaarts period accounting and credits every sterilization with the method's full birth-averting power.

A Appendix

A.1 Survey Design and Weighting

The jump in sample size reflects a change in the survey's target geography. Every round draws a sample designed to be representative at the state level, which is the level my analysis uses, while the 2015 and 2019 rounds expanded the sample to deliver district-level estimates as well. The early rounds are therefore not representative below the state, and the district-level checks in this paper accordingly use only the last two rounds.

The first two rounds, in 1992 and 1998, interviewed only *ever-married* women, a common design in older fertility surveys. From 2005 on, the survey interviews every woman aged 15 to 49 regardless of marital status. The survey provides an "all-women factor" that inflates the ever-married respondents in the early rounds so that their weighted totals represent all women of reproductive age. I apply it throughout. It equals one from 2005 on, when the sample already covers all women.[25] The reweighting recovers population totals, but it cannot put never-married women's individual records into a file that never collected them, which has two consequences. First, statistics that refer to the full population of women are affected, though to different degrees. The share of women who remain unmarried cannot be computed at all in the early rounds, so Table 1 omits marital status. All-women means of other characteristics can be computed but are only approximate. The factor matches the number of women in each age group without undoing selection within it, so at age twenty, where the already-married are systematically less educated than the still-unmarried, no reweighting of the married ones recovers the education of all twenty-year-olds. Statistics defined within the ever-married sample, such as the mean age at marriage among ever-married women, are unaffected, since the survey measures them the same way in every round. Second, this matters little for the analysis itself, because the primary analysis is restricted to ever-married women throughout. Births outside marriage are exceedingly rare in India, and a woman is expected to be married with at least one child before the public system sterilizes her, so the fertility behavior of never-married women is not the object here. I measure CFR at ages 40 to 49, by which almost every Indian woman has married, and I define the sterilization hazard over the years a woman is at risk from her first marriage onward. The ever-married frame is therefore the natural sample for the questions this paper asks, not a limitation to be corrected.

[25] Concretely, I weight by the DHS person weight multiplied by this all-women factor; every statistic and regression in the paper uses this combined weight unless a caption says otherwise.

A.2 Fertility Measures: Formulas

The period total fertility rate in year t sums the age-specific fertility rates (ASFR) observed that year,

$$\mathrm{TFR}_t \;=\; 5\sum_a \mathrm{ASFR}_{a,t} \;=\; 5\sum_a \frac{B_{a,t}}{E_{a,t}}, \tag{9}$$

over the five-year age groups $a \in \{15, 20, \ldots, 45\}$, where $B_{a,t}$ are births and $E_{a,t}$ the woman-years of exposure in age group a during year t; following the DHS convention, births and exposure come from the 36 months before each interview. Cohort completed fertility for birth cohort c is

$$\mathrm{CFR}_c \;=\; \mathbb{E}[\text{children ever born} \,|\, \text{cohort } c,\ \text{age 45–49}] \;=\; 5\sum_a \mathrm{ASFR}_{a,c}, \tag{10}$$

the same age-specific rates summed along the cohort's own life rather than across one calendar year. Tempo-adjusted period measures exist (Bongaarts and Feeney, 1998). This paper instead works with the cohort measure directly and shows in Section 8 that the timing wedge is small here.

Building on Davis and Blake (1956), Bongaarts (1978) showed that four behaviors account for most of the variation in fertility across populations, and wrote the TFR as a biological maximum scaled down by one index per behavior,

$$\mathrm{TFR} \;=\; \mathrm{TF} \cdot C_m \cdot C_c \cdot C_i \cdot C_a, \tag{11}$$

exposure through marriage (C_m), contraception (C_c), postpartum infecundability from breastfeeding (C_i), and induced abortion (C_a), each index running from zero (the behavior suppresses fertility entirely) to one (it does nothing). The contraception index is

$$C_c = 1 - 1.08\, u\, e, \tag{12}$$

where u is the share of married women currently using any method, e the average use-effectiveness of the mix, and 1.08 the sterility correction of Appendix A.4. The model's level is synthetic and not meant to match the standard TFR.

The total-fecundity benchmark TF = 15.3 in the Bongaarts model comes from the CFR of Hutterite women, whose communities bear children throughout the reproductive years without contraception, the standard estimate of a woman's biological maximum (Bongaarts, 1978). Indian women's total fecundity likely differs, given differences in nutrition, health, and breastfeeding. This does not affect the decomposition of the decline: the anchor and

counterfactual rates all scale with TF, so it cancels in every "% of decline" entry, and the log shares use the indices' log changes, in which it never appears; its value moves only the model's level, including the counterfactual levels the table displays (Appendix Figure A1).

A.3 Why the Decompositions Use CFR Rather Than the Period TFR

The period TFR aims to give a picture of fertility in a window of time, and the DHS convention is to build it from births in the 36 months before the interview. Decomposing that rate by sterilization status or social group produces terms with no behavioral interpretation, for four reasons.

- *Contamination by future sterilizers:* a woman counts as unsterilized if she has not had the procedure by the survey, but many such women will sterilize later. The window records their births to the unsterilized side, and the survey cannot reveal which women they are, so an "unsterilized" rate mixes women who will never sterilize with women on their way to sterilizing.

- *The window artifact:* a woman counts as sterilized if she had the procedure at any time before the survey, and a sterilized woman bears no further children. She can therefore contribute a birth to the window only if she sterilized shortly after a birth and that birth falls inside the 36 months. Changes in a "sterilized-women TFR" thus capture changes in how soon after their last birth women sterilize, not changes in any group's childbearing.

- *Group membership measured after the births:* the education, wealth, and residence that would define a woman's group are recorded at the interview, not when she bore the children the window counts, and the group boundaries themselves drift across rounds as schooling expands and households re-rank.

- *Tempo:* as Section 4.1 shows, the period rate mixes quantum and timing, so a "behavioral" term of a period decomposition can reflect nothing more than births shifting across calendar years.

Cohort CFR at ages 40–49 avoids each problem. Sterilization is terminal, so status is final and "ever sterilized" is well defined. Covariates are completed, each woman appears once with her full birth history rather than a three-year slice of it, and CFR is pure quantum.

A.4 The Bongaarts Sterility Correction

The constant 1.08 in the contraception index (12) is Bongaarts's correction for the overlap between contraception and natural sterility. Couples who are naturally sterile have no reason to contracept, so contraceptive users are drawn disproportionately from fecund couples, and a given level of use averts somewhat more births than it would in an unselected population. The factor scales the index up to reflect that overlap, and it corresponds to an assumed sterility prevalence of roughly seven to eight percent among women of reproductive age (Bongaarts, 1978).

The cohort index's age window: current status at ages 40–49 records a reversible method only if a woman still uses it at those ages, and most reversible use happens earlier in life, so the cohort C_c undercounts non-sterilization contraception and tilts its method split toward sterilization by construction. Comparing cohorts at the same ages removes the level of the undercount but not a trend in it: if later cohorts continue reversible use to older ages, part of the measured rise in the sterilization component is mismeasurement. The period accounting of Table 4, which covers all ages, is the check, and it ranks the channels the same way. The survey records only the method in current use at the interview and collects no retrospective method history, so a most-recently-used definition that would soften the caveat cannot be built.

A.5 The Contraception Index by Cohort

The period accounting of Section 5 can be taken to cohorts for contraception alone. Whether period or cohort measures better reflect fertility change is an open question, so this appendix reports the cohort version. It is non-standard, and contraception is the only index a cohort version can measure, because the other three indices are built from current-status inputs with no cohort analog. I compute C_c by birth cohort among currently married women observed at 40–49. Current status at those ages captures terminal methods fully but reversible ones only if still in use, a tilt toward sterilization that Appendix A.4 discusses.

Appendix Table A1 reports the levels by cohort and decomposes the change. C_c falls from 0.47 for the 1945 cohort to 0.23 for the 1975 cohort, and two exact splits of that fall tell one story. Prevalence accounts for more than the whole fall (u rises from 0.50 to 0.74) while the mix's effectiveness barely moves. By method, female sterilization alone contributes 94 percent, with the collapse of vasectomy working against the decline. Contraception spread because women adopted sterilization, and within couples the method itself moved from men to women.

The survey's ever-use questions allow a check on the current-status tilt. Each woman can be classified by the most effective method she *ever* used, with the core methods asked in every round. Under that classification the index falls from 0.41 for the 1945 cohort to 0.14 for the 1975 cohort, and female sterilization still accounts for 80 percent of the fall, with modern reversible methods at 47, rhythm and withdrawal at 14, and the collapse of ever-use of vasectomy working against the decline at −41. The sterilization-led reading of the cohort index is therefore not an artifact of measuring methods only at the interview.

Table A1: Bongaarts Contraception Index by Cohort, Split by Method

	1945 cohort	1975 cohort	Change	Contribution to ΔC_c
Prevalence (share of married women whose current method is:)				
Any method (u)	0.504	0.744	0.240	-0.251
Female sterilization	0.345	0.551	0.206	-0.221
Male sterilization (vasectomy)	0.112	0.005	-0.108	0.116
IUD	0.004	0.012	0.008	-0.008
Injectable	0.000	0.003	0.003	-0.003
Pill	0.003	0.032	0.028	-0.029
Condom	0.010	0.053	0.043	-0.040
Withdrawal	0.010	0.032	0.022	-0.019
Traditional / abstinence	0.019	0.058	0.039	-0.032
Effectiveness				
Use-effectiveness of the mix (e)	0.980	0.956	-0.024	0.016
$u \times e$	0.494	0.711	0.218	
$C_c = 1 - 1.08\, u\, e$	0.467	0.232	-0.235	-0.235

Currently married women observed at ages 40–49. A woman is classified by her most effective current method, so the method categories are mutually exclusive (a husband's vasectomy protects the couple and takes precedence). Effectiveness weights are typical-use failure rates (Trussell, 2011): sterilization 0.995, vasectomy 0.9985, IUD 0.992, injectable 0.96, pill 0.93, condom 0.87, withdrawal 0.78, traditional 0.76. The "any method" and "effectiveness" rows split ΔC_c as $-1.08(\bar{e}\,\Delta u + \bar{u}\,\Delta e)$, and the method rows split the same total as $-1.08 \sum_m e_m \Delta u_m$. A negative contribution pushes C_c down. The two splits are alternative exact decompositions, each summing to ΔC_c, so their contributions should not be added together.

Table A2: Bongaarts Accounting by Sub-Period

	1992–98	1998–2005	2005–15	2015–19	1992–2019
Observed TFR, start → end	3.45→2.89	2.89→2.71	2.71→2.21	2.21→2.01	3.45→2.01
Model log decline ($-\Delta \ln$)	0.14	0.35	-0.02	0.38	0.86
% of the period's model decline (additive)					
Marriage C_m	60	-23	–	14	14
Contraception C_c	68	88	–	81	76
Infecundability C_i	-34	33	–	3	7
Abortion C_a	6	2	–	2	3
female sterilization	64	55	–	14	39
male sterilization	-17	-3	–	0	-7
modern reversible	13	22	–	38	29
traditional & withdrawal	8	14	–	29	15

Each column applies the accounting of Table 4 to one pair of consecutive survey rounds, with the full period in the last column. Shares are the additive log-scale version, each behavior's change in $\ln C_x$ over the period's model log decline, so the four index rows sum to 100 within a column and the method rows allocate the contraception row. The 2005–15 shares are suppressed because the model's implied decline over that span is near zero, which leaves the shares undefined. Inputs and construction as in Table 4.

A.6 The Oaxaca–Blinder Decomposition

The group decompositions of Section 5 split a change in an aggregate rate into a rates part and a composition part, the two-fold Oaxaca–Blinder decomposition on cell means (Oaxaca, 1973; Blinder, 1973). This appendix states the framework and derives the midpoint identity the tables report, including how the cross-term is allocated. Write the aggregate rate in each cohort as $R_c = \sum_{g=1}^{G} w_{g,c}\, r_{g,c}$, where $r_{g,c}$ is the rate in group g of cohort c, $w_{g,c}$ the group's population share, and g runs over the values of one classifying characteristic. Let $\Delta x_g = x_{g,c_1} - x_{g,c_0}$. Substituting $w_{g,c_1} = w_{g,c_0} + \Delta w_g$ and $r_{g,c_1} = r_{g,c_0} + \Delta r_g$ and expanding,

$$R_{c_1} - R_{c_0} = \sum_g w_{g,c_0}\, \Delta r_g + \sum_g r_{g,c_0}\, \Delta w_g + \sum_g \Delta w_g\, \Delta r_g, \tag{13}$$

a rates part evaluated at initial composition, a composition part at initial rates, and a cross-term. Expanding instead around the later cohort gives the mirror image,

$$R_{c_1} - R_{c_0} = \sum_g w_{g,c_1}\, \Delta r_g + \sum_g r_{g,c_1}\, \Delta w_g - \sum_g \Delta w_g\, \Delta r_g. \tag{14}$$

Both are exact. They differ only in which cohort serves as the base, and each leaves a cross-term with no behavioral reading of its own. Averaging the two expressions cancels the

cross-terms,

$$R_{c_1} - R_{c_0} = \sum_g \frac{w_{g,c_0} + w_{g,c_1}}{2} \Delta r_g + \sum_g \frac{r_{g,c_0} + r_{g,c_1}}{2} \Delta w_g = \sum_g \bar{w}_g \, \Delta r_g + \sum_g \bar{r}_g \, \Delta w_g, \quad (15)$$

the midpoint identity the tables of Section 5 report. Equivalently, since $\bar{w}_g \Delta r_g = w_{g,c_0} \Delta r_g + \frac{1}{2} \Delta w_g \Delta r_g$ and $\bar{r}_g \Delta w_g = r_{g,c_0} \Delta w_g + \frac{1}{2} \Delta w_g \Delta r_g$, the midpoint form allocates each group's cross-term half to the within part and half to the between part. The allocation affects only the attribution between the two parts, not their sum, which equals the total change under any split; the symmetric split is Kitagawa's, and it avoids privileging either cohort as the base (Kitagawa, 1955).

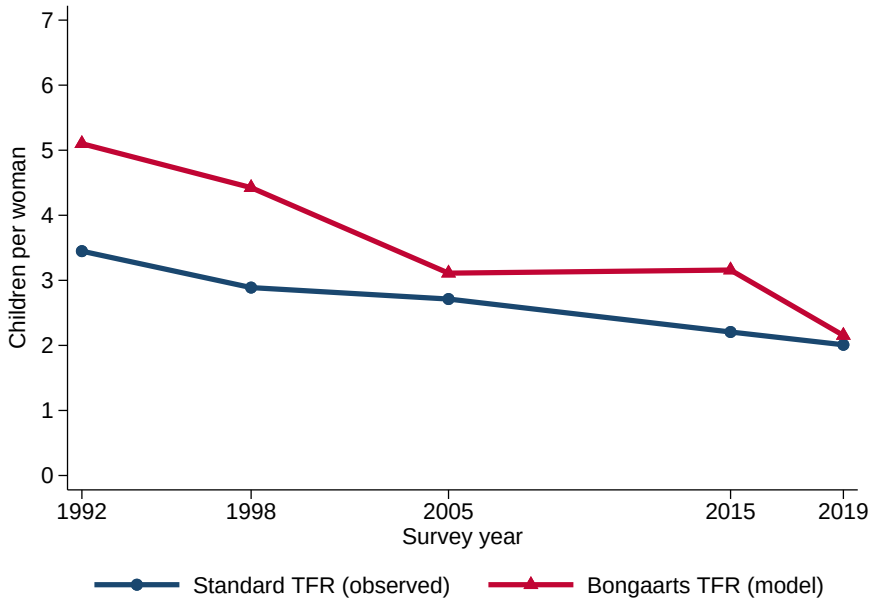
A.7 The Education-Expansion Regression

To compare the Indian decline with the Latin American cohort record of Calles and Vogl (2026), I mirror their equation (2) within one country. Collapsing the data to state-by-birth-cohort means, I estimate

$$\overline{\text{CFR}}_{sc} = \beta \, \text{AnySchool}_{sc} + \alpha_s + \alpha_c + \varepsilon_{sc}, \quad (16)$$

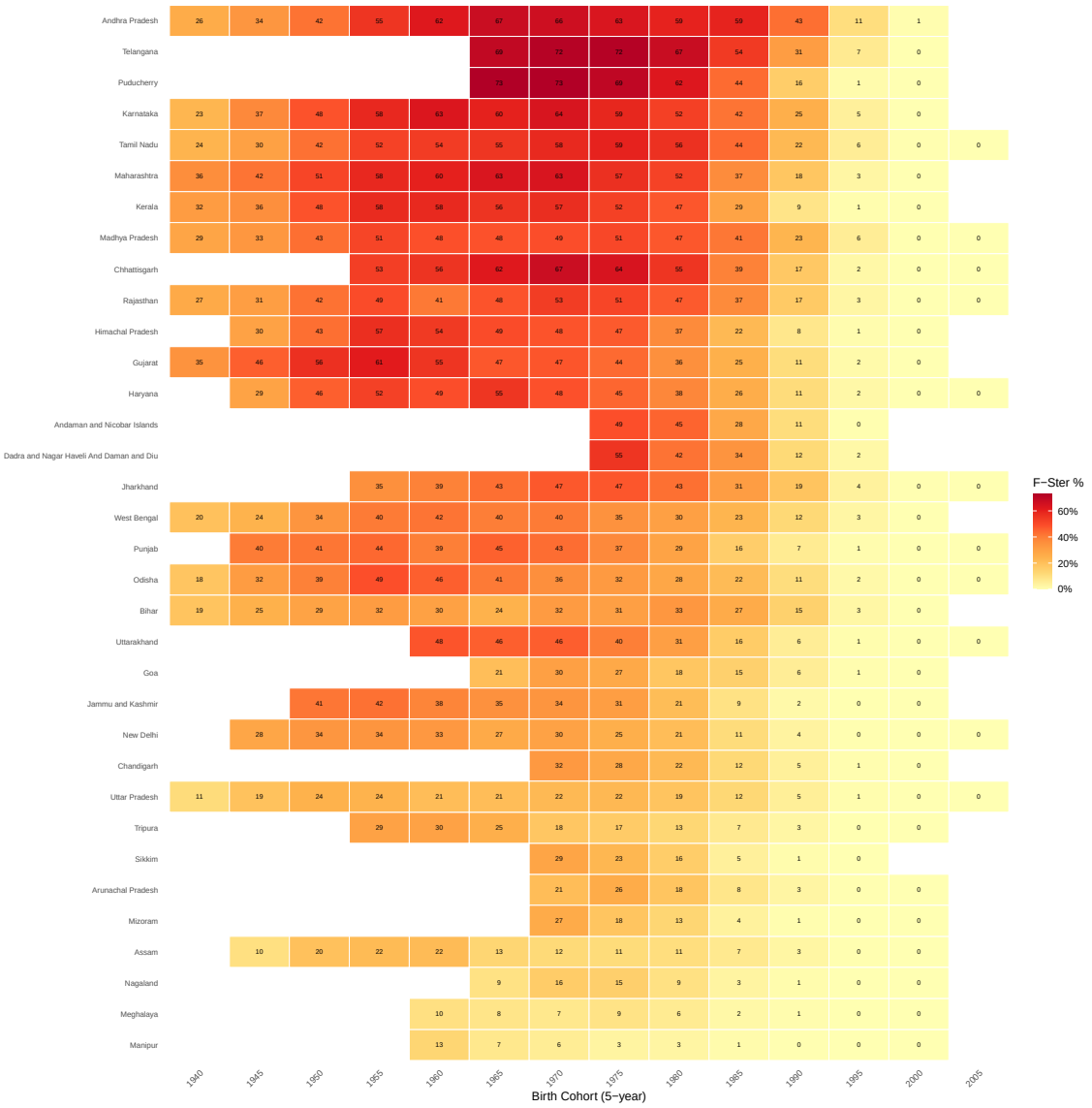
where $\overline{\text{CFR}}_{sc}$ is mean completed fertility in state s and cohort c, AnySchool_{sc} is the cell's share of women with any schooling, and α_s and α_c are state and cohort fixed effects. Cells are weighted by their survey weight and standard errors are clustered by state. The schooling coefficient is $\hat{\beta} = -1.7$ (standard error 0.9). The share of women with any schooling rose from 0.32 to 0.53 across the 1945–1970 cohorts, so the implied education contribution is $-1.7 \times 0.21 = -0.36$ children, about 20 percent of the 1.85-child CFR decline in this sample, half the 39 percent that education accounts for in the Latin American panel. Adding the cell's urban share contributes under one percent, and women's employment, which fell slightly across these cohorts, contributes little, matching their null on employment.

Figure A1: Bongaarts Model-Based TFR versus the Standard TFR



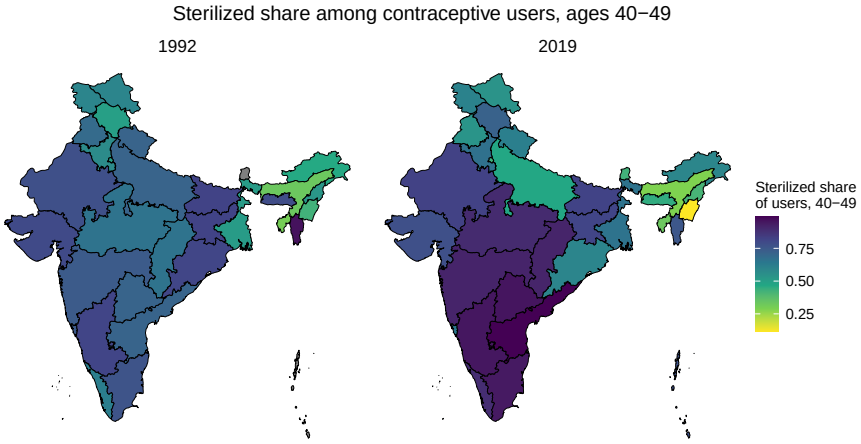
The standard TFR of equation (9) and the Bongaarts model's TFR of equation (11) (with TF = 15.3), by survey round. The model's log decline over 1992–2019 is −0.86, against −0.54 for the observed rate.

Figure A2: Sterilized Share by State and Birth Cohort



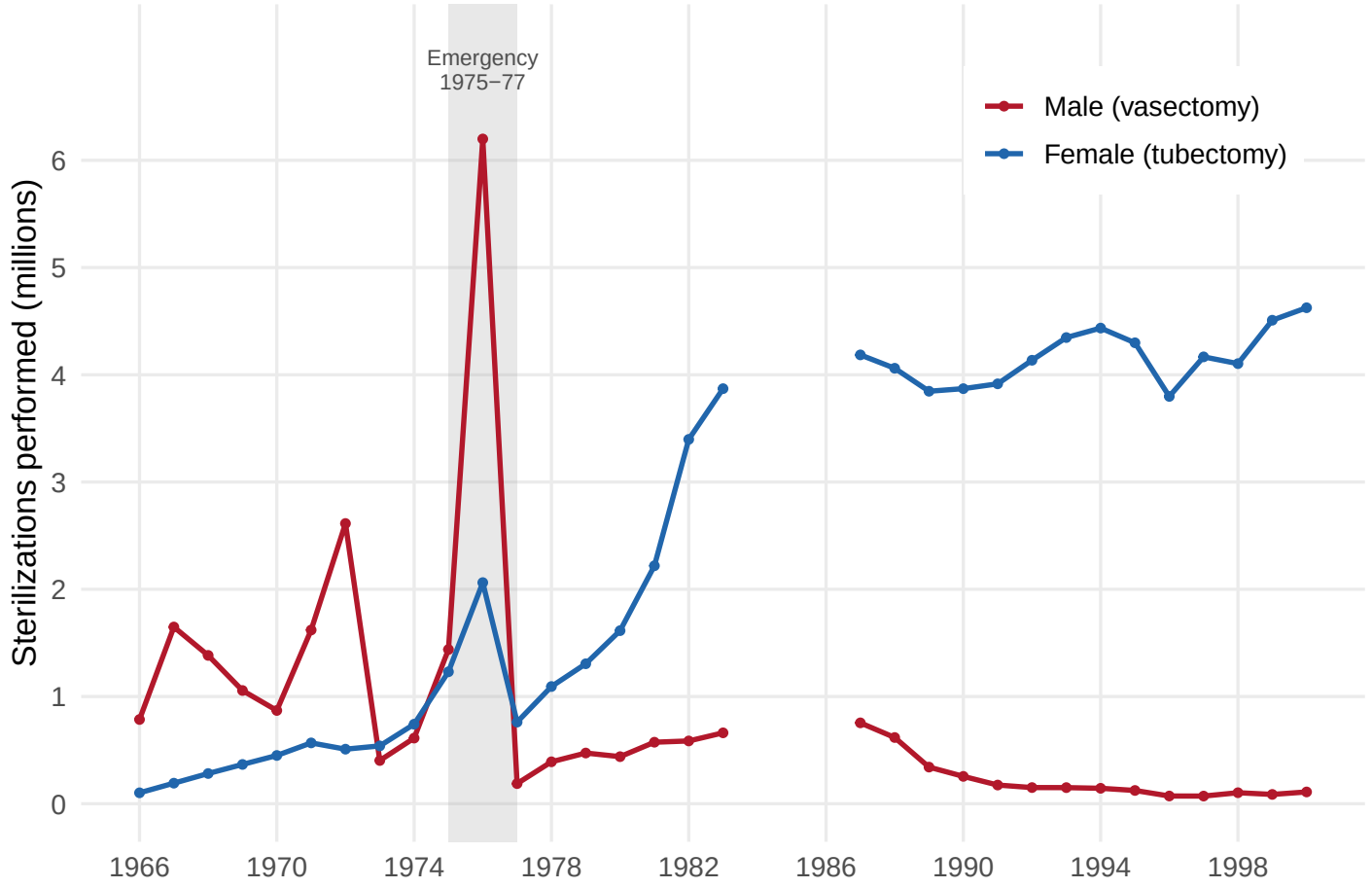
Share of women ever sterilized, by state and 5-year birth cohort, pooled across survey rounds, with cells under 50 observations suppressed. States ordered by their overall sterilized share. The youngest cohorts are observed only at ages before most sterilizations occur.

Figure A3: Sterilized Share among Contraceptive Users by State



Share of contraceptive users whose method is female sterilization, among currently married women aged 40–49 using any method (sterilization, modern reversible, or traditional), by state, in the 1992 and 2019 rounds. One color scale across the two years, with modern state boundaries throughout. In the 1992 panel the post-2000 successor states are assigned their parent state's value.

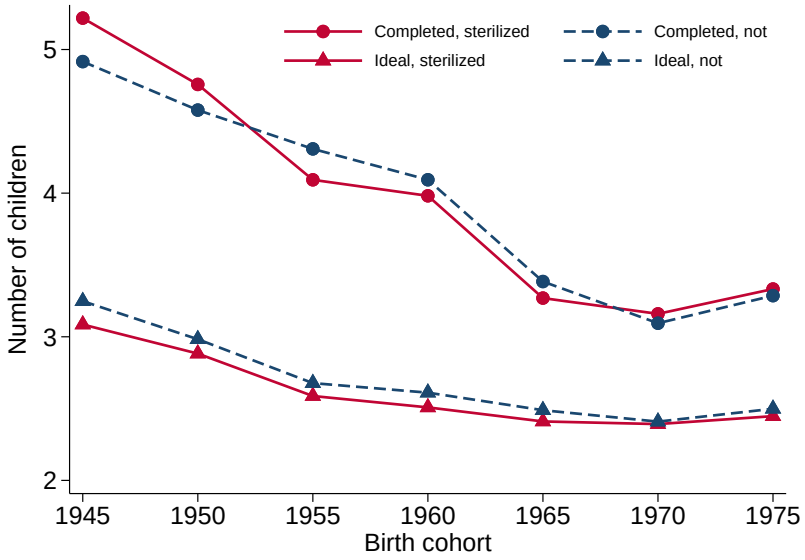
Figure A4: Sterilizations Performed in India by Sex, 1966–2000



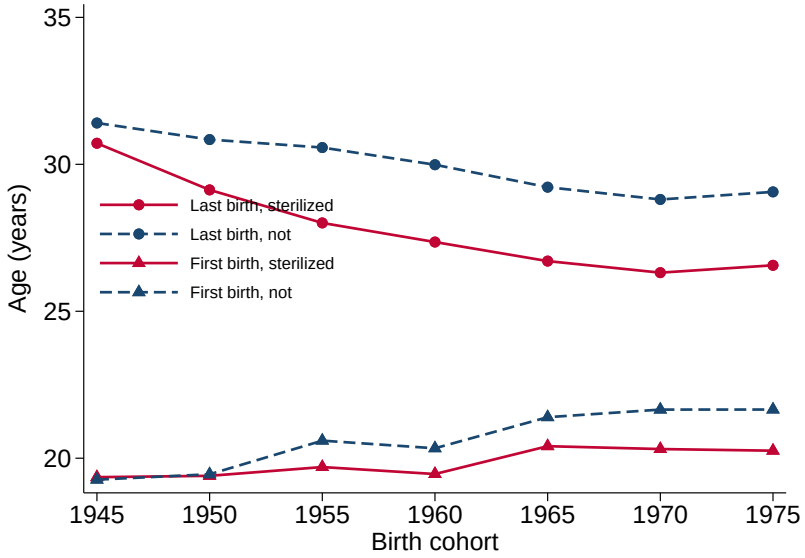
Annual counts of vasectomies (male) and tubectomies (female) performed under the national family-planning program. Fiscal years are plotted at their starting calendar year. The male/female breakdown is missing for 1984–85 and 1985–86, when the yearbooks report only totals. Source: Ministry of Health and Family Welfare annual yearbooks, digitized and compiled by Pelras and Renk (2023).

Figure A5: Fertility and Its Timing by Sterilization Status, across Cohorts

(a) Completed and Ideal Number of Children



(b) Age at First and Last Birth



Women aged 45–49, by birth cohort, split by sterilization status at the survey. Panel (a): children ever born (circles) and ideal number of children (triangles). Panel (b): mean age at first and at last birth.

A.8 Specification Details

The two-daughter specification: At the one-daughter margin, $E_i = E_i^1$. At the two-daughter margin the regression keeps every E^1 term and adds the E^2 terms, so all its

coefficients are, written out,

$$F_{is} = \pi_1 \left(E_i^2 \times \bar{C}_{is}\right) + \pi_2 \left(E_i^2 \times \bar{U}_{is}\right) + \delta\, E_i^2 + \pi_1^1 \left(E_i^1 \times \bar{C}_{is}\right) + \pi_2^1 \left(E_i^1 \times \bar{U}_{is}\right) + \delta^1 E_i^1 + \mu_1 \bar{C}_{is} + \mu_2 \bar{U}_{is} + \alpha_s + \alpha_{c(i)} + h(X_{is}) + u_{is}, \tag{17}$$

with a separate main effect and separate interactions for each margin. The reported two-daughter coefficients are π_1 and π_2 on the E^2 terms, and they measure what the second daughter adds beyond the first. One might read the one-daughter estimates off equation (17) as well and skip the separate regression, but its E^1 coefficients are a different parameter. Since a two-girl woman has $E^1 = E^2 = 1$, holding the E^2 terms fixed leaves the E^1 coefficients to measure the gradient for firstborn-girl women who did not go on to a second girl, a group defined in part by whether and what a woman bore after her first birth, outcomes the policy itself can move. The E^1-only regression instead averages over everything that follows the first birth, so it estimates the effect of the one-daughter eligibility the schemes actually offer, unconditioned on later childbearing. The two margins use different eligibility variation, and their agreement is itself a check.

Where the dose variation comes from: The variation behind the slopes runs within and across states. Within a state, coverage varies across cohorts, from zero for the benchmark cohorts already past the window at launch to full for the youngest. Across states, the same cohort faces conditional coverage in one state, unconditional coverage in another, and both in the states that ran both types. μ_1 and μ_2 absorb what coverage itself does to all women in a covered state-cohort regardless of composition, and δ absorbs the outcome gap by early-birth sex common to all state-years. Never-scheme states enter the main estimates nowhere. The all-India variant adds them as zero-dose cells, which only thickens the no-transfer benchmark, and the estimates barely move (Appendix Table A4). The linear-in-dose reading assumes each covered year contributes equally. A binarized exposure is among the robustness checks (Section 7.4).

Scheme date and composition rules: The hazard panel records each scheme's exact composition and date rules as per-scheme eligibility indicators, but the estimation deliberately does not use them. A woman's realized child composition in year t responds to the schemes, so an eligibility measure built from it would condition on an outcome, the sequence-of-treatments problem of Section 6.4; an earlier exact-composition first stage was discarded for exactly this reason. Both the hazard and the woman-level equations therefore use the predetermined firstborn-girl margin, with scheme presence as an intent-to-treat dose. A woman whose qualifying births predate her scheme's cutoff counts as exposed while facing

no incentive, which attenuates both stages toward zero and cancels in the Wald ratio. Enforcing the cutoffs directly nearly halves the average conditional dose yet leaves both stages essentially unchanged (Appendix Table A8), because at ages 40–49 the identifying exposure comes from the long-running schemes without cutoffs, Tamil Nadu's and Rajasthan's. The refined dose conditions on the timing of births, itself a choice, so it is a diagnostic rather than the preferred specification.

A.9 Additional Tables

Table A3: Geography and Scheme-Catalogue Robustness

Check	Reduced form $\pi_1 - \pi_2$		First stage $\phi_1 - \phi_2$	
	E^1	E^2	E^1	E^2
Baseline (thirteen states, parous)	-0.398*** (0.065)	-0.387*** (0.070)	0.083*** (0.016)	0.110*** (0.020)
+ region × cohort fixed effects	-0.403*** (0.066)	-0.386*** (0.070)	0.083*** (0.015)	0.117*** (0.021)
Both-scheme states only (within state)	-0.706** (0.295)	-0.685** (0.301)	0.092** (0.036)	0.193** (0.071)
Northern states only	0.069 (0.194)	0.094 (0.135)	-0.017 (0.013)	-0.053** (0.021)
Southern states, bundled (no income netting)	-0.031 (0.054)	-0.128 (0.096)	0.032** (0.011)	0.030 (0.030)
Haryana alone	0.089 (0.585)	0.143 (0.807)	-0.042 (0.185)	-0.035 (0.279)
Low-take-up schemes included	-0.404*** (0.069)	-0.392*** (0.072)	0.083*** (0.019)	0.113*** (0.023)
Bhagyalakshmi reclassified unconditional	-0.337*** (0.086)	-0.392*** (0.066)	0.069*** (0.020)	0.092*** (0.023)
Conditional arm: no-son schemes only	-0.413*** (0.072)	-0.448*** (0.050)	0.076*** (0.015)	0.101*** (0.020)
no-son schemes, child-cap controls	-0.444*** (0.060)	-0.452*** (0.049)	0.088*** (0.013)	0.116*** (0.018)
Tamil Nadu dated from 2001 restructuring	-0.431*** (0.079)	-0.397*** (0.090)	0.083*** (0.019)	0.126*** (0.027)
Madhya Pradesh dropped	-0.373*** (0.053)	-0.373*** (0.070)	0.081*** (0.016)	0.108*** (0.020)

Netted conditionality contrasts (the southern bundled row does not net income, and Haryana is a single cluster), mothers aged 40–49. E^1 indicates a firstborn girl and E^2 first two girls. Rows through "Haryana alone" restrict the sample or add fixed effects as labeled, and the rows from "Low-take-up schemes included" on re-code the scheme catalogue. Standard errors, clustered by state, in parentheses, and stars from t-statistics with 12 degrees of freedom ($^{*}\, p < 0.10$, $^{**}\, p < 0.05$, $^{***}\, p < 0.01$).

Table A4: Reduced Form and First Stage across Sample and Childless-Coding Choices

Sample and childless coding	Reduced form $\pi_1 - \pi_2$		First stage $\phi_1 - \phi_2$	
	E^1	E^2	E^1	E^2
Thirteen scheme states, parous (primary)	-0.404*** (0.067)	-0.394*** (0.070)	0.082*** (0.016)	0.111*** (0.021)
childless kept, composition controls	-0.404*** (0.067)	-0.396*** (0.070)	0.082*** (0.016)	0.112*** (0.021)
childless kept, no controls	-0.338*** (0.057)	-0.397*** (0.070)	0.111*** (0.018)	0.111*** (0.020)
All India, parous	-0.408*** (0.067)	-0.395*** (0.067)	0.082*** (0.016)	0.112*** (0.021)
childless kept, composition controls	-0.408*** (0.067)	-0.397*** (0.067)	0.082*** (0.016)	0.112*** (0.021)
childless kept, no controls	-0.342*** (0.057)	-0.398*** (0.067)	0.112*** (0.017)	0.112*** (0.021)

Netted conditionality contrasts on completed fertility (reduced form) and ever-sterilized (first stage), women 40–49. E^1 indicates a firstborn girl and E^2 first two girls. Rows vary the estimation sample (thirteen ever-scheme states versus all India with never-scheme states as zero-exposure cells) and the treatment of childless women, for whom eligibility is undefined: dropped (mothers only), kept with childless and childless-by-dose controls, or kept with no controls. Standard errors, clustered by state, in parentheses, and stars from t-statistics with 12 degrees of freedom ($^{*}\,p < 0.10$, $^{**}\,p < 0.05$, $^{***}\,p < 0.01$). The wild-cluster bootstrap p-values for the headline contrasts are more conservative (Table 10).

Table A5: The Two-Girl Margin

	First two births girls (E^2)
Panel A. Ever sterilized by ages 40–49	
Eligible × conditional exposure (ϕ_1)	0.089*** (0.016)
Eligible × unconditional exposure (ϕ_2)	-0.023 (0.026)
Sterilization condition, net of income ($\phi_1 - \phi_2$)	0.111*** (0.021)
First-stage F (excluded instrument)	29.1
Panel B. Children ever born	
Eligible × conditional exposure (π_1)	-0.166*** (0.051)
Eligible × unconditional exposure (π_2)	0.228*** (0.051)
Sterilization condition, net of income ($\pi_1 - \pi_2$)	-0.394*** (0.070)
Women	197,450

	First two births girls (E^2)
Births per induced sterilization (2SLS)	-3.54*** (0.57)
Anderson–Rubin 95% set	[-5.32, -2.39]
First-stage F	29.1
Women	197,450

The specifications of Tables 10 and 12 at the two-girl margin E^2 (first two births girls). Each regression controls all E^1 terms, and the omitted category is not having two firstborn girls. Sample: mothers aged 40–49 in the thirteen ever-scheme states. Because E^2 requires a second birth, it is not fully predetermined for women exposed after the first birth (Section 6.4); Appendix Table A9 restricts both margins to pre-scheme births. Standard errors, clustered by state, in parentheses. Stars on the $\phi_1 - \phi_2$ row are from wild-cluster bootstrap p-values (Webb weights) and all other rows use $t(12)$. $^{*}\,p < 0.10$, $^{**}\,p < 0.05$, $^{***}\,p < 0.01$.

Table A6: Conditional and Unconditional Package Effects

	(1) CEB	(2) CEB	(3) CEB	(4) CEB	(5) CEB	(6) CEB (all-Ind
$E^1\times$ Conditional-package dose (π_1 / ϕ_1)	-0.1992***	-0.1991***	-0.1909***	-0.1831***	-0.1818***	-0.1481***
	(0.0254)	(0.0255)	(0.0276)	(0.0281)	(0.0290)	(0.0416)
$E^1\times$ Unconditional-package dose (π_2 / ϕ_2)	0.1924**	0.1925**	0.2108***	0.2229***	0.2218***	0.2599***
	(0.0717)	(0.0717)	(0.0643)	(0.0640)	(0.0642)	(0.0582)
Wild-cluster p: conditional	0.002	0.002	0.003	0.004	0.001	0.059
Wild-cluster p: unconditional	0.037	0.038	0.029	0.031	0.037	0.023
Netted difference (reference)	-0.3916	-0.3916	-0.4017	-0.4060	-0.4036	-0.4080
SE	0.0724	0.0725	0.0672	0.0667	0.0671	0.0672
Women 40–49	197,470	197,470	197,450	197,450	197,450	352,558

Each column estimates equation (2) (last column: equation (1)) on mothers aged 40–49. Columns 1–5 build the controls up from bare fixed effects (state, cohort) to the full set (age, education, wealth, urban) in the thirteen ever-scheme states, and column 6 adds the never-scheme states at zero dose. π_1 is the conditional-package policy contrast, the change in the firstborn-girl minus firstborn-boy gap in completed fertility for cohorts a conditional scheme covered, against the no-scheme benchmark. π_2 is the same object for unconditional coverage. Each is a bundled intent-to-treat effect (condition, payment, and signal for π_1, and payment and signal for π_2) requiring no exclusion restriction, and the netted difference is shown for reference. Wild-cluster bootstrap p-values (Webb weights) are reported per coefficient, with standard errors clustered by state in parentheses. $^{*}\,p<0.10$, $^{**}\,p<0.05$, $^{***}\,p<0.01$.

Table A7: Scheme-Dimension Controls

	(1) CEB	(2) +NPV	(3) +monthly	(4) +no-son	(5) +cap	(6) +BPL	(7) +all
$E^1\times$ Conditional dose	-0.1816***	-0.1033	-0.1995***	0.0703	-0.2458**	-0.1400*	0.0346
	(0.0285)	(0.2455)	(0.0322)	(0.1606)	(0.0843)	(0.0728)	(0.0746)
$E^1\times$ Unconditional dose	0.2200***	0.2421***	0.1958***	0.2036***	0.2141***	0.2626***	0.0885
	(0.0635)	(0.0708)	(0.0598)	(0.0559)	(0.0637)	(0.0450)	(0.0500)
$E^1\times$ NPV dose (Rs lakh)		-0.0915					-0.3004***
		(0.2856)					(0.0424)
$E^1\times$ Monthly-stream dose			0.2880***				0.3437***
			(0.0793)				(0.0915)
$E^1\times$ No-son-rule dose				-0.3084			-0.7518***
				(0.1952)			(0.0234)
$E^1\times$ Child-cap$\leq$2 dose					0.0753		0.6456***
					(0.0803)		(0.0317)
$E^1\times$ BPL-gate dose						-0.0653	0.1295***
						(0.0838)	(0.0314)
Netted difference (cond − uncond)	-0.4016	-0.3454	-0.3953	-0.1334	-0.4599	-0.4026	-0.0539
SE	0.0666	0.2201	0.0606	0.2040	0.0808	0.0592	0.0415
wild-cluster p	0.011	0.619	0.012	0.565	0.119	0.009	0.276
Women 40–49	197,450	197,450	197,450	197,450	197,450	197,450	197,450

The reduced form of equation (2) with eligibility-by-characteristic doses added one at a time and then together: the transfer's present value (Rs lakh), a monthly-stream payment form, a no-son composition rule, a two-child cap, and a below-poverty-line gate, each built from the scheme catalogue and dosed over ages 25–39 like the treatment. Mothers 40–49 in the thirteen ever-scheme states, with state, cohort, and covariate fixed effects. Standard errors are clustered by state, with wild-cluster p-values on the netted difference. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A8: Enforcing the Schemes' Birthdate Cutoffs in the Woman-Level Exposure

Conditional dose	First stage $\phi_1 - \phi_2$		Reduced form $\pi_1 - \pi_2$	
	E^1	E^2	E^1	E^2
Scheme presence (baseline)	0.082*** (0.016)	0.111*** (0.021)	-0.404*** (0.067)	-0.394*** (0.070)
Date-refined (cutoffs enforced)	0.079*** (0.015)	0.103*** (0.018)	-0.349*** (0.080)	-0.349*** (0.086)

Woman-level first stage (ever sterilized by 40–49, equation (1)) and reduced form (children ever born, equation (2)), mothers in the thirteen ever-scheme states. The baseline row uses scheme-presence exposure. The date-refined row zeroes the conditional dose for women whose qualifying birth (the first birth at the E^1 margin; at E^2, the second birth for Haryana and Madhya Pradesh and the first girl's birth year for the Girl Child Protection states, matching each scheme's rule) predates their state scheme's birthdate cutoff: Devi Rupak 2002, Andhra Pradesh/Telangana 2005, Bhagyalakshmi and Ladli Laxmi 2006. Tamil Nadu's and Rajasthan's schemes have no cutoff. State, cohort, and covariate fixed effects. Standard errors, clustered by state, in parentheses, and stars from t-statistics with 12 degrees of freedom ($^{*}\,p < 0.10$, $^{**}\,p < 0.05$, $^{***}\,p < 0.01$).

Table A9: Eligibility from Pre-Scheme Births Only

	First stage		Reduced form	
	E^1	E^2	E^1	E^2
Baseline eligibility	0.082***	0.111***	-0.404***	-0.394***
	(0.016)	(0.021)	(0.067)	(0.070)
Women	197,450	197,450	197,450	197,450
Pre-scheme births only	0.076***	0.110***	-0.437***	-0.303**
	(0.015)	(0.028)	(0.088)	(0.130)
Women	163,632	152,141	163,632	152,141

Netted contrasts from equations (1) and (2). Each cell reports $\phi_1 - \phi_2$ (first-stage columns, ever sterilized by ages 40–49) or $\pi_1 - \pi_2$ (reduced-form columns, children ever born) for mothers aged 40–49 in the thirteen ever-scheme states, with state, cohort, and covariate fixed effects. The baseline rows define eligibility from the first births whenever they occur. The restricted rows keep only women whose qualifying births predate the first girl-child scheme in their state, the first birth at the E^1 margin and both of the first two births at the E^2 margin. Standard errors, clustered by state, in parentheses, and stars from t-statistics with 12 degrees of freedom ($^{*}\,p < 0.10$, $^{**}\,p < 0.05$, $^{***}\,p < 0.01$).

Table A10: The Sterilization Hazard on Eligibility × Active CCT

	Eligible (early births are girls):	
	(1)	(2)
	Firstborn a girl	First two births girls
Conditionality (eligible × conditional CCT)	0.0180***	0.0421***
	(0.0044)	(0.0118)
Unconditional gradient (eligible × any CCT)	-0.0023	-0.0044
	(0.0016)	(0.0033)
Woman-years	15,796,037	15,796,037

Estimates of equation (5) on the woman-year hazard panel, where the outcome is an indicator for sterilizing in a given woman-year. "Conditionality" is γ, the sterilization-condition contrast (the netted difference), and "unconditional gradient" is λ, the eligible-minus-ineligible gap under an unconditional transfer relative to no scheme. State-by-year fixed effects, with standard errors clustered by state. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A11: Effect of the Sterilization Condition on Outcomes beyond Fertility, Restricted to Completed Childbearing (Mothers 40–49)

Outcome	Package (π_1)	Cond. − uncond. ($\pi_1 - \pi_2$)	Per sterilization (2SLS)	Baseline mean	Women
Economic empowerment					
Summary index (SD units)	0.019 (0.013)	0.051 (0.040)	0.513 (0.378)	-0.01	69,376
Works for pay	0.012 (0.011)	0.035 (0.028)	0.347 (0.282)	0.37	106,425
Works: white-collar job	0.008 (0.007)	0.001 (0.009)	0.009 (0.087)	0.03	88,013
Works: agriculture	0.008 (0.011)	0.046*** (0.016)	0.445** (0.182)	0.25	88,013
Works: manual labor	0.007 (0.008)	-0.007 (0.015)	-0.064 (0.148)	0.08	88,013
Owns money she controls	-0.000 (0.013)	0.018 (0.020)	0.183 (0.177)	0.52	69,414
Health (biomarkers, 2015–19)					
Body mass index	0.073 (0.082)	-0.016 (0.094)	-0.201 (1.151)	23.61	325,300
Anemic	-0.016** (0.007)	-0.022 (0.013)	-0.268 (0.156)	0.55	303,055
Hemoglobin (g/dL)	0.059* (0.033)	0.101** (0.045)	1.236** (0.432)	11.60	303,057
Reports high blood pressure	-0.007 (0.007)	-0.015 (0.011)	-0.176 (0.134)	0.10	157,731
Reports diabetes	0.005 (0.003)	0.003 (0.004)	0.036 (0.049)	0.05	310,181
Decision-making autonomy (any say)					
Summary index (SD units)	0.021 (0.014)	0.017 (0.031)	0.171 (0.299)	0.04	82,454
Any say: her own health care	0.015 (0.013)	0.012 (0.022)	0.121 (0.201)	0.73	82,462
Any say: large purchases	0.004 (0.007)	0.007 (0.016)	0.068 (0.162)	0.72	82,461
Any say: contraceptive use	0.012 (0.009)	0.003 (0.015)	0.049 (0.271)	0.92	90,883
Experienced domestic violence (DV subsample, 2005–19)					
Ever experienced physical violence	0.022* (0.011)	0.032** (0.012)	0.228** (0.083)	0.32	46,159
Ever experienced emotional violence	-0.016 (0.014)	-0.013 (0.017)	-0.096 (0.129)	0.15	46,157
Ever experienced sexual violence	0.001 (0.006)	0.013 (0.011)	0.093 (0.080)	0.06	46,155
Gender-role attitudes					
Rejects wife-beating (index, SD units)	0.005 (0.021)	-0.025 (0.037)	-0.249 (0.369)	-0.02	88,311
Children's survival and spacing					
Any of her children has died	-0.013* (0.007)	-0.019** (0.009)	-0.224* (0.115)	0.22	352,558
Share of her children who died	-0.003 (0.002)	-0.004 (0.003)	-0.043 (0.037)	0.07	352,558
Birth spacing, post-scheme births (mo.)	2.472** (0.861)	-0.094 (0.540)	-1.002 (5.494)	40.94	52,414
Mechanically tied to the fertility effect					
Fraction of children who are sons	-0.130*** (0.036)	-0.153*** (0.044)	-1.848*** (0.485)	0.56	351,292

The regressions of Table 14 restricted to ages 40–49, where childbearing is essentially complete. Columns and notes as in Table 14.

Table A12: Reduced Form under Lower Age Floors

Completed-fertility sample	Pooled reduced form	Within-state ($E\times$state, $E\times$cohort FE)	Women
Ages 40–49	-0.404*** (0.067)	-0.091 (0.083)	197,450
Ages 38–49	-0.405*** (0.063)	-0.108 (0.075)	241,150
Ages 35–49	-0.415*** (0.053)	-0.136* (0.064)	318,785

One-daughter margin (E^1), thirteen ever-scheme states, mothers only. Lowering the floor admits the more recent cohorts with full scheme exposure. The within-state column adds eligibility-by-state and eligibility-by-cohort fixed effects, so only within-state cohort timing identifies. Standard errors, clustered by state, in parentheses, and stars from t-statistics with 12 degrees of freedom ($^{*}\,p<0.10$, $^{**}\,p<0.05$, $^{***}\,p<0.01$).

Table A13: Reduced Form across Stopping-Decision Windows

Stopping-decision window	Conditionality $\pi_1-\pi_2$
Ages 25–39	-0.403*** (0.065)
Ages 25–37	-0.388*** (0.070)
Ages 25–35	-0.373*** (0.078)
Ages 20–39	-0.438*** (0.099)
Ages 22–39	-0.418*** (0.086)

The netted conditionality contrast on completed fertility at the one-daughter margin for mothers aged 40–49, all-India specification, with exposure $\bar{C}_{is}$ rebuilt over each row's age window. Standard errors, clustered by state, in parentheses. $^{*}\,p<0.10$, $^{**}\,p<0.05$, $^{***}\,p<0.01$.

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