From Prohibition to Choice: The Impact of Abortion Legalization on Fertility and Child Investments in Nepal *

Jijeebisha Bhattarai

Ajinkya Keskar

November 18, 2025

[Click here for the latest version.]

Abstract

In societies with son-preference, the transition from high fertility to low fertility is often accompanied by a skewed sex ratio at birth. How expanding reproductive autonomy in such societies changes fertility and early-life investments in children remains unclear. We study this question in the context of Nepal by evaluating the impact of the 2002 abortion legalization. Using a triple-difference design comparing girls and boys across firstborn-sex families before and after the reform, we find that the abortion legalization substantially reduced son-biased fertility stopping: the gap in the number of children between firstborn-girl and firstborn-boy families fell by nearly three-fifths, while the probability that a girl is missing due to sex-selective abortion rose by 1.8 percentage points. A back-of-the-envelope calculation implies that roughly 1 in 75 girls is missing from post-reform birth cohorts. On investments, daughters in firstborn-girl families gained about two months of breastfeeding, closing most of the pre-existing deficit. Taken together, the policy response to abortion legalization in a son-preferring society indicates a quantity-quality trade-off: lower cost of achieving desired family size and sex mix can lead to intensified prenatal selection against girls and increased early-life investments in those who are born.

Keywords: Abortion, Fertility, Breastfeeding, Nepal

JEL codes: J13, J16, I18, I14

*Authors: Bhattarai: Binghamton University (SUNY). Email: jbhatta1@binghamton.edu. Keskar: Binghamton University (SUNY). E-mail: akeskar@binghamton.edu. We are grateful to Nishith Prakash, Gaurav Chiplunkar, Todd Pugatch, Sulagna Mookerjee, David Slichter, Solomon Polachek, and the Binghamton University Applied Labor Group for many helpful comments. We also thank the participants of the 2025 Lake Economics Development Workshop and the 2025 WeRise Conference.

1 Introduction

Fertility control is a first-order determinant of women's well-being (Goldin and Katz, 2002; Bailey, 2006). Restrictive abortion laws govern over 40% of the world's population (CRR, 2020) and shape women's lifetime outcomes and human-capital investments in children (Londoño-Vélez and Saravia, 2025). Abortion policy also shifts who is born and the conditions into which children arrive, affecting completed fertility and child outcomes (Gruber et al., 1999).

The underlying son preference in certain South Asian societies has two significant consequences: First, sex-selective abortion skews sex ratios at birth and is central to the "missing women" phenomenon (Sen, 1990; Anderson and Ray, 2010). Access to prenatal sex-detection technology alters fertility behavior—weakens son-stopping, and changes postnatal resource allocations (Anukriti et al., 2022), consistent with the quantity–quality trade-off in fertility (Becker, 1960; Becker and Lewis, 1973; Becker and Tomes, 1976). Second, breastfeeding is both a core early-life investment, directly relevant to Sustainable Development Goals (SDGs) on ending malnutrition (SDG 2), child health and survival (SDG 3), and gender equality (SDG 5), and a common biological spacing mechanism where access to modern contraception is limited (Jayachandran and Kuziemko, 2011). How changes in legal access to abortion affect both fertility and breastfeeding behavior in son-preferring societies remains unclear.

We answer this question in the context of Nepal's 2002 abortion legalization by examining the joint impact on fertility response, specifically son-biased stopping behavior, and early-life investments. Before the reform, Nepal had one of the world's most restrictive abortion laws. Abortion was a criminal offense in all circumstances, including rape and incest, and women could be imprisoned for seeking care. In 1997, about 20 percent of women in Nepali prisons had been convicted of abortion or infanticide (CREHPA, 1996). Unsafe, illegal procedures were a leading cause of maternal mortality (Tamang, 1996; Thapa et al., 2014). After a decade of advocacy and legislative debate centered on high maternal morbidity and mortality, Nepal finally legalized abortion in 2002. First-trimester surgical services began in 2004, along with facility accreditation and provider training. Second-trimester abortion training began in 2007. Beginning in 2008, the government decentralized provision by authorizing and training nurses and auxiliary nurse-midwives (Warriner et al., 2011). In 2009, the landmark Supreme Court decision in *Lakshmi Dhikta v. Nepal* held the government accountable for building the necessary institutions and implementing policies to make abortion services affordable and accessible, and medical abortion was introduced the same year. By the mid-2010s, access and utilization were widespread (Henderson et al., 2013; Adhikari, 2016).[1] To reflect this evolution in our analysis, we define Stage 1 as 2003–2007 and Stage 2 as 2008–2018. The rollout of abortion legalization provides a natural experiment to evaluate how expanded reproductive choice in a low-income, son-preference context affects fertility behavior and child well-being.

We use six nationally representative waves of the Nepal Demographic and Health Survey (1996, 2001, 2006, 2011, 2016, 2022), which provide complete retrospective birth histories for women aged 15–49. For fertility analysis, we construct a mother–year panel following women from marriage to the survey. Similarly, we use the birth histories to create child-level data to analyze early life investments. Our analysis combines three sources of variation. First, we compare families whose first child is a girl with those whose first child is a boy, which captures the incentive to continue childbearing to obtain a son. Quasi randomness of the gender of the firstborn child in the context of India is well-established in the literature (Alfano, 2017; Milazzo, 2018; Anukriti et al., 2022), and we also show that it holds in our data (see Figure A1) and provide details in Appendix B. Second, we compare cohorts born before and after legalization, which captures exposure to the reform. Third, we compare girls and boys among later-born children to measure gender gaps in investments and survival. We use these three sources of variation in a triple difference framework. The identifying assumption is that, absent the abortion law change, any secular trends in fertility or child investments would have affected first-daughter and first-son households similarly. Event-study evidence shows no differential pre-trends between firstborn girl vs. firstborn boy families. This approach accounts for general time trends in fertility and child health and isolates the reform's impact on son-biased behaviors. The child-sex margin captures gender gaps in investments and survival, and differencing by gender purges unobservable trends that equally affect both boys and girls.

Our first main result is the effect of the policy on fertility, measured as the probability of a subsequent birth in the mother–year panel. Before 2002, Nepali couples who had a daughter as their first child were 4.6 percentage points (p.p.) more likely to keep having children (and to have a larger total number of births) than those whose first child was a son – a gap driven by the pursuit of

[1] Access to prenatal ultrasound also evolved over this period. We describe the timing and enforcement in Section 3 and report the effects of ultrasound separately in Table 13.

a son. After abortion became legally available, this gap narrowed by roughly three-fifths. In other words, parents with firstborn girls became less likely to continue childbearing solely to attain a son, indicating a weakening of the son-biased fertility stopping norm[2]. This suggests that expanding reproductive choice allowed families to better align fertility with their ideal family size rather than being dictated purely by son preference. We also find effects on birth spacing. Pre-legalization, families with a firstborn daughter had birth intervals that were 7.26 months shorter than families with a firstborn son. After legalization, the spacing gap narrowed by 1.93 months. At the same time, sex composition shifted at higher parities. Among second and higher births, children born after the reform in firstborn-girl families are 1.8 percentage points less likely to be girls. This shift at higher parities is comparable in magnitude to Taiwan's experience, where the joint availability of legal abortion and prenatal sex detection increased the sex ratio at birth among higher-parity births by about 2 percentage points (Lin et al., 2014). A back-of-the-envelope calculation implies a troubling rise in missing girls: roughly 1 in 75 girls is missing from post-reform cohorts [3].

Next, we estimate effects for postnatal investments. We find an increase in breastfeeding duration for girls relative to boys, contributing to a reduction in the gender gap in breastfeeding. Prior to the reform, Nepali girls were breastfed for shorter periods (for about 1.7–2.0 months less than sons) as parents hastened to try for a son. Post-reform, daughters in firstborn-girl families were breastfed about two months longer, nearly closing the baseline gap. These results are consistent with two mechanisms. First, a selection or "wantedness" channel: with sex selection at higher parities, a larger share of unwanted daughters is not born, so surviving girls are more wanted and receive longer breastfeeding (Gruber et al., 1999; Anukriti et al., 2022). Second, a targeted substitution channel: before reform, some firstborn-girl families shortened breastfeeding duration after daughters to accelerate the next live birth (Jayachandran and Kuziemko, 2011). Once composition can be managed with abortion, the payoff to shortening daughters' breastfeeding falls. So the reliance on breastfeeding as contraception has gone down in firstborn-girl families. Both mechanisms primarily raise breastfeeding for girls, so the gap closes through increases for girls rather than declines for boys. Our decomposition is consistent with this pattern: daughters gain in firstborn-girl families, while breastfeeding for sons changes little.

Finally, we examine the effects on child vaccination coverage and under-five mortality. For

[2]Son-biased fertility stopping refers to the practice where couples continue to have children until they have at least one son, at which point they stop childbearing.

[3]The calculations are detailed in Appendix D.

vaccinations, we find muted effects with no significant changes in gender differences after the reform. Immunization coverage in Nepal was already relatively high for both genders (Ashish et al., 2017), and it appears to have continued to improve for all children, irrespective of sex. Likewise, under-five mortality rates fell over the 2000s due to general health progress, but we do not detect a significant shift in the girl–boy survival gap attributable to the abortion reform. The absence of a sizable impact on these latter outcomes suggests that the policy's primary short-run effects operated through fertility behavior and breastfeeding practices, rather than through broad changes in health service utilization or neglect.

We conduct a series of robustness checks to validate our results. First, event-study graphs and placebo tests show no evidence of differential pre-trends in the 1980–1990 window for our main outcomes, a period that precedes ultrasound diffusion. Second, we address concerns about concurrent changes in access to prenatal sex detection. We examine in isolation the effects of ultrasound on fertility. Using a proxy for the diffusion of prenatal sex detection, we replicate Anukriti et al. (2022) on the probability of an additional birth: for firstborn-girl families, the probability of a subsequent birth declines by about 1.1–2.0 percentage points in the early post-ultrasound period and by about 3.2–4.2 percentage points in the later period, closely matching prior magnitudes. We define 1990–2002 as the pre-period, during which ultrasound diffused and is treated as part of the baseline environment. Identification, therefore, relies on the parallel-trends assumption that ultrasound-related influences evolved similarly for firstborn-girl and firstborn-boy families; under this assumption, any post-2002 divergence is attributed to legalization. Third, the findings are stable across alternative specifications. Results are robust to alternative fixed-effect structures, including child-gender-specific cohort fixed effects, cohort fixed effects that vary by firstborn-sex family type, and district-year fixed effects that absorb nationwide gender-differential trends and local shocks. They are unchanged when we redefine exposure using early and late post-reform phases, when we vary the clustering level across administrative units, and when we add controls for evolving preferences using mothers' reported ideal number of children and ideal sex composition and their interactions with cohort and firstborn sex.

A large body of literature studies how abortion policy changes affect fertility and the average conditions into which children are born (Gruber et al., 1999; Pop-Eleches, 2006; Ananat et al., 2009). In son-preferring societies, evidence centers on access to prenatal sex detection rather than on policy-driven legalization (Almond et al., 2010; Hu and Schlosser, 2015; Anukriti et al., 2022). In Taiwan, Lin et al. (2014) show that abortion legalization reduced fertility at third and higher parities and lowered neonatal (but not post-neonatal) female mortality at higher parities conditional on socioeconomic status; they did not examine postnatal health investments. In Mexico City, Clarke and Mühlrad (2016) find that decriminalization reduced births and maternal mortality. We contribute to the literature in two ways. First, we add evidence on abortion legalization showing that abortion access can reduce son-biased fertility continuation in a setting with son preference. Second, in line with the fertility-driven quantity-quality trade-off, we show effects on early-life investments driven by changes in breastfeeding duration.

The remainder of the paper proceeds as follows. Section 2 shows where the paper falls within the literature Section 3 provides background on Nepal's reform and demographic context. Section 4 describes the data and measurement. Section 5 presents the empirical strategy. Section 6 reports the results on fertility, breastfeeding, vaccination, and under-five mortality. Section 7 concludes with implications for policy and future research.

2 Literature Review and Contribution

This paper situates the study within two key strands of literature: (i) research on the consequences of expanding women's reproductive autonomy on fertility and child outcomes, and (ii) the operation of the quantity–quality trade-off in contexts with entrenched son preference. Together, these literatures provide the theoretical and empirical foundation for understanding how abortion legalization in Nepal shaped fertility behavior and gender gaps in parental investments.

Evidence from developed countries points to large long-term effects of reproductive autonomy on children's outcomes.[4] Levine et al. (1996) and Gruber et al. (1999) show that abortion legalization in the United States improved the circumstances of the "marginal child," leading to higher average parental investments and better child outcomes.[5] Pop-Eleches (2006) finds that the reversal of abortion access in Romania worsened children's long-term educational and labor market

[4]For mothers in high-income settings, early access to oral contraceptives and legal abortion expanded women's control over fertility, shifting education, work, and marriage timing and reducing unintended births; women could better align childbearing with life-cycle plans (Goldin and Katz (2002); Bailey (2006)).

[5]Prior studies on abortion legalization document substantial effects on fertility and child outcomes. Levine et al. (1996) find that U.S. birth rates declined by around 8% in early-repeal states, with the largest reductions among teens, older mothers, and unmarried women. Gruber, Levine, and Staiger (1999) show that the "marginal child" not born due to legalization would have been 40–60% more likely to live in poverty, receive welfare, live in a single-parent household, and die in infancy.

trajectories, consistent with the idea that unwanted births dilute household resources. Collectively, this literature establishes that reproductive autonomy affects both the quantity of children and the quality of investments per child.

There is limited but growing empirical evidence on these long-term effects in low- and middle-income countries. Clarke and Mühlrad (2016) find that abortion legalization in Mexico reduced fertility and maternal mortality. In Taiwan, Lin et al. (2014) show that access to abortion in conjunction with prenatal sex detection altered sex ratios at birth and reduced relative female mortality at higher parities. Yet most of these studies focus on fertility and survival outcomes; fewer examine how legalization affects postnatal parental investments such as breastfeeding, vaccination, or schooling.

Economic models of the family emphasize the trade-off between the number of children (quantity) and the investments made in each child (quality) (Becker and Lewis, 1973; Becker and Tomes, 1976). In this framework, households face a budget constraint: as fertility rises, resources per child fall, while fertility decline creates scope for greater per-child investment. Empirical studies confirm this mechanism. In India, Rosenzweig and Wolpin (1980) show that exogenous fertility shocks lowered children's schooling, while Angrist et al. (2010) document similar effects in Israel. In China, Li et al. (2008) find that fertility reductions improved education. These findings underscore that fertility decline often coincides with greater human capital investments, consistent with the model's predictions.

The strength of the trade-off, however, depends on parental preferences, household resources, and cultural norms (Rosenzweig and Zhang, 2009; Black et al., 2005). When families value sons over daughters, fertility reductions may disproportionately benefit boys, leaving girls disadvantaged even as overall resources per child increase. In South Asia, strong son preference has long shaped fertility and investment decisions (Gupta (1987); Bhat and Zavier (2003)). Sons are often seen as economic and social assets, while daughters are viewed as financial liabilities due to dowry and marriage practices (Jayachandran and Pande (2017)). This preference manifests in son-biased fertility stopping, where families continue childbearing until a desired number of sons are born. As a result, girls often grow up in larger sibships, facing diluted resources relative to boys.

A large body of empirical work documents the consequences of son preference for child outcomes. Girls in son-preferring households receive less schooling, fewer health inputs, and face higher mortality, despite the biological survival advantage of female infants (Waldron (1998); Jayachandran and Kuziemko (2011)). Access to prenatal sex-detection technologies has further complicated these dynamics. In India, Anukriti et al. (2022) find that access to abortion and sex selection narrowed gender gaps in breastfeeding and vaccination by increasing the "wantedness" of surviving girls. Closest to our study, they implement a triple-difference strategy, interacting pre/post access to prenatal sex detection with firstborn-sex and child sex, to show that ultrasound-enabled sex selection moderated son-biased stopping, narrowed gender gaps in breastfeeding and immunization, and reduced excess female mortality into early childhood [6].

Similarly, Hu and Schlosser (2015) show that in areas with higher uptake of sex selection, girls' nutritional status and survival improved. In contrast, Almond et al. (2010) document that in some settings, prenatal sex determination increased female neonatal mortality without corresponding improvements in postnatal investments. These mixed results highlight that the consequences of sex selection are highly context-dependent, shaped by cultural norms, fertility preferences, and the availability of health services.

3 Fertility Trends in Nepal

In Nepal, son preference is deeply embedded in patriarchal traditions. Sons are regarded as economic and social assets, while daughters are often viewed as financial liabilities due to dowry obligations and the expectation of marriage outside the parental home (Brunson, 2010). This strong son preference shapes fertility behavior, leading to larger numbers of male children. According to the 2011 Census, among children under age 10, boys outnumbered girls by 2.2 percent, with disparities more pronounced in urban areas (5.6 percent) than rural areas (1.7 percent). Despite the biological survival advantage of female newborns (Waldron, 1998), Nepal's mortality patterns diverge: male neonatal mortality exceeds female rates (37 versus 33 per thousand live births), but female post-neonatal mortality (ages 28 days to one year) surpasses that of males (19 versus 17 per thousand) (Ministry of Health, 2012).

[6] Anukriti et al. (2022) show that the probability of a subsequent birth for firstborn-girl families fell by about 2.0–2.3 percentage points in the early post-ultrasound period and by about 3.7–4.3 percentage points in the later post period, relative to firstborn-boy families. Using our ultrasound proxy, we find a decline of 1.1–2.0 p.p. in the early post period and 3.2–4.2 p.p. in the later post period (Table 13), magnitudes that are strikingly similar. This replication reinforces that ultrasound availability reduced son-biased fertility continuation in both India and Nepal.

The spread of prenatal sex-determination technology, particularly ultrasound, has further enabled couples to act on son preference through sex-selective abortion, contributing to elevated sex ratios at birth (SRB) and broader population imbalances (Guilmoto, 2009). Ultrasound was first introduced in Nepal in 1988 at Bir Hospital in Kathmandu, when the government of India donated an ultrasound unit (Subedi and Sharma, 2013; Mukhiya and Mishra, 2025). Since then, services have expanded rapidly and are now widely accessible, with scans costing as little as USD 6 even in rural and remote areas (NHSPP, 2013). Because ultrasound diffusion may itself affect fertility behavior and child investments, we explicitly test its impact in Table 13. In our main analysis, however, we treat 1990 as the baseline year so that the estimated treatment effect captures the impact of abortion legalization, net of any influence from ultrasound availability.

At the same time, fertility in Nepal has declined sharply—from 4.6 births per woman in 1996 to 2.6 in 2011 (Ministry of Health, 2012). The legalization of abortion in 2002 (policy details below) and subsequent expansion of safe abortion services have further altered reproductive decision-making. Together, the availability of sex-detection technology, entrenched son preference, expanded abortion access, and rapid fertility decline create conditions conducive to sex-selective abortion, skewed sex ratios, and long-term demographic challenges, including shortages of marriageable women, and potentially heightened risks of gender-based violence, abduction, and trafficking (Hesketh and Xing, 2006; Bien et al., 2013).

National Safe Abortion Policy and Strategy

Prior to 2002, abortion was considered a criminal act (homicide) in the *Muluki Ain* (the National Legal Code). Due to the restrictive law, most of the abortions were conducted illegally by unqualified personnel. These service providers used many barbaric procedures, like inserting cow dung, unknown medicines, or herbal mixtures into the uterus (Tamang, 1996). Deaths from abortion-related complications accounted for more than half of the maternal deaths that occurred in major hospitals (Thapa et al., 2014). In 1997, 20% of women in Nepali jails had been convicted on charges of abortion or infanticide. Mostly poor and illiterate, they were kept in miserable conditions, unable to afford legal assistance or even to understand what had happened to them. Only women were imprisoned; their male partners and the abortion providers were not held accountable (CREHPA, 1996).

In 2002, in response to mounting evidence of maternal deaths and injuries from unsafe abortions and to expand women's reproductive autonomy, the Government of Nepal amended the *Muluki Ain* 1959, which had previously prohibited abortion under all circumstances and classified it as an offense against life. The amendment paved the way for the National Safe Abortion Policy and Strategy 2002, which guarantees access to safe abortion services under specific conditions: up to 12 weeks of gestation with the pregnant woman's consent; up to 18 weeks in cases of rape or incest; and at any stage of pregnancy if it endangers the woman's life, physical, or mental health, or in the case of a severe fetal anomaly, with the recommendation of a medical practitioner and the woman's consent (Ministry of Health, 2002). Sex-selective abortion is "prohibited"[7], and adult consent is required for girls less than 16 years old.

In 2004, Nepal's first certified abortion clinic opened, marking the beginning of a steady expansion of services. Strong government leadership ensured the implementation of safe abortion services in Nepal. The Ministry of Health established a multisector Abortion Task Force that brought together public agencies and national and international nongovernmental organizations to draft policy and design strategies for provider training and service delivery. The Nepal Society of Obstetricians and Gynecologists provided technical support in developing standardized clinical protocols and training guidelines (Wu et al., 2017). In 2004, the Abortion Task Force was dissolved and replaced by the Technical Committee for the Implementation of Comprehensive Abortion Care, which oversaw accreditation, training, and quality assurance, and ensured that policies were evidence-based and updated as new data became available (Samandari et al., 2012). Initially, training in manual vacuum aspiration (MVA) was limited to physicians; however, beginning in 2008, staff nurses and auxiliary nurse midwives were permitted to perform MVA for pregnancies up to eight weeks. Second-trimester abortion training and certification for physicians commenced in 2007, and medication abortion was incorporated into the safe abortion program in 2009. Also in 2009, the landmark Supreme Court decision in *Lakshmi Dhikta v. Nepal* held the government accountable for building the necessary institutions and implementing policies to make abortion services affordable and accessible (Samandari et al., 2012). Data shows that the policy was implemented successfully nationwide. Between 2011 and 2016 reported over 400,000 abortions were performed at legal, safe abortion sites (Ministry of Health, 2016). Maternal mortality

[7]Despite sex-selective abortion being banned, evidence suggests that it is still prevalent (Frost et al., 2013; Lamichhane et al., 2011) because of difficulty ascertaining whether families are seeking abortion for sex-selection purposes, and fear that women will resort to unsafe abortion if they are under pressure to bear sons but unable to access safe abortion services.

in Nepal decreased from 548 deaths per 100,000 live births in 2000 to 258 deaths per 100,000 live births in 2015. Research also indicates a substantial long-term reduction in maternal health risks, such as infection and injury, following the reform, with declines in sepsis observed even during the early implementation phase (Henderson et al., 2013).

4 Data and Measurement

4.1 Data

This study draws on six waves of the Nepal Demographic and Health Survey (NDHS), conducted in 1996, 2001, 2006, 2011, 2016, and 2022. The NDHS is nationally representative and provides complete retrospective birth histories for women aged 15–49, including children's year of birth, birth order, age at death, and maternal characteristics at birth. The 1996 survey interviewed 8,429 ever-married women; the 2001 survey interviewed 8,726 women; the 2006 survey interviewed 10,793 women; the 2011 survey interviewed 12,674 women; the 2016 survey interviewed 12,862 women; and the 2022 survey interviewed 14,845 women.

For the fertility analysis, we pool all surveyed women to create a woman-level dataset. The analytic sample excludes women younger than age 13 and those who had not given birth by the time of the survey. We also restrict the sample to women who began childbearing in 1980 or later,[8] yielding a final sample of 18,350 mothers. For analyses of fertility timing, we construct a mother–year panel in which women enter in their year of marriage and remain until the year of the survey.

For the mortality and postnatal investment analyses, we pool all births of surveyed women to construct a child-level dataset. To measure under-five mortality, we exclude children younger than five at the time of survey (to allow full exposure to mortality risk) and those older than 15 (to mitigate recall bias).[9] These restrictions yield 36,298 children for the mortality sample. Breastfeeding analyses focus on the last two surviving births of each mother and are limited to children at least two years old to account for censoring.[10] This leaves 5,858 children in the breastfeeding sample.

[8]We keep all women whose first child was born on or after 1980.

[9]In Appendix D, we also show results for mortality at each age interval between birth and age five. For each outcome, children younger than the cutoff age are excluded to ensure complete exposure. Results are robust to alternative age cutoffs.

[10]We further restrict the sample to children born within 20 years of the interview date.

4.2 Measurement

The NDHS includes women's complete birth histories and their children's year of birth and other outcomes. Our first fertility analysis relies on the mother's birth profile. We construct a variable *Birth* which observes each woman from the year of her marriage until the survey year and assign a value of one if the mother gave birth in a certain year and zero if not. Our second fertility analysis uses the number of children that the mother had during the time of the survey from the fertility module.

We build child-health investment outcomes from mothers' DHS birth histories and child health modules. For breastfeeding, we use the mother's report of "How many months did you breastfeed your x^{th} child?" and take breastfeeding duration in months as the outcome. To limit censoring and recall error, we restrict to the last two surviving births and to children who are at least 24 months old at interview. For vaccinations, we use DHS records (from vaccination cards or maternal recall) on whether the child received each of the eight standard EPI doses[11], and construct a simple count from 0 to 8. Finally, under-five mortality is measured with a child-level indicator equal to 1 if the child died before 60 months of age and 0 otherwise. To ensure full exposure to risk and reduce recall concerns, we restrict the mortality sample to children who are at least 60 months and at most 15 years old at the time of the survey.

The summary statistics for fertility, child health investments, and mortality are provided in Table 1. The average mother in the sample is 29 years old, with ages ranging from 15 to 49. A large majority (87 percent) identify as Hindu. Only 43 percent of mothers and 45 percent of fathers have at least a primary education. The mean household wealth index falls around the middle of the distribution (3.1 on a scale of 1 to 5). On average, mothers report having two children, with family sizes ranging from 1 to 8. Children are breastfed for about 31 months on average, though the duration varies widely across families. The under-five mortality rate is 8.9 percent, and children receive an average of 6.7 vaccinations out of a maximum of 8.

[11] The eight vaccinations include BCG, DPT 1, DPT 2, DPT 3, Polio 1, Polio 2, Polio 3, and Measles.

Table 1: Summary Statistics

	N	Mean	Std. Dev	Min	Max
Mother's age	18350	28.82	7.54755	15	49
Mother's Education (At least Primary)	18350	0.43	.4957616	0	1
Father's Education (At least Primary)	18350	0.45	.4970647	0	1
Wealth Quintile	18350	3.10	1.415311	1	5
$\mathbb{1}$ [Hindu]	18350	0.87	.3358624	0	1
Total no. of children	18350	2.02	1.064824	1	8
Months of breastfeeding	5858	31.07	6.959548	0	35
Under-five mortality (in %)	36298	8.90	28.47667	0	100
No. of vaccinations	13404	6.69	2.335607	0	8

Notes: Data source: Nepal Demographic and Health Survey (NDHS).

5 Empirical Strategy

The hypothesis we test in this paper is that the availability of abortion services, which led to large-scale sex-selective abortion, also modified conception and investment decisions in a gendered manner.

As discussed in Section 1, we estimate a difference-in-difference (DiD) and a triple-difference specification that exploits the 2002 legalization of abortion. Our DiD design compares changes in fertility across families by the sex of the firstborn child, treating 1990–2002 as the pre-period so that the estimated treatment effect captures the causal impact of abortion legalization while holding constant the contemporaneous diffusion of ultrasound technology. Since we are interested primarily in gender gaps in child investments in our analysis, we add the sex of the child as a third interaction in a triple differences-in-differences regression framework. This structure allows us to test whether legalization narrowed the disadvantages faced by daughters in firstborn-girl families relative to other children.

Our identification strategy relies on two key facts: (a) the sex of the firstborn child is effectively random, and (b) sex-selective abortion at second and higher-order births occurs primarily in families whose firstborn is a daughter. We demonstrate that the sex of the firstborn child is randomly determined in our data. The panel (a) of Figure A1 shows that the proportion of females among first births in Nepal lies within the normal range that one would expect in the absence of manipulation (47.60%–50.42%); and it shows no tendency to increase in the post-legalization period. This contrasts sharply with the time profile of the sex ratio for higher-order births preceded by a girl in panels (b) and (c). Additionally, Table 2 shows that families with firstborn boys and firstborn girls are similar along a number of observables. Exogeneity of the sex of the firstborn has also been previously defended (Gupta, 1987; Bhalotra and Cochrane, 2010; Alfano, 2017; Anukriti et al., 2022). We also demonstrate that parents who are randomly assigned to firstborn girls are more likely to practice sex selection at higher-parity births in panels (b) and (c) of A1. After the availability of abortion services in the presence of ultrasound technology, the second and third births became increasingly male, but only for families without a son. So, the interaction with first child's sex captures the differential incentives to sex select among otherwise similar families.

The central identifying assumption is that, absent access to abortion services, trends in outcomes would have evolved similarly for families with firstborn sons and firstborn daughters. In order to test for a significant difference in trends in health investments for firstborn-boy and firstborn-girl families in the pre-legalization period, we restrict the sample to the pre-legalization period, 1996–2001, and regress the outcomes of interest on the full set of interactions between indicators for firstborn girl, female birth, and year of birth, with fixed effects for birth order and district and examine the coefficients of the triple interactions. The coefficients from these regressions are presented in Figure A2, which demonstrates no significant divergence between vaccination and breastfeeding in firstborn-boy and firstborn-girl families for pre-legalization cohorts for children of parity greater than one.

5.1 Regression Specifications

Abortion was legalized nationwide in 2002, but service availability expanded gradually.[12] To capture temporal variation in access, we classify the data into three periods: pre-legalization (1990–2002), early legalization (2003–2007), and late legalization (2008–2018), when access and utilization became widespread. Differentiating between the two post-legalization periods is important, as the late period saw the introduction of second-trimester abortion training, the authorization of nurses to provide services, and the integration of medical abortion into the national program.

[12]Table A1 provides further details.

5.1.1 Child Investments

We examine child investments in three ways: months of breastfeeding, number of vaccinations, and under-five mortality. For all three, we estimate the following equation

$$\begin{aligned} Y_i = \alpha + \beta_1 FirstbornGirl_j \times Female_i \times Post_t^1 + \beta_2 FirstbornGirl_j \times Female_i \times Post_t^2 \\ + \gamma FirstbornGirl_j \times Female_i + \omega_t FirstbornGirl_j + \sigma_t Female_i + \boldsymbol{X}'_{ijt}\tau + \delta_d Female_i \\ + \nu_d FirstbornGirl_j + \psi_b Female_i + \xi_b FirstbornGirl_j + \rho_{bt} + \eta_{bd} + \phi_{dt} + \epsilon_i \quad (1) \end{aligned}$$

for child i of birth order b born to mother j in year t and district d. The dependent variable Y_i is an indicator for either the months of breastfeeding, number of vaccinations, or mortality for child i. $Post_t^1$, $Post_t^2$, and $FirstbornGirl_j$ are defined as earlier. β_1 and β_2, the coefficients of the two triple interaction terms are our coefficients of interest. This equation is estimated for second- and higher-order births, making pre-legalization births and second- and higher-order births to mothers whose firstborn is a boy our control group. Standard errors are clustered by districts, and we have seventy-five states in our sample.

We do not observe large differences in the socioeconomic characteristics of firstborn-boy and firstborn-girl families (Table 2), so selection on the sex of the firstborn is of limited concern. However, we still control for socioeconomic conditions. The vector of socioeconomic and demographic characteristics, $\boldsymbol{X}_{ijt}$, comprises indicators for household wealth quintiles, educational attainment of the child's parents, the mother's birth cohort, religion, and residence in a rural area. We control for the main effects of $FirstbornGirl_j$ and $Female_i$ and fixed effects for state, birth year (or cohort, of the child), and birth order. We also include all pairwise interactions between these controls in our specification.

We allow the birth cohort fixed effects to vary by the sex of the firsborn $\omega_t FirstbornGirl_j$, child gender $\sigma_t Female_i$, and birth order ρ_{bt}. This provides a flexible suite of controls for possibly omitted trends. We also allow district and birth order fixed effects to vary by the sex of the firstborn and by child gender $\delta_d Female_i$, $\nu_d FirstbornGirl_j$, $\psi_b Female_i$, $\xi_b FirstbornGirl_j$, and district fixed effects to vary by birth order η_{bd} and birth year ϕ_{dt}. Child gender-specific cohort fixed effects $\sigma_t Female_i$ account for any nationwide changes that may affect gender gaps in the outcomes, like a decline in son preference, improvement in maternal health, or any trends associated with modernization. Cohort fixed effects varying by the sex of the firstborn child in the family $\omega_t FirstbornGirl_j$, control for nationwide trends that may have differentially affected the investment in children from firstborn-girl versus firstborn-boy families.

We allow the district fixed effects to vary with the child gender $\delta_d Female_i$ and the sex of the firstborn child $\nu_d FirstbornGirl_j$ so that any district-level time-invariant factors can have gender-specific effects, and to ensure that we absorb any cross-sectional heterogeneity that may be correlated with the sex of the firstborn. To account for the evidence that son preference varies with birth order (Jayachandran and Pande, 2017) and that sex of the firstborn child influences the exercise of son preference, we interact indicators for child gender and sex of the firstborn child with birth order $\psi_b Female_i, \xi_b FirstbornGirl_j$. Finally, $\phi_{dt}, \eta_{bd}, \rho_{bt}$ control non-parametrically for district-specific time trends (like differential trends in son preference or availability of ultrasound, abortion, or other health services), district-specific birth order effects, and birth order specific time effects.

Specification 1 is estimated for second- and higher-order births; the 'control' group thus comprises pre-ultrasound births and second- and higher-order births to mothers whose firstborn is a boy. The coefficients β_1 and β_2 change in the *girl–boy gap* after legalization in firstborn-girl families *relative* to firstborn-boy families. We also consider the sensitivity of the estimates to conditioning upon a mother's stated desired fertility and desired sex composition of children. While self-reported fertility preference variables could be potentially endogenous (being asked during or after the fertility process), we add this to our specification for completeness.

5.1.2 Fertility

We examine the impact of the legalization of abortion in gender gaps in fertility in two ways. First, we test if the legalization changed the probability of birth in a given year for mothers with firstborn girls versus firstborn boys. To do this, we construct a retrospective mother-year panel, where each woman is observed from the year of her marriage until the survey year. We estimate

$$Birth_{it} = \alpha + \beta_1 FirstbornGirl_i \times Post_t^1 + \beta_2 FirstbornGirl_i \times Post_t^2 +$$
$$\gamma FirstbornGirl_i + \omega_t + X_i'\tau + \phi_a + \psi_b + \sigma_r + \delta_d + \nu_d FirstbornGirl_i + \theta_{dt} + \epsilon_{it} \quad (2)$$

for mother i from district d, aged a in year t, who has had $b-1$ children by year t and whose last birth occurred r years ago. The outcome variable $Birth_{it}$ equals one if the mother gave birth in year t. The variable $FirstbornGirl_i$ equals one if mother j's first child is a girl, and zero otherwise. $Post_t^1$ indicates that year t falls within the early years after the abortion legalization (2003-2007), while $Post_t^2$ corresponds to the later years after the abortion ban was lifted (2008-2018). The vector $\boldsymbol{X}_i$ comprises indicators for household wealth quintiles, the mother's and her husband's education level, region of residence, residence in a rural area, and the mother's year of birth. The fixed effects include year (ω_t), district (δ_d), birth parity (ψ_b), years since last birth (σ_r), district-specific firstborn-girl fixed effects ($\nu_d FirstbornGirl_i$), and district-specific year fixed effects (θ_{dt})[13]. The standard errors are clustered by district, and we have seventy-five districts in our sample.

The second analysis examines whether the legalization of abortion had an effect on the total number of children a woman had at the time of the survey. We estimate

$$N_{jt} = \alpha + \beta_1 FirstbornGirl_j \times Post_t^1 + \beta_2 FirstbornGirl_j \times Post_t^2 + \gamma FirstbornGirl_j + \sigma Post_t^1$$
$$+ \psi Post_t^2 + X_j'\tau + \delta_d + \nu_d FirstbornGirl_j + \theta_d Post_t^1 + \omega_d Post_t^2 + \epsilon jt. \quad (3)$$

for mother j from district d who has had N_{jt} children as of the year of the survey t. To ensure consistent exposure, we restrict the sample to mothers who had all their births within a single period—either pre-legalization, early legalization (2003–2007), or late legalization (2008-2018). Thus, $Post_t^1$ and $Post_t^2$ indicate that the woman began and completed childbearing during the early or late legalization periods, respectively. As before, $FirstbornGirl_j$ is an indicator for whether the woman's first child was a girl. The vector $\boldsymbol{X}_j$ includes controls for household wealth quintiles, the

[13]Since a full set of district–year dummies already absorbs the separate year and district effects, ω_t and δ_d are only included in columns without θ_{dt}.

education levels of the woman and her husband, rural residence, and the woman's birth year. The coefficients β_1 and β_2 test our hypothesis that there was less son bias in fertility decisions after the legalization. We interpret these coefficients as the change in fertility outcomes after legalization for firstborn-girl families *relative* to firstborn-boy families.

6 Results

This section presents the effects of abortion legalization on fertility and child investments in Nepal. We first examine fertility outcomes, focusing on son-biased fertility stopping behavior, and then turn to parental investments, including breastfeeding, vaccination, and child survival.

6.1 Effects on Fertility

In Tables 3 and 4, we present the impacts of the introduction of abortion legalization on son-biased fertility stopping. The results provide clear evidence that abortion legalization significantly reduced son-biased fertility stopping. Prior to legalization, families with a firstborn daughter were substantially more likely to continue childbearing than those with a firstborn son. The coefficient of $FirstbornGirl_i$ is positive and significant, confirming that the women whose first child was a girl were 4 percentage points (p-value < 0.01) more likely to give birth in a given year. This behavior reflects the well-documented "fertility-stopping rule" in South Asia, where households pursue additional births until they achieve their desired number of sons (Gupta (1987); Bhat and Zavier (2003)). Consistent with this pattern, our estimates show that mothers with a firstborn daughter were more likely to give birth in any given year and had, on average, more children over their reproductive span compared to firstborn-boy mothers.

After the 2002 reform, these gaps narrowed dramatically. The difference in sibship size between firstborn-girl and firstborn-boy families declined by nearly three-quarters (p-value < 0.01), suggesting that families no longer needed to rely as heavily on continued childbearing to achieve desired composition. The effects are already noticeable in the early legalization period and become more pronounced in the late legalization period.

Our empirical strategy incrementally addresses several concerns. Column (1) includes only the baseline triple-difference terms. Column (2) adds child gender-specific cohort fixed effects to

Table 2: Test of Balance in Samples by the Sex of the Firstborn

	1990-2002		2003-2007		2008-2018		All years
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	FB	FG	FB	FG	FB	FG	FB-FG
Rural	0.67	0.67	0.58	0.59	0.48	0.48	-0.0083
Mother's education							
No education	0.63	0.64	0.41	0.41	0.22	0.22	-0.00013
Incomplete secondary	0.31	0.30	0.47	0.47	0.56	0.57	0.00055
Secondary or higher	0.06	0.06	0.12	0.12	0.22	0.21	-0.00039
Father's education							
No education	0.23	0.24	0.15	0.15	0.09	0.09	0.0010
Incomplete secondary	0.30	0.30	0.32	0.31	0.31	0.32	0.0016
Secondary or higher	0.47	0.46	0.54	0.54	0.59	0.59	-0.0026
Mother's birth cohort							
1960-1975	0.35	0.37	0.01	0.01	0.00	0.00	-0.0061
1975-1990	0.50	0.48	0.95	0.95	0.37	0.39	0.0030
1990-2005	0.00	0.00	0.02	0.02	0.63	0.61	0.0059
Mother's age at birth							
12-15	0.04	0.03	0.03	0.03	0.02	0.02	0.0022
16-18	0.33	0.33	0.28	0.30	0.26	0.25	-0.0087
19-24	0.56	0.56	0.59	0.59	0.59	0.60	-0.0028
25-30	0.07	0.07	0.09	0.08	0.11	0.11	0.0025
31-49	0.01	0.01	0.01	0.01	0.02	0.02	-0.0011
Household wealth							
Second quintile	0.19	0.20	0.20	0.19	0.20	0.20	-0.0034
Third quintile	0.20	0.19	0.19	0.19	0.21	0.20	0.0061
Fourth quintile	0.20	0.18	0.18	0.19	0.20	0.20	0.0077*
Richest quintile	0.20	0.20	0.18	0.18	0.14	0.15	-0.0026

Notes: This table compares the socioeconomic characteristics of firstborn-boy (FB) and firstborn-girl (FG) families during the pre-legalization period and the two post-legalization periods in the DHS sample. The sample is restricted to first births, as only these are quasi-random. Column (7) shows the difference in sample means for the entire sample. $p < 0.1^{*} p < 0.05^{**} p < 0.01^{***}$

absorb nationwide shifts in gender gaps, such as declining son preference or improvements in maternal health that might differentially benefit boys. Column (3) adds firstborn-sex specific cohort fixed effects, ensuring that trends particular to firstborn-girl families (e.g., changes in stopping behavior) are not driving our results. In subsequent specifications, we incorporate district-year fixed effects, birth order controls, and years-since-last-birth controls, which together account for local shocks, biological differences across parity, and fertility spacing dynamics. Across all specifications, the coefficients of interest remain stable, increasing confidence that the results reflect the effect of abortion legalization rather than omitted trends.

Table 3: Probability of Birth

	(1)	(2)	(3)	(4)	(5)
FirstbornGirl	0.0440***	0.0463***			
	(0.0021)	(0.0022)			
FirstbornGirl X Post 1	-0.0234***	-0.0275***	-0.0274***	-0.0272***	-0.0271***
	(0.0027)	(0.0029)	(0.0029)	(0.0029)	(0.0030)
FirstbornGirl X Post 2	-0.0292***	-0.0353***	-0.0358***	-0.0353***	-0.0351***
	(0.0027)	(0.0028)	(0.0029)	(0.0029)	(0.0030)
FirstbornGirl X Ideal fraction of sons					0.0587***
					(0.0080)
FirstbornGirl X Ideal number of children					-0.0036*
					(0.0019)
N	566,868	566,867	566,867	560,993	560,993
Demographic Controls	×	×	×	×	×
Year FEs	×	×	×	×	×
District FEs	×	×	×	×	×
Parity FEs	×	×	×	×	×
District × year FEs	×	×	×	×	×
Years since last birth FEs		×	×	×	×
FirstbornGirl × district FEs			×	×	×

Notes: Coefficients from specification 2 estimated using OLS regression on the mother-year sample from the year of their marriage to the year of interview. The dependent variable is an indicator for whether a mother gave birth in a given year. Post 1 indicates the early legalization period from 2003-2007 and Post 2 indicates the late legalization period from 2008-2018. The sample includes all mothers who have ever given birth. SEs in parentheses are clustered by district. Data: NDHS. $p < 0.1^{*} p < 0.05^{**} p < 0.01^{***}$

The findings of Table 4 confirm the results of Table 3, and we see that the coefficient of $FirstbornGirl_j$ is positive and significant. Women with a firstborn girl child had 0.257 (p-value < 0.01) more births than women with a firstborn son. Column (3) of the table shows that the pre-legalization gap in the number of births declined by 0.099 to 0.160, or by 4% to 7%, compared to the baseline mean of 2.28 births.

Because access to abortion can reduce unwanted births, we test whether legalization brought actual fertility closer to desired fertility. We construct excess fertility as actual minus desired fertility. Column (4) of Table 4 shows large reductions in excess fertility after legalization. At baseline, the first row indicates that firstborn-girl families had 0.124 more births than desired relative to firstborn-boy families. The post-legalization coefficients for excess fertility in column (4) closely match those for actual fertility in column (1), indicating that the decline in births among firstborn-girl families reflects fewer unwanted births rather than a change in desired fertility.

Table 4 also shows that actual fertility increases with both the desired number of children and the desired son-to-daughter ratio. By contrast, excess fertility (actual minus desired) declines as the desired son share rises. This pattern implies that desired fertility rises more steeply with son preference than realized fertility. The interpretation is straightforward: parents who value sons plan for more births to reach their target composition through son-biased stopping, but conception, timing, and sex realization are not fully under their control, so actual fertility increases less than proportionately relative to stated desire, and excess fertility falls with the desired son share.

Table 4: Fertility

	Number of Births			Excess Fertility	
	(1)	(2)	(3)	(4)	(5)
FirstbornGirl	0.2019***	0.1750***	0.2571***	0.1419***	0.1235***
	(0.0188)	(0.0192)	(0.0780)	(0.0216)	(0.0214)
FirstbornGirl X Post 1	-0.1811***	-0.1518***	-0.1600***	-0.2017***	-0.1053***
	(0.0270)	(0.0319)	(0.0349)	(0.0359)	(0.0380)
FirstbornGirl X Post 2	-0.0842***	-0.0907***	-0.0997***	-0.1058***	-0.0556*
	(0.0241)	(0.0264)	(0.0279)	(0.0299)	(0.0296)
Ideal number of children		0.1782***	0.1985***		
		(0.0189)	(0.0313)		
Ideal fraction of sons		0.0654***	0.0603*		-0.5622***
		(0.0246)	(0.0315)		(0.0216)
FirstbornGirl X Ideal fraction of sons			0.0013		
			(0.0409)		
FirstbornGirl X Ideal number of children			-0.0342		
			(0.0404)		
N	18,347	13,121	13,121	18,269	13,121
Baseline mean	2.28	2.28	2.28	-.0252	-.0252

Notes: Coefficients from specification 3 estimated using OLS regression. The dependent variable in columns (1) to (3) is the number of births at the time of interview, and the dependent variable in columns (4) to (5) is the excess fertility, which equals the number of births minus the self-reported ideal number of children. Sample includes all mothers who had both their first birth and last birth within the Pre, Post1, and Post2 periods. Post 1 indicates the early legalization period from 2003-2007 and Post 2 indicates the late legalization period from 2008-2018. Baseline means are the average of the outcome variable in each column for mothers who had both their first and last births within 1990–2002. SEs in parentheses are clustered by district. Data: NDHS. $p < 0.1^{*} p < 0.05^{**} p < 0.01^{***}$

Birth Spacing

If it is the availability of sex-selective abortion rather than something else that is driving our findings, then we should see an increase in birth spacing in the post-legalization period among families with firstborn girls relative to other families. We check for this in Table 5. The estimates show that before legalization, firstborn-girl mothers had shorter birth intervals between their first and second children, about seven months sooner on average, relative to firstborn-boy mothers. After legalization, this gap narrowed significantly, suggesting that families relied less on manipulating birth spacing once abortion became available as a fertility control tool. This adjustment is consistent with the dynamic margin highlighted in the framework in Appendix A, where investments such as breastfeeding reduce the probability of conception, and legalization reduces the incentive to manipulate such investments to achieve desired composition.

Table 5: Birth spacing: Months between first and second births

	(1) No. of months
Firstborn girl	-7.265***
	(0.2880)
Firstborn girl * Post	1.933**
	(0.7827)
N	11,200
Baseline mean	31.48

Notes: OLS regression. The sample includes mothers who had at least 2 births at the time of the interview. The dependent variable is the number of months between births. Post refers to the post-legalization period as a whole (2003-2018). Baseline mean refers to the average birth spacing duration in the pre-legalization period. Standard errors in parentheses are clustered by district. Data: NDHS. $p < 0.1^{*} p < 0.05^{**} p < 0.01^{***}$

Sex Ratio at Birth

Table 6 reports coefficients that capture the impact of the reform on the probability that a birth is female among post-reform cohorts in families with a firstborn girl relative to families with a firstborn boy. Our estimates in Column 1 indicate a significant decline in the probability that a birth is a girl by 1.8 percentage points. These results are robust to the inclusion of household-level control variables (Column 2), district-specific linear trends, and district-year fixed effects (Column 5). Consistent with sex selection operating at higher parities, the sex ratio among first births remains normal, while the post-reform decline is concentrated at second and third births; the effect is larger in Stage 2 (2008–2018) than in Stage 1 (2003–2007). The magnitude is comparable to related contexts: 1.8 p.p. in India with political representation (Bhalotra et al., 2018) and 2.8 p.p. in China after land reform for second births following a firstborn girl (Almond et al., 2019), and aligns with Taiwan's experience of 2.0 p.p. higher sex ratios at birth for higher parity births under abortion plus sex detection (Lin et al., 2014). Finally, the 1.8 p.p. decline implies a meaningful selection margin: our back-of-the-envelope calculation suggests about 1 in 75 girls is missing from post-reform higher-parity births in firstborn-girl families.

Table 6: Sex Ratios at Birth

	(1)	(2)	(3)	(4)	(5)
Firstborn Girl	-0.0851	-0.0601	-0.0514	-0.0337	-0.0442
	(0.3954)	(0.4066)	(0.4071)	(0.4068)	(0.4196)
Firstborn girl * Post1	-0.7362	-0.7249	-0.7344	-0.7355	-0.8535
	(0.8635)	(0.8675)	(0.8689)	(0.8700)	(0.9035)
Firstborn girl * Post2	-1.8609**	-1.9151**	-1.8262**	-1.8235**	-1.8647**
	(0.8011)	(0.8034)	(0.8116)	(0.8129)	(0.7961)
N	70,512	70,494	70,493	70,491	70,458
Baseline mean	49.1	49.1	49.1	49.1	49.1
District FEs	×	×	×	×	×
Child year of birth FEs	×	×	×	×	×
X_{ijt}		×	×	×	×
Mother year of birth FEs			×	×	×
Additional X_{ijt}				×	×
District linear trends					×
District-year of birth FEs					×

Notes: OLS regression. The dependent variable is a dummy that equals 1 if birth is female and zero otherwise, multiplied by 100. Post 1 indicates the early legalization period from 2003-2007, and Post 2 indicates the late legalization period from 2008-2018. Baseline mean refers to the average likelihood of female births in the pre-legalization period. X_{ijt} comprises indicators for household wealth quintiles, mother's birth cohort, religion, and rural residence. Additional X_{ijt} consists of parents' educational attainment. Standard errors in parentheses are clustered by district. Data: NDHS. $p < 0.1^{*} p < 0.05^{**} p < 0.01^{***}$

6.2 Effects on Child Investments

We present the estimates of the impact of abortion legalization on parental health investments in Tables 7 to 10, presented in specification (1).

6.2.1 Breastfeeding

Turning to parental investments, we find strong evidence that abortion legalization narrowed gender gaps in breastfeeding duration. Table 7 reports the estimated impact of legalization on breastfeeding behavior. Additional control variables are sequentially incorporated across columns from left to right. The coefficient on $FirstbornGirl \times Female$ for breastfeeding in the table confirms that before the reform, daughters in firstborn-girl families were breastfed for 1.7–2.0 months less (p-value < 0.01) than their brothers, reflecting a behavioral pattern documented elsewhere in South Asia (Jayachandran and Kuziemko (2011)). This is consistent with the notion that breastfeeding doubles as a form of birth spacing: families eager to conceive again may shorten breastfeeding for daughters to accelerate the next pregnancy, particularly if they have not yet had a son.

The triple interaction coefficients, $FirstbornGirl_j \times Female_i \times Post_t^1$ and $FirstbornGirl_j \times Female_i \times Post_t^2$ indicate that this disadvantage declined significantly after legalization. In the late post-reform period, daughters in firstborn-girl families breastfed about two months longer (p < 0.01), nearly closing the baseline gap. These findings suggest that with abortion available as an alternative means of fertility control, parents no longer need to manipulate breastfeeding to pursue son-biased fertility goals.

Table 9 decomposes the results by wealth, rural residence, and maternal education. The gap in breastfeeding duration between firstborn-girl and firstborn-boy families is concentrated among poorer, rural, and less-educated households. This pattern is consistent with breastfeeding functioning as a low-cost spacing strategy where access to modern contraception is limited and reliance on traditional methods is higher.

To mitigate any censoring bias, we restrict the sample to the last two surviving births and to children older than two. We also report decomposed estimates by firstborn-sex families to confirm that the observed narrowing is not driven by compositional differences. Table 8 presents these decomposed estimates. Among firstborn-girl families, daughters were breastfed 2.21 months less before legalization (p < 0.001) but gained 2.20 months afterward (p < 0.001), effectively eliminating the gap. In firstborn-boy families, the baseline difference between sons and daughters was small (–0.19 months, p $= 0.67$) and did not change significantly after legalization (–1.12 to +0.29 months, p $= 0.26$–0.78). The main effects in Table 7 are therefore driven by improvements in firstborn-girl families.

Table 7: Breastfeeding as a Function of Abortion Legalization and the Sex of the Firstborn

	(1)	(2)	(3)	(4)	(5)
Firstborn girl * Female	-1.9927***	-1.9507***	-1.7811**	-1.8940***	-1.7360**
	(0.6728)	(0.6616)	(0.6758)	(0.6559)	(0.6710)
Firstborn girl * Female * Post1	1.7713*	1.8381*	1.8185*	1.7643*	1.7534*
	(0.9853)	(0.9636)	(0.9578)	(0.9562)	(0.9505)
Firstborn girl * Female * Post2	1.9835***	1.9423***	1.7800**	1.8093***	1.6616**
	(0.6736)	(0.6680)	(0.6741)	(0.6702)	(0.6785)
N	5,856	5,851	5,809	5,851	5,809
Baseline mean	19.3	19.3	19.3	19.3	19.3
X_{ijt}	×	×	×	×	×
Age FEs		×	×	×	×
FirstbornGirl × birth year FEs			×		×
Female × birth year FEs			×		×
Female × district FEs			×		×
Female × birth order FEs			×		×
Birth order × district FEs			×		×
District-specific time trends			×		×
FirstbornGirl × district FEs			×		×
FirstbornGirl × birth order FEs			×		×
Additional X_{ijt}				×	×

Notes: This table reports breastfeeding effects (in months) for children of second- and higher-order birth order. Results are based on the last two surviving births of a mother, and we restrict the sample to children above age two. Coefficients are from the specification 1 estimated using OLS regression. X_{ijt} comprises indicators for household wealth quintiles, mother's birth cohort, religion, and rural residence. Additional X_{ijt} consists of parents' educational attainment. Post 1 indicates the early legalization period from 2003-2007, and Post 2 indicates the late legalization period from 2008-2018. Baseline mean refers to average breastfeeding duration by mothers in the pre-legalization period. SEs in parentheses are clustered by district. Data: NDHS. $p < 0.1^{*} p < 0.05^{**} p < 0.01^{***}$

Table 8: Breastfeeding by Firstborn Sex

	Firstborn girl family			Firstborn boy family		
	(1)	(2)	(3)	(4)	(5)	(6)
Female	-2.2052***	-2.2006***	-2.1790***	-0.1877	-0.2179	-0.2525
	(0.5157)	(0.5100)	(0.5093)	(0.4505)	(0.4492)	(0.4478)
Female * Post1	0.7787	0.7839	0.7784	-1.0048	-1.1174	-1.0769
	(0.7358)	(0.7189)	(0.7223)	(0.6669)	(0.6832)	(0.6836)
Female * Post2	2.1996***	2.1989***	2.1225***	0.2444	0.2869	0.3485
	(0.5185)	(0.5166)	(0.5225)	(0.4419)	(0.4366)	(0.4430)
N	3,079	3,077	3,077	2,772	2,767	2,767
Baseline mean	18.8	18.8	18.8	19.3	19.3	19.3

Notes: This table reports breastfeeding effects (in months) for children of second- and higher-order birth order. Results are based on the last two surviving births of a mother, and we restrict the sample to children above age two. Each column is a separate OLS regression. Post 1 indicates the early legalization period from 2003-2007 and Post 2 indicates the late legalization period from 2008-2018. Baseline mean refers to average breastfeeding duration by mothers in the pre-legalization period. SEs in parentheses are clustered by district. Data: NDHS. $p < 0.1^{*} p < 0.05^{**} p < 0.01^{***}$

Table 9: Breastfeeding: Heterogeneity by Socioeconomic Status

	Mother's education		Wealth		Rurality	
	(1) Illiterate	(2) Literate	(3) Bottom 40%	(4) Top 40%	(5) Rural	(6) Urban
Firstborn girl * Female	-2.0889***	-0.2412	-2.4216**	-1.2244	-1.6887**	0.4566
	(0.7709)	(1.5559)	(1.1857)	(0.8494)	(0.7279)	(1.4490)
Firstborn girl * Female * Post1	2.6492**	-0.8513	1.7131	1.5379	1.5255	-0.6409
	(1.1633)	(1.7588)	(1.7832)	(1.1356)	(1.0265)	(2.3371)
Firstborn girl * Female * Post2	2.0511**	0.4293	2.4880**	1.4420*	1.6455**	-0.8010
	(0.8383)	(1.5634)	(1.1988)	(0.8618)	(0.7570)	(1.5082)
N	3,848	1,905	1,642	3,052	4,436	1,313
Baseline mean	18.6	19.1	18.8	18.9	18.6	20.2

Notes: The dependent variable is breastfeeding (in months) for children of second- and higher-order birth order. Results are based on the last two surviving births of a mother, and we restrict the sample to children above age two. Coefficients are from the specification 1. Each column within a panel is a separate OLS regression. Post 1 indicates the early legalization period from 2003-2007, and Post 2 indicates the late legalization period from 2008-2018. SEs in parentheses are clustered by district. Baseline mean is the mean breastfeeding duration during the pre-legalization period by mothers with the specific socioeconomic status. Data: NDHS. $p < 0.1^{*} p < 0.05^{**} p < 0.01^{***}$

6.2.2 Vaccination and Mortality

For other dimensions of child health, the effects are less pronounced. Table 10 shows that before legalization, vaccination rates were only slightly higher for boys in firstborn-girl families, and this modest difference did not narrow significantly after the reform. The small initial gap helps explain the absence of large post-reform changes: unlike breastfeeding, where girls faced a clear disadvantage, vaccination practices were already relatively equitable across family types.

This stability likely reflects Nepal's long-standing national immunization efforts. The National Immunization Program (NIP), launched in 1979 as part of the World Health Organization's Expanded Program on Immunization (EPI), sought to reduce childhood morbidity and mortality from vaccine-preventable diseases (Ministry of Health, 1979). Because all EPI vaccines are provided free of charge, coverage among children under 12 months increased sharply, with the largest gains among poorer and less-educated households. These improvements substantially narrowed equity gaps in vaccination over time (Ashish et al., 2017). This is further evident from the heterogeneity analysis in Table A4. We see that most of the gap in vaccination rates between firstborn girl versus firstborn boy families are concentrated among families in the top 40% of the wealth quintiles.

Turning to survival outcomes, Table 11 shows that before legalization, girls with a firstborn sister were significantly more likely to die before age five than those with a firstborn brother. This excess female mortality gap narrowed in the early post-legalization period, although the improve-

Table 10: Vaccination as a Function of Abortion Legalization and the Sex of the Firstborn

	(1)	(2)	(3)	(4)	(5)
Firstborn girl * Female	-0.1936*	-0.1948*	-0.2134*	-0.1882*	-0.2052*
	(0.1083)	(0.1078)	(0.1083)	(0.1054)	(0.1063)
Firstborn girl * Female * Post1	0.1725	0.1702	0.1447	0.1955	0.1681
	(0.2159)	(0.2135)	(0.1962)	(0.2084)	(0.1925)
Firstborn girl * Female * Post2	0.2061	0.2057	0.1891	0.2175	0.1990
	(0.1832)	(0.1800)	(0.1767)	(0.1808)	(0.1764)
N	13,404	13,403	13,372	13,403	13,372
Baseline mean	5.84	5.84	5.84	5.84	5.84
X_{ijt}	×	×	×	×	×
Age FEs		×	×	×	×
FirstbornGirl × birth year FEs			×		×
Female × birth year FEs			×		×
Female × district FEs			×		×
Female × birth order FEs			×		×
Birth order × district FEs			×		×
District-specific time trends			×		×
FirstbornGirl × district FEs			×		×
FirstbornGirl × birth order FEs			×		×
AdditionalX_{ijt}				×	×

Notes: This table reports vaccination effects for children of second- and higher-order birth order. Coefficients are from the specification 1 estimated using OLS regression. X_{ijt} comprises indicators for household wealth quintiles, mother's birth cohort, religion, and rural residence. Additional X_{ijt} consists of parents' educational attainment. Post 1 indicates the early legalization period from 2003-2007 and Post 2 indicates the late legalization period from 2008-2018. Baseline mean is the mean number of vaccinations received during the pre-legalization period. SEs in parentheses are clustered by district. Data: NDHS. $p < 0.1^{*} p < 0.05^{**} p < 0.01^{***}$

ment did not persist in the long run. These short-lived effects mirror findings from Taiwan (Lin et al., 2014) and parts of India (Hu and Schlosser, 2015), where access to abortion or prenatal sex detection improved relative female survival temporarily but not consistently across all contexts. In Nepal, legalization may have mitigated the most severe forms of postnatal discrimination immediately after the reform, yet entrenched cultural norms appear to have constrained longer-term progress.

Child mortality is shaped by a range of structural factors beyond fertility behavior, including health system expansion, sanitation improvements, and broader declines in under-five mortality. To isolate the effect of abortion legalization, we include child gender-specific cohort fixed effects, which absorb national trends in survival that differ by gender, such as improvements in prenatal or neonatal care that disproportionately benefit boys. District-year fixed effects further control for localized health initiatives and infrastructure expansion. These adjustments ensure that the observed short-run reduction in excess female mortality among firstborn-girl families reflects the impact of abortion legalization rather than concurrent nationwide improvements in survival.

Taken together, these results suggest that while legalization eased fertility pressures and improved breastfeeding outcomes, it did not generate broad-based equalization across all child health inputs. This pattern aligns with the model's prediction that investment responses are strongest for inputs directly linked to fertility behavior, whereas those less tied to fertility decisions exhibit weaker or no effects.

6.3 Robustness Checks

The baseline results establish that abortion legalization reduced son-biased fertility stopping and narrowed certain gender gaps in parental investments, particularly breastfeeding. In this section, we present a series of robustness checks to address potential concerns about exposure, measurement, and alternative mechanisms.

Consistent exposure to legalization

A first concern is that women whose reproductive spans straddled different policy regimes may introduce bias if part of their fertility occurred before legalization and part after. To address this, we restrict the sample to mothers whose entire fertility occurred within a single period, either pre-

Table 11: Excess Female Under-Five Mortality

	(1)	(2)	(3)	(4)	(5)
Firstborn girl * Female	0.0190**	0.0181*	0.0183**	0.0177*	0.0179**
	(0.0090)	(0.0092)	(0.0088)	(0.0091)	(0.0087)
Firstborn girl * Female * Post1	-0.0424**	-0.0412**	-0.0378*	-0.0405**	-0.0370*
	(0.0188)	(0.0189)	(0.0190)	(0.0189)	(0.0190)
Firstborn girl * Female * Post2	0.0028	0.0043	0.0014	0.0053	0.0024
	(0.0157)	(0.0158)	(0.0159)	(0.0157)	(0.0157)
N	37,089	37,088	37,060	37,087	37,059
Baseline mean	.102	.102	.102	.102	.102
X_{ijt}	×	×	×	×	×
Age FEs		×	×	×	×
FirstbornGirl × birth year FEs			×		×
Female × birth year FEs			×		×
Female × district FEs			×		×
Female × birth order FEs			×		×
Birth order × district FEs			×		×
District-specific time trends			×		×
FirstbornGirl × district FEs			×		×
FirstbornGirl × birth order FEs			×		×
Additional X_{ijt}				×	×

Notes: Sample of second- and higher-order births. Each column is a separate OLS regression. The outcome is an indicator of death before age five. We drop children who are less than five years old to allow each child in the sample full exposure to the risk of under-five mortality. X_{ijt} comprises indicators for household wealth quintiles, mother's birth cohort, religion, and rural residence. Additional X_{ijt} consists of parents' educational attainment. Post 1 indicates the early legalization period from 2003-2007 and Post 2 indicates the late legalization period from 2008-2018. Baseline mean refers to the average likelihood of under-five mortality in the pre-legalization period. SEs in parentheses are clustered by district. Data: NDHS. $p < 0.1^{*} p < 0.05^{**} p < 0.01^{***}$

legalization, early post-legalization (2003–2007), or late post-legalization (2008–2018). As shown in Table 12, the results remain qualitatively similar: firstborn-girl mothers had higher fertility than firstborn-boy mothers in the pre-legalization period, but this gap narrowed substantially after legalization. This strengthens confidence that the main results are not driven by inconsistent exposure across cohorts.

Table 12: Probability of Birth (Fertility Sample)

	(1)	(2)	(3)	(4)	(5)
FirstbornGirl	0.0302***	0.0334***			
	(0.0022)	(0.0023)			
FirstbornGirl X Post 1	-0.0282***	-0.0404***	-0.0389***	-0.0389***	-0.0343***
	(0.0051)	(0.0053)	(0.0054)	(0.0054)	(0.0054)
FirstbornGirl X Post 2	-0.0064	-0.0135***	-0.0152***	-0.0152***	-0.0113**
	(0.0043)	(0.0046)	(0.0045)	(0.0045)	(0.0045)
FirstbornGirl X Ideal fraction of sons					0.0849***
					(0.0140)
FirstbornGirl X Ideal number of children					0.0115***
					(0.0027)
N	214,070	214,070	214,070	214,070	211,970
Demographic Controls	×	×	×	×	×
Year FEs	×	×	×	×	×
District FEs	×	×	×	×	×
Parity FEs	×	×	×	×	×
District × year FEs	×	×	×	×	×
Years since last birth FEs		×	×	×	×
FirstbornGirl × district FEs			×	×	×

Notes: Coefficients from specification 2 estimated using OLS regression on the mother-year sample from the year of their marriage to the year of interview. The sample is restricted to mothers whose entire fertility occurred within a single period, either pre-legalization, early post-legalization (2003–2007), or late post-legalization (2008–2018). The dependent variable is an indicator for whether a mother gave birth in a given year. The sample includes all mothers who have ever given birth. SEs in parentheses are clustered by district. Data: NDHS. $p < 0.1^{*} p < 0.05^{**} p < 0.01^{***}$

Effects of ultrasound on fertility

Second, we address concerns about concurrent changes in access to prenatal sex detection in Table 13. Using a proxy for the diffusion of prenatal sex detection, we replicate Anukriti et al. (2022) on the probability of an additional birth: for firstborn-girl families, the probability of a subsequent birth declines by about 1.1–2.0 percentage points in the early post-ultrasound period and by about 3.2–4.2 percentage points in the later period, closely matching prior magnitudes. In our main specification, 1990–2002 serves as the pre-period and ultrasound diffusion is absorbed into the baseline; identification rests on parallel pre-trends across firstborn-girl and firstborn-boy families, so any post-2002 divergence is attributed to legalization.

Table 13: Probability of Birth (Effects of Ultrasound)

	(1)	(2)	(3)	(4)	(5)
FirstbornGirl	0.0498***	0.0589***			
	(0.0050)	(0.0055)			
FirstbornGirl X Post 1	-0.0109**	-0.0204***	-0.0203***	-0.0197***	-0.0196***
	(0.0049)	(0.0055)	(0.0056)	(0.0055)	(0.0054)
FirstbornGirl X Post 2	-0.0324***	-0.0428***	-0.0427***	-0.0422***	-0.0419***
	(0.0053)	(0.0059)	(0.0060)	(0.0058)	(0.0057)
FirstbornGirl X Ideal fraction of sons					0.0567***
					(0.0120)
FirstbornGirl X Ideal number of children					-0.0015
					(0.0022)
N	323,020	323,018	323,018	319,136	319,136

Notes: Coefficients from specification 2 estimated using OLS regression on the mother-year sample from the year of their marriage to the year of interview. The dependent variable is an indicator for whether a mother gave birth in a given year. Post 1 indicates the early ultrasound diffusion period from 1990-1994 and Post 2 indicates the late ultrasound diffusion period from 1995-2002. The sample includes all mothers who have ever given birth. SEs in parentheses are clustered by district. Data: NDHS. $p < 0.1^{*} p < 0.05^{**} p < 0.01^{***}$

Alternative definitions of sex composition

Finally, we address the concern that the gender of the first child is not a strong predictor of the children's gender composition at any given time. We examine whether the fertility effects are robust to alternative definitions of household gender composition. We use the alternative definitions of gender compostion from Alfano (2017) in Table 14, which presents specifications using the gender of the most recent child, the ratio of girls to boys in the family, and a dummy for whether daughters outnumber sons. Across all three measures, families with more daughters exhibited higher fertility in the pre-legalization period, consistent with son-biased stopping behavior. After legalization, these gaps diminished significantly. This robustness exercise shows that the results do not depend solely on defining incentives by the sex of the firstborn but rather reflect a broader pattern of son preference in fertility behavior.

Taken together, these robustness checks reinforce the central conclusion: abortion legalization reduced son-biased fertility behavior, both by lowering overall fertility gaps and by altering the timing and spacing of births. The consistency of the findings across different definitions of exposure and composition further strengthens the interpretation that the reform shifted family formation dynamics in a way consistent with the quantity–quality trade-off model, where constraints on fertility are relaxed and the marginal cost of reducing family size declines.

Table 14: Alternative definitions of children's gender composition

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable: Birth indicator						
	Measurement of G_i					
	Youngest child is female		Ratio of girls to boys		More girls than boys	
G_i	0.025***	0.028***	0.050***	0.070***	0.050***	0.0559***
	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)	(0.003)
$G_i * Post$		-0.007*		-0.037***		-0.012***
		(0.003)		(0.002)		(0.004)
District specific trends	yes	yes	yes	yes	yes	yes
Observations	190,481	190,481	131,588	131,588	206,881	206,881

Notes: Youngest child is female is a dummy variable taking the value 1 if the youngest child born before year t is a girl; Ratio of girls to boys is the total number of girls born by year t divided by the total number of boys; More girls than boys is a dummy variable taking the value 1 if in year t the total number of girls exceeds the total number of boys. SEs in parentheses are clustered by district. Data: NDHS. $p < 0.1^{*} p < 0.05^{**} p < 0.01^{***}$

7 Conclusion

This paper has examined the effects of abortion legalization in Nepal on fertility and gender gaps in child investments, situating the analysis within the broader framework of the quantity–quality trade-off. Consistent with Becker and Lewis (1973), the results show that expanding women's reproductive autonomy allowed families to reduce fertility, particularly among those with a firstborn daughter, who previously faced stronger incentives to continue childbearing. The narrowing of sibling size gaps between firstborn-girl and firstborn-boy families by as much as three-fourths (as evidenced in Table 4) provides direct evidence that abortion reform mitigated son-biased fertility stopping. By lowering the effective "price" of limiting fertility while maintaining desired composition, legalization shifted households closer to their fertility ideals without the same costs for daughters. At the same time, the reform increased scope for prenatal sex selection at higher parities. Among later births in firstborn-daughter families, the probability that a birth is female fell by 1.8 percentage points (Table 6). A back-of-the-envelope calculation implies that roughly 1 in 75 girls is missing in post-reform cohorts. This selection margin helps explain some improvements for surviving girls but represents a clear demographic and ethical cost.

On the quality dimension, abortion legalization narrowed gender gaps in breastfeeding, a domain of investment strongly linked to both fertility timing and survival (Jayachandran and Kuziemko, 2011). Daughters in firstborn-girl families, who were previously disadvantaged, experienced substantial gains in breastfeeding duration after legalization. However, effects on other investments like vaccinations were muted, and mortality improvements were modest and short-lived. Taken together, these patterns suggest that abortion legalization's main effects operated through the fertility channel, with more selective improvements in child investments. These findings extend the Becker–Lewis model to a setting with strong son preference, highlighting how reproductive autonomy reshapes both fertility behavior and within-household investment patterns. This is consistent with reduction in use of breastfeeding as a birth-spacing mechanism and an increase in the "wantedness" of girls leading to higher investments in them.

From a policy perspective, the results indicate that legal reforms expanding reproductive rights can promote gender equity indirectly by reshaping fertility behavior, but their effects on child investments depend on broader social norms. Reproductive autonomy is a necessary but not sufficient condition for closing gender gaps. Complementary policies that strengthen maternal and child health systems, expand immunization coverage, and challenge discriminatory norms are essential for ensuring that gains in autonomy translate into lasting improvements in girls' well-being.

These results also carry implications for global health goals. Breastfeeding, identified by the World Health Organization (2023) and UNICEF (2021) as central to achieving the Sustainable Development Goals, responds not only to health policy but also to underlying reproductive constraints. By easing those constraints, abortion legalization fostered a more equitable environment for early-life investments, particularly for daughters. Yet achieving the SDG targets on child survival and nutrition ultimately requires aligning legal reforms with social change, so that gains in autonomy lead to sustained improvements in health and gender equality.

Finally, the findings speak to ongoing demographic transitions in South Asia and other settings where declining fertility coincides with persistent son preference. As access to reproductive technologies expands, the interaction between autonomy and cultural norms will remain central to shaping gender equity. Future research should explore how these reforms influence later outcomes such as education, labor market participation, and women's bargaining power, as well as their intergenerational consequences. Nepal's experience demonstrates both the transformative potential of legal access to abortion and the limits imposed by enduring social norms.

References

ADHIKARI, R. (2016): "Knowledge on legislation of abortion and experience of abortion among female youth in Nepal: A cross sectional study," *Reproductive health*, 13, 48. [2]

ALFANO, M. (2017): "Daughters, dowries, deliveries: The effect of marital payments on fertility choices in India," *Journal of Development Economics*, 125, 89–104. [2], [13], [30]

ALMOND, D., L. EDLUND, H. LI, AND J. ZHANG (2010): "Long-term effects of early-life development: Evidence from the 1959 to 1961 China famine," in *The Economic Consequences of Demographic Change in East Asia*, University of Chicago Press, 321–345. [5], [7]

ALMOND, D., H. LI, AND S. ZHANG (2019): "Land reform and sex selection in China," *Journal of Political Economy*, 127, 560–585. [22]

ANANAT, E. O., J. GRUBER, P. B. LEVINE, AND D. STAIGER (2009): "Abortion and selection," *The Review of Economics and Statistics*, 91, 124–136. [4]

ANDERSON, S. AND D. RAY (2010): "Missing women: age and disease," *The Review of Economic Studies*, 77, 1262–1300. [1], [47]

ANGRIST, J., V. LAVY, AND A. SCHLOSSER (2010): "Multiple experiments for the causal link between the quantity and quality of children," *Journal of Labor Economics*, 28, 773–824. [6]

ANUKRITI, S., S. BHALOTRA, AND E. H. TAM (2022): "On the quantity and quality of girls: Fertility, parental investments and mortality," *The Economic Journal*, 132, 1–36. [1], [2], [3], [4], [5], [7], [13], [29], [45]

ASHISH, K., V. NELIN, H. RAAIJMAKERS, H. J. KIM, C. SINGH, AND M. MÅLQVIST (2017): "Increased immunization coverage addresses the equity gap in Nepal," *Bulletin of the World Health Organization*, 95, 261. [4], [25]

BAILEY, M. J. (2006): "More power to the pill: The impact of contraceptive freedom on women's life cycle labor supply," *The Quarterly Journal of Economics*, 121, 289–320. [1], [5]

BECKER, G. S. (1960): "An economic analysis of fertility," in *Demographic and Economic Change in Developed Countries*, Columbia University Press, 209–240. [1]

BECKER, G. S. AND H. G. LEWIS (1973): "On the interaction between the quantity and quality of children," *Journal of Political Economy*, 81, S279–S288. [1], [6], [31]

BECKER, G. S. AND N. TOMES (1976): "Child endowments and the quantity and quality of children," *Journal of Political Economy*, 84, S143–S162. [1], [6]

BHALOTRA, S., I. CLOTS-FIGUERAS, AND L. IYER (2018): "Pathbreakers? Women's electoral success and future political participation," *The Economic Journal*, 128, 1844–1878. [22]

BHALOTRA, S. R. AND T. COCHRANE (2010): "Where have all the young girls gone? Identification of sex selection in India," . [13]

BHAT, P. M. AND A. F. ZAVIER (2003): "Fertility decline and gender bias in Northern India," *Demography*, 40, 637–657. [6], [17]

BIEN, C. H., Y. CAI, M. E. EMCH, W. PARISH, AND J. D. TUCKER (2013): "High adult sex ratios and risky sexual behaviors: A systematic review," *PloS one*, 8. [8]

BLACK, S. E., P. J. DEVEREUX, AND K. G. SALVANES (2005): "The more the merrier? The effect of family size and birth order on children's education," *The Quarterly Journal of Economics*, 120, 669–700. [6]

BRUNSON, J. (2010): "Son preference in the context of fertility decline: limits to new constructions of gender and kinship in Nepal," *Studies in Family Planning*, 41, 89–98. [7]

CENTERS FOR DISEASE CONTROL AND PREVENTION, NATIONAL CENTER FOR HEALTH STATISTICS (2025): "National Vital Statistics System, Natality, 2016–2024," CDC WONDER Online Database, data are from the Natality Records 2016–2024, as compiled from data provided by the 57 vital statistics jurisdictions through the Vital Statistics Cooperative Program. Accessed at http://wonder.cdc.gov/natality.html. [48]

CLARKE, D. AND H. MÜHLRAD (2016): "The impact of abortion Legalization on fertility and maternal mortality: new evidence from Mexico," Tech. rep., CINCH Series. [5], [6]

CREHPA (1996): *Opinion Poll Survey on Abortion Rights for Women*. [1], [8]

CRR (2020): "The world's abortion laws," . [1]

FROST, M. D., M. PURI, AND P. R. A. HINDE (2013): "Falling sex ratios and emerging evidence of sex-selective abortion in Nepal: evidence from nationally representative survey data," *BMJ open*, 3, e002612. [9]

GOLDIN, C. AND L. F. KATZ (2002): "The power of the pill: Oral contraceptives and women's career and marriage decisions," *Journal of Political Economy*, 110, 730–770. [1], [5]

GRUBER, J., P. LEVINE, AND D. STAIGER (1999): "Abortion legalization and child living circumstances: who is the "marginal child"?" *The Quarterly Journal of Economics*, 114, 263–291. [1], [3], [4], [5]

GUILMOTO, C. Z. (2009): "The Sex Ratio Transition in Asia," *Population and Development Review*, 35, 519–549. [8]

GUPTA, M. D. (1987): "Selective discrimination against female children in rural Punjab, India," *Population and Development Review*, 77–100. [6], [13], [17]

HENDERSON, J. T., M. PURI, M. BLUM, C. C. HARPER, A. RANA, G. GURUNG, N. PRADHAN, K. REGMI, K. MALLA, S. SHARMA, ET AL. (2013): "Effects of abortion legalization in Nepal, 2001–2010," *PloS One*, 8, e64775. [2], [10]

HESKETH, T. AND Z. W. XING (2006): "Abnormal sex ratios in human populations: causes and consequences," *Proceedings of the National Academy of Sciences*, 103, 13271–13275. [8]

HU, L. AND A. SCHLOSSER (2015): "Prenatal sex selection and girls' well-being: Evidence from India," *The Economic Journal*, 125, 1227–1261. [5], [7], [27]

JAYACHANDRAN, S. AND I. KUZIEMKO (2011): "Why do mothers breastfeed girls less than boys? Evidence and implications for child health in India," *The Quarterly Journal of Economics*, 126, 1485–1538. [1], [3], [7], [23], [31]

JAYACHANDRAN, S. AND R. PANDE (2017): "Why are Indian children so short? The role of birth order and son preference," *American Economic Review*, 107, 2600–2629. [6], [15]

LAMICHHANE, P., T. HARKEN, M. PURI, P. D. DARNEY, M. BLUM, C. C. HARPER, AND J. T. HENDERSON (2011): "Sex-selective abortion in Nepal: a qualitative study of health workers' perspectives," *Women's Health Issues*, 21, S37–S41. [9]

LEVINE, P. B., A. B. TRAINOR, AND D. J. ZIMMERMAN (1996): "The effect of Medicaid abortion funding restrictions on abortions, pregnancies and births," *Journal of Health Economics*, 15, 555–578. [5]

LI, H., J. ZHANG, AND Y. ZHU (2008): "The quantity-quality trade-off of children in a developing country: Identification using Chinese twins," *Demography*, 45, 223–243. [6]

LIN, M.-J., J.-T. LIU, AND N. QIAN (2014): "More missing women, fewer dying girls: The impact of sex-selective abortion on sex at birth and relative female mortality in Taiwan," *Journal of the European Economic Association*, 12, 899–926. [3], [5], [6], [22], [27]

LONDOÑO-VÉLEZ, J. AND E. SARAVIA (2025): "The impact of being denied a wanted abortion on women and their children," *The Quarterly Journal of Economics*, 140, 1061–1110. [1]

MILAZZO, A. (2018): "Why are adult women missing? Son preference and maternal survival in India," *Journal of Development Economics*, 134, 467–484. [2]

MINISTRY OF HEALTH, D. O. H. S. (1979): *National Immunization Program (NIP)*. [25]

——— (2002): *National Safe Abortion Policy*. [9]

——— (2016): "Abortion Utilization Data," . [9]

MINISTRY OF HEALTH, N. E. (2012): *Nepal Demographic and Health Survey 2011*. [7], [8]

MUKHIYA, G. AND N. MISHRA (2025): "The history and current scenario of interventional radiology in Nepal," *Journal of Medical Imaging and Interventional Radiology*, 12, 8. [8], [41] NHSPP, N. H. S. S. P. (2013): *Antenatal Rural Ultrasound Programme: A Pilot Intervention in Dhading District, Nepal: Advocacy Brief*. [8]

POP-ELECHES, C. (2006): "The impact of an abortion ban on socioeconomic outcomes of children: evidence from Romania," *Journal of Political Economy*, 114, 744–773. [4], [5]

ROSENZWEIG, M. R. AND K. I. WOLPIN (1980): "Testing the quantity-quality fertility model: The use of twins as a natural experiment," *Econometrica*, 227–240. [6]

ROSENZWEIG, M. R. AND J. ZHANG (2009): "Do population control policies induce more human capital investment? Twins, birth weight and China's "one-child" policy," *The Review of Economic Studies*, 76, 1149–1174. [6]

SAMANDARI, G., M. WOLF, I. BASNETT, A. HYMAN, AND K. ANDERSEN (2012): "Implementation of legal abortion in Nepal: a model for rapid scale-up of high-quality care," *Reproductive Health*, 9, 7. [9]

SEN, A. (1990): "More than 100 million women are missing," *The New York Review of Books*, 37, 61–66. [1]

SUBEDI, K. S. AND P. SHARMA (2013): "Development of radiology in Nepal: gearing up for mountainous challenges," *Journal of the American College of Radiology*, 10, 291–295. [8], [41]

TAMANG, A. (1996): "Induced Abortions and Subsequent Reproductive Behaviour Among Women in Urban Areas of Nepal 26 (3 & 4)," *Social Change*, 26, 271–285. [1], [8]

THAPA, S., S. K. SHARMA, AND N. KHATIWADA (2014): "Women's knowledge of abortion law and availability of services in Nepal," *Journal of Biosocial Science*, 46, 266–277. [1], [8]

UNICEF (2021): "Breastfeeding: A Key to Sustainable Development," `https://www.unicef.org/nutrition/breastfeeding`, accessed 2025-10-09. [32]

WALDRON, I. (1998): "Sex differences in infant and early childhood mortality: major causes of death and possible biological causes Too young to die genes or gender. 1998 New York United Nations, Department of Economic and Social Affairs," *Population Division*, 64–83. [7]

WARRINER, I., D. WANG, N. M. HUONG, K. THAPA, A. TAMANG, I. SHAH, D. T. BAIRD, AND O. MEIRIK (2011): "Can midlevel health-care providers administer early medical abortion as safely and effectively as doctors? A randomised controlled equivalence trial in Nepal," *The Lancet*, 377, 1155–1161. [1]

WORLD HEALTH ORGANIZATION (2023): "Breastfeeding and the Sustainable Development Goals," `https://www.who.int/news/item/01-08-2023-breastfeeding-and-the-sustainable-development-goals`, accessed 2025-10-09. [32]

Wu, W.-J., S. Maru, K. Regmi, and I. Basnett (2017): "Abortion care in Nepal, 15 years after legalization: gaps in access, equity, and quality," *Health and Human Rights*, 19, 221. [9]

A Appendix A: Conceptual Framework

We present a general model of fertility and parental investments. The model is agnostic to context and can be specialized to different settings by adding environment-specific assumptions. We then discuss how the Nepal setting maps into this structure.

General Model: Fertility–Investment Trade-off

Preferences and technology. Parents choose the number of children $n \in \mathbb{R}_+$ and per-child investment $e \in \mathbb{R}_+$ to maximize

$$U = U(C, n, q), \qquad q = f(e),$$

where C is consumption, q is average child "quality," $U_C, U_n, U_q > 0$, and U is strictly concave. The quality production function satisfies $f'(e) > 0$, $f''(e) < 0$.

Budget and time costs. Each child involves a resource cost that depends on e. Let the per-period total cost of rearing one child be $k(e)$ with $k'(e) > 0$, $k''(e) \geq 0$. Income is Y. The static resource constraint is

$$C + n\,k(e) \leq Y.$$

A time cost formulation is equivalent if time is priced at the wage; the key object is the *shadow price* of a child, $k(e)$, which rises in e.

Optimality conditions. Let λ be the multiplier on the budget. The first-order conditions (FOCs) are

$$\frac{\partial U/\partial n}{\lambda} = k(e), \tag{A1}$$

$$\frac{\partial U/\partial q}{\lambda} f'(e) = n\,k'(e), \tag{A2}$$

together with the budget constraint. Equation (A1) equates the marginal utility of an additional child to its shadow price. Equation (A2) equates the marginal benefit of raising per-child investment to the marginal resource cost across n children. The pair (n, e) solves a standard quantity–quality trade-off.

Comparative statics (general). Let $\tilde{k}$ denote a shock to the shadow price of fertility, holding e fixed. If $\tilde{k}$ rises (children become more costly), the optimal n falls and, through (A2), e tends to rise since fewer children spread the marginal cost $n\,k'(e)$ over a smaller base. If an investment subsidy lowers $k'(e)$, then for given n the optimal e rises; the effect on n is in general ambiguous and depends on cross-substitution in U.

Dynamic Extension

The static framework can be extended to a dynamic setting in which fertility unfolds across periods $t = 1, \ldots, T$. In each period, parents decide whether to attempt another birth and how much to invest in existing children.

Let π_t denote the probability of conception in period t if parents try for a child. Some investments, such as time spent breastfeeding, reduce the time or biological readiness for conception. This can be represented by $\pi_t = \pi(e_t)$ with $\pi'(e_t) < 0$, meaning that higher investment reduces the probability of conceiving in the next period.

The household's problem is to maximize the present value of utility

$$\sum_{t=1}^{T} \beta^t U(C_t, n_t, q_t),$$

subject to the budget constraint

$$C_t + n_t k(e_t) \leq Y_t,$$

and the law of motion for n_t governed by π_t.

The dynamic structure reproduces the static trade-off each period but also adds an intertemporal margin: parents weigh the benefits of investing more in current children against the fertility cost of delaying or reducing the chance of having additional children. This makes the model especially suited to settings where some investments double as fertility-spacing tools.

Specialization to Contexts

The general model becomes empirically informative once context-specific primitives are introduced. Two common specializations in the literature are compositional preferences and fertility-control technologies.

Compositional preferences. Suppose parents value the probability of having at least one son. Let $s \in [0, 1]$ denote this probability and augment preferences as $U(C, n, q, s)$ with $U_s > 0$. In a static representation, s is increasing in n if sex is i.i.d. at birth, which raises the marginal utility of additional births when no son is present. In the dynamic extension, U_s creates a state variable indicating whether a son has arrived.

Fertility-control technology. Let $\phi \geq 0$ be the effective cost parameter for abortion or contraception. A reduction in ϕ lowers the shadow cost of achieving desired family size and composition. In the dynamic extension, a lower ϕ raises the option value of a fertility attempt and weakens the complementarity between low e_t and trying for a birth when composition targets are unmet.

Implications. With $U_s > 0$ and high ϕ, families may select higher n when a son is absent and may tilt e toward lower levels for a time-intensive subset of investments. When ϕ falls, parents can achieve composition targets with fewer births, which reduces excess fertility and relaxes the need to adjust e for timing reasons. The sign and magnitude of the investment response are ambiguous in general and depend on which investments compete most with fertility effort and on preference curvature.

Mapping to Nepal

To specialize the model to Nepal, take the following context-driven assumptions that are consistent with descriptive facts:

1. Strong son preference. Model via $U(C, n, q, s)$ with $U_s > 0$ and a state that records whether a son has arrived.

2. Legal reform. Abortion legalization is a reduction in ϕ in 2002. Ultrasound diffusion affects the precision with which composition can be targeted, which further lowers the effective cost of achieving s.

3. Investment margins. Some investments are time or spacing sensitive (for example, breastfeeding duration) and therefore enter $\pi(e_t)$ with $\pi'(e_t) < 0$. Other investments (for example, vaccination take-up) are less tied to timing and mainly shift $k'(e)$ without affecting $\pi(e_t)$.

Under these assumptions the framework yields two broad empirical implications. First, a decline in ϕ weakens the link between composition and fertility behavior, which reduces excess fertility among families with a firstborn daughter. Second, investment patterns may change because fertility constraints are relaxed. When ϕ falls, parents no longer need to suppress spacing-sensitive investments to accelerate the next conception, so those investments should rise for the affected families. The empirical analysis examines these margins directly.

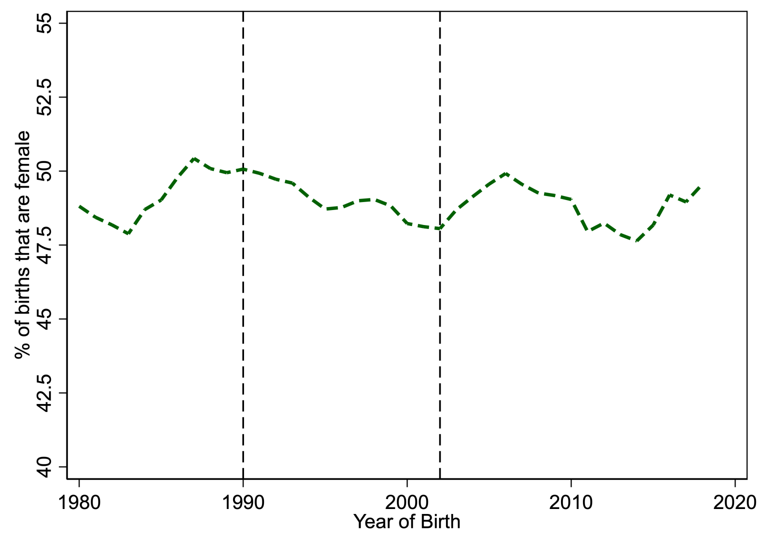
B Appendix B: Policy Details and Identification Tests

Timeline

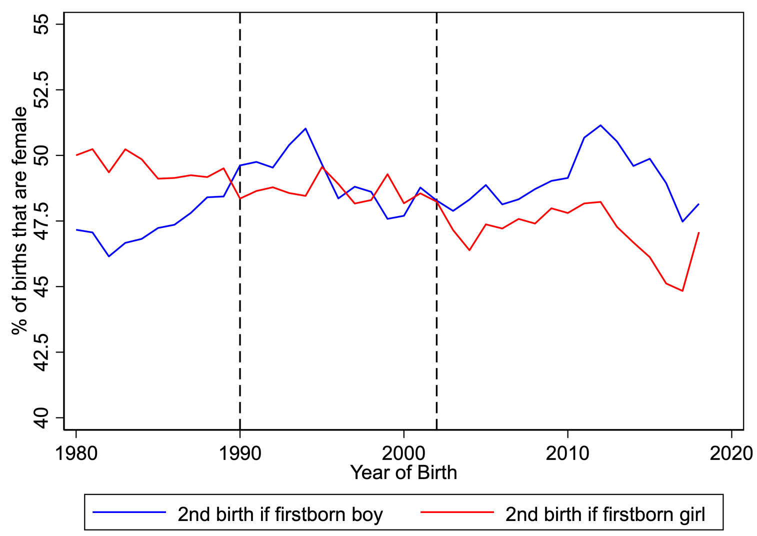
Table A1: Timeline of Abortion Services Rollout

Year	Reform
1988	Ultrasound services introduced.
2002	Abortion legalized under specific conditions.
2004	First-trimester surgical abortion services launched.
2007	Second-trimester abortion services introduced.
2009	Medical abortion introduced, rural area expansion.
2009	Supreme Court ruled abortion as a human right.
2018	Free abortion service launched in all govt. hospitals.
2020-Present	Ongoing efforts to expand services and reduce stigma.

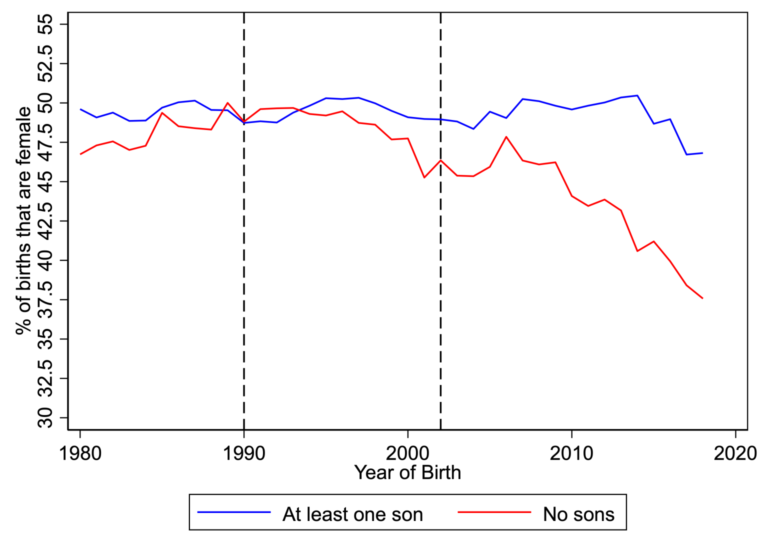
The rollout of abortion and related reproductive health services in Nepal occurred gradually over several decades. Ultrasound services were first introduced in 1988 (Subedi and Sharma, 2013; Mukhiya and Mishra, 2025), enabling prenatal sex determination well before abortion became legal. In 2002, abortion was legalized under specific conditions, marking a critical policy shift. Service provision expanded in stages: first-trimester surgical abortion became available in 2004, followed by the introduction of second-trimester procedures in 2007. Medical abortion was rolled out in 2009, alongside efforts to extend access to rural areas. That same year, the Supreme Court of Nepal ruled abortion to be a constitutional right, strengthening the legal foundation of the policy. Subsequent reforms further broadened access, including the 2018 launch of free abortion services in all government hospitals. Since 2020, ongoing initiatives have aimed to expand coverage and reduce social stigma, consolidating the reform into a nationwide reproductive health program.



(a) First births



(b) Second births



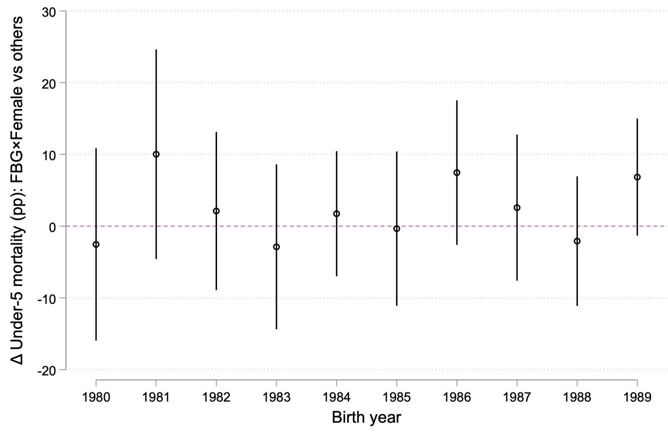
(c) Third births

Figure A1: Trends in Proportion of Females at Birth by Birth Order and Sex Composition of Older Siblings

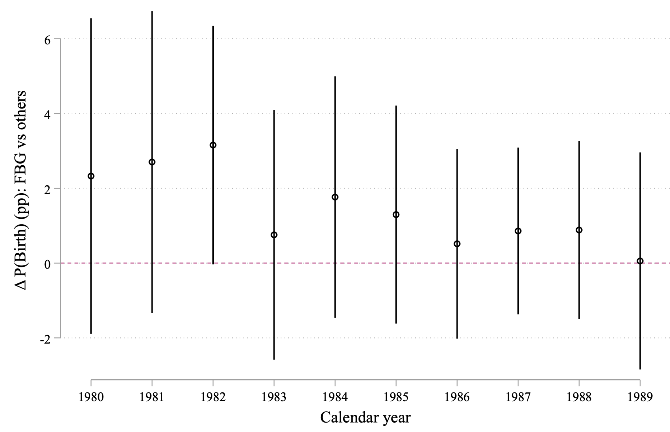
Notes: Panel (a) shows the evolution of the percentage of females among first births over time. In panels (b) and (c) the trend in the percentage of births that are female is respectively plotted for second and third births separately for families that have at least one son and families with no sons at the time of the respective birth. In all cases, the y axis shows the five-year moving average of percentage of births that are female. The figures show that, despite ultrasound availability and abortion legalization, the sex ratio of first births has remained normal. It also shows that the sex ratio at birth in families without sons starts diverging from the sex ratio in families with sons after the introduction of ultrasound and there is a clear divergence after the legalization of abortion.

Other Identification Tests

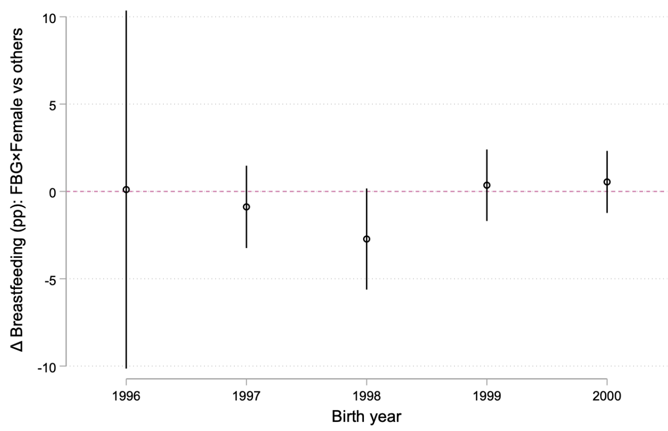
We test that the sex of the firstborn child is effectively random in panel (a) of Figure A1 and that the sex-selective abortion at second and higher-order births occurs primarily in families whose firstborn is a daughter in Figure A1. The figures show that, despite ultrasound availability and abortion legalization, the sex ratio of first births has remained normal. Therefore, it supports our assumption that the sex of the firstborn child is quasi-random. Panels (b) and (c) show that the sex ratio at birth in families without sons starts diverging from the sex ratio in families with sons after the introduction of ultrasound and there is a clear divergence after the legalization of abortion. This allows us to assume that the firstborn-sex margin captures households' incentives to continue childbearing to obtain a son.



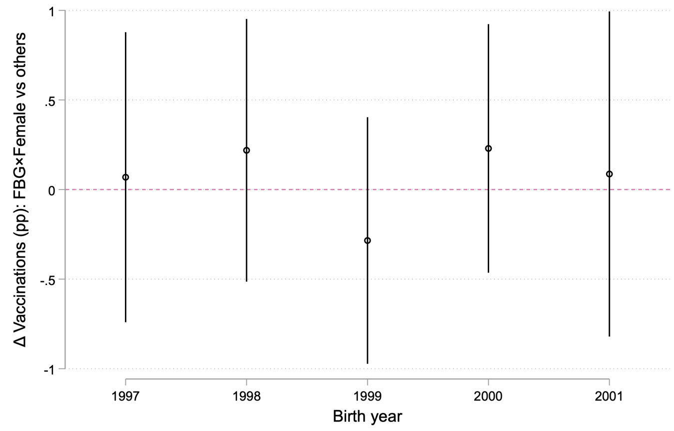
(a) Under-five Mortality



(b) Probability of Birth



(c) Breastfeeding



(d) Vaccinations

Figure A2: Test of Differential Pre-Trends in Outcomes by the Sex of the Firstborn

Parallel Trends Tests

To assess the validity of the identifying assumption, we test for pre-trends using data from 1980 to 1990, a period that predates predates any meaningful diffusion of scans outside the capital. For fertility, we regress the probability of birth on interactions between firstborn sex and year dummies. The joint test yields an F-statistic of 1.02 with a p-value of 0.43, providing no evidence of differential pre-trends across firstborn-girl and firstborn-boy families. For under-five mortality, the analogous test produces an F-statistic of 1.20 with a p-value of 0.30, again consistent with parallel trends. For breastfeeding duration, data is only available beginning with the 1996 survey, so our window is 1996-2002. Using these years, the joint test yields an F-statistic of 1.77 with a p-value of 0.13, suggesting no evidence of divergence across family types even before abortion legalization. Similarly for the number of vaccinations, the joint test F-statistic is 1.04 with a p-value of 0.4. Taken together, these results support the parallel trends assumption for both fertility and child investments.

C Appendix C: Other Heterogeneity Tests

Table A2: Probability of Birth: Heterogeneity by Socioeconomic Status

	Mother's education		Wealth		Rurality	
	(1) Illiterate	(2) Literate	(3) Bottom 40%	(4) Top 40%	(5) Rural	(6) Urban
Firstborn girl	0.0323***	0.0613***	0.0505***	0.0408***	0.0365***	0.0590***
	(0.0023)	(0.0032)	(0.0028)	(0.0036)	(0.0026)	(0.0032)
Firstborn girl * Post1	-0.0145***	-0.0267***	-0.0316***	-0.0188***	-0.0181***	-0.0314***
	(0.0029)	(0.0040)	(0.0037)	(0.0047)	(0.0036)	(0.0036)
Firstborn girl * Post2	-0.0111***	-0.0421***	-0.0371***	-0.0241***	-0.0224***	-0.0404***
	(0.0031)	(0.0035)	(0.0032)	(0.0046)	(0.0041)	(0.0034)
N	347,792	219,061	212,659	243,834	340,478	226,377

Notes: This table reports the probability of birth for children of second- and higher-order birth order for mothers with different socioeconomic status. Coefficients are from the specification 2. Each column within a panel is a separate OLS regression. Post 1 indicates the early legalization period from 2003-2007, and Post 2 indicates the late legalization period from 2008-2018. SEs in parentheses are clustered by district. Data: NDHS. $p < 0.1^{*} p < 0.05^{**} p < 0.01^{***}$

Table A3: Under-5 Mortality: Heterogeneity by Socioeconomic Status

	Mother's education		Wealth		Rurality	
	(1) Illiterate	(2) Literate	(3) Bottom 40%	(4) Top 40%	(5) Rural	(6) Urban
Firstborn girl * Female	0.0158	0.0271	0.0076	0.0256*	0.0224**	-0.0075
	(0.0101)	(0.0173)	(0.0140)	(0.0136)	(0.0101)	(0.0205)
Firstborn girl * Female * Post1	-0.0301	-0.0648**	-0.0334	-0.0381	-0.0569**	0.0072
	(0.0225)	(0.0264)	(0.0335)	(0.0251)	(0.0244)	(0.0252)
Firstborn girl * Female * Post2	-0.0085	0.0093	0.0390*	-0.0062	-0.0108	0.0361
	(0.0219)	(0.0205)	(0.0221)	(0.0220)	(0.0201)	(0.0234)
N	26,479	10,536	10,322	19,819	25,221	11,794
Baseline mean	.115	.0581	.0695	.123	.108	.0802

Notes: The dependent variable is an indicator of death before age five. Sample of second- and higher-order births. Each column is a separate OLS regression. We drop children who are less than five years old to allow each child in the sample full exposure to the risk of under-five mortality. Post 1 indicates the early legalization period from 2003-2007, and Post 2 indicates the late legalization period from 2008-2018. SEs in parentheses are clustered by district. Baseline mean refers to under-five mortality for children born in the pre-ultrasound period to mothers with the specific socioeconomic status. Data: NDHS. $p < 0.1^{*} p < 0.05^{**} p < 0.01^{***}$

Table A4: No. of Vaccinations: Heterogeneity by Socioeconomic Status

	Mother's education		Wealth		Rurality	
	(1) Illiterate	(2) Literate	(3) Bottom 40%	(4) Top 40%	(5) Rural	(6) Urban
Firstborn girl * Female	-0.2632**	0.2006	0.1588	-0.4652***	-0.2075*	0.0846
	(0.1274)	(0.1722)	(0.1688)	(0.1575)	(0.1105)	(0.2238)
Firstborn girl * Female * Post1	0.2328	-0.1910	0.0150	0.3398	0.1870	-0.0523
	(0.2414)	(0.2552)	(0.3007)	(0.2796)	(0.2101)	(0.4584)
Firstborn girl * Female * Post2	0.2107	-0.0950	-0.1285	0.3969	0.0957	0.2294
	(0.2600)	(0.2184)	(0.2623)	(0.2437)	(0.2042)	(0.3104)
N	8,812	4,513	3,845	7,067	10,682	2,642
Baseline mean	5.67	6.86	6.54	5.53	5.88	6.86

Notes: This table reports vaccination effects for children of second- and higher-order birth order. Coefficients are from the specification 1. Each column within a panel is a separate OLS regression. Post 1 indicates the early legalization period from 2003-2007, and Post 2 indicates the late legalization period from 2008-2018. SEs in parentheses are clustered by district. Baseline mean is the mean number of vaccinations received during the pre-legalization period by the children of mothers with the specific socioeconomic status. Data: NDHS. $p < 0.1^{*} p < 0.05^{**} p < 0.01^{***}$

D Back-of-the-Envelope Calculation of Missing Girls After Abortion Legalization

We adapt the accounting framework in Anukriti et al. (2022) to the Nepal setting. The aim is to use a small set of accounting identities to map my regression estimates into annual counts of girls missing at birth due to sex-selective abortion, and to compare the magnitudes. The calculations are deliberately back-of-the-envelope and should be viewed as mechanical decompositions over time, rather than as a structural behavioral model.

Notation and objects

Let N denote total live births in a given year. Let N_{FG} denote the annual number of parity ≥ 2 births that are preceded by a firstborn girl (FBG). Let F_{FG} be the female share among those births and $M_{FG} = 1 - F_{FG}$ the male share. Let Δ denote the post-legalization minus pre-legalization difference in any object. Following the natural sex ratio at birth, Wetake the female-to-male ratio to be 0.49/0.51 when converting from observed male births to expected female births. As in Anukriti et al. (2022), the expressions below are simple accounting identities that hold period-by-period.

"Missing girls" at birth (prenatal selection)

For births of parity ≥ 2 in FBG families, the number of "missing girls" at birth in a year is defined as the gap between the expected number of female births (given observed male births and the natural sex ratio) and the observed number of female births. Using the notation above, this can be written as

$$MG = N_{FG}\left(\frac{0.49}{0.51}M_{FG} - F_{FG}\right). \tag{A3}$$

A positive value of MG indicates fewer girls being born than would be expected under the natural sex ratio at birth.

The change in missing girls from the pre-legalization to the post-legalization period can be decomposed into two components:

$$\Delta MG = \Delta N_{FG}\left(\frac{0.49}{0.51}M_{FG,\text{pre}} - F_{FG,\text{pre}}\right) \; + \; N_{FG,\text{post}}\left(\frac{0.49}{0.51}\Delta M_{FG} - \Delta F_{FG}\right) \tag{A4}$$
$$\equiv \text{conception effect} \; + \; \text{sex-selective abortion effect.}$$

The first term ("conception effect") captures the change in missing girls that arises purely because the number of parity ≥ 2 births in FBG families changes. The second term ("sex-selective abortion effect") isolates the contribution of changes in the sex composition of those births, holding the number of such births fixed.

Mapping regression estimates to the table

Panel A. From the sex-ratio regressions, we obtain ΔF_{FG} (and equivalently ΔM_{FG}) as the post–pre change in the female share among parity ≥ 2 births in FBG families. We estimate

$$S \;\equiv\; \frac{N_{FG}}{N}$$

as the DHS-weighted share of parity ≥ 2 births that occur in FBG families. This share is used to scale up from regression estimates (which are at the child or mother level) to national counts.

Panel B (levels and conversions). For levels, we take N from UN vital statistics on annual live births in Nepal (United Nations, Department of Economic and Social Affairs, 2024). We convert total births into an approximate number of mothers m using the general fertility rate (GFR) for 2002:

$$m \;\approx\; \frac{N}{\text{GFR}} \times 1000,$$

where the GFR is measured as births per 1,000 women aged 15–49. We then set

$$m_{FG} \;\approx\; S \cdot m \quad \text{and} \quad N_{FG} \;\approx\; S \cdot N,$$

treating the DHS-estimated share S as representative of the national birth distribution.

Converting a fertility treatment effect to ΔN_{FG}. If the fertility specification yields a treatment-on-the-treated (TOT) effect on the total number of births per FBG mother over her reproductive span, we convert this into an annual change in N_{FG}. Under the approximation that the treatment effect on fertility is spread uniformly across a 35-year fertile span and that the regression uses a 630-month exposure window, the annualization factor is $35/630 \approx 0.056$. Multiplying the mother-level TOT by 0.056 yields the implied change in births per FBG mother per year; multiplying this quantity by m_{FG} gives the annual ΔN_{FG} used in equation (A4).

Implementation steps

1. Define the pre-legalization period as all years before 2003, and the late-legalization period as 2008 onward.

2. Estimate ΔF_{FG} (and hence ΔM_{FG}) using births of parity ≥ 2 in FBG families from the sex-ratio regressions.

3. Compute $S = N_{FG}/N$ using DHS sampling weights so that the parity and firstborn-sex composition is representative.

4. Take N from UN live-birth statistics by year. Compute m from N and the GFR so that the mother denominator corresponds to the same year or pooled window as the regression estimates.

5. If the fertility effect is reported as a mother-lifetime TOT, convert it to an annual ΔN_{FG} using the $35/630$ rule described above and multiply by m_{FG}.

Assumptions and sensitivity

The back-of-the-envelope numbers rest on several simplifying assumptions:

- **Natural sex ratio at birth.** We take the "natural" female-to-male ratio at birth to be $0.49/0.51$ and check robustness to nearby values (0.488 and 0.491).

- **Uniform timing of fertility effects.** When converting a mother-lifetime TOT into an annual change in births, We assume that the effect of legalization on completed fertility is spread uniformly across childbearing ages.

- **Stable composition of FBG families.** We treat the share $S = N_{FG}/N$ as stable over the pre- and post-legalization periods and abstract from any policy-induced changes in the probability of having a first birth or in the sex ratio at first birth.

- **Representative scaling.** We assume that DHS-based estimates of S and ΔF_{FG} can be scaled up using UN vital statistics without additional adjustment for under-reporting or sample selection.

Under these assumptions, the resulting figures should be interpreted as approximate magnitudes that translate my regression coefficients into counts of missing girls at birth (and, analogously, excess postnatal female deaths averted), rather than as precise population totals.

Alternative "missing girls" calculation using Anderson and Ray (2010)

As a complementary check, we also compute a macro-level missing girls estimate using the birth-based formula in Anderson and Ray (2010, eq. (5)). Let r denote the sex ratio at birth (male-to-female), let α denote a reference sex ratio at birth for the same group in a low-discrimination

Table A5: Decomposition and Simulation of Missing Girls

	Pre-legalization	Late-legalization	Δ (Post - Pre)
A. Regression Estimates			
F_{FG}	0.4891	0.4682	-0.0209
$S \equiv N_{FG}/N$	0.3446		
B. Decomposition			
N (births/year)	725,448		
m #mothers	$\approx$ 5,757,524		
m_{FG} #mothers with a firstborn girl	$\approx$ 0.3446*m		
N_{FG}	0.3446*N	0.3446*N-0.0055*m_{FG}	-0.0055*m_{FG}
	= 249,989	= 239,077	=-10912
Δ *number of missing girls:*			
(1) Conception effect: $\Delta N_{FG} \cdot \left(\frac{0.49}{0.51} - \frac{F_{FG,\text{pre}}}{0.51}\right)$		$\approx$ -19	
(2) Sex-selective abortion effect: $N_{FG,\text{post}} \cdot (\Delta F_{FG}) \cdot \left(-\frac{1}{0.51}\right)$		$\approx$ 9797	
(3) Total missing girls = (1)+ (2)		= 9778	
(4) Percent of missing girls		=1.35%	

setting, and let F denote the total number of female births. Following Anderson and Ray, define

$$\theta \equiv \frac{r}{\alpha} - 1,$$

and compute missing girls at birth as

$$\text{MG}^{AR} = \max\{F \cdot \theta, 0\}.$$

When $\theta < 0$, the implied missing-girls count is truncated at zero.

For Nepal, we set:

- $r_{\text{pre}} = 1.027$ (sex ratio at birth in 2001),
- $r_{\text{post}} = 1.12$ (sex ratio at birth in 2021),
- $\alpha = 1.044$ (reference sex ratio at birth for Nepali-American births (Centers for Disease Control and Prevention, National Center for Health Statistics (2025))),
- $F_{\text{post}} = 282{,}832$ (female births in Nepal in 2021).

The implied values of θ are:

$$\theta_{\text{pre}} = \frac{1.0266}{1.0438} - 1 \approx -0.016, \qquad \theta_{\text{post}} = \frac{1.12}{1.0438} - 1 \approx 0.073.$$

Because $\theta_{\text{pre}} < 0$, Weset $\text{MG}^{AR}_{\text{pre}} = 0$. For 2021,

$$\text{MG}^{AR}_{\text{post}} = F_{\text{post}} \cdot \theta_{\text{post}} = 282{,}832 \times 0.072968 \approx 20{,}638.$$

Thus, the Anderson–Ray approach implies an increase of about

$$\Delta\text{MG}^{AR} \approx 20{,}638$$

missing girls at birth, relative to the pre-reform baseline. This corresponds to roughly 7.3 percent of female births in 2021.

Putting the numbers together

The micro-based decomposition in Appendix D, which builds directly on my difference-in-differences estimates for firstborn-girl families and scales them using the national birth distribution, implies that abortion legalization accounts for roughly *one missing girl out of every seventy-five girls* who would otherwise have been born in post-reform cohorts (about 1–1.5 percent of potential female births). By contrast, the Anderson–Ray calculation, which compares Nepal's national sex ratio at birth in 2021 to a reference sex ratio for Nepali-American births, yields approximately 20,638 missing girls in 2021, or about 7.3 percent of female births in that year, with the regression-based calculation providing a lower bound and the Anderson–Ray estimate an upper-bound envelope.

D Appendix E: Other Mortality Analyses

Table A6: Excess Female Under-One Mortality

	(1)	(2)	(3)	(4)	(5)
Firstborn girl * Female	0.0109	0.0103	0.0097	0.0099	0.0094
	(0.0071)	(0.0072)	(0.0071)	(0.0071)	(0.0070)
Firstborn girl * Female * Post1	-0.0255**	-0.0246**	-0.0219*	-0.0245**	-0.0217*
	(0.0117)	(0.0117)	(0.0115)	(0.0116)	(0.0115)
Firstborn girl * Female * Post2	0.0030	0.0035	0.0029	0.0041	0.0034
	(0.0101)	(0.0102)	(0.0099)	(0.0102)	(0.0099)
N	50,354	50,353	50,329	50,352	50,328
Baseline mean	.0772	.0772	.0772	.0772	.0772

Notes: The dependent variable is an indicator of death before age one. Sample of second- and higher-order births. Each column is a separate OLS regression. We drop children who are less than one year old to allow each child in the sample full exposure to the risk of under-one mortality. Post 1 indicates the early legalization period from 2003-2007, and Post 2 indicates the late legalization period from 2008-2018. SEs in parentheses are clustered by district. Data: NDHS. $p < 0.1^{*} p < 0.05^{**} p < 0.01^{***}$

Table A7: Excess Female Under-Two Mortality

	(1)	(2)	(3)	(4)	(5)
Firstborn girl * Female	0.0123	0.0118	0.0115	0.0114	0.0111
	(0.0075)	(0.0076)	(0.0073)	(0.0076)	(0.0073)
Firstborn girl * Female * Post1	-0.0288**	-0.0282**	-0.0248*	-0.0279**	-0.0244*
	(0.0132)	(0.0132)	(0.0129)	(0.0131)	(0.0128)
Firstborn girl * Female * Post2	0.0020	0.0026	0.0010	0.0037	0.0021
	(0.0117)	(0.0119)	(0.0114)	(0.0118)	(0.0114)
N	47,418	47,417	47,396	47,416	47,395
Baseline mean	.0904	.0904	.0904	.0904	.0904

Notes: The dependent variable is an indicator of death before age two. Sample of second- and higher-order births. Each column is a separate OLS regression. We drop children who are less than two years old to allow each child in the sample full exposure to the risk of under-two mortality. Post 1 indicates the early legalization period from 2003-2007, and Post 2 indicates the late legalization period from 2008-2018. SEs in parentheses are clustered by district. Data: NDHS. $p < 0.1^{*} p < 0.05^{**} p < 0.01^{***}$

Table A8: Excess Female Under-Three Mortality

	(1)	(2)	(3)	(4)	(5)
Firstborn girl * Female	0.0129*	0.0124	0.0124	0.0120	0.0120
	(0.0077)	(0.0078)	(0.0076)	(0.0078)	(0.0075)
Firstborn girl * Female * Post1	-0.0329**	-0.0321**	-0.0294**	-0.0316**	-0.0289**
	(0.0148)	(0.0148)	(0.0145)	(0.0147)	(0.0144)
Firstborn girl * Female * Post2	0.0039	0.0048	0.0027	0.0056	0.0035
	(0.0127)	(0.0128)	(0.0122)	(0.0127)	(0.0120)
N	44,263	44,262	44,241	44,261	44,240
Baseline mean	.0955	.0955	.0955	.0955	.0955

Notes: The dependent variable is an indicator of death before age three. Sample of second- and higher-order births. Each column is a separate OLS regression. We drop children who are less than three years old to allow each child in the sample full exposure to the risk of under-three mortality. Post 1 indicates the early legalization period from 2003-2007, and Post 2 indicates the late legalization period from 2008-2018. SEs in parentheses are clustered by district. Data: NDHS. $p < 0.1^{*} p < 0.05^{**} p < 0.01^{***}$

Table A9: Excess Female Under-Four Mortality

	(1)	(2)	(3)	(4)	(5)
Firstborn girl * Female	0.0172**	0.0165*	0.0165**	0.0162*	0.0161**
	(0.0082)	(0.0083)	(0.0081)	(0.0082)	(0.0080)
Firstborn girl * Female * Post1	-0.0375**	-0.0364**	-0.0337*	-0.0360**	-0.0332*
	(0.0168)	(0.0169)	(0.0170)	(0.0169)	(0.0169)
Firstborn girl * Female * Post2	0.0041	0.0054	0.0024	0.0062	0.0032
	(0.0142)	(0.0143)	(0.0138)	(0.0142)	(0.0137)
N	40,701	40,698	40,677	40,697	40,676
Baseline mean	.0991	.0991	.0991	.0991	.0991

Notes: The dependent variable is an indicator of death before age four. Sample of second- and higher-order births. Each column is a separate OLS regression. We drop children who are less than four years old to allow each child in the sample full exposure to the risk of under-four mortality. Post 1 indicates the early legalization period from 2003-2007, and Post 2 indicates the late legalization period from 2008-2018. SEs in parentheses are clustered by district. Data: NDHS. $p < 0.1^{*} p < 0.05^{**} p < 0.01^{***}$