When Rights Become Access: Abortion Reform and Missing Girls in Nepal*

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Abstract

Reforms that expand reproductive rights change behavior only when they change effective access. We study Nepal, which legalized abortion in 2002 to reduce unsafe procedures, and ask when and where legal access affected the sex composition of births in a society with son preference. We combine quasi-random variation in firstborn sex with the staged rollout of abortion services and predetermined connectivity to city services in a triple-difference design. The rollout separates two margins. During the initial first-trimester rollout, later-birth hazards declined where services were reachable, but the male- and female-birth responses were not statistically distinguishable, and the sex composition of births did not change. Once access expanded to include second-trimester services, medication abortion, and midlevel providers, the annual probability of a female birth fell while male births did not decline. Nationally, the probability of a female birth fell by 0.49 percentage points, and the female share of later births in firstborn-girl families fell by 2.83 percentage points. The responses were concentrated within two hours of a city. Within this radius, the female birth probability and the female share declined by 1.30 and 6.52 percentage points, respectively. Back-of-the-envelope calculation maps the national composition estimate to an annual flow equivalent of about 11,900 missing girls at birth, or 1.91 percent of all births.

Keywords: Abortion; effective access; sex-selective abortion; missing girls; son preference; Nepal

JEL codes: J13, J16, I18, I14, O15

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1 Introduction

The gap between what the law says and what agents can do is a general feature of policy in developing countries: formal rights do not necessarily translate into effective capabilities in practice (Sen, 1985; Hallward-Driemeier and Pritchett, 2015). Legal entitlements to health care mean little where the provider is absent (Chaudhury et al., 2006), and even in high-income settings, legal eligibility need not translate into take-up without navigable access (Finkelstein and Notowidigdo, 2019). Reproductive rights are no exception. The canonical evidence on contraception and abortion in the United States is also evidence on effective access: the pill's effects are identified from consent laws that determined which young women could obtain an already legal technology (Goldin and Katz, 2002), from sales bans that governed where it could be bought (Bailey, 2010), and from confidential-access rules (Myers, 2017); distance to providers remains a first-order determinant of abortion use (Lindo et al., 2020).

Fertility control is a first-order determinant of women's well-being (Bailey, 2006), and legal abortion access can reduce unsafe procedures, improve maternal health, and give women greater control over the timing and number of births (Levine et al., 1996; Miller and Valente, 2016; Clarke and Mühlrad, 2021; Londoño-Vélez and Saravia, 2025). But the same policies can also shape who is born (Gruber et al., 1999; Pop-Eleches, 2006). In societies where son preference is central to the "missing women" phenomenon (Sen, 1990; Anderson and Ray, 2010; Anukriti and Calvi, 2026), falling desired fertility raises the cost of achieving a son through continued childbearing (Jayachandran, 2017), and son preference can be expressed before birth (Bhalotra and Cochrane, 2010; Lin et al., 2014; Alfano, 2017; Anukriti et al., 2022). Existing evidence shows that legalization can activate this margin: abortion reform has been shown to skew sex ratios at higher parities in Taiwan and Nepal (Lin et al., 2014; Adhikari, 2026). Yet sex selection requires a service bundle in practice: learning fetal sex, typically not reliably determined until the second trimester (Epner et al., 1998; Amankona et al., 2025), and reaching a provider able to terminate the pregnancy within the legal gestational window. If either component is missing, the law changes, but the sex composition of births need not. What remains unclear is when and where a reform activates this margin. This paper asks whether effective access, not legality, governs a reform's unintended consequences in settings with adverse gender norms.

We study this question in Nepal, which provides a unique setting for separating legalization from effective access. The law changed nationally in 2002, when Nepal legalized abortion to reduce unsafe procedures (Thapa et al., 2014; Ministry of Health, 2002). But legal access arrived in stages (see Appendix Figure A1 for the timeline): registered first-trimester surgical services began in 2004; second-trimester provision and physician training began in 2007; and access expanded further in 2008–2009 through medication abortion and provision by nurses and auxiliary nurse-midwives (Warriner et al., 2011; Samandari et al., 2012). Sex-selective abortion remained illegal throughout, but such restrictions are difficult to enforce when fetal-sex information is produced in routine prenatal care, and the motive for abortion is not observed (Anukriti et al., 2022). The reform therefore created variation in effective access across both time and geography, and both dimensions shaped whether sex-selective abortion was feasible.

Over time, the initial rollout offered only first-trimester procedures. Fetal sex can sometimes be learned near the end of the first trimester, but it is less routinely and less reliably determined before the second trimester (Epner et al., 1998; Amankona et al., 2025). The later expansion relaxed this timing constraint by adding certified second-trimester services; medication abortion and midlevel provision broadened the legal abortion system during the same period. Across space, acting on this margin required reaching the bundle on which it depended: a way to learn fetal sex and a provider able to terminate the pregnancy within the relevant legal window. Ultrasound had been introduced before legalization and had diffused through clinical and antenatal care by the reform period (Subedi and Sharma, 2013; Mukhiya and Mishra, 2025). The practical constraint was therefore less the existence of fetal-sex information than access to a legal procedure after that information became usable. Second-trimester services required certified physicians at accredited facilities and were concentrated in better-connected places (Wu et al., 2017); in 2014, well after legalization, a majority of abortions were still estimated to occur outside approved facilities, with use concentrated in the developed central region around Kathmandu and lowest in remote western districts (Puri et al., 2016; Wu et al., 2017). The scope to act on the sex-selection margin should therefore rise with connectivity to urban services.

Our empirical strategy follows from this timing-and-geography structure. We use complete birth histories from four GPS-linked waves of the Nepal Demographic and Health Survey: 2001, 2006, 2011, and 2016, and study two primary outcomes. First, a post-first-birth mother-year panel decomposes annual birth hazards into any-birth, male-birth, and female-birth components, separating changes in fertility from changes in the sex composition of births. Second, among second- and higher-order live births, the female share provides a direct measure of birth composition.

The design combines three sources of variation. First, following the literature, we use first-born sex as quasi-random variation in son-seeking incentives (Dahl and Moretti, 2008; Bhalotra and Cochrane, 2010; Milazzo, 2018; Anukriti et al., 2022) and show that firstborn sex is unaffected by the reforms. Under son preference, firstborn-girl families have stronger incentives to continue childbearing or select on fetal sex in pursuit of a son than otherwise comparable firstborn-boy families. Second, we exploit the staged rollout of legal services, distinguishing a pre-service baseline from 1990 to 2003, an early first-trimester rollout from 2004 to 2007, henceforth "early rollout," and an expanded-access period from 2008 to 2017, henceforth "expanded access." Third, we use predetermined connectivity to urban services to proxy for whether households could reach the bundle of medical and informational inputs needed for prenatal sex selection.[1] Our preferred access measure is year-2000 travel time to the nearest city or town with a population of at least 50,000, from the Global Accessibility Map, measured before Nepal legalized abortion in 2002 (Nelson, 2008). The main access specification distinguishes clusters within two hours of a city from those farther away. To assess robustness, we use continuous measures of contemporary road-network travel time at alternative city-size thresholds and a market-access index that aggregates surrounding population with weights that decline with travel time (Luxen and Vetter, 2011; Donaldson and Hornbeck, 2016).

Our main specification is a triple-difference design that asks whether the firstborn-girl response changes after legal services expand and whether that change is larger where services are reachable. The two rollout stages imply different outcomes, which we formalize using a model of fertility and sex selection in Section 3. During the early first-trimester rollout, access may reduce later-birth hazards in better-connected places, but male and female births should move together if acting on fetal-sex information remains difficult within the legal window. After access expands to the second trimester and a broader provider base, prenatal selection becomes more feasible: female births and the female share of later births should decline relative to male births, with the largest response where the relevant services can be reached. The identifying assumption is that, absent effective abortion access, the difference in later-birth outcomes between firstborn-girl and

[1] Appendix Figure A2 provides descriptive support for this interpretation: districts closer to cities had more registered Comprehensive Abortion Care centers by 2004 and greater hospital capacity in 2007/08. See Section 2 for details.

firstborn-boy families would not have shifted differentially across more and less connected places.

The results follow the rollout and access structure. First, the national birth-hazard estimates show that son preference was present before legal services were available, but operated on the continuation margin. In the pre-service period, firstborn-girl families were 2.44 percentage points more likely to have another birth in a given year than firstborn-boy families—the well-documented son-biased continuation pattern. On average, nationally, the initial first-trimester rollout did not produce a robust change in the national firstborn-girl differential. After access expanded to include certified second-trimester services, medication abortion, and midlevel provision, the annual probability of a female birth fell by 0.49 percentage points (p-value < 0.05) for firstborn-girl families relative to firstborn-boy families, while the male-birth estimate was small and statistically insignificant. The child-level composition estimates show the same timing. During the initial first-trimester rollout, the firstborn-girl differential in the female share of later births changed little relative to firstborn-boy families. During expanded access, the female share of later births in firstborn-girl families fell by 2.83 percentage points (p-value < 0.05). The timing is therefore informative. Son preference existed before the reform, but expanded access changed the margin on which it operated: from continuing fertility until a son arrived to having fewer daughters.

The geography of the response separates the quantity margin from the composition margin. During the early first-trimester rollout, the broader travel-time radius shows a fertility response but not a clear sex-composition response. Within two hours of a city, the annual probability of any birth fell by about 1 percentage point (p-value < 0.1) for firstborn-girl families relative to firstborn-boy families, and the female-share estimate is statistically insignificant. The initial rollout therefore affected fertility across the broader connected area, without changing the composition of births.

During expanded access, the compositional response extends across the broader effective-access radius. Within two hours of a city, the annual probability of any birth fell by 1.49 percentage points (p-value < 0.01) for firstborn-girl families relative to firstborn-boy families. The decline was driven by female births: the annual probability of a female birth fell by 1.30 percentage points (p-value < 0.01), while male births did not decline. The equality test rejects equal male- and female-birth responses (p-value < 0.05), and the female share of later births fell by 6.52 percentage points (p-value < 0.01). The expanded-access effect is largest closest to cities and attenuates with travel time. Our main result is therefore that the initial first-trimester rollout changed the quantity of births across the broader connected area, while the composition response spread across that area only after the service bundle expanded.

Finally, we translate the estimated change in birth composition into an additional flow measure of missing girls at birth (Sen, 1990; Coale, 1991; Anderson and Ray, 2010).[2] Adapting the accounting exercise in Anukriti et al. (2022), the estimates imply an annual flow equivalent of about 11,900 missing girls at birth, or 1.91% of all births and 3.90% of expected female births.

Our first contribution is to the literature on missing girls and prenatal sex selection. Existing work shows that when parents can learn fetal sex, son preference can be expressed before birth rather than through continued childbearing or differential treatment after birth. In India and China, prenatal sex-detection technology increased sex-selective abortion and altered fertility, mortality, and investments among surviving girls (Bhalotra and Cochrane, 2010; Chen et al., 2013; Hu and Schlosser, 2015; Anukriti et al., 2022). A related strand shows that sex selection also responds to the economic incentives underlying son preference, such as dowries and inheritance laws (Alfano, 2017; Bhalotra et al., 2020). Closest to our setting, Lin et al. (2014) show that abortion legalization in Taiwan increased male-biased sex ratios at birth when prenatal sex detection was already available, and Adhikari (2026) finds a decrease in the share of later-born girls in Nepal after abortion access expanded beyond the first trimester. We study the implementation margin behind these responses. Nepal allows us to separate legalization from effective access and to distinguish the quantity and sex-composition margins because services arrived in stages and reached places unevenly.

The results also clarify which margin changed. Anukriti et al. (2022) show that ultrasound diffusion in India increased the abortion of girls, but also narrowed gender gaps in mortality and parental investments among girls who were born. Related work shows that discrimination before birth can also occur within pregnancies carried to term: Bharadwaj and Lakdawala (2013) find more prenatal care for male fetuses in high-son-preference settings. We find little evidence of comparable changes among surviving girls in Nepal. Expanded abortion access does not detectably improve measured mortality, anthropometrics, or vaccination outcomes for girls who are born. The response we document is therefore primarily selection into birth, not a systematic improvement in measured outcomes among children who are born.

[2] Our accounting concerns girls missing at birth. The broader missing-women literature shows that gender-biased mortality continues later in the life cycle: Anderson and Ray (2010) find that most missing women in India and a substantial share in China are adults, while Calvi (2020) links excess mortality among older women in India to declining intrahousehold bargaining power and individual poverty.

The second contribution is to the evidence on policy implementation and effective access in reproductive health. Much of the abortion and contraception literature studies variation in usable access—through consent rules, sales restrictions, provider supply, confidentiality, or distance—rather than formal legality alone (Goldin and Katz, 2002; Bailey, 2010; Myers, 2017; Lindo et al., 2020). We show that the same distinction can govern a reform's unintended demographic consequences. The closest prior evidence from Nepal reinforces this interpretation. Valente (2014) finds that proximity to the initial Comprehensive Abortion Care rollout reduced the probability that a conception resulted in a live birth, but did not increase sex selection against girls. Miller and Valente (2016) show that the same rollout reduced modern contraceptive use, consistent with substitution between abortion and contraception as methods of fertility control. Our early-rollout results are consistent with both findings: first-trimester access affected the quantity margin, but not the sex-composition margin. Once the legal window and provider base expanded, female births and the female share declined most in better-connected places. Effective access therefore depends on whether households can reach the full service bundle needed to learn fetal sex and terminate a pregnancy within the relevant gestational window.

The findings indicate that reproductive-rights reforms in son-preferring societies need complementary policies that improve gender attitudes and reduce son preference (Dhar et al., 2022), alongside effective regulation of prenatal sex determination. More broadly, the paper adds to the evidence that well-intentioned policies can have unintended consequences (Bharadwaj et al., 2020; Calvi and Keskar, 2026). In this case, legal abortion access advanced an important maternal-health and reproductive-rights objective, closely aligned with SDG targets on reducing maternal mortality, ensuring access to sexual and reproductive health care, and protecting reproductive rights (SDGs 3.1, 3.7, and 5.6) (United Nations, 2015), but it also expanded the scope for prenatal selection where son preference persisted, and services were reachable.

2 Son Preference, Prenatal Sex Detection, and Abortion Access in Nepal

Three features of Nepal make it useful for separating legalization from effective access and for distinguishing the quantity and sex-composition margins of the fertility response. The first is persistent son preference. Sons occupy a central role in lineage continuity, old-age support, inheritance, and religious and social obligations, while daughters often marry out of the natal household and may be associated with marriage-related costs (Brunson, 2010). This preference shapes fertility behavior. When parents cannot select before birth, families without a son continue childbearing. Once abortion access makes acting on fetal-sex information feasible, the same preference can instead be expressed through the sex composition of later births.

Figure A3 shows that the relevant conditions are present in Nepal over the sample period. Panel A plots the ideal number of sons and daughters: both fall as desired fertility declines, and desired sons exceed desired daughters in every survey. Panel B plots the ideal fraction of sons, which remains above one-half throughout. Thus, the per-child tilt toward sons persists even as desired family size shrinks. This is in line with the pattern shown by Jayachandran (2017): a son preference that has not disappeared, combined with declining fertility, leaves less room to secure a son without exceeding the desired family size. As the quantity route narrows, the same preference is more likely to operate through the sex composition of births, where selection is feasible—the margin this paper studies.

The second feature is that prenatal sex-detection technology was already present before the relevant abortion-access shock. This setting is similar to Lin et al. (2014), who study abortion legalization in Taiwan in a setting where ultrasound was already used in standard prenatal care. In Nepal, ultrasound services were introduced in 1988 at Bir Hospital in Kathmandu, before abortion was legalized, and expanded thereafter through clinical and antenatal-care channels (Subedi and Sharma, 2013; Mukhiya and Mishra, 2025). By the reform period, ultrasound was therefore a pre-existing complement to abortion access, and reports describe ultrasound as widely available, with scans reported in 2013 to cost USD 6 outside major cities (Nepal Health Sector Support Programme (NHSSP), 2013). Fetal sex can sometimes be learned near the end of the first trimester, but it is less routinely and less reliably determined before the second trimester (Epner et al., 1998; Amankona et al., 2025). An initial rollout limited to first-trimester procedures, therefore, left a narrow practical window for selection on fetal sex, while later access to certified second-trimester services made that margin more feasible.

The third feature is the staged and spatially uneven expansion of legal abortion access. Before 2002, abortion was criminalized, and unsafe procedures were an important contributor to maternal mortality (Thapa et al., 2014; Ministry of Health, 2002). Legalization in 2002 changed formal rights before it changed effective access.[3] First-trimester surgical services began in 2004 through the rollout of registered Comprehensive Abortion Care centers (Valente, 2014; Miller and Valente, 2016). Second-trimester provision and physician training began in 2007, and access expanded further in 2008–2009 through medication abortion and through provision by nurses and auxiliary nurse-midwives (Warriner et al., 2011; Samandari et al., 2012; Wu et al., 2017). Sex-selective abortion remained illegal, but such restrictions are difficult to enforce when fetal-sex information is produced in routine prenatal care, and the motive for abortion is not directly observed (Anukriti et al., 2022). We therefore distinguish three periods in the analysis: a pre-service baseline, 1990–2003; an early first-trimester rollout, 2004–2007; and an expanded-access period, 2008–2017.

Effective access also varied across space. Acting on fetal sex required reaching the relevant service bundle: fetal-sex information and abortion care within the relevant gestational window. In particular, second-trimester services required certified physicians at accredited facilities and were concentrated in better-connected areas (Wu et al., 2017). Access remained incomplete even after legalization. In 2014, a majority of abortions were still estimated to occur outside approved facilities; abortion use was higher in more developed areas and lower in remote western districts (Puri et al., 2016; Wu et al., 2017). This spatial variation is central to the empirical design. If expanded legal access enabled prenatal sex selection, the response should be larger where households could reach urban reproductive-health services.

Appendix Figure A2 provides direct descriptive evidence that the predetermined city-access measure captures this service geography. We classify Nepal's 75 historical districts by their year-2000 population-weighted median travel time to a city with at least 50,000 residents, before legalization or the rollout of legal services (Nelson, 2008; WorldPop and Center for International Earth Science Information Network, 2018). By the end of 2004, districts within one hour of a city had 0.30 registered Comprehensive Abortion Care centers per 100,000 residents, compared with 0.12–0.13 in the other access cells; 80 percent had at least one registered center, compared with 19 percent beyond four hours. The same gradient appears in higher-tier medical capacity: in 2007/08, districts within one hour averaged 1.80 hospitals listed by the Department of Health

[3]The 2002 reform was part of a broader rights-based response to unsafe abortion and criminal punishment. Before legalization, women could be prosecuted and imprisoned for abortion-related charges, and advocacy around reform emphasized both maternal mortality and the criminalization of women seeking care (CREHPA, 2000; Thapa et al., 2014; Ministry of Health, 2002). After legalization, the 2009 *Lakshmi Dhikta v. Nepal* decision reinforced the state's obligation to make abortion services affordable and accessible (Samandari et al., 2012; Wu et al., 2017).

Services, compared with 1.03 beyond four hours (Central Bureau of Statistics, 2002; Department of Health Services, 2009). These patterns show that pre-reform city connectivity is associated with where abortion services arrived first and where medical capacity was concentrated.

3 A Simple Model of Fertility and Sex Selection

We develop a simple conceptual framework for the choice of a family that wants a son and has not yet had one. The model gives a threshold condition: a family selects on fetal sex when the effective cost of doing so falls below the shadow cost of the additional live birth implied by continued childbearing.

Consider family i after its first birth, when it chooses how to pursue a son. A family whose first child is a girl enters this decision without a son. For simplicity, a family whose first child is a boy is treated as having satisfied its demand for a son and serves as the comparison group.[4]

Let $V_i > 0$ denote the value of obtaining at least one son, and assume that it is large enough for the family to continue seeking one. Let $b_i \equiv c + \varphi_i > 0$ denote the net shadow cost of an additional live birth, and let $G(\cdot)$ denote its distribution among families on the son-seeking margin. The term c includes the resource, health, and time costs of another birth, net of any utility from the additional child. The term $\varphi_i \geq 0$ is the shadow cost of exceeding the family's desired size. As desired fertility falls, the family-size constraint binds more tightly and φ_i rises. Because both strategies considered below eventually produce a son, V_i does not enter the comparison between them.

A conception is male with probability q, independently across conceptions. Ultrasound is assumed to provide information on fetal sex before the selection decision. Because ultrasound was already used in routine prenatal care before the reform, we treat fetal-sex information as a pre-existing input and focus on the incremental cost of acting on it.

Let τ_i denote travel time from the household to the nearest urban service center. The effective cost of terminating an identified female pregnancy in policy period t is

$$K_t(\tau_i) = \kappa_t + m\tau_i, \tag{1}$$

where κ_t is a policy-regime-specific fixed cost and $m > 0$ is the full cost of an additional hour

[4]This normalization is not essential. If some firstborn-boy families also seek another son, firstborn sex shifts the intensity of son-seeking rather than turning it off. This attenuates the empirical contrast but does not change the model's main predictions.

of travel. The fixed costs include procedural and diagnostic fees, search costs, provider scarcity, stigma, and legal risk. The distance component includes travel time, fares, repeat visits, and foregone work.

The policy periods order the fixed cost as $\kappa_{\text{pre}} = \infty$, $\kappa_{\text{early}} > \kappa_{\text{expanded}} \geq 0$. The normalization $\kappa_{\text{pre}} = \infty$ denotes that the sex-selection option was effectively unavailable in the pre-service period. During the early rollout, legal first-trimester abortion became available, but the effective cost of acting on fetal-sex information remained high because the information and legal-service windows overlapped only narrowly. Expanded access lowered this cost through certified second-trimester services and a broader provider base.

The family chooses between two ways of obtaining a son. Under *continuation*, the family carries every pregnancy to term and continues childbearing until a son is born. The number of live births is geometric with success probability q, so the expected cost is $C_i^C = b_i/q$. Under *selection*, the family terminates each identified female pregnancy and carries the first male pregnancy to term. The expected number of female pregnancies terminated before the first male conception is $(1-q)/q$, and exactly one live birth occurs. The expected cost is therefore $C_i^S = b_i + K_t(\tau_i)(1-q)/q$.

The difference between the two strategies is $C_i^S - C_i^C = \frac{1-q}{q}[K_t(\tau_i) - b_i]$. The family therefore selects on fetal sex iff:

$$K_t(\tau_i) < b_i \qquad \Longleftrightarrow \qquad \kappa_t + m\tau_i < c + \varphi_i. \tag{2}$$

Selection is chosen when the effective cost of terminating and replacing a female conception is below the shadow cost of the additional live birth generated by continuation. For a family with $b_i > \kappa_t$, condition (2) defines an individual travel-time threshold, $\tau_{it}^* = (b_i - \kappa_t)/m$. The family selects when $\tau_i < \tau_{it}^*$ and continues childbearing when $\tau_i \geq \tau_{it}^*$.

The share selecting at travel time τ in period t is:

$$\sigma_t(\tau) = \Pr[b_i > \kappa_t + m\tau] = 1 - G(\kappa_t + m\tau)\,. \tag{3}$$

The selection share falls with travel time, rises when the policy lowers κ_t, and rises with the fertility squeeze. In particular, $\sigma_{\text{pre}}(\tau) = 0$ and $\sigma_{\text{early}}(\tau) \leq \sigma_{\text{expanded}}(\tau)$.

3.1 Testable Predictions

Prediction 1 (timing). *The sex-composition response should be weaker during the early rollout than under expanded access.* Before services, the effective cost of selection is prohibitive. During the early rollout, first-trimester abortion can affect the quantity of births, but the fixed cost of acting on fetal-sex information remains high. Male- and female-birth probabilities may therefore decline together, with little change in the female share. Once expanded access lowers κ_t, the female-birth probability and the female share of later births should fall relative to male births.

Prediction 2 (geography). *Under expanded access, the sex-composition response should be largest where services are easiest to reach and should weaken with travel time.* This follows directly from equation (3). The early rollout can generate a fertility response where first-trimester services are reachable without producing the same access gradient in birth composition.

Prediction 3 (margin switching). *Without effective access, son preference operates through continuation; with effective access, it increasingly operates through prenatal selection.* Under continuation, a family without a son has more live births, and those additional births may include both boys and girls. Under selection, expected female live births fall. Over the completed son-seeking sequence, the expected number of male live births remains one under either strategy, while total live births fall by the expected number of female births avoided.

Corollary (fertility squeeze). *At a given level of access, selection is more likely when the shadow cost of an additional birth is high.* A tighter desired-family-size constraint raises φ_i, increases b_i, expands the family's travel-time threshold, and raises the selection share in equation (3). Effective access and the fertility squeeze therefore jointly determine the selection threshold.

4 Data and Measurement

4.1 Nepal DHS birth histories

We combine four nationally representative GPS-linked rounds of the Nepal Demographic and Health Survey (NDHS): 2001, 2006, 2011, and 2016. All rounds collected retrospective birth histories from women aged 15–49. The birth histories record each child's month and year of birth, sex, birth order, and survival status, and the surveys also collect maternal and household characteristics. The rounds interviewed 8,726 ever-married women in 2001 and 10,793, 12,674, and 12,862 women in 2006, 2011, and 2016, respectively.

We construct two analysis samples. The first is a post-first-birth mother-year panel, used to distinguish changes in the number of births from changes in their composition. A mother enters the risk set after her first live birth, when the firstborn sex is known, and contributes one observation per subsequent calendar year observed before the survey. For each mother-year, we define three outcomes: any live birth, a male live birth, and a female live birth. The male- and female-birth outcomes are unconditional on a birth occurring and equal zero in years with no birth, so the any-birth outcome equals their sum by construction. The second is a child-level sample of second- and higher-order live births from 1990 to 2017, used to study the sex composition of births; the primary outcome is an indicator for a female birth. We assign births and mother-years to the three service periods defined above: the pre-service baseline (1990–2003, which includes the post-legalization but pre-service years), early rollout (2004–2007), and expanded access (2008–2017).

We restrict the sample to women married by 2016 and to outcomes through 2017 to avoid combining the original service rollout with the new statutory framework established by Nepal's 2018 reproductive health and public health legislation (Government of Nepal, 2018b,a).

4.2 Connectivity to city services

Next, we merge DHS clusters with measures of connectivity to city services. Our preferred measure is travel time in 2000 to the nearest city or town with a population of at least 50,000, from the Global Accessibility Map (Nelson, 2008). Because it is measured before legalization and the subsequent rollout of services, we interpret it as a predetermined measure of access to urban reproductive-health services. The capital city of Kathmandu is included in the candidate city set, so the measure captures general city connectivity rather than proximity to the capital specifically. In the main analysis, we classify clusters as being within or beyond two hours of the nearest city and use four travel-time cells to trace the dose response. Figure 2 maps this preferred measure on the common GPS-linked cluster support.

We also assess the robustness of our results using three alternative contemporary measures of city access. The first two are contemporary road-network travel times to the nearest city with at least 50,000 and 100,000 residents, computed using the Open Source Routing Machine (OSRM), an open-source routing engine for shortest paths on OpenStreetMap road networks (Luxen and Vetter, 2011). We transform each measure as the negative log of one plus travel time and standardize it within the relevant estimation sample to have mean zero and standard deviation one. The third is a population-weighted gravity measure of market connectivity, following Donaldson and Hornbeck (2016). For cluster c, the measure is $MA_c = \sum_j \frac{\text{Population}_j}{(\tau_{cj}+30)^\theta}$, where Population_j is the population of city j and τ_{cj} is the road-network travel time, in minutes, from cluster c to city j. The decay parameter θ governs how quickly a city's contribution falls with travel time; we set $\theta = 2$, giving an inverse-square penalty. The 30-minute offset prevents very short travel times from mechanically dominating the index. We take the log of this measure and standardize it in the same way. Thus, all three measures are oriented so that higher values indicate better access, and a one-unit increase represents a one-standard-deviation improvement in connectivity. Appendix Figure A4 maps these measures on the common GPS-linked cluster support. Together, the alternatives test whether the estimated access gradient depends on the city-size threshold, the routing method, or the functional form used to measure connectivity.

5 Empirical Strategy

We test when and where the expansion of legal abortion shifted the fertility response from the quantity margin to the sex-composition margin. The analysis combines three sources of variation. The first is the sex of the firstborn child. In a son-preferring setting, families whose first child is a girl face stronger pressure to continue childbearing to have a son than families whose first child is a boy. The second is the timing of the implementation of legal services. We compare outcomes in a pre-service baseline, an early first-trimester rollout, and an expanded-access period. The third is baseline connectivity to city services, which shifts whether the service bundle needed for selection could be reached. The design therefore asks whether later-birth outcomes changed differentially for firstborn-girl families relative to firstborn-boy families after legal access expanded, and whether that differential was larger where services were more reachable.

5.1 Quasi-random variation in firstborn sex

The key family-level variable is an indicator for whether the first live birth was a girl. Following previous work, we use firstborn sex as quasi-random variation in son-seeking incentives (Dahl and Moretti, 2008; Bhalotra and Cochrane, 2010; Milazzo, 2018; Anukriti et al., 2022). Under son preference, firstborn-girl families have stronger incentives to continue childbearing or select on fetal sex in pursuit of a son than otherwise comparable firstborn-boy families. We therefore compare subsequent fertility and birth-composition outcomes across the two groups.

We assess the quasi-randomness of firstborn sex in three ways. First, Appendix Table A1 compares the demographic and socioeconomic characteristics of firstborn-girl and firstborn-boy families separately by policy period. The differences are small, and period-specific joint F-tests fail to reject the null that the covariates do not jointly predict firstborn-girl status: the p-values are 0.395 in the pre-service baseline, 0.872 during the early rollout, and 0.646 during expanded access. Second, Appendix Table A2 shows that the firstborn-girl share remains close to one-half within the preferred travel-time cells. The corresponding joint balance tests fail to reject the null of no joint covariate predictability in every cell, with p-values ranging from 0.254 to 0.837. Third, Figure 1, Panel A, shows that the female share among first births fluctuates around the natural benchmark of 48.8 percent and does not shift discontinuously after legalization or expanded access. An omnibus test fails to reject the null that the bin-specific deviations from 48.8 percent are jointly zero (p-value $= 0.977$).

Together, these checks provide no evidence that first-birth sex ratios were manipulated by the reform or differed systematically across access cells and support treating firstborn sex as a quasi-random source of variation in son-seeking incentives.

5.2 Regression specifications

We estimate three related specifications. The first uses the post-first-birth mother-year panel to decompose changes in annual fertility into male- and female-birth components. The second uses second- and higher-order births to estimate the change in the female share of births. The third adds baseline connectivity to city services and tests whether these responses are larger inside the effective-access radius.

Annual birth hazards

We begin with the mother-year panel because it distinguishes changes in the number of births from changes in their composition. Let G_i equal one if mother i's first birth was a girl. Let $\mathscr{P} = \{E, A\}$, where P_{Et} indicates the early rollout and P_{At} indicates expanded access; the omitted period is the 1990–2003 pre-service baseline. Mothers are observed in DHS cluster c, district d, and calendar year t. For outcome $k \in \{\text{Any}, \text{Male}, \text{Female}\}$, we estimate

$$Y_{icdt}^{(k)} = \alpha^{(k)} + \gamma^{(k)} G_i + \sum_{p \in \mathscr{P}} \beta_p^{(k)} (G_i \times P_{pt}) + \delta_d^{(k)} + \omega_t^{(k)} + \boldsymbol{X}'_{icdt} \Gamma^{(k)} + \varepsilon_{icdt}^{(k)}. \tag{4}$$

The outcome $Y_{icdt}^{(k)}$ is one of three indicators: any live birth, a male live birth, or a female live birth in calendar year t. The male- and female-birth outcomes are unconditional on a birth occurring and equal zero in years with no birth. Consequently, $Y_{icdt}^{(\text{Any})} = Y_{icdt}^{(\text{Male})} + Y_{icdt}^{(\text{Female})}$ by construction. Because the three equations use the same observations and regressors, the any-birth coefficient is likewise the sum of the corresponding male- and female-birth coefficients. This accounting identity is central to the empirical design: a general fertility response should reduce male and female births together, whereas prenatal sex selection should produce a larger decline in female births.

District fixed effects, $\delta_d^{(k)}$, absorb time-invariant district differences. Calendar-year fixed effects, $\omega_t^{(k)}$, absorb aggregate changes in fertility and the main effects of the policy periods. The coefficient $\gamma^{(k)}$ captures the pre-service difference between firstborn-girl and firstborn-boy families. For the any-birth outcome, $\gamma^{(\text{Any})}$ captures the son-biased continuation premium and tests whether, before services were available, firstborn-girl families were more likely to continue childbearing. The coefficients $\beta_E^{(k)}$ and $\beta_A^{(k)}$ measure how this differential changed during the early-rollout and expanded-access periods. Standard errors are clustered at the district level.

The vector $\boldsymbol{X}_{icdt}$ includes the remaining controls and additional fixed effects added across specifications. The baseline specification includes district and calendar-year fixed effects and controls for household wealth, religion, rural status, parental education, cluster elevation, and distance to the national border. We then add mother year-of-birth fixed effects, which absorb differences in maternal cohort composition across retrospective survey histories. The most saturated specification also controls for the mother's stated son and fertility preferences, measured by the ideal fraction of sons, and the total ideal number of children.

Female share of later births

The annual-birth specification is unconditional on a birth occurring. We complement it with a child-level specification that directly examines the sex composition of realized births. Let S_{bicdt} equal one if the second- or higher-order birth b, born to mother i, is female and zero if it is male. We estimate

$$S_{bicdt} = \alpha + \gamma G_i + \sum_{p \in \mathscr{P}} \pi_p (G_i \times P_{pt}) + \delta_d + \omega_t + \boldsymbol{X}'_{bicdt} \Pi + \nu_{bicdt}. \tag{5}$$

District fixed effects, δ_d, absorb time-invariant district differences, while child birth-year fixed effects, ω_t, absorb aggregate changes in fertility conditions and sex ratios. The coefficients π_E and π_A measure how this differential changed during the early rollout and expanded-access periods.

All specifications include the common baseline controls. The second specification adds mother's year-of-birth fixed effects. The most saturated specification additionally includes birth-year-by-region fixed effects and controls for the mother's stated son and fertility preferences, measured by the ideal fraction of sons, and the total ideal number of children. Standard errors are clustered at the district level.

Equations (4) and (5) answer complementary questions. The mother-year equations show whether this compositional change arose through fewer female births, fewer births of both sexes, or some combination of the two. The female-share equation asks whether realized later births became more male-biased, but conditions on a birth occurring.

Access-radius triple difference

The access mechanism predicts that expanded legal abortion services should matter most where the relevant services were easier to reach. We test this prediction using baseline connectivity to city services. Let A_c indicate that cluster c is within two hours of the nearest city in 2000. Clusters beyond two hours form the omitted access group. We then replace this binary indicator with four travel-time cells to show how the response changes with access.

For each annual-birth outcome $k \in \{\text{Any}, \text{Male}, \text{Female}\}$, we estimate

$$
\begin{aligned}
Y_{icdt}^{(k)} = {} & \alpha^{(k)} + \gamma^{(k)} G_i + \rho^{(k)} A_c + \eta^{(k)} (G_i \times A_c) \\
& + \sum_{p \in \mathscr{P}} \left[\beta_p^{(k)} (G_i \times P_{pt}) + \lambda_p^{(k)} (P_{pt} \times A_c) + \theta_p^{(k)} (G_i \times P_{pt} \times A_c) \right] \\
& + \delta_d^{(k)} + \omega_t^{(k)} + \boldsymbol{X}'_{icdt} \Gamma^{(k)} + \varepsilon_{icdt}^{(k)}.
\end{aligned} \tag{6}
$$

In the specifications with separate district and year fixed effects, $\beta_p^{(k)}$ measures the change in the firstborn-girl differential outside the radius, while $\theta_p^{(k)}$ measures the additional change inside the radius. The implied inside-radius response is therefore $\beta_p^{(k)} + \theta_p^{(k)}$. The main triple-difference coefficient, $\theta_A^{(k)}$, asks whether the expanded-access change in the firstborn-girl differential was larger where city services were reachable.

We estimate the corresponding child-level specification by replacing $Y_{icdt}^{(k)}$ with the female-birth indicator S_{bicdt} and calendar-year fixed effects with child birth-year fixed effects. Its triple-difference coefficient asks whether the expanded-access change in the female share of later births was larger inside the access radius.

The hazard and female-share regressions follow the same specification ladder. Importantly, this design allows us to estimate a fully saturated triple difference specification that includes district-by-year, firstborn-girl-by-year, and firstborn-girl-by-district fixed effects. These fixed effects absorb time-varying district shocks, aggregate changes specific to firstborn-girl families, and persistent district differences by firstborn sex.[5] The most saturated specification also controls for the mother's ideal fraction of sons and total ideal number of children. Standard errors are clustered at the district level.

Finally, we test whether the results depend on the particular radius cutoff. We replace the binary access indicator A_c in equation (6) with each of the alternative standardized continuous connectivity measures C_c described in Section 4. All measures are oriented so that higher values indicate better access. The expanded-access triple interaction then measures how the firstborn-girl response changes with a one-standard-deviation improvement in connectivity.

[5]Note that the national difference-in-differences specifications in equations (4) and (5) by design cannot separately absorb national shocks that differentially affect firstborn-girl families in a given year. The saturated access specification absorbs such shocks using firstborn-girl-by-year fixed effects and identifies the triple-difference coefficient from whether the change is larger inside the predetermined access radius than outside it within district-years.

6 Results

We begin with descriptive evidence on birth sex composition by parity and firstborn sex. We then report the national benchmark, decomposing the annual birth response into male and female births and comparing the corresponding female-share estimate with the existing national evidence. We next turn to the paper's main result: whether these responses are concentrated where city services were easier to reach. Finally, we examine whether the reform changed measured outcomes among girls who were born.

6.1 Descriptive Evidence

Figure 1, panels (b) and (c), show the female shares of second and third births separately for firstborn-boy and firstborn-girl families, similar to Anukriti et al. (2022). These are the margins on which prenatal sex selection should appear. In a son-preferring setting, families with a firstborn girl have stronger incentives to seek a later son; if expanded access enabled prenatal selection, the female share should fall disproportionately among their later births. Before expanded access, female shares among firstborn-girl families do not show a sustained deficit relative to firstborn-boy families. After expanded access, the gap opens in the predicted direction: the female share falls more sharply among firstborn-girl families, especially at third births. The descriptive evidence therefore points to the same margin as the empirical design: the divergence appears among later-parity births, after access expands, and among families with stronger demand for a son.

6.2 National Benchmark: Quantity and Composition

Appendix Table A3 shows that, among later births that occurred, expanded access reduced the female share in firstborn-girl families. In the most saturated specification, the expanded-access coefficient is -2.83 percentage points (p-value < 0.05). Relative to the baseline female share of 49.1%, this is a 5.8% decline. By contrast, the early-rollout coefficient is statistically insignificant.

Table 1 asks whether this change reflects a general decline in births or a sex-specific decline in female births. The table uses the post-first-birth mother-year panel and reports annual probabilities of any birth, a male birth, and a female birth using the specification in equation (4).

The first row documents the baseline son-biased continuation pattern. Before legal services were available, firstborn-girl families were more likely to have another birth in a given year. Across specifications, they were 2.44–2.54 percentage points more likely to have any birth, 1.32–1.34 percentage points more likely to have a male birth, and 1.10–1.20 percentage points more likely to have a female birth (p-value < 0.01 for every estimate). The baseline continuation pattern is therefore stable across specifications. Thus, firstborn-girl families had more births before legal services were available, but those additional births included both boys and girls. This raises annual birth probabilities without necessarily changing the female share of births. Before expanded access, son preference operated primarily through continued childbearing.

The post-reform interactions show a different pattern. During the early rollout, the coefficients are small and statistically insignificant across all three outcomes in all specifications. After expanded access, however, the detectable response is concentrated in female births. In Panel A, which includes district and calendar-year fixed effects and the baseline controls, the female-birth coefficient is -0.47 percentage points (p-value < 0.1), while the male-birth coefficient is essentially zero. Panel B adds mother year-of-birth fixed effects; the female-birth coefficient remains -0.48 percentage points (p-value < 0.05), and the male-birth coefficient remains small and statistically insignificant. Panel C adds the mother's ideal fraction of sons and total ideal number of children, and gives the preferred estimates. The any-birth coefficient is -0.51 percentage points (p-value < 0.1), the female-birth coefficient is -0.49 percentage points (p-value < 0.05), and the male-birth coefficient remains essentially zero. The male- and female-birth coefficients differ at the 10% level.

The any-birth outcome equals the sum of the male- and female-birth outcomes. The decomposition therefore shows that the negative any-birth estimate is accounted for by fewer female births, not by fewer male births. Taken together, the results indicate that, after expanded access, later births in firstborn-girl families became more male-biased because fewer girls were born.

Point-estimate decomposition. The unconditional female-birth hazard can decline for two reasons: fewer later births may occur, or a smaller share of those births may be girls. We use the preferred estimates in Panel C of Table 1 to separate these two margins. Let B denote any later birth and F a female birth. Let subscripts 0 and 1 denote the baseline and expanded-access counterfactuals underlying the point estimates, and let $\Delta P(X) = P_1(X) - P_0(X)$. The probability identity is as follows (see Appendix C for the derivation):

$$\Delta P(B \cap F) = \underbrace{\Delta P(B)\, P_0(F \mid B)}_{\text{quantity margin}} + \underbrace{P_1(B)\left[P_1(F \mid B) - P_0(F \mid B)\right]}_{\text{sex-composition margin}}.$$

The first term measures how the female-birth hazard would change if the probability of any birth changed while the female share of births remained fixed. The second term measures the change in the female share of realized births, scaled by the expanded-access probability of any birth.

In the pre-service baseline, the annual hazards among firstborn-boy families are 18.67% for any birth and 9.15% for a female birth. Their ratio implies a baseline female share of 0.490. The estimated expanded-access change in the any-birth hazard is -0.51 percentage points. Holding the baseline female share fixed, this decline in births would reduce the female-birth hazard by -0.25 percentage points. This is the quantity component.

The estimated change in the female-birth hazard is -0.49 percentage points; hence, the sex-composition component is -0.24 percentage points. Therefore, 51.1% of the decline in the female-birth hazard is accounted for by fewer later births, while 48.9% is accounted for by a more male-biased composition of realized births.

6.3 Access Radius and Geographic Heterogeneity

The national estimates mask substantial geographic heterogeneity in effective access. Table 2 tests whether the annual birth response was larger where the services required for prenatal selection were easier to reach, using the specification in equation (6). The table defines clusters within two hours of a city as being inside the effective-access radius. In Columns (1) and (2), the rows without an access-cell label report the response beyond two hours, while the rows labeled "Under 2 hours" report the corresponding response inside the radius. Columns (3) and (4) add firstborn-girl-by-year fixed effects, which absorb the common outside-radius response; the reported access coefficient therefore measures the additional change inside the radius relative to outside it.

We begin with the early rollout. Column (1) includes district and calendar-year fixed effects and the common baseline controls. Beyond two hours, the early-rollout differentials are 0.05 percentage points for any birth, 0.13 percentage points for a male birth, and −0.07 percentage points for a female birth; all three are small and statistically insignificant. Inside the radius, the annual probability of any birth declines by 0.93 percentage points (p-value < 0.05). The female-birth estimate is -0.73 percentage points (p-value < 0.05), while the male-birth estimate is -0.21 percentage points and statistically insignificant. However, the male- and female-birth responses are not statistically different. Adding mother's year-of-birth fixed effects in Column (2) leaves the results qualitatively unchanged.

Column (3) estimates the fully saturated triple-difference specification. District-by-year fixed effects absorb time-varying district shocks, including local economic changes, health-service expansion, and district-specific implementation.[6] Firstborn-girl-by-year fixed effects absorb national changes in fertility, son preference, ultrasound use, or reporting that differentially affect firstborn-girl families. Firstborn-girl-by-district fixed effects absorb persistent differences by firstborn sex across districts. Column (4) further controls for the mother's ideal fraction of sons and total ideal number of children, and gives the preferred estimates. In this specification, the early-rollout any-birth differential inside the radius relative to outside it is -0.99 percentage points (p-value < 0.1), and the female- and male-birth differentials are not statistically different.

The corresponding female-share estimates in Table 4 are also statistically insignificant across the early-rollout specifications. The early rollout therefore provides evidence of a decline in the number of births where services were reachable, but no evidence of a change in their sex composition.

The pattern changes after expanded access. In Column (1), there is once again no detectable response beyond the two-hour radius: the expanded-access differentials for any birth, a male birth, and a female birth are small and statistically insignificant. Inside the radius, by contrast, the annual probability of any birth declines by 1.51 percentage points (p-value < 0.01). Most of this decline is associated with fewer female births. The female-birth probability declines by 1.26 percentage points (p-value < 0.01), while the male-birth estimate is -0.25 percentage points and statistically insignificant. Adding mother's year-of-birth fixed effects in Column (2) leaves this pattern nearly unchanged.

Column (3) shows the estimates from the fully saturated triple-difference specification. The any-birth differential inside the radius relative to outside it is -1.47 percentage points (p-value < 0.01). The female-birth differential is -1.29 percentage points (p-value < 0.01), while the male-

[6] These district-by-year fixed effects also absorb local shocks from the Maoist conflict, the 2015 earthquake, and the 2015 border blockade that are common to families within a district-year; confounding of the triple interaction would require such shocks to affect firstborn-girl families differentially.

birth differential remains small and statistically insignificant. Column (4) adds the stated son-preference controls and gives the preferred estimates. The coefficients remain nearly unchanged: the any-birth differential is -1.49 percentage points (p-value < 0.01), the female-birth differential is -1.30 percentage points (p-value < 0.01), and the male-birth differential is -0.19 percentage points and statistically insignificant (p-value $= 0.587$). The male- and female-birth coefficients are statistically different (p-value < 0.05). Thus, the decline in births inside the effective-access radius is predominantly accounted for by fewer female births.

Table 4 reports the corresponding estimates for the female share of later births. The early-rollout estimates provide no evidence of a compositional response and remain statistically insignificant across specifications. After expanded access, however, the female-share estimates display the same geographic concentration as the annual birth hazards. In Column (1), the expanded-access estimate beyond two hours is -0.96 percentage points and statistically insignificant (p-value $= 0.534$), while the estimate inside the radius is -6.79 percentage points (p-value < 0.01). The preferred estimate in Column (4) implies an additional inside-radius decline relative to the outside of 6.52 percentage points (p-value < 0.01). Relative to the baseline female share of 49.07%, this represents a 13.3% decline.

Figure 3 replaces the two-hour cutoff with four cells of predetermined travel time and reports the additional firstborn-girl response in each cell relative to places beyond four hours from a city. During the early rollout, the any-birth response is concentrated in better-connected places, but the female share displays no systematic access gradient. During expanded access, the female-birth and female-share responses are largest closest to cities and attenuate with travel time. The male-birth estimates remain statistically insignificant across access cells and show no comparable gradient. The response pattern therefore shows that the binary-radius result is not driven by an arbitrary two-hour cutoff.

Appendix Figure A5 provides suggestive evidence for the fertility-squeeze corollary in Section 3. We define a high baseline fertility squeeze as a district mean ideal family size below the sample median in the 2001 NDHS, before legalization. At either level of access, the expanded-access female-share estimates are more negative in high-squeeze districts, and the largest point estimate occurs where a high fertility squeeze coincides with being within two hours of a city, even though the high-minus-low-squeeze contrasts are imprecisely estimated.

Taken together, the access estimates reveal responses that the national averages conceal. During the initial first-trimester rollout, fertility declined where services were reachable, even though the national estimates show little aggregate response; male- and female-birth responses were not statistically distinguishable, leaving birth composition unchanged across the broader travel-time catchment. After the service bundle expanded, the response became both compositional and sharply geographic: female births and the female share declined in better-connected places, with little detectable response farther from cities, while male births showed no comparable access gradient. The geographic design therefore identifies effective access as the margin governing both when and where the reform changed birth composition.

6.4 Outcomes among Girls Who Are Born

Appendix Table A4 examines whether the change in who was born was accompanied by changes in outcomes among girls who were born. The early-rollout and expanded-access estimates are statistically indistinguishable from zero for under-five mortality, height-for-age, weight-for-age, and vaccinations in both the baseline and saturated specifications, with no systematic pattern across outcomes. Appendix Table A5 repeats the mortality analysis using the access-radius design and similarly finds no evidence that mortality changed differentially where services were reachable.

The contrast with Anukriti et al. (2022) helps clarify the margin of adjustment. In India, ultrasound diffusion increased the abortion of girls but also narrowed gender gaps in under-five mortality and parental investments among children who were born, a pattern consistent with substitution from postnatal toward prenatal discrimination. We find no evidence of an analogous reallocation in Nepal. The detectable response to expanded abortion access is therefore narrower: it changed the sex composition of births without measurably improving survival, health, or investments among girls who were born.

7 Robustness Checks

The access-radius results in the main text show that the expanded-access response is concentrated where services were reachable. This section addresses the robustness of our findings to functional form assumptions, measurement in retrospective birth histories, placebo timing, estimator choice, and sensitivity to the definition of the access radius.

Functional form. The annual birth-hazard results use linear probability models. Table 3 re-estimates the national specification using stacked female- and male-birth complementary log-log hazards and reports the female-birth hazard ratio relative to the male-birth hazard ratio. In the preferred specification, this ratio is statistically insignificant during the early rollout (p-value $=$ 0.267) and 0.895 during expanded access (p-value < 0.05). A ratio below one indicates that the female-birth hazard declined relative to the male-birth hazard. The sex-specific separation in the annual-birth results is therefore not an artifact of the specification.

Recall and migration. Retrospective birth histories could generate a spurious decline in female births if distant births are reported differently from recent births. Appendix Table A6 restricts the preferred female-birth hazard specification to mother-years within fifteen and ten years of the interview. The expanded-access estimates are -0.53 percentage points in the fifteen-year window (p-value < 0.05) and -0.49 percentage points in the ten-year window (p-value < 0.10). A separate concern is that access is measured at the survey cluster rather than at the mother's location at the time of birth. Appendix Table A7 restricts the sample to mothers who report residing in the survey cluster since before the relevant mother-year. The travel-time gradient remains qualitatively unchanged.

Placebo timing. We conduct two placebo exercises that ask whether the response appears when the firstborn-sex margin or effective abortion access should be weak. First, Appendix Table A8 restricts the mother-year panel to women aged at least 35 years with at least 3 prior births. Remaining fertility is limited in this sample, so firstborn sex should have little effect on the incentive to seek another son. The early-rollout and expanded-access interactions are close to zero and statistically insignificant for any birth, a male birth, and a female birth. Second, Appendix Figure A6 examines the late pre-legalization period, when ultrasound was present and diffusing but legal abortion services were unavailable. The joint tests fail to reject that the coefficients for the 1993–2001 pre-legalization bins are jointly zero for either the annual probability of a female birth ($p = 0.347$) or the female share of later births ($p = 0.717$). Lastly, if city connectivity were capturing only differential access to ultrasound, a similar composition gradient should have appeared before expanded abortion access. Instead, the early-rollout female-share estimates in Figure 3 display no systematic travel-time gradient; the gradient emerges only during expanded access. Together, these patterns are consistent with fetal-sex information being necessary but not sufficient for prenatal selection: effective access to the broader abortion-service bundle was the moving margin.

Alternative difference-in-differences estimators. Appendix Table A10 tests whether the female-share result is sensitive to the two-way fixed-effects OLS implementation. We recast the design using the repeated-cross-section estimator of Callaway and Sant'Anna (2021), treating firstborn-girl families as entering treatment with the legal-service rollout and firstborn-boy families as never-treated controls. The early-rollout estimates remain small and statistically insignificant, whereas the expanded-access estimates are negative and statistically significant. The doubly robust estimator of Sant'Anna and Zhao (2020) gives qualitatively similar results.

Alternative access measures. Appendix Table A9 replaces the preferred predetermined radius with standardized continuous measures based on contemporary road-network travel time to cities of at least 50,000 and 100,000 residents, computed using OSRM (Luxen and Vetter, 2011), and with a gravity-based measure of market connectivity. Following the market-access logic of Donaldson and Hornbeck (2016), the gravity measure sums the population of surrounding cities, discounting each city by inverse-square travel time. It is therefore higher for clusters that can reach larger nearby populations more quickly. The expanded-access gradient is negative for the female-birth hazard and the female share under all three measures. The early-rollout female-share gradient remains small and statistically insignificant.

2022 survey-wave extension. Our preferred sample uses the four NDHS waves fielded during the 1990–2017 outcome window. The 2022 wave was fielded entirely after that window, postdates Nepal's administrative restructuring, requiring its clusters to be mapped to the common 75-district geography, and followed the COVID-19 disruptions. The concern is therefore the alignment of interview-date covariates and cluster geography with the outcome window. Appendix Tables A11 and A12 report results that include the 2022 wave. The national female-birth and female-share estimates and the expanded-access connectivity gradients remain qualitatively unchanged.

8 Missing-Girls Accounting

Appendix Table A13 uses a back-of-the-envelope calculation to translate the composition estimate into an additional flow measure of missing girls at birth (Sen, 1990; Coale, 1991; Anderson and Ray, 2010), adapting the accounting exercise in Anukriti et al. (2022). Appendix Section B gives the details. Applying the preferred expanded-access estimate to average annual births in 2008–2017 implies an additional sex-selection flow equivalent of 11,892 girls missing at birth per year, equal to 1.91% of all births and 3.90% of expected female births. As a benchmark, our national estimate is approximately two-thirds of the estimate in Bhalotra and Cochrane (2010) that sex-selective abortion in India during 1995–2005 was equivalent to 6% of potential female births.

9 Conclusion

This paper studies how the expansion of legal abortion access changed fertility and the sex composition of births in Nepal. The staged rollout separates two margins. The initial first-trimester rollout reduced later-birth hazards where services were reachable, but the composition of births did not change. Once access expanded to include second-trimester services, medication abortion, and midlevel providers, the annual probability of a female birth declined, while male births did not. The results imply an additional annual flow of about 11,900 missing girls at birth.

The timing and geography clarify what changed. Son preference was present before the reform and initially operated through continued childbearing. Expanded access shifted part of this response from the quantity of births to their composition. The response was largest closest to cities and weakened with travel time, consistent with the concentration of abortion services and higher-tier medical capacity in better-connected places. Formal legality was therefore not sufficient: the unintended composition effect emerged when the service bundle needed to act on fetal-sex information became reachable.

These findings do not imply that abortion access should be restricted; instead, they point to a design problem for reproductive-rights reforms in son-preferring societies. The same investments that make safe services usable can also broaden the scope for prenatal selection, even when underlying preferences remain unequal.

An important question for future research is which complementary policies can preserve safe, legal abortion access while limiting sex selection. School-based programs can produce persistent improvements in gender attitudes (Dhar et al., 2022), but whether such interventions can reduce son preference and change fertility behavior remains an open question. How restrictions on prenatal sex determination can be enforced without pushing women toward unsafe procedures is also unresolved. Understanding how to address these two margins—the preference for sons and the capacity to act on fetal-sex information—remains central to designing rights-expanding reforms without reinforcing the inequalities they seek to reduce.

Declaration of Generative AI and AI-Assisted Technologies

During the preparation of this work, the authors used ChatGPT (OpenAI) and Claude (Anthropic) for writing assistance and code development. The authors reviewed the output and take full responsibility for the article's content.

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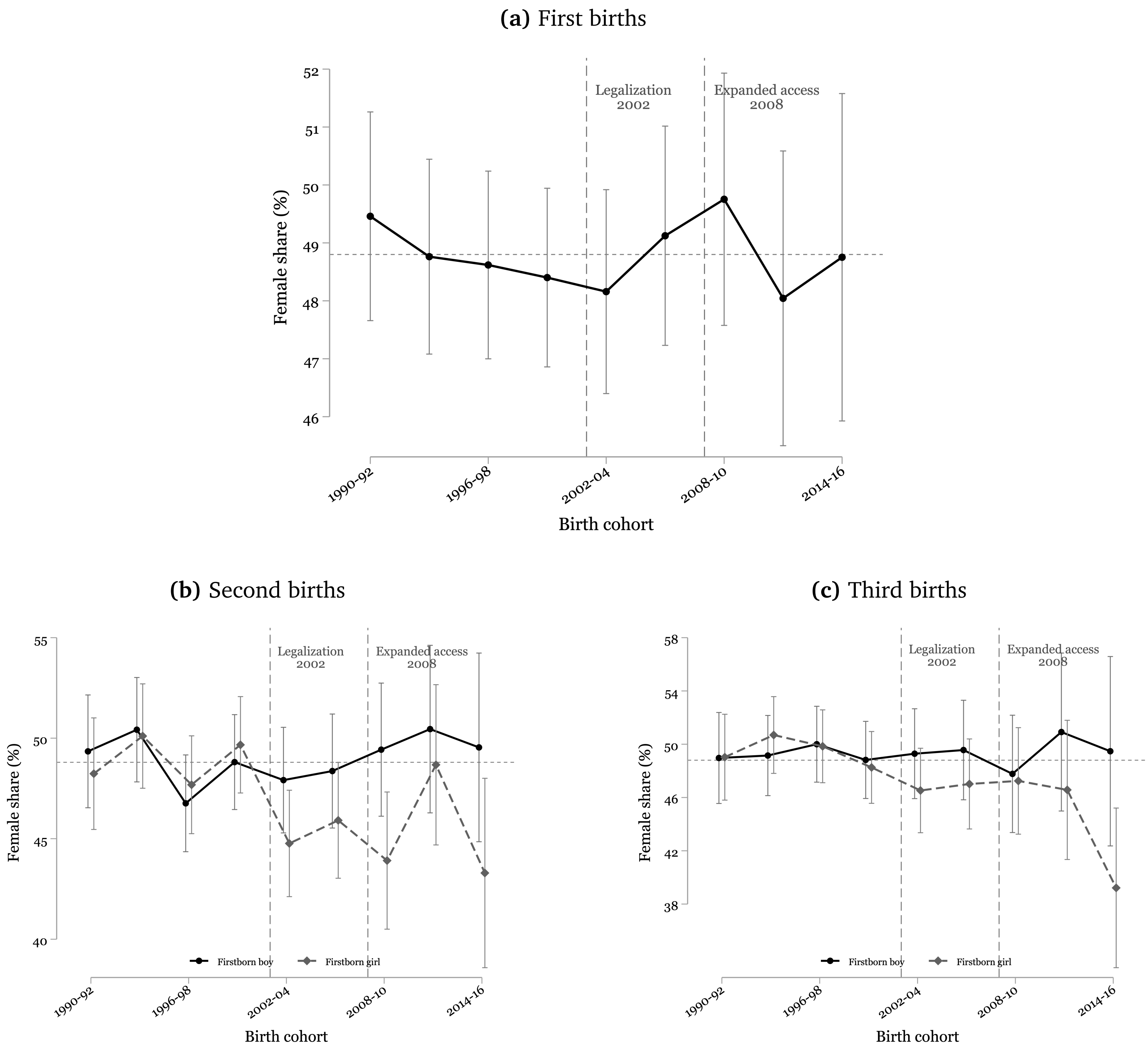
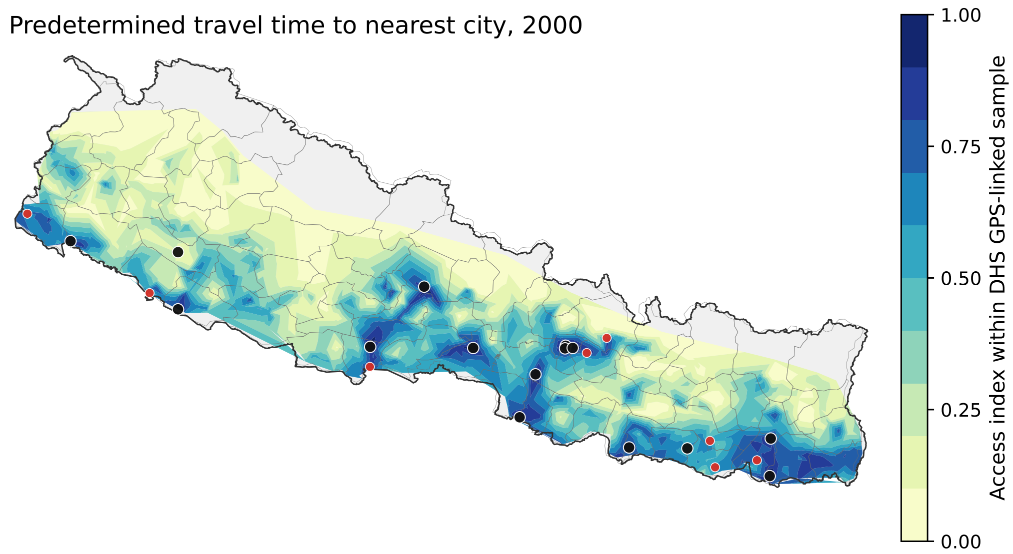
Tables and Figures

Figure 1: Female Share at Birth by Parity and Firstborn Sex



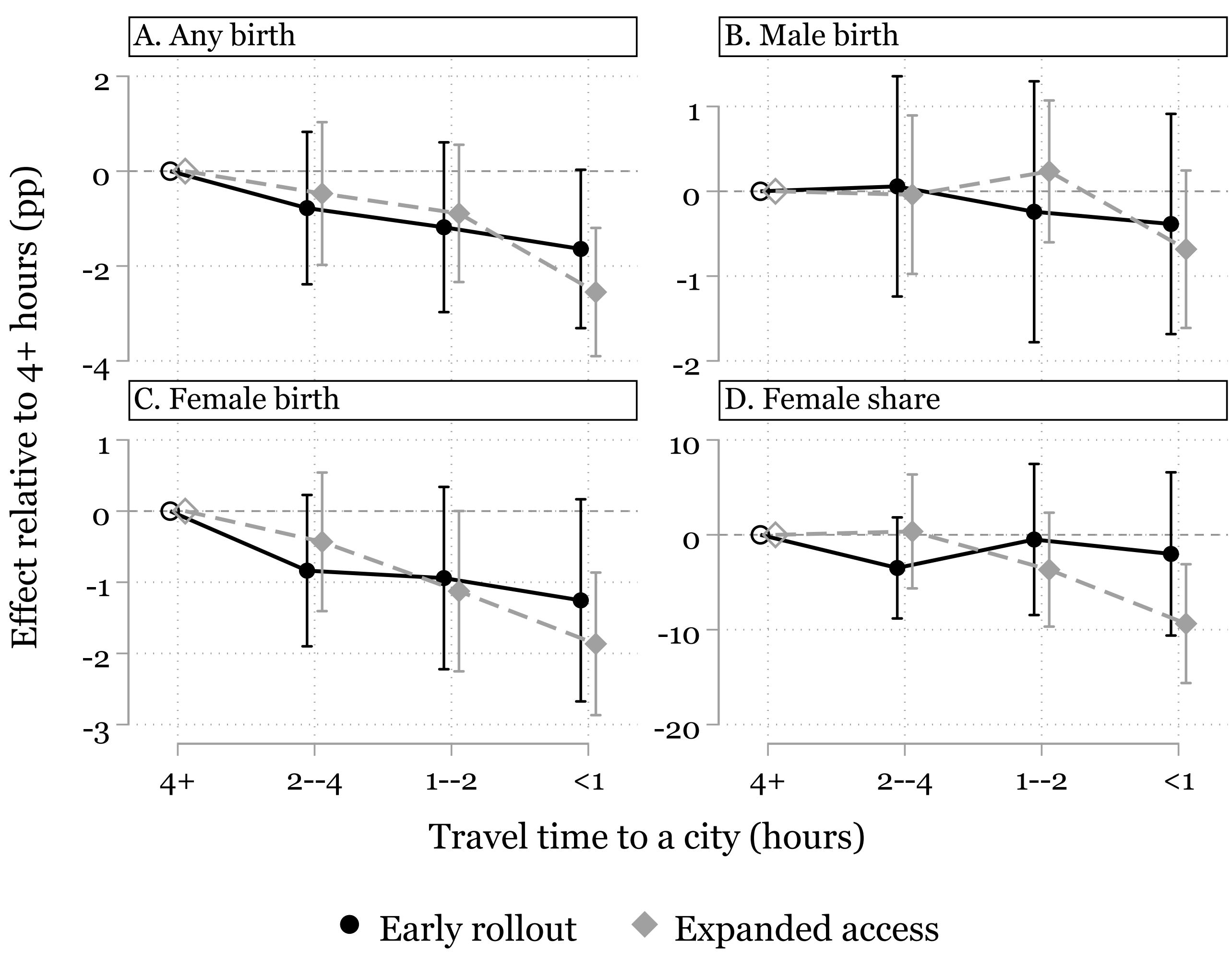
Notes: Panel (a) plots first births. Panels (b) and (c) split second and third births by firstborn-boy (solid black circles) and firstborn-girl (dashed gray diamonds) families. Points are unweighted female shares in three-year birth-cohort bins from 1990 to 2016 in the four GPS-linked NDHS waves; capped bars are 95 percent binomial confidence intervals. Dashed vertical lines mark legalization in 2002 and expanded access in 2008; the dashed horizontal line marks the natural sex ratio at birth (48.8% female). The omnibus p-value for the district-clustered joint test that all first-birth bin deviations from 48.8 percent are zero is 0.977 in Panel a.

Figure 2: Predetermined Geographic Access to City Services



Notes: Darker shading denotes shorter year-2000 travel time to the nearest city or town with at least 50,000 residents. The map shows a 0–1 access index across DHS GPS-linked cluster-wave observations in the analysis waves; gray regions lie outside the interpolation support. District boundaries use the 75-district geography. Larger black dots are cities of at least 100,000 residents; smaller red dots are cities of 50,000–99,999. The access measure predates abortion legalization and the service rollout.

Figure 3: Geographic Response by Rollout Period



Notes: The figure replaces the preferred two-hour indicator with four cells of year-2000 travel time to the nearest city with at least 50,000 residents. Points report the additional change in the firstborn-girl differential in each access cell relative to places beyond four hours, along with the 95% confidence interval. Panels report the annual probability of any birth, a male birth, and a female birth, and the female share of second- and higher-order births. Outcomes are multiplied by 100. The hazard panels include district-by-calendar-year and firstborn-girl-by-calendar-year fixed effects; the female-share panel includes district-by-child-birth-year and firstborn-girl-by-child-birth-year fixed effects. All panels also include firstborn-girl-by-district and mother year-of-birth fixed effects, together with the common controls and stated-fertility-preference controls used in the preferred access specification. Data: NDHS.

Table 1: National Benchmark: Annual Birth Hazards

	(1) Any birth	(2) Male birth	(3) Female birth
Panel A. District and calendar-year fixed effects			
Firstborn girl	2.507***	1.322***	1.184***
	(0.226)	(0.145)	(0.146)
Firstborn girl × Early rollout	-0.299	0.006	-0.305
	(0.326)	(0.240)	(0.209)
Firstborn girl × Expanded access	-0.490	-0.024	-0.466*
	(0.321)	(0.169)	(0.239)
Observations	363,935	363,935	363,935
Panel B. Mother year-of-birth fixed effects added			
Firstborn girl	2.538***	1.340***	1.198***
	(0.194)	(0.136)	(0.131)
Firstborn girl × Early rollout	-0.241	0.038	-0.279
	(0.303)	(0.235)	(0.198)
Firstborn girl × Expanded access	-0.518*	-0.037	-0.481**
	(0.284)	(0.165)	(0.218)
Observations	363,935	363,935	363,935
Panel C. Stated-preference controls added			
Firstborn girl	2.444***	1.341***	1.103***
	(0.203)	(0.135)	(0.138)
Firstborn girl × Early rollout	-0.265	0.018	-0.282
	(0.302)	(0.233)	(0.201)
Firstborn girl × Expanded access	-0.512*	-0.020	-0.492**
	(0.288)	(0.162)	(0.221)
Observations	360,216	360,216	360,216
Baseline mean	18.67	9.51	9.15

Notes: OLS regressions on the post-first-birth mother-year panel in GPS-linked clusters from the 2001, 2006, 2011, and 2016 NDHS waves. Outcomes are indicators for any birth, a male birth, or a female birth in a calendar year, multiplied by 100; male- and female-birth outcomes equal zero in no-birth years. Panel A includes district and calendar-year fixed effects and the common baseline controls: household wealth, religion, rural status, parental education, cluster elevation, and distance to the national border. Panel B adds mother's year-of-birth fixed effects. Panel C also controls for the mother's stated fertility and son preference, measured by the ideal fraction of sons and the total ideal number of children. The omitted period is the 1990–2003 pre-service baseline; early rollout denotes 2004–2007, and expanded access denotes 2008–2017 in the analysis sample. Baseline means are for firstborn-boy families in the omitted period. Standard errors in parentheses are clustered by district. Statistical significance at the 1, 5, and 10 percent levels is indicated by ***, **, and *.

Table 2: Access and Annual Birth Hazards: Two-Hour Travel-Time Radius

	Any birth				Male birth				Female birth			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Firstborn girl × Early rollout	0.05	0.11	–	–	0.13	0.16	–	–	-0.07	-0.05	–	–
	(0.44)	(0.41)			(0.31)	(0.31)			(0.32)	(0.30)		
Firstborn girl × Early rollout × Under 2 hours	-0.93**	-0.87**	-0.97*	-0.99*	-0.21	-0.17	-0.31	-0.31	-0.73**	-0.70**	-0.66	-0.69
	(0.41)	(0.38)	(0.53)	(0.54)	(0.32)	(0.30)	(0.40)	(0.40)	(0.28)	(0.27)	(0.45)	(0.45)
Firstborn girl × Expanded access	0.11	0.01	–	–	0.11	0.06	–	–	-0.00	-0.05	–	–
	(0.44)	(0.38)			(0.23)	(0.23)			(0.32)	(0.29)		
Firstborn girl × Expanded access × Under 2 hours	-1.51***	-1.43***	-1.47***	-1.49***	-0.25	-0.21	-0.18	-0.19	-1.26***	-1.22***	-1.29***	-1.30***
	(0.36)	(0.31)	(0.47)	(0.50)	(0.25)	(0.22)	(0.34)	(0.34)	(0.24)	(0.23)	(0.35)	(0.36)
Observations	363,935	363,935	363,935	360,216	363,935	363,935	363,935	360,216	363,935	363,935	363,935	360,216
Baseline mean	18.68	18.68	18.68	18.67	9.52	9.52	9.52	9.51	9.15	9.15	9.15	9.15
Baseline controls	×	×	×	×	×	×	×	×	×	×	×	×
District FE	×	×	×	×	×	×	×	×	×	×	×	×
Calendar-year FE	×	×	×	×	×	×	×	×	×	×	×	×
Mother year-of-birth FE		×	×	×		×	×	×		×	×	×
District-by-calendar-year FE			×	×			×	×			×	×
Firstborn-girl-by-calendar-year FE			×	×			×	×			×	×
Firstborn-girl-by-district FE			×	×			×	×			×	×
Son preference controls				×				×				×
p-value: Female birth = Male birth Early rollout												0.568
p-value: Female birth = Male birth Expanded access												0.030

Notes: OLS regressions on the post-first-birth mother-year panel in GPS-linked clusters from the 2001, 2006, 2011, and 2016 NDHS waves. Outcomes are indicators for any birth, a male birth, or a female birth in a calendar year, multiplied by 100; male- and female-birth outcomes equal zero in no-birth years. Travel time is year-2000 travel time to the nearest city or town with at least 50,000 residents. Rows report Firstborn girl × period interactions and Firstborn girl × period × travel-time access-cell interactions; rows without an access-cell label report the omitted access cell (2+ hours) where estimable. The omitted period is the 1990–2003 pre-service baseline; early rollout denotes 2004–2007, and expanded access denotes 2008–2017. All columns include the common baseline controls for household wealth, religion, rural status, parental education, cluster elevation, and distance to the national border, in addition to the fixed effects shown in the bottom rows. Column (2) adds mother's year-of-birth fixed effects. Columns (3)–(4) add district-by-calendar-year, firstborn-girl-by-calendar-year, and firstborn-girl-by-district fixed effects. Column (4) also controls for the mother's stated son preference, measured by the ideal fraction of sons and the total ideal number of children. In columns (3)–(4), the omitted-cell firstborn-girl-by-period effects are absorbed by the firstborn-girl-by-calendar-year fixed effects and shown as dashes. Baseline means are column-specific means among firstborn-boy mother-years in the omitted period. The bottom two rows report p-values for equality of the male-birth and female-birth access-cell coefficients in column (4). Standard errors in parentheses are clustered by district. Statistical significance at the 1, 5, and 10 percent levels is indicated by ***, **, and *.

Table 3: Nonlinear Hazard-Ratio Robustness: Female vs. Male Birth Hazards

	Female-birth HR / Male-birth HR		
	(1)	(2)	(3)
Firstborn girl	0.991	0.991	0.976
	(0.019)	(0.019)	(0.019)
Firstborn girl × Early rollout	0.949	0.948	0.952
	(0.041)	(0.042)	(0.043)
Firstborn girl × Expanded access	0.900**	0.900**	0.895**
	(0.047)	(0.048)	(0.047)
Mother-year observations	363,935	363,935	360,216
Birth events	56,933	56,933	56,339
District FE	×	×	×
Calendar-year FE	×	×	×
Baseline controls	×	×	×
Mother year-of-birth FE		×	×
Son preference controls			×
Early-rollout p-value: female-birth HR = male-birth HR	0.233	0.229	0.267
Expanded-access p-value: female-birth HR = male-birth HR	0.045	0.047	0.036

Notes: The table reports female-birth hazard ratios divided by male-birth hazard ratios from stacked cause-specific complementary log-log models on the same post-first-birth mother-year sample as Table 1. A value below one means that the female-birth hazard response is lower than the male-birth hazard response. Each model stacks one male-birth and one female-birth risk row for each mother-year and fully interacts the listed controls and fixed effects with the female-birth indicator. All columns include district and calendar-year fixed effects and the common baseline controls: household wealth, religion, rural status, parental education, cluster elevation, and distance to the national border. Column (2) adds mother's year-of-birth fixed effects. Column (3) also controls for the mother's stated fertility and son preference, measured by the ideal fraction of sons and the total ideal number of children. The omitted period is the 1990–2003 pre-service baseline; early rollout denotes 2004–2007, and expanded access denotes 2008–2017. Standard errors in parentheses are clustered by district. Data: NDHS. Statistical significance at the 1, 5, and 10 percent levels is indicated by ***, **, and *.

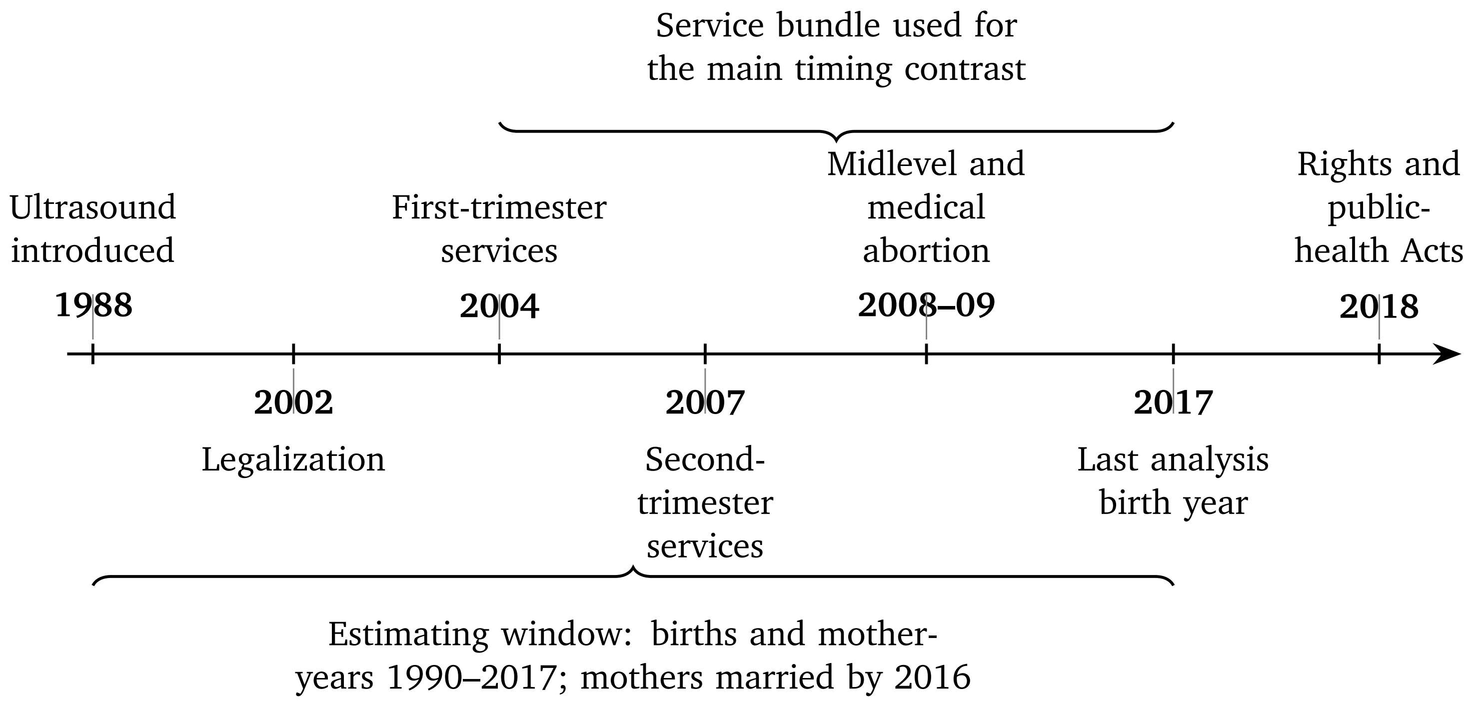
Table 4: Access and the Composition of Births: Two-Hour Travel-Time Radius

	Female share among second- and higher-order births			
	(1)	(2)	(3)	(4)
Firstborn girl × Early rollout	-0.98	-0.97	–	–
	(1.40)	(1.40)		
Firstborn girl × Early rollout × Under 2 hours	-1.39	-1.50	0.19	0.27
	(1.92)	(1.92)	(2.53)	(2.55)
Firstborn girl × Expanded access	-0.96	-1.01	–	–
	(1.53)	(1.53)		
Firstborn girl × Expanded access × Under 2 hours	-6.79***	-6.66***	-6.57***	-6.52***
	(1.81)	(1.79)	(2.33)	(2.34)
Observations	55,287	55,287	55,234	54,670
Baseline mean	49.06	49.06	49.05	49.07
Baseline controls	×	×	×	×
District FE	×	×	×	×
Child year-of-birth FE	×	×	×	×
Mother year-of-birth FE		×	×	×
District-by-child-year FE			×	×
Firstborn-girl-by-child-year FE			×	×
Firstborn-girl-by-district FE			×	×
Son preference controls				×

Notes: OLS regressions on second- and higher-order births in GPS-linked clusters from the 2001, 2006, 2011, and 2016 NDHS waves; the dependent variable is an indicator for a female birth, multiplied by 100. Travel time is year-2000 travel time to the nearest city or town with at least 50,000 residents. Rows report coefficients on Firstborn girl × period interactions and Firstborn girl × period × travel-time access-cell interactions. Rows without an access-cell label report the omitted access cell (2+ hours) where estimable. The omitted period is the 1990–2003 pre-service baseline; early rollout denotes 2004–2007 and expanded access denotes 2008–2017 in the analysis sample. Column (1) includes district and child year-of-birth fixed effects and the common baseline controls: household wealth, religion, rural status, parental education, cluster elevation, and distance to the national border. Column (2) adds mother's year-of-birth fixed effects. Columns (3) and (4) add district-by-child-year, firstborn-girl-by-child-year, and firstborn-girl-by-district fixed effects. Column (4) also controls for the mother's stated fertility and son preference, measured by the ideal fraction of sons and the total ideal number of children. In columns (3) and (4), the omitted access-cell period effects are absorbed by the firstborn-girl-by-child-year fixed effects and shown as dashes. Baseline means are for firstborn-boy families in the pre-service period. Standard errors in parentheses are clustered by district. Statistical significance at the 1, 5, and 10 percent levels is indicated by ***, **, and *.

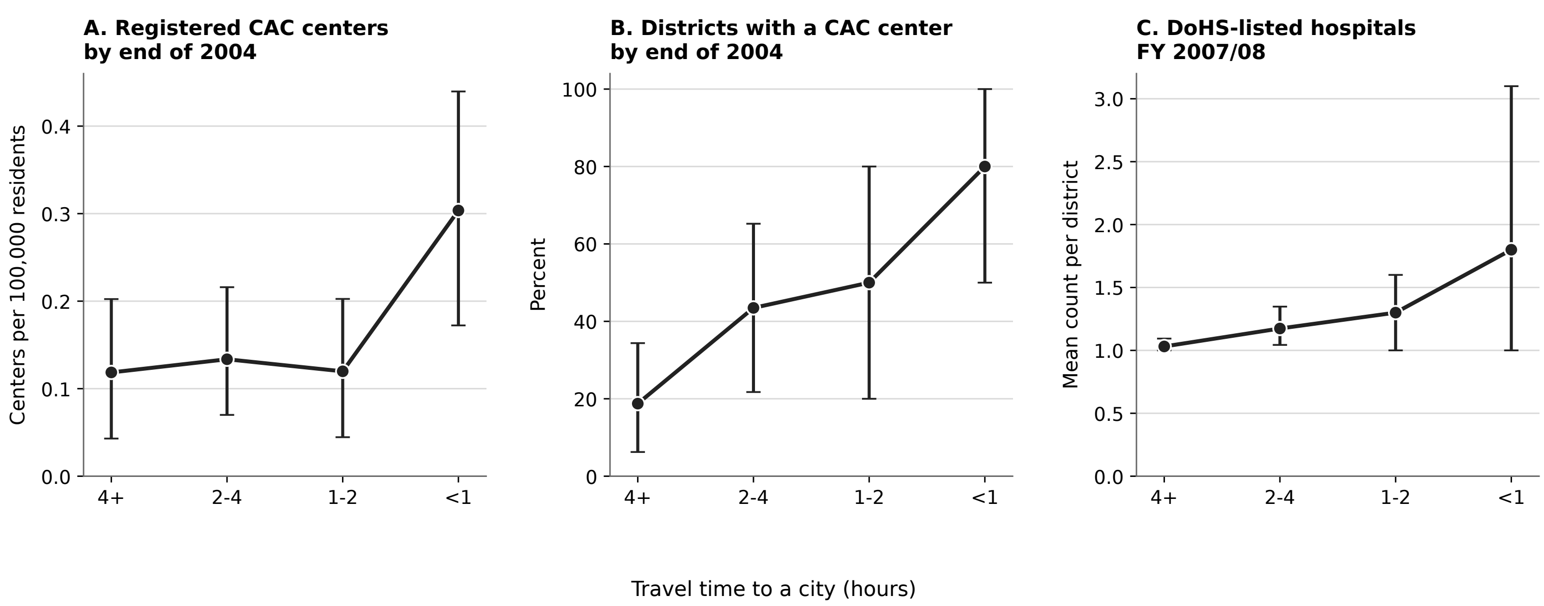
A Additional Tables and Figures

Figure A1: Timeline of Abortion Services Rollout



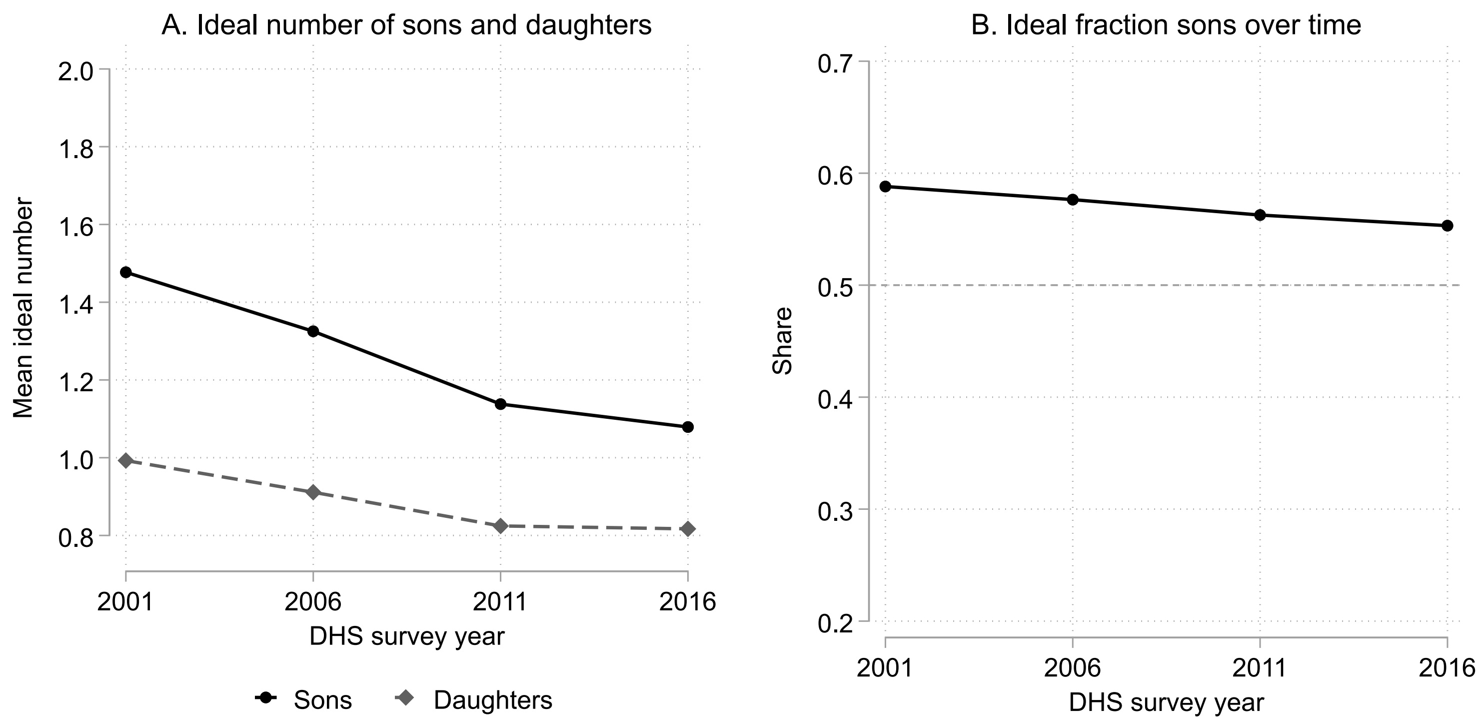
Notes: The figure summarizes the chronology for abortion service rollout in Nepal.

Figure A2: Medical-Service Availability by Predetermined City Access



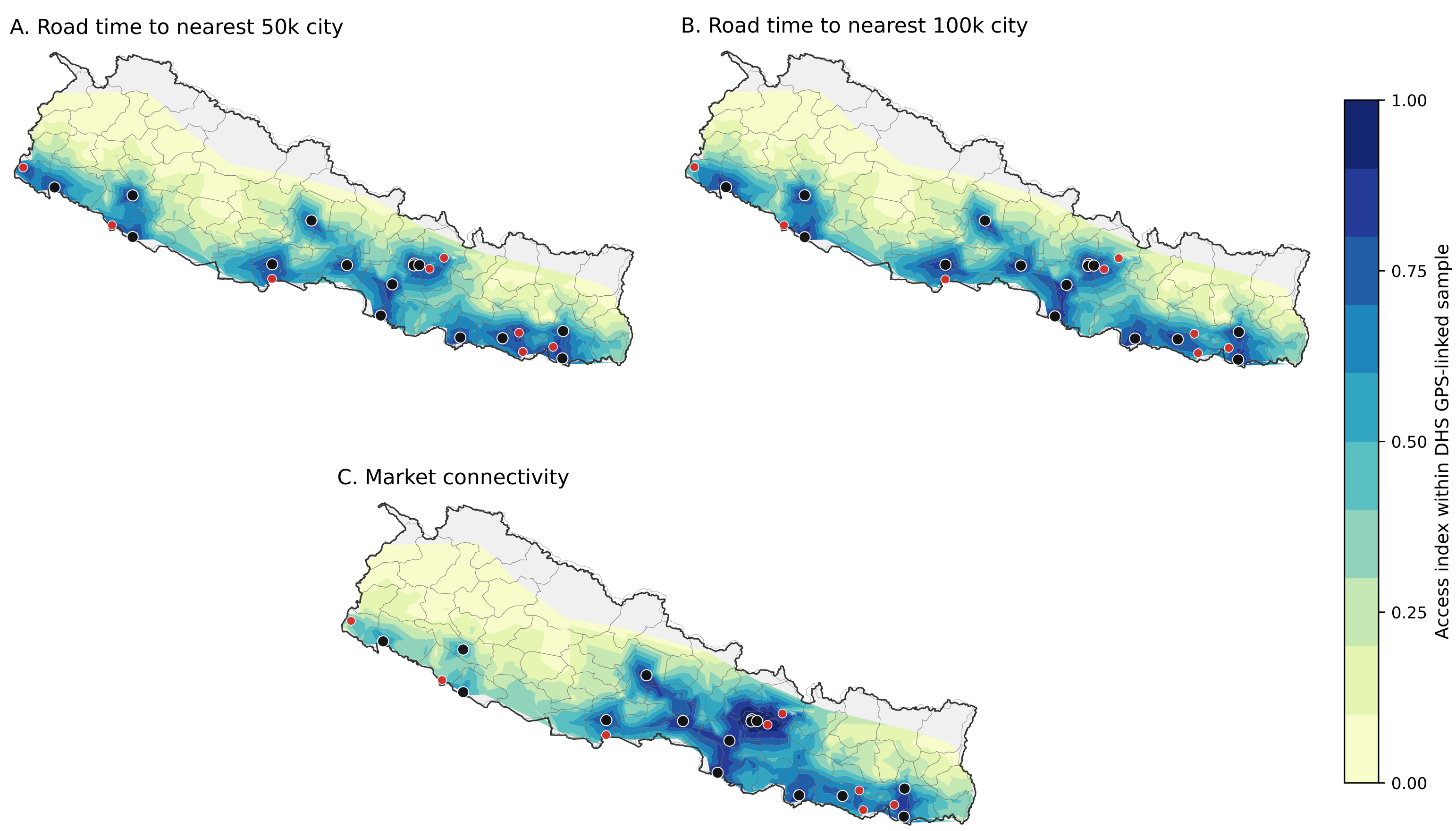
Notes: Districts are classified by their year-2000 population-weighted median travel time to the nearest city with at least 50,000 residents. Panel A reports registered Comprehensive Abortion Care (CAC) centers by the end of 2004 per 100,000 residents. Panel B reports the share of districts with at least one registered CAC center by the end of 2004. Panel C reports the mean number of hospitals listed in FY 2064/65 (2007/08). Vertical bars are 95 percent district-bootstrap confidence intervals based on 2,000 replications.

Figure A3: Son Preference in Stated Fertility Preferences



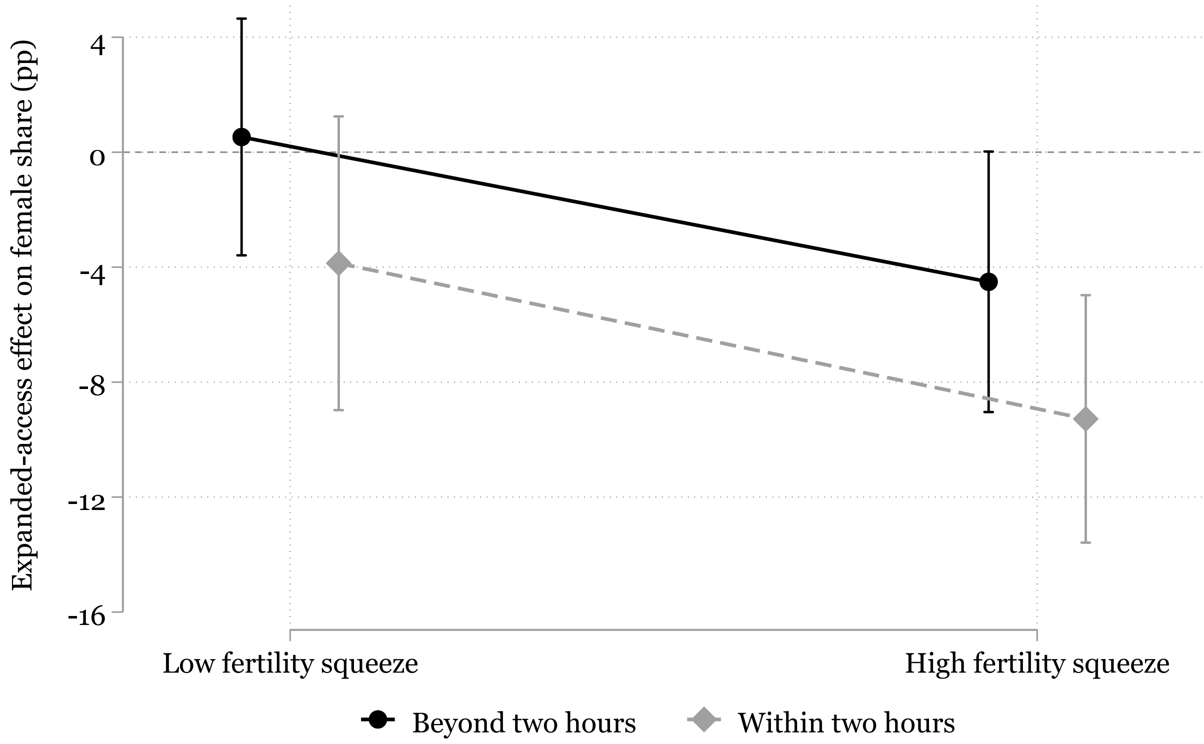
Notes: The figure reports DHS-weighted means among distinct respondents in the 2001, 2006, 2011, and 2016 NDHS waves. Panel A plots the reported ideal number of sons and daughters. Panel B plots the ideal fraction of sons, (ideal sons + 0.5 × ideal either sex)/ideal total children; the dashed line marks parity at 0.5.

Figure A4: Robustness Measures: Geographic Variation in Access to City Services



Notes: Darker shading denotes higher access. Each panel maps a 0–1 rank index over the common support of DHS GPS-linked cluster-wave observations in the analysis sample; gray regions lie outside the interpolation support. District boundaries use the 75-district geography. Larger black dots are cities with at least 100,000 residents; smaller red dots are cities with 50,000–99,999 residents. Panels A and B use the negative log of one plus contemporary OSRM road-network travel time, in minutes, to the nearest city with at least 50,000 and 100,000 residents, respectively (Luxen and Vetter, 2011). Panel C uses the corresponding log population-weighted gravity measure with an inverse-square travel-time penalty and a 30-minute offset (Donaldson and Hornbeck, 2016). All three measures use road-network times computed in June 2026 through the public OSRM routing service and a GeoNames city gazetteer downloaded in June 2026. The regression measures are standardized to have mean zero and standard deviation one.

Figure A5: Fertility Squeeze and Effective Access



Notes: The figure reports implied Firstborn girl × Expanded access effects on the female share of second- and higher-order births by the preferred two-hour access indicator and a baseline fertility-squeeze indicator. High fertility squeeze denotes districts below the sample median of the mean ideal family size in the 2001 NDHS, before legalization. The specification otherwise follows Column (2) of Table 4. Vertical bars are 95 percent confidence intervals based on standard errors clustered by district. Data: NDHS.

Table A1: First-Birth Covariate Balance by Period

	Pre-service baseline 1990–2003		Early rollout 2004–2007		Expanded access 2008–2017		All years
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	FBB	FBG	FBB	FBG	FBB	FBG	FBB–FBG
Rural	0.6800	0.6747	0.6182	0.6300	0.4941	0.4821	0.0046
	(0.4665)	(0.4685)	(0.4860)	(0.4829)	(0.5001)	(0.4998)	(0.0062)
Mother's education (years)	2.4481	2.4605	4.2721	4.2781	6.0417	6.1679	-0.0468
	(3.6384)	(3.6615)	(4.1066)	(4.1157)	(4.1057)	(4.0666)	(0.0524)
Father's education (years)	5.6317	5.5476	6.6810	6.6481	7.3832	7.4253	0.0443
	(4.0736)	(4.1191)	(3.7952)	(3.7530)	(3.5277)	(3.3956)	(0.0513)
Mother's age at first birth	19.9095	19.9729	20.2875	20.2146	20.7068	20.8300	-0.0571
	(3.0812)	(3.1419)	(3.3086)	(3.2546)	(3.4706)	(3.3978)	(0.0413)
Household wealth quintile	2.9894	2.9712	2.9670	2.9201	2.9141	2.9663	0.0087
	(1.4458)	(1.4596)	(1.4407)	(1.4430)	(1.3758)	(1.3828)	(0.0184)
Hindu	0.8706	0.8673	0.8695	0.8779	0.8651	0.8746	-0.0010
	(0.3356)	(0.3393)	(0.3369)	(0.3274)	(0.3417)	(0.3312)	(0.0043)
Observations	8100	7663	1878	1827	2469	2377	24314
Joint F-test p-value	0.395		0.872		0.646		

Notes: The sample consists of first births in GPS-linked clusters from the 2001, 2006, 2011, and 2016 NDHS waves, with nonmissing access measures, geography controls, and covariates. Periods are defined by the child's birth year: pre-service baseline (1990–2003), early rollout (2004–2007), and expanded access (2008–2017). FBB denotes firstborn boy, and FBG denotes firstborn girl. Columns (1)–(6) report means by firstborn sex and period; parentheses report standard deviations. Column (7) reports the pooled firstborn-boy minus firstborn-girl difference, with standard errors in parentheses. The observation count in column (7) is the pooled count across columns (1)–(6). The joint F-test row reports period-specific p-values from OLS regressions of firstborn-girl status on the listed covariates; the null is that all covariate coefficients are zero. Education is measured in completed years; household wealth is the DHS wealth quintile. Statistical significance at the 1, 5, and 10 percent levels is indicated by ***, **, and *.

Table A2: First-Birth Balance by Predetermined Travel Time

Period	Access cell	N	Firstborn girl share (%)	Joint balance p-value
Pre-service	Under 1 hour	2,788	49.3	0.837
Pre-service	1–2 hours	2,869	47.9	0.440
Pre-service	2+ hours	10,106	48.6	0.415
Early rollout	Under 1 hour	670	49.9	0.750
Early rollout	1–2 hours	682	48.4	0.403
Early rollout	2+ hours	2,353	49.4	0.254
Expanded access	Under 1 hour	963	49.5	0.732
Expanded access	1–2 hours	833	50.4	0.274
Expanded access	2+ hours	3,050	48.5	0.751

Notes: The table reports first-birth balance within cells of the preferred predetermined travel-time measure. The cells separate clusters less than one hour, one to two hours, and two or more hours from the nearest city or town with at least 50,000 residents. The joint balance p-value comes from regressing firstborn-girl status on rural status, mother's and father's years of education, mother's age at first birth, household wealth quintile, and a Hindu indicator in the indicated cell. The sample is restricted to first births from the 2001, 2006, 2011, and 2016 NDHS waves in GPS-linked clusters, with nonmissing travel time, geography controls, and covariates. Travel time is measured in 2000, before abortion legalization.

Table A3: National Benchmark: Female Share of Later Births

	(1)	(2)	(3)	(4)
	Female share among second- and higher-order births			
Firstborn girl	-0.175	-0.179	-0.189	-0.488
	(0.479)	(0.484)	(0.490)	(0.491)
Firstborn girl × Early rollout	-1.120	-1.144	-1.096	-1.058
	(1.087)	(1.088)	(1.091)	(1.107)
Firstborn girl × Expanded access	-2.769**	-2.768**	-2.799**	-2.833**
	(1.296)	(1.293)	(1.271)	(1.274)
Observations	55,287	55,286	55,286	54,724
Baseline mean	49.06	49.06	49.06	49.08
District FE	×	×	×	×
Child birth-year FE	×	×	×	×
Baseline controls	×	×	×	×
Mother year-of-birth FE		×	×	×
Birth-year-by-region FE			×	×
Son preference controls				×

Notes: OLS regressions on second- and higher-order births in GPS-linked clusters from the 2001, 2006, 2011, and 2016 NDHS waves. The dependent variable is an indicator for a female birth, multiplied by 100. Column (1) includes district and child birth-year fixed effects and the common baseline controls: household wealth, religion, rural status, parental education, cluster elevation, and distance to the national border. Column (2) adds mother's year-of-birth fixed effects. Column (3) adds birth-year-by-region fixed effects. Column (4) adds controls for the mother's reported ideal fraction of sons and total ideal number of children. The omitted period is the 1990–2003 pre-service baseline; early rollout denotes 2004–2007, and expanded access denotes 2008–2017 in the analysis sample. Baseline means are for firstborn-boy families in the omitted period. Standard errors in parentheses are clustered by district. Statistical significance at the 1, 5, and 10 percent levels is indicated by ***, **, and *.

Table A4: Child Quality Outcomes

	(1) Under-five mortality	(2) Height-for-age z-score	(3) Weight-for-age z-score	(4) Vaccinations
Panel A. Baseline controls				
Firstborn girl × Female child	0.77	-0.056	-0.000	-0.102
	(0.74)	(0.116)	(0.093)	(0.109)
Firstborn girl × Female child × Early rollout	-2.37	-0.054	-0.077	-0.069
	(1.66)	(0.142)	(0.116)	(0.253)
Firstborn girl × Female child × Expanded access	-3.35	0.034	-0.061	0.130
	(2.11)	(0.141)	(0.118)	(0.184)
Baseline mean	10.6	-2.28	-1.76	6.47
N	31,653	6,469	6,472	13,230
Panel B. Saturated controls				
Firstborn girl × Female child	0.69	-0.082	-0.007	-0.135
	(0.74)	(0.115)	(0.095)	(0.110)
Firstborn girl × Female child × Early rollout	-2.37	-0.017	-0.049	-0.051
	(1.67)	(0.145)	(0.121)	(0.256)
Firstborn girl × Female child × Expanded access	-3.38	0.034	-0.078	0.138
	(2.16)	(0.141)	(0.119)	(0.183)
Baseline mean	10.6	-2.28	-1.76	6.49
N	31,337	6,439	6,442	13,104

Notes: The table reports triple-difference estimates for second-and-higher-order births in GPS-linked clusters from the 2001, 2006, 2011, and 2016 NDHS waves. The first row in each panel reports the level coefficient on firstborn-girl status interacted with child sex (the pre-service female-child differential in firstborn-girl families); subsequent rows interact it with the indicated policy period. The omitted period is the 1990–2003 pre-service baseline; early rollout denotes 2004–2007 and expanded access denotes 2008–2017 in the analysis sample. Panel A includes district fixed effects, child birth-year fixed effects, household wealth-quintile fixed effects, region fixed effects, religion fixed effects, and father- and mother-education fixed effects. Panel B adds female-child-by-birth-year fixed effects, female-child-by-district fixed effects, firstborn-girl-by-birth-year fixed effects, and linear controls for the mother's reported ideal fraction sons and total ideal number of children. Under-five mortality is multiplied by 100 and restricted to children observed for at least 60 months after birth. Vaccinations are the number of recorded vaccines. Baseline means are omitted-cell means among male later-born children in firstborn-boy families during the pre-service baseline, in each column's regression sample. Standard errors in parentheses are clustered by district. Statistical significance at the 1, 5, and 10 percent levels is indicated by ***, **, and *.

Table A5: Child Mortality: Saturated Travel-Time Robustness

	Infant mortality	Under-five mortality
Travel-time radius: under 2 hours		
Firstborn girl × Female child × Early rollout × Under 2 hours	2.69	3.22
	(2.09)	(2.77)
Firstborn girl × Female child × Expanded access × Under 2 hours	1.79	0.17
	(2.99)	(4.55)
Observations	45,217	32,849
Baseline mean	8.31	11.11

Notes: OLS regressions on second- and higher-order births in the GPS-linked child sample from the 2001, 2006, 2011, and 2016 NDHS waves. Outcomes are indicators for mortality by age one and by age five, multiplied by 100. The infant-mortality sample requires the child to be observed for at least 12 months; the under-five sample requires at least 60 months. Rows report coefficients on Firstborn girl × Female child × period × access-cell interactions from fully saturated quadruple-difference specifications. The omitted access cell is two or more hours from the nearest city with at least 50,000 residents. All regressions include district-by-child-year and mother year-of-birth fixed effects; the common baseline controls for household wealth, religion, rural status, parental education, cluster elevation, and distance to the national border; the mother's ideal fraction of sons and total ideal number of children; and saturated firstborn-sex and child-sex year and district margins. Baseline means are for firstborn-boy families with male children in the pre-service period. Standard errors in parentheses are clustered by district. Statistical significance at the 1, 5, and 10 percent levels is indicated by ***, **, and *.

Table A6: Measurement Robustness: Annual Female-Birth Hazard

	(1) Female birth	(2) Female birth
Firstborn girl	1.139***	1.109***
	(0.151)	(0.185)
Firstborn girl × Early rollout	-0.313	-0.342
	(0.211)	(0.254)
Firstborn girl × Expanded access	-0.526**	-0.490*
	(0.235)	(0.272)
Observations	327,382	270,185
Baseline mean	8.58	7.94
Sample	≤15 years recall	≤10 years recall

Notes: OLS regressions on the post-first-birth mother-year panel using the preferred annual-hazard specification. The dependent variable is an indicator for a female birth in a calendar year, multiplied by 100. All columns control for the mother's stated son preference, measured by the ideal fraction of sons and the total ideal number of children. The common baseline controls are household wealth, religion, rural status, parental education, cluster elevation, and distance to the national border. Column (1) restricts the sample to mother-years within fifteen years of the survey interview, and column (2) to mother-years within ten years, limiting scope for recall error and differential omission of distant births. The sample uses GPS-linked clusters from the 2001, 2006, 2011, and 2016 NDHS waves. Baseline mean is the mean outcome for firstborn-boy families during the 1990–2003 pre-service baseline. Standard errors in parentheses are clustered by district. Statistical significance at the 1, 5, and 10 percent levels is indicated by ***, **, and *.

Table A7: Survey-Cluster Residence and the Travel-Time Estimates

	(1) Female birth	(2) Female birth
Firstborn girl × Expanded access × Under 2 hours	-1.09***	-0.76*
	(0.40)	(0.40)
Observations	253,611	233,923
Baseline mean	9.11	9.03
Sample	All classified mother-years	Resident since risk year

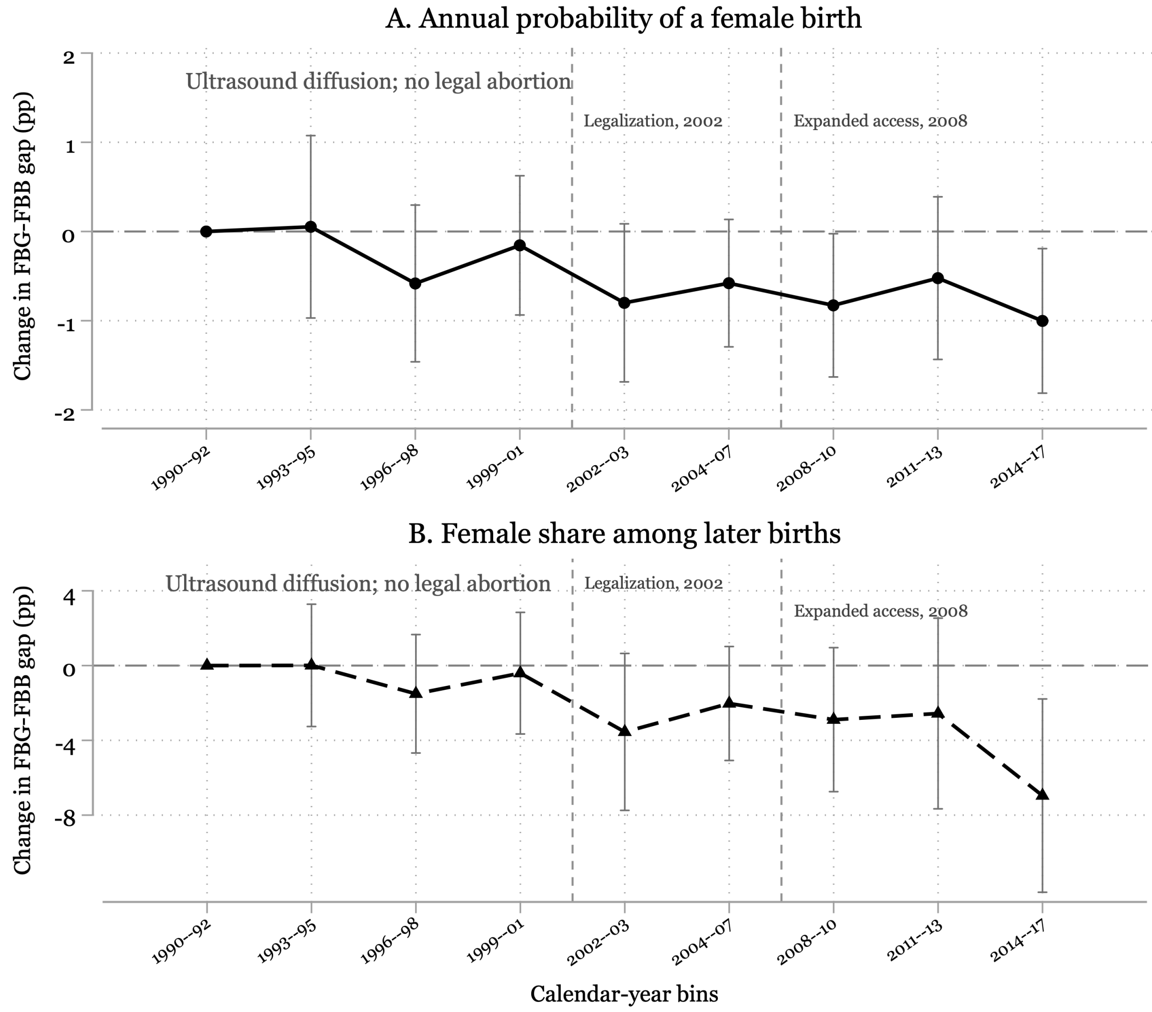
Notes: OLS regressions on the post-first-birth mother-year panel in GPS-linked clusters; the dependent variable is an indicator for a female birth in a calendar year, multiplied by 100. The table restricts to waves in which the raw NDHS individual file reports years in current residence. A mother-year is classified as resident since the risk year if the mother reports always living in the current place or reports at least as many years in the current place as years since that calendar year; visitor responses are excluded from the residence split. The reported coefficient is the additional expanded-access firstborn-girl effect within two hours relative to the omitted cell, which is two or more hours from the nearest city with at least 50,000 residents. All specifications include district-by-calendar-year, firstborn-girl-by-calendar-year, firstborn-girl-by-district, and mother year-of-birth fixed effects and the common baseline controls for household wealth, religion, rural status, parental education, cluster elevation, and distance to the national border. They also control for the mother's stated son preference, measured by the ideal fraction of sons and the total ideal number of children. Baseline means are for firstborn-boy families during the pre-service period. Standard errors in parentheses are clustered by district. Statistical significance at the 1, 5, and 10 percent levels is indicated by ***, **, and *.

Table A8: Placebo: Older High-Parity Mother-Years

	(1) Any birth	(2) Male birth	(3) Female birth
Firstborn girl	0.570	0.288	0.283
	(0.354)	(0.231)	(0.224)
Firstborn girl × Early rollout	0.021	0.004	0.016
	(0.439)	(0.319)	(0.274)
Firstborn girl × Expanded access	-0.157	0.037	-0.194
	(0.371)	(0.233)	(0.267)
Baseline mean	7.92	4.00	3.91
Observations	77,984	77,984	77,984

Notes: The table uses the post-first-birth mother-year panel and restricts to high-parity older risk years, defined as mother age at least 35 and next-birth order at least four. Outcomes are indicators for any birth, a male birth, or a female birth in a calendar year, multiplied by 100. Male- and female-birth outcomes equal zero in years with no birth. The sample uses GPS-linked clusters from the 2001, 2006, 2011, and 2016 NDHS waves and requires nonmissing preferred access and geography controls. The omitted period is the 1990–2003 pre-service baseline; early rollout denotes 2004–2007 and expanded access denotes 2008–2017. All columns include district-by-calendar-year and mother year-of-birth fixed effects; the common baseline controls for household wealth, religion, rural status, parental education, cluster elevation, and distance to the national border; and the mother's ideal fraction of sons and total ideal number of children. Baseline means are column-specific means among firstborn-boy families in the omitted period and in the corresponding regression sample. Standard errors in parentheses are clustered by district. Statistical significance at the 1, 5, and 10 percent levels is indicated by ***, **, and *.

Figure A6: Ultrasound Diffusion and Abortion Access



Notes: The figure plots changes in the firstborn-girl minus firstborn-boy gap relative to the 1990–1992 bin. Panel A reports the annual probability of a female birth in the post-first-birth mother-year panel; Panel B reports the female share among second- and higher-order births. Coefficients are in percentage points. Both specifications include district, outcome-year, and mother year-of-birth fixed effects, the common baseline controls, and stated-fertility-preference controls. The omnibus *p*-values for the district-clustered joint tests that the 1993–1995, 1996–1998, and 1999–2001 coefficients are zero are 0.347 for Panel A and 0.717 for Panel B. Capped bars are 95 percent confidence intervals based on standard errors clustered by district. Vertical dashed lines mark legalization in 2002 and expanded access in 2008. Data: NDHS.

Table A9: Connectivity Heterogeneity: Alternative Continuous Access Measures

	(1)	(2)	(3)	(4)	(5)
	Female share later births	Any birth	Male birth	Female birth	p-value Female Birth = Male Birth
Panel A. Road time to nearest 50,000-person city					
Firstborn girl × Early rollout	-1.25	-0.29	0.02	-0.31	0.304
	(1.14)	(0.29)	(0.23)	(0.20)	
Firstborn girl × Early rollout × Connectivity	-0.19	-0.76***	-0.30	-0.47**	0.611
	(1.15)	(0.27)	(0.23)	(0.20)	
Firstborn girl × Expanded access	-3.07**	-0.53**	-0.02	-0.51**	0.064
	(1.32)	(0.27)	(0.16)	(0.21)	
Firstborn girl × Expanded access × Connectivity	-2.34**	-0.85***	-0.24	-0.61***	0.101
	(1.03)	(0.24)	(0.15)	(0.18)	
Panel B. Market connectivity					
Firstborn girl × Early rollout	-1.16	-0.30	0.02	-0.31	0.302
	(1.14)	(0.30)	(0.23)	(0.20)	
Firstborn girl × Early rollout × Connectivity	1.06	-0.69**	-0.42*	-0.27	0.669
	(1.11)	(0.30)	(0.24)	(0.21)	
Firstborn girl × Expanded access	-3.13**	-0.54*	-0.02	-0.52**	0.061
	(1.32)	(0.27)	(0.16)	(0.21)	
Firstborn girl × Expanded access × Connectivity	-1.97*	-0.66***	-0.17	-0.48***	0.198
	(0.99)	(0.22)	(0.14)	(0.18)	
Panel C. Road time to nearest 100,000-person city					
Firstborn girl × Early rollout	-1.27	-0.29	0.02	-0.31	0.306
	(1.14)	(0.29)	(0.23)	(0.20)	
Firstborn girl × Early rollout × Connectivity	-0.04	-0.69**	-0.31	-0.38*	0.821
	(1.14)	(0.28)	(0.23)	(0.20)	
Firstborn girl × Expanded access	-3.08**	-0.53*	-0.02	-0.51**	0.065
	(1.33)	(0.27)	(0.16)	(0.21)	
Firstborn girl × Expanded access × Connectivity	-1.88*	-0.67***	-0.20	-0.46**	0.242
	(0.97)	(0.24)	(0.15)	(0.18)	
Baseline mean	49.07	18.67	9.51	9.15	
Observations	54,669	360,216	360,216	360,216	

Notes: Connectivity is standardized to mean zero and standard deviation one, with larger values indicating better access. Because mean connectivity is zero, the Firstborn girl × period coefficients report the estimated response at mean connectivity; the triple interactions report how that response changes with a one-standard-deviation increase in connectivity. Column (1) uses second- and higher-order births; columns (2)–(4) use the post-first-birth mother-year panel; column (5) reports the p-value for equality of the male-birth and female-birth coefficients in the corresponding row. Outcomes are multiplied by 100. The sample uses GPS-linked clusters from the 2001, 2006, 2011, and 2016 NDHS waves. Column (1) includes district-by-child-birth-year fixed effects; columns (2)–(4) include district-by-calendar-year fixed effects. All columns also include mother year-of-birth fixed effects; the common baseline controls for household wealth, religion, rural status, parental education, cluster elevation, and distance to the national border; and the mother's ideal fraction of sons and total ideal number of children. The omitted period is 1990–2003; early rollout is 2004–2007; expanded access is 2008–2017. Road-network times were computed in June 2026 using the public OSRM routing service (Luxen and Vetter, 2011); all three measures serve as contemporary robustness checks. Following Donaldson and Hornbeck (2016), market connectivity is a population-weighted gravity index. The decay parameter θ governs how quickly a city's contribution falls with travel time; $\theta = 2$ gives an inverse-square penalty, and a 30-minute additive offset prevents very short travel times from dominating the index. Baseline means are column-specific means among firstborn-boy families in the omitted period. Standard errors are clustered by district. Significance levels: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A10: Callaway–Sant'Anna Robustness for Female Share Among Later Births

	(1)	(2)	(3)
	Female share among second- and higher-order births		
ATT: early rollout	-1.058	-0.543	-0.908
	(1.107)	(1.068)	(1.151)
ATT: expanded access	-2.833**	-4.602***	-5.489***
	(1.274)	(1.536)	(1.392)
Observations	54,724	54,725	54,725
Baseline mean	49.1	49.1	49.1
Specification	OLS preferred	CS baseline controls	CS DRIMP + preference controls

Notes: The dependent variable is an indicator for a female second- or higher-order live birth, multiplied by 100. The sample is the four-wave GPS-linked baseline sample: 2001, 2006, 2011, and 2016 NDHS waves. Column (1) reproduces the preferred OLS female-share specification. Columns (2)–(3) report repeated-cross-section group-time average treatment effects following Callaway and Sant'Anna (2021). In the CS recoding, firstborn-girl families first enter treatment in the 2004–2007 legal-service rollout period, and firstborn-boy families are never-treated controls. Event time zero is 2004–2007; event time one is expanded access, 2008–2017. "CS baseline controls" uses the outcome-regression estimator with district indicators and the common baseline controls. "CS DRIMP + preference controls" uses the doubly robust improved estimator of Sant'Anna and Zhao (2020), with the same baseline controls, and additionally includes the ideal fraction of sons and total ideal children. Baseline mean is the column-specific mean dependent variable for second- and higher-order births in firstborn-boy families during 1990–2003. Standard errors in parentheses are clustered by district. Significance levels: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A11: National Effects with the 2022 NDHS Wave

	Any birth	Male birth	Female birth	Female share
Firstborn girl × Early rollout	-0.16	-0.03	-0.13	-0.37
	(0.29)	(0.22)	(0.20)	(1.04)
Firstborn girl × Expanded access	-0.31	0.02	-0.34**	-1.99**
	(0.24)	(0.16)	(0.17)	(0.85)
Observations	449,723	449,723	449,723	67,741
Baseline mean	18.95	9.65	9.30	49.10
p-value: Female birth = Male birth, early rollout		0.751		
p-value: Female birth = Male birth, expanded access		0.098		

Notes: The table adds the 2022 NDHS retrospective histories to the four-wave baseline. Rows report national firstborn-girl difference-in-differences for the early-rollout and expanded-access periods. The hazard columns use the post-first-birth mother-year panel and include district, calendar-year, and mother year-of-birth fixed effects. The female-share column uses second- and higher-order births and additionally includes birth-year-by-region fixed effects. All columns control for household wealth, religion, rural status, parental education, cluster elevation, distance to the national border, the mother's ideal fraction of sons, and her total ideal number of children. Outcomes are multiplied by 100. Baseline means are for firstborn-boy families in the pre-service period. The final two rows report p-values for equality of the male-birth and female-birth coefficients. Standard errors in parentheses are clustered by district. Statistical significance at the 1, 5, and 10 percent levels is indicated by ***, **, and *.

Table A12: Connectivity Heterogeneity: Alternative Continuous Access Measures (with 2022 Wave)

	(1) Female share later births	(2) Any birth	(3) Male birth	(4) Female birth	(5) p-value Female Birth = Male Birth
Panel A. Road time to nearest 50,000-person city					
Firstborn girl × Early rollout	-0.44	-0.18	-0.03	-0.15	0.698
	(1.05)	(0.28)	(0.22)	(0.20)	
Firstborn girl × Early rollout × Connectivity	-0.33	-0.78***	-0.32	-0.46**	0.661
	(1.01)	(0.26)	(0.21)	(0.19)	
Firstborn girl × Expanded access	-2.17**	-0.34	0.02	-0.36**	0.082
	(0.82)	(0.22)	(0.15)	(0.16)	
Firstborn girl × Expanded access × Connectivity	-1.81**	-0.84***	-0.24*	-0.59***	0.064
	(0.75)	(0.20)	(0.13)	(0.14)	
Panel B. Market connectivity					
Firstborn girl × Early rollout	-0.41	-0.18	-0.03	-0.15	0.690
	(1.04)	(0.28)	(0.22)	(0.20)	
Firstborn girl × Early rollout × Connectivity	0.41	-0.69**	-0.37*	-0.32	0.882
	(0.99)	(0.27)	(0.21)	(0.22)	
Firstborn girl × Expanded access	-2.19***	-0.35	0.01	-0.36**	0.079
	(0.82)	(0.23)	(0.16)	(0.16)	
Firstborn girl × Expanded access × Connectivity	-1.42*	-0.69***	-0.20	-0.49***	0.165
	(0.74)	(0.20)	(0.14)	(0.15)	
Panel C. Road time to nearest 100,000-person city					
Firstborn girl × Early rollout	-0.47	-0.18	-0.03	-0.15	0.697
	(1.05)	(0.28)	(0.22)	(0.20)	
Firstborn girl × Early rollout × Connectivity	-0.20	-0.69**	-0.32	-0.37*	0.845
	(0.99)	(0.28)	(0.21)	(0.20)	
Firstborn girl × Expanded access	-2.18**	-0.34	0.02	-0.36**	0.083
	(0.83)	(0.23)	(0.15)	(0.16)	
Firstborn girl × Expanded access × Connectivity	-1.28*	-0.71***	-0.25*	-0.46***	0.243
	(0.71)	(0.21)	(0.13)	(0.14)	
Baseline mean	49.09	18.95	9.65	9.30	
Observations	67,718	449,723	449,723	449,723	

Notes: This table repeats Appendix Table A9 after adding the 2022 NDHS retrospective histories. Standard errors are clustered by district. Significance levels: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

B Back-of-the-Envelope Calculation of Missing Girls

We adapt the accounting framework from Anukriti et al. (2022) to map the regression estimates to annual flow equivalents for girls missing at birth.

Let N denote total live births in a year and $N_{FG,\text{post}}$ the annual number of parity-≥ 2 births preceded by a firstborn girl (FBG) during expanded access. Let ΔF_{FG} be the preferred expanded-access change in the female share of those births, and let $\Delta M_{FG} = -\Delta F_{FG}$ be the corresponding change in the male share. 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); this is the same benchmark family as the 48.8 percent female share used in the figures. The additional sex-selection flow equivalent is

$$\Delta MG_{\text{sex}} = N_{FG,\text{post}}\left(\frac{0.49}{0.51}\Delta M_{FG} - \Delta F_{FG}\right) = N_{FG,\text{post}}\left(\frac{-\Delta F_{FG}}{0.51}\right). \tag{B1}$$

ΔF_{FG} is the preferred expanded-access coefficient from Table A3 (−2.83 percentage points). The share $S_{\text{post}} \equiv N_{FG,\text{post}}/N$ is the DHS-weighted share of all expanded-access births that are parity ≥ 2 in FBG families; N is the 2008–2017 average of annual live births from UN World Population Prospects 2024. This calculation isolates the sex-composition response. Appendix Table A13 provides the details.

Table A13: Sex-Selection Flow Equivalent

	Value
A. Inputs	
ΔF_{FG} (expanded-access female-share estimate)[a]	−0.0283
N (average births/year, 2008–2017)[b]	621,543
$S_{\text{post}} \equiv N_{FG,\text{post}}/N$[c]	0.3445
$N_{FG,\text{post}} = S_{\text{post}} \times N$	214,103
B. Sex-Selection Flow Equivalent	
Missing girls/year: $N_{FG,\text{post}}(-\Delta F_{FG}/0.51)$	11,892
As percent of all annual births	1.91%
As percent of expected female births	3.90%
As percent of potential total births	1.88%

[a] ΔF_{FG} is the coefficient on *Firstborn girl* × *Expanded access* from the preferred female-share specification in Table A3, equal to −2.83 percentage points.
[b] Annual births are from United Nations World Population Prospects 2024 (United Nations, 2024). We use the average number of live births in 2008–2017, the expanded-access window in the active analysis sample. Expected female births are computed as $0.49 \times N$. Potential total births equal observed births plus the sex-selection flow equivalent.
[c] S_{post} is the DHS-weighted share of all live births in 2008–2017 that are second- or higher-order births following a firstborn girl.

C Derivation of the Quantity–Composition Identity

Let B denote the event that any later birth occurs and F the event that a realized birth is female. For each counterfactual state $t \in \{0,1\}$, the multiplication rule implies

$$P_t(B \cap F) = P_t(B)P_t(F \mid B). \tag{C1}$$

The change in the unconditional probability of a female birth is therefore

$$\begin{aligned}
\Delta P(B \cap F) &= P_1(B)P_1(F \mid B) - P_0(B)P_0(F \mid B) \\
&= \left[P_1(B)P_1(F \mid B) - P_1(B)P_0(F \mid B)\right] \\
&\quad + \left[P_1(B)P_0(F \mid B) - P_0(B)P_0(F \mid B)\right] \\
&= P_1(B)\left[P_1(F \mid B) - P_0(F \mid B)\right] + \left[P_1(B) - P_0(B)\right]P_0(F \mid B) \\
&= \underbrace{\Delta P(B)P_0(F \mid B)}_{\text{quantity margin}} + \underbrace{P_1(B)\left[P_1(F \mid B) - P_0(F \mid B)\right]}_{\text{sex-composition margin}}.
\end{aligned} \tag{C2}$$