From Two Daughters to a Hurried Third: Son Preference, Birth Spacing, and Mortality

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Abstract

Son preference raises child mortality through sex selection, discriminatory investment and early weaning. We document a fourth: birth spacing. Bangladeshi mothers of two daughters conceive a third child sooner, compressing the interval below the WHO-recommended 24 months. Instrumenting a sub-24-month third conception with the sex composition of the first two births, we estimate a 10.2 pp higher risk of third-child neonatal death in Matlab and Chakaria surveillance data and 11.4 pp in nine Bangladesh DHS rounds. The child harmed is the one the parents wanted, male with even probability. Nationally, this implies about 35,000 excess neonatal deaths over four decades.

Keywords: son preference, birth spacing, infant mortality, Bangladesh, HDSS.
JEL codes: J13, I15, O15, J16.

1 Introduction

Son preference raises child mortality through multiple channels that the literature has established well: sex-selective abortion, which intensifies as desired family size falls (Jayachandran, 2017), and discriminatory allocation of food, health care, and parental time among children already born (Jayachandran and Pande, 2017; Anukriti et al., 2022). A third channel involves no such choice: Indian mothers wean daughters earlier than sons because nursing suppresses fecundity and parents who have not yet had a son want to conceive again; excess female mortality follows from a fertility preference rather than a discriminatory allocation (Jayachandran and Kuziemko, 2011). This paper

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documents a fourth channel of the mortality effects of son preference, operating through birth spacing, and shows that the child who bears the cost is the one the parents are trying to have. We provide causal evidence on this new margin using the sex composition of the first two births, which is as good as randomly assigned where prenatal sex selection is rare and which generates the variation in the timing of the third conception.

Parents who want a son and have had two daughters do not simply have a third child; they have that child sooner. Short interpregnancy intervals are in turn a well-documented mortality risk: the World Health Organization recommends at least 24 months between a live birth and the next pregnancy (World Health Organization, 2007), and in Matlab itself intervals below 15 months carry roughly twice the first-week mortality risk of intervals of three to five years (DaVanzo et al., 2008). Since the third child is male with roughly even probability, the cost falls on sons and daughters alike.

Bangladesh offers empirical leverage on the question. Abortion is legally restricted and sex-selective abortion is rare (Bongaarts, 2013; Talukder et al., 2015; Chao et al., 2019), so the sex composition of the first two births is plausibly as good as randomly assigned; son preference is strong enough to move fertility at parity two; and the data are large enough that a spacing effect on mortality is detectable. Since Angrist and Evans (1998), sibling sex composition has been used as exogenous variation in family *size*; we use it as variation in the *timing* of the next conception, instrumenting a third conception within 24 months of the second birth with girl-girl, since under son preference it is two daughters that create the pressure to conceive again quickly.

We estimate the same specification on two independent data sources: the Matlab and Chakaria Health and Demographic Surveillance Systems (HDSS) operated by ICDDR,B, which register vital events prospectively and yield 34,426 mothers with ≥ 2 live births over 2005–2024, and nine rounds of the Bangladesh Demographic and Health Survey (BDHS), 1993–94 to 2022 (NIPORT, 2024), yielding 39,651 mothers within the DHS 10-year recall window.

Fertility timing. Girl-girl mothers are 11.9 pp more likely to have a third birth in the HDSS and 10.0 pp in the BDHS, and 3.1 pp and 4.4 pp more likely to conceive a third within 24 months. The boy-boy coefficients on a third birth are 0.015 in the HDSS and 0.016 in the BDHS, a small fraction of the girl-girl estimates, and on the short-spacing margin they are indistinguishable from zero. That asymmetry rules out a symmetric gender-mix preference, and a falsification at the first-to-second birth margin excludes a son preference that operates after a single daughter. The pattern fits son preference under a desired-family-size cap, though an asymmetric gender-mix preference with strong son weights would also produce it.

Mortality. Instrumenting the sub-24-month third-conception indicator with girl-girl (first-stage $F = 67.7$ in the HDSS, $F = 60.3$ in the BDHS), mothers shifted into a short interval face a 10.2 pp higher risk of third-child neonatal death in the HDSS (95% CI 2.4–17.9) and 11.4 pp in the BDHS (95% CI 2.6–20.3); the infant-death LATEs are 13.6 pp and 17.3 pp, and all four are significant. Mortality outcomes are defined over the full two-child sample, so selection into third birth does not contaminate them.

Robustness checks. Two threats matter. The first is prenatal sex selection, which would make sex composition chosen rather than drawn. Son preference would raise the male share at the third birth among girl-girl mothers relative to boy-boy mothers, who have no motive to select; the difference is 0.010 in both sources ($p = 0.44$ and $p = 0.40$). The second is that two daughters might harm the third child through lower investment rather than a hurried conception. The two mechanisms make opposite predictions about the third child's sex, and the estimates match spacing: the male excess in neonatal mortality is no smaller in girl-girl families, the girl-girl effect disappears among widely spaced third births, and the harm is confined to the first 28 days. The estimates are stable across eleven sample and specification cuts, and inference survives a wild cluster bootstrap.

Mortality costs. We use the BDHS estimates for the national calculation, since that sample is nationally representative. Under the assumptions in Supplemental Appendix Table S7, the BDHS coefficient implies about 890 excess third-child neonatal deaths per year and 1,620 excess infant deaths, or roughly 35,000 and 65,000 across the 1983–2022 span of the sample. At the Robinson et al. (2019) preferred value-of-statistical-life transfer for low- and middle-income countries, the national cost is \$0.6–\$1.1 billion per year and \$24–\$44 billion in aggregate. The estimates also identify where an existing policy lever applies: family-planning counseling in Bangladesh already promotes spacing and is delivered at the household level, and the households for which that message has the highest return are mothers of two daughters, in the months after the second birth.

Related literature and contribution. The paper speaks to two literatures. The first is the health cost of son preference, which has established multiple channels including sex-selective abortion and discriminatory allocation among children already born (Jayachandran, 2017; Bhalotra and Cochrane, 2010; Jayachandran and Pande, 2017; Anukriti et al., 2022; Jayachandran and Kuziemko, 2011). Birth spacing appears in it only as an outcome of son preference, never as a channel to the next child's survival. We add a margin running through the timing of the next conception rather than through the care of children already born, and one whose cost falls not on a daughter but on the third child, whichever its sex.

The second is the mortality cost of short interpregnancy intervals. The association is large and

well documented (Rutstein, 2005; DaVanzo et al., 2008; Conde-Agudelo et al., 2012), but short intervals cluster in families whose children face higher mortality risk for other reasons. Within-mother designs difference it out (Molitoris et al., 2019) but not confounders that vary across a mother's births, and dynamic panel models recover causal effects only parametrically (Bhalotra and van Soest, 2008; Saha and van Soest, 2013); to the best of our knowledge, no work uses exogenous variation in birth spacing itself. Plausibly exogenous sex composition provides the variation, and we use it to estimate the mortality effect itself.

2 Two Complementary Data Sources

We use two independent data sources and apply the same identification strategy to each. Supplemental Appendix Section S1 gives the full description of both.

The first is the **Matlab and Chakaria Health and Demographic Surveillance Systems (HDSS)**, run by ICDDR,B in two rural sub-districts (Alam et al., 2017; Hanifi et al., 2012). Field workers register births, deaths and migrations by date on a fixed visiting cycle, so vital events are recorded prospectively rather than recalled, and both sites record a conception date for each pregnancy, so third conceptions are dated directly. We use births from 2005 to 2022 in Matlab and 2010 to 2024 in Chakaria. Since 1977 half the Matlab area has received maternal and child health and family planning services from ICDDR,B and half from government facilities; Section 5 re-estimates within each.

The second is the **Bangladesh Demographic and Health Survey (BDHS)**, a nationally representative repeated cross-section that elicits a complete birth history from every ever-married woman aged 15–49 (NIPORT, 2024). We pool all nine rounds, 1993–94 to 2022. It is recall-based but national, so it tests whether the HDSS results hold across the country.

2.1 Sample construction

In both sources we keep mothers with at least two observed live births of known sex and date, and treat the second birth as the analytical clock-start. We drop mothers whose third live birth is recorded within seven months of the second. Such a birth cannot be a subsequent pregnancy carried to term, and in the data it is the co-twin of the second child (Supplemental Appendix Section S12). A twin has no conception interval of its own for sex composition to move, and twins die at several times the singleton rate. The exclusion is a definition rather than a result: keeping these mothers, with the twin coded as a short-spaced third conception, moves the 2SLS estimate on third-child neonatal death from 0.102 to 0.084 in the HDSS and from 0.114 to 0.136 in the BDHS, both still significant at the 5% level (Supplemental Appendix Table S6, row R12). This yields **34,426 mothers**

and 81,789 live births in the HDSS. In the BDHS we further keep mothers whose second live birth falls within 120 months of interview, the ten-year window the DHS Program uses for subgroup mortality estimates (Croft et al., 2018), giving **39,651 mothers**, 31% of them urban. Because each round contributes its own preceding decade, BDHS second births span 1983–2022 and are centered on 2005.3, earlier than the HDSS window. Section 5 examines sensitivity to the recall window.

Mothers with at least two live births are the population of interest rather than a sub-sample, since the two-daughter margin is defined only at parity two. The restriction could threaten identification only through the first birth, since progression to parity two depends on the first child's sex; restricting to mothers whose first child is a girl holds that margin fixed, and the girl-girl coefficients reproduce the full-sample estimates (Supplemental Appendix Section S2). A second feature of the surveillance data, in both Matlab and Chakaria, works against us rather than for us. Surveillance records births only within its window, so a mother whose earlier children were born before the site opened enters our sample as though her first observed birth were her first. Where those unobserved children include a son, two observed daughters create no pressure to conceive again, so such a mother is a non-complier coded as treated, and her presence pulls the estimates toward zero. Chakaria records true birth order, and restricting to mothers at true parity one strengthens the spacing and mortality coefficients (Supplemental Appendix Section S10).

2.2 Variable construction

The instrument is *girl-girl* ($= 1$ if the first two live births are both female). The endogenous regressor is short birth spacing, an indicator for a third conception within 24 months of the second birth. We also construct *boy-boy*, which enters the decomposition in Table 2 alongside girl-girl with mixed-sex families as the omitted category. In the BDHS, which records only birth dates, the third conception is dated as the third birth minus nine months; Supplemental Appendix Section S12 gives the rule.

For the third child we define neonatal death (≤ 28 days) and infant death (≤ 365 days) on the full two-child sample: the outcome equals 1 if the mother has a third live birth that dies within the window and 0 otherwise, including when there is no third birth. It is set missing only when observation is too short to determine it, which is why the infant outcome has slightly fewer mothers than the neonatal one; Supplemental Appendix Table S6 shows the estimates are unchanged when every mother is required to be observed for 36 or 60 months. A mother is observed until the end of her site's extract or, in Matlab, until her own permanent departure if that comes first; out-migration is unrelated to the instrument (Supplemental Appendix Section S1). Defining mortality this way rather than conditioning on a third birth keeps selection into third birth out of the estimates.

Controls are mother's age at first birth, her age at second birth, her years of education, multiple births among the first two and the first child's survival to the second birth in the HDSS; and mother's

age at second birth, her schooling, household electricity and an improved water source in the BDHS, the four recorded in all nine rounds. Only mother's age at first birth and her education are fixed before the first birth; Supplemental Appendix Table S6 reports estimates using that pair alone.

2.3 Summary statistics

Table 1 compares girl-girl mothers with all other mothers in both sources, the contrast the estimates rest on. Mother's age at her first birth and her years of schooling are predetermined, and in the BDHS so is her religion, which neither HDSS site records. None differs materially: the joint F on the block is 0.19 ($p = 0.83$) in the HDSS and 1.75 ($p = 0.16$) in the BDHS, and the Muslim share, 0.9 pp lower among girl-girl mothers ($p = 0.02$), is the one cell significant on its own.

Panel B is realized after the first birth and can respond to that child's sex, so differences there are consequences of the sex draw rather than evidence against it. The first-to-second interval is 1.0 months shorter for girl-girl mothers in the HDSS and 1.8 months shorter in the BDHS, which could be son preference operating one parity earlier: every girl-girl mother has a first daughter, and a first daughter shortens the interval to the second birth, while the second child's sex, realized after that interval is complete, does not move it. First-child survival is 0.9 pp higher in girl-girl families, which is the male-female survival gap. Supplemental Appendix Section S2 documents both. Because these are outcomes of the first child's sex, conditioning on them is a choice rather than a requirement, and the estimates do not depend on it (Supplemental Appendix Table S6, row R10). Multiple births are left out of the balance table altogether, since twins share sex far more often than two singletons and so raise the chance of a girl-girl pair mechanically.

Panel C, the fertility response the paper estimates, diverges sharply in both sources: girl-girl mothers are 12.1 pp (HDSS) and 9.8 pp (BDHS) more likely to have a third live birth and 3.0 pp and 4.5 pp more likely to conceive a third within 24 months, all at $p < 0.001$, with joint F statistics of 134 and 125 against 0.19 and 1.75 on what was fixed before the first birth.

3 Empirical Strategy and Identification

3.1 Empirical Strategy

We estimate two specifications: a decomposition of each outcome by the sex composition of the first two births, which is our test for son preference, and a two-stage least squares estimate of the mortality effect of short birth spacing, with girl-girl as the instrument.

The first specification regresses each mother-level outcome on indicators for boy-boy and girl-girl:

Table 1: Balance: girl-girl versus all other first-two sex compositions

	HDSS				BDHS, 10-yr recall			
	Other	GG	Diff.	p	Other	GG	Diff.	p
Panel A: fixed before the first birth								
Mother's age at first birth (years)	22.99	23.01	+0.02	0.778	18.56	18.58	+0.02	0.688
Mother's years of secular education	7.02	7.04	+0.02	0.696	5.30	5.25	−0.04	0.364
Muslim (share)	–	–	–	–	0.895	0.886	−0.009	0.018
Joint F			0.19	0.826			1.75	0.155
Panel B: realized between the first and second birth								
Mother's age at second birth (years)	27.32	27.25	−0.07	0.268	22.81	22.66	−0.15	0.002
Months between births 1 and 2	51.92	50.87	−1.04	0.008	51.35	49.56	−1.78	<0.001
First child survived to 2nd birth (share)	0.944	0.953	+0.009	0.001	0.904	0.913	+0.009	0.006
Joint F			6.74	<0.001			13.32	<0.001
Panel C: fertility outcomes								
Total live births per mother	2.33	2.50	+0.17	<0.001	2.46	2.59	+0.13	<0.001
Third birth ever (share)	0.280	0.401	+0.121	<0.001	0.333	0.430	+0.098	<0.001
3rd conception within 24 months (share)	0.078	0.109	+0.030	<0.001	0.207	0.252	+0.045	<0.001
Joint F			134.39	<0.001			124.51	<0.001
N mothers	25,757	8,669			30,174	9,477		

Notes: Girl-girl (GG) mothers are compared with all other mothers, boy-boy and mixed-sex together. Columns 1–4 are the HDSS analytical sample (Matlab and Chakaria mothers with ≥ 2 live births); columns 5–8 are the BDHS 10-year recall sample pooled over nine rounds, 1993–94 to 2022. Religion is not recorded in the HDSS, so the HDSS joint F in Panel A covers the two variables it observes. Diff is the raw difference in means, girl-girl minus other, with its p-value from a regression on the girl-girl indicator alone. *Joint F* regresses the girl-girl indicator on every variable in the panel, within site and year-of-second-birth cells in the HDSS and district and round cells in the BDHS, and tests that all coefficients are zero. Standard errors are clustered by village (HDSS) and primary sampling unit (BDHS). Panels are ordered by when each variable is settled; only Panel A is fixed before any child's sex is known (Section 2). Mothers with a twin birth remain in the sample but twinning is not shown as a balance row, since twins share sex far more often than singletons and so raise the chance of a girl-girl pair mechanically; Supplemental Appendix Table S6 re-estimates with those mothers dropped.

$$Y_i = \alpha + \beta_{\text{BB}}\,\text{BoyBoy}_i + \beta_{\text{GG}}\,\text{GirlGirl}_i + X_i'\gamma + \delta_{v(i)} + \lambda_{t(i)} + \varepsilon_i. \tag{1}$$

Here, i indexes the mother, v the village and t the year of the second birth, and mixed-sex families are the omitted category. $\delta_{v(i)}$ represents village fixed effects and $\lambda_{t(i)}$ year-of-second-birth fixed effects, and X_i collects the mother and household controls listed in Section 2. In the BDHS we use district fixed effects, since the sampling cluster is round-specific and never resampled, so cluster fixed effects would absorb most of the variation in the instrument (Supplemental Appendix Section S8 reports them).

Under a symmetric preference over sibling sex mix the two coefficients should be similar; under son preference β_{GG} should dominate, and it does (Section 4). That asymmetry is why girl-girl, not same-sex, is the instrument.

Standard errors are clustered at the village level in the HDSS (191 villages) and at the primary sampling unit in the BDHS. We re-test the main results with the wild cluster bootstrap in Section 5.

The second specification instruments short birth spacing with girl-girl:

$$\text{Short}_i = \pi_0 + \pi_1\,\text{GirlGirl}_i + X_i'\pi_2 + \delta_{v(i)} + \lambda_{t(i)} + u_i, \tag{2}$$

$$H_i = \alpha + \beta\,\widehat{\text{Short}}_i + X_i'\gamma + \delta_{v(i)} + \lambda_{t(i)} + \varepsilon_i. \tag{3}$$

Here, Short_i equals one if the third conception occurs within 24 months of the second birth, and H_i is an indicator for third-child neonatal or infant death. The fixed-effect structure is unchanged from equation (1). Equation (2) folds boy-boy into the comparison group alongside mixed-sex families, which changes the coefficient negligibly, since the two do not differ significantly in spacing. The coefficient β is a local average treatment effect for mothers whose third conception is moved earlier by the sex composition of the first two.

3.2 Identification Strategy

Identification requires three conditions, the logic of Angrist and Evans (1998) applied to the timing of the next conception rather than to completed family size. The first is relevance: girl-girl status must move the timing of the third conception, and it does, with a first-stage F of 67.7 in the HDSS and 60.3 in the BDHS.

The second is that sex composition is as good as randomly assigned. Sex at birth is not chosen, so absent prenatal sex selection a girl-girl pair occurs by chance, independent of the sex of earlier births and of anything else about the family. Sex selection is therefore the threat, and Section 5 tests

for it. The sample also conditions on reaching a second birth, itself selected on the sex of the first, but that is precisely the population of interest.

The third is exclusion: girl-girl status must affect third-child mortality only through the timing of the third conception. Two threats stand out. The first is that two daughters change how parents treat the third child directly, whenever it was conceived, for instance by investing less in a third daughter. This is the analogue of the standard objection to sibling sex composition as an instrument for family size: Conley (2000) finds that the sex mix of siblings has direct effects on children's outcomes, and Black et al. (2005) note that such effects invalidate the instrument. The second is that son preference might operate after any daughter rather than only after two, so that the girl-girl coefficient would combine the effect of two daughters with whatever a single daughter does to the household. Section 4 addresses the second by showing that a first daughter alone moves neither the timing of the second conception nor the second child's survival; Section 5 addresses the first.

The village and year-of-second-birth fixed effects (district and round in the BDHS) absorb time-invariant differences in mortality risk across places and national trends in child survival. Because the instrument is plausibly randomly assigned, the mother and household controls affect precision rather than consistency.

4 Results

4.1 Son preference and the first stage

Table 2 reports the decomposition in both data sources, side by side. The girl-girl coefficient on third birth is $+0.119$ in the HDSS and $+0.100$ in the BDHS. On a third conception within 24 months the girl-girl coefficients are $+0.031$ and $+0.044$, both significant at $p < 0.001$, while the boy-boy coefficients are -0.000 and $+0.004$, neither distinguishable from zero. The pattern holds in each HDSS site (Supplemental Appendix Figure S1). The short-spacing column is also the first stage for the 2SLS estimates that follow.

Two alternative preferences could produce a fertility response to sibling sex composition without any preference for sons, and the decomposition rules out both. The first is a preference for a mixed sibship. Such parents would accelerate the third conception after two boys as after two girls, and they do not: the boy-boy coefficient on a third conception within 24 months is indistinguishable from zero in both sources. The second is a diffuse preference for sons operating at every parity, under which parents would hurry after any daughter. That predicts a response at the first-to-second birth margin, and there is none: having a first daughter moves neither the timing of the second conception (0.0067, $p = 0.13$, first-stage $F = 2.3$) nor second-child neonatal mortality (0.0005, $p = 0.74$; Supplemental Appendix Table S11).

Table 2: Test for son preference: fertility timing by sibling sex composition

	HDSS		BDHS, 10-yr recall	
	3rd birth	3rd conc. $\leq$ 24m	3rd birth	3rd conc. $\leq$ 24m
Omitted category: mixed-sex				
BoyBoy	0.0146**	−0.0003	0.0155***	0.0037
	(0.0059)	(0.0046)	(0.0056)	(0.0055)
GirlGirl	0.1191***	0.0313***	0.0998***	0.0443***
	(0.0062)	(0.0042)	(0.0059)	(0.0059)
Ybar (mixed-sex)	0.274	0.077	0.325	0.204
Mother + household controls	Yes	Yes	Yes	Yes
Geographic + year FE	Yes	Yes	Yes	Yes
N mothers	32,909	26,507	37,743	28,122

Notes: OLS linear-probability models. Boy-boy and girl-girl enter together with mixed-sex families omitted, so each coefficient is the shift relative to a mixed-sex sibship. HDSS columns include village and year-of-second-birth fixed effects; BDHS columns include district and survey-round fixed effects. Both carry the mother and household controls described in Section 2. The girl-girl coefficient on third birth is of the same order in the two data sources (HDSS 0.119, BDHS 0.100), and the BB-vs-GG asymmetry replicates in both. Standard errors in parentheses are clustered at the village level in the HDSS columns and at the primary sampling unit in the BDHS columns. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

The acceleration therefore appears only once two daughters have arrived, which is what a desired family size of about three combined with a preference for at least one son implies: with two daughters and one birth left, the next conception is the last chance for a son. It is also consistent with the parity-1 null in Angrist and Evans (1998) and with differential stopping being a response to the sex composition of children already born (Clark, 2000).

4.2 Short birth spacing and third-child mortality

Table 3 presents the main IV results side by side. Panel B reports the first stage, in which girl-girl raises the probability of a third conception within 24 months of the second birth by 3.1 pp in the HDSS and 4.3 pp in the BDHS. It is strong in both, with F statistics of 67.7 and 60.3. Panel A reports the effect of that spacing on third-child mortality.

In the HDSS, the LATE on third-child neonatal death is $+0.102$ (s.e. 0.039, $p = 0.010$); in the BDHS it is $+0.114$ (s.e. 0.045, $p = 0.011$). The corresponding LATEs on third-child infant death are $+0.136$ in the HDSS and $+0.173$ in the BDHS, again significant in both. Among compliers, son-preference-induced short birth spacing therefore raises the probability that the third child dies in the neonatal window by 10.2–11.4 pp, and in the infant window by 13.6–17.3 pp. The two sources differ in sampling frame, measurement and period, and agree on sign and significance, with overlapping confidence intervals.

Table 3: Effect of short birth spacing on third-child mortality: 2SLS

	HDSS		BDHS, 10-yr recall	
	Neonatal death	Infant death	Neonatal death	Infant death
Panel A: third-child mortality (dependent variable in the column heading)				
Girl-girl (reduced form)	0.0031***	0.0043***	0.0049***	0.0082***
	(0.0012)	(0.0015)	(0.0019)	(0.0026)
Short birth spacing (2SLS)	**0.102*****	**0.136*****	**0.114****	**0.173*****
	(0.039)	**(0.050)**	**(0.045)**	**(0.056)**
Outcome mean	0.0071	0.0103	0.0164	0.0279
Panel B: first stage (dependent variable: third conception within 24 months of the second birth)				
Girl-girl	0.0310***	0.0317***	0.0431***	0.0472***
	(0.0038)	(0.0040)	(0.0055)	(0.0058)
First-stage F	67.7	62.9	60.3	67.3
Geographic + year FE	Yes	Yes	Yes	Yes
Mother + household controls	Yes	Yes	Yes	Yes
N mothers	25,928	24,856	28,024	26,178

Notes: Two-stage least squares. The endogenous variable is short birth spacing, an indicator for a third conception within 24 months of the second birth, instrumented with girl-girl. Panel A reports the girl-girl reduced form and the 2SLS coefficient on short birth spacing, both for the mortality outcome named in the column heading. Panel B reports the first stage, whose dependent variable is short birth spacing itself and is therefore the same in all four columns; the coefficients differ slightly across columns only because each is estimated on that column's own 2SLS sample. Mortality outcomes are defined on the full two-child sample, with mothers who have no third birth coded 0; the infant columns have fewer mothers only because a third child followed for less than 365 days has an undetermined outcome and is set missing (Supplemental Appendix Table S6, rows R3 and R4, require 36 or 60 months of observation for every mother and leave the coefficients unchanged). The outcome mean is taken over each column's own 2SLS sample. HDSS columns include village and year-of-second-birth fixed effects; BDHS columns include district and survey-round fixed effects. Both carry the mother and household controls described in Section 2. Standard errors in parentheses are clustered at the village level in the HDSS columns and at the primary sampling unit in the BDHS columns. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

These LATEs are large. Conditional on a third birth, third-child neonatal mortality is 1.79% in the HDSS and 3.39% in the BDHS, so the point estimates imply complier mortality of 12.0% and 14.8%, a 6.7-fold and a 4.4-fold increase on each source's own baseline. The descriptive literature reports smaller relative risks, about 2.1 for first-week mortality after an interval under 15 months in Matlab (DaVanzo et al., 2008) and a pooled DHS neonatal odds ratio of 2.7 below 18 months (Rutstein, 2005). Those are comparisons among mothers who chose different intervals, with the confounding DaVanzo et al. (2008) identify in their own design; ours is a local average for a narrower and more exposed group.

Where in the first year does the effect fall? Infant death contains neonatal death, so the infant estimate is not independent evidence. Splitting the first year at day 28, the girl-girl coefficient on post-neonatal death (29–365 days) is $+0.0009$ ($p = 0.35$) in the HDSS and $+0.0021$ ($p = 0.12$) in the BDHS, neither distinguishable from zero, and the neonatal window accounts for 79% and 71% of the infant coefficients (Supplemental Appendix Section S9). The effect is concentrated in the first 28 days.

5 Robustness

Is the sex composition of the first two births selected? Prenatal sex selection is potentially the most important threat to identification: if parents abort female fetuses, the sex composition of the first two births is chosen rather than drawn. Abortion is illegal in Bangladesh except to save the woman's life, and legal menstrual regulation ends at 10–12 weeks (Singh et al., 2017), before fetal sex can reliably be determined; the national sex ratio at birth has stayed at its natural level for half a century (Talukder et al., 2015), well short of what stated son preference would imply (Bongaarts, 2013). Regional data are less settled, with elevated ratios reported for the eastern divisions where both HDSS sites lie (Talukder et al., 2015), which is one reason we test the sites directly. Any selection we fail to detect biases the estimates toward zero, since it removes the most determined son-seekers, the strongest compliers, from the girl-girl group. Table 4 tests the population margin and then the margin the instrument exploits, in both sources and on identical definitions. Because sex-selective abortion raises the male share, only departures *above* the biological benchmark are evidence of it. We use the conventional 105 males per 100 females; the natural reference level that Chao et al. (2019) estimate for southern Asia from vital registration, census and survey data is 1.052 (95% uncertainty interval 1.040–1.063), which contains it; Bangladesh is not among the twelve countries in which they find strong evidence of imbalance.

Panel A tests whether the sex ratio at birth departs from the benchmark at any parity. In the HDSS the male share is statistically indistinguishable from the benchmark at parities two, three

and four; in the BDHS it is indistinguishable at parity two, above the benchmark at parity three (0.5204, $p = 0.05$) and below it at parity four (0.4971, $p = 0.05$). At parity one the male share is significantly below the benchmark in both sources (0.4914 in the HDSS; 0.5045 in the BDHS). A deficit of males is the opposite of what selection produces, and reflects entry into a sample defined by having at least two live births: mothers whose first child is a daughter are more likely to progress to a second birth (Supplemental Appendix Table S1).

Panel B tests the margin the instrument exploits, the third birth to a mother with two daughters, by comparing girl-girl with boy-boy mothers: both reach the same parity, but boy-boy mothers have no reason to select and girl-girl mothers the strongest, so son-preference selection would raise the girl-girl male share *relative to* the boy-boy share. It does not: the girl-girl minus boy-boy difference is $+0.010$ ($p = 0.44$) in the HDSS and $+0.010$ ($p = 0.40$) in the BDHS, neither distinguishable from zero.

One feature of the BDHS column deserves attention. The girl-girl male share at the third birth is 0.5326, above the benchmark ($p = 0.009$), which on its own is what sex selection would look like. But the boy-boy share is also elevated, at 0.5229, and not significantly different; boy-boy mothers have no motive to abort a female fetus, so an elevation the two groups share cannot be attributed to son preference, and mixed-sex families, at 0.5114, are on the benchmark ($p = 0.90$).

Table 4: Tests for prenatal sex selection

	HDSS			**BDHS, 10-yr recall**		
	N	Estimate	p	N	Estimate	p
Panel A: sex ratio at birth, by parity						
Parity 1	34,426	0.4914	<0.001	39,651	0.5045	0.002
Parity 2	34,426	0.5098	0.366	39,651	0.5155	0.192
Parity 3	10,690	0.5045	0.111	14,109	0.5204	0.050
Parity 4	1,937	0.5090	0.781	4,150	0.4971	0.052
Panel B: sex ratio at the third birth, by composition of the first two						
Girl-girl	3,475	0.5079	0.614	4,076	0.5326	0.009
Boy-boy	2,541	0.4978	0.148	3,555	0.5229	0.201
Mixed-sex	4,674	0.5056	0.364	6,478	0.5114	0.901
girl-girl minus boy-boy		0.0101	0.440		0.0097	0.397

Notes: Each cell is the observed share of male births, tested against the conventional biological sex ratio of 105 males per 100 females (male share 0.5122), which lies inside the southern Asia reference interval of Chao et al. (2019), 1.040–1.063. p-values are two-sided, from a one-sample test of a proportion; the girl-girl minus boy-boy row tests equality of two proportions. Sex-selective abortion raises the male share, so only shares above the benchmark are evidence of it. HDSS columns use the analytical sample of mothers with at least two live births; BDHS columns use the pooled 10-year recall sample of Tables 2 and 3, and Panel A counts births to those same mothers. Section 5 discusses the parity-1 deficit in Panel A and the girl-girl and boy-boy shares in Panel B.

Is it short spacing, or under-investment in the third child? The competing explanation is that girl-girl parents harm the third child not by conceiving it sooner but by investing less in it once it arrives, particularly if it is another daughter. The sex of the third child separates the two mechanisms, because they make opposite predictions about how third sons and third daughters fare relative to each other. Male neonates are the more fragile sex (Kraemer, 2000); among first children in our data, neonatal mortality is higher for boys than for girls by $+0.015$ (Supplemental Appendix Table S2). A short interval harms a third son and a third daughter alike, so under the spacing mechanism this male excess should be no smaller in girl-girl families than elsewhere; under differential investment the third daughter is the disfavored child, so the male excess should shrink or reverse. The estimates match spacing: among third children in girl-girl families the male excess in neonatal mortality is $+0.0115$, no smaller than the $+0.0026$ in mixed-sex families, and the interaction of girl-girl with a female third child is -0.0078 ($p = 0.14$), the opposite sign to discrimination (Supplemental Appendix Table S4).

Two further tests agree. First, if the harm runs through spacing it should disappear where spacing cannot operate: among third births more than 36 months after the second, the girl-girl coefficients fall to 0.0036 ($p = 0.25$) on neonatal death and 0.0047 ($p = 0.21$) on infant death (Supplemental Appendix Table S5). Second, the harm is confined to the first 28 days, whereas under-investment in food, care and health-seeking would show up after the neonatal window; it does not (Section 4). Together they place the harm at birth rather than in how the third child is subsequently treated.

Is the result an artifact of sample or specification? Supplemental Appendix Table S6 re-estimates the main coefficients across eleven alternative samples and specifications: dropping twins and triplets, requiring 36 or 60 months of observation past the second birth, each site separately, the two halves of Matlab, village-by-five-year-cohort fixed effects, pre-first-birth controls only, the site-end observation window that ignores mothers' own out-migration, and keeping the co-twins of the second birth that Section 2 excludes. The fertility-timing coefficient is significant at $p < 0.001$ in every row, and the mortality coefficients lose 5% significance only in the Chakaria-only and Matlab service-area cuts, which retain a quarter and two-fifths of the mothers; in the service area the point estimates are also about half the pooled ones.

One of those cuts is more than a robustness check, because it tests the mechanism. If the channel runs through fertility timing, the family-planning services ICDDR,B has provided in half of Matlab since 1977 (Section 2) should dampen the response, and they do: the girl-girl effect on a third conception within 24 months is 0.0206 in the service area against 0.0312 in the comparison area, a suggestive rather than significant difference ($p = 0.18$). The mortality coefficients, 0.0015 and 0.0046, do not differ significantly either ($p = 0.20$), so the harm is not an artifact of the program.

Does the BDHS result depend on the recall window or on geography? The BDHS estimates use the DHS ten-year recall window (Section 2). Widening that window adds births recalled from further back and, because every round contributes its own preceding years, shifts the sample toward earlier calendar periods (mean year of second birth 2005 at ten years, 1998 with no restriction); the survey cannot separate recall distance from era.

The 2SLS estimate on third-child neonatal death is $+0.091$ ($p = 0.06$) at eight years, $+0.114$ ($p = 0.01$) at ten, $+0.059$ ($p = 0.16$) at twelve, and $+0.027$ ($p = 0.23$) with no recall restriction at all. What falls is the point estimate, not the precision: the first stage strengthens throughout, from $F = 60$ at ten years to $F = 276$ unrestricted. Two features of the grid point to recall rather than era. If the earlier cohorts simply had weaker son preference, the fertility response would fall alongside the mortality response. It does the opposite, rising from 0.043 at ten years to 0.061 unrestricted. Only the mortality reduced form falls, which is what omission of deaths among distant births would produce, since an unreported death cannot be attributed to spacing. The infant-death estimate decays more slowly and is significant at the 5% level at every window from eight years outward ($+0.055$, $p = 0.045$ unrestricted). Below ten years precision falls away: at six years neither estimate is significant at the 5% level.

Geography is absorbed at district level, recovered from the recorded zila code in the 1990s rounds and from cluster coordinates thereafter; coarsening to division or refining to the survey cluster leaves the reduced forms unchanged (Supplemental Appendix Section S8). The estimates also hold separately in rural and urban subsamples, with 2SLS estimates of 0.122 and 0.111 on third-child neonatal death, both significant at the 10% level (Supplemental Appendix Table S17).

Is inference reliable with 191 unevenly sized villages? Cluster-robust standard errors can undercover with clusters as few and uneven as the 191 villages here. Re-testing with the wild cluster bootstrap of Cameron et al. (2008), with Rademacher weights and $B = 999$ replications under the null, and for the 2SLS estimate with the wild restricted efficient bootstrap of Davidson and MacKinnon (2010), every result significant at 5% remains so; the 2SLS p-value on third-child neonatal death moves from 0.008 to 0.006 (Supplemental Appendix Table S10).

6 Back-of-the-Envelope Mortality Cost

We translate the mortality estimates into excess deaths and monetized costs, using the BDHS alone, since it is the only one of our two sources that is nationally representative. Three caveats apply. The reduced-form coefficient is treated as constant and applied to assumed population shares (Supplemental Appendix Table S7, panel A); the estimand is identified for mothers who reach two live births, so the effect is assumed to extrapolate to comparable mothers; and the aggregate holds

the annual cohort at its recent level, which understates the earlier decades.

Under the assumptions in Supplemental Appendix Table S7 panel A, namely 3.0 million live births per year, 30% of them second live births, and 24% of two-child mothers having two daughters, about **216,000 mothers per year reach ≥ 2 live births with girl-girl first two**. The second-birth share is conservative, since the BDHS puts it at 32–34% of recent births. Applying the girl-girl coefficients to this population yields Table 5.

Table 5: Implied national mortality cost of son-preference-induced short birth spacing

	Third-child neonatal	Third-child infant
Excess deaths		
Per year	887 [231, 1,542]	1,618 [650, 2,587]
Aggregate, 1983–2022	35,467 [9,237, 61,697]	64,731 [25,982, 103,479]
Monetized cost, elasticity-1.5 VSL transfer ($683K)		
Per year	$605M [$158M, $1.05B]	$1.10B [$444M, $1.77B]
Aggregate, 1983–2022	$24.22B [$6.31B, $42.13B]	$44.20B [$17.74B, $70.66B]

Notes: BDHS only, since it is the nationally representative source. Each cell applies the girl-girl reduced-form coefficient (0.0041, SE 0.0015 for neonatal death), estimated on the full two-child sample, to 216,000 mothers per year reaching ≥ 2 live births with two daughters (Supplemental Appendix Table S7, panel A); brackets are 95% confidence intervals from the coefficient's standard error. Because it uses the full sample, the coefficient differs from the reduced form in Table 3, which is estimated on the 2SLS sample. Cost uses the Robinson et al. (2019) elasticity-1.5 VSL transfer, approximately $682,811 at Bangladeshi income; Supplemental Appendix Tables S8–S9 give the sensitivity. Neonatal deaths are a subset of infant deaths, so the columns are not additive. Aggregates multiply the per-year figure by the 40-year span of the sample, holding the annual cohort at its recent level.

Magnitudes. The son-preference distortion accounts for roughly **890 excess third-child neonatal deaths per year** in Bangladesh (95% CI 230–1,540), or **35,000** across the 1983–2022 span of the BDHS. Over the full first year of life the figures are **1,620** per year and **65,000** in aggregate, the same harm over a longer window rather than an addition to it. At the Robinson et al. (2019) preferred transfer for low- and middle-income countries, the national cost is **$0.6–$1.1 billion** per year and **$24–$44 billion** in aggregate, on the neonatal and infant measures respectively. Alternative VSL values within the published range change the level but not the comparison across outcomes (Odihi et al., 2021; Robinson et al., 2019); Supplemental Appendix Tables S8–S9 report the sensitivity.

7 Conclusion

We instrument the timing of the third conception with the sex composition of the first two children and ask whether son-preference-induced compression of the birth interval raises third-child mortality. It does, in two independent sources, and the behavior behind it is confined to two-daughter

families: two sons produce no response on the short-spacing margin.

The channel is of the kind Jayachandran and Kuziemko (2011) identify in a different setting: son preference lowering child survival without any parent choosing to invest less in a daughter. What differs here is who bears the cost: not a daughter, but the third-born, conceived sooner because the parents want a son and male with roughly even probability.

The design identifies a fertility acceleration once a couple has had two daughters and the mortality cost it imposes on the third child. The deeper preference behind it is not separately identified: son preference under a desired-family-size cap and an asymmetric gender-mix preference with strong son weights both rationalize the boy-boy versus girl-girl asymmetry and the null at the first-to-second birth margin.

The two data sources have different scope, coverage, and measurement problems, and yet their first-stage, reduced-form and IV estimates agree, which makes it unlikely that the channel is an artifact of either source; the checks in Section 5 place the harm at birth rather than in how the third child is later treated.

The cost is not small. Applied to the national birth cohort, the BDHS estimates imply about 890 excess third-child neonatal deaths and 1,620 excess infant deaths a year, some 35,000 and 65,000 over the four decades the survey covers, or \$0.6–\$1.1 billion a year at a value-of-statistical-life transfer for Bangladesh. The policy implication is actionable. Family-planning counseling in Bangladesh already promotes birth spacing, and our estimates identify the households for which that message has the highest return: mothers of two daughters, in the months immediately following the second birth. Whether counseling can move behavior in households under pressure to produce a son is a separate question, and one that warrants future experimental studies.

Two limitations are worth noting. First, both sources record live births only. A hurried third conception that ends in miscarriage or abortion is not observed, so the spacing we instrument is the interval to a third live birth rather than to the next pregnancy. Second, surveillance records births only within its window, so a mother's first observed birth is not always her first; Chakaria's recorded birth order indicates that 60% of first observed births are true first births, and this measurement error attenuates rather than inflates the estimates (Supplemental Appendix Section S10).

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alphabetical. All errors are our own.

CRediT authorship contribution statement

Mohammad Islamul Haque: Conceptualization, Methodology, Software, Formal analysis, Data curation (BDHS), Writing – original draft, Writing – review & editing. **Syed Manzoor Ahmed Hanifi:** Conceptualization, Resources, Data curation (HDSS), Investigation, Writing – review & editing.

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Declaration of competing interest

The authors declare no competing interest. The Matlab and Chakaria HDSS data were provided by ICDDR,B, which employs one author and granted permission for their use and publication but had no role in the design, analysis, or conclusions of the study and did not review the manuscript before submission.

Data availability

The Bangladesh DHS birth-recode files (NIPORT and ICF, 1994–2024) are available from the DHS Program (https://dhsprogram.com/data/) on registration. The de-identified Matlab and Chakaria HDSS extracts used in this paper (ICDDR,B, 2025) will be deposited, with the permission of ICDDR,B, together with all code in a public replication repository on acceptance.

Declaration of generative AI and AI-assisted technologies in the manuscript preparation process

During the preparation of this work the authors used Claude (Anthropic) in order to write and debug code and to edit the draft for grammar, sentence structure and length. After using this tool, the authors reviewed and edited the content as needed and take full responsibility for the content of the published article.

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