The Intergenerational Health Effects of Child Marriage Bans [*]

Dung D. Le [†] Teresa Molina [‡] Yoko Ibuka [§] Rei Goto [¶]

June 18, 2024

Abstract

Using data from 18 countries, we investigate the effects of child marriage bans on infant and under-5 mortality. We use variation in mothers' exposure to the ban across cohorts within each country and regional variation in "treatment intensity," calculated based on child marriage prevalence and marriage age prior to the ban. We find that child marriage bans reduced infant and under-5 mortality, with magnitudes of 14.3 and 19.9 percent corresponding to a one standard deviation change in treatment intensity. Reductions were driven by low-income countries and less wealthy households, primarily due to increases in age at first marriage and birth.

Keywords: child marriage bans, early marriage, child mortality, Demographic and Health Surveys
JEL classification: I10, I15, J12, J18

[*]This work has greatly benefited from the feedback of seminar/meeting participants at Japanese Economic Association, the Japan-Taiwan-Korea Health Economics Association Joint Conference, Hitotsubashi University, Sophia University, University of Connecticut, University of Hawaii at Manoa, NBER, and Essen Health Conference. Financial support from JSPS Kakenhi (23K20578) is greatly appreciated.
[†]Waseda Institute for Advanced Study (WIAS), Waseda University
[‡]Department of Economics, University of Hawaii at Manoa
[§]Corresponding author. Faculty of Economics, Keio University
[¶]Graduate School of Business Administration, Keio University

1 Introduction

There is substantial causal evidence documenting that delaying age of marriage yields better education and labor market outcomes for women (Field and Ambrus, 2008; Sunder, 2019; Asadullah and Wahhaj, 2019), as well as better health and education outcomes for their children (Chari et al., 2017; Sunder, 2019; Asadullah and Wahhaj, 2019; Garcia-Hombrados, 2022; Sekhri and Debnath, 2014). At the same time, child marriage – where one or both of the spouses are under the age of 18 – is widely recognized as a violation of human rights and both a driver and consequence of gender inequality. For governments interested in eliminating the practice of child marriage, age of marriage laws are perhaps the most direct policy lever available. While many studies have examined the effects of these laws on the outcomes of women (Bergstrom and Özler, 2023), there has been much less research on the effects of these laws on the health of the next generation.

In this paper, we estimate the intergenerational effect of child marriage bans, which set a legal minimum age of marriage to 18, on mortality rates among children of the affected women. Existing work shows that child marriage bans improve women's socioeconomic outcomes and delay age of marriage in some contexts (McGavock, 2021; Rokicki, 2021; Wilson, 2022). We also know, primarily from studies that use age of menarche as an instrumental variable, that delaying age of marriage leads to better outcomes for children (Chari et al., 2017; Sunder, 2019; Asadullah and Wahhaj, 2019; Garcia-Hombrados, 2022; Sekhri and Debnath, 2014). However, it is not clear whether and to what extent child marriage bans will improve child health, especially given that age of marriage laws are often not properly enforced (Collin and Talbot, 2023; Batyra and Pesando, 2021) and can sometimes lead to substitution away from marriage to informal unions (Bellés-Obrero and Lombardi, 2023). In addition, child marriage bans, when they are enforced, may have the unintended effect of making child brides more hesitant to seek prenatal or postnatal care (for fear of legal punishment), which could lead to worse health outcomes for their children.

In short, it is not clear whether child marriage bans will generate health benefits for the next generation. Three papers investigate an Ethiopian reform that raised the legal minimum age of marriage and reach different conclusions about the effects of this policy on infant mortality (McGavock, 2021; Rokicki, 2021; Garcia-Hombrados, 2022). In this paper, we expand on this body of work by examining child marriage bans in 18 developing countries.

For this analysis, we use the MACHEquity Child Marriage Policy Database, which contains information on child marriage bans over the period 1995 to 2012, to identify the timing of the bans in each country (?). This country-level dataset is then linked to the Demographic and Health Surveys (DHS), representative surveys of women aged 15-49 in low- and middle-income countries (LMICs), from which the main outcomes are extracted. After combining the two sources, we have 18 countries that legally banned child marriage under the age of 18 during the period from 1995 to 2012.

To isolate the causal effects of the bans, we rely on a similar strategy as Wilson (2022), which uses the same data to analyze the effect of child marriage bans on women's socioeconomic outcomes. This strategy exploits two sources of variation: subnational regional differences in the pre-ban age at marriage and variation across cohorts within countries in exposure to the bans. Following estimation strategies commonly used in the economic literature, we employ a generalized difference-in-differences framework that relies on this geographic and cohort-level variation. We first calculate a region-specific measure of treatment intensity, defined such that locations where, in the pre-ban period, the occurrence of child marriage was common and child brides married particularly young are considered to have high treatment intensity, which means that individuals in these locations should be more affected by a child marriage ban.[1] The treatment intensity variable is then interacted with an indicator for cohorts exposed to the bans (i.e., individuals under the age of 18 at the time a ban was implemented in their country). If the bans reduce child mortality, then there should be a larger gap between cohorts born before and after a ban in areas with high treatment intensity compared to areas of low treatment intensity.

Our results show that the bans have statistically significant and sizable effects on infant and under-5 mortality. Specifically, we find that a one standard deviation (SD) increase in treatment intensity reduced infant and under-5 mortality by about 0.95 and 1.97 percentage points, respectively (approximately 14.3 and 19.9 percent relative to the mean of pre-ban cohorts). Our estimates are robust to various specification tests, including those that account for contemporaneous policies and differential trends due to regional characteristics. The pattern of our coefficients from a standard event study analysis, as well as the approach proposed by Callaway et al. (2024), lends support to the validity of our empirical strategy.

Examining heterogeneous effects of the bans, we find that the effects were driven by low-income countries and households with lower wealth. These results suggest that raising the minimum age at marriage might be more beneficial for low-income countries and individuals living in households

[1] This approach is similar to many other generalized difference-in-differences papers (Lucas, 2010; Bleakley, 2007; Cutler et al., 2010; Wilson, 2022; Barofsky et al., 2015).

with limited financial resources.

The bans appear to be reducing child mortality primarily by delaying age at first marriage and first birth. Women going into marriage or pregnancy with more maturity should have more agency and bargaining power, which are likely to be important for prenatal and postnatal health investments. Neither increased maternal schooling nor employment rates appear to be important mechanisms in this setting.

Our paper contributes to a large body of evidence on the impacts of child marriage laws on the outcomes of women and their children. While most work has focused on the impacts on women – age at marriage, education, and economic outcomes (Collin and Talbot, 2023; McGavock, 2021; Wilson, 2022; Bellés-Obrero and Lombardi, 2023; Wang and Wang, 2017; Dahl, 2010; Bharadwaj, 2015) – our study aims to shed light on the impacts on child mortality, for which existing evidence is scant, mixed, and currently only focused on Ethiopia (McGavock, 2021; Rokicki, 2021; Garcia-Hombrados, 2022). A key distinguishing feature of our study, compared to these three, is its scope: our sample includes 18 developing countries across four continents. In addition to providing more external validity, the use of pooled data from multiple countries helps reduce the possibility of biased estimates due to the low rates of infant and under-5 mortality, as discussed by Dursun et al. (2017).[2] This also allows us to investigate sources of heterogeneity and potential underlying mechanisms.

This study is also closely related to a well-established literature on the effects of delaying marriage (outside of the context of child marriage laws). Studies in this literature typically use age at menarche as an instrumental variable for age at marriage (Field and Ambrus, 2008; Asadullah and Wahhaj, 2019; Chari et al., 2017; Sunder, 2019; Garcia-Hombrados, 2022; Sekhri and Debnath, 2014). Because age at menarche may not satisfy the exclusion restriction for a number of reasons, our paper offers valuable evidence on this topic by using a different source of variation.[3] In addition, our focus on child marriage laws allows us to shed light on the effectiveness of a potential policy lever for reducing early marriage.

[2]It has been suggested that in the case of rare events (e.g., infant and child mortality) where the mean value is small, estimates from a small sample size could result in bias. Specifically, Dursun et al. (2017), using three waves of the Turkish DHS, show that small sample sizes with low average values of child mortality and low birth weight lead to biased estimates in their study. In addition, Chari et al. (2017) and McGavock (2021) argue that low child mortality rates tend to produce noisy estimates due to low statistical power.

[3]Huang et al. (2019) describe how menarcheal age may be correlated with cognitive development trajectories, early exposure to family-related disruptions, stress, and malnutrition, which can generate violations of the exclusion restriction if appropriate controls are not available. Wilson (2022) suggests that selection in the marriage market may also lead to violations.

Child marriage bans are just one example of an intervention aimed at reducing child marriage, and our study therefore speaks to a broader literature on this more general class of interventions. Bergstrom and Özler (2023) review studies on 15 categories of interventions (including age of marriage laws, job opportunities, and cash transfers) and conclude that none have been consistently found to be effective at reducing child marriage. Nevertheless, several recent papers have documented that community-based education and economic incentives can reduce child marriage (Cohen et al., 2023; Chow and Vivalt, 2022; Buchmann et al., 2023). The results of our study suggest that the benefits of these less traditional interventions could extend to the children of the affected women as well.

Previous economic literature in developing countries has shown that education reforms and health interventions do not only have positive effects on the directly affected individuals, but also have intergenerational effects on health and mortality (Grépin and Bharadwaj, 2015; Keats, 2018; Akresh et al., 2018; Cornelissen and Dang, 2022; De Neve and Fink, 2018; Walker et al., 2023). Our study contributes to this large body of literature by revealing child marriage bans as yet another government policy that has the potential to improve the health of the next generation.

2 Background and Data

In this section, we provide some background on child marriage across the globe and describe the two sources of data used in our analysis: country-level data on the timing of the bans in each country (MACHEquity Child Marriage Policy Database) and individual-level data on child mortality and related outcomes (the DHS).

2.1 Child Marriage

Since the 1940's, several international agreements have contributed to forming a general global recognition of the need for action against child marriage.[4] For example, the 1995 Beijing Declaration and Platform for Action urged all countries to "enact and strictly enforce" a legal minimum age at marriage and reaffirm age 18 as the accepted legal minimum age at marriage. The declaration also called for banning child marriage worldwide (United Nations, 1995). After the Beijing Declaration, the proportion of countries allowing child marriage fell significantly – from 80% in

[4]See Wilson (2022) for a more detailed description of these agreements.

1995 to 56% in 2013 (Arthur et al., 2018). Eradicating child marriage is now one of the main targets of the fifth Sustainable Development Goal (Wodon et al., 2017).

Despite international efforts, child marriage still takes place across many countries, cultures, religions, and ethnicities. Child marriage is much more prominent among girls than boys, and it is most common in South Asia and sub-Saharan Africa. Globally, it is estimated that 29% of women aged 20 to 49 were married before the age of 18. In South Asia, this figure is 56% (UNICEF, 2017).

2.2 MACHEquity Child Marriage Policy Database

We use the MACHEquity child marriage policy database to identify the timing of child marriage bans in each country. This database was constructed using various reliable legal sources including country government websites, the Foreign Law Guide, the Lexadin World Law Guide, and the NATLEX database, among others (World Policy Center, 2022). It contains detailed information on the minimum legal age of marriage with parental consent and minimum legal age without parental consent from 1995 to 2012 for both males and females. The minimum age at marriage in each country is categorized into five groups: "18 years or older"; "16 or 17 years old"; "14 or 15 years old"; "9 to 13 years old"; and "No minimum age." Because allowing child marriage without parental consent is not common and because child marriage is more prevalent among females, we follow Wilson (2022) and focus on the legal minimum age at marriage with parental consent for females. We further restrict the database to countries that have information on the legal minimum age at marriage with parental consent in 1995, resulting in a balanced panel of 105 countries. Of these countries, there are 18 countries which changed their legal minimum age at marriage with parental consent to 18 during the 1995-2012 period and also have a DHS survey following this law change. Appendix Table A1 lists all of these countries, along with the year of the child marriage ban and the most recent survey year of the DHS. The earliest countries to ban child marriage were Kazakhstan and Peru in 1998 and 1999, respectively, while Liberia banned child marriage most recently in 2012. There is substantial variation in the time between ban year and survey year, ranging from 1 to 17 years.

2.3 Demographic and Health Survey (DHS) data

We extract child mortality and other outcomes from the DHS, a set of health-focused household surveys administered to women of childbearing age (ICF, 2018). For countries with multiple DHS surveys fielded after the implementation of a child marriage ban, we use the most recent survey available.

The DHS uses a stratified two-stage cluster design. Specifically, it first draws enumeration areas from national census files and then draws a sample of households from each enumeration area. Over the past three decades, the DHS has asked a comparable set of questions to representative samples of women aged 15-49. Because we are interested in child health, we restrict our attention to women who report having ever given birth. We also drop any respondents who were born after a child marriage ban was implemented their country to reduce the likelihood of picking up indirect effects operating through their mother's exposure to the ban. This restriction only drops 25 women, leaving us with a sample size of 203,945 women from 18 countries, with an average age of 23.6 at the time of the ban and 32.9 at the time of survey, as reported in column 1 of Table 1. 42% of the sample lives in an urban area.

Although each survey round is a cross-section, the survey collects a retrospective birth history that documents information on each child ever born alive to the respondent. The birth history module records detailed information on the month of birth of the child, whether he or she died and the age in months at death. These questions are the same across survey rounds and countries, making the child death measures comparable across countries.

Using the birth history and reported age at death, we construct indicators of infant mortality (death between birth and the first birthday) and under-5 mortality (death between birth and the fifth birthday), two outcomes that are widely used in economic studies of child death (Kuecken et al., 2021; Chou et al., 2010; Lu and Vogl, 2023; Kammerlander and Schulze, 2023; Baird et al., 2011) and documented in international reports (United Nations Inter-agency Group for Child Mortality Estimation, 2018).

We focus on mortality of the first-born child because we do not have completed fertility for all mothers in our sample. If child marriage bans affect the number of children or birth spacing, effects on higher-parity births could be driven by changes in these variables and, subsequently, changes in the intrahousehold allocation of resources across children. Approximately 6.3% and 9.3% of women have experienced the death of their first-born child under age 1 and age 5, respectively.

To explore potential mechanisms, we also utilize information on our sample mothers' educational attainment, employment, age at first marriage, age at first birth, and total fertility. As we report in column 1 of Appendix Table A2, the mothers in our sample married and gave birth young (on average at ages 18.9 and 20.1, respectively), with 29% of them having given birth before turning 18. Educational attainment (5.7 years) and employment rates (0.58) are low. At the time of survey, mothers in the sample have an average of 3.6 children – a high number given that the respondents themselves are 32.9 years old on average.

Table 1: Summary Statistics

	(1) Unweighted	(2) Weighted
Age at survey	32.9	32.9
	(8.52)	(8.47)
Age at ban	24.4	24.5
	(9.95)	(10.1)
Urban	0.42	0.42
	(0.49)	(0.49)
Female child	0.48	0.48
	(0.50)	(0.50)
Infant Mortality	0.063	0.064
	(0.24)	(0.25)
Under-5 Mortality	0.093	0.095
	(0.29)	(0.29)
Observations	203945	203945

Notes: Standard deviations in parentheses. Sample restricts to women who have ever given birth and who were born after the child marriage ban was implemented in their country. Column 2 weights the sample using the methods described in section 3.3.

3 Empirical Strategy

3.1 Generalized Difference-in-Differences

Countries that implement child marriage bans are systematically different in various ways from countries that do not; often, a ban is implemented as a response to very high rates of child marriage. Cross-country comparisons would therefore fail to provide a causal estimate of the effect of child marriage bans on child health. For this reason, our entire analysis restricts to countries that did implement a child marriage ban (sometime between 1995 and 2012) and compares children born to women of different birth cohorts. Women under 18 at the time of a ban's implementation would have been affected by the ban, whereas those 18 and older would have not. Simply comparing the children of affected and unaffected cohorts, however, does not allow us to isolate the effect of the ban from other trends in child health across maternal birth cohorts. We therefore also take advantage of within-country variation in how consequential a ban would actually be, given existing marriage practices. In sum, our empirical strategy relies on two sources of variation: (1) within-country variation in exposure to the bans across cohorts, and (2) within-country variation in "treatment" intensity across regions, where our treatment of interest is a child marriage ban. A region is defined by the largest geographic administrative region in the country, along with the urban-rural status of the exact location. For example, urban areas of one state will form one region, while rural areas of that same state will form a separate region.

Following Wilson (2022), we define treatment intensity in subnational region r, focusing on "pre-ban" cohorts aged 18 to 30 at the time of the ban, as follows:[5]

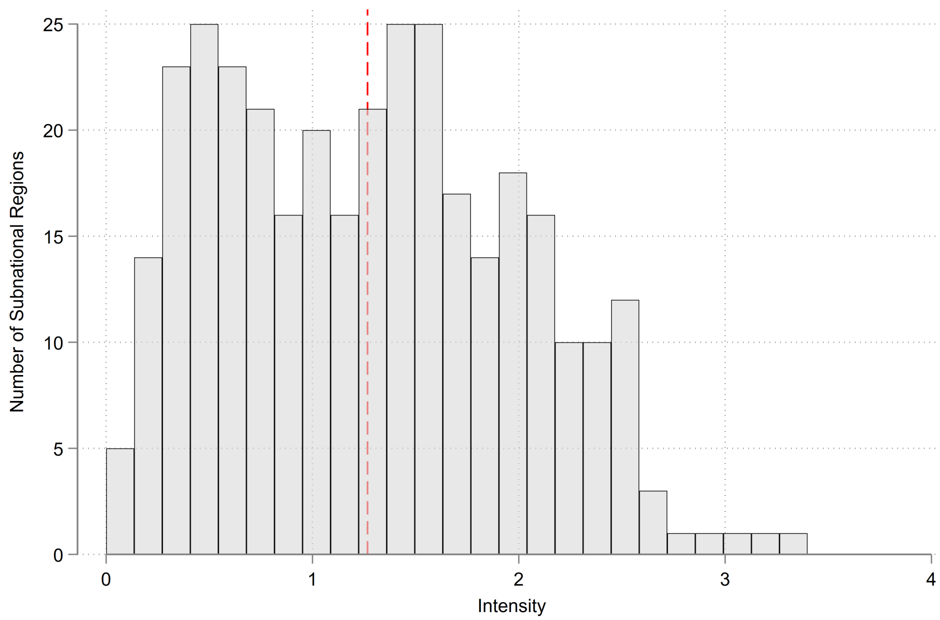
$$\text{intensity}_r = \frac{\sum_{i=1}^{N_r} \mathbb{1}(\text{married before 18})_i \times (18 - \text{age first married}_i)}{N_r} \tag{1}$$

The denominator (N_r) is the number of women in region r who belong to these pre-ban cohorts, unaffected by the ban because they were at least 18 years old when the ban was implemented in their country. The numerator is the summation (across all N_r women in pre-ban cohorts in region r) of the product of two terms: an indicator for getting married before the age of 18 (extensive margin) and the difference between 18 and the woman's age at first marriage (intensive margin). This definition treats a ban as having greater intensity in subnational regions where the pre-ban average age at marriage is lower. The idea is that a ban should have stronger effects where, in the absence of the ban, women were marrying at younger ages. Defining treatment intensity in this way takes advantage of considerable regional variation in the prevalence of child marriage pre-ban. Figure 1 exhibits the distribution of our treatment intensity variable, which has a mean of 1.27 and a standard deviation of 0.72.

To estimate the effect of child marriage bans on child mortality of the next generation, we restrict to the first-born child of all mothers in the DHS who were born before the child marriage

[5] While the upper age limit of 30 ensures that we are calculating intensity using a similar age-at-ban distribution across countries, dropping this restriction generates a variable that is highly correlated (r=0.96) with our current intensity variable and results that are very similar to our current set of results.

Figure 1: Distribution of treatment intensity variable



Notes: Treatment intensity is calculated as described in Equation (1). The vertical dashed line depicts the mean (1.27). The standard deviation is 0.72

ban was implemented in their country. We estimate the following specification:

$$y_{ikr} = \alpha(\text{post-ban}_{kc(r)} \times \text{intensity}_r) + \gamma_{kc(r)} + \theta_r + \epsilon_{ikr}, \tag{2}$$

where y_{ikr} represents the outcome of interest (firstborn mortality indicator or a related potential mechanism) for mother i in cohort k in subnational region r, which is part of a country that we denote using subscript c. $postban_{kc}$ is an indicator variable coded as one if cohort k in country c is under the age of 18 at the time of the of the ban (the affected post-ban cohorts) and zero otherwise (unaffected pre-ban cohorts). We are interested in α, the coefficient on the interaction between $intensity_r$ and $postban_{kc}$, which captures how the difference in child mortality between affected and unaffected cohorts varies across areas with different treatment intensity. Like Wilson (2022), we control for both γ_{kc} and θ_r. Country-specific cohort effects (γ_{kc}) account for factors like age at the time of survey and country-specific differences in marriage or fertility patterns across cohorts. Subnational region fixed effects (θ_r) control for time-invariant unobserved characteristics specific to each region, including geographic and cultural factors. In all estimations, we cluster standard errors at the subnational region level to account for potential correlations of error terms within regions.[6]

The validity of this empirical strategy rests on the assumption that, conditional on time-invariant subnational region fixed effects and country-specific cohort fixed effects, the relationship between treatment intensity and outcomes would have remained the same for cohorts who were 18 and older and cohorts under 18 at the time of ban, if the ban had not been implemented.

3.2 Event Study

One way to shed light on this assumption is to estimate event study regressions that will reveal whether the relationship between treatment intensity and child mortality was changing across cohorts among the set of pre-ban cohorts (none of whom were exposed to the ban). If this is the case, it would suggest the relationship between treatment intensity and mortality was being affected by something other than the implementation of a child marriage ban in a country, and therefore signal a potential violation of the identifying assumption.

In our event study regressions, we replace the post-ban indicator with a set of dummies representing the respondent's age at the time of the ban, as described in the following specification:

$$y_{ikr} = \sum_{t=-15, t\neq 0}^{14} \alpha_t(\mathbb{1}(\text{age at ban}_{kc(r)} = 18 + t) \times \text{intensity}_r) + \gamma_{kc(r)} + \theta_r + \epsilon_{ikr}. \tag{3}$$

Here, $\mathbb{1}(\text{age at ban}_{kc} = 18 + t)$ refers to a series of dummies indicating the respondent's age at the time of the ban, ranging from 3 through 32, which creates 15 pre-ban and 15 post-ban cohorts.[7] $\mathbb{1}(\text{age at ban}_{kc} = 18)$ is omitted as the reference cohort. The set of α_t coefficients are the main coefficients of interest. Since women aged 18 and older at the time of a ban should not be affected by the ban, we expect the coefficients for these cohorts to be statistically insignificant, indicating no differential pre-trends across the treatment intensity distribution for the unaffected cohorts who turned 18 prior to the bans.

A growing literature highlights that the coefficients from Equation (3), essentially a two-way fixed effects regression with staggered treatment timing, might be difficult to interpret if treatment effects are heterogeneous across regions (Roth et al., 2023). We therefore also use the methods

[6]In total, our study sample consists of 339 subnational regions.

[7]We combine those aged 3 and younger at the time of the ban and combine all cohorts aged 32 and older at the time of the ban.

proposed by Callaway et al. (2024). Their approach takes advantage of the methods in Callaway and Sant'Anna (2021), designed for a binary treatment, that produce estimates with a straightforward interpretation even under treatment effect heterogeneity, and applies them to the case of a continuous treatment variable.

3.3 Weighting Procedure

Because we are interested in child health, our analysis necessarily restricts to women who have ever given birth. It is therefore important to determine whether our variable of interest ($postban_{kc} \times intensity_r$) predicts selection into the sample. If it does, this would imply that a significant α in Equation (2) could be due to changes in the composition of our mother sample – specifically, differences between mothers in the affected versus unaffected cohorts varying across the treatment intensity distribution. This could happen if child marriage bans affected the extensive margin of the fertility decision (most plausibly by reducing the likelihood of a woman ever giving birth). Alternatively, because the DHS surveys women of different ages, differences in the timing of first birth across the treatment intensity distribution could also result in sample composition issues.

To investigate further, we examine all women in the DHS, including those who have never given birth. Our outcome of interest is an indicator variable for having ever given birth (which is equal to 1 for all mothers in our main analysis sample). We then estimate Equation (2) using this indicator as our outcome variable and report our results in column 1 of Table 2. The positive and statistically significant interaction coefficient reveals that our variable of interest does indeed predict selection into the sample. The sign of the coefficient helps shed light on why. It is unlikely that the bans are actually inducing women (who otherwise would not have had any children) to have children, which is what would be implied by a positive coefficient. Rather, we argue the positive coefficient stems from the fact that the DHS captures women at different ages. The likelihood of being a mother (and therefore being in our sample) is lower for women who are younger at the time of survey, and post-ban women (in later birth cohorts) are younger than pre-ban women at the time of survey. Because areas with high treatment intensity are places where women traditionally married early relative to low intensity areas, post-ban women in high intensity areas are more likely to have already married and given birth by the time they are interviewed and are therefore more likely to be included in our mother sample. In pre-ban cohorts, on the other hand, where average age at survey is much higher, the vast majority of women across the treatment intensity distribution have already had a child.

Table 2: Selection

	(1) In Mother Sample	(2) In Mother Sample	(3) In Mother Sample
post-ban × intensity	0.0830***	0.0157	-0.0003
	(0.0100)	(0.0145)	(0.0111)
Added Survey Age Controls	None	Linear Age × Treatment	Age Dummies × Treatment
Mean outcome	0.74	0.74	0.74
Observations	275,115	275,115	275,115

Notes: Standard errors clustered at subnational region level are in parentheses. * p< 0.1, ** p< 0.05, *** p< 0.01. Sample includes all women who were born after the child marriage ban was implemented in their country. *post-ban* is an indicator variable coded as one if individuals are under the age of 18 at the time of the of the ban. All specifications control for country-cohort fixed effects and subnational region fixed effects.

To test whether this does indeed explain the positive selection coefficient, we repeat the selection regression described above and allow for trends in motherhood by age at survey to vary across the treatment intensity distribution. In column 2, we do this by controlling for an age-by-intensity interaction, and in column 3, we control for age fixed effects interacted with the intensity variable (both of which we are only able to do because we have data from multiple countries surveyed in different years). As shown in columns 2 and 3 of Table 2, the main coefficient of interest ($postban_{kc} \times intensity_r$) is no longer statistically significant and is much smaller in magnitude. This supports our explanation for the positive coefficient in column 1: age at survey is an important predictor of selection into the mother sample and trends in motherhood by age at survey vary across regions of different treatment intensity levels.[8] We also note that – even after properly accounting for age at survey – there is no evidence that child marriage bans reduce the likelihood of women becoming mothers (the coefficient in column 2 is positive, and the coefficient in column 3 is close to zero).

The fact that there are more post-ban mothers in high intensity regions than in low intensity regions complicates our interpretation of α in Equation (2), which relies essentially on a difference in differences: between children in high intensity and low intensity regions, born to mothers in post-ban vs. pre-ban cohorts. These "additional" mothers gave birth at an earlier age, and this could affect the child mortality difference-in-differences estimates if childbearing age affects child mortality. To deal with this issue, when we estimate Equations (2) and (3), we re-weight our mother

[8] While our main specification controls for age at survey through the country-cohort fixed effects (γ_{kc}), these do not allow for survey age trends to vary across subnational regions.

sample to equalize the age (at survey) distribution across subnational regions (to match the overall age distribution of the mother sample).[9] This is similar in spirit to adding the age-by-intensity controls used in Table 2 without imposing a specific functional form.

Column 2 of Tables 1 and A2 report summary statistics for our mother sample calculated using these weights. By construction, average age at survey is identical across the two columns. In addition, the weighted and unweighted samples look very similar in terms of all other variables.

4 Results

4.1 Main Results

Table 3 presents the main results estimated from Equation (2) using the weighted sample. The results show that the bans had strong and sizeable effects on infant and under-5 mortality. Specifically, a one-unit increase in the intensity of the bans significantly reduced infant and under-5 mortality by approximately 1.3 and 2.7 percentage points, respectively. In terms of SD, a one SD (i.e., 0.728 unit) increase in the intensity of the bans corresponds to reductions in infant and under-5 mortality of approximately 0.95 and 1.97 percentage points. These effect sizes are equivalent to about 14.3% and 19.9% reductions in infant and under-5 mortality relative to the mean of the unaffected pre-ban group.

Table 3: Effects of child marriage bans on first-born child mortality

	(1) Infant Mortality	(2) Under-5 Mortality
post-ban × intensity	-0.013***	-0.027***
	(0.003)	(0.005)
Mean outcome of pre-ban cohorts	0.066	0.099
Observations	203,945	203,945

Notes: Standard errors clustered at subnational region level are in parentheses. * p< 0.1, ** p< 0.05, *** p< 0.01. *post-ban* is an indicator variable coded as one if individuals are under the age of 18 at the time of the of the ban. All specifications use the weights described in section 3.3 and control for country-cohort fixed effects and subnational region fixed effects.

[9] Let N denote the total number of women in the sample, N_a denote the total number of women of age a, and N_r denote the total number of women in region r. For women in region r aged a at the time of survey, a weight equal to $\frac{\frac{1}{N}\sum_N \mathbb{1}(region=r)}{\frac{1}{Na}\sum_{Na} \mathbb{1}(region=r)}$, which is equivalent to $\frac{\frac{1}{N}\sum_N \mathbb{1}(age=a)}{\frac{1}{Nr}\sum_{Nr} \mathbb{1}(age=a)}$, equalizes the age distribution across regions.

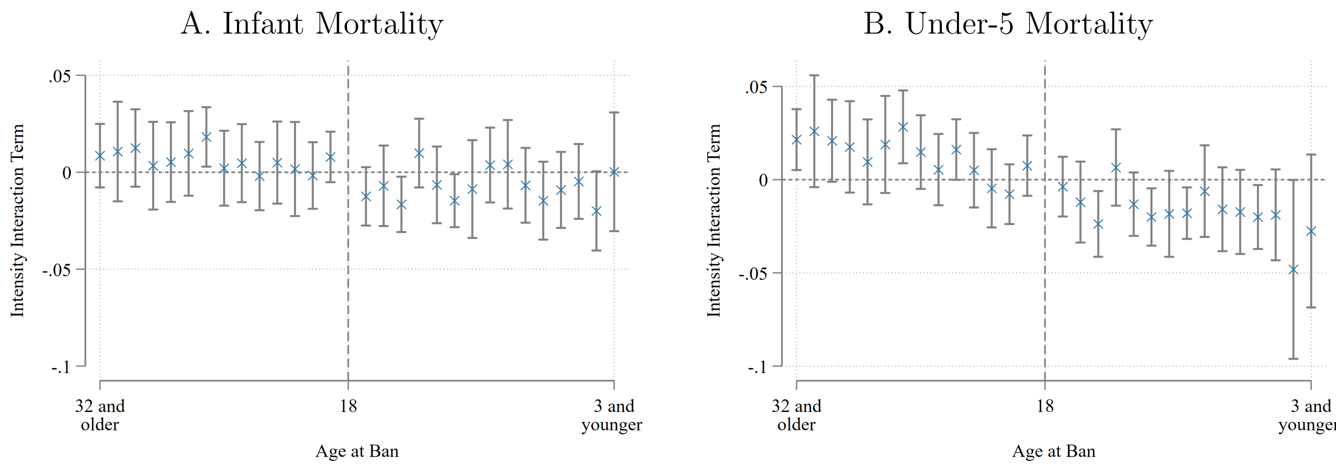
One way to interpret the coefficients in Table 3 is that treatment intensity, which was positively correlated with infant and under-5 mortality for pre-ban cohorts, became less positively correlated for cohorts born after a child marriage ban, which we attribute to high treatment intensity regions seeing larger reductions in child mortality due to greater "exposure" to the bans. While we have interpreted this as evidence that child marriage bans decreased child mortality, an event study regression can add further support by isolating exactly when the intensity-mortality correlation began to change.

Figure 2 provides a graphical illustration of our event study results. The x-axis represents the respondent's age at the time of the ban. For each age, we report the regression coefficients along with 95% confidence intervals estimated from Equation (3), which represent the change in the relationship between treatment intensity and child mortality for a particular cohort, relative to the reference cohort group. The dashed vertical line marks this reference group – those aged 18 at the time of the ban. Since the bans only impose restrictions on individuals under the age of 18, the relationship between treatment intensity and child mortality should not vary across cohorts for those aged 18 and older at the time of the bans. Consistent with this, the infant mortality figure reveals a flat pattern of (mostly) statistically insignificant estimates for cohorts aged over 18 who should not have been affected by the bans. The pre-trend is relatively flat for under-5 mortality as well, though it does exhibit a slight downward trend. When we use the methods of Callaway et al. (2024), pre-trends are flat for both outcome variables (Appendix Figure B1). These results lend support to the assumption that the relationship between treatment intensity and child mortality would have remained the same if the bans had not been implemented, and that the change in this relationship (summarized by the coefficient estimates in Table 3) is indeed due to the child marriage bans.

As for the ban-affected cohorts, for under-5 mortality, there is a clear shift downwards. With only one exception, the coefficients for post-ban cohorts are negative and several are statistically significant. Although the coefficients for cohorts aged 16 and 17 at the time of the ban are small in magnitude, the treatment intensity interactions become more negative and statistically significant for younger cohorts. The larger effect sizes for younger cohorts likely reflects the fact that women who are older but still under the age of 18 at the time of the bans are less exposed to the ban than younger cohorts – either because it takes some time for them to learn about the bans or for the bans to be fully enforced, or because they are more likely to already be married by the time the ban is implemented. For postnatal mortality, the pattern is similar but slightly weaker. Most (but not all) post-ban coefficients are negative, though the coefficients are smaller in magnitude and the coefficients are less precisely estimated than those in the under-5 figure.

In Appendix Figure B1, the results for under-5 mortality are consistent with our regression and event study results. As mentioned above, they in fact provide even stronger support for the absence of significant pre-trends. Though the results for infant mortality also reveal no evidence of pre-trends, the pattern of the post-ban coefficients is much weaker. Nevertheless, all in all, these results provide support for our interpretation of the regression results in Table 3: child marriage bans led to a statistically significant reduction in both infant and under-5 mortality.

Figure 2: Event Study Results



Notes: Standard errors clustered at region level. Figure reports coefficients (and 95% confidence intervals) on the interactions between treatment intensity and age at ban from Equation (3). The dashed vertical line denotes the omitted reference cohort, those aged 18 at the time of the ban.

4.2 Heterogeneity Analysis

For which countries and individuals were the effects of these bans the largest? We first estimate heterogeneous effects by country income levels based on World Bank income classifications. We find that the magnitude of the negative effect of child marriage bans decreases with country income: child marriage bans reduced infant and under-5 mortality primarily in low-income and lower-middle countries. As reported in Table 4, coefficient estimates for middle-income countries are statistically insignificant and significantly different from estimates in low-income countries. Children in low-income countries, where child marriage, child mortality, and poverty rates are higher, (UNFPA-UNICEF, 2019; Wodon et al., 2017), benefit the most from the bans.

Table 4: Heterogeneous effects by country income classification

	Infant Mortality				Under-5 Mortality			
	(1) Low Income	(2) Lower-Middle Income	(3) Middle Income	(4) Difference	(5) Low Income	(6) Lower-Middle Income	(7) Middle Income	(8) Difference
post-ban × intensity	-0.028***	-0.010***	-0.004		-0.047***	-0.023***	-0.009	
	(0.007)	(0.003)	(0.011)		(0.009)	(0.006)	(0.012)	
Low vs. Middle				-0.024*				-0.038**
				(0.012)				(0.015)
Low-Mid vs. Middle				-0.006				-0.014
				(0.011)				(0.013)
Mean outcome of pre-ban cohorts	0.087	0.059	0.041		0.13	0.088	0.055	
Observations	65,434	43,910	40,193		65,434	43,910	40,193	

Notes: Standard errors clustered at subnational region level are in parentheses. * p< 0.1, ** p< 0.05, *** p< 0.01. *post-ban* is an indicator variable coded as one if individuals are under the age of 18 at the time of the of the ban. All specifications use the weights described in section 3.3 and control for country-cohort fixed effects and subnational region fixed effects. Country income levels are taken from World Bank income classifications.

Table 5 presents heterogeneity by individual and household characteristics. In panel A, we show that results are similar for boys and girls. Though coefficient estimates are larger for boys, they are not significantly different from the estimates for girls. Panel B reveals that differences between rural and urban areas in the effects of the bans were small and statistically insignificant, which is in contrast with the schooling benefits of these bans, documented by Wilson (2022) to be concentrated in urban areas. This suggests that schooling may not be an important mechanism for these child mortality effects, as we discuss in more detail in the next sub-section. Differences were more pronounced across household wealth. Defining poor households as those in the bottom two quintiles of each country's asset index distribution, we find larger estimates for poor than rich households. The difference between the two groups is statistically significant for under-5 mortality but not for infant mortality. These results suggest that raising the minimum age at marriage might be more beneficial for those with limited financial resources.

4.3 Potential Mechanisms

What are the mechanisms underlying the effects documented above? We know from Wilson (2022) that child marriage bans affected women's decisions related to marriage timing, fertility timing, education (in urban areas only), and employment. In theory, all of these are possible mechanisms that could have generated the reductions in infant and under-5 mortality that we document.

Table 5: Heterogeneous effects by child gender, urban status, and household wealth

	Infant Mortality			Under-5 Mortality		
Panel A: By child's gender	(1)	(2)	(3)	(4)	(5)	(6)
	Boys	Girls	Difference	Boys	Girls	Difference
post-ban × intensity	-0.016***	-0.009***	0.007	-0.028***	-0.025***	0.003
	(0.005)	(0.002)	(0.006)	(0.009)	(0.004)	(0.009)
Mean outcome of pre-ban cohorts	0.073	0.059		0.11	0.092	
Observations	77,703	71,832		77,703	71,832	
Panel B: By rural-urban status	(1)	(2)	(3)	(4)	(5)	(6)
	Rural	Urban	Difference	Rural	Urban	Difference
post-ban × intensity	-0.011***	-0.017***	-0.006	-0.027***	-0.026***	0.001
	(0.004)	(0.004)	(0.006)	(0.007)	(0.006)	(0.009)
Mean outcome of pre-ban cohorts	0.078	0.051		0.12	0.072	
Observations	85,320	64,215		85,320	64,215	
Panel C: By household wealth	(1)	(2)	(3)	(4)	(5)	(6)
	Rich	Poor	Difference	Rich	Poor	Difference
post-ban × intensity	-0.010**	-0.017***	-0.007	-0.016**	-0.035***	-0.020**
	(0.004)	(0.004)	(0.006)	(0.006)	(0.006)	(0.008)
Mean outcome of pre-ban cohorts	0.056	0.080		0.082	0.12	
Observations	85,169	64,361		85,169	64,361	

Notes: Standard errors clustered at subnational region level are in parentheses. * p< 0.1, ** p< 0.05, *** p< 0.01. *post-ban* is an indicator variable coded as one if individuals are under the age of 18 at the time of the of the ban. All specifications use the weights described in section 3.3 and control for country-cohort fixed effects and subnational region fixed effects.

In Table 6, we use our main regression specification to estimate the effects of child marriage bans on these outcomes in our sample of mothers. We find statistically significant increases in age at first marriage and age at first birth (columns 1 and 2). We do not find any significant effects on educational attainment or current employment (columns 3 and 4).[10] This suggests that, although many studies have documented a causal link from parental socioeconomic status (including maternal education and household income) to child health (Currie, 2009; Almond et al., 2018; Thomas et al., 1991), this does not appear to be an important mechanism in this particular context. This conclusion is also supported by the fact that Wilson (2022) found schooling effects only in urban areas, whereas we found similarly sized mortality effects in both rural and urban areas. In results not reported here but available upon request, we show that changes in husbands' characteristics do not appear to be an important mechanism either: the bans have no statistically significant effects on current husbands' education levels or employment status (for women who are currently married or living with their partner).

We therefore focus our attention on the importance of marriage and fertility timing. Age at marriage and first birth could both be important factors because a woman who goes into a marriage or into her first pregnancy with more maturity may have more agency and bargaining power when it comes to securing health investments for herself during pregnancy and for her child immediately after birth.

There may also be biological reasons why older maternal age may have improved child survival in our sample. In the medical literature, it has been suggested that the body of a teenager is still not fully developed and not optimal for the development of a successful pregnancy, and that lack of psychological maturity from mothers might lead to inadequate antenatal and postnatal health behaviors, which in turn affect child health (Chen et al., 2007; Olausson et al., 1999; UNICEF-WHO, 2022). We show in column 5 that child marriage bans led to a reduction in the share of mothers giving birth under the age of 18, which is of note because several studies in developing countries find that children who are born to mothers under 18 have a higher likelihood of dying than those born to older mothers (Neal et al., 2018). That said, while these studies typically control for basic confounders like education and wealth, it is difficult to isolate the biological effect of young maternal age from the effect of all possible omitted variables.

[10] Wilson (2022) only found significant effects on years of schooling in urban areas, which is also the case in our sample (results available upon request). For employment, our estimated effects are much smaller than in Wilson (2022) because employment effects are smaller in our restricted sample of mothers.

Finally, we also show in column 6 that child marriage bans resulted in a reduction in total fertility (measured at the time of the survey). Because we do not have completed fertility for all mothers in our sample, this reduction could be driven by of one (or more) of the following: a delay in first birth, increased birth spacing, or a reduction in total fertility. If child marriage bans lead women to increase birth spacing or reduce total fertility, this would result in more resources being available for the firstborn child in their first years of life, another potential driver of improvements in child mortality.

Table 6: Potential mechanisms

	(1) Age at first marriage	(2) Age at first birth	(3) Years of schooling	(4) Currently employed	(5) Mother before 18	(6) Total fertility
post-ban × intensity	1.09***	1.03***	0.18	0.016	-0.030***	-0.85***
	(0.094)	(0.071)	(0.16)	(0.011)	(0.0071)	(0.058)
Mean outcome of pre-ban cohorts	19.3	20.5	5.54	0.61	0.26	4.09
Observations	194,406	203,945	203,900	203,727	203,945	203,945

Notes: Standard errors clustered at subnational region level are in parentheses. * $p< 0.1$, ** $p< 0.05$, *** $p< 0.01$. *post-ban* is an indicator variable coded as one if individuals are under the age of 18 at the time of the of the ban. All specifications use the weights described in section 3.3 and control for country-cohort fixed effects and subnational region fixed effects.

4.4 Robustness Checks

Our interpretation of the $postban_{kc} \times intensity_r$ coefficient as the causal effect of child marriage bans relies on the assumption that, conditional on all the included fixed effects, the difference between the pre-ban and post-ban cohorts would have remained the same across the treatment intensity distribution in the absence of the bans. A contemporaneous policy that affected child mortality for pre-ban and post-ban cohorts differently, and for which subnational variation in intensity or exposure was correlated with our treatment intensity variable, could violate this assumption. During the early 2000s, numerous developing countries reformed their education systems by either increasing the number of compulsory years of schooling or making education free. We examine whether our main results could be driven by the effects of education reforms by directly controlling for these reforms in our main specification. We search for all education reforms that have been implemented in our study countries.[11] We use the timing of each reform, the schooling grades affected by each reform, and the year of birth of our respondents to generate an indicator for cohort-based exposure to the reform. If these reforms affected everyone in each cohort in each country equally, this would be captured by our cohort-country fixed effects. If, however, there were subnational spatial variation in the intensity of these reforms that was correlated with our treatment intensity variable, then this could be a source of bias. We therefore interact our education reform exposure indicator with our child marriage ban treatment intensity variable and add this to Equation (2) as a control. The results in Panel A of Appendix Table A4 are very similar to our baseline estimates, suggesting these education reforms were not an important confounder in our main analysis.

In addition to contemporaneous policies, another potential violation could stem from baseline regional characteristics, correlated with our treatment intensity variable, that generate differential trends in child mortality across cohorts. For example, among pre-ban cohorts, areas with high treatment intensity had higher rates of infant and under-5 mortality. This could have resulted in larger improvements in child mortality in these areas due to mean reversion or catch up, even if the bans had not been implemented. To address this potential issue, in panel B of Table A4 we report the results of estimating Equation (2) with additional controls for subnational region-specific characteristics at baseline (infant mortality, under-5 mortality, and women's years of schooling) interacted with the post-ban dummy.[12] Our results remain statistically significant (and are in fact larger in magnitude than our main estimates), alleviating concerns that differential trends based on these baseline characteristics, rather than treatment intensity, were driving our results above.

Because the DHS does not collect information on the respondents' place of residence at age 18, we use their current residence to assign the treatment intensity variable. To investigate the extent to which non-classical measurement error in this variable might be biasing our results, we check whether our results hold for those who have never migrated. Restricting our sample to respondents who have never migrated results in a much smaller sample size, though the bulk of this reduction is due to the fact that this question is not asked in all countries. Despite the large drop in sample size, however, our estimates are robust to this sample restriction (Panel C, Appendix Table A4).

We test the sensitivity of our estimates to various other sample restrictions. Appendix Figure

[11] Details of these reforms are summarized in Appendix Table A3.

[12] We utilize 13 countries that have a DHS survey prior to the child marriage ban. The five countries that do not have data in the pre-ban period are Albania, Ethiopia, Maldives, the Gambia, and Sierra Leon.

A1 plots the coefficients and 95% confidence intervals from various specifications that either impose an upper limit, lower limit, or both an upper and lower limit on age at the time of the bans. Panel A displays results restricting the pre-ban cohorts to those aged 35 and under, 40 and under, and 45 and under at the time of the bans; panel B displays results restricting the post-ban cohorts to those 3 and older, 5 and older, and 7 and older at the time of the bans; and panel C shows results imposing combinations of the two sets of restrictions. In all panels, coefficients are very similar to those of the main results and remain statistically significant at the 5% level.

All of the results presented above use the weighting procedure described in section 3.3, employed to account for differential selection into motherhood (by age at survey) across the treatment intensity distribution. Another way to address this issue is to directly control for age-by-treatment interactions in our main specification. Because age at survey is highly correlated with age at ban, which is one of our main sources of variation, we prefer the weighting method to this approach. However, we show in Table A5 that our results are not sensitive to this choice. Columns 1 and 2 report results that do not use any weights, columns 3 and 4 report unweighted results with the addition of a linear age-by-treatment interaction, and columns 5 and 6 report unweighted results with the addition of treatment interacted with age fixed effects. Across all specifications, we find statistically significant negative coefficients.

5 Conclusion

We examine the impacts of increasing the legal minimum age of marriage to 18 on first-born child mortality in 18 developing countries. Using a sample of over 200,000 women drawn from the DHS, our results show that banning child marriage significantly reduced first-born infant and under-5 mortality. Our results are robust to many specification checks that provide support for the validity of our empirical strategy. We also find that the effects are heterogeneous: low-income countries and less wealthy households benefited most from the bans.

We find that delays in age at marriage and first birth are channels through which raising the minimum age of marriage affected child mortality. Findings from our study highlight the importance of evaluating large-scale policies with respect not only to their primary, targeted outcomes but also to potential downstream effects. Our findings also suggest that banning child marriage could be an important policy instrument for improving the health of the next generation.

References

Akresh, Richard, Daniel Halim, and Marieke Kleemans. 2018. "Long-Term and Intergenerational Effects of Education: Evidence from School Construction in Indonesia." *The Economic Journal* 133 (650): 582–612. 10.3386/w25265.

Almond, Douglas, Janet Currie, and Valentina Duque. 2018. "Childhood Circumstances and Adult Outcomes: Act II." *Journal of Economic Literature* 56 (4): 1360–1446. 10.1257/jel.20171164.

Arthur, Megan, Alison Earle, Amy Raub et al. 2018. "Child Marriage Laws around the World: Minimum Marriage Age, Legal Exceptions, and Gender Disparities." *Journal of Women, Politics & Policy* 39 (1): 51–74. 10.1080/1554477X.2017.1375786.

Asadullah, M Niaz, and Zaki Wahhaj. 2019. "Early Marriage, Social Networks and the Transmission of Norms." *Economica* 86 (344): 801–831. 10.1111/ecca.12291.

Baird, Sarah, Jed Friedman, and Norbert Schady. 2011. "Aggregate Income Shocks and Infant Mortality in the Developing World." *Review of Economics and Statistics* 93 (3): 847–856. 10.1162/REST_a_00084.

Barofsky, Jeremy, Tobenna D Anekwe, and Claire Chase. 2015. "Malaria Eradication and Economic Outcomes in Sub-Saharan Africa: Evidence from Uganda." *Journal of Health Economics* 44 118–136. 10.1016/j.jhealeco.2015.08.002.

Batyra, Ewa, and Luca Maria Pesando. 2021. "Trends in Child Marriage and New Evidence on the Selective Impact of Changes in Age-at-Marriage Laws on Early Marriage." *SSM - Population Health* 14 100811. 10.1016/j.ssmph.2021.100811.

Bellés-Obrero, Cristina, and María Lombardi. 2023. "Will You Marry Me, Later?" *Journal of Human Resources* 58 (1): 221–259. 10.3368/jhr.58.3.1219-10621R2.

Bergstrom, Katy, and Berk Özler. 2023. "Improving the Well-Being of Adolescent Girls in Developing Countries." *The World Bank Research Observer* 38 (2): 179–212. 10.1093/wbro/lkac007.

Bharadwaj, Prashant. 2015. "Impact of changes in marriage law: Implications for fertility and school enrollment." *Journal of Human Resources* 50 (3): 614–654. 10.3368/jhr.50.3.614.

Bhuwania, Pragya, Kate Huh, and Jody Heymann. 2023. "Impact of Tuition-Free Education Policy on Child Marriage and Early Childbearing: Does Secondary Matter More?" *Population and Development Review* 49 (1): 43–70. 10.1111/padr.12538.

Bleakley, H. 2007. "Disease and Development: Evidence from Hookworm Eradication in the American South." *The Quarterly Journal of Economics* 122 (1): 73–117. 10.1162/qjec.121.1.73.

Buchmann, Nina, Erica Field, Rachel Glennerster, Shahana Nazneen, and Xiao Yu Wang. 2023. "A Signal to End Child Marriage: Theory and Experimental Evidence from Bangladesh." *American Economic Review* 113 (10): 2645–2688. 10.1257/aer.20220720.

Callaway, Brantly, Andrew Goodman-Bacon, and Pedro HC Sant'Anna. 2024. "Event-Studies with a Continuous Treatment." 10.3386/w32118, NBER Working Paper No. w32118.

Callaway, Brantly, and Pedro HC Sant'Anna. 2021. "Difference-in-differences with multiple time periods." *Journal of econometrics* 225 (2): 200–230. 10.1016/j.jeconom.2020.12.001.

Chari, A V, Rachel Heath, Annemie Maertens, and Freeha Fatima. 2017. "The Causal Effect of Maternal Age at Marriage on Child Wellbeing: Evidence from India." *Journal of Development Economics* 127 42–55. 10.1016/j.jdeveco.2017.02.002.

Chen, X-K, S W Wen, N Fleming, K Demissie, G G Rhoads, and M Walker. 2007. "Teenage Pregnancy and Adverse Birth Outcomes: A Large Population Based Retrospective Cohort Study." *International Journal of Epidemiology* 36 (2): 368–373. 10.1093/ije/dyl284.

Chou, Shin-Yi, Jin-Tan Liu, Michael Grossman, and Ted Joyce. 2010. "Parental Education and Child Health: Evidence from a Natural Experiment in Taiwan." *American Economic Journal: Applied Economics* 2 (1): 33–61. 10.1257/app.2.1.33.

Chow, Vinci, and Eva Vivalt. 2022. "Challenges in Changing Social Norms: Evidence from Interventions Targeting Child Marriage in Ethiopia." *Journal of African Economies* 31 (3): 183–210. 10.1093/jae/ejab010.

Cohen, Isabelle, Maryam Abubakar, and Daniel Perlman. 2023. "Pathways to Choice: A Bundled Intervention against Child Marriage." 10.26085/C31C71, CEGA Working Paper Series, No. WPS-230.

Collin, Matthew, and Theodore Talbot. 2023. "Are Age-of-Marriage Laws Enforced? Evidence from Developing Countries." *Journal of Development Economics* 160 102950. 10.1016/j.jdeveco.2022.102950.

Cornelissen, Thomas, and Thang Dang. 2022. "The Multigenerational Impacts of Educational Expansion: Evidence from Vietnam." *Labour Economics* 78 102243. 10.1016/j.labeco.2022.102243.

Currie, Janet. 2009. "Healthy, Wealthy, and Wise: Socioeconomic Status, Poor Health in Childhood, and Human Capital Development." *Journal of Economic Literature* 47 (1): 87–122. 10.1257/jel.47.1.87.

Cutler, David, Winnie Fung, Michael Kremer, Monica Singhal, and Tom Vogl. 2010. "Early-Life Malaria Exposure and Adult Outcomes: Evidence from Malaria Eradication in India." *American Economic Journal: Applied Economics* 2 (2): 72–94. 10.1257/app.2.2.72.

Dahl, Gordon B. 2010. "Early teen marriage and future poverty." *Demography* 47 (3): 689–718. 10.1353/dem.0.0120.

De Neve, Jan-Walter, and Günther Fink. 2018. "Children's Education and Parental Old Age Survival – Quasi-Experimental Evidence on the Intergenerational Effects of Human Capital Investment." *Journal of Health Economics* 58 76–89. 10.1016/j.jhealeco.2018.01.008.

Dursun, Bahadir, Resul Cesur, and Inas Rashad Kelly. 2017. "The Value of Mandating Maternal Education in a Developing Country." 10.3386/w23492, NBER Working Paper, No. w23492.

Field, Erica, and Attila Ambrus. 2008. "Early Marriage, Age of Menarche, and Female Schooling Attainment in Bangladesh." *Journal of Political Economy* 116 (5): 881–930. 10.1086/593333.

Garcia-Hombrados, Jorge. 2022. "Child Marriage and Infant Mortality: Causal Evidence from Ethiopia." *Journal of Population Economics* 35 (3): 1163–1223. 10.1007/s00148-021-00873-y.

Grépin, Karen A, and Prashant Bharadwaj. 2015. "Maternal Education and Child Mortality in Zimbabwe." *Journal of Health Economics* 44 97–117. 10.1016/j.jhealeco.2015.08.003.

Huang, Jian, Wim Groot, John G Sessions, and Yinyen Tseng. 2019. "Age of Menarche, Adolescent Sexual Intercourse and Schooling Attainment of Women." *Oxford Bulletin of Economics and Statistics* 81 (4): 717–743. 10.1111/obes.12284.

ICF. 2018. "Demographic and Health Surveys." `https://dhsprogram.com/`, [accessed 01 December 2022].

Kammerlander, Andreas, and Günther G Schulze. 2023. "Local Economic Growth and Infant Mortality." *Journal of Health Economics* 87 102699. 10.1016/j.jhealeco.2022.102699.

Keats, Anthony. 2018. "Women's Schooling, Fertility, and Child Health Outcomes: Evidence from Uganda's Free Primary Education Program." *Journal of Development Economics* 135 142–159. 10.1016/j.jdeveco.2018.07.002.

Kuecken, Maria, Josselin Thuilliez, and Marie-Anne Valfort. 2021. "Disease and Human Capital Accumulation: Evidence from the Roll Back Malaria Partnership in Africa." *The Economic Journal* 131 (637): 2171–2202. 10.1093/ej/ueaa134.

Lu, Frances, and Tom Vogl. 2023. "Intergenerational Persistence in Child Mortality." *American Economic Review: Insights* 5 (1): 93–109. 10.1257/aeri.20210604.

Lucas, Adrienne M. 2010. "Malaria Eradication and Educational Attainment: Evidence from Paraguay and Sri Lanka." *American Economic Journal: Applied Economics* 2 (2): 46–71. 10.1257/app.2.2.46.

McGavock, Tamara. 2021. "Here Waits the Bride? The Effect of Ethiopia's Child Marriage Law." *Journal of Development Economics* 149 102580. 10.1016/j.jdeveco.2020.102580.

Neal, Sarah, Andrew Amos Channon, and Jesman Chintsanya. 2018. "The Impact of Young Maternal Age at Birth on Neonatal Mortality: Evidence from 45 Low and Middle Income Countries." *PLOS ONE* 13 (5): e0195731. 10.1371/journal.pone.0195731.

Olausson, Petra Otterblad, Sven Cnattingius, and Bengt Haglund. 1999. "Teenage Pregnancies and Risk of Late Fetal Death and Infant Mortality." *BJOG: An International Journal of Obstetrics and Gynaecology* 106 (2): 116–121. 10.1111/j.1471-0528.1999.tb08210.x.

Rokicki, Slawa. 2021. "Impact of Family Law Reform on Adolescent Reproductive Health in Ethiopia: A Quasi-Experimental Study." *World Development* 144 105484. 10.1016/j.worlddev.2021.105484.

Roth, Jonathan, Pedro HC Sant'Anna, Alyssa Bilinski, and John Poe. 2023. "What's trending in difference-in-differences? A synthesis of the recent econometrics literature." *Journal of Econometrics* 235 (2): 2218–2244. 10.1016/j.jeconom.2023.03.008.

Sekhri, Sheetal, and Sisir Debnath. 2014. "Intergenerational Consequences of Early Age Marriages of Girls: Effect on Children's Human Capital." *Journal of Development Studies* 50 (12): 1351–1368. 10.1080/00220388.2014.915657.

Sunder, Naveen. 2019. "Marriage Age, Social Status, and Intergenerational Effects in Uganda." *Demography* 56 (6): 2123–2146. 10.1007/s13524-019-00829-8.

Thomas, Duncan, John Strauss, and Maria-Helena Henriques. 1991. "How does mother's education affect child height?" *Journal of human resources* 26 (2): 183–211. 10.2307/145920.

UNFPA-UNICEF. 2019. "Investing in Knowledge for Ending Child Marriage: Publications Catalogue 2018-2019." `https://www.unicef.org/bulgaria/media/6111/file/BGR-Investing-in-knowledge-for-ending-child-marriage.pdf`, [accessed 10 March 2023].

UNICEF. 2017. "Ending Child Marriage - Progress and prospects." June, `https://www.refworld.org/reference/themreport/unicef/2017/en/117022`, [accessed 16 April 2024].

UNICEF-WHO. 2022. "Early Childbearing." `https://data.unicef.org/topic/child-health/adolescent-health/#:~:text=Obstetric%20fistula%2C%20eclampsia%2C%20puerperal%20endometritis,among%20girls%20aged%2015%2D19.`, [accessed 9 February 2023].

United Nations. 1995. "Beijing Declaration and Platform for Action. 16th Plenary Meeting." `https://www.un.org/womenwatch/daw/beijing/pdf/BDPfA%20E.pdf`, [accessed 8 March 2023].

United Nations Inter-agency Group for Child Mortality Estimation. 2018. "Levels & Trends in Child Mortality: Report 2018, Estimates Developed by the United Nations

Inter-Agency Group for Child Mortality Estimation." https://www.unicef.org/reports/levels-and-trends-child-mortality-report-2018, [accessed 8 March 2023].

Walker, Michael W, Alice H Huang, Suleiman Asman et al. 2023. "Intergenerational child mortality impacts of deworming: Experimental evidence from two decades of the Kenya Life Panel Survey." 10.3386/w31162, NBER Working Paper, No. w31162.

Wang, Chunbei, and Le Wang. 2017. "Knot yet: minimum marriage age law, marriage delay, and earnings." *Journal of Population Economics* 30 771–804. 10.1007/s00148-017-0632-5.

Wilson, Nicholas. 2022. "Child Marriage Bans and Female Schooling and Labor Market Outcomes: Evidence from Natural Experiments in 17 Low- and Middle-Income Countries." *American Economic Journal: Economic Policy* 14 (3): 449–477. 10.1257/pol.20200008.

Wodon, Quentin, Chata Male, Ada Nayihouba, Adenike Onagoruwa, Aboudrahyme Savadogo, Ali Yedan, and Jeff Edmeades. 2017. "Economic Impacts of Child Marriage: Global Synthesis Report." https://documents1.worldbank.org/curated/en/530891498511398503/pdf/116829-WP-P151842-PUBLIC-EICM-Global-Conference-Edition-June-27.pdf, [accessed 20 February 2023].

World Policy Center. 2022. "Marriage." https://www.worldpolicycenter.org/topics/childmarriage/policies, [accessed 01 December 2022].

A Appendix

Table A1: Child marriage bans and DHS survey years

Country	Ban year	Survey year	Years between ban and survey
Albania	2003	2017-18	14-15
Benin	2004	2017-18	13-14
D.R.C.	2009	2013-14	4-5
Egypt	2008	2014	6
Ethiopia	2000	2016	16
Guinea	2008	2018	10
Jordan	2001	2017-18	16-17
Kazakhstan	1998	1999	1
Liberia	2012	2014	2
Madagascar	2007	2008-09	1-2
Maldives	2001	2016-17	15-16
Namibia	1996	2013	17
Nepal	2003	2016	13
Nigeria	2003	2018	15
Peru	1999	2000	1
Sierra Leone	2007	2013	6
The Gambia	2005	2013-14	8-9
Togo	2007	2013	6

Data sources: The World Policy Analysis Center and the Demographic and Health Surveys. D.R.C denotes Democratic Republic of the Congo.

Table A2: Summary Statistics for Potential Mechanisms

	(1) Unweighted	(2) Weighted
Years of schooling	5.74	5.70
	(5.31)	(5.30)
	203,900	203,900
Currently employed	0.58	0.58
	(0.49)	(0.49)
	203,727	203,727
Age at first marriage	18.9	18.8
	(4.50)	(4.46)
	194,407	194,407
Age at first birth	20.1	20.0
	(4.22)	(4.15)
	203,945	203,945
Mother before 18	0.29	0.29
	(0.45)	(0.45)
	203,945	203,945
Total fertility	3.59	3.68
	(2.30)	(2.37)
	203,945	203,945

Notes: Means, standard deviations (in parentheses), and observation counts are reported. Sample restricts to women who have ever given birth and who were born after the child marriage ban was implemented in their country. Column 2 weights the sample using the methods described in section 3.3.

Table A3: Education reforms in study countries

Country	Type of reforms	Year of reforms	Primary/ secondary school starting age	Cohorts exposed to reforms
Albania	Increase compulsory school from 8 to 9	2012	6/11	1998
D.R.C.	Free primary and secondary schools	2015	6/12	2009
Egypt	Increase compulsory school from 9 to 12	2015	6/12	2002
Liberia	Free primary and secondary schools	2003/2011	6/12	1997/1999
Nigeria	Free primary and secondary schools	2004	6/12	1998
Peru	Increase compulsory school from 6 to 8	2012	6/13	2000
Sierra Leone	Free primary and secondary schools	2000/2004	6/12	1994
The Gambia	Free primary and secondary schools	2004	7/13	1997

Notes: Education reform information is taken from previous literature (Bhuwania et al., 2023; Wilson, 2022). D.R.C denotes Democratic Republic of the Congo.

Table A4: Robustness tests: Education reforms, region-specific trends, and migration

	(1) Infant Mortality	(2) Under-5 Mortality
Panel A: controlling for education reforms		
post-ban × intensity	-0.017***	-0.026***
	(0.003)	(0.004)
Mean outcome of pre-ban cohorts	0.066	0.099
Observations	203,945	203,945

Panel B: allowing for differential trends by baseline characteristics

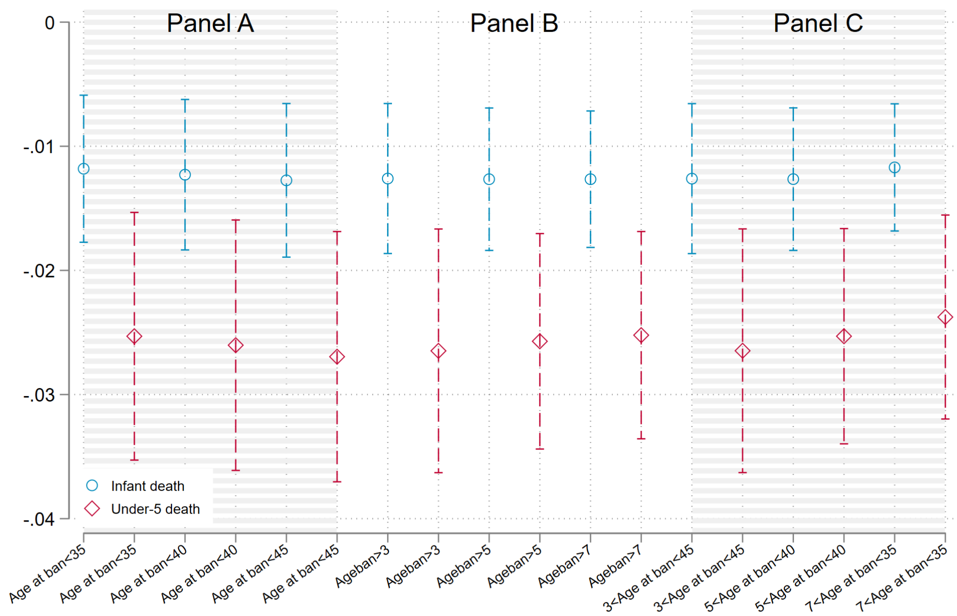
	(1) Infant Mortality	(2) Under-5 Mortality
post-ban × intensity	-0.019***	-0.032***
	(0.004)	(0.005)
Mean outcome of pre-ban cohorts	0.063	0.095
Observations	100,269	100,269

Panel C: restricting to non-movers

	(1) Infant Mortality	(2) Under-5 Mortality
post-ban × intensity	-0.015***	-0.029***
	(0.005)	(0.006)
Mean outcome of pre-ban cohorts	0.061	0.093
Observations	67,963	67,963

Notes: Standard errors clustered at subnational region level are in parentheses. * p< 0.1, ** p< 0.05, *** p< 0.01. *post-ban* is an indicator variable coded as one if individuals are under the age of 18 at the time of the of the ban. All specifications use the weights described in section 3.3 and control for country-cohort fixed effects and subnational region fixed effects. Panel A additionally controls for an interaction between an education reform exposure indicator and the treatment intensity variable. Using only 13 countries that have data available in the pre-ban period, Panel B additionally controls for the post-ban dummy interacted with subnational region-specific characteristics at baseline (infant mortality, under-5 mortality, and years of schooling). Panel C restricts the analysis sample to those who are still living in their place of birth (in countries where migration information was recorded).

Figure A1: Coefficient estimates for alternative samples



Notes: Standard errors clustered at region level. Figure reports coefficients (and 95% confidence intervals) on the postban-intensity interaction term in Equation (2), using the weighting procedure described in section 3.3, for various alternative samples based on age at the time of the ban.

Table A5: Effects of child marriage bans on first-born child mortality: unweighted estimates

	(1) Infant Mortality	(2) Under-5 Mortality	(3) Infant Mortality	(4) Under-5 Mortality	(5) Infant Mortality	(6) Under-5 Mortality
post-ban × intensity	-0.012***	-0.026***	-0.008**	-0.010**	-0.010***	-0.014***
	(0.003)	(0.005)	(0.003)	(0.004)	(0.004)	(0.005)
Added Survey Age Controls	None	None	Linear Age × Treatment	Linear Age × Treatment	Age Dummies × Treatment	Age Dummies × Treatment
Mean outcome of pre-ban cohorts	0.066	0.099	0.066	0.099	0.066	0.099
Observations	203,945	203,945	203,945	203,945	203,945	203,945

Notes: Standard errors clustered at subnational region level are in parentheses. * p< 0.1, ** p< 0.05, *** p< 0.01. *post-ban* is an indicator variable coded as one if individuals are under the age of 18 at the time of the of the ban. All specifications are unweighted and control for country-cohort fixed effects and subnational region fixed effects.

B Alternate Event Study Method

Our empirical approach can be framed as the estimation of the effect of a treatment (our intensity variable) with "staggered adoption" across birth cohorts (our time dimension). Staggered adoption here refers to the fact that the intensity variable only becomes relevant for cohorts born after a certain year, which depends on when a child marriage ban was implemented in a specific country. Callaway and Sant'Anna (2021) propose methods for estimating the dynamic causal effects of a treatment with staggered adoption across multiple periods. These methods are not plagued with the same issues that make it difficult to interpret two-way fixed effects event study regressions (like the one in Equation (3)) when there are heterogeneous treatment effects across groups treated at different times. Because the methods in Callaway and Sant'Anna (2021) were designed for a binary treatment, we use extensions proposed by Callaway et al. (2024) to apply their methods to our continuous treatment setting.

This approach essentially boils down to the use of Callaway and Sant'Anna (2021) methods for multiple different binary treatment variables. We estimate two sets of event study treatment effects – one for a "low intensity" treated group and one for a "high intensity" treated group, both of which involve using the same "never-treated" group as a control. The estimation of the low-intensity treatment effects do not use any high intensity observations, and the estimation of the high-intensity treatment effects ignores all low intensity observations.

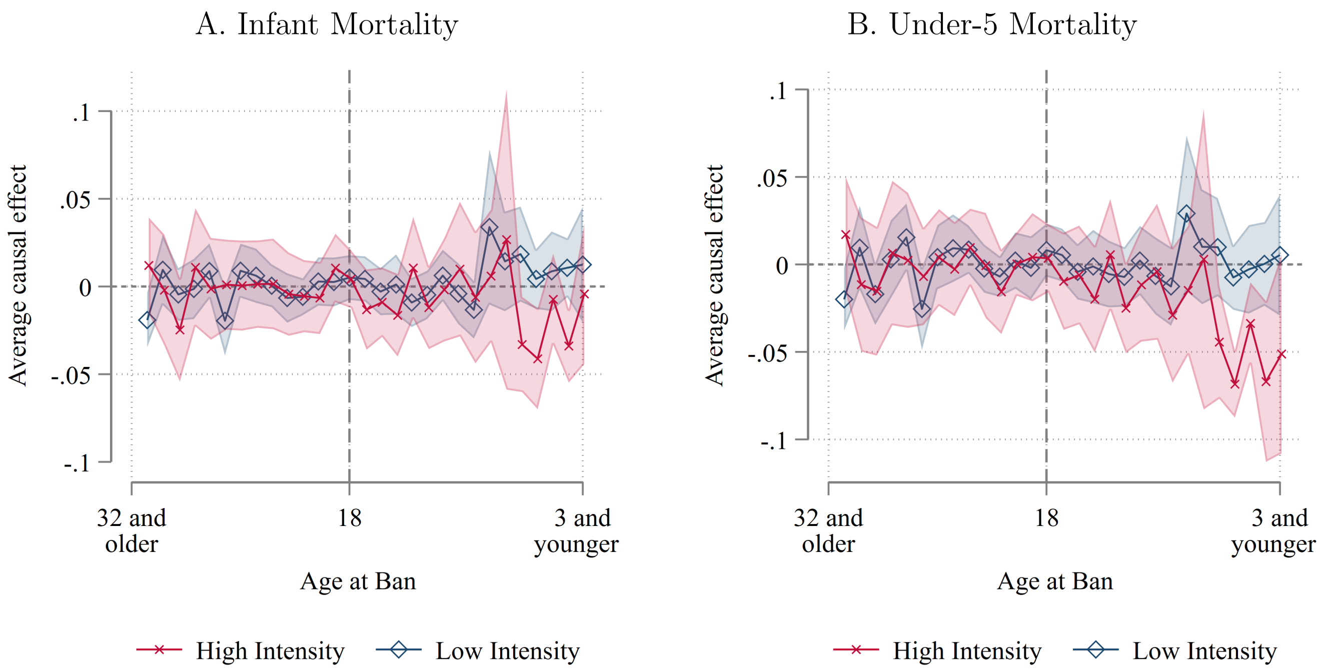
Strictly speaking, we do not have any never-treated regions in our sample because we restrict to countries that implemented a child marriage ban. However, regions where child marriage was extremely uncommon before the ban can be thought of as essentially untreated. We therefore define those with treatment intensity below 0.23 – the 5th percentile of the region-level treatment intensity distribution – as never-treated.[13] To define low and high intensity, we split the remaining regions at the 75th percentile (1.8). For computational efficiency, we collapse our data to the region-cohort level, averaging the outcome variables and summing the weights, which we apply to this analysis as well.

Results are shown in Appendix Table B1. There are several points to note. First, across both outcome variables, the low intensity coefficients are close to zero and statistically insignificant

[13] A region where 23% of women get married at age 17 would generate an intensity variable of 0.23 (though there are of course many other scenarios that could result in this intensity level). A child marriage ban is unlikely to have much of an effect in a region like this, where the vast majority of women are marrying as adults and the women who do marry as children are close to 18.

across both pre-ban and post-ban cohorts, suggesting the ban had little effect in these low intensity regions. Second, the high intensity coefficients for pre-ban cohorts are also close to zero, implying no evidence of the high intensity regions exhibiting different cohort trends among those unaffected by the ban. Finally, for under-5 mortality (and to a lesser extent for infant mortality), the post-ban coefficients for high intensity regions are mostly negative and grow in magnitude, becoming statistically significant for the youngest (most affected) cohorts.

Figure B1: Callaway et al. (2024) Results



Notes: Results generated from region-cohort-level data. Figure reports estimates (and 95% confidence intervals) of the average treatment effect for each age at ban, using Callaway and Sant'Anna (2021) methods. We treat regions under the 5th percentile of the treatment distribution as never-treated. Among the remaining treated regions, low intensity regions are those with treatment under the 75th percentile, and high intensity regions are those with treatment greater than or equal to the 75th percentile.