The Intergenerational Health Effects of Child Marriage Bans

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Abstract

Using a sample of over 200,000 women across 18 countries, we investigate the effects of child marriage bans on infant and under-5 mortality rates of the next generation. We exploit variation in exposure to the ban across cohorts within each country, as well as subnational regional variation in "treatment intensity," which we define based on the prevalence of and average age among child marriages prior to the ban. We find that child marriage bans reduced infant and under-5 mortality, with magnitudes of 7.6 and 7.3 percent corresponding to a one SD change in treatment intensity, respectively. Reductions were driven by low-income countries, less wealthy households, and boys. Increases in age at first marriage and first birth, which may have led to better bargaining power and agency for these mothers during the prenatal and postnatal period, appear to be the main drivers of the mortality reductions documented.

Keywords: child marriage bans, early marriage, child mortality, developing countries, Demographic and Health Surveys.

JEL Classification: I10, I15, J12, J18

I. 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. 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 form 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 this small 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 (World Policy Center 2023). 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.[6] 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 0.50 and 0.72 percentage points, respectively (approximately 7.6 and 7.3 percent relative to the mean of control sample). Our estimates are robust to various specification tests, including those that account for contemporaneous policies and differential trends due to regional characteristics. We conduct an event study analysis, which shows that the relationship between treatment intensity and child mortality only starts changing for cohorts aged younger than 18 at the time of the ban, which 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 with limited financial resources. Consistent with hypotheses about male survival being more sensitive to improvements in the fetal health environment, our estimates indicate that the effects were completely driven by boys. We find little differences between rural and urban areas in the effects of the bans.

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

We show that 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 (Wang and Wang 2017; Dahl 2010; Bharadwaj 2015; Collin and Talbot 2023; McGavock 2021; Wilson 2022; Bellés-Obrero and Lombardi 2023) – 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, Cesur, and Kelly (2017).[7] 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; Sunder 2019;

[7] Low rates of infant, under-5 and child mortality in each country preclude us from reliably estimating country-specific effects of the bans rather than the overall and pooled effects. It has been suggested that in the case of rare events (e.g., child mortality) where the mean value is small (usually at 5-7% of the total sample), estimates from a small sample size could result in bias. Specifically, Dursun, Cesur, and Kelly (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.

Asadullah and Wahhaj 2019;Chari et al. 2017; Sunder 2019; Asadullah and Wahhaj 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.[8] 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.

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 Ozler (2022) 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, Abubakar, and Perlman 2023; Chow and Vivalt 2022; Buchmann et al. 2021). The results of our study suggest that the positive effects of these interventions could extend to the children of the affected women.

Previous economic literature in developing countries has shown that education reforms and conditional cash transfers 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, Halim, and Kleemans 2018; Cornelissen and Dang 2022; De Neve and Fink 2018; Baird et al. 2010; Behrman and Hoddinott 2001; Glewwe and Kassouf 2012; Brauw and Peterman 2020). Our study contributes to this large body of literature by revealing child marriage bans as

[8] 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.

yet another non-health-related government policy that has the potential to improve the health of the next generation.

II. 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).

A. 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.[9] 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 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% (UNFPA-UNICEF 2019).

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

B. The MACHEquity Child Marriage Policy Database

We use this country-level dataset 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 (MACHEquity 2014). 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.

C. Demographic and Health Survey (DHS) data

[Table 1 about here]

We extract child mortality and other outcomes from the DHS, a set of health-focused household surveys administered to women of childbearing age. 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 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, Thuilliez, and Valfort 2021; Chou et al. 2010; Lu and Vogl 2023; Kammerlander and Schulze 2023;

Baird, Friedman, and Schady 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. Given the retrospective nature of the DHS, this sample restriction may also help mitigate recall bias concerns if details about the first birth are more salient than subsequent births. Approximately 9.3% and 6.3% of women have experienced the death of their first-born child under age 5 and age 1, respectively.

To explore potential mechanisms, we also utilize information on our sample mothers' educational attainment, employment, age at first marriage, age at first birth, total fertility, early motherhood, and use of contraceptives. For mothers who are married or cohabiting with their partner, we also use information about their partner's age. As we report in Appendix Table A5, 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. On average, our sample respondents are 7.3 years younger than their partners. At the time of survey, 34% of the sample report using any form of contraception and have an average of 3.6 children – a high number given that the respondents themselves are 32.9 years old on average.

III. Empirical strategy

A. Generalized Difference-in-Differences (DID) specification

To estimate the causal effect of child marriage bans (the "treatment" of interest) on child mortality, we rely on two sources of variation: (1) country-level variation in the timing of the bans, which generates within-country variation in exposure to the bans across cohorts, and (2) within-country variation in treatment intensity across regions. A region is defined using two variables: the largest geographic administrative region in the country, along with the urban-rural status of the exact location. For example, urban areas of state A will form one region, while rural areas of state A will form a separate region.

Following Wilson (2022), we define treatment intensity in subnational region r in country c, focusing on "pre-ban" cohorts aged 18 to 30 at the time of the ban:[10]

$$intensity_{rc} = \frac{\sum_{i=1}^{N_{rc}} \mathbf{1}(married\ age{<}18)_{irc} \times (18 - age_first_marriage)_{irc}}{N_{rc}}. \qquad (1)$$

The denominator (N_{rc}) is the number of women in region *r* and country *c* 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_{rc} women in pre-ban cohorts in region *r* and country *c*) 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 getting married 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 (Table 1).

[10] 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 about here]

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

$$y_{ikcr} = \alpha_0 + \alpha_1(intensity_{rc} \times postban_{kc}) + \delta(intensity_{rc} \times age_{kc}) + \gamma_{kc} + \theta_{rc} + \epsilon_{ikcr}, \quad (2)$$

where y_{ikcr} represents the outcome of interest (firstborn mortality indicators or a related potential mechanism) for mother *i* in cohort *k* in country *c* and subnational region *r*. $postban_{kc}$ 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_1, the coefficient on the interaction between $intensity_{rc}$ and $postban_{kc}$, which captures how the gap 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_{rc}. Country-specific cohort effects (γ_{kc}) account for factors like age at the time of survey and country-specific differences in marriage rates across cohorts. Subnational region fixed effects (θ_{rc}) control for time-invariant unobserved characteristics, including geographic and cultural factors. In all estimations, we cluster standard errors at the country-region level to account for potential correlation within a subnational region.[11]

One important distinction between this specification and that of Wilson (2022) is the inclusion of the interaction between treatment intensity and age at the time of survey ($intensity_{rc} \times age_{kc}$) as a control. Although our country-cohort fixed effects control for age at the time of survey (in the same way across all regions), $intensity_{rc} \times age_{kc}$ allows for trends in the outcomes of interest by age at the time of survey to vary across the treatment intensity

[11] In total, our study sample consists of 339 country-region combinations.

distribution. As we discuss in more detail in section A1, trends in marriage rates by age at the time of survey could vary across the treatment intensity distribution because women in high intensity areas typically marry early, and this could also lead to differential trends in our outcomes of interest. Controlling for $intensity_{rc} \times age_{kc}$, which we are only able to do because we have data from multiple countries that were surveyed in different years, ensures that α_1, our parameter of interest, isolates the causal effect of the ban from any differential trends in age at survey across the intensity distribution. In column 1 of Appendix Table A4, we show that our interaction variable of interest ($intensity_{rc} \times postban_{kc}$) does not predict selection into our sample (when we control for the $intensity_{rc} \times age_{kc}$ interaction term), alleviating concerns that child marriage bans might be changing the composition of our sample.

The validity of our generalized DID specification rests on the assumption that, conditional on time-invariant subnational region fixed effects, country-specific cohort fixed effects, and the $intensity_{rc} \times age_{kc}$ interaction term, 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.

B. Event study style regressions

One way to shed light on this assumption is to estimate regressions with an event study structure. This 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 $postban$ indicator with a set of dummies representing the respondent's age at the time of the ban, as described in the following specification:

$$y_{ikcr} = \sum_{t=1-}^{35+} \alpha_t \left(\mathbf{1}(ageban_{kc} = t) \times intensity_{rc}\right) + \sum_{t=1-}^{35+} \beta_t \, \mathbf{1}(ageban_{kc} = t) \quad (3)$$
$$+\delta(intensity_{rc} \times age_{kc}) + \gamma_{kc} + \theta_{rc} + \epsilon_{ikcr}.$$

Here, $\mathbf{1}(ageban_{kc} = t)$ (ranging from t=1 to 35 years old[12]) refers to a series of dummies indicating the respondent's age at the time of the bans. $\mathbf{1}(ageban_{kc} = 18)$ is omitted as the reference cohort. The set of α_t coefficients are the main coefficients of interest. Since females aged 18 and older at the time of the bans should not be affected by the bans, 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.

IV. Results

A. Main results

[Table 2 about here]

Table 2 presents the main results estimated from Equation (2). 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 0.7 and 1 percentage point, respectively. In terms of SD, a one SD (i.e., 0.72 unit) increase in the intensity of the bans corresponds to reductions in infant and under-5 of approximately 0.50 and 0.72 percentage points. These effect sizes are equivalent to about 7.6% and 7.3% reductions in infant and under-5 mortality relative to the mean of the unaffected pre-ban group.

[12] We combine those aged 0 and 1 at the time of ban, and combine all cohorts aged 35 and older at the time of ban.

[Figure 2 about here]

One way to interpret the coefficients in Table 2 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, across both mortality indicators, the figure reveals a relatively flat pattern of statistically insignificant estimates (with only one exception in each figure) for cohorts aged over 18 who should not have been affected by the bans. 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 2) is indeed due to the child marriage bans.

As for the ban-affected cohorts, a similar pattern emerges for both mortality indicators. With only one exception in each figure, the vast majority of coefficients for post-ban cohorts is negative. Although the bans do not appear to have immediately large impacts on child mortality (e.g., cohorts aged 16 and 17 at ban), 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. All in all, these results provide support for our interpretation of the regression results in Table 2: child marriage bans led to a statistically significant reduction in both infant and under-5 mortality.

B. Heterogeneous Effects

[Table 3 about here]

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.[13] We find that child marriage bans reduced infant and under-5 mortality primarily in low-income countries. Coefficient estimates for lower-middle and middle-income countries are statistically insignificant and, in most cases, significantly different from estimates in low-income countries[14]. Low-income countries, where child marriage, child mortality, and poverty rates are higher, (UNFPA-UNICEF 2019; Wodon et al. 2017), benefit more from the bans.

[Table 4 about here]

[13] https://datahelpdesk.worldbank.org/knowledgebase/articles/906519

[14] To test whether coefficients of child mortality indicators significantly differ by country income levels, we interact country income levels variable with all covariates in Equation 2.

Table 4[15] presents heterogeneity by individual characteristics. First, we document that the bans had significantly larger effects on boys than girls. In fact, our pooled results in Table 2 appear to be completely driven by effects on boys: the coefficient estimates for girls are positive, small in magnitude, statistically insignificant, and significantly different from the coefficient estimates for boys. Both evolutionary theory and biological evidence point to the higher survival rates of female fetuses and infants, particularly when fetal health conditions are poor (Trivers and Willard 1973; Stevenson 2000). One explanation for this heterogeneity, therefore, is that child marriage bans led to improvements in the *in utero* health environment, which differentially benefited the more vulnerable male fetuses.

We also examine the effects by urban-rural status and household wealth. 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 have been concentrated in urban areas. This suggests that schooling may not be an important mechanism for these child mortality effects, an issue we explore in more detail in the next sub-section. Differences were more pronounced across household wealth.[16] For both infant and under-5 mortality, the effects of child marriage bans were only statistically significant in poor households. These results suggest that raising the minimum age at marriage might be more beneficial for those with limited financial resources.

C. *Potential Mechanisms*

[Table 5 about here]

[15] Similar to Table 3, we add interaction terms of child gender, urban indicator, and poor indicator with all covariates in Equation 2 to examine whether coefficients of child mortality indicators differ by these variables.
[16] We use household wealth to define rich and poor groups in which the rich group is defined as individuals living in household with wealth being greater than the median of household wealth. The poor group is defined oppositely.

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. In Table 5, we use our main specification to estimate the effects of child marriage bans on these outcomes in our sample of mothers. Like Wilson (2022), we find statistically significant increases in age at first marriage and age at first birth (columns 1 and 2). In contrast with Wilson (2022), however, we do not find any significant effects on educational attainment or current employment (columns 3 and 4).[17] 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, Currie, and Duque 2018, Thomas, Strauss, and Henriques 1991), this does not appear to be an important mechanism in this particular context.

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. In fact, when we examine the age gap between our respondents and their partners (among those who are currently married or living with their partner), we find that child marriage bans lead to a smaller age gap (column 5), which could be an indication of better bargaining power. Relatedly, we also find that child marriage bans increase the likelihood of a

[17] Wilson (2022) only found significant effects on years of schooling in urban areas, though his estimate for the full sample was much larger in magnitude (0.269) than our estimate (0.004). When we estimate these regressions separately for urban and rural areas, we do not find significant effects on education or employment in any group (results available upon request). There are two reasons for these differences: effects are smaller for our restricted sample of mothers, and coefficients are smaller due to our inclusion of the age at survey interacted with treatment intensity control variable.

woman using contraception at the time of survey (column 6), another possible indication of increased bargaining power. This result also provides evidence of greater use of preventative healthcare (albeit not during the relevant period of pregnancy and childbirth; we unfortunately only have information about prenatal care utilization for a small subset of recent births in the DHS).

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, Cnattingius, and Haglund 1999; UNICEF and World Health Organization 2022). We show in column 7 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, Channon, and Chintsanya 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.

Finally, we also show in column 8 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.[18]

D. Robustness checks

Our interpretation of the $intensity_{rc} \times postban_{kc}$ 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.

HIV/AIDS campaigns are one concern because these campaigns were expanded in the early 2000s in many developing countries and may have contributed to reductions in child mortality. We utilize information on HIV knowledge in the DHS to test whether these campaigns may be biasing our estimates of the causal effect of child marriage bans. Specifically, we construct z-scores for the general knowledge of the mothers in our sample on HIV/AIDS and maternal-to-child transmission. Given that we found no effects of child marriage bans on years of schooling, we would expect no effect of these bans on HIV knowledge. However, if the intensity and timing of HIV campaigns were correlated with the intensity and timing of child marriage bans, we would expect to detect an increase in HIV knowledge associated with our $intensity_{rc} \times postban_{kc}$ interaction term. Therefore, as a falsification test, we replace child mortality outcomes in Equation (2) with z-scores for HIV knowledge. We find no significant relationship between our interaction

[18] Infant mortality is less likely to be affected by this channel because younger siblings would be born (at the earliest) toward the end of the firstborn's first year, the cutoff for defining infant mortality.

term of interest and HIV knowledge (Appendix Table A2), suggesting the related HIV/AIDS campaigns are unlikely to be biasing our main results.

Another potential type of contemporaneous policy is education reform. 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.[19] 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 A3 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, even if the bans had not been implemented. Similarly, treatment intensity might be correlated with ethnic

[19] Details of those reforms are summarized in Appendix Table A6.

composition, and differential cohort trends based on ethnicity could have occurred for reasons other than the bans. To address this potential issue, in panel B of Table A3 we report the results of estimating Equation (2) with additional controls for subnational region-specific characteristics at baseline interacted with the post-ban dummy.[20] 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 A3).

Finally, we test the sensitivity of our estimates to various other sample restrictions. Appendix Figure A1 plots the coefficients along with 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

[20] We utilize 13 countries that have data available in the pre-ban period to average child mortality indicators, ethnicity, religion, age at marriage, and years of schooling at subnational regional level. We then interact these averages with the indicator of exposed cohorts and using those interaction terms as additional covariates in our regressions. This strategy also accounts for mean reversion issues. The five countries that do not have data in the pre-ban period are Albania, Ethiopia, Maldives, the Gambia, and Sierra Leon.

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 10% level.

V. Conclusions

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 generalized DID estimations 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, less wealthy individuals, and boys 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 their secondary effects. Our findings also suggest that banning child marriage could be an important policy instrument for improving the health of the next generation.

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Appendix

A1. Selection into Sample

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 child marriage bans affect the probability of being included in the analysis sample. If child marriage bans affect the extensive margin of the fertility decision and therefore the composition of our sample, this would make it difficult to separate the effect of the bans on child mortality from sample composition effects. To investigate this possibility, we now 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 women in our main analysis sample). We then estimate Equation (2) for this larger sample of women, using this indicator as our outcome variable, and report our results in column 1 of Table A4. The statistically insignificant coefficient of 0.002 indicates that child marriage bans do not affect selection into our sample. While the bans might be affecting the timing of first births and number of births at the time of survey (as shown in Table 5), they do not appear to be affecting the extensive margin decision about whether to have children at all.

In the next two columns, we illustrate the importance of the $intensity_{rc} \times age_{kc}$ control. This variable helps account for differential cohort trends in marriage and motherhood (and therefore selection into the sample) across the treatment intensity distribution. These potentially problematic differential trends stem from the following facts. First, women who are older at the time of survey are more likely to have ever been married (i.e., there is a positive relationship between age at survey and marriage rates). Second, this relationship between age at survey and marriage rates may vary systematically across the intensity distribution because intensity is defined by marriage age distributions among pre-ban cohorts, which may have some persistent

determinants that also affect post-ban cohorts. Specifically, in high intensity areas where women typically marry early, younger women are more likely to be married than in low intensity areas. Because marriage rates cannot exceed 100% (and seem to reach a maximum of around 90% at age 40), the increase in marriage rates with age at survey is less steep in high intensity areas compared to low intensity areas. Finally, age at the time of survey is correlated with (but not completely collinear with) age at the time of ban, which is one of the two key sources of variation in our analysis. This generates a positive and significant coefficient when we regress an indicator for being ever married on a specification similar to Equation (2) that drops the $intensity_{rc} \times age_{kc}$ interaction term. The positive coefficient in column 2 is due to the fact that the (negative) gap between affected (younger post-ban) cohorts and unaffected (older pre-ban cohorts) is significantly smaller (i.e., less negative) in high intensity areas.

Because marriage and motherhood are highly correlated, repeating this simplified specification on an indicator for having ever given birth yields a similar result – a positive and significant coefficient, as shown in column 3. The difference between columns 1 and 3 suggest that the $intensity_{rc} \times age_{kc}$ interaction term is crucial for properly accounting for differential trends that affect selection into our sample.

The Intergenerational Health Effects of Child Marriage Bans

Tables and Figures

Main Tables

Table 1: Summary statistics

Variable	Mean	Standard deviation	Minimum	Maximum
Age at time of survey	32.86	8.52	15	49
Age at time of ban	23.63	10.06	0	48
Urban	0.422	0.494	1	0
Mortality of first-born child				
Infant	0.063	0.243	0	1
Under-5	0.093	0.291	0	1
Treatment intensity	1.27	0.72	0	3.40
Observations		203,945		

Notes: The sample restricts to women who have ever given birth and who were born after the child marriage ban was implemented in their country.
Data source: Demographic and Health Surveys.

Table 2: Effects of child marriage bans on first-born child mortality

Outcome	Infant mortality	Under-5 mortality
	(1)	(2)
postban × intensity	-0.007**	-0.010**
	(0.003)	(0.004)
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. $postban$ 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, subnational region fixed effects, and the interaction between treatment intensity and age at the time of survey.
*p<0.1; **p<0.05; ***p<0.01

Table 3: Heterogeneous effects by country income classification

	Infant mortality				Under-5 mortality			
	Low income	Low-middle	Middle	Diff	Low income	Low-middle	Middle	Diff
postban × intensity	-0.018***	-0.003	0.001	-	-0.030***	0.003	0.011	-
	(0.006)	(0.005)	(0.012)	-	(0.008)	(0.005)	(0.014)	-
Diff: low-middle vs. low income				0.015*				0.033***
				(0.008)				(0.010)
Diff: middle vs. low income				0.020				0.041**
				(0.013)				(0.016)
Mean outcome of pre-ban cohorts	0.087	0.059	0.041		0.134	0.088	0.055	
Observations	78,927	70,744	54,274		78,927	70,744	54,274	

Notes: Standard errors clustered at subnational region level are in parentheses. $postban$ is an indicator variable coded as one if individuals are under the age of 18 at the time of the of the ban. Diff stands for differences. Country income levels are taken from World Bank income classifications. All specifications control for country-cohort fixed effects, subnational region fixed effects, and the interaction between treatment intensity and age at the time of survey.

*p<0.1; **p<0.05; ***p<0.01

Table 4: Heterogeneous effects by child gender, urban-rural areas and household wealth

	Infant mortality			Under-5 mortality		
Panel A: by child's gender	Boys	Girls	Difference	Boys	Girls	Difference
postban × intensity	-0.016***	0.002	0.018**	-0.021**	0.003	0.024*
	(0.005)	(0.005)	(0.008)	(0.008)	(0.007)	(0.013)
Mean outcome of pre-ban cohorts	0.073	0.059		0.106	0.092	
Observations	105,665	98,274		105,665	98,274	
Panel B: by rural-urban areas	Rural	Urban	Difference	Rural	Urban	Difference
postban × intensity	-0.008*	-0.011**	-0.003	-0.013*	-0.013**	0.000
	(0.005)	(0.005)	(0.007)	(0.007)	(0.007)	(0.010)
Mean outcome of pre-ban cohorts	0.078	0.051		0.119	0.072	
Observations	117,828	86,112		117,828	86,112	
Panel C: by household wealth	Rich	Poor	Difference	Rich	Poor	Difference
postban × intensity	-0.002	-0.013**	-0.011	0.002	-0.018**	-0.020*
	(0.004)	(0.006)	(0.008)	(0.006)	(0.008)	(0.010)
Mean outcome of pre-ban cohorts	0.054	0.084		0.076	0.131	
Observations	113,724	90,207		113,724	90,207	

Notes: Standard errors clustered at subnational region level are in parentheses. $postban$ 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, subnational region fixed effects, and the interaction between treatment intensity and age at the time of survey.
*p<0.1; **p<0.05; ***p<0.01

Table 5: Potential mechanisms

Outcomes	Age at first marriage (1)	Age at first birth (2)	Years of schooling (3)	Currently employed (4)	Age gap (5)	Contraceptive use (6)	Mother before 18 (7)	Total fertility (8)
postban × intensity	0.610***	0.651***	0.004	-0.006	-0.328**	0.028***	-0.040***	-0.181***
	(0.120)	(0.089)	(0.115)	(0.009)	(0.132)	(0.008)	(0.013)	(0.068)
Mean outcomes of unaffected cohorts	19.266	20.483	5.537	0.607	7.074	0.368	0.262	4.090
Observations	194,406	203,945	203,900	203,727	173,282	203,945	203,945	203,945

Notes: Standard errors clustered at subnational region level are in parentheses. $postban$ is an indicator variable coded as one if individuals are under the age of 18 at the time of the of the ban. All outcomes are measured among women at first birth. All specifications control for country-cohort fixed effects, subnational region fixed effects, and the interaction between treatment intensity and age at the time of survey.
*p<0.1; **p<0.05; ***p<0.01

Appendix Tables

Table A1: Summary of country samples and child marriage bans across country

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	16 or 17	18 or older

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

Table A2: Falsification test: Effects of child marriage bans on HIV/AIDS knowledge

Outcome	HIV/AIDS knowledge	Maternal-to-child transmission knowledge
postban × intensity	0.002	-0.005
	(0.004)	(0.006)
Mean outcomes of unaffected cohorts	0.083	0.377
Observations	177,189	177,189

Notes: Standard errors clustered at subnational region level are in parentheses. $postban$ 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, subnational region fixed effects, and the interaction between treatment intensity and age at the time of survey.
*p<0.1; **p<0.05; ***p<0.01

Table A3: Additional robustness checks: region-specific trends, and migration

	Infant mortality	Under-5 mortality
Panel A: controlling for education reforms		
postban × intensity	-0.011***	-0.011***
	(0.003)	(0.004)
Mean outcome of pre-ban cohorts	0.066	0.099
Observations	203,945	203,945
Panel B: allowing for trends based on baseline regional characteristics		
postban × intensity	-0.019***	-0.036***
	(0.004)	(0.004)
Mean outcome of pre-ban cohorts	0.063	0.095
Observations	100,273	100,273
Panel C: restricting to those still living in place of birth		
postban × intensity	-0.014**	-0.017***
	(0.007)	(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. $postban$ is an indicator variable coded as one if individuals are under the age of 18 at the time of the of the ban.

In addition to all covariates specified in the main specification, Panel A additionally controls for an interaction between an education reform exposure indicator and the treatment intensity variable; Panel B additionally controls for the post-ban dummy interacted with subnational region-specific characteristics at baseline (child mortality, ethnicity, religion, age at marriage, and years of schooling).

Panel B uses only 13 countries that have data available in the pre-ban period.

Panel C restricts the analysis sample to those who are still living in their place of birth (in countries where migration information was recorded).

*p<0.1; **p<0.05; ***p<0.01

Table A4: Selection into sample

	Ever given birth (1)	Ever married (2)	Ever given birth (3)
postban × intensity	0.002	0.139***	0.091***
	(0.015)	(0.009)	(0.009)
$intensity \times$ age control	Yes	No	No
Mean outcomes of unaffected cohorts	0.741	0.768	0.741
Observations	275,115	275,115	275,115

Notes: $postban$ is an indicator variable coded as one if individuals are under the age of 18 at the time of the of the ban. Standard errors clustered at subnational region level are in parentheses. The analysis sample consists of all women in the DHS, including those who have never given birth. Ever given birth is an indicator variable equal to 1 for all women in our main analysis sample.
*p<0.1; **p<0.05; ***p<0.01

Table A5: Summary of mechanism outcomes

Variable	Definition	Mean	SD	Min	Max	N
Age at first marriage	Age at time of first marriage (among ever-married women)	18.884	4.505	7	48	194,406
Age at first birth	Age at time of first birth	20.094	4.215	7	48	203,945
Years of schooling	Completed years of schooling	5.743	5.312	0	20	203,900
Currently employed	=1 if the respondent reported working at the time of the survey	0.579	0.494	0	1	203,727
Age gap	Difference between partner's age and mother's age (among women who are currently married or cohabiting with partner)	7.371	7.201	-34	79	173,282
Contraceptive use	=1 if the respondent reported using any contraceptive method at the time of the survey	0.342	0.474	0	1	203,945
Mother before 18	=1 if the respondent gave birth before the age of 18	0.287	0.453	0	1	203,945
Total fertility	The total number of children ever born to the respondent	3.592	2.296	1	10	203,945

Notes: SD stands for standard deviations; N denotes number of observations.
Data source: Demographic and Health Surveys.

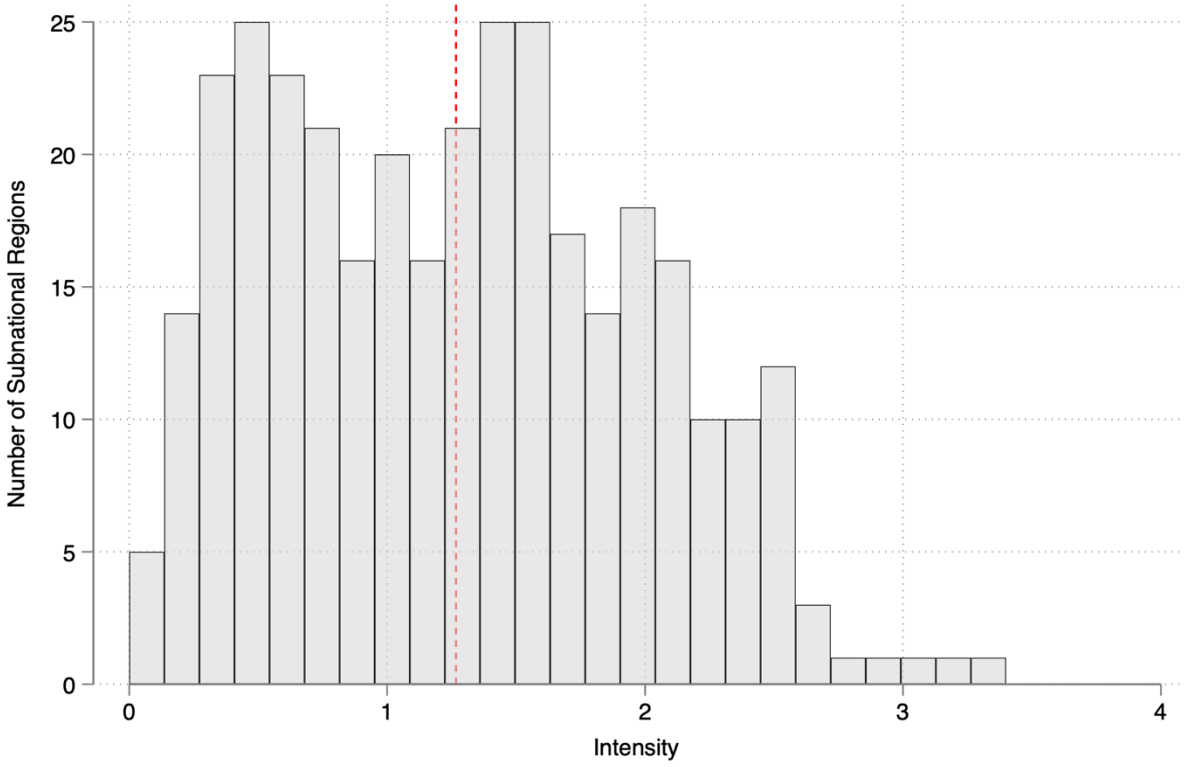
Table A6: Education reforms in our country samples

Country	Type of reform	Year of reform	Primary/ secondary school starting age	First birth cohort exposed to the reform
Albania	Increase compulsory school from 8 to 9 years	2012	6/11	1998
D. R.C.	Free primary and secondary schools	2015	6/12	2009
Egypt	Increase compulsory school from 9 to 12 years	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 years	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, Huh, and Heymann 2023; Wilson 2022).

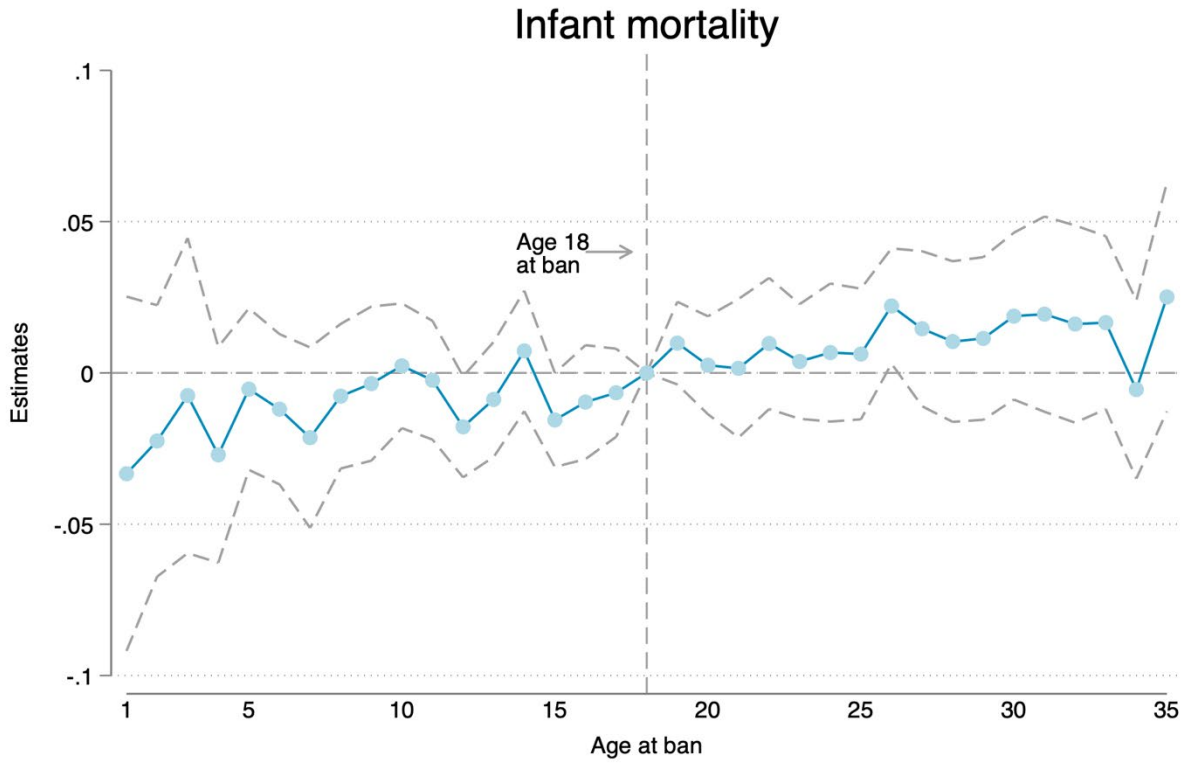
Main Figures

Figure 1: Distribution of treatment intensity variable

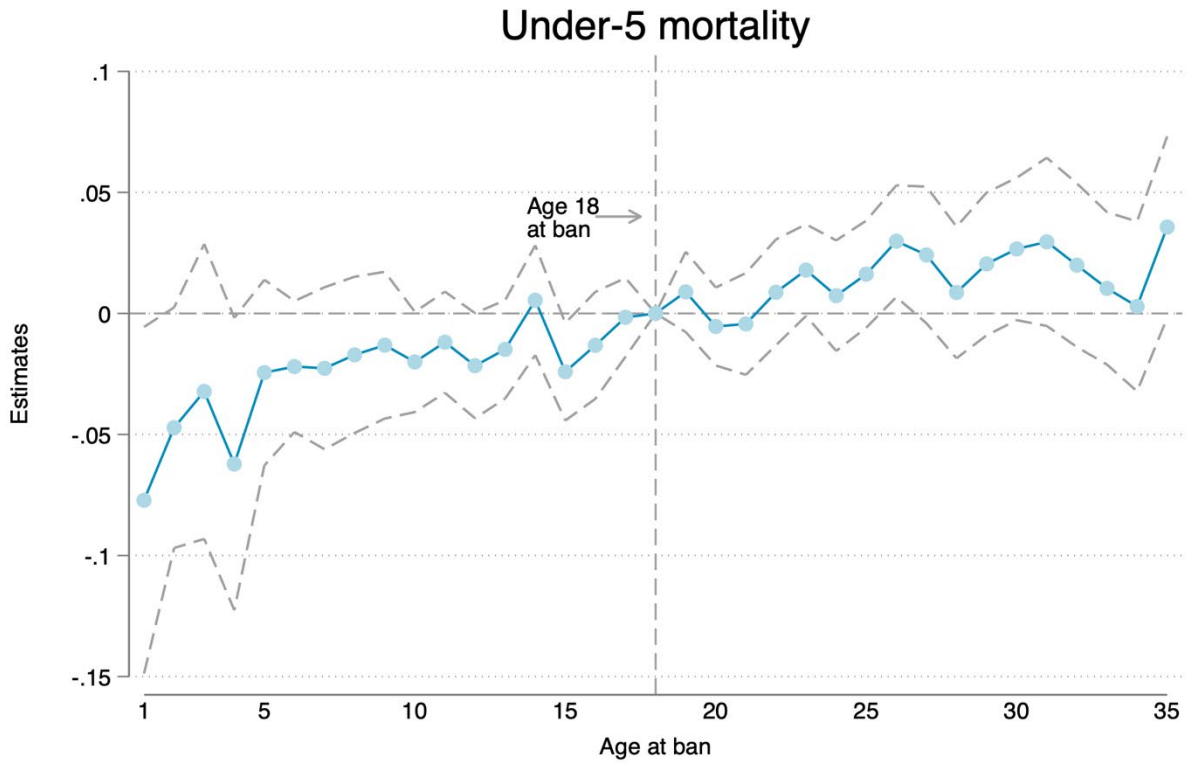


Notes: Treatment intensity is calculated as described in Equation (1). The vertical red line depicts the mean (1.27).

Figure 2: Event study
Panel A: Infant mortality



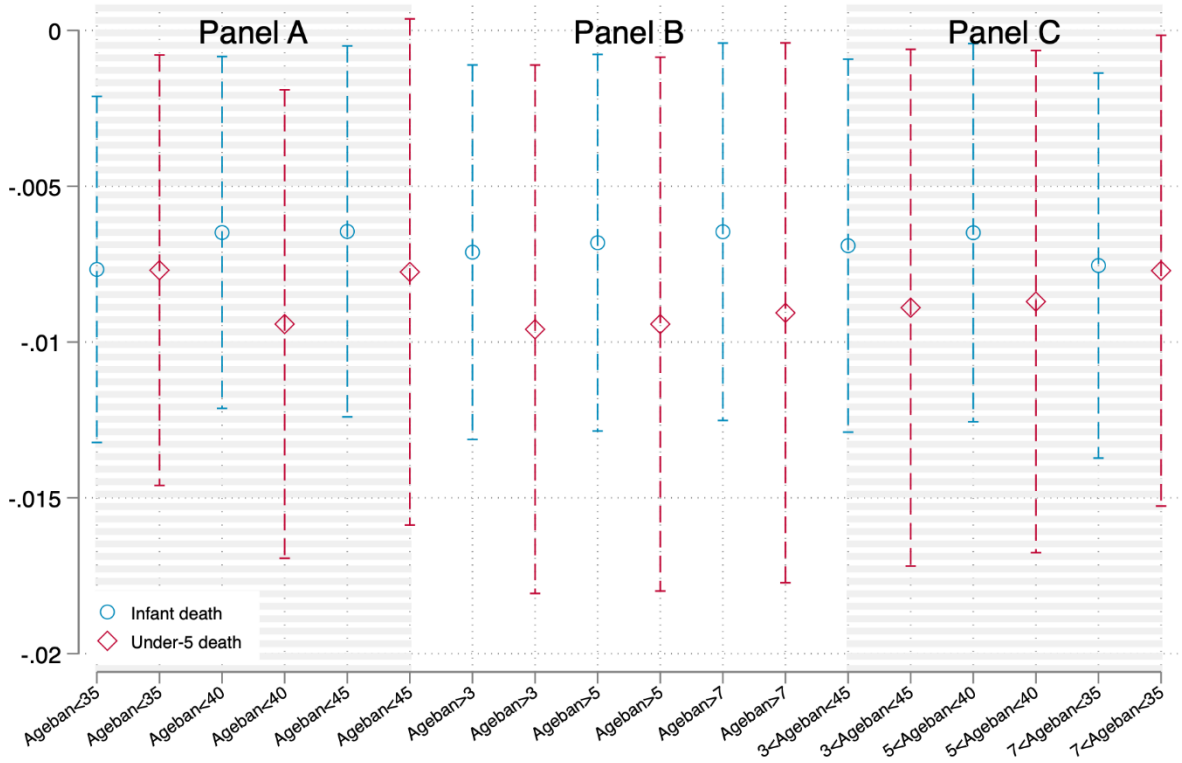
Panel B: Under-5 mortality



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.

Appendix Figures

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), for various alternative samples.
Panel A restricts the pre-ban cohorts to those aged 35 and under, 40 and under, and 45 and under. Panel B restricts the post-ban cohorts to those 3 and older, 5 and older, and 7 and older. Panel C imposes combinations of the two sets of restrictions.