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Child Marriage, Legal Regimes, and Life Outcomes

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This paper studies how minimum-marriage-age laws shape women's outcomes in sub-Saharan Africa. We develop a three-stage model linking legal enforcement, pre-market educational investment, marriage-market selection, and intra-household bargaining. In the model, stricter enforcement delays marriage, later marriage increases the human capital women bring into marriage, and these entry conditions affect household outcomes through selection and bargaining. We test the model using a fuzzy regression discontinuity design across eleven African border pairs, comparing women who live near the same international border but face different legal regimes. Women on the stricter side of the border complete 0.27 additional education levels, have their first birth 0.84 years later, have 0.29 fewer children, and are 3.2 percentage points more likely to work in white-collar employment. Years of education also increase by 0.69 years, although this estimate is sensitive to spatial correction of standard errors. Taken together, the results suggest that minimum-marriage-age laws affect women's lives not only by delaying marriage directly, but also by changing pre-marital investment and the conditions under which women enter household bargaining.

Keywords: Household bargaining, child marriage, human capital, border design

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A.I. Declaration

Generative AI tools were used in the writing of this thesis. These include assistance with code writing and debugging in Stata, R, and LaTeX, as well as grammar and punctuation checks. These tools were also used to improve the readability of the text. The models used were ChatGPT (OpenAI) and Claude (Anthropic).

All content produced with the assistance of AI tools was reviewed and edited before inclusion. All ideas, arguments, and decisions are entirely our own, as are any remaining errors.

Contents

I. Introduction

Marriage has been central to social organisation for as long as there have been societies to organise, yet economists largely did not treat it as an economic institution until Becker (1973, 1974) offered a framework for treating it as something other than a cultural given. Since then, the economics of marriage has grown substantially, absorbing insights from matching theory, household bargaining, and human capital accumulation (Wahhaj, 2018). Still, a narrower question about what happens to women's lives when they marry young, and what role law plays in enabling that, has not received enough empirical attention, particularly in sub-Saharan Africa where the practice remains widespread. This paper tries to fill part of that gap, by studying child marriage as both a market equilibrium outcome and a policy object.

The paper has a theoretical and an empirical part. A large body of empirical work documents that women who marry young fare worse on a range of different outcomes: education, fertility, employment, health (Field and Ambrus, 2008; Wilson, 2022). What that work generally cannot tell us is why. Does early marriage truncate schooling directly? Does it shift bargaining power inside the household? Does it change the composition of who enters the marriage market at later ages, and in ways that feed back into individual decisions? Without a formal framework, these mechanisms are difficult to distinguish and difficult to test separately. The model serves an organisational purpose: it makes precise which margins the legal environment should move, in what order, and why, so that the empirical results can be evaluated against specific predictions rather than post-hoc rationalisations.

The model connects three objects: the legal regime, the marriage market, and the married household. A girl's family chooses how much to invest in her education before she enters the marriage market, knowing that this investment will be worth more the older she is when she marries, because the returns to education are convex in time. Once she enters the market, the legal minimum age operates as a cost on underage marriage. A stricter regime lowers marriage hazards before the threshold, which delays marriage and changes who remains unmarried at later ages. That process of selection feeds directly into the household problem, because the state a woman carries into marriage (her education, her human capital, the assets she brings) determines how that household will operate. The household problem is specified explicitly, and its value function is what enters the pre-marriage value of accepting a match. The model is therefore recursive: legal enforcement shapes the distribution of marriage timing, timing shapes household entry conditions, and those entry conditions shape the downstream outcomes we observe in the data.

The empirical strategy is designed with this chain in mind. We use Demographic and Health Survey data from eleven border pairs in sub-Saharan Africa, where two neighbouring countries share a land border but differ in their minimum legal marriage age. The border creates a sharp discontinuity in the legal regime while holding a great deal else constant. We estimate a fuzzy regression discontinuity following the border design of Anderson (2018), using residence on the high-age side of the border as an instrument for child marriage, through which the legal environment affects women's outcomes. The estimand is the total causal effect of the child marriage legal environment on women's outcomes. The enforcement cost of underage marriage is the primitive that differs across the border, and it affects outcomes through two simultaneous channels: realized marriage timing and anticipatory pre-market investment by families who expect their daughters to marry young. Our main results are the reduced-form effects of residing on the strict side. We also report IV estimates that scale these reduced-form effects by the first-stage compliance rate.

The reduced-form results are consistent with the model's predictions. Women on the strict side of the border have completed higher levels of education and are more likely to have received any schooling at all, relative to women on the lenient side. Age at first birth increases, children ever born decreases, and age at first sex is delayed. White-collar employment rises on the extensive and intensive margins. Broad labour-force participation also shifts: women on the strict side are more likely to report not working, though the model treats this margin as theoretically ambiguous. Paid-in-cash employment shows no significant effect.

The contribution is a recursive framework in which legal enforcement, pre-market investment, marriage market selection, and household bargaining are connected by a single structure, paired with an empirical strategy that uses legal discontinuities at international borders to provide plausibly causal evidence under the border-smoothness assumption.

The remainder of the paper proceeds as follows. Section II reviews the related literature on child marriage, female legal rights, and intra-household bargaining. Section III develops the three-stage theoretical model. Section IV describes the data sources and sample construction. Section V specifies the empirical strategy and identification assumptions. Section VI reports the main estimates and heterogeneity analysis. Section VII presents the robustness checks. Section VIII discusses the interpretation of the findings, limitations, and directions for future research. Section IX concludes.

II. Literature Review

We organise the review around the theoretical and empirical mechanisms used in this study: marriage timing as an economic decision, human-capital accumulation before and around marriage, legal enforcement, household bargaining, and empirical identification.

A. Marriage Timing as an Economic Decision

The systematic study of marriage as an economic institution begins with Becker (1973) and Becker (1974), who modelled the marriage market as an assignment problem in which individuals sort on the basis of comparative advantage and complementarities in household production. The central insight is that marriage generates surplus through specialisation and risk-sharing, and that who marries whom, and when, is the outcome of a competitive equilibrium rather than a social convention. Subsequent work formalised this using tools from matching theory. Shimer and Smith (2000) extended the framework to allow for search frictions, establishing conditions under which positive assortative mating arises when match quality is complementary. Our model contains this search structure: women meet potential grooms with exogenous probability and marriage occurs when the match surplus is non-negative after accounting for the legal wedge.

A related strand asks why marriage timing varies systematically with education and economic conditions. Iyigun and Lafortune (2016) propose a model in which educational and marriage decisions are jointly determined, showing that minimum age laws can raise attainment by delaying the point at which marriage competes with schooling. Our model formalises such a complementary mechanism as returns to pre-market investment are convex in the age at marriage; early marriage depresses both investment and attainment through two reinforcing channels.

The marriage market also responds to economic incentives and local institutions. Corno et al. (2020) study how income shocks affect the timing of child marriage in sub-Saharan Africa and India, finding that droughts raise early marriage hazards in bride price societies through a consumption-smoothing motive. Their paper establishes that marriage timing in the region responds to economic incentives in ways consistent with an equilibrium model, which supports treating marriage timing as an optimising outcome rather than just a cultural one.

Wahhaj (2018) is the direct theoretical predecessor to our model in the child marriage literature. His framework captures how parental preferences and social norms sustain early marriage in equilibrium, but does not embed a solved household problem into the continuation value of marriage. We take inspiration from his core market structure and extend it by adding Stages 1 and 3, which connects legal enforcement to fertility, labour supply, and bargaining power in a formal manner.

B. Child Marriage and Human Capital Accumulation

The empirical literature most directly related to our work studies the consequences of early marriage for women's human capital and later outcomes. Field and Ambrus (2008) is the closest antecedent. They exploit variation in age at menarche as an instrument for age at first marriage in rural Bangladesh, finding that delaying marriage by one year increases female educational attainment by 0.22 years. Their mechanism is closely related to ours: early marriage curtails schooling by moving girls out of the educational sphere and into marriage at ages when they would otherwise continue accumulating human capital.

The connection between marriage matching and human capital investment is developed by Chiappori et al. (2017), who embed marriage matching into a transferable utility framework and show that rising returns to education reinforce assortative matching on schooling. Our model inherits this result: the Pareto weight that determines intra-household bargaining in Stage 3 of our model is increasing in education and human capital at marital entry, so anything that changes the distribution of entry states changes the distribution of within-marriage outcomes throughout the marriage.

Jayachandran and Lleras-Muney (2009) provide supporting evidence by showing that longer expected life horizons raise educational investment in developing countries, because the returns to schooling accumulate over time. This is the empirical counterpart to the $\gamma > 1$ assumption in our model: the years closest to market entry are the most productive precisely because they sit on the steepest part of the accumulation curve, so early marriage removes the years of highest marginal return.

The marriage market also interacts with cultural institutions that shape investment in daughters. Ashraf et al. (2020) show that the bride price custom creates incentives for parents to educate their daughters, since a more educated daughter commands a higher transfer at marriage. School construction programs in Indonesia and Zambia raised female schooling significantly among ethnic groups with the bride price custom but had no effect among groups without it. This mechanism is present in Stage 1 of our model, where parental investment responds to the anticipated marriage market returns.

Heath and Mobarak (2015) study how the growth of Bangladesh's garment sector changed girls' schooling, work, marriage, and fertility decisions. Girls exposed to garment-sector opportunities delay marriage and childbirth and are more likely to remain in school at younger ages, while older girls are more likely to engage in wage work. Their evidence supports the broader mechanism that marriage timing responds to the economic returns to female education and work: when opportunities outside early marriage improve, schooling becomes more valuable and early marriage becomes more costly. Duflo et al. (2015) demonstrate a related schooling-cost channel in a seven-year randomized evaluation in Kenya, showing that education subsidies reduce dropout, pregnancy, and marriage, while an HIV curriculum stressing abstinence until marriage does not reduce pregnancy or STI.

C. Legal Regimes and Enforcement

The strand of work closely related to the policy object in this paper studies how legal regimes governing marriage and family law change women's outcomes. The central insight across this literature is that legal regimes affect behaviour not only through direct enforcement but through the outside options and expectations they create. A woman's position inside a marriage depends on what exit from that marriage looks like, and the legal rules that shape outside options matter even for women who never exercise them.

Wilson (2022) uses legislative reforms raising the minimum legal marriage age across seventeen low and middle-income countries as natural experiments, finding significant positive effects on female schooling and labour market participation. Our approach differs from Wilson's in two important respects. First, our identification strategy uses cross-border variation in legal regimes rather than within-country pre/post variation, addressing the concern that legislative reforms are themselves endogenous to trends in women's outcomes. Second, our estimates are grounded in a theoretical model that specifies the mechanisms through which legal enforcement operates, so the results carry interpretive content beyond just the reduced-form results.

Collin and Talbot (2023) address a closely related but distinct question: whether statutory minimum-age laws are binding in practice. Using regression-discontinuity methods on DHS and MICS data from over one hundred countries, they test whether marriage rates change discontinuously at each country's legal cutoff. They find limited evidence that minimum-age laws are consistently enforced, and argue that legal reform is more likely to matter when paired with monitoring and enforcement capacity. Their findings motivate our interpretation of the legal regime as an enforcement-relevant environment rather than merely a statutory rule on paper. They also provide context for our urban-rural heterogeneity analysis: if formal institutions are more accessible in urban areas, the legal discontinuity may be more binding there.

Buchmann et al. (2023) use a field experiment in Bangladesh to test whether financial incentives and girls' empowerment programs can delay marriage. They find that girls eligible for a financial incentive for at least two years are 19 percent less likely to marry underage, while the empowerment program did not reduce adolescent marriage. This evidence is consistent with the broader premise of our model that marriage timing responds to incentives and constraints in the marriage market, although their intervention operates through financial incentives rather than legal enforcement.

Legal rules also provide one source of exogenous variation in marriage timing. Dahl (2010) uses variation in US state laws governing minimum marriage age, compulsory schooling, and child labour as instruments for early teen marriage and schooling choices, finding that women who marry young are substantially more likely to live in poverty as adults.

Voena (2015) estimates a structural model of household decision-making, showing that divorce laws affect couples' intertemporal choices, with effects that depend on the property-rights regime governing marital assets. This forward-looking response to family law is related to the logic of Stage 1 in our model, though the setting differs: in our framework, families' education investment responds to the expected marriage-timing environment rather than to divorce risk. Fernández and Wong (2014) provide complementary evidence that changes in the risk of marital dissolution can affect women's labour-supply decisions, finding that higher divorce probability and changes in the wage structure can each explain a large share of the rise in married women's labour force participation in the United States.

D. Household Bargaining, Fertility, and Labour Supply

Our married-household problem draws on the collective household model of Chiappori (1988) and Chiappori (1992). The core idea is that household members may have distinct preferences, but household choices can be represented as Pareto-efficient allocations governed by a sharing rule or Pareto weights. Chiappori et al. (2002) extend this framework by showing how distribution factors, including sex ratios and divorce laws, can shift household members' bargaining positions without directly changing preferences or the joint budget set. Our model applies this logic to marital entry states: in our framework, women who enter marriage with more education and human capital have stronger outside options, which raises their Pareto weight and shifts household choices over fertility and labour supply.

Ashraf et al. (2014) test the role of household bargaining in fertility decisions using a field experiment in Zambia. They vary whether married women receive access to contraceptives alone or in the presence of their husbands, and find that women assigned to receive access with their husbands are less likely to seek family-planning services, less likely to use concealable contraception, and more likely to give birth. Their results show that fertility choices can reflect bargaining frictions and asymmetric information within marriage, rather than only a single household preference. This mechanism is related to Lemma 4 of our model, where lower female bargaining weight expands the region in which birth is chosen. Our contribution is to connect the source of bargaining power at marital entry, including education and human capital shaped by child-marriage legal regimes, to the fertility and labour-supply outcomes studied in the household-bargaining literature.

Garcia-Hombrados (2022) uses age discontinuities in exposure to Ethiopia's Revised Family Code to estimate the causal effect of delaying teenage cohabitation on infant mortality. He finds that a one-year delay in age at cohabitation reduces the probability that a woman's first-born child dies during infancy by 3.8 percentage points, with the mechanism closely linked to the resulting delay in age at first birth.

E. Identification: Legal Variation and Border Designs

The empirical challenge in this literature is selection. Women who marry young differ from those who marry later in ways that are correlated with almost every outcome of interest. Naive comparisons confound the effect of marriage timing with the characteristics of women who marry at different ages. Credible identification therefore requires variation in marriage timing that is generated by legal, biological, institutional, or policy variation rather than by the woman's own characteristics or family preferences.

Reform-based designs provide one such source of variation. The approach in Wilson (2022), discussed above, uses spatial and cohort variation in exposure to child-marriage bans in a difference-in-differences framework. Relative to reform-based designs, our approach uses cross-border discontinuities in legal regimes rather than within-country exposure to legislative reform. This shifts the identifying comparison from changes over time within reforming jurisdictions to nearby populations on opposite sides of a border that face different legal marriage-age environments.

The closest methodological antecedent to our empirical strategy is Anderson (2018), who uses variation in colonial legal origins across African borders as an instrument for gender-related legal institutions, studying their effects on female HIV prevalence. Anderson's border design is the foundation of our own identification approach. We follow their empirical design by using GPS-linked individual microdata from the DHS, which allows each survey cluster to be located relative to the legal cutoff and enables a regression discontinuity framework.

The framework that underpins our design rests on methodological foundations laid out by Imbens and Lemieux (2008) and Lee and Lemieux (2010). These papers establish the conditions under which sharp and fuzzy RD designs identify local average treatment effects, characterise the complier population whose behaviour is shifted by the instrument, and set out the diagnostic tests that allow researchers to assess whether the identifying assumptions hold in practice.

F. Gaps this Paper Addresses

Taken together, the existing literature leaves two gaps this paper addresses. The first is theoretical. Existing work explains marriage as an equilibrium outcome, shows that marriage timing responds to incentives, and studies how bargaining power affects household choices. What is less developed is a single framework connecting the legal minimum marriage age, pre-market educational investment, marriage-market selection, and intra-household bargaining. Relative to Wahhaj (2018), our model keeps the focus on early marriage as a marriage-market outcome but extends the analysis to the household continuation problem and to downstream outcomes through marital entry states. It therefore treats the legal regime, the marriage market, and the married household as connected parts of a single dynamic problem rather than as separate stages.

The second gap is empirical. The literature has made significant progress in credibly estimating particular effects of child marriage in specific countries and settings, including schooling in Bangladesh (Field and Ambrus, 2008), schooling and labour-market outcomes across multiple countries (Wilson, 2022), and infant mortality in Ethiopia (Garcia-Hombrados, 2022). What is less common is a single empirical design that tests the joint predictions of a recursive model. We estimate effects on schooling, fertility and skilled employment from the same border discontinuity and the same sample, which lets us read the pattern of outcomes against the model's predictions rather than evaluating each margin in isolation.

III. Theoretical Framework

The model proceeds in three stages. Stage 2 (the marriage market) is a reduced-form marriage-market block inspired by Wahhaj (2018). It preserves the key insight that delaying marriage is costly because future match opportunities are uncertain, but abstracts from Wahhaj's asymmetric-information and reputation structure to focus on the legal wedge and its downstream consequences. Stages 1 and 3 are the extensions introduced in this paper. Stage 1 is a pre-market education investment problem in which a girl's family chooses schooling investment before she enters the marriage market: when early marriage is anticipated, the expected returns to education are lower, depressing investment before any marriage occurs. We formalise this through a convex accumulation function and embed it in the recursive structure of the model. Stage 3 is a married-household bargaining problem in which the woman's entry state (her education and human capital at the time of marriage) determines her bargaining weight and the household's subsequent choices over fertility, labour supply, and schooling. The stages are numbered in the order experienced by the woman. The family's Stage 1 investment decision responds to the expected Stage 2 marriage-market equilibrium; the Stage 2 outcome shapes the entry state carried into Stage 3; and the Stage 3 value of that entry state is what Stage 1 families anticipate when choosing κ.

A. Setup

Two jurisdictions are indexed by $j \in \{\text{Strict}, \text{Lenient}\}$, corresponding to the two sides of an international border. Residence is fixed and determines the applicable legal regime. The minimum legal age for female marriage in jurisdiction j is $\bar{a}_j$, with $\bar{a}_{\text{Strict}} = 18$ and $\bar{a}_{\text{Lenient}} < 18$. The only primitive that differs across jurisdictions is the expected utility-equivalent cost of contracting an underage marriage. For a woman of age a, the enforcement wedge $\Lambda_j(a)$ is positive for $a < \bar{a}_j$ and zero otherwise:

$$\Lambda_j(a) = \mathbf{1}\{a < \bar{a}_j\}\ell_j(a), \quad \ell_j(a) \geq 0, \tag{1}$$

where $\ell_j(a)$ captures the expected utility-equivalent cost of enforcement (invalidation risk, administrative barriers, anticipated sanctions), which may vary by age and jurisdiction.

Assumption 1 (Single-primitive legal variation)**.** *All non-policy primitives (preferences, technology, non-policy institutions) are identical across jurisdictions.*

Bargaining primitive We introduce here a single bargaining primitive $\theta \in (0,1)$ that will appear in both Stage 2 and Stage 3. In Stage 2, θ governs the fraction of her gross marriage gain that the woman retains after intra-household frictions. In Stage 3, θ enters the Pareto weight $\omega = \Omega(b_0, h_0, e_0; \theta)$ that determines how household preferences aggregate. The same θ appears in both roles by assumption: it is a single deep parameter of the woman's bargaining strength, and we do not derive the link between its Stage 2 and Stage 3 manifestations from a more primitive bargaining protocol. The mapping $\Omega(\cdot;\theta)$ is treated as maintained throughout, with weak monotonicity in each argument; the micro-foundation is discussed in Appendix A.

B. *Stage 1: Pre-Market Education Investment*

Before entering the marriage market at age a_0, a girl's family chooses an education investment $\kappa \in [0, \bar{\kappa}]$ at convex cost $c(\kappa)$, with $c' > 0$ and $c'' > 0$. Let a_s denote the school-starting age and $T_m = a_m - a_s$ the years of potential schooling before marriage. The educational endowment at marital entry is

$$e_0(T_m;\kappa) = e_b + \kappa \cdot T_m^{\gamma}, \quad \gamma > 1, \tag{2}$$

where $e_b \geq 0$ is a baseline education level and κT_m^{γ} is the family-investment component. The parameter $\gamma > 1$ imposes increasing returns to schooling exposure: each additional year before marriage builds on skills accumulated in prior years, making the return strictly convex in T_m. This is consistent with dynamic complementarity in skill formation (Ben-Porath, 1967; Cunha and Heckman, 2007): human capital is self-productive, so earlier investments raise the productivity of later ones. Early marriage truncates accumulation before girls reach the high-return portion of the schooling path.

The family forms expectations over the equilibrium distribution of a_m under the applicable legal regime j, noting that $M(a_m;\kappa)$ (defined in Stage 2 below) already integrates over idiosyncratic draws ξ internally. The jurisdiction-specific investment problem is

$$\kappa_j^* = \arg \max_{\kappa \in [0,\bar{\kappa}]} \mathbb{E}_{a_m|j}\big[M(a_m;\kappa)\big] - c(\kappa), \tag{3}$$

where the outer expectation is taken only over the equilibrium distribution of marriage ages under regime j. Here M is the daughter's expected value from the marriage–household continuation problem; the family maximises her individual expected welfare rather than a joint household objective. The first-order condition is

$$\mathbb{E}_{a_m|j}\left[\frac{\partial M}{\partial e_0}\cdot T_m^{\gamma}\right] = c'(\kappa_j^*), \tag{4}$$

where the sign $\partial M/\partial e_0 > 0$ follows from two channels: the direct effect of e_0 on the wage w^f and on the continuation V_2 (the latter via Assumption 5 combined with $\partial e_1/\partial e_0 = 1$), and the weak monotonicity of the Pareto weight $\omega = \Omega(b_0, h_0, e_0; \theta)$ in e_0. At any optimal household choice d^*, both channels move the woman's value M in the same direction, so the FOC has the standard interpretation.

Proposition 1 (Education investment and marriage-age exposure are complements)**.** *For any fixed κ, the expected marginal return to education investment is strictly increasing in expected marriage-age exposure $\mathbb{E}[T_m \mid j]$. If the strict regime induces a weak upward shift in the distribution of T_m, then $\kappa^*_{Strict} \geq \kappa^*_{Lenient}$, with strict inequality if the shift is strict over a set of positive probability and the optimum is interior. The complementarity condition is*

$$\frac{\partial^2 e_0}{\partial \kappa\, \partial T_m} = \gamma T_m^{\gamma-1} > 0,$$

so the marginal return to each unit of investment κ is strictly increasing in schooling exposure T_m. Families facing shorter expected schooling horizons under lenient regimes therefore rationally invest less. Stage 2 provides the mechanism by which stricter enforcement shifts the distribution of marriage ages upward. (Proof in Appendix A)

Remark 1. With convex returns ($\gamma > 1$), the marginal return to additional schooling exposure is highest at longer durations. Early marriage removes those years, truncating accumulation where the curve is steepest. The lenient legal regime therefore depresses educational attainment through two channels: realised truncation from lower a_m in equilibrium, and the anticipatory reduction in κ that follows from lower expected marriage age.

C. Stage 2: The Marriage Market

Time is discrete and indexed by age $a \in \mathscr{A} \equiv \{a_0, a_0+1, \ldots, a_1\}$, where a_0 is entry into the marriage market and a_1 is the terminal age. At age a, an unmarried woman meets a potential groom with probability $\lambda(a) \in (0,1)$, weakly decreasing in age: meeting rates are highest at younger ages when the pool of unmatched men is largest, and fall as the cohort thins. A woman who declines a match at age a faces a lower probability of receiving an offer at $a+1$, which is why early marriage can be an equilibrium outcome even when later marriage yields a higher entry state.

Let $M(a;\kappa_j^*)$ denote the woman's expected lifetime value from the household continuation problem, conditional on marrying at age a:

$$M(a;\kappa_j^*) = \mathbb{E}_\xi\left[U_1^f(S_0, d^*(S_0,\omega);\omega) + \beta V_2(S_1)\right], \tag{5}$$

where U_1^f is the woman's period-1 flow utility at the household's optimal choice $d^* = (f_1^*, s_1^*, \ell_1^*)$, and $V_2(S_1)$ is the period-2 continuation value evaluated at the updated state $S_1 = (h_1, e_1, n_1)$. M is the woman's value at the equilibrium household choice; it differs from her post-bargaining marriage-market payoff $U_j^f(a)$, which adds the surplus-split adjustment described below. The woman's gross gain from accepting a match at age a, net of the enforcement cost and her outside option, is

$$\Delta_j(a;\kappa_j^*) = M(a;\kappa_j^*) - \Lambda_j(a) - V_j(a+1;\kappa_j^*). \tag{6}$$

The woman accepts the match if and only if $\Delta_j(a) \geq 0$.

Assumption 2 (No indifference)**.** *At the equilibrium investment κ_j^*, $\Delta_j(a;\kappa_j^*) \neq 0$ for every $a \in \mathscr{A}$. This rules out measure-zero knife-edge cases where the woman is exactly indifferent, avoiding randomised tie-breaking and ensuring that the equilibrium marriage-age distribution is uniquely determined by the Stage 2 backward induction.*

Assumption 3 (Male participation)**.** *The man's value from the match weakly exceeds his outside option whenever $\Delta_j(a) \geq 0$, and weakly exceeds his outside option throughout the marriage continuation thereafter. The feasibility constraint therefore binds only for the woman, both at acceptance and during the household problem, and $\lambda(a)$ is purely the probability of a meeting. In settings where men also face legal exposure (through sanctions on grooms or their families), the feasibility constraint could bind for both parties; we abstract from this channel.*

Under Assumption 3, the woman is the only party whose participation can bind, and the exit conditions defined in Stage 3 apply to her alone. The woman's realised marriage-market payoff if the match is accepted is

$$U_j^f(a) = V_j(a+1) + \theta\,\Delta_j(a), \tag{7}$$

where $\theta \in (0,1)$ is the fraction of her gross marriage gain $\Delta_j(a)$ that she retains net of intra-household bargaining frictions. The complementary $(1-\theta)\Delta_j(a)$ is the share of marriage surplus that does not accrue to her, reflecting her bargaining position within the match. The same bargaining position also shows up in Stage 3 through the Pareto weight $\omega < 1$, so θ governs both the Stage 2 surplus split and the Stage 3 within-marriage distortion as a single primitive.

The per-period flow utility while unmarried, $u^u(a)$, is bounded and weakly decreasing in a. This captures the social and economic pressures that intensify with age in sub-Saharan Africa: stigma, declining match quality, and family pressure are all consistent with a falling $u^u(a)$. For $a < a_1$, the value of remaining unmarried under jurisdiction j satisfies

$$V_j(a) = u^u(a) + \delta\Big[(1-\lambda(a))V_j(a+1) + \lambda(a)\max\{U_j^f(a), V_j(a+1)\}\Big], \qquad (8)$$

with terminal condition $V_j(a_1) = \bar{V}$, common across jurisdictions under Assumption 1. The marriage hazard is $h_j(a) = \lambda(a)\mathbf{1}\{\Delta_j(a) \geq 0\}$. The discount factor δ in the marriage-market Bellman and the discount factor β in the household objective are distinct: δ discounts across marriage-market ages, while β discounts between period 1 and period 2 within the household.

Proposition 2 (Stricter enforcement reduces underage marriage hazards)**.** *For $a < \bar{a}_{Strict}$, if $\Lambda_{Strict}(a) \geq \Lambda_{Lenient}(a)$, then $h_{Strict}(a) \leq h_{Lenient}(a)$, with strict inequality whenever $\Delta_{Lenient}(a) \geq 0 > \Delta_{Strict}(a)$, i.e. whenever the wedge causes a woman to decline a match she would otherwise have accepted. This result holds in partial equilibrium, for a fixed continuation value, and extends to general equilibrium by backward induction from the terminal condition $V_j(a_1) = \bar{V}$; (Proof in Appendix A)*

The enforcement wedge provides the direct mechanism through which the strict regime shifts the distribution of marriage ages upward. A richer model with individual heterogeneity and reputation, in the spirit of Wahhaj (2018), would also generate selection effects above the legal threshold as women retained by enforcement differ from those who married earlier. We abstract from that channel and focus on the direct timing effect of the enforcement wedge.

D. Stage 3: The Married-Household Problem

Conditional on marriage at age a_m, the couple enters a two-period household problem with initial state

$$S_0(a_m, \xi) = \big(b_0(a_m, \xi),\, h_0(a_m, \xi),\, e_0(T_m; \kappa_j^*),\, n_0 = 0\big), \qquad (9)$$

where b_0 denotes assets accumulated by the family on the daughter's behalf before marriage, h_0 female human capital, $e_0 = e_b + \kappa_j^* T_m^\gamma$ educational attainment, and ξ an i.i.d. match-specific draw on a compact support capturing idiosyncratic variation in assets, human capital, and match quality at the time of marriage, observed by both parties upon meeting but not before. The Pareto weight $\omega \in (0,1)$ is fixed within marriage and determined at entry by $\omega = \Omega(b_0, h_0, e_0; \theta)$, with θ the bargaining primitive introduced in the Setup. The mapping $\Omega(\cdot;\theta)$ is weakly increasing in each of (b_0, h_0, e_0) and weakly increasing in θ, so women who marry later, with higher entry states, enter marriage with greater bargaining power; the micro-foundation for this monotonicity is discussed in Appendix A.

In period 1, the household allocates the woman's unit time endowment among birth (f_1), continued schooling (s_1), and labour-market participation (ℓ_1), with $f_1 + s_1 + \ell_1 = 1$ and each indicator binary, so exactly one activity is chosen. The household maximises

$$W(S_0, \omega) = \max_{f_1, s_1, \ell_1} \omega\left[\log c_1 + \upsilon_f f_1 - \chi_s s_1 - \chi_\ell \ell_1\right] + (1-\omega)\left[\log c_1 + \upsilon_m f_1\right] + \beta V_2(S_1), \tag{10}$$

subject to $c_1 \leq w^f(h_0, e_0)\ell_1 + y_m + Rb_0$, where $y_m \geq 0$ is the man's exogenous income and $R > 0$ is the gross return on assets. Parameters are: $\upsilon_f \geq 0$ and $\upsilon_m > \upsilon_f$, the woman's and man's utility benefits from a child; $\chi_s > 0$, the woman's non-pecuniary cost of continued schooling; $\chi_\ell > 0$, her disutility of labour; $\phi_s, \phi_\ell > 0$, human capital accumulation rates from schooling and work; and $w^f(h_0, e_0)$, the female wage, strictly increasing in both arguments. Because the time allocation is binary, only one activity is chosen, and when $\ell_1 = 0$ consumption equals $y_m + Rb_0$. We assume throughout that $y_m + Rb_0 > 0$, so that log consumption is well defined. Period-1 choices update the state: $h_1 = h_0 + \phi_\ell \ell_1 + \phi_s s_1$, $e_1 = e_0 + s_1$, and $n_1 = f_1$. The net payoffs from each activity are

$$\begin{aligned}
\Pi_f &= \log(y_m + Rb_0) + \omega\upsilon_f + (1-\omega)\upsilon_m + \beta V_2(h_0, e_0, 1), \\
\Pi_s &= \log(y_m + Rb_0) - \omega\chi_s + \beta V_2(h_0 + \phi_s, e_0 + 1, 0), \\
\Pi_\ell &= \log\left(w^f(h_0, e_0) + y_m + Rb_0\right) - \omega\chi_\ell + \beta V_2(h_0 + \phi_\ell, e_0, 0).
\end{aligned}$$

The household selects $\max\{\Pi_f, \Pi_s, \Pi_\ell\}$.

Assumption 4 (Child-utility gap exceeds period-1 costs)**.** $\upsilon_m - \upsilon_f > \max\{\chi_s, \chi_\ell\}$.

Assumption 5 (Period-2 continuation value)**.** *The period-2 value function $V_2(h_1, e_1, n_1)$ is the joint household continuation value. It is bounded, strictly increasing in (h_1, e_1), and satisfies $V_2(h_1, e_1, 1) < V_2(h_1, e_1, 0)$ for all (h_1, e_1). The gain from avoiding a child is in-*

creasing in both arguments, uniformly over all (h_1, e_1) and all $\omega \in (0,1)$. Additional cross-point conditions required for Lemmas 1 and 2 are stated in Appendix A.

Lemma 1 (Education raises the return to schooling relative to birth)**.** *Under Assumption 5, $\Pi_s - \Pi_f$ is strictly increasing in e_0. There exists a threshold $\bar{e}(b_0, h_0; \theta)$, implicitly defined by $\Pi_s = \Pi_f$ (with $\omega = \Omega(b_0, h_0, \bar{e}; \theta)$ satisfied jointly), such that schooling is preferred to birth whenever $e_0 > \bar{e}(b_0, h_0; \theta)$. The dependence on h_0 runs through the continuation V_2 (which enters Π_s and Π_f at different state arguments) and through ω; the dependence on b_0 runs through ω alone, since the consumption term $\log(y_m + Rb_0)$ is common to Π_s and Π_f and cancels in their difference. (Proof in Appendix A)*

Lemma 2 (Human capital raises the return to work relative to birth)**.** *Under Assumption 5, $\Pi_\ell - \Pi_f$ is strictly increasing in h_0. There exists a threshold $\bar{h}(b_0, e_0; \theta)$, implicitly defined by $\Pi_\ell = \Pi_f$, such that work is preferred to birth whenever $h_0 > \bar{h}(b_0, e_0; \theta)$. The dependence on e_0 runs through V_2 and through the wage $w^f(h_0, e_0)$; the dependence on b_0 runs through ω and through the wage gain $\log(w^f + y_m + Rb_0) - \log(y_m + Rb_0)$, which is decreasing in b_0. (Proof in Appendix A)*

Lemma 3 (Higher entry states delay fertility and shift the activity choice)**.** *Under Lemmas 1 and 2:*

(i) $f_1 = 0$ *whenever* $e_0 > \bar{e}(b_0, h_0; \theta)$ *or* $h_0 > \bar{h}(b_0, e_0; \theta)$*;*
(ii) $s_1 = 1$ *is optimal when* $e_0 > \bar{e}(\cdot)$ *and* $\Pi_s \geq \Pi_\ell$*;*
(iii) $\ell_1 = 1$ *is optimal when* $h_0 > \bar{h}(\cdot)$ *and* $\Pi_\ell \geq \Pi_s$*.*

The period-1 birth probability is weakly decreasing in (e_0, h_0). Above the thresholds, schooling is weakly increasing in e_0 (when $\Pi_s \geq \Pi_\ell$) and labour participation is weakly increasing in h_0 (when $\Pi_\ell \geq \Pi_s$). Below the thresholds, the allocation among $\{f_1, s_1, \ell_1\}$ is determined by the relative magnitudes of $\{\Pi_f, \Pi_s, \Pi_\ell\}$, and the comparative static on any single activity with respect to (e_0, h_0) is not signed without further restrictions. (Proof in Appendix A)

Lemma 4 (Lower bargaining power weakly raises the birth rate)**.** *Under Assumption 4, $\Pi_f - \Pi_s$ and $\Pi_f - \Pi_\ell$ are both strictly decreasing in ω. A lower bargaining weight therefore expands the parameter region in which birth is chosen. (Proof in Appendix A)*

Remark 2. When ω is low because the woman entered marriage young with low (e_0, h_0), her preferences carry less weight and the household tilts toward the man's preferred outcome of more children. The same channel reinforces the activity-mix shift away from schooling and work, which are the inputs into human capital required for skilled occupations later.

The woman's outside option from leaving marriage at the end of period 1 is approximated by $\bar{V}^f(e_1,h_1,n_1) = \psi_e e_1 + \psi_h h_1 - \psi_n n_1$, with $\psi_e, \psi_h, \psi_n > 0$. This is a reduced-form approximation to the equilibrium re-entry value; the linear specification preserves the qualitative comparative statics on (e_1,h_1,n_1) that drive the prediction on exits from marriage. Children reduce the outside option because they lower remarriage prospects. She exits if $\bar{V}^f(e_1,h_1,n_1) > \omega V_2(h_1,e_1,n_1)$, where ωV_2 is her share of the household continuation value from remaining in the marriage. By Assumption 3, the man's participation does not bind, so exit is a one-sided decision.

Remark 3 (Entry states and the outside-option channel). When $f_1 = 1$ is chosen (in the region $e_0 \leq \bar{e}(\cdot)$ and $h_0 \leq \bar{h}(\cdot)$), no human capital or education accumulates ($h_1 = h_0$, $e_1 = e_0$) and children lower the outside option by $\psi_n > 0$. Early marriage therefore weakens the outside option through lower (e_1,h_1) and higher n_1. Whether this translates into a lower observed rate of exits depends on the relative magnitude of the corresponding shift in the stay value ωV_2.

The monotonicity of e_0 in a_m follows directly from the accumulation technology in equation (2) and holds for any $\kappa_j^* > 0$ and $\gamma > 1$; it is a consequence of the model rather than a maintained assumption.

Assumption 6 (Monotone marital entry states)**.** *The mappings $b_0(a,\xi)$ and $h_0(a,\xi)$ are weakly increasing in the age at marriage a. Combined with the strict monotonicity of e_0 in a_m noted above, all three entry-state components are weakly increasing in a_m.*

Assumption 7 (Dominance of asset and human-capital effects below the thresholds)**.** *In the region where $e_0 \leq \bar{e}(\cdot)$ and $h_0 \leq \bar{h}(\cdot)$, the positive effects of higher (b_0,h_0,e_0) on Π_f through consumption and through V_2 jointly dominate the negative effect operating through the higher Pareto weight $\omega = \Omega(b_0,h_0,e_0;\theta)$, so the net effect of an increase in a_m on $M(a_m)$, evaluated at the equilibrium choice $f_1 = 1$, is non-negative.*

Proposition 3 (Marriage age and the continuation value from marriage)**.** *Under Assumptions 6 and 7 and Lemmas 1–3, the woman's value from entering marriage is weakly increasing in age at marriage:*

$$a' \geq a \implies M(a';\kappa_j^*) \geq M(a;\kappa_j^*).$$

(Proof in Appendix A)

Remark 4. If marrying later raises the woman's marital entry state, why does early marriage arise in equilibrium? The answer is that $M(a)$ is the value conditional on marrying at age a, not the value of waiting. A woman who declines a match at age a does not receive $M(a+1)$ with certainty. Instead, she enters the next period unmarried, receives the continuation value $V^j(a+1)$, faces a weakly lower probability of meeting another potential groom, $\lambda(a+1) \leq \lambda(a)$, and may also face lower unmarried flow utility because $u^u(a)$ is weakly decreasing in age. Early marriage can therefore arise even when later marriage would generate a better marital entry state, because waiting is risky and the value of remaining unmarried may decline with age.

Proposition 4 (Integrated downstream implications of delayed marriage)**.** *Suppose the strict regime induces a weak upward shift in the distribution of age at marriage. Under Assumption 6 and Lemmas 1–4:*

(a) the distributions of e_0, h_0, and b_0 at marital entry shift weakly upward, and consequently e_1 and h_1 shift weakly upward;

(b) the distribution of age at first birth shifts weakly upward;

(c) the period-1 birth rate shifts weakly downward; labour-market choices with higher human-capital returns become more likely, mapping to high-skill occupations in the labour market; broad labour-force participation is ambiguous because fertility at low entry states competes with work;

(d) the distribution of ω shifts weakly upward, weakly reducing the period-1 birth rate through Lemma 4;

(e) the outside option $\bar{V}^f(e_1,h_1,n_1)$ shifts weakly upward, because delayed marriage yields higher (e_1,h_1) and lower n_1. The stay value $\omega V_2(h_1,e_1,n_1)$ also shifts weakly upward, since both ω and V_2 rise with delayed marriage. Whether this translates into more observed exits depends on the relative magnitudes of the two shifts and is therefore theoretically ambiguous.

(Proof in Appendix A)

E. Testable Predictions

Proposition 4 maps the model's recursive structure onto a set of observable cross-border predictions. Some follow directly from the direction of the comparative statics; others concern the pattern of effects across outcomes, which carries stronger diagnostic value.

Education: Propositions 1 and 4(a) jointly imply that women under stricter legal regimes accumulate more education. Proposition 1 establishes the pre-market channel: families invest more before the marriage market opens, because the expected return rises with anticipated marriage age. Stage 3 adds the within-marriage channel: women who marry later face less truncation of schooling already in progress. The pre-market channel also implies that the effect should be visible at the school-entry margin, since families who expect early marriage may withdraw their daughters from school before any marriage occurs.

Fertility timing and level: Proposition 4(b) predicts that age at first birth shifts upward under stricter enforcement, as delayed marriage delays the onset of childbearing. Proposition 4(c) and Lemma 4 predict lower completed fertility through two channels: women who enter marriage with higher education and human capital choose birth less often in period 1, and their higher Pareto weight shifts household decisions away from the man's preferred fertility outcome.

Skilled employment: Proposition 4(c) implies that skilled occupations should be more strongly affected than broad labour-force participation, which Lemma 3 leaves ambiguous below the thresholds. The mechanism is that the switch from birth toward schooling or work requires entry states to exceed $\bar{e}(\cdot)$ or $\bar{h}(\cdot)$, and the resulting human capital accumulation raises the returns to occupations where education has a steep gradient.

Urban and rural heterogeneity: The female wage function $w^f(h_0, e_0)$ has a steeper gradient in urban labour markets, where the returns to education are higher. On the lenient side, families forgo larger returns by underinvesting, so the cost of early marriage is higher in cities. The gain from switching to a strict regime is correspondingly larger in urban areas, operating through both the direct timing channel and the anticipatory investment channel. The model therefore predicts a wider strict-minus-lenient gap in education and skilled employment in urban areas than in rural areas, providing a within-sample test of the mechanism.

Exits from marriage: Delayed marriage raises both the outside option $\bar{V}^f$ (through higher (e_1, h_1) and lower n_1) and the within-marriage stay value ωV_2 (through higher ω and higher V_2). The net effect on observed union dissolutions is therefore theoretically ambiguous, as Proposition 4(e) states.

IV. Data

A. Demographic and Health Surveys

The individual-level data come from the Demographic and Health Surveys (DHS), a programme of nationally representative household surveys conducted across sub-Saharan Africa continuously since the early 1990s. Each survey round interviews women aged 15–49, collecting detailed information on marriage, fertility, education, employment, and household decision-making. The DHS is the primary source of comparable individual-level data on women's outcomes across low-income countries. A key feature of the DHS design is that respondents are assigned GPS coordinates at the cluster level, which allows us to locate each woman relative to international borders. We use these coordinates to assign each woman to a border pair and to compute her signed distance from the relevant legal cutoff.

B. World Policy Analysis Center

Legal minimum marriage ages come from the World Policy Analysis Center (WORLD), which tracks national family law across countries and years. We use WORLD to construct a panel of minimum legal marriage ages with parental consent for all sub-Saharan African countries from 1990 onwards. For each potential border pair, we verify the minimum age on each side and identify the years during which a legal age gap exists. A pair is included in the analysis only if the legal age difference has been in force for at least 20 years, ensuring that a sufficiently large cohort of women has been exposed to each regime. We restrict to pairs where one side has a minimum age of 18 or above and the other has a minimum age strictly below 18, creating a binary contrast between strict and lenient jurisdictions. The twenty-year threshold follows from the width of the contested age window. For most border pairs, that window spans only two or three years, and women pass through it quickly. A shorter exposure period concentrates the estimation on one or two thin birth cohorts, making results sensitive to survey timing rather than the legal environment. Twenty years typically spans multiple DHS rounds, giving the sample women from several distinct cohorts who were exposed to the legal regime at the relevant ages.

The 11 border pairs used in the analysis, along with their legal ages and exposure periods, are listed in Table 15 in the appendix. Figure 1 shows the geographic distribution of treatment and control countries across sub-Saharan Africa.

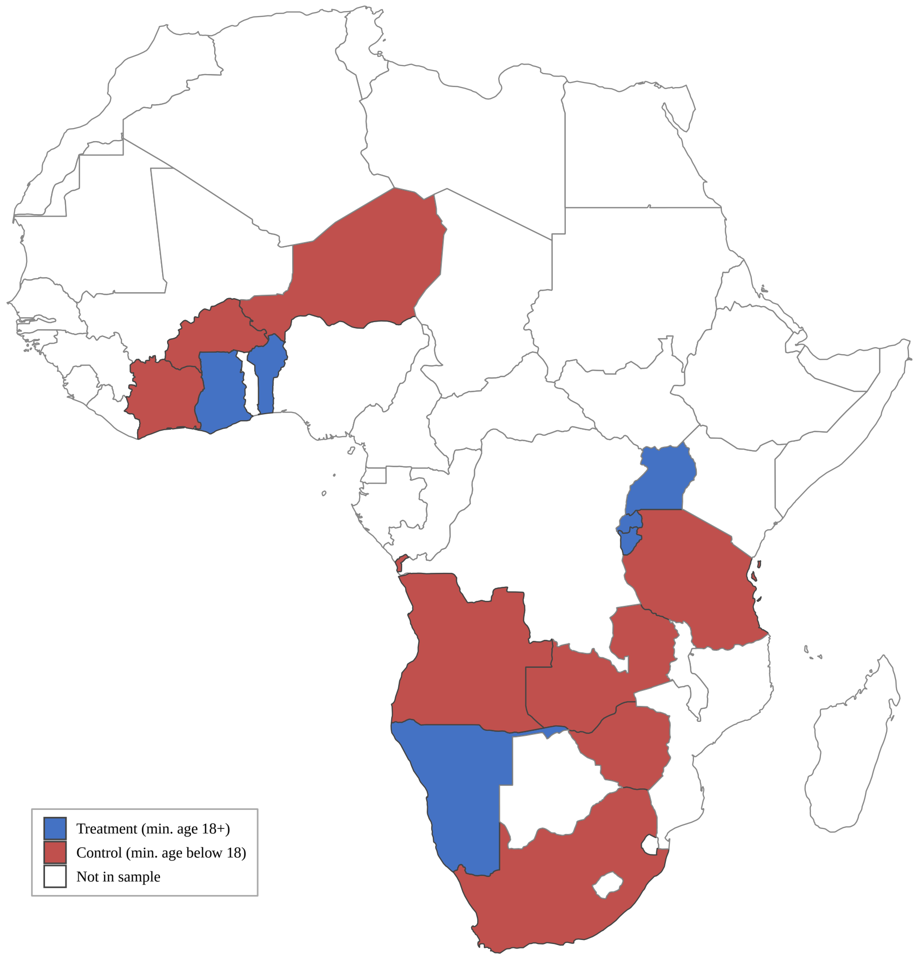

Figure 1: Treatment and Control Countries

C. Pixel-Level Geographic Controls

To absorb fine-grained variation in local development conditions near the border, we construct a set of pixel-level geographic controls matched to each DHS cluster using GPS coordinates. Controls are measured at the 12.5 km $\times$ 12.5 km level and drawn from three sources.

Nighttime light intensity comes from the Visible Infrared Imaging Radiometer Suite (VIIRS, 2022). Nighttime lights are a well-established proxy for local economic activity and infrastructure in sub-Saharan Africa, and pick up fine-grained variation in living standards close to the border that country-level income measures would miss. Population density comes from PRIO-GRID v3.0.0, which provides gridded estimates of population counts at the pixel level. Agricultural suitability comes from the SUIT dataset (ESRI GRID format), which measures the inherent capacity of land to support crop cultivation independent of current land use.

Two additional sources were collected but are not included in the main specifications. Wetland and water body coverage from the Global Lakes and Wetlands Database (GLWD v2.0) and the malaria ecology index of Kiszewski et al. (2004) showed insufficient variation within the narrow bandwidth around the border to contribute meaningfully to identification.

D. Ethnicity Data

Ethnicity fixed effects are constructed using the Murdock (1959) map of pre-colonial African ethnic homelands, the standard source for assigning ethnicity to geo-referenced observations in the African economics literature. Because many ethnic homelands straddle international borders, the Murdock boundaries are largely orthogonal to the national legal boundaries that generate our treatment variation. We assign each DHS cluster to an ethnic homeland by overlaying the GPS coordinates of the cluster on the Murdock shapefile, and include a fixed effect for each ethnic group in all specifications. This absorbs time-invariant cultural, linguistic, and institutional variation at the group level, including customary marriage practices and kinship structures.

V. Empirical Strategy

A. Identification

The paper estimates a fuzzy regression discontinuity design in which treatment assignment is determined by residence relative to an international border. Let RD_{ibet} denote the signed distance to the border cutoff for individual i in border pair b, ethnicity e, and survey year t. Positive values indicate residence on the treated side of the border. The estimation sample is restricted to individuals within a symmetric bandwidth around the cutoff ($|RD_{ibet}| \leq 100\,\text{km}$ in the main specification).

The estimand is the total causal effect of exposure to a strict child marriage legal environment on women's outcomes. As established in Section III, the enforcement cost of underage marriage is the only primitive that differs across the border, and it operates through two simultaneous channels (realised marriage timing and anticipatory pre-market investment, Stages 2–3 and 1 respectively). Because the anticipatory channel acts even on families whose daughters' realised child-marriage status would not be shifted by the law, the IV does not identify a LATE on realised child marriage. Instead, the IV rescales the reduced-form effect of legal exposure by the first-stage compliance rate, producing a Wald-scaled estimate of the total effect of the legal environment. The reduced-form estimates are accordingly the primary objects of inference; the IV magnitudes characterise this total effect for the population whose marriage behaviour is shifted by the legal discontinuity, but should not be interpreted as the effect of realised child marriage holding expectations fixed.

Let $f_b(RD_{ibet})$ denote a border-pair-specific slope control that allows the relationship between distance and outcomes to differ on each side of the cutoff within each border pair:

$$f_b(RD_{ibet}) = \alpha_b RD_{ibet} + \beta_b RD_{ibet} \cdot Treated_{ibet}, \tag{11}$$

where α_b is the slope on the control side and $\alpha_b + \beta_b$ is the slope on the treated side. This allows the running variable to kink at the cutoff, separately for each border pair.

First stage

$$CM_{ibet} = \alpha + \pi\, Treated_{ibet} + f_b(RD_{ibet}) + X'_{ibet}\gamma + \lambda_b + \delta_e + \tau_t + \varepsilon_{ibet}. \tag{12}$$

Reduced form

$$Y_{ibet} = \alpha + \rho\, Treated_{ibet} + f_b(RD_{ibet}) + X'_{ibet}\gamma + \lambda_b + \delta_e + \tau_t + u_{ibet}. \tag{13}$$

Second stage

$$Y_{ibet} = \alpha + \theta\, \widehat{CM}_{ibet} + f_b(RD_{ibet}) + X'_{ibet}\gamma + \lambda_b + \delta_e + \tau_t + \nu_{ibet}, \tag{14}$$

where CM_{ibet} is instrumented using $Treated_{ibet}$. Standard errors are clustered at the survey-cluster level.

B. Variable Definitions

The outcome variable Y_{ibet} covers six measures drawn from the DHS: an indicator for multiple unions, years of completed education, age at first birth,[1] total children ever born, number of living children, and age at first sex.[2] We additionally examine education level, an ordered categorical variable coded as 0 (no education), 1 (primary), 2 (secondary), and 3 (higher education). We also examine domestic-service employment. Domestic work is an indicator equal to one if a working woman reports domestic service as her primary occupation, and zero otherwise among working women.[3] We also construct two white-collar employment indicators. White-collar (extensive) equals one if the woman reports a professional or technical occupation, and zero otherwise, with non-working women coded as zero. White-collar (intensive) is defined analogously but restricted to working women only.[4] The treatment variable CM_{ibet} is an indicator equal to one if the individual married before age 18, and zero otherwise. The instrument $Treated_{ibet}$ equals one for individuals residing on the high-age side of the border. The running variable RD_{ibet} is the signed distance from the border cutoff, with positive values indicating residence on the treated side. The slope control $f_b(RD_{ibet})$ is a border-pair-specific function of the running variable that allows the slope to differ on each side of the cutoff within each border pair, as specified above. The control vector X_{ibet} includes age, and an indicator for urban residence. The fixed effects λ_b, δ_e, and τ_t correspond to border pair, ethnicity, and survey year, respectively. Together they absorb time-invariant differences across border segments and ethnic groups, as well as aggregate shocks that are common across survey rounds.

[1] Age at first birth is defined only for women who have given birth at least once; women who have not yet given birth are excluded from this outcome.

[2] Age at first sex uses DHS variable `v525`, retaining only numeric age responses. Responses coded as never had sex (0), at first union (96), inconsistent (97), do not know (98), or missing (99) are excluded. We do not recode the "at first union" response to age at first union in order to avoid mechanically linking this outcome to marriage timing. Table 20 in the appendix documents the share of censored observations and mean age at interview for both timing outcomes by treatment status; the larger share of not-yet-observed outcomes on the treated side is itself consistent with delayed fertility and sexual debut.

[3] Domestic work is defined only among working women since the question of occupational category is not meaningful for non-workers. The smaller observation counts for this outcome reflect this restriction.

[4] Working women with missing occupation codes are excluded from both white-collar outcomes rather than coded as zero, since their true occupational category is unknown. Coding them as non-white-collar would incorrectly classify them as low-skill workers and could bias the estimates downward.

C. Identifying Assumption

The first identifying assumption requires that residence on the high-age side of the border affects outcomes only through the child marriage legal environment, and not through any independent channel correlated with both the instrument and the outcomes. Our design follows the border-institution logic of Anderson (2018), which imposes the additional assumption that potential outcomes would vary smoothly at the border absent the discontinuity in the relevant legal environment. We address the standard threats to these assumptions through several complementary arguments.

The most direct threat is that the two sides of a border differ in cultural practices, kinship systems, or social norms in ways that independently shape marriage behaviour and downstream outcomes. The ethnicity fixed effects δ_e (constructed as described in Section IV) directly address this concern: because Murdock ethnic homelands straddle national borders, these fixed effects compare women within the same pre-colonial ethnic group on opposite sides of the legal threshold, holding kinship structure, customary marriage practices, and community norms constant.

A second concern is that the high-age country may simply be richer or better-served by infrastructure, producing better outcomes for women through channels unrelated to marriage law. To address this, every specification includes three pixel-level controls matched to each DHS cluster using GPS coordinates: nighttime light intensity from VIIRS, population density from PRIO-GRID, and agricultural suitability from the SUIT dataset, all measured at the 12.5 km $\times$ 12.5 km level. The main estimates are robust to the inclusion of these controls, which speaks against the development gradient as an alternative explanation. Border-pair fixed effects λ_b absorb remaining time-invariant differences at the border-segment level, and the border-pair-specific slopes $f_b(RD_{ibet})$ allow for smooth within-country gradients in conditions as a function of distance from the cutoff.

The migration robustness check in Section VII. A provides further support for the identification strategy: excluding individuals who migrated within three years before their marriage leaves the main estimates largely unchanged.

A potential concern is that minimum marriage age laws form part of a broader policy bundle, co-moving with compulsory schooling age, minimum employment age, and age of consent in ways the instrument would capture in their entirety. We address this directly using data from the WORLD Policy Analysis Center on compulsory schooling ages and minimum employment ages for all countries in our sample. Neither policy dimension co-moves systematically with the minimum marriage age across our 11 border pairs: the direction of differences varies across pairs, with some pairs showing the control country stricter, others the treatment country stricter, and several pairs identical. We also assess the broader legal environment using the World Bank's Women, Business and the Law indicators across five dimensions, reported in Table 19. The pattern is broadly balanced and does not show a systematic advantage on the treatment side. Two countries warrant specific attention. The four Namibia border pairs are the only pairs where the treatment country has both a stricter marriage age law and a stricter compulsory schooling age; the education results are robust to dropping these pairs (Table 11). Niger, as a control country, has weaker laws across multiple dimensions, meaning the control side in the Benin–Niger pair is doubly disadvantaged relative to the treatment side. The results are robust to dropping Niger border pairs as well (Table 12), confirming that neither outlier drives the main estimates.

One feature of the design deserves explicit discussion. The model's Stage 1 mechanism implies that the legal regime shapes families' pre-market education investment before any marriage occurs. This anticipatory channel does not violate the exclusion restriction: families underinvest because they anticipate early marriage, and they anticipate early marriage because the legal environment permits it. The investment response is entirely downstream of the enforcement cost and operates through no other channel. The exclusion restriction therefore holds at the level of the legal environment rather than at the level of the individual marriage decision.

Other threats regard multiple hypothesis testing and spatially correlated errors. These are addressed via Romano-Wolf correction and Conley standard errors in Section VII. Table 1 summarises these threats and responses.

Table 1: Identification Threats and Responses

Threat	Why it matters	How it is addressed
Ethnic and cultural differences	Marriage norms, preferences, and institutions may differ systematically across the border	Murdock (1959) ethnicity fixed effects compare women within the same pre-colonial homeland on opposite sides of the border
Development gradients	Richer or better-served areas may have better outcomes independently of legal regimes	Pixel-level controls for nighttime lights, population density, and agricultural suitability
Smooth spatial trends	Outcomes may vary continuously with distance from the border, confounding the discontinuity	Border-pair-specific linear slopes and kinks in distance from the cutoff
Policy bundle confound	Other child-protection laws (compulsory schooling, minimum employment age) may co-move with marriage age laws	WORLD Policy Analysis Center data show no systematic co-movement across 11 border pairs; Namibia and Niger pairs with co-moving laws excluded as robustness check (Table 11 and Table 12)
Migration around marriage	Women may cross the border to marry under a more favourable legal regime, violating the residence-based identification assumption	Migration robustness excludes women who migrated within three years before their marriage (Table 7)
Spatially correlated errors	Residuals may be correlated with nearby observations, understating standard errors	Primary clustering at the DHS survey-cluster level ($\approx$700 clusters); Conley spatial standard errors as robustness check (Table 9 and Table 10)
Multiple testing	Significance across many outcomes may reflect false discoveries rather than true treatment effects	Romano-Wolf stepdown correction controls the family-wise error rate within three pre-specified outcome families; results are robust (Table 8)

Notes: Each row states a potential threat to the identifying assumption that the border discontinuity in minimum legal marriage age is the only discontinuous determinant of outcomes at the cutoff. The right column describes the design feature or robustness check that addresses the threat.

D. Covariate balance.

Covariate balance tests are reported in Table 14 in Appendix A.3. Several covariates show statistically significant differences across the cutoff. Treated women are approximately one year younger on average. Nighttime light intensity is approximately three times higher on the treated side (treated mean 2.45 versus control mean 0.84, with an RD-adjusted difference of +0.95 at the 1% level), and population density is approximately four times higher (treated mean 7,398 versus control mean 1,873 people per km^2, with an RD-adjusted difference of +794 at the 1% level). Agricultural suitability is somewhat lower on the treated side. These imbalances motivate including the corresponding controls. Table 13 reports estimates that drop the geographic controls (nighttime lights, population density, and agricultural suitability) and the ethnicity fixed effects jointly; the sign and significance pattern is preserved across all outcomes and bandwidths, indicating that the main results are not driven by these covariates. The age imbalance is directly controlled for through the individual-level age control in X_{ibet}. We acknowledge, however, that these development-related imbalances are not fully resolved by these controls if the controls themselves are imperfect proxies for the underlying development gradient.

E. Sample and Border Pairs

The estimation sample is drawn from the DHS surveys described in Section IV, restricted to women within a symmetric bandwidth of the relevant border cutoff. The analysis covers eleven border pairs, listed in Table 15 in the appendix together with the minimum legal marriage age on each side and the period during which the cross-border legal difference is in force. Border pairs are included when four conditions are met: the two countries share a land border, their minimum legal marriage ages differ, at least one DHS survey round with GPS data is available for both sides of the border during the exposure period, and the legal age difference has been in force for at least 20 years, ensuring a sufficiently large cohort of women exposed to each regime. Two pairs that satisfy the legal criterion were excluded: the Central African Republic–Cameroon pair and the Republic of Congo–Angola pair, both of which lacked sufficient DHS coverage on one side of the border.

One pair, Rwanda–Tanzania, warrants a brief note. Rwanda's minimum legal marriage age was 18 prior to 2016, the same threshold as the majority of pairs in the sample. Following a 2016 reform, the minimum age was raised to 21. Despite this, the definition of child marriage is held constant throughout the analysis: a woman is classified as having married as a child if and only if she married before age 18, regardless of the legal threshold in her country of residence. For the Rwanda–Tanzania pair, we additionally restrict identification to cohorts of women who were aged 15–18 during the pre-reform period, when Rwanda's legal threshold was 18 and directly comparable to the other pairs. This ensures that the identifying variation for Rwanda–Tanzania comes from the same margin as the rest of the sample (the 18-year threshold) and is not contaminated by the post-reform discontinuity at 21.

The main specification uses a 100 km bandwidth around the cutoff, with 50 and 200 km serving as alternative specifications. Data-driven bandwidths were not used as these would yield different distances and polynomials for each outcome variable, altering the samples for each and would not ensure comparability. We exclude women aged under 18 with missing age at first marriage, as these observations cannot be used to construct the child marriage indicator. This restriction affects a small number of young never-married women and does not materially alter the sample composition.

F. Cohort Analysis

For each border pair, we retain only women who were within the early marriage-age window, defined as between the control country's minimum legal age and 18, during the years in which the cross-border legal difference was in force. For example, if the legal age gap between a pair operated from 1995 to 2023, and the relevant age window is 16–18, we include only women who were aged 16–18 at some point during 1995–2023. This restriction means that every woman in the estimation sample was directly exposed to the legal regime during the period when it was binding for her. Unlike designs that include women of all ages and rely on cohort variation to identify exposure, our sample is already restricted to the most plausible complier population by construction: women for whom the minimum legal age was a potentially binding constraint during their early marriage years. This makes the estimates more precisely targeted but also means cohort variation cannot be used as an additional source of identifying variation within the sample. Summary statistics for the full country samples appear in Tables 16 and 17 in the appendix.

VI. Results

A. Main Estimates

Table 2 reports the reduced-form estimates across all outcomes and bandwidth choices. Each coefficient measures the intent-to-treat effect of residing on the strict side of the border on the outcome. The main specification uses a 100 km bandwidth; the 50 and 200 km results serve as alternative specifications.

Table 2: Reduced-Form Effect of Treatment Across Bandwidths

	Bandwidth (km)		
Outcome	50	100	200
Human capital			
Education (years)	0.671***	0.695***	0.560***
	(0.237)	(0.202)	(0.180)
Education level	0.247***	0.266***	0.231***
	(0.045)	(0.039)	(0.035)
Fertility			
Age at first birth	0.907***	0.836***	0.821***
	(0.171)	(0.147)	(0.127)
Children ever born	−0.368***	−0.292***	−0.268***
	(0.080)	(0.065)	(0.055)
Number of living children	−0.293***	−0.225***	−0.199***
	(0.072)	(0.057)	(0.049)
Age at first sex	0.646***	0.453***	0.350***
	(0.126)	(0.114)	(0.102)
Household and labour market			
Multiple unions	0.011	0.001	0.001
	(0.013)	(0.010)	(0.008)
Not working	0.070**	0.067***	0.069***
	(0.029)	(0.022)	(0.019)
Domestic work	0.000	−0.007	−0.007*
	(0.004)	(0.005)	(0.004)
Paid in cash	−0.019	−0.002	−0.017
	(0.038)	(0.029)	(0.026)
White-collar (extensive)	0.030***	0.032***	0.026***
	(0.010)	(0.008)	(0.007)
White-collar (intensive)	0.043***	0.046***	0.039***
	(0.013)	(0.011)	(0.009)

Notes: Reduced-form estimates. The treatment indicator equals one for women residing on the high-age side of the border. Standard errors clustered at the DHS survey-cluster level in parentheses. All regressions include border-pair fixed effects, border-pair-specific slopes and kinks, pixel-level geographic controls, ethnicity fixed effects, and survey-year fixed effects. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Human capital: The education results operate through two channels, both introduced in the model and pulling in the same direction. The first is within-marriage truncation: marriage can truncate formal schooling, removing years on the steepest part of the accumulation curve. The second is pre-market underinvestment: families on the lenient side of the border, anticipating early marriage, invest less in daughters' education before the marriage market is ever reached. Figure 2 illustrates the two channels. Residing on the strict side raises educational attainment by 0.27 grade levels and 0.69 years at the 100 km bandwidth, significant at the 1 percent level and stable across bandwidths under cluster-level inference.

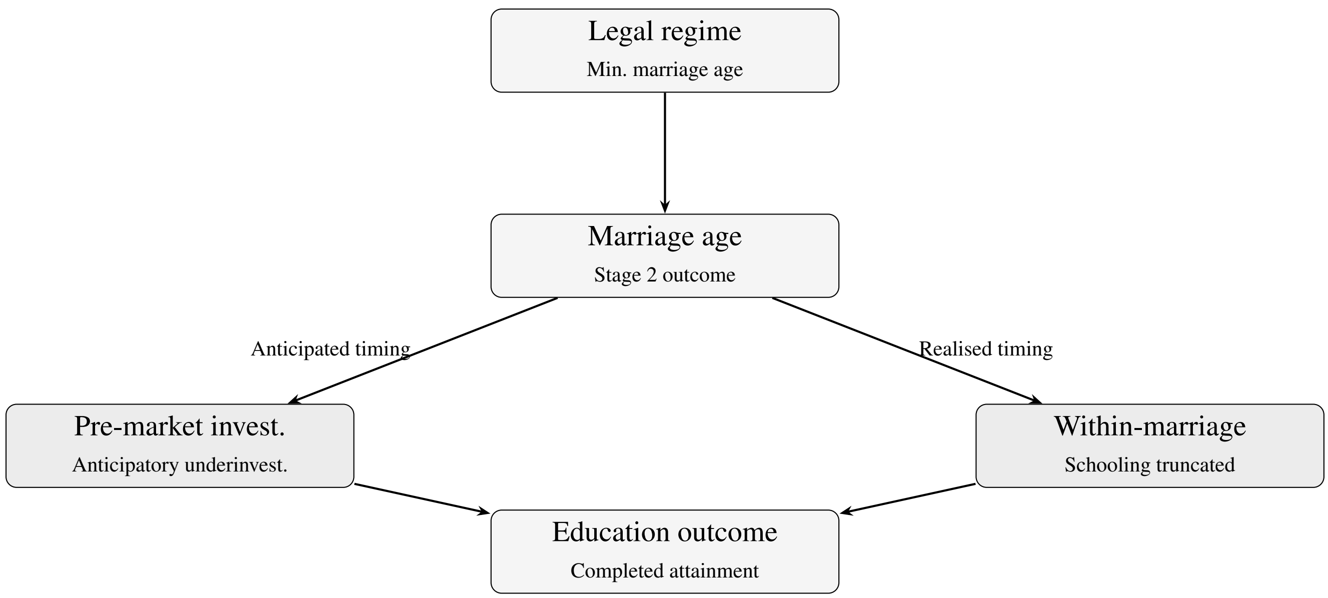

Figure 2: Two channels from the legal environment to education

Table 3 decomposes the education effect into extensive and intensive margins.[5] Residing on the strict side raises the probability of receiving any education by 10.8 percentage points at the 100 km bandwidth, significant at the 1 percent level and stable across bandwidths. The estimated contrast appears to operate partly through the school-entry margin. Conditional on some schooling, the strict side adds 0.025 further years at 100 km, though this is not statistically significant. The extensive margin is more prominent in the decomposition than the intensive margin.

[5]The margin decomposition uses the same reduced-form specification as Table 2.

Table 3: Reduced-Form Effects: Education Margins

	Bandwidth (km)		
Outcome	50	100	200
Education (years)	0.671***	0.695***	0.560***
	(0.237)	(0.202)	(0.180)
Any education	0.094***	0.108***	0.096***
	(0.025)	(0.022)	(0.020)
Education (years $\mid$ schooling > 0)	0.127	0.025	0.067
	(0.219)	(0.177)	(0.151)
Education level	0.247***	0.266***	0.231***
	(0.045)	(0.039)	(0.035)

Notes: Reduced-form estimates. The treatment indicator equals one for women residing on the high-age side of the border. Standard errors clustered at the DHS survey-cluster level in parentheses. All specifications include border-pair fixed effects, border-pair-specific slopes and kinks, pixel-level geographic controls, ethnicity fixed effects, and survey-year fixed effects. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Fertility: Women on the strict side of the border have their first birth 0.84 years later, have 0.29 fewer children ever born, and have 0.22 fewer living children, all at the 100 km bandwidth. These patterns are consistent with Stage 3 of the model: women who enter marriage with lower human capital and a weaker bargaining position face earlier and higher fertility. The age-at-first-birth effect is stable across all bandwidths, which is expected given the close link between marriage timing and onset of childbearing in this context.

Age at first sex is 0.45 years later on the strict side of the border at the 100 km bandwidth, significant at the 1 percent level. The model does not formally predict this outcome, but the direction is consistent with marriage timing as a driver of sexual debut.

The age at first sex variable is non-missing only for women who have had sex, so the sample drops women who had not yet had sex, a group likely correlated with treatment on the strict side. This sample restriction also weakens the first stage for this outcome (discussed in Section B). The reduced-form estimate should be read as the effect among sexually active women rather than the full population, which is why we treat it as auxiliary evidence rather than a core prediction.

Household and labour market: Multiple-union status shows no significant effect across any bandwidth. The model's prediction here is about exit from marriage, which Proposition 4(e) leaves theoretically ambiguous: delayed marriage raises both the woman's outside option $\bar{V}^f$ (through higher (e_1, h_1) and lower n_1) and her within-marriage stay value ωV_2 (through higher ω and higher V_2), and which shift dominates is not pinned down by the model. Multiple-union status is in any case an imperfect proxy for exit: it captures voluntary dissolution but also widowhood, polygamy, abandonment, and remarriage propensity, each of which may vary across our eleven border pairs and is not held constant by the legal marriage-age discontinuity.

On employment, residing on the strict side raises the not-working rate by 6.7 percentage points at the 100 km bandwidth ($p < 0.01$), rising to 6.9 points ($p < 0.01$) at 200 km. This implies that a stricter legal environment raises the share of women who are not working. The model is largely silent on broad labour-force participation as a clean prediction (Lemma 3 leaves it ambiguous because fertility at low entry states competes with work). The fact that the coefficient runs counter to a simple "bargaining raises participation" intuition indicates that the broad-participation outcome is not the right margin to test the model's mechanism. The model also abstracts from male income endogeneity, household consumption-smoothing, and selection into informal work, all of which could plausibly drive women on the lenient side into low-skill participation. Paid in cash shows no significant effect across bandwidths.

The white-collar results are more consistent with the model. Residing on the strict side raises the probability of white-collar employment by 3.2 percentage points on the extensive margin and by 4.6 percentage points among working women, both significant at the 1 percent level and stable across bandwidths. Because skilled occupations have steeper returns to education, they are the first margin affected by an accumulation loss. Domestic work shows no statistical significance at 100 km.

B. IV Estimates and First-Stage Diagnostics

The reduced-form estimates reported above capture the intent-to-treat effect of residing on the strict side of the border. We now turn to the IV-2SLS estimates, which scale these reduced-form effects by the first-stage compliance rate. Table 4 reports the first-stage estimates. The coefficient on *Treated* is approximately -0.15 across most outcomes: being on the strict side reduces the probability of child marriage by about 15 percentage points. First-stage F-statistics exceed 30 at the 100 km bandwidth for most outcomes. The exception is age at first sex, where the first-stage coefficient is approximately -0.09 and the F-statistic falls to 15.48 at 100 km, because the sample excludes women who have never had sex.

Table 5 reports the IV-2SLS estimates. Each coefficient divides the reduced-form effect by the first-stage coefficient, producing a Wald-scaled estimate of the total effect of the child marriage legal environment on the complier population. The IV coefficients are large: -4.5 years of education, -6.3 years to first birth, 1.9 additional children. These magnitudes follow mechanically from dividing modest reduced-form effects by a first stage of roughly 0.15.

We regard the reduced-form estimates as the main results and the IV coefficients as a supplementary scaling exercise. The reduced-form effects have a clear interpretation as the intent-to-treat effect of stricter legal exposure. The IV coefficients characterise what these effects imply for the population whose marriage timing responds to the legal discontinuity, but the modest first stage means that any residual bias in the reduced form is amplified by the same factor. Table 18 reports Kleibergen–Paap F-statistics and Anderson–Rubin p-values for all outcomes and bandwidths. The F-statistics exceed 30 at the 100 km bandwidth for most outcomes, well above conventional thresholds for instrument relevance. Due to the lower F-statistic of age at first sex, we additionally report AR p-values as a robustness check, since the AR test is valid regardless of instrument strength and provides an alternative basis for inference on the IV estimates. The AR p-values confirm the significance of the main results throughout.

Table 4: First-Stage Estimates by Outcome and Bandwidth

	First-stage coef.			F-statistic		
Outcome	50 km	100 km	200 km	50 km	100 km	200 km
Human capital						
Education (years)	-0.152^{***}	-0.153^{***}	-0.145^{***}	32.80	49.69	59.76
	(0.027)	(0.022)	(0.019)			
Education level	-0.152^{***}	-0.153^{***}	-0.146^{***}	32.83	49.87	59.85
	(0.027)	(0.022)	(0.019)			
Fertility						
Age at first birth	-0.133^{***}	-0.133^{***}	-0.140^{***}	22.49	34.16	49.57
	(0.028)	(0.023)	(0.020)			
Children ever born	-0.152^{***}	-0.153^{***}	-0.146^{***}	32.83	49.87	59.82
	(0.027)	(0.022)	(0.019)			
Number of living children	-0.152^{***}	-0.153^{***}	-0.146^{***}	32.83	49.87	59.82
	(0.027)	(0.022)	(0.019)			
Age at first sex	-0.103^{***}	-0.093^{***}	-0.098^{***}	12.25	15.48	23.04
	(0.029)	(0.024)	(0.020)			
Household and labour market						
Multiple unions	-0.151^{***}	-0.153^{***}	-0.146^{***}	32.45	49.63	59.80
	(0.027)	(0.022)	(0.019)			
Not working	-0.152^{***}	-0.154^{***}	-0.146^{***}	32.83	50.10	60.05
	(0.027)	(0.022)	(0.019)			
Domestic work	-0.166^{***}	-0.155^{***}	-0.152^{***}	31.97	45.20	56.86
	(0.029)	(0.023)	(0.020)			
Paid in cash	-0.166^{***}	-0.155^{***}	-0.152^{***}	31.91	45.29	56.90
	(0.029)	(0.023)	(0.020)			
White-collar (extensive)	-0.152^{***}	-0.155^{***}	-0.148^{***}	32.43	50.90	61.73
	(0.027)	(0.022)	(0.019)			
White-collar (intensive)	-0.167^{***}	-0.157^{***}	-0.155^{***}	31.43	46.31	58.92
	(0.030)	(0.023)	(0.020)			

Notes: Each row reports the outcome-specific first-stage coefficient on the treatment indicator from the regression of child marriage on treatment and controls, with clustered standard errors in parentheses, and the corresponding Kleibergen–Paap F-statistic for instrument relevance. All regressions include border-pair fixed effects, border-pair-specific slopes and kinks, pixel-level geographic controls, ethnicity fixed effects, and survey-year fixed effects. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5: IV-2SLS Effect of Child Marriage Across Bandwidths

	Bandwidth (km)		
Outcome	50	100	200
Human capital			
Education (years)	-4.421^{***}	-4.545^{***}	-3.849^{***}
	(1.424)	(1.244)	(1.161)
Education level	-1.626^{***}	-1.735^{***}	-1.586^{***}
	(0.297)	(0.270)	(0.241)
Fertility			
Age at first birth	-6.824^{***}	-6.291^{***}	-5.854^{***}
	(1.145)	(0.893)	(0.731)
Children ever born	2.422^{***}	1.904^{***}	1.839^{***}
	(0.407)	(0.302)	(0.262)
Number of living children	1.928^{***}	1.470^{***}	1.369^{***}
	(0.364)	(0.272)	(0.248)
Age at first sex	-6.272^{***}	-4.876^{***}	-3.577^{***}
	(1.683)	(1.207)	(0.923)
Household and labour market			
Multiple unions	-0.071	-0.006	-0.008
	(0.092)	(0.064)	(0.057)
Not working	-0.464^{**}	-0.439^{***}	-0.475^{***}
	(0.199)	(0.152)	(0.145)
Domestic work	-0.002	0.048	0.045^{*}
	(0.023)	(0.032)	(0.027)
Paid in cash	0.112	0.016	0.109
	(0.228)	(0.186)	(0.171)
White-collar (extensive)	-0.198^{***}	-0.208^{***}	-0.179^{***}
	(0.072)	(0.060)	(0.051)
White-collar (intensive)	-0.256^{***}	-0.293^{***}	-0.248^{***}
	(0.089)	(0.079)	(0.068)

Notes: IV-2SLS estimates. Child marriage is instrumented using residence on the high-age side of the border. Standard errors clustered at the DHS survey-cluster level in parentheses. All regressions include border-pair fixed effects, border-pair-specific slopes and kinks, pixel-level geographic controls, ethnicity fixed effects, and survey-year fixed effects. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

C. Heterogeneity by Urban and Rural Residence

Table 6 splits the reduced-form estimates by urban and rural residence and presents results at the 100 and 200 km bandwidths. The 50 km bandwidth is excluded for urban women because the urban subsample within 50 km of the border contains only 5,797 women across 11 border pairs, which is too small for reliable inference. The education effects are significant in both subsamples but substantially larger in urban areas. Residing on the strict side raises education level by 0.4 levels for urban women and 0.24 for rural women at the 100 km bandwidth, both significant at the 1 percent level. Educational attainment also rises by 1.8 years at 100 km for urban women, compared with 0.5 years in rural areas.

A note of caution is warranted in reading these results. The pattern in education-years is not stable across bandwidths: the 200 km urban education-years estimate (0.87) is much smaller than the 100 km estimate (1.8) and is less statistically significant, while the rural estimate remains close to its 100 km value. The 100 km urban result is the largest single number in the heterogeneity table and is correspondingly the most likely to reflect sampling variation.

The urban-rural pattern is consistent with at least two distinct mechanisms. One is steeper returns to education in urban labour markets: when $\partial w^f/\partial e_0$ is larger, the truncation of the convex accumulation path κT_m^γ at an early age entails a greater loss in lifetime human capital, and families on the urban side rationally invest more in κ to begin with, amplifying the attainment gap. A second is the reach of statutory law: urban areas have better access to formal legal institutions, so the minimum legal marriage age is more binding there, and the discontinuity at the border is correspondingly sharper. In rural areas, customary norms and weak state capacity mean the formal law reaches fewer people. These two mechanisms are observationally equivalent in our design and we cannot distinguish them.

The same two mechanisms apply to the fertility and white-collar results. Urban women on the strict side reach age at first birth 1.34 years later than urban women on the lenient side, against 0.72 years for rural women, and the white-collar employment effect is roughly double in urban areas on both margins. Both patterns are consistent with the model: stronger urban education effects push more women above the $\bar{e}$ and $\bar{h}$ thresholds in Lemmas 1 and 2, shifting period-1 choices away from birth and toward schooling or skilled work, and the steeper urban wage gradient amplifies the white-collar margin specifically. The urban-rural decomposition is therefore consistent across the chain from education to fertility to skilled employment.

Table 6: Reduced-Form Effects by Urban and Rural Residence

	Urban		Rural	
Outcome	100	200	100	200
Human capital				
Education (years)	1.804***	0.871*	0.501**	0.505***
	(0.510)	(0.518)	(0.195)	(0.177)
Education level	0.401***	0.255***	0.245***	0.236***
	(0.089)	(0.090)	(0.040)	(0.036)
Fertility				
Age at first birth	1.339***	1.181***	0.716***	0.696***
	(0.272)	(0.243)	(0.160)	(0.139)
Children ever born	−0.371***	−0.206*	−0.302***	−0.297***
	(0.105)	(0.108)	(0.070)	(0.060)
Number of living children	−0.349***	−0.164*	−0.209***	−0.209***
	(0.096)	(0.097)	(0.061)	(0.054)
Age at first sex	0.878***	0.755***	0.464***	0.365***
	(0.224)	(0.183)	(0.129)	(0.115)
Household and labour market				
Multiple unions	−0.032**	−0.035***	0.009	0.007
	(0.014)	(0.012)	(0.012)	(0.010)
Not working	0.076*	−0.005	0.076***	0.088***
	(0.043)	(0.034)	(0.026)	(0.022)
Domestic work	0.001	0.006	0.001	−0.000
	(0.019)	(0.016)	(0.002)	(0.002)
Paid in cash	0.036	0.034	−0.041	−0.053*
	(0.052)	(0.037)	(0.034)	(0.031)
White-collar (extensive)	0.050**	0.046**	0.022***	0.017***
	(0.024)	(0.022)	(0.006)	(0.005)
White-collar (intensive)	0.086**	0.065*	0.030***	0.023***
	(0.041)	(0.034)	(0.008)	(0.007)

Notes: Urban and rural subsamples defined using DHS urban–rural classification. Standard errors clustered at the DHS survey-cluster level in parentheses. All specifications include border-pair fixed effects, border-pair-specific slopes and kinks, pixel-level geographic controls, ethnicity fixed effects, and survey-year fixed effects. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

VII. Robustness Checks

A. Robustness: Controlling for Migration

A potential concern is that individuals migrate across borders specifically to contract an underage marriage, which would then contaminate our geographic discontinuity. To address this, Table 7 excludes individuals from the sample who migrated within three years before their marriage.

The pattern is nearly unchanged. Our general pattern in education, fertility and labour market outcomes are stable. The sample drops from approximately 54,213 to 49,416 observations at the 100 km bandwidth, indicating that a meaningful number of migrants were excluded while the main pattern remains similar.

B. Robustness: Romano-Wolf Multiple-Hypothesis Correction

As the results span multiple outcome families, a natural concern is that some significant findings may arise from testing multiple outcomes rather than genuine treatment effects. To address this, Table 8 reports Romano-Wolf stepdown p-values (Romano and Wolf, 2005, 2016) for the reduced-form estimates at the 100 km bandwidth, adjusted within three pre-specified outcome families: education, fertility, and labour-market outcomes. The Romano-Wolf procedure controls the family-wise error rate under arbitrary dependence across outcomes within each family, making it more conservative than uncorrected p-values.

The adjustment leaves the core findings intact. Education and fertility all retain significance after Romano-Wolf correction. In the labour-market family, not working and both white-collar outcomes remain significant at the 5 percent level, while domestic work is insignificant after correction. The close correspondence between clustered and Romano-Wolf p-values across all three families reduces concern that the findings are driven by multiple testing.

C. Robustness: Conley Spatial Standard Errors

Women near each other on opposite sides of the border may have correlated residuals even after controlling for ethnicity and geography, as they may share the same local labour market, infrastructure, and unobserved neighbourhood characteristics. Standard clustering at the DHS survey-cluster level accounts for within-cluster correlation but does not allow for cross-cluster spatial dependence.

We address this in two ways. Our first spatial-robustness specification, Table 9, reports the reduced-form estimates with Conley spatial standard errors (Conley, 1999) computed at a 100 km cutoff. This choice matches the main bandwidth and allows arbitrary correlation among observations within that radius. This is a rather conservative method, as this maintains the assumption that such characteristics are correlated.

The second approach uses a data-driven cutoff selected from the empirical residual covariogram (Lehner, 2026), specifically the first distance bin at which the smoothed residual covariance becomes non-positive, and is reported in Table 10. This lets the data determine the appropriate radius rather than imposing it. Lehner (2026) shows that the relationship between the bandwidth and the magnitude of spatial HAC standard errors is inverse-U shaped, implying that both too narrow and too wide bandwidths can lead to underestimated standard errors. This motivates using a data-driven selector rather than defaulting to the widest possible bandwidth.

Education level, age at first birth, children ever born, number of living children, age at first sex, and both white-collar measures retain significance at the 5 percent level or lower under both the Conley 100 km and data-driven spatial corrections at the main bandwidth. Not working also retains significance at the 5 percent level under both approaches. Education in years is the outcome most sensitive to the spatial correction: under Conley 100 km, the standard error (0.548) is more than double the DHS-clustered standard error (0.202) and the coefficient loses significance. Under the data-driven correction (Lehner, 2026), the standard errors are somewhat tighter (SE 0.404) and the coefficient recovers marginal significance at the 10 percent level, suggesting the fixed 100 km cutoff is conservative relative to the empirically selected radius.

D. Robustness: Excluding Namibia and Niger Border Pairs

Four border pairs (Angola–Namibia, Namibia–South Africa, Namibia–Zambia, and Namibia–Zimbabwe) share Namibia as the treatment country that has both a stricter minimum marriage age and a stricter compulsory schooling age than its control counterparts. This raises the concern that better outcomes near Namibia reflect the stronger education environment rather than the higher marriage age. If compulsory schooling were the driver, dropping these pairs should attenuate the education estimates. Table 11 shows the opposite: the education effect is slightly larger without Namibia, and fertility and white-collar results are stable. This rules out compulsory schooling as the primary driver of the education result for these pairs.

Niger also presents a concern. As a control country, Niger has weaker laws across multiple dimensions (no formal property rights for women, no right to own a bank account, and a low age of consent), meaning the control side in the Benin–Niger pair is doubly disadvantaged relative to the treatment side. This could exaggerate the apparent treatment effect by attributing to the marriage-age law what is in fact a broader legal disadvantage on the control side. Additionally, Niger has extreme summary statistics as seen in Table 16. Table 12 replicates the reduced-form estimates excluding Niger border pairs. The sign and significance pattern is preserved across all outcomes, with point estimates broadly similar to the main specification, suggesting the results are not driven by Niger's outlier legal environment.

Using data from the World Bank's Women, Business and the Law indicators and the WORLD Policy Analysis Center, we assess whether treatment and control countries differ systematically in their broader legal environment for women across five dimensions: age of consent, minimum compulsory education, women's property rights, the right to work outside the home, and the right to own a bank account. Table 19 summarises the comparisons across all 11 border pairs. The pattern is broadly balanced and does not show a systematic advantage on the treatment side. These issues are highlighted in Wilson (2022), which we make sure to account for.

E. Robustness: Excluding Geographic Controls

Table 14 shows imbalances in nighttime light intensity, population density, and agricultural suitability at the cutoff. Although these variables are included as controls in the main specification, one might worry that the results are sensitive to their inclusion, and may be driving our results. One could make a similar argument for our ethnicity fixed effects. We run our results without these controls and fixed effects.

Point estimates are somewhat larger without geographic controls (education and some fertility effects are amplified), suggesting that development-related covariates absorb some variation correlated with both treatment and outcomes. The sign and significance pattern is preserved across all outcomes and bandwidths, confirming that the main results are not driven by the geographic vector.

VIII. Discussion

The reduced-form estimates in Section VI show that women on the strict side of the border have more education, later first births, fewer children, later sexual debut, and greater access to skilled employment. The estimates are not equally informative for every outcome. The most stable findings, and the ones closest to the model's mechanisms, are educational attainment, age at first birth, children ever born, living children, and white-collar employment. Broad labour-force participation also shifts significantly, but the model leaves its sign ambiguous and the empirical pattern is not easily mapped onto the bargaining channel. The evidence is weaker for paid-in-cash employment, domestic work, and multiple unions, where the estimates are either small, statistically insignificant, or noisy across specifications.

A. Law, enforcement, and what the border design identifies

The model treats the legal regime as an enforcement wedge: the expected cost of contracting an underage marriage. The empirical design does not observe that wedge directly. It observes a discontinuity in the legal environment at the border and uses that discontinuity as an instrument for child marriage. The estimates show that stricter legal exposure changes marriage behaviour and lifetime outcomes near the border. They do not separately identify whether the effect comes from the statutory minimum age, actual enforcement, perceived enforcement, administrative barriers, or some combination of these.

The urban-rural heterogeneity is consistent with both a reach-of-law interpretation and a returns-to-schooling interpretation, and should motivate rather than establish the enforcement reading (Section VI. C).

The policy implication is therefore more limited than a simple enforcement claim. Legal environments with stricter minimum ages matter when they change the expected cost of underage marriage. That expected cost may come from courts, registration systems, sanctions, school reporting, community monitoring, or legal awareness. The paper does not estimate these channels separately. It shows that stricter legal environments are associated with lower child marriage near borders and with better outcomes across education, fertility, and skilled employment. The model explains why those effects may extend beyond marriage timing itself.

B. Limitations

Several limitations follow from the way the model and empirical design fit together.

The model separates pre-market investment, marriage-market timing, and household decisions after marriage. The empirical design estimates their combined effect. This is appropriate for the policy question, since a legal change can move all three margins at once. It is less informative about their relative importance. The education-margin decomposition supports the importance of school entry, but it does not observe parental expectations. The fertility estimates are consistent with a bargaining channel, but they do not separate bargaining power from the direct timing of marriage and childbirth.

The estimated effects apply to women whose marriage behaviour responds to the legal discontinuity: compliers who are likely close to the legal margin and whose outcomes may be especially sensitive to delayed marriage. The estimates should not be extrapolated to women whose marriages would occur early regardless of the law, or to settings where the legal threshold has no effect on behaviour.

The model gives bargaining power and outside options a central role, but the DHS outcomes are indirect. Fertility and white-collar employment are affected by bargaining in the model, but they are not direct measures of bargaining power. Isolating these channels separately would require data on separation, divorce, control over income, assets, household decisions, fertility preferences, and outcomes after separation.

The theoretical framework abstracts from divorce, endogenous matching, post-marriage migration, child investment, spousal age gaps, and heterogeneity in male characteristics. These omissions are useful for tractability, but they narrow the interpretation. A stricter legal regime may affect whom women marry as well as when they marry. It may also affect transfers, match quality, and household formation in ways the current model does not separately identify.

The sample covers eleven border pairs in sub-Saharan Africa. The border design is valuable because it compares nearby populations exposed to different legal regimes, but the estimates remain local to the countries, cohorts, and institutions in the sample. The mechanisms may apply elsewhere, but the magnitudes need not. They will depend on schooling systems, labour markets, customary marriage rules, bride-price practices, state capacity, and norms around separation and remarriage.

C. Future research

A direct test of pre-marriage investment would be the clearest next step. The current evidence points toward the school-entry margin, but it cannot observe the family decision that the model places before marriage-market entry. A stronger test would follow girls before marriage and record enrolment, attendance, grade progression, parental expectations, and local marriage-market conditions. The central question is whether expected marriage timing changes education decisions before any marriage occurs.

Future work should also measure enforcement rather than infer it from legal exposure. The model's enforcement wedge could be proxied with local data on birth registration, marriage registration, court distance, legal-aid access, administrative sanctions, school reporting rules, or knowledge of the statutory age. These measures would help separate the law on paper from the institutions that make the law binding.

A third extension is to study bargaining with better outcomes. Multiple unions is too crude a proxy for exits to carry the outside-option interpretation. Future work should use direct measures of household decision-making, control over income, asset ownership, fertility preferences, separation, divorce, and post-separation welfare. These data would help distinguish improved bargaining within continuing marriages from increased ability to leave marriages.

Another question is whether later interventions can offset some of the losses associated with early marriage. The model treats child marriage as producing a low entry state, but it does not imply that all later outcomes are fixed. Adult education, vocational training, childcare, contraception, legal aid, or cash transfers for already-married young women could raise human capital and outside options after marriage. The present paper cannot evaluate these policies, but they follow from the mechanism.

A structural extension would also be useful. A quantitative model with endogenous matching, spousal heterogeneity, bride-price transfers, divorce, and child investment could discipline counterfactual policy exercises. Such a model could compare statutory reform, enforcement, schooling subsidies, employment opportunities, and combined interventions. The value would be practical: to learn which margin explains the largest share of the reduced-form effects.

D. Policy implications

The results suggest three policy implications. First, minimum-age laws can matter when they change the effective environment in which marriage decisions are made. The evidence does not imply that raising the statutory age mechanically improves outcomes. Rather, it shows that women exposed to stricter legal regimes experience lower child-marriage rates and better outcomes in education, fertility timing, fertility levels, sexual debut, and white-collar employment. The relevant policy object is therefore not the legal text alone, but the extent to which the law changes the expected cost of underage marriage.

Second, legal reform is likely to be most effective when paired with enforcement and administrative capacity. In the model, the operative legal mechanism is the expected cost of underage marriage, which depends on whether households believe the rule is visible, credible, and costly to violate. In practice, this points to the importance of birth and marriage registration systems, local administrative capacity, access to legal information, and credible sanctions. A statutory threshold that is widely unknown or easily ignored is unlikely to produce the same effects as a legal regime that is actually enforced.

Third, child-marriage policy should be evaluated using a broader set of outcomes than marriage timing alone. The theoretical framework shows that marriage laws may affect families' decisions before marriage, women's bargaining position within marriage, and later-life outcomes after marriage. The empirical results are consistent with this broader chain: the strict-law side shows improvements not only in child marriage itself, but also in schooling, fertility, and skilled employment. This means that policy evaluations focused only on the number of marriages delayed may understate the full effect of legal regimes.

IX. Conclusion

The main takeaway from this paper is that minimum marriage-age laws matter most when they alter the environment in which families and marriage markets make decisions. The evidence does not support a simple interpretation in which statutory reform mechanically transforms women's outcomes. Instead, the results point to a more specific mechanism: when legal regimes raise the expected cost of underage marriage, they can delay marriage, increase the returns to investing in girls' education, and improve the conditions under which women enter married households.

This interpretation helps connect the empirical findings. The effects on education, fertility timing, fertility levels, sexual debut, and white-collar employment are not separate outcomes moving independently. They are consistent with a single process in which delayed marriage changes both pre-marital investment and post-marital household decisions. The legal regime appears to move outcomes most clearly where education and human capital are central.

The findings therefore suggest that child-marriage law should be viewed as part of a broader human-capital and household-bargaining environment, not only as a narrow family-law intervention. At the same time, the results should be interpreted with caution. The empirical design identifies the effect of exposure to stricter legal regimes near international borders, not the universal effect of raising a statutory age in every institutional setting. The policy relevance of the findings depends on whether legal thresholds are credible, visible, and enforceable enough to change household expectations.

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Appendix

This appendix contains supporting proofs, tables, and graphs.

A.1 Proofs

Proof of Proposition 1 The cross-partial $\partial^2 e_0/\partial\kappa\,\partial T_m = \gamma T_m^{\gamma-1} > 0$ for $\gamma > 1$, so the FOC left side is strictly increasing in the distribution of T_m. Since the integrand $\frac{\partial M}{\partial e_0} \cdot T_m^\gamma$ is non-negative and strictly increasing in T_m (given $\gamma > 1$ and $\frac{\partial M}{\partial e_0} > 0$), the expected marginal return is increasing under first-order stochastic dominance (FOSD) shifts in the distribution of T_m. If the strict regime induces a FOSD upward shift in the distribution of T_m, the FOC left side is strictly larger under the strict regime, so $\kappa^*_{\text{Strict}} \geq \kappa^*_{\text{Lenient}}$ by the implicit function theorem applied to the FOC. □

Equilibrium existence We establish existence constructively in three steps, treating families as atomistic price-takers with respect to the jurisdiction-specific marriage-age distribution μ^j, defined over $\mathscr{A}$.

Step 1 (Stage 1) For any fixed distribution μ^j, the family's investment problem is

$$\max_{\kappa\in[0,\bar{\kappa}]} \sum_{a\in\mathscr{A}} \mu^j(a)M(a;\kappa) - c(\kappa).$$

The feasible set $[0,\bar{\kappa}]$ is compact. Since $M(a;\kappa)$ is continuous in κ (it is a finite sum of terms continuous in $e_0 = e_b + \kappa T_m^\gamma$) and $c(\kappa)$ is continuous, the objective is continuous. By Weierstrass, an optimal $\kappa_j^*(\mu^j)$ exists.

Step 2 (Stage 2) Given κ_j^*, the marriage-market problem is finite-horizon over $\mathscr{A}$. Under Assumption 2, $\Delta^j(a;\kappa_j^*) \neq 0$ for all a, so the acceptance decision $\mathbf{1}\{\Delta^j(a) \geq 0\}$ is everywhere single-valued. The value function $V^j(a)$ and the resulting marriage hazard $h^j(a)$ are therefore uniquely determined by backward induction from $V^j(a_1) = \bar{V}$.

Step 3 (Stage 3) The household chooses from the finite action set $\{f_1, s_1, \ell_1\}$. Given (S_0,ω), optimal household choices and the resulting match value $M(a;\kappa_j^*)$ are well defined.

Aggregate consistency An equilibrium requires that the distribution families observe is the one generated by Stage 2 behaviour: $\mu^j = \mu^j(\kappa_j^*(\mu^j))$, where the right-hand side is the marriage-age distribution implied by the backward-induction solution to Stage 2. This is a fixed-point condition on the finite-dimensional simplex over $\mathscr{A}$. Since the mapping

$\mu^j \mapsto \kappa_j^*(\mu^j) \mapsto \mu^j$ is continuous (Weierstrass gives continuity of κ_j^* in μ^j under standard regularity, and the backward-induction solution is continuous in κ_j^* under Assumption 2), Brouwer's fixed-point theorem guarantees existence of an equilibrium distribution.

Proof of Proposition 2 and GE extension *Partial equilibrium*: Fix a and hold $V^j(a+1)$ and $M(a)$ constant. Since $\Delta^j(a) = M(a) - \Lambda^j(a) - V^j(a+1)$ is strictly decreasing in $\Lambda^j(a)$, a higher enforcement cost reduces the woman's net gain and weakly reduces the marriage hazard. Strict inequality holds when the feasibility constraint binds for one regime but not the other.

General equilibrium extension: The full GE result follows by backwards induction from $V^j(a_1) = \bar{V}$, and applies at ages where marriage occurs in at least one regime (i.e. $U_j^f(a) > V^j(a+1)$ for some j), which is the relevant case for the enforcement wedge to have bite. At $a_1 - 1$, $V^j(a_1)$ is fixed and the partial-equilibrium result applies directly. At $a_1 - 2$, the stricter wedge reduces the marriage hazard at $a_1 - 1$, which weakly lowers $V^j(a_1 - 1)$ under the strict regime (fewer marriages at $a_1 - 1$ remove the option value $U_j^f > V^j$ from the Bellman). A lower $V^j(a_1 - 1)$ enters $\Delta^j(a_1 - 2) = M(a_1 - 2) - \Lambda^j(a_1 - 2) - V^j(a_1 - 1)$ with a positive sign, partially offsetting the direct wedge effect. The net effect on $\Delta^j(a_1 - 2)$ is negative provided the direct Λ^j effect dominates this feedback, a condition satisfied whenever $\Lambda^j(a_1 - 2) \geq 0$ and the reduction in V^j is bounded by the option value removed. Restricting attention to ages below the legal threshold (where $\Lambda^j > 0$) ensures this dominance holds. The direction is preserved at each induction step, so the partial-equilibrium comparative static extends to equilibrium, resolving the circularity in the existence argument. □

Additional conditions for Assumption 5 The cross-point conditions required to close the proofs of Lemmas 1 and 2 are:

$$V_2(h_0 + \phi_s, e_0 + 1, 0) - V_2(h_0, e_0, 1) \text{ is strictly increasing in } e_0,$$
$$V_2(h_0 + \phi_\ell, e_0, 0) - V_2(h_0, e_0, 1) \text{ is strictly increasing in } h_0.$$

These conditions involve evaluations at different (h_1, e_1) points and are not implied by pointwise increasing differences alone; they are maintained assumptions.

Proof of Lemma 1 $\Pi_s - \Pi_f = -\omega\upsilon_f - (1-\omega)\upsilon_m - \omega\chi_s + \beta[V_2(h_0+\phi_s, e_0+1, 0) - V_2(h_0, e_0, 1)]$. The continuation term $\beta[V_2(h_0+\phi_s, e_0+1, 0) - V_2(h_0, e_0, 1)]$ is strictly increasing in e_0 by the cross-point condition above. The ω-dependent flow terms have derivative $(\upsilon_m - \upsilon_f - \chi_s) \cdot \partial\omega/\partial e_0 > 0$ under Assumption 4, so the ω-feedback reinforces the continuation term. Together, $\Pi_s - \Pi_f$ is strictly increasing in e_0, and the crossing condition implies the threshold $\bar{e}$ exists. □

Proof of Lemma 2 $\Pi_\ell - \Pi_f = \log(w^f + y_m + Rb_0) - \log(y_m + Rb_0) - \omega\chi_\ell - \omega\upsilon_f - (1-\omega)\upsilon_m + \beta[V_2(h_0+\phi_\ell, e_0, 0) - V_2(h_0, e_0, 1)]$. The wage term is strictly increasing in h_0 since $\partial w^f/\partial h_0 > 0$. The continuation term is strictly increasing in h_0 by the cross-point condition above. The ω-dependent flow terms have derivative $(\upsilon_m - \upsilon_f - \chi_\ell) \cdot \partial\omega/\partial h_0 \geq 0$ under Assumption 4, so the ω-feedback reinforces the other two terms. Together, $\Pi_\ell - \Pi_f$ is strictly increasing in h_0, and the crossing condition implies the threshold $\bar{h}$ exists. □

Threshold fixed points The threshold $\bar{e}$ in Lemma 1 is implicitly ω-dependent since $\omega = \Omega(b_0, h_0, e_0; \theta)$ is increasing in e_0, so the threshold condition $\Pi_s = \Pi_f$ defines a fixed-point equation in e_0. As shown in the proof of Lemma 1, both the continuation term and the ω-dependent flow terms in $\Pi_s - \Pi_f$ are increasing in e_0: the ω-feedback contributes $(\upsilon_m - \upsilon_f - \chi_s) \cdot \partial\omega/\partial e_0 > 0$ under Assumption 4, reinforcing rather than opposing the continuation term. The cross-point condition in Assumption 5 is therefore more than sufficient to guarantee existence and uniqueness of the crossing. The same applies to $\bar{h}$ in Lemma 2. □

Proof of Lemma 3 By Lemma 1, $e_0 > \bar{e}$ implies $\Pi_s > \Pi_f$, so $f_1 = 1$ is not chosen. By Lemma 2, $h_0 > \bar{h}$ implies $\Pi_\ell > \Pi_f$, so again $f_1 = 1$ is not chosen. This establishes (i). Parts (ii) and (iii) follow by comparing Π_s and Π_ℓ directly. Monotonicity follows because $\Pi_s - \Pi_f$ is increasing in e_0 and $\Pi_\ell - \Pi_f$ is increasing in h_0. □

Proof of Lemma 4 $\partial\Pi_f/\partial\omega = \upsilon_f - \upsilon_m$ and $\partial\Pi_s/\partial\omega = -\chi_s$, so $\partial(\Pi_f - \Pi_s)/\partial\omega = \upsilon_f - \upsilon_m + \chi_s < 0$ under Assumption 4. Analogously, $\partial(\Pi_f - \Pi_\ell)/\partial\omega = \upsilon_f - \upsilon_m + \chi_\ell < 0$. Since both differences are decreasing in ω, a lower ω makes Π_f more attractive relative to the alternatives. □

Proof of Proposition 3 By Assumption 6, higher a_m implies weakly higher (b_0, h_0, e_0). Two cases arise. First, if the higher entry state satisfies $e_0 > \bar{e}$ or $h_0 > \bar{h}$, Lemmas 1–3 imply birth is not chosen; instead schooling or work is selected, accumulating higher (e_1, h_1). By Assumption 5, V_2 is increasing in (h_1, e_1), and the optimal payoff Π^* is also increasing in (h_0, e_0) through both the wage $w^f(h_0, e_0)$ and the continuation value, so M is strictly higher. Second, if both marriage ages produce entry states below both thresholds so that birth is chosen in both cases, Lemmas 1–3 do not apply. Three effects operate: higher b_0 raises consumption and hence Π_f; higher (h_0, e_0) raises $V_2(h_0, e_0, 1)$; but higher (b_0, h_0, e_0) also raises ω, and since $\partial\Pi_f/\partial\omega = \upsilon_f - \upsilon_m < 0$, higher ω lowers Π_f. By Assumption 7, the positive b_0 and V_2 effects dominate. In both cases $M(a'; \kappa_j^*) \geq M(a; \kappa_j^*)$ for $a' \geq a$. □

Proof of Proposition 4 Part (a) follows from Assumption 6 and the monotonicity of e_0 in a_m through T_m^γ. Parts (b)–(c) follow from Lemmas 1–3: higher (e_0, h_0) implies lower Π_f relative to Π_s and Π_ℓ, shifting the period-1 choice away from birth. Part (d) follows from $\omega = \Omega(b_0, h_0, e_0; \theta)$ increasing in (b_0, h_0, e_0) combined with part (a). Part (e) follows directly: delayed marriage raises (e_1, h_1) and lowers n_1, each of which increases $\bar{V}^f$. □

Micro-foundation for Ω The monotonicity of $\Omega(b_0, h_0, e_0; \theta)$ can be derived from Nash bargaining in the household: the woman's outside option is increasing in (b_0, h_0, e_0), so her bargained share rises with her entry state. See Chiappori (1992) and Browning et al. (2014) for the collective household framework from which Ω can be micro-founded. We treat Ω as a maintained assumption here rather than deriving it from a specified period-2 bargaining problem.

A.2 Robustness checks

Tables 7–13 present the robustness checks described in Section VI. Table 7 controls for migration; Table 8 applies Romano-Wolf multiple-hypothesis correction; Table 9 uses Conley spatial standard errors; Table 10 uses data driven spatial standard errors; Table 11 excludes the four Namibia border pairs; Table 12 excludes the Niger pair; Table 13 excludes geographic controls.

Table 9 and Table 10 use R, which is why the point estimates are marginally different from Table 2, as all other results are done via Stata.

Table 7: Reduced-Form Effects, Restricting to Non-Migrants

	Bandwidth (km)		
Outcome	50	100	200
Human capital			
Education (years)	0.665***	0.654***	0.527***
	(0.234)	(0.206)	(0.187)
Any education	0.094***	0.103***	0.093***
	(0.024)	(0.023)	(0.020)
Education (years $\mid$ schooling > 0)	0.091	−0.008	0.027
	(0.214)	(0.180)	(0.156)
Education level	0.246***	0.257***	0.224***
	(0.044)	(0.040)	(0.036)
Fertility			
Age at first birth	0.864***	0.814***	0.817***
	(0.165)	(0.142)	(0.124)
Children ever born	−0.357***	−0.281***	−0.276***
	(0.080)	(0.065)	(0.055)
Number of living children	−0.283***	−0.213***	−0.204***
	(0.071)	(0.057)	(0.049)
Age at first sex	0.619***	0.462***	0.367***
	(0.133)	(0.116)	(0.102)
Household and labour market			
Multiple unions	0.014	0.003	0.004
	(0.014)	(0.011)	(0.009)
Not working	0.074**	0.071***	0.075***
	(0.029)	(0.022)	(0.020)
Domestic work	0.000	−0.008	−0.008*
	(0.004)	(0.005)	(0.004)
Paid in cash	−0.021	−0.001	−0.014
	(0.038)	(0.029)	(0.026)
White-collar (extensive)	0.028***	0.032***	0.026***
	(0.010)	(0.008)	(0.007)
White-collar (intensive)	0.040***	0.046***	0.039***
	(0.013)	(0.011)	(0.009)

Notes: The sample excludes women who migrated within three years before their marriage. Standard errors clustered at the DHS survey-cluster level in parentheses. All other specifications identical to the main reduced-form table. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 8: Romano-Wolf Multiple-Hypothesis Adjustment: Reduced-Form Estimates

Outcome	RF coef.	Clustered p	Romano-Wolf p
Panel A: Education outcomes			
Education (years)	0.695^{***}	0.000	0.004
Education level	0.266^{***}	0.000	0.001
Panel B: Fertility outcomes			
Age at first birth	0.836^{***}	0.000	0.001
Children ever born	-0.292^{***}	0.000	0.001
Number of living children	-0.225^{***}	0.000	0.001
Age at first sex	0.453^{***}	0.000	0.001
Panel C: Labour-market outcomes			
Not working	0.067^{***}	0.000	0.004
Domestic work	-0.005	0.291	0.212
White-collar employment	0.032^{***}	0.000	0.003
White-collar employment (intensive)	0.046^{***}	0.000	0.003

Notes: Reduced-form estimates at the 100 km bandwidth. The coefficient is the estimated effect of residence on the strict-law side of the border. All regressions include border-pair fixed effects, border-pair-specific slopes and kinks, geographic controls, ethnicity fixed effects, survey-year fixed effects, respondent age, and urban/rural controls. Clustered p-values use standard errors clustered at the DHS cluster level. Romano-Wolf p-values are stepdown adjusted within the outcome families in Panels A–C and control the family-wise error rate under dependence across outcomes. Stars on the coefficient are based on Romano-Wolf adjusted p-values: $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$.

Table 9: Reduced-Form Estimates with Conley Spatial Standard Errors: 100 km Cutoff

	Bandwidth (km)		
Outcome	50	100	200
Human capital			
Education (years)	0.671^{*}	0.725	0.563
	(0.388)	(0.548)	(0.537)
Any education	0.094^{**}	0.109^{*}	0.097^{*}
	(0.043)	(0.058)	(0.057)
Education (years $\mid$ schooling > 0)	0.127	0.051	0.058
	(0.369)	(0.438)	(0.383)
Education level	0.247^{***}	0.272^{***}	0.233^{**}
	(0.069)	(0.098)	(0.097)
Fertility			
Age at first birth	0.907^{***}	0.843^{***}	0.803^{***}
	(0.138)	(0.131)	(0.147)
Children ever born	-0.368^{***}	-0.290^{***}	-0.242^{***}
	(0.078)	(0.075)	(0.087)
Number of living children	-0.293^{***}	-0.223^{***}	-0.179^{**}
	(0.072)	(0.069)	(0.072)
Age at first sex	0.646^{**}	0.465^{**}	0.353^{**}
	(0.296)	(0.183)	(0.168)
Household and labour market			
Multiple unions	0.011	−0.002	0.001
	(0.013)	(0.010)	(0.010)
Not working	0.070^{*}	0.068^{**}	0.067^{*}
	(0.036)	(0.033)	(0.041)
Domestic work	0.000	−0.005	−0.004
	(0.005)	(0.006)	(0.007)
Paid in cash	−0.019	−0.003	−0.016
	(0.065)	(0.055)	(0.058)
White-collar (extensive)	0.034^{***}	0.036^{***}	0.029^{**}
	(0.011)	(0.013)	(0.012)
White-collar (intensive)	0.043^{***}	0.048^{***}	0.040^{***}
	(0.014)	(0.015)	(0.014)

Notes: Reduced-form estimates. The coefficient is the estimated effect of residence on the strict-law side of the border. Standard errors in parentheses are Conley spatial-HAC standard errors with a fixed 100 km cutoff. Stars are based on Conley 100 km standard errors. All regressions include border-pair fixed effects, border-pair-specific slopes and kinks, pixel-level geographic controls, ethnicity fixed effects, and survey-year fixed effects. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 10: Reduced-Form Estimates with Data-Driven Conley Spatial Standard Errors

	Bandwidth (km)		
Outcome	50	100	200
Human capital			
Education (years)	0.671**	0.725*	0.563
	(0.338)	(0.404)	(0.394)
Any education	0.094***	0.109**	0.097*
	(0.033)	(0.044)	(0.053)
Education (years | schooling > 0)	0.127	0.051	0.058
	(0.327)	(0.244)	(0.247)
Education level	0.247***	0.272***	0.233***
	(0.063)	(0.075)	(0.066)
Fertility			
Age at first birth	0.907***	0.843***	0.803***
	(0.167)	(0.141)	(0.149)
Children ever born	−0.368***	−0.290***	−0.242***
	(0.085)	(0.069)	(0.084)
Number of living children	−0.293***	−0.223***	−0.179***
	(0.070)	(0.060)	(0.063)
Age at first sex	0.646**	0.465***	0.353**
	(0.270)	(0.161)	(0.143)
Household and labour market			
Multiple unions	0.011	−0.002	0.001
	(0.014)	(0.010)	(0.009)
Not working	0.070**	0.068**	0.067*
	(0.033)	(0.029)	(0.038)
Domestic work	0.000	−0.005	−0.004
	(0.004)	(0.006)	(0.007)
Paid in cash	−0.019	−0.003	−0.016
	(0.053)	(0.047)	(0.050)
White-collar (ext.)	0.034***	0.036***	0.029***
	(0.010)	(0.010)	(0.009)
White-collar (int.)	0.043***	0.048***	0.040***
	(0.012)	(0.013)	(0.011)

Notes: Reduced-form estimates. The coefficient is the estimated effect of residence on the strict-law side of the border. Standard errors in parentheses are Conley spatial-HAC standard errors using a data-driven spatial cutoff selected from the empirical residual covariogram (Lehner, 2026). The selected cutoff is the first distance bin at which the smoothed residual covariance becomes non-positive, with residuals aggregated to the DHS-cluster level. All regressions include border-pair fixed effects, border-pair-specific slopes and kinks, pixel-level geographic controls, ethnicity fixed effects, and survey-year fixed effects. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 11: Reduced-Form Effects, Excluding Namibia Border Pairs

	Bandwidth (km)		
Outcome	50	100	200
Human capital			
Education (years)	0.671**	0.765***	0.618***
	(0.261)	(0.220)	(0.197)
Any education	0.102***	0.122***	0.107***
	(0.027)	(0.025)	(0.022)
Education (years $\mid$ schooling > 0)	0.026	0.010	0.075
	(0.259)	(0.201)	(0.174)
Education level	0.264***	0.293***	0.255***
	(0.049)	(0.043)	(0.038)
Fertility			
Age at first birth	0.905***	0.890***	0.903***
	(0.187)	(0.159)	(0.137)
Children ever born	−0.401***	−0.317***	−0.293***
	(0.089)	(0.072)	(0.061)
Number of living children	−0.322***	−0.242***	−0.213***
	(0.080)	(0.062)	(0.054)
Age at first sex	0.678***	0.464***	0.365***
	(0.143)	(0.128)	(0.115)
Household and labour market			
Multiple unions	0.011	−0.001	−0.001
	(0.015)	(0.011)	(0.009)
Not working	0.046	0.043*	0.030
	(0.031)	(0.023)	(0.019)
Domestic work	−0.001	−0.009*	−0.008*
	(0.004)	(0.005)	(0.004)
Paid in cash	−0.038	−0.020	−0.038
	(0.041)	(0.030)	(0.027)
White-collar (extensive)	0.036***	0.041***	0.034***
	(0.010)	(0.008)	(0.007)
White-collar (intensive)	0.044***	0.049***	0.040***
	(0.013)	(0.011)	(0.009)

Notes: The four Namibia border pairs are excluded from the sample. Standard errors clustered at the DHS survey-cluster level in parentheses. All other specifications identical to the main reduced-form table. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 12: Reduced-Form Effects, Excluding Niger Border Pairs

	Bandwidth (km)		
Outcome	50	100	200
Human capital			
Education (years)	0.666***	0.692***	0.567***
	(0.237)	(0.202)	(0.181)
Any education	0.094***	0.107***	0.097***
	(0.025)	(0.022)	(0.020)
Education (years $\mid$ schooling > 0)	0.126	0.025	0.066
	(0.219)	(0.177)	(0.151)
Education level	0.246***	0.265***	0.234***
	(0.045)	(0.039)	(0.035)
Fertility			
Age at first birth	0.909***	0.837***	0.829***
	(0.171)	(0.147)	(0.127)
Children ever born	−0.368***	−0.292***	−0.268***
	(0.080)	(0.065)	(0.055)
Number of living children	−0.292***	−0.225***	−0.199***
	(0.072)	(0.057)	(0.049)
Age at first sex	0.646***	0.453***	0.351***
	(0.126)	(0.114)	(0.102)
Household and labour market			
Multiple unions	0.011	0.001	0.002
	(0.013)	(0.010)	(0.008)
Not working	0.070**	0.067***	0.069***
	(0.029)	(0.022)	(0.019)
Domestic work	0.000	−0.008	−0.007*
	(0.004)	(0.005)	(0.004)
Paid in cash	−0.019	−0.002	−0.014
	(0.038)	(0.029)	(0.026)
White-collar (extensive)	0.030***	0.032***	0.027***
	(0.010)	(0.008)	(0.007)
White-collar (intensive)	0.042***	0.046***	0.039***
	(0.013)	(0.011)	(0.009)

Notes: Border pairs involving Niger are excluded from the sample. Standard errors clustered at the DHS survey-cluster level in parentheses. All other specifications identical to the main reduced-form table. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 13: Reduced-Form Effects, Excluding Geographic Controls and Ethnicity Fixed Effects

	Bandwidth (km)		
Outcome	50	100	200
Human capital			
Education (years)	0.467*	0.711***	0.591***
	(0.246)	(0.197)	(0.163)
Any education	0.080***	0.095***	0.089***
	(0.025)	(0.021)	(0.017)
Education (years $\mid$ schooling > 0)	−0.126	0.016	0.085
	(0.208)	(0.168)	(0.129)
Education level	0.222***	0.277***	0.253***
	(0.046)	(0.037)	(0.030)
Fertility			
Age at first birth	0.855***	0.825***	0.879***
	(0.165)	(0.125)	(0.096)
Children ever born	−0.360***	−0.326***	−0.331***
	(0.075)	(0.058)	(0.048)
Number of living children	−0.313***	−0.270***	−0.271***
	(0.066)	(0.052)	(0.043)
Age at first sex	0.470***	0.395***	0.375***
	(0.125)	(0.108)	(0.083)
Household and labour market			
Multiple unions	0.005	−0.008	−0.007
	(0.013)	(0.009)	(0.007)
Not working	0.050*	0.063***	0.063***
	(0.027)	(0.019)	(0.015)
Domestic work	−0.002	0.004	0.007*
	(0.004)	(0.005)	(0.004)
Paid in cash	0.008	0.036	0.027
	(0.038)	(0.025)	(0.020)
White-collar (extensive)	0.028***	0.034***	0.027***
	(0.009)	(0.007)	(0.005)
White-collar (intensive)	0.041***	0.051***	0.041***
	(0.013)	(0.009)	(0.007)

Notes: Nighttime light intensity, population density, and agricultural suitability are excluded from the control vector. Ethnicity fixed effects are also excluded. Standard errors clustered at the DHS survey-cluster level in parentheses. All other specifications identical to the main reduced-form table. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

A.3 Sample description and diagnostics

Covariate Balance Table 14 reports balance tests for predetermined covariates at the RD cutoff. The table shows raw differences between treated and control groups within 100 km of the border, estimated via the reduced-form RD specification without outcome controls. Several covariates show statistically significant differences. Treated women are approximately one year younger on average, and nighttime light intensity and population density are higher on the treated side. Agricultural suitability is lower on the treated side.

Two points bear on the interpretation of these imbalances. First, and most directly, we re-ran all main specifications after dropping nighttime lights and population density from the control vector entirely. The estimates are nearly identical to those in Table 2, suggesting that the development-related imbalance documented in Panel A is not mechanically driving the results. These robustness results are reported in Table 13. Second, the age imbalance reflects differences in cohort composition across the border and is directly controlled for through the individual-level age control in X_{ibet}.

Table 14: Balance of Covariates at the RD Cutoff

Variable	Control mean	Treated mean	RD difference	S.E.
Age	26.079	25.339	-1.045^{***}	(0.253)
Urban	0.288	0.261	-0.000	(0.039)
Nighttime lights	0.842	2.452	0.953^{***}	(0.139)
Population density (people/km^2)	1,873	7,398	794.1^{***}	(158.2)
Agricultural suitability	0.478	0.407	-0.050^{***}	(0.010)

Notes: The table reports control-group means, treated-group means, and the RD difference from a regression of each covariate on treatment, including border-pair fixed effects, border-pair-specific slopes, and treated-side kinks in distance to the border. Standard errors clustered at the DHS cluster level in parentheses. Population density is measured using 12.5 km $\times$ 12.5 km pixels and converted to people per km^2 by dividing the pixel population by 156.25. All estimates use observations within 100 km of the cutoff. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Sample and legal environment Table 15 lists all eleven border pairs. For each pair, the treatment country has the higher minimum legal marriage age. The exposure period is defined by the availability of DHS survey waves that overlap with the period during which the legal age difference was in place. For RWA–TZA, Rwanda raised its minimum age to 21 in 2016; identification for this pair uses only cohorts aged 15–18 during the pre-reform period, so the legal contrast at the 18-year margin is consistent across all pairs.

Table 15: Border Pairs, Legal Marriage Age Differences, and Exposure Period

Border pair	Treatment country	Control country	Period	Treatment age	Control age
AGO–NAM	Namibia	Angola	1996–2023	18	15
BEN–BFA	Benin	Burkina Faso	2004–2023	18	17
BEN–NER	Benin	Niger	2004–2023	18	15
BFA–CIV	Côte d'Ivoire	Burkina Faso	1995–2023	18	17
BFA–GHA	Ghana	Burkina Faso	1999–2023	18	17
BDI–TZA	Burundi	Tanzania	1995–2023	18	15
NAM–ZAF	Namibia	South Africa	1996–2023	18	15
NAM–ZMB	Namibia	Zambia	1996–2021	18	16
NAM–ZWE	Namibia	Zimbabwe	1996–2021	18	16
RWA–TZA	Rwanda	Tanzania	1995–2023	21	15
TZA–UGA	Uganda	Tanzania	1995–2023	18	15

Notes: Treatment country has the higher minimum legal marriage age (Treatment age). Control country has the lower minimum legal marriage age (Control age). For RWA–TZA, Rwanda raised its minimum age to 21 in 2016; identification uses only cohorts aged 15–18 in the pre-reform period.

Descriptive statistics Tables 16 and 17 report mean and median summary statistics by country. Treatment countries (minimum age $\geq$ 18) have lower rates of child marriage, later age at first birth, and higher education on average than control countries, consistent with the direction of the main results. The variation across countries in observation counts reflects differences in the number of DHS survey waves and the geographic extent of the sample within each bandwidth. We note that Niger is the smallest country sample (3,594 observations) and has the highest pre-18 marriage rate in the sample (86%). The results are robust to excluding Niger border pairs, as shown in Table 12.

Table 16: Mean Summary Statistics by Country

Country	Obs.	Age	Married	Before 18	First birth	Educ. (yrs)	Employed
Angola	21,650	25.36	0.63	0.47	18.35	5.86	0.54
Benin	29,155	20.72	0.51	0.50	18.61	4.33	0.57
Burkina Faso	66,550	24.34	0.72	0.54	18.85	3.33	0.68
Burundi	19,701	23.40	0.53	0.30	20.14	4.72	0.78
Ghana	19,423	25.51	0.59	0.34	20.23	7.43	0.72
Côte d'Ivoire	19,950	26.38	0.67	0.44	18.99	3.64	0.62
Namibia	42,062	21.69	0.25	0.32	19.16	8.78	0.36
Niger	3,594	18.97	0.71	0.86	16.78	2.56	0.19
Rwanda	30,248	24.08	0.47	0.14	21.58	5.60	0.76
South Africa	5,834	24.65	0.28	0.16	19.98	10.45	0.27
Tanzania	83,220	25.22	0.68	0.41	19.25	6.98	0.67
Uganda	20,358	23.55	0.67	0.50	18.38	6.63	0.73
Zambia	35,572	25.46	0.66	0.46	18.52	7.39	0.50
Zimbabwe	17,640	22.42	0.60	0.42	19.07	9.22	0.39
Treatment avg.		24.57	0.50	0.35	19.86	6.86	0.62
Control avg.		24.19	0.64	0.49	18.82	5.79	0.52

Notes: Mean values by country for the full sample. Treatment countries have minimum legal marriage age ≥ 18; control countries have minimum legal marriage age < 18. Child marriage is defined as first marriage or cohabitation before age 18 and is only defined for ever-married women. Employed is defined as one minus the not-working indicator. Age is respondent age at interview (15–49).

Table 17: Median Summary Statistics by Country

Country	Obs.	Age	Married	Before 18	First birth	Educ. (yrs)	Employed
Angola	21,650	24	1	0	18	6	1
Benin	29,155	20	1	0	18	4	1
Burkina Faso	66,550	23	1	1	18	0	1
Burundi	19,701	23	1	0	20	5	1
Ghana	19,423	25	1	0	20	9	1
Côte d'Ivoire	19,950	25	1	0	18	0	1
Namibia	42,062	21	0	0	19	9	0
Niger	3,594	19	1	1	17	0	0
Rwanda	30,248	23	0	0	21	5	1
South Africa	5,834	25	0	0	19	11	0
Tanzania	83,220	24	1	0	19	7	1
Uganda	20,358	23	1	1	18	6	1
Zambia	35,572	25	1	0	18	7	1
Zimbabwe	17,640	22	1	0	19	10	0

Notes: Median values by country. Age refers to respondent age at interview (15–49). Child marriage is defined as marriage before age 18 and is only defined for ever-married women. Medians are computed using available observations for each variable.

Instrument strength and weak-IV robust inference. Table 18 reports weak-IV diagnostics for all outcomes and bandwidths. The Kleibergen–Paap F-statistic is smaller at narrower bandwidths but remains above 10 in all cases. The Anderson–Rubin p-values provide weak-instrument-robust inference and are reported alongside the F-statistics. The AR p-values confirm that the significant results in Table 5 are not artefacts of instrument weakness.

Table 18: Weak-IV Diagnostics Across Outcomes

		Bandwidth (km)		
Outcome	Statistic	50	100	200
Human capital				
Education (years)	KP F	32.80	49.69	59.76
	AR p	0.005	0.001	0.002
Education level	KP F	32.83	49.87	59.85
	AR p	0.000	0.000	0.000
Fertility				
Age at first birth	KP F	22.49	34.16	49.57
	AR p	0.000	0.000	0.000
Children ever born	KP F	32.83	49.87	59.82
	AR p	0.000	0.000	0.000
Number of living children	KP F	32.83	49.87	59.82
	AR p	0.000	0.000	0.000
Age at first sex	KP F	12.25	15.48	23.04
	AR p	0.000	0.000	0.001
Household and labour market				
Multiple unions	KP F	32.45	49.63	59.80
	AR p	0.421	0.926	0.890
Not working	KP F	32.83	50.10	60.05
	AR p	0.014	0.002	0.000
Domestic work	KP F	31.97	45.20	56.86
	AR p	0.915	0.132	0.097
Paid in cash	KP F	31.91	45.29	56.90
	AR p	0.622	0.932	0.519
White-collar (extensive)	KP F	32.43	50.90	61.73
	AR p	0.002	0.000	0.000
White-collar (intensive)	KP F	31.43	46.31	58.92
	AR p	0.001	0.000	0.000

Notes: KP F is the Kleibergen–Paap rk Wald F-statistic for instrument relevance. AR p is the Anderson–Rubin weak-instrument-robust p-value for the null of no causal effect of child marriage on the outcome. All specifications use the same bandwidth-specific samples, controls, and fixed effects as the main IV table.

A.4 RDD Graphs

The figures in this section present visual diagnostics for the regression discontinuity design. For the main outcomes, we plot binned means against signed distance to the relevant international border, with negative values denoting the control side and positive values denoting the treated side. The dots are constructed from twenty equal-observation bins on each side of the cutoff, so that each bin contains approximately the same number of observations within its side of the border. The solid lines show separate linear fits estimated on either side of the cutoff within the 100 km bandwidth. Because the figures pool multiple border pairs with different baseline outcome levels, they are intended as descriptive diagnostics rather than formal evidence. Inference is based on the reduced form and IV specifications reported in the main text.

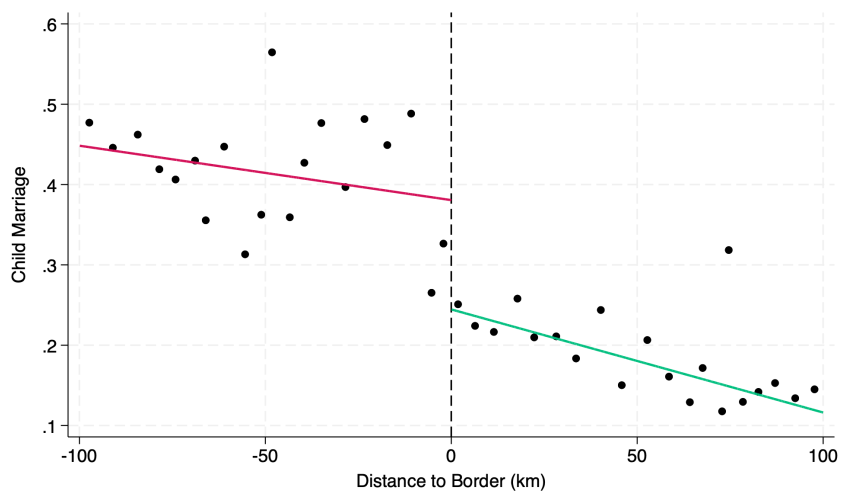

Figure 3: First Stage

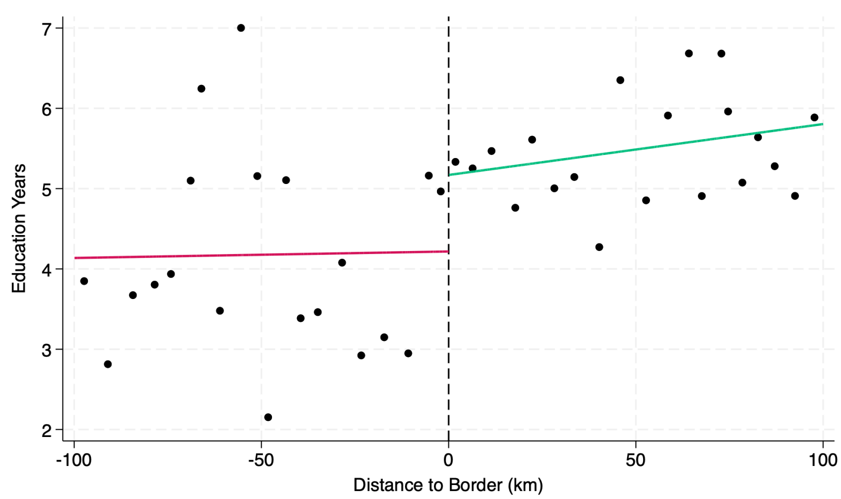

Figure 4: Education (Years)

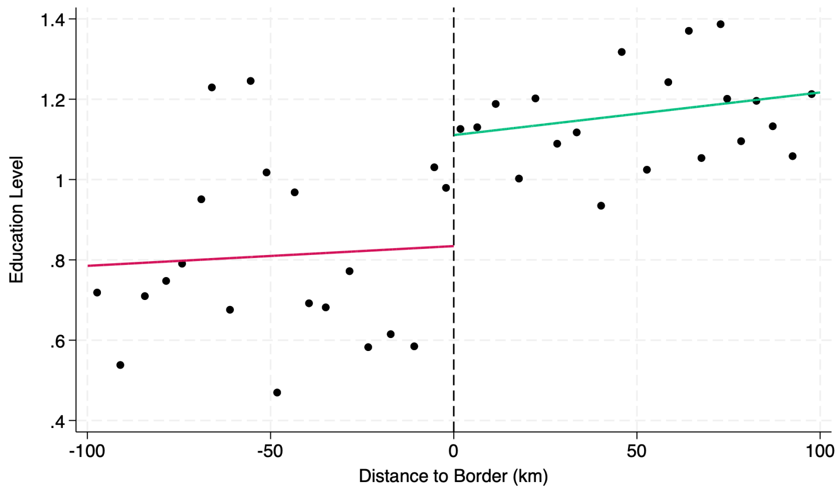

Figure 5: Education (Level)

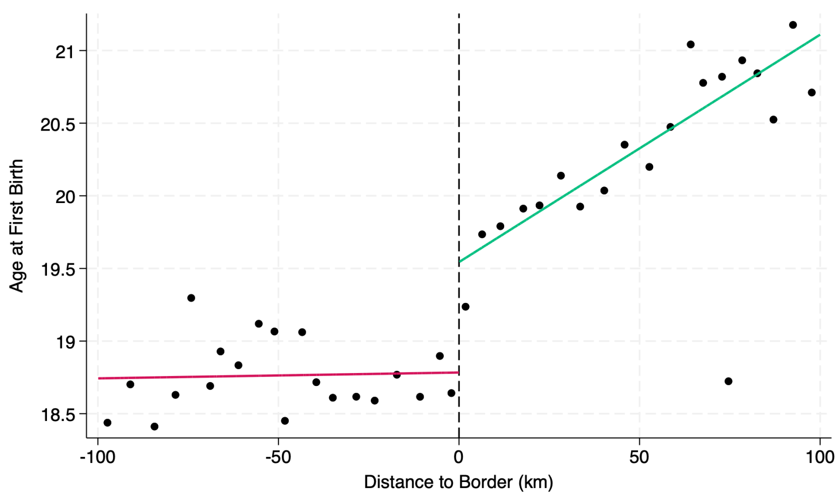

Figure 6: Age at First Birth

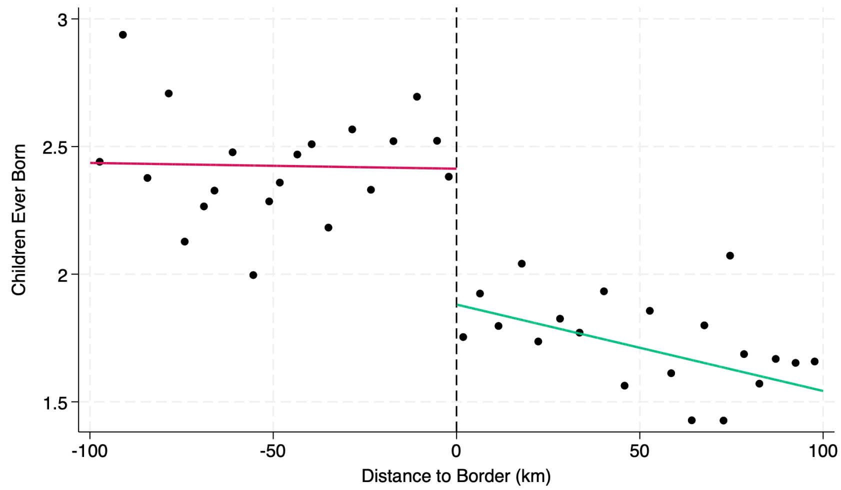

Figure 7: Children Ever Born

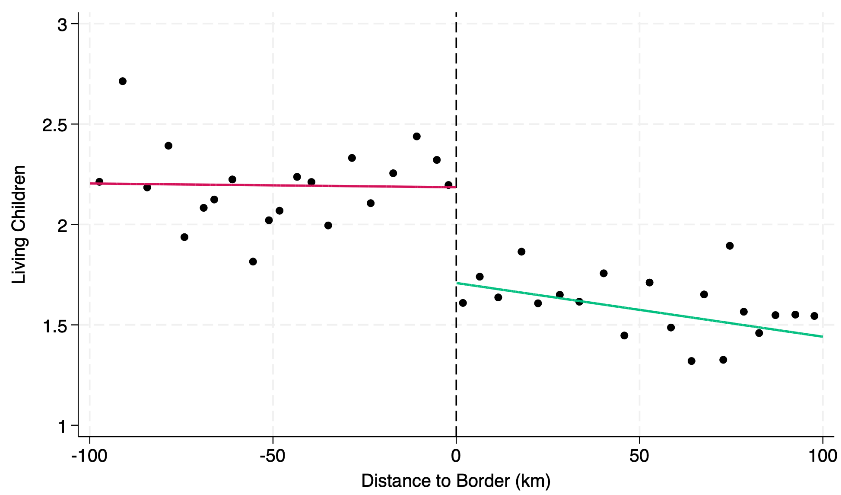

Figure 8: Living Children

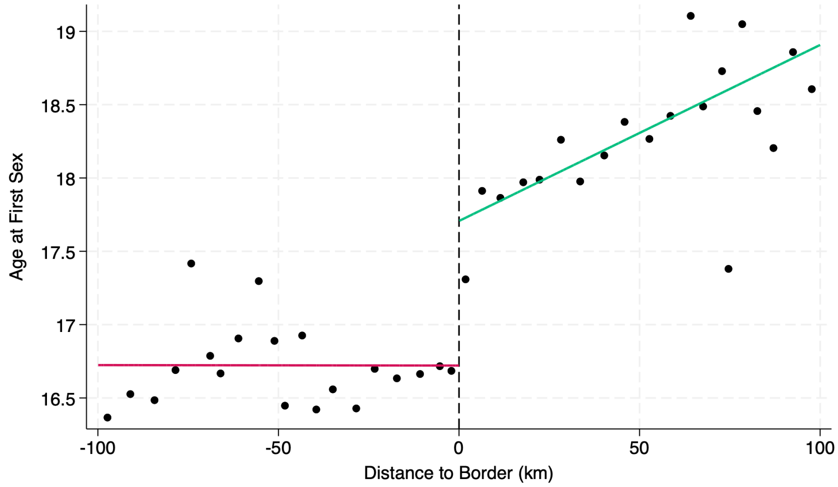

Figure 9: Age at First Sex

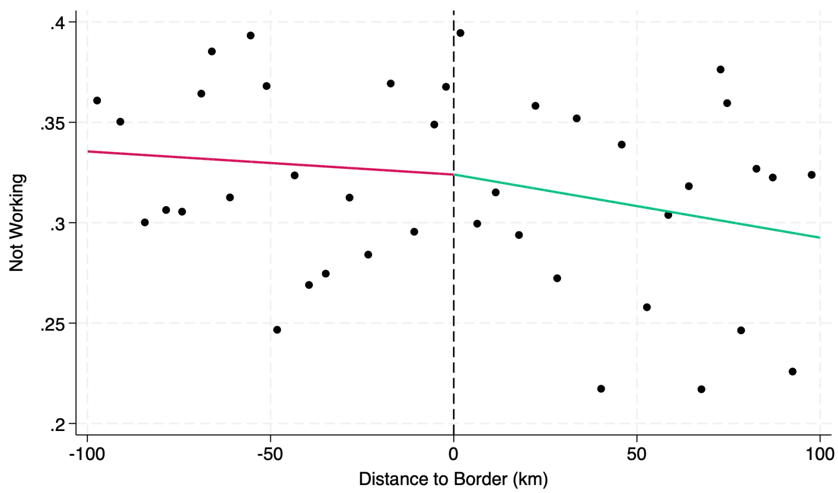

Figure 10: Not Working

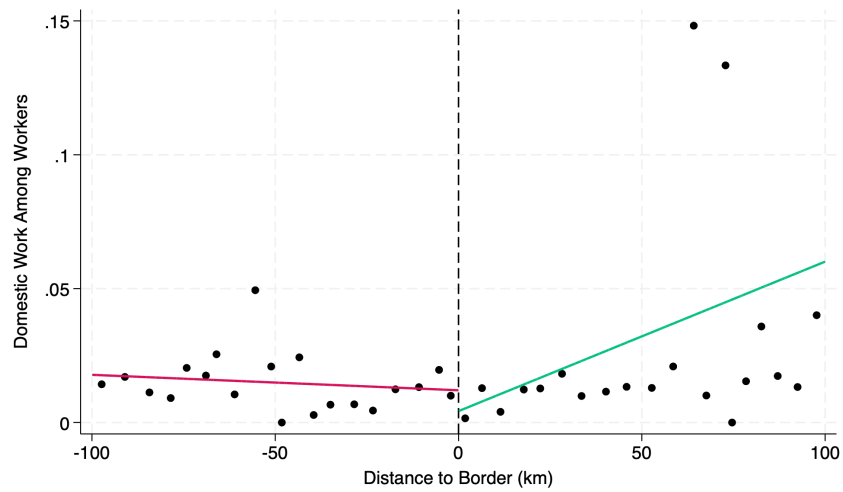

Figure 11: Domestic Work

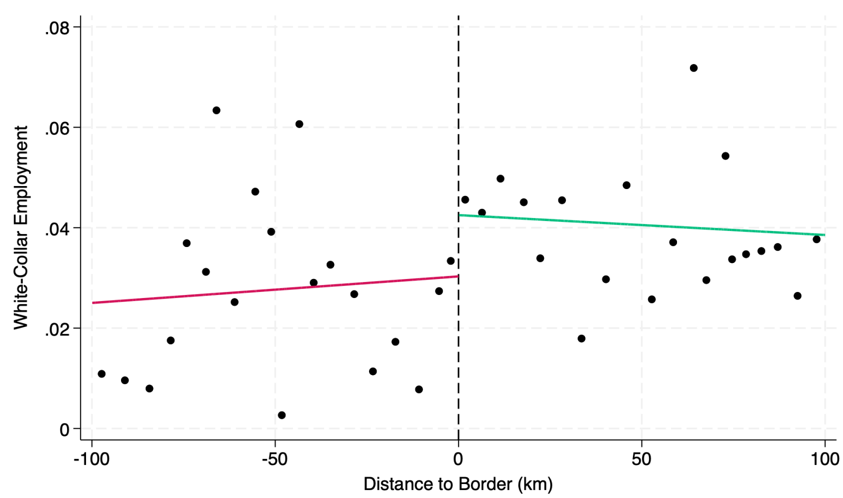

Figure 12: White Collar Extensive

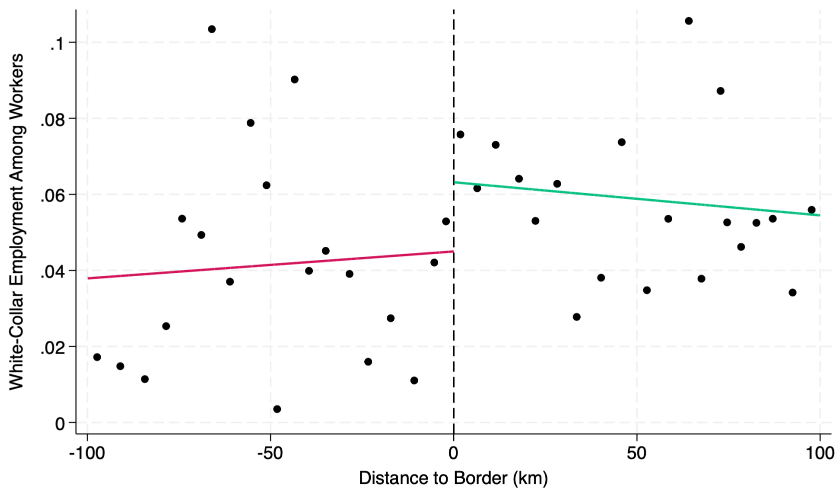

Figure 13: White Collar Intensive

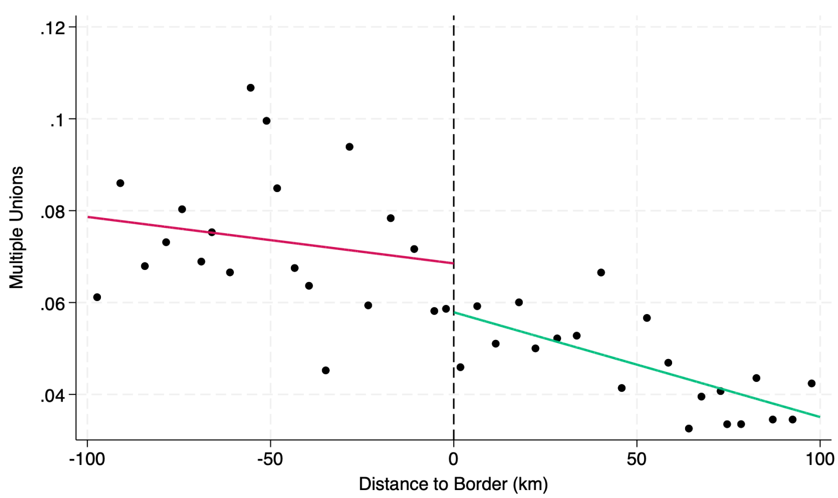

Figure 14: Multiple Unions

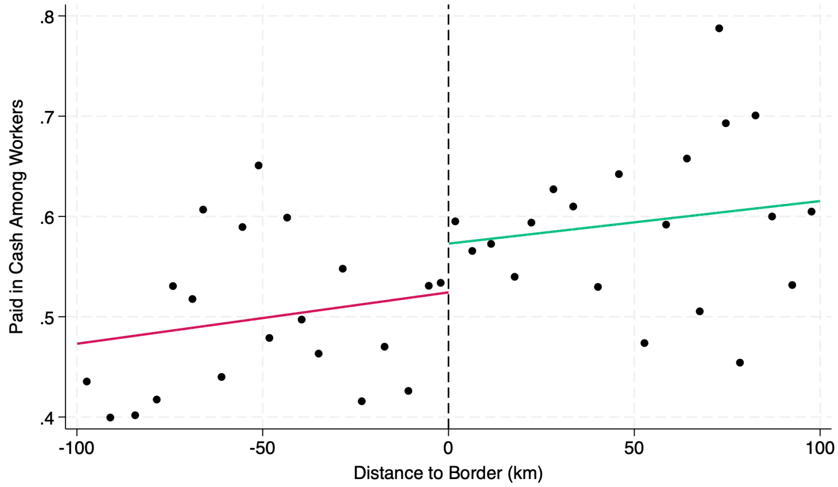

Figure 15: Paid in Cash

A.5 Different Laws and Policy

Table 19: Cross-Border Legal Comparisons Across 11 Border Pairs

Law	Treatment Higher	Control Higher	Same
Age of Consent	1	2	8
Minimum Education	4	3	4
Women's Property Rights	4	2	5
Right to Work Outside Home	1	4	6
Right to Own a Bank Account	1	0	10

Notes: Each row compares the legal environment on the treatment side (higher minimum marriage age) against the control side across all 11 border pairs. "Same" indicates that both sides have identical legal provisions. All 11 pairs have complete data across all five indicators. Sources: World Bank Women, Business and the Law; WORLD Policy Analysis Center.

Using data from the World Bank's Women, Business and the Law indicators and the WORLD Policy Analysis Center, we compare the legal environment on the treatment and control sides of each border pair across five dimensions: age of consent, minimum years of compulsory education, women's property rights, the right to work outside the home, and the right to own a bank account. Table 19 summarises the results across all 11 border pairs.

The pattern is broadly balanced. For the right to own a bank account, the two sides are identical in 10 of 11 pairs. The right to work outside the home is the same in 6 of 11 pairs, with control-side countries having higher protections in 4 pairs. Women's property rights and minimum education show more variation, but neither is systematically higher on the treatment side. Age of consent is the same in 8 of 11 pairs. Taken together, these indicators suggest that treatment and control countries are not systematically different in their broader legal environment for women, reducing concern that the main estimates are driven by concurrent differences in women's rights outside the marriage-law channel.

A.6 Missing Age at First Sex and Age at First Birth

Age at first birth and age at first sex are defined only for women who have experienced the event. Women who have not yet given birth or have never had sex are excluded from the respective outcome samples. Table 20 documents the share of such censored observations and the mean age at interview by treatment status. The treated side has a substantially larger share of women for whom the event has not yet occurred: 44 percent versus 32 percent for first birth, and 31 percent versus 17 percent for first sex. These women are not younger on average, which is consistent with genuine delay rather than a composition effect. The reduced-form estimates on both timing outcomes should therefore be interpreted conditional on observing the event. The larger share of censored observations on the treated side is itself consistent with the model's predictions of delayed fertility and sexual debut.

Table 20: Censoring in Timing Outcomes by Treatment Status

	Treated side		Control side	
Outcome not yet observed	Share	Mean age	Share	Mean age
No first birth yet	44.22%	19.20	32.17%	18.75
Never had sex	30.56%	18.49	17.00%	17.37

Notes: The table reports the share of women for whom the timing outcome has not yet been observed and their mean age at interview, separately by treatment status, within the 100 km bandwidth. Women with no first birth are excluded from the age-at-first-birth sample; women who have never had sex are excluded from the age-at-first-sex sample. Mean ages are computed at the time of the DHS interview.