Paper presented at the International Seminar for Family Demography and Family Law

The Selective Impact of Changes in Age-at-Marriage Laws on Early Marriage: Policy Challenges and Implications for Women's Higher-Education Attendance

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Abstract

This study explores the extent to which changes in age-at-marriage laws are effective in curbing early marriage and, if so, whether delays in age at marriage brought about by legal changes increase women's likelihood to participate in higher education. To answer these questions, we combine individual-level data from Demographic and Health Surveys (DHS) and Multiple Indicator Cluster Surveys (MICS) with longitudinal information on policy changes from the PROSPERED project for six low- and middle-income countries from three broad regions: Benin and Mauritania (Sub-Sharan Africa), Tajikistan and Kazakhstan (Central Asia), and Nepal and Bhutan (South Asia). We adopted regression discontinuity design to obtain estimates of the causal effect of changes in age-at-marriage laws on early marriage and educational outcomes. Our results suggest that these laws work only selectively – specifically, significant reductions in early marriage following the law implementation are observed only in two out of the six countries – yet when they work, their impact on early marriage has important implications for women's higher-education attendance. In Tajikistan and Nepal, an increase in the legal age at marriage by one or two years, respectively, leads to a 20-60 percentage-point higher likelihood of attending some form of higher education. In light of the significant human capital gains documented in countries where laws proved to have an impact, we conclude by arguing that, in order for changes in laws to be effective, better laws must be accompanied by better enforcement and monitoring to delay marriage and protect the rights of women and girls. Adequate policy implementation and enforcement are necessary preconditions for actual change and should be the subject of greater international attention and investments.

Keywords: Early marriage; Laws; Policies; Higher Education; Women's Status

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Introduction

Early marriage – defined as the marriage of children below 18 – is widely recognized in international human rights agreements as a harmful and discriminatory global practice. International governmental, academic, and advocacy stakeholders have called for countries to establish legislative frameworks that prohibit early marriage and close legal loopholes that permit marriage below the age of 18 (Human Rights Watch 2011, 2013; Jensen and Thornton 2003; Walker 2012). The disproportionately high rate of early marriage among girls compared to boys is also widely documented and recognized by the international scholarship and community as reflecting persistent gender inequalities (Pesando and Abufhele 2019; UNICEF 2014) and slow economic development (Vogelstein 2013).

Although not the direct target of a Sustainable Development Goal (SDG) *per se*, reducing early marriage is critical to achieving the majority of the SDGs set by the United Nations (UN). For instance, early marriage disproportionately affects rural and disadvantaged girls in low- and middle-income countries (LMICs) creating cycles of poverty that perpetuate inequalities (Dahl 2010; Otoo-Oyortey and Pobi 2003). Early marriage also keeps girls in poverty by depriving them of opportunities such as education and access to paid employment (Delprato et al. 2015; Field and Ambrus 2008; Sunder 2019). Women marrying in teenage years or younger have little say in terms of when they marry and whom they marry (Jensen and Thornton 2003) and have low post-marital agency within unions (Crandall et al. 2016; Yount, Crandall, and Cheong 2018), often resulting in frequent instances of domestic violence (Hindin and Fatusi 2009; Nasrullah, Zakar, and Zakar 2014; Rahman et al. 2014). Teen brides may also be unable to negotiate access to safe sex and medical care, leaving themselves vulnerable to health risks such as sexually transmitted infections and early pregnancies (Godha, Hotchkiss, and Gage 2013; Nour 2006), which in turn correlate with worse pregnancy outcomes for mothers (Ashcraft and Lang 2006; Fraser, Brockert, and Ward 1995; Ganchimeg et al. 2014; Raj 2010) and worse health outcomes for children (Efevbera et al. 2017).

In light of the sustained prevalence of early marriage and the array of negative consequences ensuing from it, there is heated discussion on the effectiveness of policies aimed at curbing the practice. Changes in age-at-marriage laws have featured prominently among the measures adopted by governments. Arthur et al. (2018) documented improvements in the frequency of countries to adopt legal provisions that prohibit marriage below the age of 18, and some research provides evidence of a significant association between protective laws and lower rates of early marriage (Bharadwaj 2015; Maswikwa et al. 2015), as well as declines in adolescent fertility rates (Kim et al. 2013). Conversely, looking at a sample of 60 countries, Collin and Talbot (2018) found that – despite increases in legal provisions – most countries are not enforcing the proposed laws, and enforcement is not getting better over time. These results suggest that renewed efforts to outlaw early marriage may not deter the practice, even where the incidence of early marriage is declining – a finding that is largely echoed in Kidman and Heymann (2016). In a similar spirit, Arthur et al. (2018) documented persistent widespread discriminatory provisions in legislation that disadvantage girls, alongside legal exceptions to minimum age provisions based on parental consent and customary/religious laws that create loopholes that lower the legal minimum age at marriage below the age of 18. A systematic review on legal interventions to curb early marriage found positive results in terms of decreasing the proportion married or increasing age at marriage in six cases, positive and negative findings in one case, and no statistical impacts on the proportion married or age at marriage in four other instances (Kalamar, Lee-Rife, and Hindin 2016). As such, the understanding of the proper functioning of these laws across diverse contexts remains inadequate.

Despite the complexities of identifying effective policies to prevent or reduce early marriage, this remains a key priority for scholars and policymakers concerned with raising children and women's status by boosting their human-capital opportunities. There's in fact a rich literature relating later age at marriage with positive educational outcomes in LMICs. For instance, using instrumental variable (IV) techniques to account for the potential endogeneity of early marriage, Delprato et al. (2015) found a delay in early marriage by one year to be associated with an increase of half a year of education in Sub-Saharan Africa, and nearly one third of a year in South-West Asia, as well as a lower likelihood of dropping out from secondary school of 5.5 percent in the latter region. Similarly, Field and Ambrus (2008) found each additional year that marriage was delayed in Bangladesh to be associated with 0.22 additional years of schooling and 5.6 percent higher literacy. Polyakova (2018) found that delaying marriage by a year in Nigeria was associated with an 8.9 percentage-point increase in the probability of obtaining some secondary education, and with a 10–11 percentage-point increase in the likelihood of completing secondary school. Lastly, a recent study from Uganda suggests that a one-year delay in marriage for women led to higher educational attainment (0.5–0.75 years), literacy (10 percentage points) and, ultimately, labor force participation rates (Sunder 2019).

Although perhaps implicit in some of the above findings, none of the above studies focuses specifically on higher education or university-related outcomes. This is surprising, as higher education is currently at the core of the post-2015 SDG agenda (target 4.3), which aims to ensure that by 2030 all women and men have "equal access to affordable quality technical, vocational and tertiary education, including university" (Ilie and Rose 2016; United Nations 2015). Although gender gaps in education at the primary and secondary school levels have considerably narrowed or even reversed (Grant and Behrman 2010; Psaki, McCarthy, and Mensch 2018), gender equality in education cannot be achieved so long as women are underrepresented in participation in higher education, which remains the case in many LMICs (Ilie and Rose 2016; Myers and Griffin 2019). This renewed emphasis on higher education is linked, among other reasons, to recent attention to its benefits for individuals' life-course trajectories and for societies, in ways that are argued to contribute to economic development, poverty reduction, and societal wellbeing (Bloom et al. 2014). Relatedly, the focus on gender gaps in higher education also calls for a better understanding of the drivers or root causes of these imbalances, together with the policy levers that might be adopted to tackle the issue.

Low levels of higher-education attendance among women in LMICs could be related to gender inequalities in access to higher education, alongside cultural factors influencing perceptions about the role of women within households and society. In many settings, women are expected to marry and have children early, thus limiting their opportunities to complete secondary school and/or achieve higher educational levels (Jensen and Thornton 2003). The lack of recognition of social status of women other than a wife and mother might imply that the expectation to formally participate in the labor market in such contexts might be low, deeming participation in higher education not a priority (Parsons et al. 2015). In light of the evidence reviewed thus far, there is ample reason to expect a delay in marriage age to increase women's chances to attend some form of higher education (McCleary-Sills et al. 2015; Otoo-Oyortey and Pobi 2003).

This study thus provides two contributions. First, we explore the extent to which changes in age-at-marriage laws are effective in curbing early marriage across six LMICs, namely Benin, Bhutan, Kazakhstan, Mauritania, Nepal, and Tajikistan. Second, in those contexts where laws prove to be effective, we further investigate whether delays in age at marriage brought about by legal changes might boost women's likelihood to participate in higher education. We do so by combining individual-level data from Demographic and Health Surveys (DHS) and Multiple Indicator Cluster Surveys (MICS) with longitudinal information on policy changes from the PROSPERED Child Marriage Database. Our findings are twofold. On the negative side, we find significant reductions in early marriage following policy changes only in two out of the six countries considered, namely Nepal and Tajikistan. On the positive side, our results suggest that in countries where changes in age-at-marriage laws are effective, women are significantly more likely to attend some form of higher education. In Tajikistan and Nepal, an increase in the legal age at marriage by one and two years, respectively, leads to a 20-60 percentage-point higher likelihood of attending higher education.

Taken together, our findings shed additional light on the mixed and highly context-specific effectiveness of policies aimed at curbing early marriage and suggest that, in order to be fully effective, changes in laws must be accompanied by close enforcement and monitoring to delay marriage and protect the rights of women and girls.

Data

We focus on six countries that passed a law increasing the legal minimum age at marriage since 1995 and that have a DHS or MICS conducted after the law implementation (refer to Table 1). We used the Policy-Relevant Observational Studies for Population Health Equity and Responsible Development (PROSPERED) database to identify these countries as well as years in which the respective laws were passed (Nandi, Vincent, and Atabay 2018). Subsequently, searching through other sources such as, for example, official governmental documents, we identified the month of the law implementation in each country.

Table 1: Details of the minimum age-at-marriage laws, datasets, country characteristics, all countries

Country	Survey	Date of Law Implementation	Minimum Age at Marriage		% In Union Before Age 18	% Entered Higher Education	Typical Age of Secondary School Completion
			After New Law	Before New Law			
Tajikistan	DHS 2017	07/2011	18	17	12,3	19,1	18
Nepal	DHS 2016	09/2002	18	16	49,1	15,4	18
Kazakhstan	MISC 2015	12/1998	18	17	7,8	40,0	18
Bhutan	MISC 2010	07/1996	18	16	30,6	3,2	19
Benin	DHS 2017	08/2004	18	15	35,5	2,4	19
Mauritania	MISC 2015	07/2001	18	at puberty (14)	35,2	3,9	18

Notes: For Nepal: age at marriage with parental consent; for the rest of the countries: age at marriage in general. The survey data do not permit to fully distinguish between marriage and cohabitation as they refer to unions in general. We use 14 as the age of puberty in Mauritania. The average age at menarche, which is a culmination of a process of puberty, tends to be between ages 13 and 15, depending on a country (Thomas et al. 2001). Since we could not identify the average age at menarche for Mauritania, we use age 14 as an approximation. Percentages (%) are weighted estimates calculated from each survey for women aged 20-49. Typical age of secondary school completion is calculated based on the information about the typical age of entrance into secondary school and the theoretical duration of secondary school from UNESCO (http://data.uis.unesco.org/).

In the process of the selection of countries, we excluded those: (i) that had surveys conducted only among ever-married women, (ii) for which we were unable to identify the exact date (year and month) of the law, and (iii) that had a survey conducted shortly after the law implementation and thus provided insufficient information about women who were affected by it. Our final sample consists of six countries from three broad regions: Benin and Mauritania (Sub-Sharan Africa), Tajikistan and Kazakhstan (Central Asia), and Nepal and Bhutan (South Asia). The minimum age at marriage prior to the law implementation differed between the countries. Following the law, women in all countries are legally allowed to marry no earlier than at the age of 18.

Both DHS and MICS are nationally representative surveys of women of reproductive age (15-49) and have similar designs, which facilitates the cross-national analysis. These surveys provide information about women's date of birth (month and year), age at first union, and level of education. We define early marriage as a first union that took place before the age of 18. Using information about educational attendance, we identify women who entered some form of higher (post-secondary, or tertiary, or university-level) education. In MICSs for Kazakhstan and Mauritania and in DHSs, the post-secondary level of education is called "higher"; in MICS for Bhutan it is called "college/university." The six countries covered by our analysis represent settings with diverse levels of early marriage and higher-education attendance, as presented in Table 1. The percentage of women who entered first union before the age of 18 ranged from around 8 in Kazakhstan to as high as 49 in Nepal, according to the listed surveys. While only 2% of women entered some form of higher education in Benin, 40% of women attended higher education in Kazakhstan.

Beyond casting light on the relationship between early marriage and higher-education attendance, our focus on post-secondary education has an important advantage. Since we classify early union as a first union before the age of 18 and women typically enter higher education at the age of 18 or later in all countries (refer to Table 1, typical age of secondary school completion), we minimize the risk that our results are influenced by reverse causality. Our research design ensures the correct temporal sequencing of the treatment and the outcome, the violation of which is a common concern in studies of the relationship between union formation and education (secondary education, in particular).

We focus on women who were at least 20 years old at the time of a survey in each country to make sure that all women included in our analysis passed the typical age of entrance into higher education (refer to Table 1, typical age of secondary school completion)

Methods

To assess the effectiveness of the laws, we examine whether increases in the minimum age at marriage to 18 reduced the prevalence of early marriage and, subsequently, whether this change affected women's higher-education attendance. For this purpose, we use a regression discontinuity design (RDD). In the context of our study, this quasi-experimental method is based on the idea that women who were aged just below and just above the minimum age-at-marriage at the time of the law implementation are comparable on both observed and unobserved characteristics, and differ only in terms of their exposure to the law. Relying on the assumption that women's exposure to the change in the law is exogenous, we aim to capture the causal effect of early marriage on the likelihood of attending higher education. The main advantage of this empirical strategy is that it accounts for both observed and unobserved characteristics that might affect whether women marry early and whether they enter higher education (endogeneity issue).

The first step of RDD involves establishing whether the minimum age-at-marriage laws reduced early marriage, i.e., whether there is a discontinuity in the probability of early marriage at the cut-off that distinguishes women who were exposed to the law from those who were not. Since the probability of early marriage is unlikely to fall to zero among women who were subject to the new law (i.e., it is likely that not all women will comply with the law and some will still marry before the age of 18), we employ a fuzzy RDD specification.

Fuzzy RDD is equivalent to an instrumental variable approach and can be implemented with the two-stage least squares (2SLS) regression (Angrist and Pischke 2009). In our case, the outcome variable is a binary indicator of higher education attendance and the treatment is a binary variable describing whether a woman married before the age of 18. The treatment is instrumented using a binary indicator of exposure to the minimum age-at-marriage law among women who were at least 20 years old at the time of the survey. The exposure indicator is described in the detail in the following sections. In the first stage, we regress the treatment indicator D on the instrument Z (Eq.1). In the second stage, we regress the outcome Y on the predicted value of the treatment D from the first stage. The estimate of interest is the local average treatment effect that can be interpreted as a causal effect for the subgroup that complies with the instrument (i.e. the new minimum age-at-marriage law).

$$D_i = \alpha_0 + \alpha_1 Z_i + \alpha_2 X_+ v_i \qquad \text{(Eq.1)}$$

$$Y_i = \beta_0 + \beta_1 \widehat{D}_i + \beta_2 X_+ \varepsilon_i \qquad \text{(Eq.2)}$$

We conduct and report the results of 2SLS, but our main analyses are based on bivariate probit models for two reasons. First, our endogenous variable (early marriage) as well as the outcome variable (higher educational attendance) are binary. Moreover, higher-education attendance in some countries covered by our analyses is relatively low (refer to Table 1). Since the probability of entering higher education is close to zero in some countries, the modelling approach that constrains the outcome variable to the plausible range of 0 to 1 might be more adequate than a linear model that can produce results beyond these bounds.

Bivariate probit model involves a simultaneous estimation of the probability of early marriage and higher education attendance. Instead of regressing the outcome Y on the predicted values of the treatment D, as in the second stage equation, it is implemented as a system of two equations that are modelled jointly. Similarly, as in the first stage of the 2SLS, in the first equation, we regress the binary early marriage indicator on a binary indicator describing women's exposure to the minimum age-at-marriage law. In the second equation, we regress a binary indicator describing women's education attendance on a binary early marriage indicator. In such a defined bivariate probit model, the correlation between the error terms of the first and the second equation (rho) allows testing for endogeneity of early marriage and higher education attendance.

Apart from the results of the 2SLS and bivariate probit models, we also present results from an OLS (linear probability models) and probit models that do not account for the endogeneity of early marriage. For the linear probability models, we report coefficients; for the probit models, we report marginal effects. In all of the models for Nepal and Benin, we control for women's religion (X in Eq.1 and Eq.2). These are the only two countries where the religion variable is available. We are unable to control for other pre-treatment characteristics in any of the countries, such as for example wealth or place of residence, because this information in DHS and MICS refers to that at the time of the survey.

Using information about the month and year of women's birth and policies' implementation, we group women into those whose ability to marry before the age of 18 was changed by the policy implementation ("treated", henceforth) and those who were not affected by the policy ("control", henceforth). We create a binary indicator which takes a values of 1 for women who were affected by the new law, and 0 for women who were not affected (Z in Eq.1). For instance, in Tajikistan the minimum age at marriage was raised by 1 year (from 17 to 18) in July 2011 (refer to Table 1). We classify women as treated if they were younger than exactly 17 years old at the moment of the policy, i.e., if they were still 16 in July 2011. These women were only exposed to the legal age at marriage of

18. We classify women as control if they were more than exactly 17 years old at the moment of the policy, i.e., women who were at least 17 years and 1 month old in July 2011. These women had an opportunity to legally marry before the age of 18. We exclude women who turned exactly 17 in July 2011, since we cannot determine whether that happened before or after the policy was implemented. We define the treated and control groups in a similar manner for the rest of the countries.

RDD requires choosing a width of the interval (bandwidth) that specifies which women around the cut-off defining the treated and control groups are included in the analysis. This bandwidth needs to be sufficiently large to ensure adequate sample size yet sufficiently small to ensure that control and treatment groups are comparable. For our main analyses, we present a 3-year (i.e., 36-month) bandwidth, but we conduct sensitivity analyses with 2- and 4-year bandwidths (i.e., 24- and 48-month bandwidths). We provide descriptive statistics for the control and the treatment groups as defined for the purpose of our main analyses in Table 2. Preliminary statistics already suggest that early marriage was lower among the treated, while higher-education attendance was higher.

Table 2: Details of the treated and control groups, all countries

	Age at Law Implementation		**N**		**% in Union Before Age 18**		**% Entered Higher Education**	
Country	**Treated**	**Control**	**Treated**	**Control**	**Treated**	**Control**	**Treated**	**Control**
Tajikistan	13-16	17-20	1,233	1,146	8,5	12,9	23,4	18,4
Nepal	12-15	16-19	1,241	1,128	45,1	53,1	22,0	13,0
Kazakhstan	13-16	17-20	1,206	1,142	6,7	10,7	49,8	39,0
Bhutan	12-15	16-19	1,468	1,462	29,7	32,9	4,8	3,6
Benin	11-14	15-18	1,902	1,627	36,6	38,7	3,0	2,8
Mauritania	10-13	14-17	1,573	1,398	37,9	35,7	3,9	3,7

Notes: Percentages (%) are weighted estimates. N: number of observations. Treated and control groups defined using the 3-year (36-month) bandwidth, which is the main specification used for the purpose of the analyses reported in Table 3 and 4.

Results

Effectiveness of changes in age-at-marriage laws

Figure 1 provides a graphical depiction of the fuzzy RDD specification described above, aimed at evaluating whether changes in age-at-marriage laws effectively reduced early marriage. Each graph reports the share of women – aged 20 and above at the time of the survey – who entered first union before the age of 18 in each country, as a function of the age at law implementation. The dotted lines correspond to the treatment group, i.e., those girls whose ability to marry was changed by the law, while the solid lines correspond to the control group, i.e., those girls who were unaffected by the law implementation. Figure 1 provides evidence of a clear discontinuity only in Tajikistan and Nepal, while the remaining panels do not provide sufficient evidence – and consistent-enough patterns – to conclude that the laws were effective in reducing early marriage.

These findings are confirmed by coefficient estimates reported in Table 3. These are bivariate associations predicting the probability of a girl entering a union before age 18 (dummy=1 if entered a union before age 18) as a function of treatment (dummy=1 if exposed to the new law). We report coefficients from both a linear probability model (LPM) and marginal effects from a probit specification. While the sign is consistently negative in all countries except for Mauritania, the estimates are only statistically significant in Tajikistan and Nepal. In these two countries, being exposed to the law reduced the likelihood of entering marriage before age 18 by 5 and 8 percentage points, respectively. Note that these estimates also serve as the first stage in an IV 2SLS approach, hence the F-test reported alongside the statistically significant coefficient estimates for Tajikistan and Nepal – above the conventional threshold of 10, especially in Nepal. Robustness checks with a wider and a narrower bandwidth are presented in Supplementary Materials Table S.1. The only difference between the three specifications is that the use of a 4-year bandwidth produces a significant decrease in the probability of early marriage in Kazakhstan and Bhutan as well. Nonetheless, the sensitivity of the results to the choice of the bandwidth (statistically insignificant change in case of 3- and 2-year bandwidths) and the lack of visible discontinuities for Kazakhstan and Bhutan in Figure 1 provide us with insufficient evidence to be able to claim that the laws reduced the probability of early marriage in these two countries.

Figure 1: Proportion (Prop.) of women who entered first union before the age of 18 by the age at the law implementation and their exposure to the law, women who were at least 20 years old at the time of the survey, weighted percentages, all countries

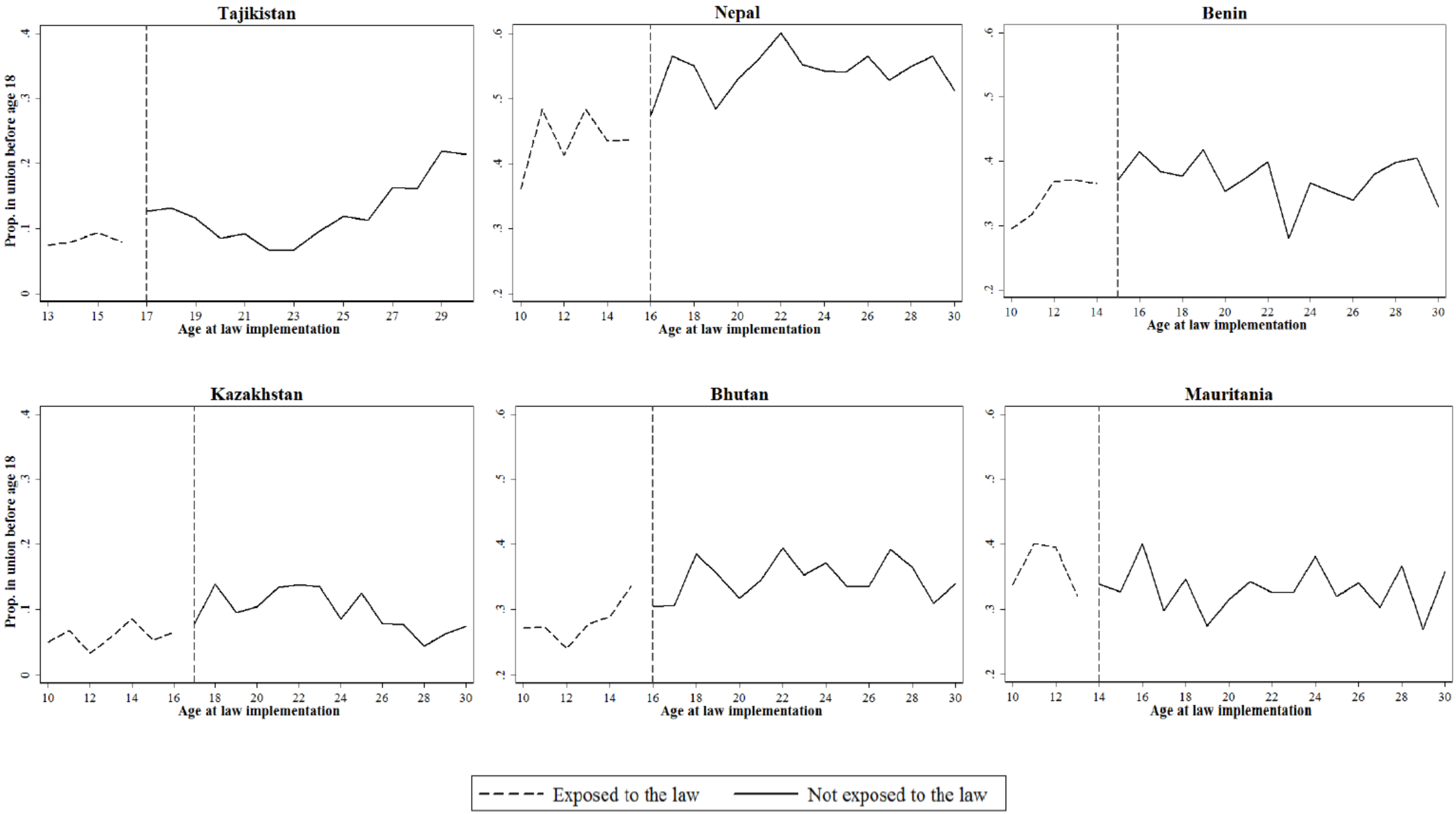

Table 3: Results of the OLS models (coefficients), F-statistics for statistically significant associations and probit models (marginal effects). Outcome: 1 – first union before age 18 (early union), 0- no first union before age 18 (reference category). Independent variable: 1 – treated, 0 – control (reference category), all countries

	LPM (Coefs)	F statistic	Probit (MEs)
	Early union		**Early union**
Tajikistan	-0.045**	10.95	-0.045**
	(0.014)		(0.014)
Nepal	-0.083***	17.30	-0.083***
	(0.020)		(0.020)
Kazakhstan	-0.021	-	-0.021
	(0.012)		(0.012)
Bhutan	-0.029	-	-0.029
	(0.017)		(0.017)
Benin	-0.015	-	-0.015
	(0.016)		(0.016)
Mauritania	0.016	-	0.016
	(0.017)		(0.017)

*** p<0.001, ** p<0.01, *p<0.05

Notes: Coefs: Coefficients; MEs: marginal effects. Control for religion in Nepal and Benin only, as these are the only countries where the variable is available. MEs for Nepal and Benin are calculated as average marginal effects. Results without controls for religion are essentially unchanged and available upon request. Robust standard errors in parentheses. Standard errors are clustered at the enumeration area level. P-values for Kazakhstan and Bhutan are p~0.08, hence we report it here as not statistically significant. The main reason is that we need a strong instrument to evaluate the impacts on higher-education attendance.

Implications for higher-education attendance

Table 4 provides results on whether changes in age-at-marriage laws increased women's likelihood to attend higher education in those countries in which the laws proved effective in curbing early marriage, namely Tajikistan and Nepal. We answer this question by means of a bivariate probit specification accounting for the plausible endogeneity of early marriage and provide complementary estimates from a simple probit neglecting the endogeneity issue. For these specifications, we report marginal effects. The second stage of the 2SLS approach obtained by a simple LPM is reported in Supplementary Materials Table S.2. Despite the level of consistency across the two estimation strategies – both suggesting that endogeneity is an issue of concern that ought to be taken into account – we take estimates from the bivariate probit as our preferred one. As expected, the LPM delivers predicted values that are outside of the plausible range, due to relatively low shares of women attending higher education (around 0.20 or less).

Our results provide evidence of a negative and statistically significant effect of entering early marriage on the probability of attending higher education. A simple probit model suggests that early marriage decreases the probability of attending higher education by 22 and 24 percentage points in Tajikistan and Nepal, respectively. Taking the endogeneity of early marriage into account by considering the role of changes in the laws, these effects are further magnified, as early marriage is associated with a lower probability of attending higher education by 35 and 59 percentage points. The results of the robustness checks with a wider and narrower bandwidth are very similar (refer to Supplementary Materials Tables S.3 and S.4).

Table 4: Results of bivariate probit models (marginal effects), Nepal and Tajikistan

		Bivariate Probit (MEs)		**Probit (MEs)**
		Early Union	**Higher Education**	**Higher Education**
Tajikistan	Treated (ref.: control)	-0.048***	-	-
		(0.012)		
	Early union (ref.: no early union)	-	-0.352***	-0.225***
			(0.014)	(0.019)
		biprobit rho=0.985***		
Nepal	Treated (ref.: control)	-0.097***	-	-
		(0.017)		
	Early union (ref.: no early union)	-	-0.587***	-0.244***
			(0.044)	(0.018)
		biprobit rho=0.865***		

*** p<0.001, ** p<0.01, *p<0.05

Notes: ME: marginal effects; Ref: reference category. Control for religion included in Nepal (variable not available in Tajikistan). MEs for Nepal are calculated as average marginal effects. Results without control for religion are essentially unchanged and available upon request. Robust standard errors in parentheses. Standard errors are clustered at the enumeration area level. The last column reports marginal effects from a simple probit specification which does not take into account the potential endogeneity of early marriage (for reference purposes only). Results from a simple 2SLS model – rather than bivariate probit – are reported in Supplementary Materials Table S.2. Results are fully consistent, yet predicted values are outside of plausible boundaries due to shares of women who enter higher education below or around 0.20.

We acknowledge that this is a big effect size, yet these coefficients need to be contextualized by taking some possible factors into account. First, the higher magnitude of the bivariate probit coefficients might suggest a downward bias deriving from either unobservable and omitted variables or measurement error in reporting age at marriage or marriage status ("attenuation bias"), which is far from infrequent in sample surveys (Dahl 2010). Second, and partly related to the above, the high magnitudes may shed concerns on the validity of the instrument. While there is no *a priori* theoretical argument to believe that changes in marriage laws are an invalid instrument for early marriage, this cannot be tested with the data at hand, hence the claim cannot be ruled out. Third, and perhaps most likely, it is important to note that the fuzzy RDD allows us to estimate merely the local average treatment effect on compliers, a special case of the local average treatment effect. This means that our results should be interpreted as the causal effect for the subgroup of individuals that comply with the instrument, for whom the returns to later marriage might be larger than average.

Conclusions

This study has explored the extent to which changes in age-at-marriage laws are effective in curbing early marriage and, if so, whether delays in age at marriage brought about by legal changes increase women's likelihood to participate in higher education – an oft-neglected scholarly outcome, yet one that is central to the post-2015 SDG agenda and is now at the forefront of important discussions within the international community. We tackled our two research questions using survey data from six LMICs located in different regions of the world combined with longitudinal information on policy changes. We adopted simple causal inference techniques to obtain estimates of the causal effect of changes in age-at-marriage laws on early marriage and educational outcomes. In so doing, we reached two different sets of findings. The first set of results is rather worrisome and calls for governments and policymakers' attention to make policy implementation more uniformly effective. The second set of findings is encouraging and suggests that effective policy implementation – i.e., legal provisions accompanied by adequate enforcement and monitoring – may importantly and positively shape women's life-course trajectories, hence contributing to raising women's status within society. Specifically, we found significant reductions in early marriage following the policy changes only in two out of the six countries considered, namely Nepal and Tajikistan. Yet in these countries where the changes in age-at-marriage laws were effective, women were significantly more likely to attend some form of higher education. In Tajikistan and Nepal, an increase in the legal age at marriage by, respectively, one and two years, led to a 20-60 percentage-point higher likelihood of attending higher education.

Our first finding relating to the mixed and context-specific effectiveness of policy changes aimed at curbing early marriage aligns with claims made by Arthur et al. (2018) and Collin and Talbot (2018) that, despite the increasing prevalence of legal provisions aimed at increasing the legal age at marriage, the level of enforcement varies widely, and legal exceptions based on parental consent and customary or religious laws remain in place – alongside high rates of illegal marriages (Collin and Talbot 2018) – thus preventing the full effectiveness of the legal provisions. Unfortunately, we did not have data on exact implementation procedures, monitoring, or enforcement, but several sources suggest that these are serious issues (Bharadwaj 2015; Kidman and Heymann 2016).

Our second finding relating to the implications of changes in laws for higher education is instead novel in the literature, thus adding to scholarly research on the implications of early marriage for educational outcomes, typically measured earlier in life (Field and Ambrus 2008; Delprato et al. 2015; Polyakova 2018; Sunder 2019, among others). Also, this literature tends to be focused on sub-Saharan Africa, India, and Bangladesh, thus making our results for Nepal and Tajikistan informative and stressing an additional layer of novelty from a purely "geographical" standpoint. As discussed above, we acknowledge that our estimated coefficients taking into account the endogenous nature of early marriage are quite sizeable in both relative and absolute sense. While it is hard to evaluate these effect sizes in light of the relevant literature on the topic – as we are not aware of any study focusing on higher education as an outcome – we can nonetheless relate our findings to those of similar studies using changes in marriage laws as instrumental variables. For instance, using an IV approach, Dahl (2010) found that a woman who marries young in the US is 31 percentage points more likely to live in poverty when she is older, an estimate that is more than double its simple OLS counterpart. Given the importance of higher education for women's later-life outcomes in contexts where higher education is not widely diffused such as LMICs (Ilie and Rose 2016; Schendel and McCowan 2016) – relative to, for instance, the US – we are confident that our estimates are not "too big" to be deemed unreliable. If we considered attending higher education as one effective pathway to increase lifetime earnings and boost social mobility – shortly, to "get out of poverty" (Kilty 2015; Shimeles 2016) – our estimates would actually be quite aligned with those reported by Dahl (2010) in the US, thus underscoring the significance of this educational outcome in resource-deprived contexts.

Preventing early, coerced, and forced marriage has been on the global agenda for several decades, first in 2000 with the Millennium Development Goals (MDGs) highlighting the reduction of child marriage as a global priority, and then in 2015 as part of the global agenda with the establishment of the SDGs. We have here argued that SDG Goal 5 – focusing on gender equality to empower all women and girls – is linked with progress on the elimination of early marriage, yet it is also inextricably linked with SDG Goal 4 (target 4.3), related to better access and more gender-equal participation in higher education. Significant progress is nowhere close in either respect, yet a clear implication ensuing from this study is that better enforcement and monitoring of legal provisions concerning the minimum age at marriage has the potential to raise women's status by simultaneously enabling the achievement of *both* goals.

We thus posit two clear implications of this research for policy and speculate on a third point. First, the laudable goal of legislation curbing or banning early marriage must be accompanied by capacity-building and resourcing for more legal enforcement. Second, monitoring the efficacy of deterrence, including through exploiting cheap and plentiful micro-level data as we do here, is essential to test and improve the link from laws to ages at marriage, the outcome targeted by policy and the one that matters most for women and girls' later-life outcomes. Third, we speculate on the possibility that national marriage policies might have a more meaningful impact if part of a comprehensive, multi-pronged, and context-sensitive approach targeting poverty and rooted social norms in all their forms.

Acknowledgements

We gratefully acknowledge support for this paper through the Global Family Change (GFC) Project (http://web.sas.upenn.edu/gfc), a collaboration between the University of Pennsylvania, University of Oxford (Nuffield College), Bocconi University, and the Centro de Estudios Demogràficos (CED) at the Universitat Autònoma de Barcelona. Funding for the GFC Project is provided through NSF Grant 1729185 (PIs: Kohler & Furstenberg), ERC Grant 694262 (PI: Billari), ERC Grant 681546 (PI: Monden), the Population Studies Center and the University Foundation at the University of Pennsylvania, and the John Fell Fund at the University of Oxford.

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Supplementary Material

Table S.1: Sensitivity analyses with a 4- and a 2-year bandwidth instead of a 3-year bandwidth. Results of the OLS models (coefficients), F-statistics for statistically significant associations and probit models (marginal effects). Outcome: 1 – first union before age 18 (early union), 0- no first union before age 18 (reference category). Independent variable: 1 – treated, 0 – control (reference category), all countries

	4-year bandwidth			**2-year bandwidth**		
	LPM (Coefs)	**F statistic**	**Probit (MEs)**	**LPM (Coefs)**	**F statistic**	**Probit (MEs)**
	Early union		**Early union**	**Early union**		**Early union**
Tajikistan	-0.036**	10.03	-0.036**	-0.051**	9.6	-0.051**
	(0.011)		(0.011)	(0.016)		(0.016)
Nepal	-0.083***	22.02	-0.082***	-0.083***	11.14	-0.083***
	(0 .018)		(0 .018)	(0 .025)		(0 .025)
Kazakhstan	-0.033**	11.42	-0.033**	-0.027	-	-0.027
	(0.010)		(0.010)	(0.015)		(0.015)
Bhutan	-0.046**	9.72	-0.046**	-0.007	-	-0.007
	(0.015)		(0.015)	(0.020)		(0.0202)
Benin	-0.027	-	-0.027	-0.016	-	-0.0157
	(0.014)		(0 .014)	(0.0197)		(0.0197)
Mauritania	0.021	-	0.021	0.019	-	0.019
	(0.015)		(0.015)	(0.022)		(0.022)

*** p<0.001, ** p<0.01, *p<0.05

Notes: Coefs: Coefficients; MEs: marginal effects. Control for religion in Nepal and Benin only, as these are the only countries where the variable is available. MEs for Nepal and Benin are calculated as average marginal effects. Results without controls for religion are essentially unchanged and available upon request. Robust standard errors in parentheses. Standard errors are clustered at the enumeration area level. P-values for Kazakhstan (2-year window) and Benin (4-year window) are p~0.06, hence we report it here as not statistically significant. The main reason is that we need a strong instrument to evaluate the impacts on higher-education attendance.

Table S.2: Results of two-stage least square models (coefficients), Nepal and Tajikistan

		1st stage	2nd stage
		Early Union	**Higher Education**
Tajikistan	Treated (ref.: control)	-0.045**	
		(0.013)	
	Early union (ref.: no early union)		-1.304**
			(0.491)
	Cons	0.134***	0.398***
		(0.011)	(0.057)
Nepal	Treated (ref.: control)	-0.083***	
		(0.020)	
	Early union (ref.: no early union)		-1.007***
			(0.244)
	Buddhist (ref.: Muslim)	-0.307***	-0.215*
		(0.067)	(0.100)
	Hindu	-0.202***	-0.074
		(0.053)	(0.072)
	Other	-0.270***	-0.164
		(0.073)	(0.099)
	Cons	0.764***	0.761***
		(0.052)	(0.183)

*** $p<0.001$, ** $p<0.01$, * $p<0.05$

Notes: Ref: reference category. Control for religion included in Nepal (variable not available in Tajikistan). Results without control for religion are essentially unchanged and available upon request. Robust standard errors in parentheses. Standard errors are clustered at the enumeration area level.

Table S.3: Sensitivity analyses with a 4- and a 2-year bandwidth instead of a 3-year bandwidth. Results of bivariate probit models (marginal effects), Nepal and Tajikistan

		4-year bandwidth			2-year bandwidth		
		Bivariate Probit (MEs)		Probit (MEs)	Bivariate Probit (MEs)		Probit (MEs)
		Early Union	Higher Education	Higher Education	Early Union	Higher Education	Higher Education
Tajikistan	Treated (ref.: control)	-0.045***			-0.055***		
		(0.010)			(0.014)		
	Early union (ref.: no early union)		-0.347***	-0.228***		-0.343***	-0.226***
			(0.013)	(0.017)		(0 .015)	(0.021)
		biprobit rho=0.987***			biprobit rho=0.988***		
Nepal	Treated (ref.: control)	-0.098***			-0.089***		
		(0.014)			(0.023)		
	Early union (ref.: no early union)		-0.617***	-0.248***		-0.535***	-0.246***
			(0.017)	(0.016)		(0.096)	(0.020)
		biprobit rho=0.951***			biprobit rho=0.756***		

*** p<0.001, ** p<0.01, *p<0.05

Notes: ME: marginal effects; Ref: reference category. Control for religion included in Nepal (variable not available in Tajikistan). MEs for Nepal are calculated as average marginal effects. Results without control for religion are essentially unchanged and available upon request. Robust standard errors in parentheses. Standard errors are clustered at the enumeration area level. The last column reports marginal effects from a simple probit specification which does not take into account the potential endogeneity of early marriage (for reference purposes only). Results from a simple 2SLS model – rather than bivariate probit – are reported in Supplementary Materials Table S.4. Results are fully consistent, yet predicted values are outside of plausible boundaries due to shares of women who entered higher education below or around 0.20.

Table S.4: Sensitivity analyses with a 4- and a 2-year bandwidth instead of a 3-year bandwidth. Results of two-stage least square models (coefficients), Nepal and Tajikistan

		4-year bandwidth		**2-year bandwidth**	
		1st stage	**2nd stage**	**1st stage**	**2nd stage**
		Early Union	**Higher Education**	**Early Union**	**Higher Education**
Tajikistan	Treated (ref.: control)	-0.036**		-0.051**	
		(0.011)		(0.016)	
	Early union (ref.: no early union)		-1.895**		-1.271*
			(0.665)		(0.503)
	Cons	0.122***	0.450***	0.139***	0.383***
		(0.010)	(0.069)	(0.014)	(0.059)
Nepal	Treated (ref.: control)	-0.083***		-0.083**	
		(-0.018)		(0.025)	
	Early union (ref.: no early union)		-1.186***		-0.696**
			(0.246)		(0.236)
	Buddhist (ref.: Muslim)	-0.287***	-0.250*	-0.276**	(-0.111)
		(0.062)	(-0.099)	(0.086)	(0.097)
	Hindu	-0.186***	-0.093	-0.150*	0.016
		(0.051)	(0.075)	(0.068)	(0.064)
	Other	-0.257***	-0.210*	-0.276**	-0.093
		(0.069)	(0.102)	(0.090)	(0.098)
	Cons	0.737***	0.865***	0.714***	0.504**
		(0.051)	(0.178)	0.068	(0.168)

*** p<0.001, ** p<0.01, *p<0.05

Notes: Ref: reference category. Control for religion included in Nepal (variable not available in Tajikistan). Results without control for religion are essentially unchanged and available upon request. Robust standard errors in parentheses. Standard errors are clustered at the enumeration area level.