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The World Bank Economic Review, 37(4), 2023, 599–619
https://doi.org10.1093/wber/lhad020
Article

OXFORD

Female Education and Brideprice: Evidence from Primary Education Reform in Uganda

Masaru Nagashima and Chikako Yamauchi

Abstract

Universal primary education (UPE) policies have been shown to improve educational attainment and delay marriage and childbearing, particularly among rural girls. This disproportionate improvement in female relative to male education can change the bargaining structure between the wife and the husband. Furthermore, with the expectation of this change, decisions about marriage-market entry, matching, and marital arrangements, such as brideprice, can change. In particular, greater female bargaining power can increase the share of marriages without a brideprice in settings where husbands may demand a refund upon divorce. Using first-hand data on marital transfers and exploiting Uganda's UPE, which abolished primary school fees in 1997, this study shows that longer UPE exposure is associated positively with female education and negatively with brideprice practice. The results imply that UPE policies can affect women's marital lives by empowering them in household decisions. The study also discusses the consistency of the results with other potential mechanisms, such as selective marriage-market entry, marital squeeze, and assortative matching.

JEL classification: I21, I25, J12, O55, Z13

Keywords: universal primary education, female education, brideprice, Uganda

1. Introduction

In the pursuit of achieving universal primary education (UPE), one of the most important development goals, many developing countries have abolished primary school fees. The economic literature has demonstrated that these reforms increased educational attainment, particularly for rural girls, and delayed their marriage and childbearing.[1] A more significant change in female versus male education can induce a

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The research for this article was financially supported by the Japan Society for the Promotion of Science (grant numbers 25101002, 15H02619, and 18J11688). The authors thank three anonymous referees, Alistair Munro, Tomoya Matsumoto, Shinpei Sano, Michelle Rao, Erika Seki, Juan Manuel del Pozo Segura, Stephan Litschig, Yoshito Takasaki, and seminar participants at the University of the Philippines, Makerere University, 11th Applied Econometrics Conference at Osaka University, Policy Analysis Research Workshop at GRIPS, Centre for the Studies of African Economies 2017 Conference, Japanese Economic Association 2017 Spring Meeting, International Association of Applied Econometrics 2017 Conference, and Nordic Conference on Development Economics 2019 for helpful comments. A supplementary online appendix is available with this article at *The World Bank Economic Review* website.

1 For example, see Chicoine (2021); Zenebe Gebre (2020); Adu Boahen and Yamauchi (2018); Osili and Long (2008) for Sub-Saharan African countries and Behrman (2015); Andriano and Monden (2019); Moussa and Omoeva (2020) for a multi-country comparison.

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change in the bargaining structure between the wife and the husband, and expecting this, it can alter their decisions about marriage-market entry and matching. These changes may further affect marital arrangements, such as brideprice—a marital wealth transfer from the groom to the bride's parents—since it is determined through a negotiation process (e.g., Gaspart and Platteau 2010). This paper investigates the relationship between UPE and the brideprice practice and explores possible underlying mechanisms.

How can a UPE reform affect brideprice practice? Since the brideprice can be refunded to the groom upon divorce,[2] it can be considered an additional cost of leaving a marriage for the women and their parents. Women's expected gain from marriage may then increase when they can marry without a brideprice. In such a setting, UPE is likely to raise women's outside options, strengthen their bargaining power relative to their husbands, and eventually increase the number of marriages without a brideprice payment. In addition, the expectation of these changes in marital life may lead to a different equilibrium in marital matching through changes in educational investments (see, e.g., Chiappori, Salanié, and Weiss 2017). Furthermore, changes in polygyny, which has declined due to education (see, e.g., Fenske 2015), can also reduce brideprice practices (Tertilt 2005). Relaxation of the marriage squeeze due to an increased supply of educated women can also contribute to the decline in brideprice practices (Becker and Lewis 1973).

Uganda provides an interesting setting to analyse the impact of UPE on brideprice. In the country, brideprice is still practiced widely (see, e.g., Bishai and Grossbard 2010), and school fees were abolished for all pupils enrolled in primary schools in and after 1997. This implies that older cohorts, few of whom were in school in the reform year, were less likely to benefit, while the younger cohorts were increasingly more likely to benefit from the reform. This study exploits this unique feature of the Ugandan UPE as a source of exogenous variation in a reduced-form analysis. Using first-hand data on marital transfer, which are not widely available in public data, this study investigates whether female education and brideprice practice exhibit differential trends for the younger treatment cohorts compared to the older control cohorts.

This analysis finds increasing and decreasing trends for female educational attainment and brideprice practice, respectively, among the younger treatment cohorts, whereas no significant trends were observed for either outcome among the control cohorts. These newly emerged trends indicate that every 10-percentage-point increase in the UPE coverage rate, which was experienced by those born a year later, is associated with an increase in female education by 0.19 to 0.26 years and a decrease in the probability of brideprice practice by 2.0 to 3.1 percentage points. Over the decade in which UPE coverage increased from 0 to 1, the probability of brideprice practice decreased by 20 to 31 percentage points, or approximately one-third of the pre-UPE average (approximately 75 percent). As a result, the proportion of those receiving a brideprice was approximately 50 percent for the 1990 cohort, which was fully exposed to UPE.

These results, coupled with field observations, do not appear inconsistent with the scenario that female education increased their outside options and relative bargaining power, thereby helping them achieve an alternative marital arrangement—marriage without a brideprice. On the other hand, it is shown that the results of this study are unlikely to be driven by changes in other marital characteristics, such as age at marriage, and other mechanisms, such as marital sorting, polygyny, and the marriage squeeze. At the same time, there can be other mechanisms, not going through female education, consistent with the results based on the reduced-form analysis. They include the wealth effect of UPE on households with primary school children and a decrease in marginal productivity at home due to the decline in the quality of education after UPE.

2 This is confirmed in the focus group discussions (FGDs), which were conducted in three selected villages in Eastern and Central Uganda in March 2020 with six to eight women per village who were interviewed in the survey. While there may be some variation in the specific arrangement, brideprice repayment upon divorce appears to be common in Uganda (e.g., Bishai and Grossbard 2010) and in other parts of Africa (see, e.g., Platteau and Gaspart 2007; Gaspart and Platteau 2010 for Senegal). See Section Mechanisms for the Decline of Brideprice Practice for further discussion.

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The present study extends the scope of the literature on UPE policies by showing that they can affect women's welfare by altering marital arrangements such as brideprice practice. UPE is likely to have made it easier for Ugandan women to remove one of their barriers to divorce through the increase in their outside options. Additionally, this study's findings relate to the pathways through which female education affects reproductive and child outcomes (Chicoine 2021; Keats 2018; Masuda and Yamauchi 2020; Moussa and Omoeva 2020; Zenebe Gebre 2020). To the extent that outcomes such as later childrearing benefit women, an increase in female relative bargaining power in marriage can give rise to these previous findings.

The results of this study also provide important implications for the literature on brideprice. It has been shown that brideprice acts as a means of consumption smoothing for parents of young girls (Corno, Hildebrandt, and Voena 2020; Dekker and Hoogeveen 2002; Hoogeveen, van der Klaauw, and van Lomwel 2011). The negative effect of UPE policies on the likelihood of brideprice transfers can limit the scope for consumption smoothing for parents in the short run. In the long run, however, it is unclear how the increased uncertainty about brideprice receipt will change their investment in girls' education. On the one hand, they will likely lose the incentive to invest in her education, as the expected return from the marriage market is lower (Ashraf et al. 2020). On the other hand, if earnings opportunities for women exist, parents might start to expect returns from female education from the labor market.[3]

The remainder of this paper is structured as follows. The next section provides a brief summary of primary education reform in Uganda and a description of the data set. Then the estimation strategy and analytical results are discussed. After a consideration of potential mechanisms behind the findings, the last section concludes the paper.

2. Universal Primary Education in Uganda

The educational system of Uganda consists of seven years of primary education, six years of secondary education, and three to five years of tertiary education. Children are supposed to commence schooling at the age of 6. Before UPE, the net enrollment rate among children aged 6 to 12 years was approximately 60 percent (Deininger 2003). A major impediment at that time was said to be the costs of schooling, both direct and indirect, borne by parents and family (Nishimura, Yamano, and Sasaoka 2008).

To address the financial problem of education, a reform called UPE was initiated in January 1997 to eliminate school fees (Uganda Bureau of Statistics 2003). The UPE scheme provided each school with sufficient funding to cover private education costs such as tuition and PTA contributions (Ministry of Education and Sports 1999). The government of Uganda announced UPE in December 1996 (Grogan 2008) and launched a nationwide advertising campaign that informed nearly all parents and guardians of school-age children of the reform (Uganda Bureau of Statistics and ORC Macro 2001).

The UPE reform resulted in monumental changes in Uganda. The number of enrolled children aged 6 to 8 increased from 2.7 million in 1996 to 5.3 million in 1997 and further to 7.3 million in 2002 (Uganda Bureau of Statistics 2003). With the gross enrollment rate rising from 74.3 percent in 1995 to 135.8 percent in 2000, the reform was said to have achieved universal access to primary education (Riddell 2003). The effect of UPE was found to be larger for girls and poor households than for boys and wealthier households (Deininger 2003; Nishimura, Yamano, and Sasaoka 2008).[4] Additionally, the reform reduced delayed enrollment and increased the probability of completing higher grades (Nishimura, Yamano, and Sasaoka 2008). Although there are many people who never attend or drop out of school (Uganda Bureau of Statistics and ORC Macro 2001), it would not be an exaggeration to say that Uganda's UPE has been

3 Note that these changes are conditional on some possibility of divorce and the existence of earning opportunities for women. In settings with no possibility of divorce, the return on female education is considered to be solely derived from the marriage market (Deng et al. 2023).

4 Section Male Education and the Labor-Market Equilibrium Effect also shows that UPE did not significantly affect male education.

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successful in increasing educational attainment. This large expansion of primary education, particularly among girls, is further found to delay marriage and childrearing and improve the health status of children (Behrman 2015; Keats 2018; Masuda and Yamauchi 2020). However, it remains unclear whether this increase in female educational attainment was accompanied by changes in marital arrangements such as brideprice practices.

3. Data

3.1. Survey Design

This study uses data obtained from the fifth wave of the Research on Poverty, Environment, and Agricultural Technologies (RePEAT) survey in Uganda, undertaken from September through December 2015. The RePEAT survey comprises a panel data set from the first wave in 2003 to the sixth wave in 2022. In 2003, 10 randomly chosen households were interviewed from each of 94 randomly chosen rural villages from the eastern, western, and central districts.[5] In 2015, the survey was extended to cover five more randomly selected households in each of the formerly surveyed villages, and 23 additional villages (15 households each) were added from two northern districts. In total, the 2015 RePEAT survey constitutes a data set of 1,755 households from 117 villages.

Questions about brideprice practice were asked only for each woman's first marriage for the following reasons. First, the brideprice agreed upon at the time of the first marriage is likely to correctly reflect the decisions regarding parental investment in the bride's human capital to the extent that her parents do not anticipate the divorce and remarriage of their daughter when she marries for the first time (Arunachalam and Logan 2016). Second, the decision-making mechanism for brideprice in a remarriage may differ from that in the first marriage, and these differences may depend on many factors, such as the gender of the person who remarries,[6] whether remarriage follows divorce, separation, or widowhood, and unobservable factors, including preferences for remarriage and social norms. This study avoids analytical complications by focusing solely on women's first marriage.[7]

This study thus uses the sample of women who had ever married at the time of the 2015 survey and were between 24 and 49 years of age (i.e., born from 1966 to 1991).[8] This age restriction was imposed to obtain an adequate number of cohorts for the estimation exercise. While most women were likely to have married at least once by the age of 24 before UPE,[9] previous studies report that the reform delayed

5 This initial 2003 sample was built on the project on land management (Pender et al. 2004), which involved 107 communities from two-thirds of the regions. Although the 2003 RePEAT excluded the samples in the north and northeastern regions, the sample in the land management project represented seven of the nine major farming systems of the country (Yamano et al. 2004).

6 For example, Fafchamps and Quisumbing (2005) find that men and women have significantly different probabilities in Ethiopia.

7 Among the control group subjects (women born between 1966 and 1978 who have ever married; see Section Male Education and the Labor-Market Equilibrium Effect for details), 73.6 percent are found to live with their first marital partner. The remaining 26.4 percent have experienced a divorce or separation, of whom 68.9 percent (or 18.2 percent of the total control women) live with a non-first marital partner.

8 Attempts were made to ask questions about the women themselves. However, a proxy interview was allowed if the woman was unavailable and the alternative respondents knew a great deal about her first marriage. Another attempt was made to collect information from male household members, since if a male interviewee had remarried and his first partner was no longer available in the survey, asking him about his first marriage would increase the sample size. However, the use of such male-queried data was abandoned because for their female partners, other critical information (collected in the education and demography sections that were used for existing household members only) was missing.

9 Uganda Bureau of Statistics (UBOS) and ICF International Inc. (2012) shows, for example, that the share of women who had never married in the age group between 25 and 29 years was 5.6 percent, with a sharp decline from 23.9 percent in the age group between 20 and 24 years.

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marriage, and the findings below also support this finding. Thus, this study analyses the possibility of sample selection, which suggests that it is unlikely to spuriously generate the main results regarding the impact of UPE on brideprice practices.

3.2. Major Variables

Women aged 24 to 49 years who had ever been married were asked whether their first marriage partner agreed to pay a brideprice. This study defines a variable for brideprice practice that takes value 1 if the answer to this question was yes.[10] The survey also collected information about the characteristics of the first marriage and of the women at their first marriage, such as the marital year and whether the marriage was based on love or arrangement.[11] Furthermore, the survey asked about the respondent's religion and location of residence before and after her first marriage.

This study constructs a variable measuring educational attainment using the survey responses on each individual's highest grade of education. This study compares these responses to the education system in Uganda and calculates the minimum years of schooling required to achieve the person's reported highest grade of education. This variable is used as years of schooling throughout the paper.

4. Empirical Strategy

To estimate the association between UPE exposure and outcomes such as female education and brideprice practices, it is important to note that UPE introduced free primary education to all who were enrolled in primary school in and after 1997, regardless of their year of birth.[12] Since older cohorts were more likely to have left primary school by the time the UPE started, they were less likely to be exposed to the reform. Figure 1 indeed shows that the share of those who were enrolled in primary school in 1997 is zero for cohorts born in or before 1978. On the other hand, the share of women who were still in primary school as of 1997 exhibits an increasing trend for younger cohorts born in or after 1979.[13]

10 Women were further asked about the amount of the brideprice agreed upon to be paid in cash, cattle, or other forms. Respondents were asked to value cattle and other transfers in real terms (they were asked to report how much would it cost to buy the same amount of cattle (or other) now) and to report any cash payment in the nominal amount. This approach was intended to suppress recall bias. However, the inflation adjustment turned out to be sufficiently problematic that the cash amount was barely comparable to cattle and other brideprice values due to rampant inflation rates in the late 1980s and early 1990s, when there was internal conflict in Uganda. As high inflation rates may induce grooms to switch from in-kind transfer to cash payment, using only in-kind transfer information may be inappropriate. Additionally, since it is shown below that brideprice practice is declining, statistical analysis of brideprice amounts conditional on agreement may not be straightforward. For these reasons, data on the value of brideprices are not used in this study.

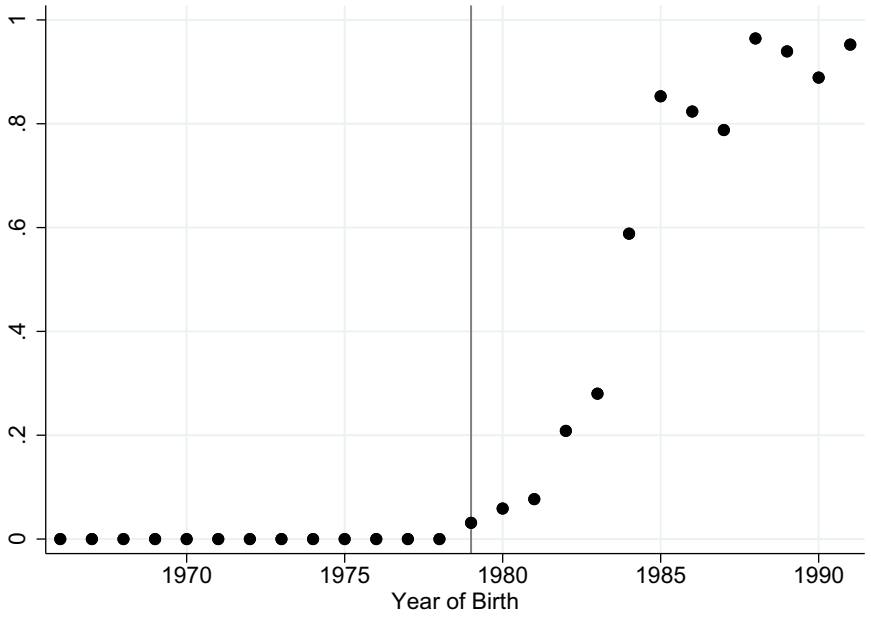
11 The survey attempted to collect information about the landholdings of the female's natal family. However, the variables contain too many missing values and thus cannot be used for analysis. Moreover, the survey did not collect information about gift exchange and reciprocity.

12 In the original plan, up to four children per household were supposed to be the target of free primary education. However, everyone was eventually provided with free primary education as long as they were enrolled in primary school (Grogan 2008).

13 The measurement of enrollment status in 1997 is based on a survey question that asked for the year in which each person left primary education. In particular, the survey asked about attendance at a UPE-funded school. However, the data show that many women who reported having attended UPE-funded schools had already left their primary school by 1997. This finding may be due to misreporting, but it is more likely that the survey question was misunderstood, as it did not clarify whether the school was free while they were enrolled there. In this case, since UPE abolished tuition fees at all primary schools, women may have been confused about this question, particularly if they had acquired some posterior knowledge about the nature of the reform.

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Figure 1. Share of Women Enrolled in Primary School under Universal Primary Education.



Source: Research on Poverty, Environment, Agriculture, and Technology survey in Uganda in 2015.
Note: This figure plots the share of women enrolled in primary school in and after 1997. The enrollment status is calculated based on the year each woman left primary school. The sample used to draw the figure includes women aged 24 to 49 years who have ever married.

This study thus defines non-UPE-exposed cohorts as those born in or before 1978 and UPE-exposed cohorts as those born in 1979 and later.[14] If the trend in educational attainment changed as UPE was launched, the change is likely to be observed from the cohort that started to be exposed to it. This is confirmed by the relationship in fig. 2, where both educational attainment and the proportion of women who had a brideprice payment agreed upon at their first marriage exhibit little trend for the non-UPE-exposed cohorts (born in or before 1978). From around the birth year of 1979, educational attainment starts to show an increasing trend, while the proportion of women with a brideprice exhibits a decreasing trend.[15] That is, the relationship between the year of birth and years of education became steeper for the cohorts exposed to UPE. Furthermore, a more negative trend in whether a brideprice was agreed upon at the first marriage emerged only among the UPE-exposed cohorts.

Based on these considerations, this study estimates the following equation:

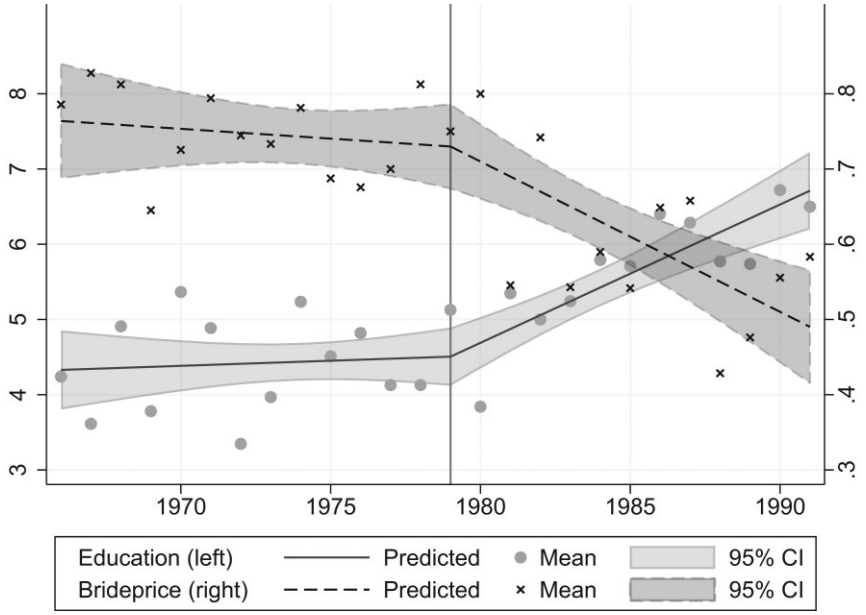
$$y_i = \gamma_0 + \gamma_1(z_i - c) + \gamma_2 \mathbf{I}\{z_i \geq c\}(z_i - c) + W_i\gamma_3 + e_i,$$

14 A somewhat non-smooth distribution of the birth year is found around the cutoff (supplementary online appendix fig. S3.1). This study confirms the robustness of the results by excluding each birth cohort and estimating the change in the trend (supplementary online appendix fig. S3.2). The results for smaller bandwidths are generally unchanged but are not shown for brevity. The authors thank anonymous referees for suggesting this analysis.

15 While the information on the year of leaving primary school is available only in the RePEAT data, the 1995 and 2000 Demographic and Health Surveys, which are much larger, also indicate that the share of girls who had been in school but had not completed primary education drops below .05 at age 18 (supplementary online appendix fig. S3.3; see also Keats 2018). If this pattern continued in 1997, the UPE is unlikely to have affected a significant fraction of women born in or before 1978 (i.e., 19 years or older in the reform year), thereby creating a change in the trend at the birth year of 1979.

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Figure 2. Years of Education and the Brideprice Practice in Uganda.



Source: Research on Poverty, Environment, Agriculture, and Technology survey in Uganda in 2015.
Note: This figure plots the years of education and the share of women who had a brideprice payment agreed upon at their first marriage for each birth cohort and their kinked linear fits. The calculation includes women aged 24 to 49 who have ever married. The vertical line represents 1979, the cutoff of the analysis of this study.

where y_i denotes an outcome of woman i, z_i her year of birth, c the cutoff year, which is 1979, and W_i the premarital controls. Using $z_i - c$ as a running variable, γ_1 estimates the trend of the outcome for the non-exposed cohorts, which is approximated by the slope of the fitted line on the left-hand side of the cutoff (fig. 2). The trend for the exposed cohorts is $\gamma_1 + \gamma_2$, which is similarly represented by the slope of the fitted line on the right-hand side of the cutoff year. The parameter of interest is γ_2, as it indicates how much the outcome trend changed between the exposed and non-exposed cohorts of women. For robustness, this study estimates this equation using different bandwidths while keeping the same number of birth cohorts below and above the cutoff.[16]

Prior studies on Uganda's UPE have defined the cutoff years as 1983 (Keats 2018) and 1984 (Behrman 2015) and estimated the impact of female education based on regression discontinuity design (RDD). In contrast, since there can be a concern that a large-scale reform such as UPE may not satisfy the exclusion restriction due to its multifaceted influence,[17] this study estimates the impact of UPE exposure in the reduced-form analysis. For this purpose, it makes more sense to define the cutoff at the cohort where the trend is likely to have changed, which is that first exposed to UPE. In addition, the two exercises below support the strategy. First, the RDD approach with this study's cutoff yields a small and insignificant level change in education (supplementary online appendix table S4.1). Second, the regression with this paper's specification and cutoffs such as 1983 and 1984 is likely to suffer from attenuation in the estimated effect of UPE exposure, as the relatively younger women in the control cohorts were actually exposed to UPE

16 One might consider comparing women from ethnicities with and without historical brideprice practice. The results of merging the historical data of the Ethnographic Atlas (Murdock 1967; Gray 1999) suggest that all the ethnic groups in the data exhibit a high proportion of women receiving a brideprice in the past (supplementary online appendix table S4.4). We relied on Olson et al. (1996), Stokes (2009), the Joshua Project, Ethnologue, and Wikipedia to match RePEAT ethnicity codes to the Ethnography Atlas's ethnic groups.

17 See, for instance, Khanna (2019) for the case of affirmative action in India.

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and the estimated trend for the control cohorts increasingly resembles that for the treatment cohorts as the cutoff is moved to later years. Supplementary online appendix tables S4.2 and S4.3 indeed report smaller and insignificant estimates. These results suggest that the impact of UPE exposure is likely to be appropriately estimated with this study's cutoff and reduced-form specification.

The estimated association is unlikely to result from the anticipation of future UPE reforms. First, election results are ex ante uncertain. In 1996, along with the incumbent Yoweri Museveni, two other candidates were running for presidential office: Paul Ssemogerere and Kibirige Mayanja. Although Museveni ultimately won over three times more votes than any other candidate, he lost in many districts in the northern region and in some of the central and eastern regions to the second-place candidate Ssemogerere (Uganda Electoral Commission 1996). Moreover, Museveni's slogan of anti-multiparty politics was said to be unpopular (The Independent 1996). Given the limited information network and coverage in Uganda in 1996, it is unlikely that even those voters in his winning constituencies were able to predict his popularity in other places, let alone his overall victory in the race. In addition, there was another election in June 1996 for members of Parliament.[18] These two elections within a year were likely to create substantial uncertainty over subsequent politics in Uganda.

Second, President Museveni was not said to be enthusiastic about implementing UPE, but rather about infrastructure development. Furthermore, the government as a whole, not only the newly elected president, showed little to no interest in pursuing the removal of primary school tuition, despite the call for it by international society (Stasavage 2005). All these facts support that the reform was indeed suddenly introduced.[19]

The interpretation of the estimated impact of UPE exposure would be difficult if UPE were accompanied by reforms to other policies and legal systems related to female education or brideprice practices. One such possibility is the amendment of the penal code that redefined sexual abuse against youths under the age of 18, which is the legal marital age in Uganda.[20] However, it came into effect in 2007, when the oldest control cohort was 28 years old and thus not subject to the reform. Another possibility is marriage law, for which many bills have been drafted since the Constitutional Court declared in 2004 that marriage and divorce laws were inconsistent with the constitution adopted in 1995 (Okello, G. M. & Ors., v. Attorney General 2004). A series of bills were proposed in 2009, 2013, and 2017, but none has been passed to date. These reforms are therefore unlikely to confound this study's analysis.[21] A third possibility is the legal trial of brideprice practices in Uganda, which is discussed in greater detail in Section Impact of the 2007 Brideprice Trial.

Some may still be concerned that UPE exposure may be correlated with covariates that also correlate with female education and brideprice practice. While the data are limited, this study uses available predetermined covariates and examines their trends for cohorts born in years around the cutoff. Figure 3 shows that covariates including parental education, premarital religion and residence, and ethnicity exhibit a

18 Large national projects such as UPE usually require parliament's approval regarding the budget and implementation plan. Thus, Museveni's victory in the presidential election is unlikely to have been sufficient to implement UPE.

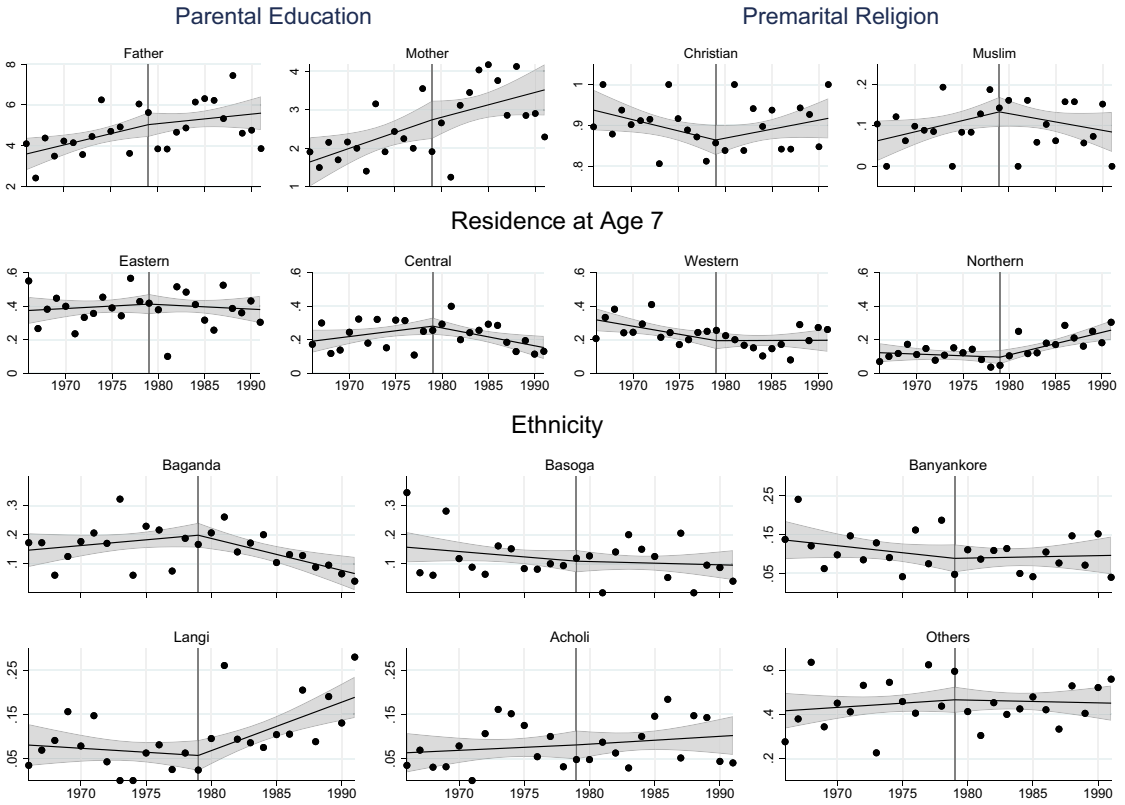
19 Museveni's manifesto (Museveni 1996a) states that he was planning to initiate a reform to allow parents to send four children per household to school for free in 1997. However, 1997 was noted only in the written manifesto: it was never addressed in his oral speech (Museveni 1996b). That is, information on the timing of UPE introduction was available only to those who were literate and able to obtain a copy of his manifesto or those who were in touch with someone who could read it. The number of politically literate voters in this study's rural sample data was likely to be small, judging from the fact that the government launched a massive campaign to publicize the UPE reform after its announcement in December 1996 (Grogan 2008): if the reform had already been well known to the public, the government would not have needed such a substantial campaign.

20 Specifically, this amendment extended the definition of defilement from having sexual intercourse with a girl aged 18 or younger to performing any sexual act with any person younger than 18 years of age (Doya 2017).

21 Similarly, policies pertaining to HIV and AIDS, such as condom provision and anti-stigma campaigns (Tumushabe 2006), are unlikely to have created a sudden change.

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Figure 3. Trend in Predetermined Covariates.



Source: Research on Poverty, Environment, Agriculture, and Technology survey in Uganda in 2015.
Note: These figures show the birth cohort mean and its kinked linear fit for the indicated variables. The sample used to draw the figures includes women aged 24 to 49 years who have ever married. The vertical line represents 1979, the cutoff of the analysis of this study.

smooth trend.[22] This suggests that the results of this study are unlikely to be driven by the correlation of UPE exposure with these observable factors.

5. Results

5.1. Descriptive Analyses

Table 1 presents the summary statistics of the major variables for the older control group (born in or before 1978) and the younger treatment group (born in or after 1979). Panel A shows that most demographic characteristics, such as region of residence at age seven and ethnicity, are balanced between the younger and older cohorts.[23] Panel B shows that the treatment women are indeed more educated than the control

22 The results show a slight increase in the trend in the share of women who are from the northern region and of Langi ethnicity (table 1 and supplementary online appendix fig. S1.1). To determine whether this could bias the results, the main regression equation is re-estimated by excluding women from the northern region and belonging to the Langi. The results confirm the robustness of the main findings (supplementary online appendix fig. S3.4).

23 While the proportion of Langi women was larger in the treatment group, it is unlikely to be a confounding factor in the estimation, as discussed above (fig. 3, supplementary online appendix fig. S3.4). Additionally, as discussed in Section Empirical Strategy, all the ethnic groups in the sample are reported to have a significant marital transfer (supplementary online appendix table S4.4). The increase in Langi observations may be

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Table 1. Summary Statistics of the Major Variables

Sample	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Women born in 1966–1978.				Women born in 1979–1991.				(2) = (6)
variables	*N*	Mean	Median	Std. dev.	*N*	Mean	Median	Std. dev.	*t*-statistic
Panel A. Demographic characteristics									
Age	523	42.71	43	3.616	733	29.71	30	3.843	−60.57***
Region at age 7: East	468	0.387	0	0.488	662	0.361	0	0.481	−0.88
Region at age 7: Central	468	0.235	0	0.424	662	0.248	0	0.432	0.49
Region at age 7: West	468	0.259	0	0.438	662	0.210	0	0.408	−1.91*
Region at age 7: North	468	0.118	0	0.322	662	0.169	0	0.375	2.41**
Own ethnicity: Baganda	504	0.177	0	0.382	701	0.168	0	0.374	−0.37
Own ethnicity: Basoga	504	0.129	0	0.335	701	0.117	0	0.322	−0.63
Own ethnicity: Banyankore	504	0.117	0	0.322	701	0.101	0	0.302	−0.87
Own ethnicity: Langi	504	0.069	0	0.254	701	0.110	0	0.313	2.39**
Own ethnicity: Acholi	504	0.077	0	0.267	701	0.088	0	0.284	0.68
Own ethnicity: Any other	504	0.431	0	0.496	701	0.415	0	0.493	−0.53
Panel B. Education variables									
Years of education	514	4.449	5	3.305	719	6.439	6	4.003	9.24***
Partner's years of education	433	6.367	6	3.603	477	6.964	7	3.549	2.52**
Primary: 1 if attended in any grade	514	0.790	1	0.408	719	0.894	1	0.308	5.12***
Primary: Age of enrollment	341	7.639	7	1.500	587	7.305	7	1.567	−3.18***
Primary: Age of leaving school *1	321	9.330	12	8.302	517	11.532	13	6.440	4.30***
Secondary: 1 if attended in any grade	514	0.123	0	0.328	719	0.300	0	0.459	7.52***
Tertiary: 1 if attended in any grade	514	0.008	0	0.088	719	0.061	0	0.240	4.82***
Panel C. First marriage variables									
Ever married	521	0.946	1	0.226	731	0.776	1	0.417	−8.49***
Age at first marriage	465	17.66	18	4.746	535	17.90	18	4.064	0.87
Marriage type: Love	474	0.928	1	0.258	532	0.898	1	0.302	−1.67*
Own premarital residence: Within LC1	475	0.236	0	0.425	534	0.288	0	0.453	1.89*
Own premarital residence: Within subcounty	475	0.251	0	0.434	534	0.245	0	0.431	−0.19
Own premarital residence: Within district	475	0.208	0	0.407	534	0.200	0	0.401	−0.32
Own premarital residence: Within Uganda	475	0.303	0	0.460	534	0.253	0	0.435	−1.79*
Own premarital religion: Christian	474	0.903	1	0.296	532	0.889	1	0.314	−0.72
Own premarital religion: Muslim	474	0.095	0	0.293	532	0.111	0	0.314	0.83
1 if having ever divorced/separated	435	0.264	0	0.442	522	0.255	0	0.436	−0.34
Marital status: Polygynous	521	0.184	0	0.388	731	0.115	0	0.319	−3.46***
1 if having brideprice paid	471	0.743	1	0.437	527	0.620	1	0.486	−4.17***
1 if having cash brideprice paid	350	0.757	1	0.429	327	0.783	1	0.413	0.79
1 if having cattle brideprice paid	350	0.731	1	0.444	327	0.667	1	0.472	−1.84*
1 if having other brideprice paid	350	0.751	1	0.433	327	0.746	1	0.436	−0.16

Source: Research on Poverty, Environment, Agriculture, and Technology survey in Uganda in 2015.

Note: This table shows summary statistics (number of observations (*N*), mean, median, and standard deviation) of the major variables for the sample women who were born from 1966 to 1991. The control group consists of women born from 1966 to 1978, while the treatment group consists of women born from 1979 to 1991. The age of leaving primary school was asked of those who were born in 1972 or after and had completed at least some primary education. Statistical significance is denoted by *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$.

women. Years of education increased by approximately two years on average, and the treatment women enrolled in primary school at an earlier age and left school at an older age than did the control women.

Panel C presents a summary of marital characteristics. While the probability of having ever married is lower for the younger treatment women, this is most likely because they are younger than those in the

related to the internal conflict in the 1980s and 1990s, but there is no clear explanation for these significant differences.

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control group.[24] Among those who had ever married, love marriages slightly decreased and local matches increased, whereas polygynous unions significantly decreased.

Finally, a large drop is observed in the proportion of women who had agreed to a brideprice at their first marriage. Figure 2 also demonstrates that the linear fit for the control cohorts is almost flat, while a downward trend is observed for the treatment cohorts, suggesting a change in the trend at the cutoff. Below, it is investigated how this change is associated with exposure to UPE across different cohorts.

5.2. Main Results

Panel A of table 2 shows the estimated change in the trend of years of education for women born in or after the cutoff relative to those born before the cutoff. The interaction term generally has a positive and significant coefficient estimate,[25] which reflects an increase in the trend only for the treatment cohorts (fig. 2). That is, the relationship between the year of birth and years of education became steeper for the cohorts exposed to UPE. The estimated change in the slope is insignificant in the cases with bandwidths of three and four years, which is likely due to the small sample size. Since it seems difficult to obtain a reliable estimate for these bandwidths, this study focuses mainly on the estimates with bandwidths larger than four throughout the paper. The results for the small bandwidths are shown for transparency purposes. Panel B of table 2 presents the estimated change in the trend in whether a brideprice was paid at the first marriage. It shows that a more negative trend emerged only among the exposed cohorts. The results are statistically significant for bandwidths of 10 years and longer.[26]

These estimates imply that, only for younger cohorts, being born one year later is associated with an increase in the average years of education by 0.19 to 0.26 years and a decrease in the probability of brideprice practice by 2.0 to 3.1 percentage points. This is likely due to an increase in the UPE coverage rate. The share of women who were enrolled in primary school in or after 1997 and thus benefited from UPE increased from 0 for the 1978 cohort (the youngest control cohort) to 1 for the 1988 cohort (fig. 1). A simple interpolation suggests that the coverage rate increased by 10 percentage points annually among the younger cohorts.[27] Thus, the above-mentioned changes can be interpreted as being associated with a 10-percentage-point increase in the coverage rate. These are newly emerged trends only for the younger cohorts. When the outcome levels are compared between the two cohorts with no UPE coverage (1978) and full UPE coverage (1988), the probability of brideprice practice decreased by 20 to 31 percentage points, or approximately one-third of the pre-UPE average (74.3 percent, table 1). This suggests that after all the women were covered, the proportion of those receiving a brideprice was approximately 43 to 54 percent in 1990 (see also fig. 2). It would be interesting for a future study to determine whether this trend continued after the coverage rate reached 100 percent.

One could be concerned that the younger cohorts may have also experienced changes in marital characteristics such as age at marriage, whether it was arranged, and whether it was polygynous. These changes might in turn have led to the decline in brideprice practices. However, the results in table 3 suggest that

24 One potential concern is that brideprice practice is observed only for women who have ever married. If the composition of ever-married women differs between the older and younger cohorts, estimates for the impact of UPE exposure may be biased. However, Section Sample Selection Due to Delayed Marriage shows that selection into marriage does not appear to drive the regression results.

25 As discussed in Kolesár and Rothe (2018), the standard errors clustered at the year-of-birth level are not always larger than the heteroskedasticity-robust counterparts. Following their recommendation, only the heteroskedasticity-robust estimates are reported elsewhere.

26 For bandwidths shorter than 10 years, the estimated change is insignificant but still negative and of similar magnitude.

27 The increase in the coverage rate was likely also accompanied by an increase in the number of years in which each treated woman was able to attend primary school without fees. For example, girls who were in the final grade at the start of UPE (most likely older girls) received free education only in their final grade, while girls who entered primary schools after the launch of UPE received it in all the grades in which they enrolled.

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Table 2. Estimated Effects of Universal Primary Education Exposure on Female Education and Brideprice Receipt Status

Bandwidth	(1) 13 years	(2) 12 years	(3) 11 years	(4) 10 years	(5) 9 years	(6) 8 years	(7) 7 years	(8) 6 years	(9) 5 years	(10) 4 years	(11) 3 years
					Panel A. Years of education						
Year of birth − 1979	0.032	0.024	−0.009	−0.003	−0.066	−0.045	0.020	−0.134	−0.209	−0.125	−0.100
	(0.031)	(0.036)	(0.042)	(0.048)	(0.054)	(0.065)	(0.077)	(0.104)	(0.130)	(0.183)	(0.280)
	[0.036]	[0.046]	[0.047]	[0.060]	[0.053]	[0.074]	[0.101]	[0.115]	[0.110]*	[0.145]	[0.231]
I{year of birth ≥ 1979}	0.187	0.198	0.253	0.256	0.375	0.369	0.244	0.542	0.577	0.597	0.495
× (year of birth − 1979)	(0.059)***	(0.067)***	(0.075)***	(0.091)***	(0.105)***	(0.124)***	(0.152)	(0.201)***	(0.250)**	(0.365)	(0.672)
	[0.056]***	[0.069]***	[0.076]***	[0.099]**	[0.088]***	[0.126]**	[0.164]	[0.211]**	[0.246]**	[0.291]*	[0.759]
N	894	847	776	708	651	572	504	426	364	301	206
Adj. R^2	0.164	0.156	0.145	0.138	0.143	0.123	0.120	0.136	0.129	0.106	0.186
					Panel B. 1 if brideprice was paid at the first marriage						
Year of birth − 1979	−0.003	0.001	0.003	0.004	−0.002	−0.000	0.003	0.006	0.010	0.028	0.037
	(0.005)	(0.005)	(0.005)	(0.006)	(0.007)	(0.009)	(0.011)	(0.014)	(0.019)	(0.025)	(0.043)
	[0.005]	[0.005]	[0.005]	[0.006]	[0.005]	[0.005]	[0.007]	[0.012]	[0.019]	[0.018]	[0.029]
I{year of birth ≥ 1979}	−0.020	−0.027	−0.031	−0.030	−0.014	−0.019	−0.028	−0.029	−0.032	−0.049	−0.152
× (year of birth − 1979)	(0.008)**	(0.009)***	(0.010)***	(0.012)**	(0.014)	(0.018)	(0.022)	(0.029)	(0.037)	(0.049)	(0.105)
	[0.009]**	[0.009]***	[0.009]***	[0.012]**	[0.010]	[0.011]*	[0.014]*	[0.019]	[0.034]	[0.040]	[0.104]
N	891	843	773	705	649	571	504	426	365	301	206
Adj. R^2	0.135	0.142	0.143	0.126	0.103	0.086	0.073	0.083	0.057	0.062	0.053

Source: Research on Poverty, Environment, Agriculture, and Technology survey in Uganda in 2015.

Note: This table shows the coefficient estimates of the year of birth minus the cutoff of 1979 and its interaction with the indicator for it being equal to or larger than the cutoff from the regressions of years of education (Panel A) and the indicator for whether a brideprice was paid at the first marriage (Panel B). All regressions include a constant, the year of birth minus the cutoff, and dummies for ethnicity and region of residence at age seven as covariates. The regressions are run for women aged 24 to 49 years who have ever married and were born in years within the indicated bandwidth of the cutoff. Reported in parentheses are standard errors robust to heteroskedasticity, while those in brackets are clustered at the year of birth. Statistical significance is denoted by *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$.

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Table 3. Estimated Effects of Universal Primary Education Exposure on Marital Characteristics

Bandwidth	(1) 13 years	(2) 12 years	(3) 11 years	(4) 10 years	(5) 9 years	(6) 8 years	(7) 7 years	(8) 6 years	(9) 5 years	(10) 4 years	(11) 3 years
Age at marriage	0.187	0.176	0.160	0.161	0.125	−0.091	−0.318	−0.079	−0.172	−0.296	0.151
	(0.078)**	(0.091)*	(0.108)	(0.131)	(0.155)	(0.164)	(0.198)	(0.277)	(0.345)	(0.465)	(0.866)
N	893	846	776	709	654	577	509	432	370	305	209
Adj. R^2	0.021	0.017	0.012	0.014	0.013	0.039	0.036	0.012	0.035	0.049	0.059
1 if love marriage	0.005	0.005	0.010	0.010	0.008	0.005	−0.001	−0.011	−0.031	−0.049	0.029
	(0.005)	(0.006)	(0.006)	(0.007)	(0.008)	(0.010)	(0.012)	(0.016)	(0.021)	(0.027)*	(0.062)
N	900	851	782	714	658	580	512	434	371	307	211
Adj. R^2	0.109	0.120	0.108	0.116	0.102	0.152	0.165	0.141	0.191	0.240	0.043
1 if living in premarital LC1	0.011	0.007	0.004	0.007	0.004	−0.003	−0.004	−0.015	0.008	0.039	0.013
	(0.008)	(0.009)	(0.010)	(0.011)	(0.013)	(0.016)	(0.019)	(0.025)	(0.034)	(0.046)	(0.093)
N	903	854	783	716	658	579	511	434	371	306	210
Adj. R^2	0.204	0.210	0.209	0.233	0.212	0.187	0.201	0.179	0.143	0.227	0.156
1 if polygyny	0.002	−0.001	−0.006	−0.001	0.004	0.006	−0.005	−0.011	0.002	−0.003	−0.016
	(0.007)	(0.008)	(0.008)	(0.010)	(0.012)	(0.014)	(0.017)	(0.021)	(0.027)	(0.038)	(0.068)
N	909	860	789	720	662	583	515	437	374	309	212
Adj. R^2	0.020	0.026	0.028	0.010	0.018	0.031	0.038	0.082	0.092	0.107	0.113
1 if divorced or separated	0.011	0.019	0.022	0.017	0.015	0.012	0.033	0.019	0.011	0.019	0.023
	(0.008)	(0.009)**	(0.011)*	(0.012)	(0.014)	(0.017)	(0.022)	(0.030)	(0.037)	(0.050)	(0.106)
N	850	806	739	676	624	550	483	412	351	290	199
Adj. R^2	0.060	0.054	0.045	0.056	0.051	0.042	0.043	0.036	0.084	0.066	0.090

Source: Research on Poverty, Environment, Agriculture, and Technology survey in Uganda in 2015.

Note: This table shows the coefficient estimates of the interaction between the year of birth minus the cutoff of 1979 and the indicator for it being equal to or larger than the cutoff from the regressions of marital characteristics listed in the leftmost column. All regressions include a constant, the year of birth minus the cutoff, and the dummies for ethnicity and region of residence at age seven as covariates. The regressions are run for women aged 24 to 49 years who have ever married and were born in years within the indicated bandwidth of the cutoff. Reported in parentheses are standard errors robust for heteroskedasticity. Statistical significance is denoted by *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$.

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these characteristics were generally unchanged. No significant effect of UPE exposure is observed for whether the marriage is polygynous, whether a woman lives in a premarital community, or whether she is not divorced by the time of the survey. The impact on whether marriage was arranged is also generally insignificant. Only the results for age at marriage indicate that women with a longer UPE exposure married later, but the estimates are significant only in the cases of the largest two bandwidths, and the point estimates are inconsistent across different bandwidths. These results suggest that the changes in the trends of female education and brideprice practice are unlikely to be related to changes in marital characteristics.

Another concern might be that the primary fee abolition attracted many girls to school and kept them unmarried until later ages. Since the treatment group was aged between 24 and 36 years at the time of the survey, some of them might not have married by then partly due to UPE. If this was accompanied by changes in the composition of married women with respect to unobserved factors influencing brideprice practice, they could have resulted in observed changes in the overall likelihood of brideprice practice. However, for such selective entry into marriage to drive the main results, women who are likely to receive a brideprice must have stayed away from marriage, while those who reject it have entered marriage. To the extent that female education is associated with female empowerment and autonomy, it might be natural to conjecture that younger cohorts who attained more education are likely to have become less likely to enter marriage early and less likely to have agreed to a brideprice at marriage. The possibility of selective entry into marriage is investigated in Section Sample Selection Due to Delayed Marriage.

5.3. Impact of the 2007 Brideprice Trial

Section Empirical Strategy argues that the results are unlikely to be confounded by reforms on sexual abuse against youth and the marital system. This section shows they are also unlikely to be driven by the effects of a legal trial against brideprice practice. Since the 2000s, criticism of brideprice practice has strengthened in Uganda. It was claimed that such a culture, combined with virilocality, might dehumanise women, and brideprice practice was referred to as *commoditisation* of women (Wendo 2004; MIFUMI Project 2009). Those against the practice filed a constitutional trial in 2007, for which the final decision was declared in 2015 (Library of Congress 2015). It may be that younger women who enjoyed less expensive primary education were increasingly exposed to the information regarding this trial and, as a result, asked their partner not to have a brideprice for their marriage. This hypothesis is investigated by estimating the main equation with additional control variables that capture how many years have passed between 2007 and the year of marriage as follows:

$$y_i = \eta_0 + \eta_1(z_i - c) + \eta_2 \mathbf{I}\{z_i \geq c\}(z_i - c) + \theta_1(m_i - 2007) + \theta_2 \mathbf{I}\{m_i \geq 2007\}(m_i - 2007) + W_i\phi + v_i,$$

where m_i denotes the year in which woman i married. Parameter θ_2 allows testing whether the legal trial filed in 2007 was associated with female education and brideprice practice. While controlling for this correlation, the impacts of UPE exposure can be estimated by η_2. The results in supplementary online appendix table S2.1 show that the main findings, the impacts of UPE exposure on female education and brideprice practices, indeed remain largely unchanged. The slight loss of significance is likely due to the collinearity between the year of birth and that of marriage.

6. Mechanisms for the Decline of Brideprice Practice

UPE in Uganda is shown to have created unexpected exposure to free primary education for younger cohorts and increased female education while decreased the likelihood of brideprice practice at first marriage. Now the study discusses the potential mechanisms for these changes. Specifically, the potential

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Table 4. Estimated Coefficients of Linear Regressions of Education, Brideprice, and Divorce

Dep. var.	(1) 1 if having brideprice paid	(2)	(3) 1 if having ever divorced or separated	(4)	(5) 1 if having ever divorced or separated	(6)
Years of education	−0.001	−0.002	−0.020	−0.018	–	–
	(0.008)	(0.008)	(0.008)**	(0.009)**		
1 if having brideprice paid	–	–	–	–	−0.320	−0.308
					(0.061)***	(0.063)***
Year of birth − 1979	–	Y	–	Y	–	Y
Year of marriage	–	Y	–	Y	–	Y
N	402	394	370	359	372	364
Adj. R^2	0.064	0.063	0.067	0.070	0.148	0.135

Source: Research on Poverty, Environment, Agriculture, and Technology survey in Uganda in 2015.

Note: This table shows the coefficient estimates of the year of birth minus the cutoff of 1979 and the dummy for whether a brideprice was paid at marriage. Reported in parentheses are standard errors robust to heteroskedasticity. Statistical significance is denoted by *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$. All regressions include a constant and the dummies for ethnicity and region of residence at age seven as covariates. The regressions are run for women aged 37 to 49 years who have ever married.

mechanisms for the decline of the brideprice practice through female education are first discussed, followed by alternative explanations that do not go through female education.

6.1. Relative Female Bargaining Power within Marriage

As discussed in the introduction, it is hypothesized that female relative bargaining power might have been strengthened within marriage as due to increased female education through the increased outside option. This in turn might have induced women to ask their partners not to pay a brideprice in settings where it must be repaid upon divorce.[28] The FGD participants suggested that if women with more education wanted to marry without a brideprice, they would ask their husbands not to pay it. This suggests that more-educated women may negotiate for marriage without a brideprice to enhance their utility from marriage.

The available data do not allow direct testing of the effect of UPE on female bargaining power at the time of marriage, as no indicator is available for it. However, it is likely to be one possible mechanism, and it is worth discussing the following two pieces of supporting evidence. First, for the above mechanism to operate, divorce must be a possible means to leave a relationship. In Uganda, it is indeed not uncommon. The Hague Institute for Innovation of Law (2020) reports that 7 percent of adult Ugandans experience a divorce or separation every year, and approximately a quarter of women in the data reported having ever divorced or separated (table 1). Second, if brideprice increases the cost of divorce and makes it harder for women to leave the marriage, a negative correlation should be observed between brideprice practice and the probability of marital dissolution. In fact, this study finds a positive association of UPE exposure with the dummy variable for whether a woman has ever experienced marital dissolution for all the available bandwidths, although it is statistically insignificant (table 3).[29] Additionally, among the non-UPE-exposed cohorts, those who married with a brideprice agreement were less likely to have experienced marital dissolution (columns 5 and 6 in table 4).

28 The FGDs suggested that the amount to be repaid can vary across the sample villages. For instance, participant women in one village told the authors that the entire brideprice must be repaid no matter what. Women in another village said that the repayment amount depends upon the reason for the divorce: if the divorce results from the husband's fault, the wife and her parents may be partly exempted from the repayment.

29 Note that the divorce and separation indicator is likely increasingly censored from the right for younger cohorts. The impact of UPE on divorce cannot be directly investigated since the available data do not contain the age of divorce.

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6.2. Sample Selection Due to Delayed Marriage

While the above-mentioned scenario is not inconsistent with the results, there are other possible mechanisms through which UPE leads to a decrease in brideprice practices. One alternative mechanism might involve more comprehensive marriage-market models, in which the matching equilibrium is affected by the expectations of prospective grooms, brides, and their parents, and their expectations are formed before marital partner search or even human capital investment decisions (see, e.g., Chiappori, Salanié, and Weiss 2017). In such comprehensive models, the marital match itself is likely to be influenced by the UPE reform because a reduction in the cost of education can affect all equilibrium decisions, such as educational attainment and marital outcomes, through expectation formation and the matching process. One potential implication from such models may pertain to the sample selection due to delayed marriage: UPE may have pushed younger women out of the sample of married women. If the women at the margin would have received a brideprice in the absence of UPE, the exclusion of these women would cause a spurious decline in the likelihood of brideprice practice among this study's sample. On the other hand, if women who were induced to pursue more education were not different from the rest of the women in terms of the tendency to receive a brideprice, sample selection is unlikely to explain the negative impact of UPE on brideprice practices.

To see whether sample selection can explain the results, this study estimates the impact of UPE exposure on whether women have ever married by age 24 using all women regardless of their marital status.[30] The results show that UPE had a negative effect on the likelihood of having ever married by age 24 (Panel A, supplementary online appendix table S2.2) when the bandwidth is nine years or larger. This is likely to reflect the fact that the probability is stable for the control group, while it is decreasing for the treatment group (supplementary online appendix fig. S1.2). If delayed marriage causes a bias in the main results, its impact is likely to be weakened by limiting the estimation sample to the bandwidths of eight years or fewer and excluding the five youngest cohorts who are more likely to be affected by censoring by 24 years of age.[31] The results are presented in Panels B and C of supplementary online appendix table S2.2. Under the specifications with bandwidths of five to eight years (columns 6–9), the effects on the years of education and brideprice practices have the same sign and are of similar magnitude to the main estimates, although they are of marginal significance, most likely due to the smaller sample size.[32] These results suggest that even if UPE delayed marriage, it did not push women who were likely to have a brideprice agreed upon out of the marriage market. Nevertheless, as this study cannot conduct a robustness check for the bandwidths of nine years and longer, it remains inconclusive whether this is a possibility.

The present study supplements this analysis by examining the distribution of age at first marriage among the control group and its correlation with average educational attainment as well as the probability of brideprice practice. This analysis is motivated by the idea that if sample selection were the underlying mechanism for the decline in brideprice practice, a later marriage would have been associated with a *larger* chance of brideprice practice in the absence of the UPE reform. The results show that in the common age range for marriage (13–21 years), no systematic correlation is found between the age of marriage and the likelihood of brideprice practice, while those who married later tend to have more education (supplementary online figs S1.3 and S1.4). These findings are further corroborated by conducting a *t*-test of the difference in education, as well as the probability of brideprice practice, between control women who married at younger ages and those who married at older ages (supplementary online appendix table S4.5). While this analysis lacks exogenous variation to precisely pin down the relationship among female education, marital age, and brideprice practice, the prereform absence of a correlation between the age

30 Twenty-four is the age of the youngest birth cohort in the sample, born in 1991, in the survey year 2015.

31 Specifically, the main regression equation is re-estimated by retaining all the control cohorts while narrowing the bandwidth for the treatment cohorts. The authors thank an anonymous referee for suggesting this analysis.

32 As discussed in Section Main Results, the results with bandwidths of three or four years are considered unreliable due to the small sample sizes.

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at marriage and brideprice does not seem to support the argument that later marriage, induced by UPE, made the women who would otherwise have married earlier and received a brideprice deferentially attrite from the sample and thereby spuriously reduced brideprice practice in the data.

6.3. Marital Squeeze and Matching Patterns

Comprehensive marriage-market models are likely to also imply that UPE can affect the number of women entering the marriage market at different ages and with varying levels of education, which in turn can affect the patterns of the marriage squeeze and assortative matching. Becker (1991) predicts that a brideprice can arise when there is a relative excess supply of men in the marriage market.[33,34] Under UPE, an increase in the number of educated women might have eased the competition among men for educated partners. As a result, the incentive for men to pay a brideprice might have decreased. If this mechanism were driving the main results, it is likely that educational assortative matching would have intensified as long as the preference for marrying a partner with a similar level of education did not change, since educated men are no longer constrained by the limited supply of equally educated women.

To investigate this, this study checks the trend in marital sorting in years around the cutoff.[35] Following Eika, Mogstad, and Zafar (2018), the marital sorting parameter is computed to examine whether there was a systematic change in the marital matching pattern between cohorts unexposed and exposed to UPE.[36] The results in supplementary online appendix fig. S1.5[37] show no systematically differential trend peculiar to cohorts born in 1979 or later or to more educated couples. These results suggest that a change in assortative matching is unlikely to drive the main results. Additionally, following Ashraf et al. (2020), the association is estimated between a couple's educational attainment intensified for the treatment group.[38] The results in supplementary online appendix table S2.3 show that, consistent with Ashraf et al. (2020), there was no differential assortative matching between the younger and older cohorts, although both indicate some degree of assortative matching.[39] These results again suggest that exposure to UPE is unlikely to have caused brideprice practices to decline through changes in marital sorting.

33 Polygyny may have created the relative excess supply of men in the marriage market. However, no statistically significant change is found in the share of women in polygynous unions around the cutoff (table 3).

34 Note that the model by Becker (1991) treats no brideprice as a payment of zero. In other words, it considers the extensive margin of brideprice as in a corner solution model. Thus, the association between female education and brideprice practice is restricted to be in the same direction at the extensive and intensive margins.

35 The authors thank an anonymous referee for suggesting this analysis.

36 The estimated parameter is the sample analogue of

$$\sigma(s^f, s^m) = \frac{\Pr(S^f = s^f, S^m = s^m)}{\Pr(S^f = s^f)\Pr(S^m = s^m)},$$

where S^f and S^m are the education levels of the married women and their partners. It measures the ratio of the likelihood of an actual match in terms of education to the likelihood that the same match would occur at random given the observed education distribution. Values larger than unity indicate that the observed matching pattern is assortative.

37 Panels (a) through (c) use the educational attainment categories that split the married female sample into five roughly equal-sized groups and combine different numbers of birth cohorts to compute the marital sorting parameter. Panel (d) uses educational categories that split the male partner sample roughly equally in size. The groupings are intended to reduce the small sample noise. It is shown that the grouping choices do not drive the results by using different ways to combine observations.

38 This study uses two measurements of education: years of education, as in the main analysis, and a dummy for primary completion, as in Ashraf et al. (2020). Noting the potential confounding due to changes in the marginal distribution of female education (Eika, Mogstad, and Zafar 2018), this study compares the results when flipping the male and female education variables.

39 While the change in assortativeness is not estimated to be statistically significant in terms of years of education (columns 1 and 2), it is slightly significant and negative in terms of the completion of primary education (columns 3 and 4). The findings remain unchanged when female education is used as the dependent variable (columns 5 and 6).

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6.4. Male Education and the Labor-Market Equilibrium Effect

This study has so far ignored the possibility that UPE affected male, not only female, education. If male education was also affected, the discussion must be revised to allow both sides of the marital partnership to change. However, no evidence is found that suggests that male education changed as a result of UPE exposure. When the effect of UPE exposure on male education is estimated using the data for males in the household survey data, UPE exposure was barely associated with male education (supplementary online appendix table S2.4). This is consistent with Keats (2018) and Masuda and Yamauchi (2020), who found that the reform had little impact on male education, and with Deininger (2003) and Nishimura, Yamano, and Sasaoka (2008), who reported a lesser impact of UPE on male education. The absence of an effect on male education is perhaps because it was already as large as completing the primary level among the control cohorts.[40]

Another channel might be the equilibrium effect in the labor market. That is, UPE can increase the share of educated women in the labor market, and the marginal returns to an additional year of schooling can decrease in equilibrium. If brideprice reflects the labor-market returns to female education and compensates the bride's family for her foregone contribution under virilocality, a decrease in returns to female education may reduce brideprice practice. However, the data show no significant change in the likelihood of women having a non-agricultural paid job (supplementary online appendix table S4.6).[41] Thus, the channel operating through changes in labor-market returns to female education may not be the most credible explanation for the findings on the rise in female education and the fall in brideprice practices.

6.5. UPE's Wealth Effect and Learning

While the above discussion has concentrated on the linkage between the UPE effects on female education and brideprice practice, the reduced-form estimation can also imply that the UPE effect on brideprice practice has nothing to do with its impact on educational attainment. One such channel that bypasses female education is the wealth effect of UPE. That is, free primary education may have relaxed the budget constraints of households with girls. In addition, the Ugandan reform is shown to have reduced family size (Burlando and Bbaale 2022), thereby indirectly further loosening the constraint. Through the quantity–quality trade-off (Becker and Lewis 1973), this may have improved the development of physical health and non-cognitive skills. These factors can in turn improve young women's outside options and relative bargaining power without necessarily operating through female education. As a result, a larger number of households may have declined to follow brideprice practices.

Another channel bypassing female education is related to the quality of learning. The sudden removal of primary school fees in Uganda may have compromised learning quality (Ogawa and Nishimura 2015).[42]

40 The authors thank an anonymous referee for raising another hypothesis that men, not just women, may dislike brideprice practice because, without it, they could bring more resources into their own households. While the lack of a significant change in male education does not seem to suggest that this hypothesis is the leading mechanism in the current data, this is likely to be an interesting hypothesis left for future studies.

41 Note that this study's outcome measures whether the surveyed women had a non-agricultural paid job *at the time of the survey* and hence may not directly reflect the expectation formed by the husband as of the time he married his wife. However, these results using ex post labor-market participation may still provide suggestive evidence. Additionally, Peet, Fink, and Fawzi (2015) show that the gender-specific returns to education in Uganda remained much the same in 2009 and 2010, when the women of the cutoff cohort were 30 or 31 years old. Although their estimates do not compare this study's treatment and control cohorts, they indicate that the channel through changes in labor-market returns to female education may not be the most credible explanation for the main findings on the rise in female education and the fall in brideprice practices.

42 As an informative indicator, supplementary online appendix fig. S3.5 shows a sudden increase in the pupil–teacher ratio in 1997, the year of UPE introduction. The ratio has since declined gradually but remained higher than its pre-UPE levels 10 years after the reform. For other dimensions of education related to the quality of learning, see Ogawa and Nishimura (2015).

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UPE may then have lowered women's productivity given their educational attainment and eventually decreased brideprice practices. Data limitations do not permit conducting even suggestive analyses regarding these pathways, and investigating them would be a fruitful avenue for future study.

7. Conclusion

Abolishing primary school fees under UPE policies has been shown to boost education, delay marriage, and decrease fertility. The aim of this study is to extend the literature and to show that UPE can influence women's marital life by reducing the likelihood of a brideprice payment, which can be an obstacle for women who want divorce in settings where husbands can demand its refund. This finding implies that UPE's impact on the brideprice practice can be expected in societies where divorce is conceivable. The obtained results imply that in the decade in which UPE covered all women in Uganda (1979 to 1988), the likelihood of brideprice practice decreased by 20 to 31 percentage points, or approximately one-third of the prereform average. These results imply that UPE is likely to affect women's lives not only by improving their economic and reproductive outcomes, but also by expanding their choice of marital arrangements.

Based on the literature on brideprice and the field group discussions, this study conjectures that the decline in brideprice practice may be related to female bargaining power within marriage, which is likely to improve as female education increases their outside options. Several channels through which UPE alters brideprice practices are explored. However, UPE exposure is not found to have affected other marital characteristics, such as polygyny or whether a woman lives in a premarital community. Other mechanisms are also discussed, such as selective entry into the marriage market, marital squeeze, and assortative matching, as well as non-educational factors such as wealth effects of UPE on the natal family.

While UPE has likely expanded women's choice over marital arrangements, the long-term welfare of women who actually divorce remains unclear. It would be fruitful for future research to investigate the impact of UPE on long-term marital history and other measures of overall welfare for women and their families in settings with a non-negligible probability of divorce. Additionally, given that the UPE impact on brideprice is likely to alter factors affecting parental incentives to invest in their daughter's education, it would also be intriguing to examine how parental expectations for returns on daughters' education from the labor and marriage markets evolve.

Finally, it is worth discussing the limitations of this study. First, it is a descriptive exercise investigating trend breaks across cohorts in female education and brideprice practices. Second, the data in this study are insufficient to test some of conjectures on the mechanisms discussed due to data limitations. A more rigorous data collection and analytical framework overcoming these limitations is, therefore, likely to further benefit the literature.

Data Availability

The data underlying this article are available in the article and in its online supplementary material.

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