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Abstract

Brideprice is a marital wealth transfer from a groom to his bride's parents. Many developed countries once had this practice and witnessed its decline, while some less developed countries are in the middle of cultural change. Economic development has been indicated as a closely related factor, but little is known about exactly what contributes to the decline of brideprice practices. Based on the regression kink design framework exploiting primary education reform in Uganda, we find that female education reduces the probability of a brideprice payment. This finding suggests that female education is one of the factors facilitating cultural decline and helps explain the disappearance of the brideprice practice in contemporary high-income countries and its current decline in Africa. We do not find evidence that the human capital compensation hypothesis or assortative matching explains the decline in the brideprice practice. We consider as the potential mechanisms (1) the trade-off for the bride's parents between immediate brideprice payment and altruistic utility from their daughter's sound marriage without brideprice and (2) the intrahousehold bargaining power between the bride and groom.

Keywords: brideprice, female education, culture and institution, fuzzy regression kink design, universal primary education, marital institution, Uganda.

JEL classification codes: I21, I25, O55, Z13.

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1 Introduction

Brideprice is a marital wealth transfer sent from the groom to the parents of the bride.[1] Brideprice used to be practiced in many parts of the world, including contemporaneously high-income countries, where the amount of brideprice payment is reported to have been so significant that it sometimes represented a large burden, particularly for poor households (Anderson, 2007). Although the practice has been largely discontinued in such countries, it is still prevalent in many parts of Africa. Anecdotes indicate that brideprice serves as a token of gratitude to the bride's parents for their efforts in raising their daughter. Observationally, brideprice is practiced more frequently in areas with high virilocality—the bride leaves her natal family and joins the groom's family upon marriage—polygyny—a man marries more than one woman—and high female engagement in household agriculture (Anderson, 2007; Goody, 2011).

The economic literature on brideprice suggests that it compensates the bride's family for the labour income that she would have earned and contributed to her family if she were not married (Becker, 1991; Anderson, 2007). In this framework, brideprice positively reflects female education to the extent that human capital matters for her foregone economic activities.[2] Recent studies have also considered the brideprice practice as an indicator of greater female agricultural engagement after marriage (e.g., Jacoby 1995). Partly due to this economic contribution, females from ethnicities that have historically practiced brideprice are found to be less likely to suffer from domestic violence (Alesina et al., 2016). When the costs of primary education are lowered, only parents from the

[1] Anecdotes suggest that there are cases in which the groom's parents help him with this wealth transfer. There are also cases where this wealth transfer is only agreed upon at the establishment of the marriage, and the actual payment is made later. In this particular study, we use data from Uganda, where it is usually only the groom who pays, and the transfer can be made later. We discuss these particular contexts in more detail in the subsequent sections.

[2] The positive association between brideprice and female human capital is also posited in social sciences other than economics (e.g., Bell 1998; Goody 2011).

ethnic groups practicing brideprice are shown to invest in female education (Ashraf et al., 2020). In addition to the human capital hypothesis, upon which these studies are based, assortative matching in the marriage market also implies a positive association between brideprice and female education. That is, educated women are likely to marry educated men who are likely to be from wealthy families.

This stream of the literature takes brideprice culture for granted and, within a given cultural framework, analyses the positive relationship between the human capital accumulation of females and the amount of brideprice. Since an increase in female education is a typical phenomenon during economic development, it may lead more parents to invest more in female education and demand marital wealth transfer, as demonstrated by (Ashraf et al., 2020). Parents of daughters, in fact, seem to consider marital transfer an important financial source, at least in the short run, since early marriages of daughters are found to increase during periods of transitory economic shocks in societies with brideprice culture (Corno et al., 2020). It may thus be unsurprising if, as a result, the share of marriages with a brideprice payment increases in a country with an increase in female education. However, history in the longer run suggests the opposite: brideprice culture has *disappeared* during economic development, despite an increase in female education. As Anderson (2007) points out, little is known about how brideprice culture has diminished and how it is related to the process of economic development.

Other studies have examined the strategic behaviours of the husband, the bride, and the bride's parents when bargaining over the brideprice payment. For example, the existence of a brideprice payment at marriage is found to be correlated with an increased probability of divorce (Platteau and Gaspart, 2007). Gaspart and Platteau (2010) models and empirically tests the possibility that the parents of the bride may accept a lower brideprice if they worry about the likelihood of their daughter's divorce and the resulting loss of altruistic utility. Although these analyses consider bargaining over the *amount of the brideprice*, it is possible that these strategic motives lead the marriage to involve no brideprice at all. We thus intend to empirically analyse when such a cultural decline occurs and whether it is related to an increase in female education.

Specifically, we estimate the effect of female education on the cultural practice of brideprice payment, exploiting the introduction of universal primary education (UPE) in Uganda. This reform abolished school fees for all pupils who were enrolled in primary schools in and after 1997. The reform was announced in December 1996 and initiated in January 1997 and thus was almost surely unpredictable by Ugandan families. Since it affected everyone who was in school at the time it was introduced, the benefits of the reform were lower for older cohorts and greater for younger cohorts. As such, the increased years of education shows a clear kink when shown in a graph on the year of birth. This exogenous source of variation is used in our fuzzy regression kink design estimation to investigate the relationship between female education and brideprice culture.[3]

While it is difficult to collect historical data from countries that have already experienced a decline in brideprice practices, countries that are going through relevant cultural changes provide an ideal case for studying the factors contributing to the disappearance of brideprice culture. Uganda is a particularly suitable case, as almost 100% of its population had practiced brideprice until approximately the 1980s. However, recently, this practice has been diminishing (Anderson, 2007). Together with this decline in brideprice payments, there has been a significant increase in female educational attainment due

[3]We find that the reform increased educational attainment among females but not among males, whose educational attainment had already been above the primary level prior to the UPE. This finding is consistent with that of Keats (2018) and implies that the effect of education is not realized by males but rather by females.

primarily to the UPE, which was not anticipated.

We first find that being born one year later increases educational attainment for females who were exposed to UPE by approximately .2 to .4 years, in addition to the general trend observed for females who were not exposed to UPE. Exploiting this as an exogenous variation, we then find that a one-year increase in female education reduces brideprice practice by approximately 9.9 to 12.3 percentage points. At the same time, we find no evidence that more years of education lead to an increase in the non-agricultural job status of women. We do not find any change in assortative matching in terms of male and female education or in marital characteristics such as polygyny and love marriage. These results indicate that the decline in brideprice payment is not due to a change in the valuation of female productivity or a matching pattern in the marriage market. In other words, the results suggest that the decline in brideprice culture may not be consistently explained by conventional theory, such as assortative matching and human capital compensation.

From our estimation results, we discuss possible mechanisms through which female education led to this cultural decline. That is, better educated females and their parents may become more aware of the potential downsides of brideprice, such as differential practices of sexual infidelity between spouses (Bishai and Grossbard, 2010), domestic violence (Kaye et al., 2005), and divorce (Platteau and Gaspart, 2007; Gaspart and Platteau, 2010), which are likely to reduce the utility of married females.[4] When the parents of better educated females make a decision about whether to receive a brideprice payment, altruistic parents face the tradeoff between the immediate payment of brideprice and the future sound marital life of their daughters. If their altruistic utility increases in

[4]From local media reports, it appears that brideprice practice is still common in Uganda, where these potential downsides of brideprice are also sometimes discussed (New Vision, 2016a,b; Daily Monitor, 2016a,b, 2017, 2018a,b,c, 2019).

female education, they may choose not to receive brideprice payment. This interpretation follows the discussion by Gaspart and Platteau (2010). Another hypothesis based on anecdotal evidence from our field work is that better educated brides are likely to have greater bargaining power relative to their grooms and may thus demand marriage without brideprice payment. This is because the presence of brideprice payment may increase female disutility from marriage due to domestic violence and differential spousal infidelity. It may also raise the cost of divorce, as the refundable brideprice serves as debt for the female and her parents.

As Gaspart and Platteau (2010) emphasize, these proposed mechanisms do not rule out the existing models. Rather, we claim that one next step for future study is to unify these complementary approaches. We discuss a potential direction of future research toward unifying these approaches.

We contribute to the brideprice literature by providing a new piece of evidence that the cultural practice can change and decline when female education increases. Most studies in the brideprice literature have examined the *intensive margin*: within a given cultural framework, they analyse the relationship between the amount of brideprice and socio-economic variables, including human capital investment. Our study considers the *extensive margin*: we investigate when and why brideprice culture declines. Our finding suggests that female education is likely one of the factors that facilitate cultural decline and help explain the disappearance of brideprice practice in contemporary high-income countries and its current decline in Africa. Additionally, we provide the first empirical evidence based on a natural experiment on the impact of female education, as the available studies have relied on cross-sectional variation due to data limitations. More broadly, our study is related to the growing literature on culture and institutions (Alesina and

Giuliano, 2015). In particular, it shows evidence of how an institution that is aimed at accelerating human capital accumulation affects local culture in the course of economic development.

The rest of this paper is structured as follows. The next section provides a brief overview of the related economic literature. After a summary on primary education reform in Uganda, we describe our dataset and identification strategy. Then, we present the estimation results and discuss our interpretation. The last section concludes by discussing implications for Uganda and for future studies.

2 Literature Review

2.1 Marriage and brideprice in Sub-Saharan Africa

Wealth transfers upon marriage are termed differently depending upon who sends and receives them. They are called brideprice if the groom and his family send them to the bride's parents and dower if they send them to the newly married couple, particularly the bride. Similarly, transfers from the bride's side are called dowry if the groom receives them and groomprice if the groom's family receives them (Papps et al., 1983). Among these four channels, we focus on brideprice, which is predominant in Sub-Saharan Africa (Fafchamps and Quisumbing, 2007).

The economic research related to brideprice can be traced back to at least the seminal work by Becker (1991) on the marriage market, in which women and men search for their marriage partner. His model suggests that assortative matching and compensation for human capital can affect brideprice.[5] First, assortative matching—men and women with

[5]Marriage squeeze, another potential factor that can affect brideprice, is a situation in which the numbers of men and women are unbalanced in the marriage market, at which point marital payment arises for the purpose of clearing the market. An excess supply of men in the marriage market incentivizes

similar traits are more likely to marry than are those with dissimilar traits—suggests that a more educated woman is likely to marry a more educated man who is also likely to come from a wealthier family. Thus, a more educated woman may be paid a larger brideprice than a less educated woman. In Sub-Saharan Africa, assortative matching is found to have a strong influence on marital formation and wealth transfer in Ethiopia (Fafchamps and Quisumbing, 2005a,b).

Second, the human capital compensation hypothesis suggests that brideprice is larger for women with greater human capital because it compensates their parents for letting go of a family member that could provide labour (Becker, 1991). This hypothesis can be particularly important in Sub-Saharan Africa, where women's contribution to household agricultural production is significant, and virilocality is common (Anderson, 2007). However, empirical evidence on the human capital compensation hypothesis is relatively scarce in the literature; the available studies include Platteau and Gaspart (2007) and Ashraf et al. (2020).

Platteau and Gaspart (2007) show that brideprice is significantly higher for educated women compared to uneducated women in Senegal based on small cross-sectional data. Ashraf et al. (2020) employs a quasi-experimental method to examine the impact of female education on brideprice. Exploiting the regional variation in school openings and the fact that only a subset of ethnic groups practice brideprice payment, their triple difference estimation shows that school construction booms in Indonesia and Zambia increased the bride's level of education only among brideprice-practicing ethnic groups, which then led to larger brideprice payments. The above authors claim that brideprice

them and their parents to pay a higher brideprice (Becker, 1991). This situation, however, appears less relevant in Sub-Saharan Africa, where son preference is not found (Rossi and Rouanet, 2015). In contrast, marriage squeezes have been investigated intensively in South and East Asia, where son preference is observed (e.g., Rao 1993; Francis 2011).

culture incentivises parents to invest in girls' education, which indeed leads to higher brideprice payments.

One seemingly missing piece in these studies is viewpoints on whether the cultural practice of brideprice payment has declined and, if so, whether this decline is in any way related to female education. Instead, within the given cultural framework, these studies consider how education and brideprice interact. That is, the human capital compensation hypothesis claims that brideprice increases when females have greater human capital, since it can make her family better off through greater household income and more efficient home production. The assortative matching hypothesis also states that brideprice is larger for highly educated women than for less educated women because highly educated women are likely to be matched with a highly educated, and therefore wealthy, man. These hypotheses are concerned with the intensive, and not the extensive, margin of cultural practice, which seems to be a serious theoretical limitation, as societies experiencing major changes such as an educational reform can exhibit changes in the extensive margin; *i.e.*, individuals can stop practicing brideprice. This paper sheds light on this issue and attempts to empirically investigate when cultural practice declines.

Another strand of the literature has analysed the strategic behaviours of the groom, the bride, and her parents to further deepen the economic understanding of the causes and consequences of the brideprice practice. Bishai and Grossbard (2010) postulate and show that brideprice payment in Uganda is associated with a lower probability of adultery on the part of the wife but not the husband. The proposed logic behind this finding is that the payment of brideprice that the husband can recover upon marital disruption increases his intrahousehold bargaining power, suppressing his wife's extramarital relations but not his own. Gaspart and Platteau (2010) analyse the decisions of the husband and the bride, in addition to those of her father, who is altruistic towards the bride, where the agents face a bargaining situation over the brideprice. To the extent that the bride's parents care about the probability of her marital dissolution, modelled as a function of brideprice, or that parental altruistic utility depends on the bride's persuasion, which may be affected by her educational attainment, her parents may decrease the amount of the brideprice, for which the authors show empirical evidence.[6] These studies imply that an increase in female education may lead to a rise in her bargaining power against her husband or her father and thus to an overall decline in brideprice practice. However, findings from these studies may not bear causal interpretation due to the lack of a credible identification strategy and small sample sizes. Our study attempts to provide the first piece of evidence by employing a novel causal estimation method applied to our own collected data and interpreting our findings in light of anecdotes assembled from our field work.

2.2 Literature on culture and institutions

From a broader perspective of the economic literature, our present paper can be contextualized as an empirical study on the relationship between culture and institutions. According to a recent survey (Alesina and Giuliano, 2015), one approach is to build a model in which culture and institutions affect each other and analyse their co-evolution (e.g., Alesina et al. 2015 and Fernández 2013). Studies using this approach, however, inevitably face empirical difficulty in identifying the two-way influencing system (Alesina and Giuliano, 2015).

The other approach is to analyse the one-way influence of culture on institutions or vice versa. In this stream of the literature, it seems that many studies have looked at

[6] As Gaspart and Platteau (2010) note, these analyses do not entirely replace the approach based on marriage market interactions (Becker, 1991); rather, they complement the behavioural analyses by bringing individual strategic motives into the analytical framework.

the effects of culture on economic variables: examples include the effect of individualistic norms on agricultural production (Olsson and Paik, 2016) and the effect of witchcraft beliefs on social capital formation (Gershman, 2016). A relatively limited number of studies have examined the effects of institutions on cultural variables, such as the land titling effect on individualism and materialism (Di Tella et al., 2007) and the impact of commercial legislation on church going and religious donations (Gruber and Hungerman, 2008). Our present paper adds a new piece of evidence to this strand of the literature, as it investigates the effect of a formal institution—a primary education policy—on a cultural behaviour—brideprice practice.

The study most closely related to ours is Ashraf et al. (2020), which examines the effect of female education on brideprice. While the above authors consider the intensive margin of the brideprice culture—whether female education increases brideprice—our current study focuses on the extensive margin—whether female education renders a decline in brideprice culture. By providing new empirical evidence on the effect of female education on brideprice culture, our study contributes to the emerging literature on the relationship between culture and institutions.

3 Universal Primary Education in Uganda

The educational system of Uganda consists of 7 years of primary education, 6 years of secondary education, and 3 to 5 years of tertiary education. Children are supposed to commence schooling at the age of 6. The net enrolment rate was low before the start of universal primary education (UPE): among children aged 6 to 12 years, 62.1% attended primary school in 1992 (Deininger, 2003). There were also large disparities across income levels and geographic locations. For example, the attendance rate was higher in urban areas (78%) than in rural areas (66%) (Demographic and Health Survey, 2004).

A major impediment at that time was said to be the costs of schooling, both direct and indirect, borne by parents and family. Tuition paid by households covered more than 80% of the total finances of schools (Nishimura et al., 2008). Households also paid other costs of education, including costs for uniforms, textbooks, and contributions to the Parents and Teachers Association (PTA). These costs were likely a heavy burden particularly for poor households. In fact, only 45.7% of children from low-income households attended school, while the equivalent figure was 81.7% for those from high-income households (Demographic and Health Survey, 2004).

To tackle the financial problem of education, a reform called 'universal primary education' (UPE) was initiated in January 1997 that eliminated school fees (Uganda Bureau of Statistics, 2003). Specifically, the UPE scheme provided each school with enough funding to cover private education costs such as tuition and PTA contributions (Ministry of Education and Sports, 1999). Uganda had, by then, put into practice a variety of educational reforms likely to bring about qualitative improvements in education, such as curriculum changes, teacher training, and primary completion examination criteria. However, it was not until December 1996 that the abolition of school fees was announced by President Museveni, who had been elected in May 1996 (Grogan, 2008). The announcement was followed by a governmental advertising campaign, which informed nearly all parents and guardians of school-age children of the reform (Demographic and Health Survey, 2004).

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 enrolment rate rising from 74.3% in 1995 to 135.8% 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 et al., 2008). Additionally, the reform reduced delayed enrolment and increased the probability of completing higher grades (Nishimura et al., 2008). Although there are still quite a few people who never attend or who drop out of school (Demographic and Health Survey, 2001), it would not be an exaggeration to say that Uganda's UPE has been successful in increasing educational attainment. We take advantage of this sudden increase in education brought about by the UPE to investigate the impact of education on brideprice practice. The exogeneity of this policy change is discussed in more detail in Section 5.2.

4 Data

4.1 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 dataset from the first wave in 2003 to the fifth wave in 2015. In 2003, 10 randomly chosen households were interviewed from each of 94 randomly chosen rural villages from the eastern, western and central districts (Yamano et al., 2004). 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 dataset of 1,755 households from 117 villages. The RePEAT survey is designed to collect data on the agricultural activities of rural households. In the fifth wave, a specially made questionnaire was added to ask about individual characteristics and behaviours at the time of marriage, including brideprice payment.

The questions about brideprice payments were asked only for each female's first marriage for the following reasons. First, the brideprice agreed upon at the time of the first marriage is likely to correctly measure the value of 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 be different than that in the first marriage, and these differences may depend on many factors, such as the gender of the person who remarries;[7] whether the remarriage follows divorce, separation, or widowhood; and unobservable factors including feelings about remarriage. This study avoids the analytical complications arising from these complexities by focusing only on women's first marriage.

This study sample consists of females who had ever been married at the time of the survey and were between 24 and 49 years of age.[8] We imposed this age restriction expecting to obtain a representative sample of females in this age group in rural Uganda because the median age at first marriage was 17.9, and the share of females who had never married in the age group between 25 and 29 years was 5.6%, with a sharp decline from 23.9% in the age group between 20 and 24 years (Demographic and Health Survey,

[7]For example, Fafchamps and Quisumbing (2005b) find that men and women remarry with significantly different probabilities in Ethiopia, the country just north of Uganda.

[8]Attempts were made to ask questions of the females themselves. However, a proxy interview was allowed if the female was unavailable and the alternative respondents knew a great deal about her first marriage. Another attempt was made to collect information from male respondents, since if a male interviewee had remarried and his first partner was no longer available in the RePEAT survey, asking him about his first marriage would increase the sample size of the study. However, the use of such male-queried data was abandoned because for such female partners, other critical information (collected in the education and demography sections that were used for existing household members only) is missing, and thus, regression analyses could not be performed.

2012).

4.2 Major variables

Females aged 24 to 49 years who had ever been married were asked whether their first marriage partner agreed to pay any amount in some form of brideprice.[9] The survey also collected information about the characteristics of the first marriage and of the women in their first marriage, such as the year of the marriage and whether the marriage was based on love or arrangement.[10] Furthermore, the survey asked about the respondent's religion and location of residence before and after the first marriage to identify any changes due to the marriage.

To construct a variable measuring educational background, this study uses the survey responses on the highest grade of education achieved by each individual. Comparing these responses to the education system in Uganda, the minimum years of schooling required to achieve the person's highest grade of education is calculated. This variable is used as the years of schooling throughout the paper.

[9]If the answer to this question was 'yes', then females were further asked about the amount of 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 so problematic that the cash amount was barely comparable to cattle and other brideprice amounts due to rampant inflation rates in the late 1980s and early 1990s, when there was internal conflict in Uganda. Therefore, the data on the value of brideprice payments are not used in this study.

[10]The survey attempted to collect information about the land holdings 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.

5 Estimation Strategy

5.1 Regression kink design estimation

In evaluating the impact of education on brideprice, it is important to note that the UPE introduced free primary education to all those who were enroled in primary school in and after 1997, regardless of their year of birth.[11] Since older females were less likely to remain enroled in primary schools in any given year, they were less likely to be exposed to the reform. This aspect is clearly illustrated in Figure I, where the share of females who were enroled in primary schools in 1997 is zero for cohorts born in or before 1978[12], whereas it is increasing for younger cohorts born in or after 1979. This aspect is likely to have resulted in a kinked increase in their years of education, which is likely to show accelerated growth for younger females who were increasingly exposed to free primary education over a longer period of time. We confirm this finding in Figure II, where educational attainment is stable for females within the non-UPE-exposed cohorts (born in or before 1978), while it exhibits a steady increase for females within the UPE-exposed cohorts (born in or after 1979).[13] Our aim is to exploit this kinked increase in the years of education to causally estimate the effect of female education on brideprice transactions in Uganda. The nature of the reform implies that the pattern of exposure is better characterized by

[11] 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 enroled in primary school (Grogan, 2008).

[12] The measurement of enrolment status in 1997 is based on a survey question that asked for the year in which each person left primary education. In the survey, we asked about attendance at a UPE-funded school and found quite a few females who reported having attended UPE-funded schools and having already left their primary school by 1997. This fact may be due to misreporting, but it is more likely that our question was simply misunderstood: we asked whether they attended a UPE-funded school and did not clarify whether the school was free while they were enroled there. In this case, since the UPE abolished tuition fees at all primary schools, females may have been confused about this question, particularly if they acquired some posterior knowledge about the nature of the UPE to answer our survey question.

[13] The vertical line indicates the year 1979, our cutoff that divides the control and treatment cohorts in our estimation described below. For details, see Section 5.2.

regression kink design (RKD) estimation than by regression discontinuity design (RDD) estimation, which has been used by previous studies (Keats, 2018; Behrman, 2015). While the existence of a jump in the share of females exposed to the reform, or in the years of education, is a necessary condition for the RDD estimation, the figures also fail to exhibit such a jump at the cutoffs proposed by previous studies (1983 in Keats (2018) and 1984 in Behrman 2015).

[Figures I and II about here]

Based on these considerations, we employ fuzzy RKD (FRKD) estimation, which takes the years of education as the treatment variable and the year of birth as the running variable.[14] Let y be a generic outcome, s be the years of education, z be the year of birth, and c be the cutoff. Then, the treatment-on-the-treated (TOT) parameter in the FRKD, τ, is expressed (Card et al., 2015) as follows:

$$\tau = \frac{\lim_{z_0 \to +c} \left. \frac{d\,\mathrm{E}(y|z)}{dz} \right|_{z=z_0} - \lim_{z_0 \to -c} \left. \frac{d\,\mathrm{E}(y|z)}{dz} \right|_{z=z_0}}{\lim_{z_0 \to +c} \left. \frac{d\,\mathrm{E}(s|z)}{dz} \right|_{z=z_0} - \lim_{z_0 \to -c} \left. \frac{d\,\mathrm{E}(s|z)}{dz} \right|_{z=z_0}} \tag{1}$$

where the change in the first-order derivative of the conditional expectation of the outcome at the cutoff is evaluated by the change in the first-order derivative of the conditional expectation of the years of education.

To obtain the estimate of the TOT parameter, we follow the literature (Lundqvist et al., 2014; Manoli and Turner, 2014; Tirgil et al., 2018) and estimate the following

[14] It would be ideal if we had more precise measurements, such as date of birth. However, birth year is the most precise measure in our data.

model by two-stage least squares regression, specified as follows:

$$y_i = \alpha_0 + \alpha(z_i - c) + \beta\hat{s}_i + W_i\phi + u_i \tag{2}$$

$$s_i = \gamma_0 + \gamma(z_i - c) + \delta\mathbf{I}\{z_i \geq c\}(z_i - c) + W_i\psi + v_i \tag{3}$$

where $\mathbf{I}\{A\}$ denotes an indicator function that takes the value of one if condition A in the following bracket holds and zero otherwise, $\hat{s}_i$ denotes the predicted years of education from equation (3), and W denotes premarital controls. This two-equation model is estimated for females of h cohorts below and above the cutoff, c. The parameter β represents the parametric analogue of the TOT parameter, τ, based on the assumption that the conditional expectation functions of y and s are linearly specified.[15] For statistical inference, we compute the heteroscedasticity-robust standard errors and perform the usual two-sided significance test.[16]

5.2 Identification assumptions and the choice of cutoff

The identification of β hinges upon the exogeneity of the introduction of the UPE, which in this case is that Ugandan females could not precisely manipulate whether they attended a UPE-funded school (Lee and Lemieux, 2010). This condition is likely to be met in our setting for the following two reasons.

First, the results of the presidential election preceding the introduction of the UPE

[15] As in Ashraf et al. (2020), we considered using additional exogenous variation to compare females from ethnicities with and without the historical practice of brideprice. However, we abandoned the use of such information after merging the historical data of the Ethnographic Atlas (Murdock, 1967) and its updates (Gray, 1999), since all the ethnic groups in our data (except for non-Uganda nationals) have had a substantial amount of brideprice payments in the past, as shown in Appendix Table B.1.

[16] According to the extant literature, everyone enroled in primary school was the beneficiary of UPE, which suggests that exposure to UPE is equivalent to leaving primary school in or after 1997. We do not use this information explicitly in the estimation, primarily because it changes the research question—from examining the effect of a one-year increase in female schooling on the cultural practice of brideprice to that of UPE exposure— at the expense of richer information on treatment intensity.

were *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 candidates, he lost in quite a few districts in the northern region and in some in the central and eastern regions to the second-place candidate, Ssemogererere (Uganda Electoral Commission, 1996). Moreover, Museveni's then 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 who supported Museveni 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 the members of Parliament.[17] These two elections within a year are likely to have created large uncertainty over the subsequent politics of Uganda.

Second, President Museveni was said to be reluctant to implement the UPE, and he arguably placed a larger emphasis on infrastructure development in his economic development planning. Furthermore, the government as a whole, and not just 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.[18]

To strengthen the validity of this identifying assumption, we choose 1979 as the cutoff

[17]Large national projects such as the UPE usually require approval by Parliament in regard to the budget and implementation plan. Thus, Museveni's victory in the presidential election would have been insufficient to put the UPE into practice.

[18]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 the 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 the manifesto. The proportion of the politically literate in our rural sample data is likely to be small, judging from the fact that the government launched a massive political 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, then the government would not have needed such a massive campaign.

for the year of birth, which is the threshold below which the share of females exposed to the UPE is zero and above which it is increasing (Figure I). This cutoff choice is likely to further increase the credibility of our identification, since with this cutoff, the average years of education show a clear slope change and no level change (Figure II), which satisfies the continuity assumption of the treatment variable in the RKD estimation (Card et al., 2015).[19] Additionally, compared to later years chosen by previous studies (Behrman, 2015; Keats, 2018), our cutoff raises the opportunity cost of schooling, which intuitively is higher at an older age, when females could otherwise work as family labourers or for wages outside of the household. The larger opportunity cost makes it more difficult for females in the older cohorts to manipulatively remain enroled in primary school. In sum, the sudden introduction of the UPE and our choice of cutoff are likely to validate our identification.

Our identification may fail if the effect of UPE on brideprice practices is brought about by any factor other than a change in female education in a kinked manner. One such factor is male education, and the direction of the potential bias in the estimate of β arising from the effect of UPE on male education is ambiguous. For instance, the bias may be positive if males with greater educational attainments are more likely to offer brideprices or pay a larger brideprice, as their natal family is likely to be wealthier or they earn more in the labour market. Conversely, the bias may be negative if such males have better negotiation skills and persuade the bride's parents into a marriage with no brideprice or a lower brideprice. However, as we discuss in Sections 6.4 and 6.5, UPE did not significantly affect male education.

Our identification may also fail if this educational reform triggered reforms of other

[19] Specifically, Assumption 3a of Card et al. (2015) rules out the case in which the treatment variable (years of education in our setting) has no level change at the cutoff.

policies and legal systems related to brideprice practices. One possibility is the amendment of the penal code that re-defined the defilement of youths under the age of eighteen, which is the same as the legal marital age in Uganda. 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). However, the amendment of the penal code came into effect in 2007, when females in our oldest treatment cohort were 28 years old and thus not subject to the reform. Therefore, it is unlikely that our estimation exploiting the kink for cohorts born in 1979 or later will fail due to this concern.

Another potential confounding reform is the marriage law, for which many bills have been drafted and discussed since the Constitutional Court declared in 2004 that the laws governing marriage and divorce were inconsistent with the Constitution adopted in 1995 (Okello, G. M. & Ors., v. Attorney General, 2004). Uganda Law Reform Commission (2010) created the basis for a series of bills, including those in 2009, 2013, and 2017. However, none of these bills has been put into effect to date, and thus, no major reform has been made with regard to the marriage act. Therefore, these bills have had barely any meaningful impact on the marriages in our data.[20]

6 Results

6.1 Descriptive analyses

Table I presents the summary statistics of the major variables from our data for the older control group (born in or before 1978) and the younger treatment group (born in

[20] Similarly, policies pertaining to HIV and AIDS, such as condom provision and anti-stigma campaigns (Tumushabe, 2006), are unlikely to have created a kinked relationship for cohorts born in our cutoff year and contaminated our findings.

or after 1979). Panel A shows that the proportion of females from the Langi, an ethnic group mostly in the northern region of Uganda, is larger in the treatment group than in the control group. This finding may be related to the internal conflict in the 1980s and 1990s, but we do not have a clear explanation for these significant differences. Therefore, dummies for the location of residence at age 7 and ethnicity are always included in our regression analyses.

Panel B shows that the treatment females are indeed more educated than the control females. The years of education increased by approximately two years on average, and the treatment females enrolled in primary school at an earlier age and left school at an older age than did the control females. The treatment females were more likely to repeat at least one grade in primary school and to proceed to secondary and tertiary education.

[Table I about here]

Panel C presents a summary of marital characteristics. While the probability of having ever married is lower for the younger treatment females, this is most likely because they are younger than those in the control group.[21] When we focused on those who had ever married, the proportion of love marriages slightly decreased and that of local mating increased. The share of females in a polygynous union shows a relatively large and significant change between the treatment and control groups. Finally, we observe a large change in brideprice practice. In particular, the proportion of females who had brideprice paid for their first marriage was significantly lower for treatment females than for control females.

[21] One potential concern is that brideprice payment is observed only for females who have ever married. If the composition of females who have ever married is different for the older and younger cohorts, then the treatment effect estimates of the observable variables for these females may be biased. However, we find that marital age does not affect our regression results. Specifically, we limit the sample to those women who married at age 24 or younger, since we can observe marriages that occurred below 24 years of age for all the cohorts in our data. This approach may lead to endogenous sample selection, but even so, it did not change our results and conclusions. For more detail, see Section 6.4.

Our key outcome variable, whether females had a brideprice paid for their marriage, demonstrates a kink at the cutoff. Figure III plots the proportion of females who received a brideprice for their first marriage and shows that the slope of the linear fit for the control cohorts is almost flat, while a downward trend is found for the treatment cohorts, thereby making a kink at the cutoff. This kink indicates that there may have been a behavioural change in the cultural practice of brideprice. We investigate this more rigorously through regression analyses.

[Figure III about here]

6.2 Tests for identification assumptions

The identification assumptions of the effect of female education on marriage and brideprice payment behaviours imply that there exists no kink in the predetermined covariates at the cutoff. Figures IVa to IVn show the trend of several premarital covariates, including parental years of education, premarital religion, the region of residence when females were seven years old, and major ethnicities. For most of these variables, we fail to find a clear slope change at the cutoff. These observations are further confirmed in Appendix Table B.2, where the slope change, expressed by the coefficient estimate of the interaction term, $\mathbf{I}\{\text{Year of birth} \geq 1979\} \cdot (\text{Year of birth} - 1979)$, is insignificant across these pretreatment covariates. The exceptions include a decrease in the share of females from the central region and an increase in that from the northern region.[22] Although these significant changes are small in magnitude and not robust across bandwidths, we always control for past residence and ethnicity in the following regression analyses.

[22]Correspondingly, we observe a small decrease in the share of Baganda females (Figure IVi), most of whom are from the central region, and an increase in the shares of Langi and Acholi females (Figures IVl and IVm, respectively), most of whom are from the northern region.

[Figure IV about here]

Another important testable implication is whether the density of year of birth is smooth around the cutoff of 1979. For our first check, we use the histogram shown in Figure V. Due to the small sample size of each cohort, the distribution is relatively noisy.[23] However, no noticeable sorting of density around the cutoff appears to be present. The non-existence of sorting is further checked by the statistical test proposed by Frandsen (2017), which detects sorting around the cutoff when the running variable is discrete.[24] The results shown in Table II reveal that the running variable indeed has a smooth density around our cutoff. The absence of skewed density is not surprising, since the cutoff is not used for any administrative purpose for UPE exposure in practice.

[Figure V about here]

[Table II about here]

6.3 Main results

To more rigorously examine the impact of female education on brideprice, we now turn to the regression analysis. Table III shows the estimated kink coefficient in the first-stage equation (3). The interaction term, $\mathbf{I}\{z \geq 1979\} \cdot (z - 1979)$, generally has a positive and significant coefficient estimate. For bandwidths of 8 years or longer, the F statistic approaches or passes the usual rule-of-thumb value of 10. The positive coefficient is likely to reflect the kink in the years of education at the cutoff, which increased only for the treatment cohorts (Figure II), implying that the relationship between the year of birth and years of education became positively steeper for the cohorts exposed to UPE.

[23] In Section 6.4, we show that our results are robust to this noise.

[24] The standard density test proposed by McCrary (2008) is inappropriate in our setting since this method requires the running variable to be strictly continuous, while we use year of birth, which takes integer values and is thus discrete (Frandsen, 2017).

[Table III about here]

Table IV presents the estimated effect of female education on whether a brideprice was paid at the first marriage. It shows that female education had a negative and significant effect on the probability of having a brideprice paid for marriage. These estimates are consistent with Figures II and III, suggesting that an increase in female education for cohorts born in or after 1979 has led to a decrease in the brideprice receipt status for these cohorts. The statistically significant estimates from Tables III and IV imply that being born one year later increased female education by .19 to .26 years, which in turn decreased the share of females with a brideprice payment for marriage by 1.9 to 2.8 percentage points, respectively. For bandwidths shorter than 10 years, the coefficient estimates for the treatment effect are insignificant but still negative. These results suggest that an increase in female education led to a decline in receiving a brideprice for marriage.

[Table IV about here]

One could be concerned that the increased years of education may have changed the marital characteristics that caused the decline in brideprice practice. Table V presents the estimates of the treatment effect on marriage characteristics. We find in Panel A that females with more years of education married at an older age, a finding consistent with those of prior studies using data from Uganda (e.g., Masuda and Yamauchi 2018 and Keats 2018). This result raises the concern that age at marriage may bias our main estimates through a potentially differential marriage probability. We therefore conduct a robustness check by limiting the sample of females to only those who married at younger ages and confirm the robustness of our main findings. For details, see Section 6.4.

[Table V about here]

In contrast, Panels B and C show that the levels of love marriage and local mating did not change much due to female education. The significant estimate for love marriage with a bandwidth of 11 years is likely to be just by chance, as Figure VI shows no substantial kink in its trend at the cutoff. The share of polygynous unions shows a decrease over the years according to the summary statistics (Table I), but Panel D shows null effect estimates. Figure VII shows a consistent downward trend in polygynous marriage with no kink at the cutoff. The long-term decline in the presence of polygyny is consistent with previous studies reporting the negative linkage between education and polygyny (Tertilt (2005), with an equilibrium model, and Fenske (2015), with empirical evidence).[25] In Panel E, the estimates for the non-divorcee indicator are all small in magnitude and insignificant.[26] These findings suggest that the change in brideprice receipt for females with more years of education is unlikely due to a change in marital characteristics.

[Figures VI and VII about here]

6.4 Robustness checks and additional analyses

Our analysis is intended to reveal the effect of female education on brideprice payment practices, exploiting free primary education in Uganda as the source of exogenous variation. However, it is possible that males, not just females, benefited from the UPE and that highly educated males accumulated more wealth to pay to their brides' families for marriage. Alternatively, it is also possible that such males develop better negotiation skills and persuade the brides' parents to agree to a lower brideprice. These possibilities imply that our estimation may identify the mixed effect of male and female education

[25] The dummy for polygamy concerns the current marital union and, thus, may not necessarily indicate that the first marriage was polygynous, which is a limitation of our data.

[26] The divorce status may be right-censored, which is another limitation of our data.

instead of the effect of only female education. To examine this implication, we repeat the first-stage regression for males with our RePEAT data. The results in Appendix Table B.5 show that UPE did not affect the educational attainments of males: in particular, the point estimates are generally small and insignificant throughout, which is consistent with Keats (2018), who found that male education did not respond to the Ugandan UPE, and with Deininger (2003) and Nishimura et al. (2008), who reported lesser impacts of UPE on male education. This finding supports our identification exploiting UPE to estimate the effects of female education on brideprice practices and marital behaviours.

Additionally, our specification of estimation equations may introduce bias into the estimated effect of female education. In particular, our identification may fail if there exists a discontinuous jump at the cutoff[27] or non-linearity in the expectation function of brideprice payment practice.[28] Appendix Table B.6 shows that our main findings are robust to allow for a discontinuous jump at the cutoff.[29] In addition, Appendix Table B.7 shows that the regression equation is better specified as a linear, rather than quadratic, function based on the Akaike information criterion (AIC).[30] Furthermore, the main findings are nearly unchanged when the regression equations are estimated using the limited information maximum likelihood (LIML) method (Hayashi, 2000), whose results

[27]Card et al. (2015) shows that if there is a discontinuity in the conditional expectation function, the identification of the TT parameter in the RKD framework fails.

[28]The linear specification in the RKD estimation may spuriously produce a significant treatment effect when the underlying true model is a smooth function that continuously changes its slope around the cutoff.

[29]To allow for a potential jump at the cutoff, we estimate the following equations:

$$y_i = \alpha_0 + \alpha_1(z_i - c) + \beta\hat{s}_i + W_i\phi + u_i$$
$$s_i = \gamma_0 + \gamma_1(z_i - c) + \mathbf{I}\{z_i \geq c\}[\delta_0 + \delta_1(z_i - c)] + W_i\psi + v_i.$$

[30]Specifically, we compare the AIC from the estimation of the following two reduced-form equations:

$$y_i = \gamma_0 + \gamma_1(z_i - c) + \delta_1\mathbf{I}\{z_i \geq c\}(z_i - c) + W_i\phi + r_i, \text{ and}$$
$$y_i = \gamma_0 + \gamma_1(z_i - c) + \gamma_2(z_i - c)^2 + \mathbf{I}\{z_i \geq c\}[\delta_1(z_i - c) + \delta_2(z_i - c)^2] + W_i\psi + r_i.$$

are shown in Appendix Table B.8. Therefore, it is unlikely that our main results are severely affected by the specification assumptions or finite sample bias.

Alternatively, one may be concerned that our choice of cutoff is invalid and that those used by previous studies, such as 1983 (Keats, 2018) or 1984 (Behrman, 2015), are preferable. Figures I and II show that the kinks in the probability of exposure to the UPE, as well as in the years of education, started in 1979 in our dataset. If we use later years as the cutoff, then we expect that the first-stage kink will be estimated to be smaller and potentially insignificant, which could make the treatment effect estimate more imprecise and unstable. The estimated results using 1983 as the cutoff in Appendix Table B.9 are in line with this view, showing insignificant coefficient estimates for the first stage and the treatment effect. The point estimates are also inflated, which is likely due to the weak first-stage estimates. These results indicate that it is important to choose the year at which the increasing exposure to free education started.

Furthermore, Figure V shows that the density of the year of birth is somewhat noisy, which may arise from misreporting and possibly bias the treatment effect estimate. The drop in the density for the year of birth of 1981 may be particularly worrisome.[31] To address this concern, we re-estimate the model by excluding each of the birth cohorts within the bandwidth. The results, partly reported in Appendix Figures A.2a to A.2d, show that the estimated effects and their confidence intervals are robust to such sample restriction.[32] Similarly, the change in the composition of females between the treatment and control cohorts in terms of factors such as ethnicity and region of residence at age seven may affect our main estimates if they are inadequately controlled for. To address this issue, we re-estimate the effects by excluding the Langi females and those who lived

[31]The density test also suggests that the year of birth of 1981 may raise the concern of potential sorting, although the policy or survey design does not provide any incentive for manipulative reporting.

[32]The results for other bandwidths are not shown for brevity but are available upon request.

in the northern region at age seven. The results are reported in Appendix Table B.10 and show that our main estimates are robust. Some might also be worried that the fluctuations in parental education and own pre-marital religion (Figures IVa through IVd) may bias our main estimates, but controlling for them does not alter our main findings (Appendix Tables B.11 and B.12).[33] These results suggest that the noise in the density of the year of birth, as well as the predetermined covariates, are unlikely to drive our estimates.

The potential confounding effect of the legal trial against brideprice practice in Uganda is worth noting. Specifically, some in Uganda have criticized brideprice practice, claiming that such a culture, combined with virilocality, may dehumanize females, referring to the payment of brideprice as treating females as a '*commodity*' (Wendo, 2004; MIFUMI Project, 2009). If such a social debate confounds the estimation of the effect of female education, then it should only affect the marriage of females in the treatment cohorts in an intensifying manner. However, this result is unlikely because such a debate cannot affect only the marriage of females born in or after the cutoff year of 1979. Furthermore, even when we allow for a differential trend of brideprice payment for females who married in or after 2007,[34] which is the year in which the trial against brideprice practice was initiated, the estimated negative effect of female education remains essentially unchanged (Appendix Table B.13).[35]

One might also be concerned about a possible general equilibrium effect. That is,

[33]Specifically, we include the years of education of the bride's mother, father, both of them, and pre-marital religion dummies linearly in the covariate vector W of equations (2) and (3). However, if we simply estimate the model, the sample size used for estimation with parental education is reduced substantially due to missing values for these variables. Therefore, we present results from a specification with an indicator for whether these variables are missing.

[34]Specifically, we estimate the following equations:

$$y_i = \alpha_0 + \alpha_1(z_i - c) + \beta\hat{s}_i + \theta_0 m_i + \mathbf{I}\{m_i \geq 2007\}(\theta_1 + \theta_2 m_i) + W_i\phi + u_i$$
$$s_i = \gamma_0 + \gamma_1(z_i - c) + \delta\mathbf{I}\{z_i \geq c\}(z_i - c) + \rho_0 m_i + \mathbf{I}\{m_i \geq 2007\}(\rho_1 + \rho_2 m_i) + W_i\psi + v_i$$

where m_i denotes the year in which female i married.

[35]The finding of little to no confounding due to the social debate is in line with anecdotes from surveyed females, who told us that they were unaware of either the debate or the court case.

the UPE might have increased the share of educated females in the marriage and labour markets and decreased the returns to education. This decline in returns to education, in turn, might have reduced the incentives for grooms to pay brideprice to marry better educated females. However, this is unlikely to be entirely driving our results, since the magnitude of the increase in female education does not seem so large as to completely change the composition of females in the marriage market. In our data, the average number of years of schooling only increased from five to seven over 13 years of our treatment period. Additionally, past studies do not indicate a sudden reduction in the returns to female education in recent years (*e.g.*, Peet et al., 2015).[36] These findings from our data and the literature suggest that the general equilibrium effect is unlikely to explain our finding of cultural change.

We further consider the potential selection in terms of marriage and censoring by marital age. First, we examine whether the choice to marry is a result of increased female education. Table B.3 shows the regression results for an indicator for having ever married using all the females in our data, regardless of marital history.[37] In Panel A, female education is found to have a significant negative effect on marital probability. However, Panel B shows that the estimates from a quadratic specification are much smaller and insignificant when all bandwidths are available.[38] Here, we present the results from the quadratic model, since the AIC for the reduced-form regressions (Panel C) suggests that the linear model is not always preferable to the quadratic model. Therefore, our data are

[36]Note that the attempt to interpret our finding of the negative effect of female education on the probability of receiving brideprice as a result of a decline in returns to education implicitly assumes the human capital compensation hypothesis, which predicts the amount of brideprice received by the bride's parents depends upon the returns to her education. However, we discuss in Section 6.5 that the human capital compensation hypothesis cannot explain cultural decline.

[37]Correspondingly, Figure A.1 shows the share of married females in each cohort for all the females in our data, regardless of marital history.

[38]The estimates from the quadratic model are less precise, as expected (Card et al., 2016; Gelman and Imbens, 2019). However, the insignificance does not solely stem from the larger standard errors since the point estimates are also considerably smaller.

inconclusive regarding the possibility of selective marital behaviours.

Second, given this inconclusiveness, we examine whether the probability of brideprice payment is a function of females' marital age.[39] In our data, the marital information is censored at age 24 for the youngest cohort compared to age 49 for the oldest cohort (when using the largest bandwidth of 13 years). When we replicate the regressions, limiting the sample to those who married at the age of 24 or younger, the estimated effects are strikingly similar to the main results, despite the potentially endogenous sample restriction (Appendix Table B.4).[40] This finding suggests that at least for females who are already married, the effects of female education are not systematically different across ages at marriage. Moreover, regardless of age at marriage, Table V shows that marital characteristics are not systematically different for the younger treatment females and older control females. Therefore, our findings on the effect of female education on brideprice practice are unlikely to be substantially biased due to a potentially endogenous choice to marry.

6.5 Mechanism through which female education reduces brideprice practices

We have shown that an increase in female education reduces the probability of receiving a brideprice for a first marriage. We have also shown that this cultural change does not seem to have been caused by a change in marital behaviours. Based on our above findings, we now examine the mechanism of the cultural change brought about by educational reform.

[39] For example, parents of younger females may be more likely to receive a brideprice if marital age is linked to virginity, which may be valued in the marriage market (Ambrus et al., 2010), or the husband may feel more obliged to pay a brideprice for a younger bride, as her natal family may require greater compensation.

[40] The number of observations is close to the results in Table IV, suggesting that most females marry at fairly young ages and are not actually subject to censoring.

First, as we discuss in Section 2.1, the human capital compensation hypothesis suggests that a brideprice is paid to compensate for a bride's human capital (Becker, 1991; Anderson, 2007). According to this hypothesis, to the extent that greater female education implies a larger contribution to household production, parents who face a higher cost when giving a daughter away receive a larger brideprice. For this hypothesis to explain the decline in brideprice practice, either female education or the returns to it should have declined. However, we find that Ugandan females exposed to UPE *increased* their educational attainments, and the literature does not suggest a drop in returns to female education over our analysis cohorts (Peet et al., 2015).[41]

One might also be concerned that, according to the human capital compensation hypothesis, the control cohorts whose education was relatively low would appear less attractive in the marriage market and thus be paid less. If this tendency is strong, they might have received no brideprice, and our effect estimates may be attenuated. When estimating the main model excluding females from the control cohorts born in years close to the cutoff, the point estimates become slightly larger in magnitude but the differences are virtually imperceptible (Appendix Table B.15). Given that female education attainment and returns to female education do not seem to have declined, that the spillover effect seems to be minimal, if it exists at all, and that the attenuation bias would not reverse the conclusion, we deem the human capital compensation hypothesis unlikely to explain the whole cultural change.

Second, the assortative matching hypothesis suggests that educated women are more

[41]In addition, we test one of the predictions of human capital compensation theory by estimating the effect of female education on the probability of females having non-agricultural jobs. The estimated effects are small and insignificant, as shown in Table B.14. Strictly speaking, the effect of female education on female employment should arise from the husband's *ex ante* expectation at marriage, while the analysis here is concerned with the *ex post* self-report by females at the time of the survey. The measurement may thus be inaccurate, which is a limitation of our data.

likely to marry educated men, who can afford to pay a higher brideprice. For this theory to account for the decline in brideprice practice, the correlation between females and their partners' education needs to have weakened. However, the simple correlation coefficient of women's own and their partners' years of education became larger (0.408) for the younger treated females than for the older control females (0.312). More rigorously, we regress partners' years of education on the indicator for being born in or after 1979, female education, and their interaction term. The results in Appendix Table B.16 show that on the one hand, female education is significantly correlated with a woman's partner's education, indicating the existence of assortative matching. On the other hand, the coefficient estimate for the interaction term is positive yet insignificant, suggesting that the assortative nature of marital mating becomes neither stronger nor weaker for the younger treated cohorts. These results show that the assortative matching hypothesis cannot explain the decline in brideprice practice.

We now discuss an alternative hypothesis that can explain our findings. That is, more education may have changed women's perception of brideprice and led them to refuse to receive a brideprice for marriage. Several studies have shown that women in Uganda are worried about domestic violence and the sexual infidelity of those husbands who have paid a brideprice (Wendo, 2004; Kaye et al., 2005; MIFUMI Project, 2009; Bishai and Grossbard, 2010). Another study reports a higher probability of divorce for couples with brideprice payments in Senegal (Platteau and Gaspart, 2007).[42] In the media, articles on violence related to brideprice have been reported even after the Supreme Court decision in 2015.[43] The expectation of domestic violence, differential extramarital affairs, and divorce induced by the payment of a brideprice are likely to decrease the future utility

[42]While divorce, per se, may not be a bad outcome, given the differential treatment of men and women after divorce (Fafchamps and Quisumbing, 2005b), it may be the last resort for women.

[43]Examples include Daily Monitor (2016a,b, 2017, 2018a,b,c, 2019) and New Vision (2016a,b).

of women. Then, their altruistic parents might become increasingly averse to receiving a brideprice, as their daughters become more educated and more aware of these possible drawbacks. In other words, the parents of educated females may choose their daughters' sound future marital life at the expense of a wealth transfer in the form of a brideprice. This interpretation is consistent with the findings of Gaspart and Platteau (2010), who argue that an increase in female education may help daughters strengthen their influence on their altruistic parents and persuade them to consider their daughters' potential loss of utility contingent on a large brideprice payment.

Our focus group discussions (FGDs) inviting selected surveyed females raise yet another hypothesis.[44] Females present at our FGDs generally agreed on important predictions of the model by Gaspart and Platteau (2010). For instance, the presence of brideprice may lock up the bride in a marriage where she is not treated well (*e.g.*, with intense domestic violence by the husband); female education is associated with a larger brideprice but also with marriages involving no brideprice payment; and it is positively associated with the outside option and the probability of divorce. However, the women disagreed with one of the critical predictions of the study by saying that females with more education, if they want to marry with no brideprice at all, would ask their husband not to pay brideprice, rather than ask their parents not to receive it. Additionally, our field observation found that an increasing number of females decide who and when to marry, with no parental consent at all, on top of the large share of females who marry a man based on love (approximately 90%, Table I). These findings from our FGDs suggest that, if the presence of brideprice payment increases domestic violence and other maltreatment of females within marriage, rejecting the brideprice payment is likely to increase female

[44] We conducted three FGDs in three selected villages in Eastern and Central Uganda in March 2020. Our FGDs accommodated six to eight females who were among the survey subjects.

utility.[45] In addition, it is likely to lower the cost of divorce, as the brideprice serves as debt. Thus, if female education increases female bargaining power, it may then decrease marriages with brideprice payment. This hypothesis can explain the decline in brideprice practice as a result of an increase in female education, even if education brings no change in parents' altruism toward their daughters.

These possible mechanisms—parental altruism and female bargaining—may sound unique to Uganda, where there has been a social debate on whether brideprice practice should be banned (Wendo, 2004; MIFUMI Project, 2009). However, as discussed by Anderson (2007), the practice has declined or is declining in areas where such a societal discourse does not seem intense or the contemporary concept of human rights has not yet fully developed. Even in Uganda, quite a few females in our FGDs were unaware of either the debate or the law suit. Moreover, our results show that the presence of concern about the legitimacy of brideprice practice does not confound our estimate of the negative effect of female education (see section 6.4 and Appendix Table B.13). It would be interesting to investigate whether a similar impact on brideprice practice is found in other settings of female education.

As Gaspart and Platteau (2010) mention, our proposed hypotheses do not rule out the assortative matching mechanism or the human capital compensation motive for brideprice payment. Rather, these hypotheses and our interpretations based on parental altruism or intramarital bargaining can coexist. That is, conventional theories postulate that the amount of brideprice is endogenously determined as a function of female education, as-

[45]For instance, if we slightly modify the settings in Gaspart and Platteau (2010), the payoff of a marrying female may be written as $U = (1-S)(Y - D \times V) + S(W - D \times B)$, where S is an indicator for separation or divorce, Y denotes household production, V is disutility from domestic violence, W is her outside option, D is an indicator of the presence of a brideprice payment, and B is the amount of the brideprice. Assuming $V, B > 0$, the presence of a brideprice payment decreases her payoff both within and outside of marriage. This is because brideprice is assumed to be associated with domestic violence within marriage and it becomes a debt to the husband once marriage falls apart.

suming that whether to pay brideprice is exogenously determined. Our hypotheses call for an extension such that the presence of brideprice payment also be determined endogenously as a function of female education. Put differently, a more comprehensive model of the marriage market that features brideprice payment would separate the decision on whether to pay brideprice from how much to pay as brideprice and endogenise both of them with respect to female education. Such an overarching framework may seamlessly unify the several proposed mechanisms in the literature and provide conditions that determine which effects—the effects of female education on the amount and practice of brideprice—dominate in which cases, thereby explaining the cultural decline in relation to an expansion of female education during economic development. It would be fruitful for future study to theoretically analyse a comprehensive model and empirically test its validity.

7 Conclusion

Many developed countries used to have a brideprice practice and witnessed its decline, while some less developed countries are in the middle of a cultural change. Economic development has been identified as a closely related factor, but little is known about exactly what contributes to the decline of brideprice practices. In this study, we utilise a natural experiment in Uganda and provide the first evidence that an expansion of female education leads to a decline in this cultural practice. Our estimation results show that a year increase in female education reduces the probability of brideprice payment by approximately 10 to 12 percentage points. To the best of our knowledge, this finding is the first evidence of its kind in the economic literature on brideprice.

As a potential mechanism for cultural change, we first considered that conventional hypotheses, namely, the human capital compensation and assortative matching hypotheses, are unlikely to fully explain cultural change. We have, therefore, discussed two alternative conjectures. One relates the cultural decline to parental altruism toward marrying daughters: altruistic parents may give up the brideprice transfer and opt for a higher expected marital utility for their daughters, since doing so is likely to reduce the chance of bind their daughter in a marriage in which she is ill-treated. Another focuses on the relative bargaining power in the marital union: brides with more education may have greater bargaining power and convince grooms to marry without a brideprice payment.

Finally, our study is not free of limitations. First, our proposed conjectures on the mechanism through which female education affects brideprice practice await theoretical formalisation and empirical examination. Similarly, our finding on the negative effect of female education on brideprice practice needs to be tested for external validity. An increase in the human capital of females is a common phenomenon during economic development and is thus likely to have contributed to cultural change in many countries. It would therefore be beneficial to investigate whether similar changes have occurred in other countries that have experienced major educational reforms and to identify the mechanisms for which such changes took place.

Second, our study does not deny the existence of other mechanisms that may also play a role in the decline in brideprice culture. It is possible that the cultural decline reported worldwide (Anderson, 2007) may have also been facilitated for reasons other than an increase in female education. Studies exploring these other mechanisms would thus enrich understanding of the transformation of brideprice practice. Third, our estimation results are relevant only for cohorts born in years in the small neighbourhood of the cutoff. The findings would be different if the cultural behaviour farther away from the cutoff cohort is considered. It would be fruitful for future research to overcome these challenges and explore the impact of institutional reforms on cultural changes, particularly brideprice practice.

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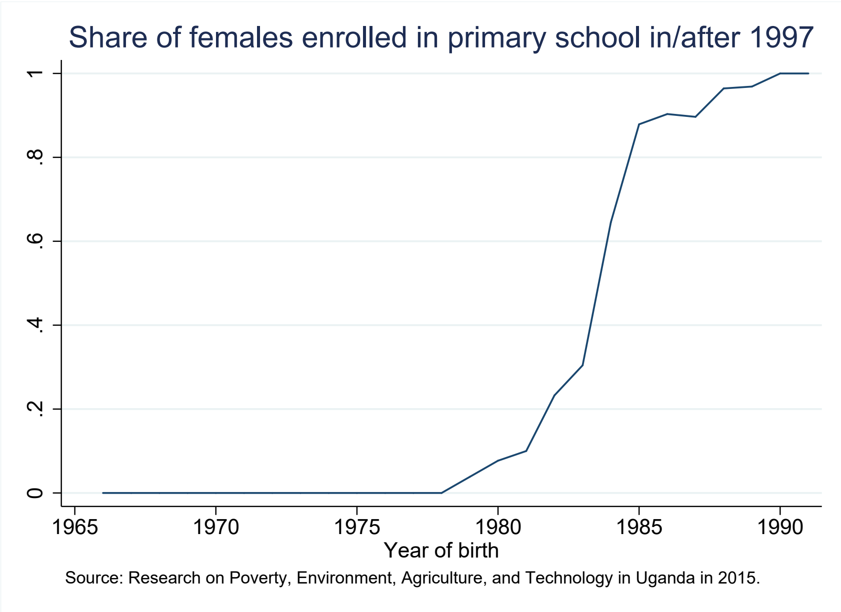
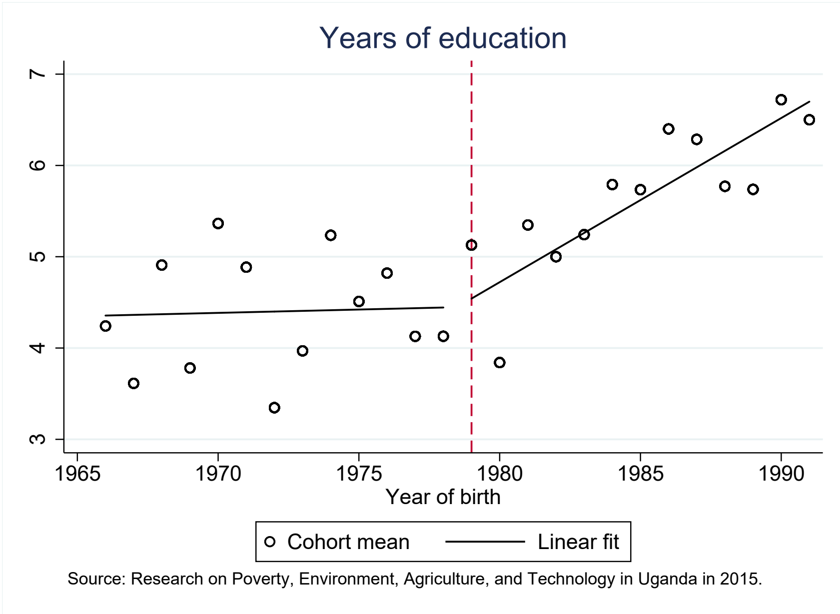
Figures.



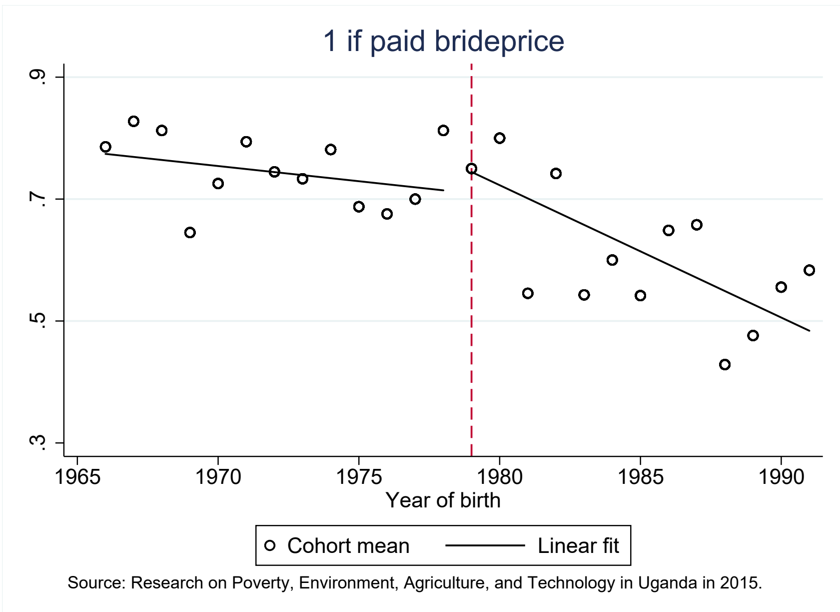
Notes. This figure plots the share of females in each birth cohort of the females aged 24 to 49 who have ever married and were enrolled in primary school in and after 1997. The enrolment status is based on the question about the years in which each female enrolled and left primary school rather than on the self-reported measure of their UPE receipt.

Figure I: Share of females enrolled in primary school in or after 1997.



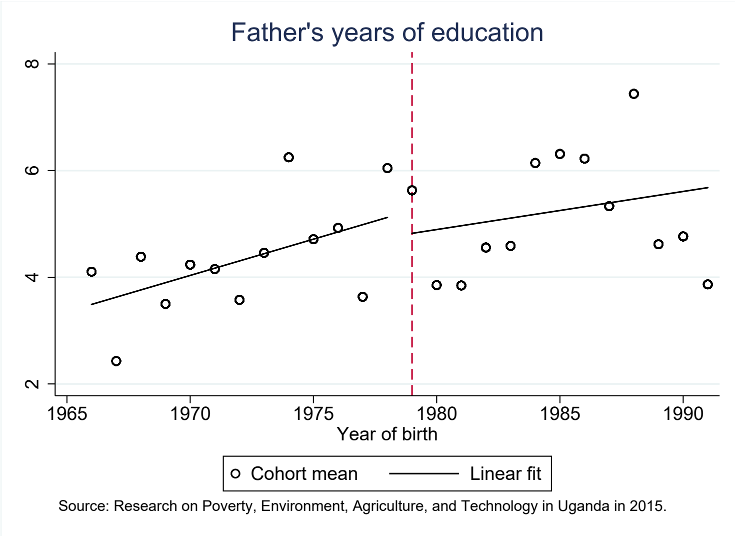
Source. Research on Poverty, Environment, Agriculture, and Technology in Uganda in 2015.
Notes. This figure plots the average years of education of females born in each year and its linear fit for females aged 24 to 49 who have ever married. Years of education are defined as the minimum years of schooling required to achieve the highest grade of education reported by the respondent. The dashed vertical line represents the year 1979, the cutoff of our analysis, as explained in detail in Section 5.2.

Figure II: Years of education of females.

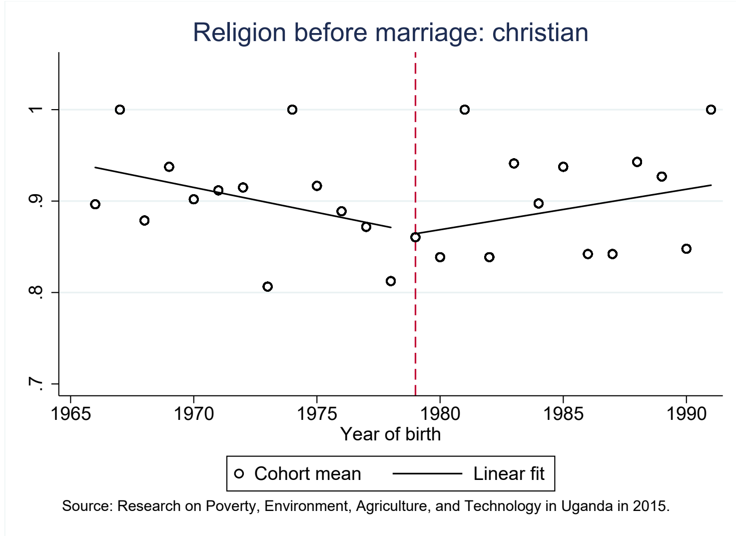


Notes. This figure shows the share of females who agreed to a brideprice payment for their first marriage for each birth cohort and its linear fit. The dashed vertical line represents the year 1979, the cutoff of our analysis, as explained in detail in Section 5.2.

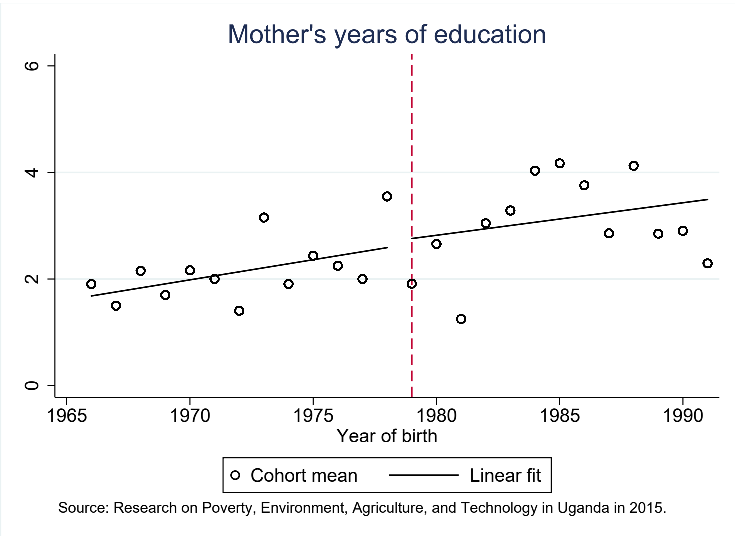
Figure III: Share of females who had a brideprice for marriage.



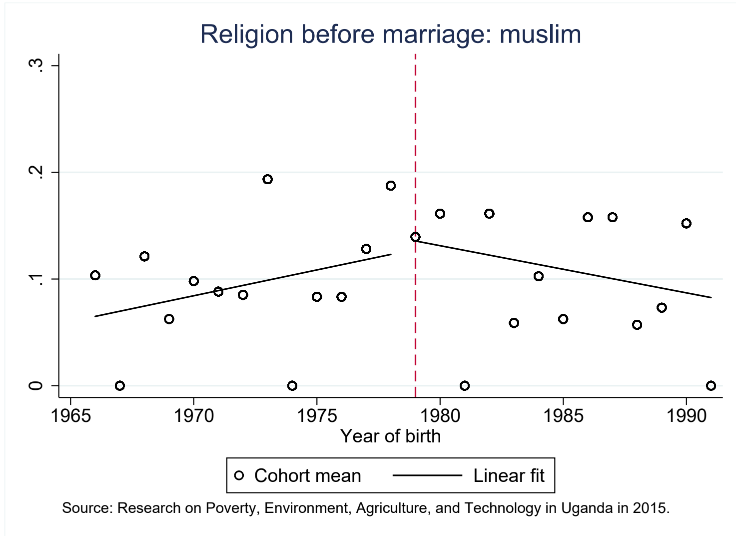
(a) Father's years of education.



(c) Share of females who were Christian prior to marriage.

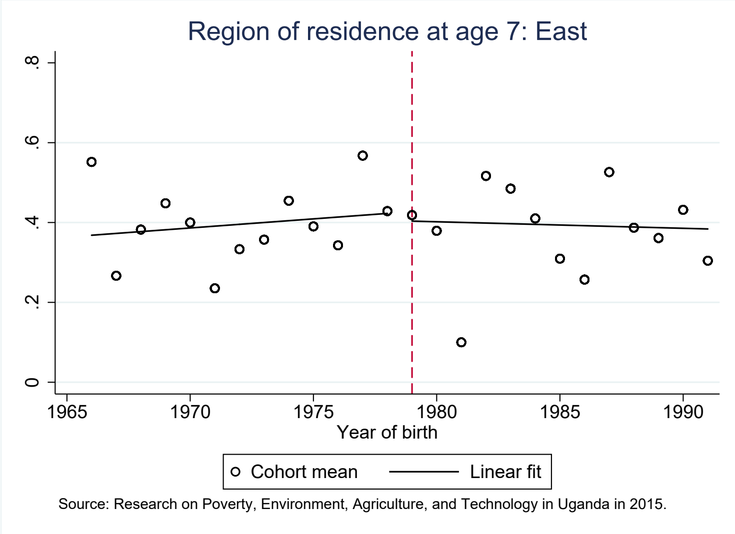


(b) Mother's years of education.

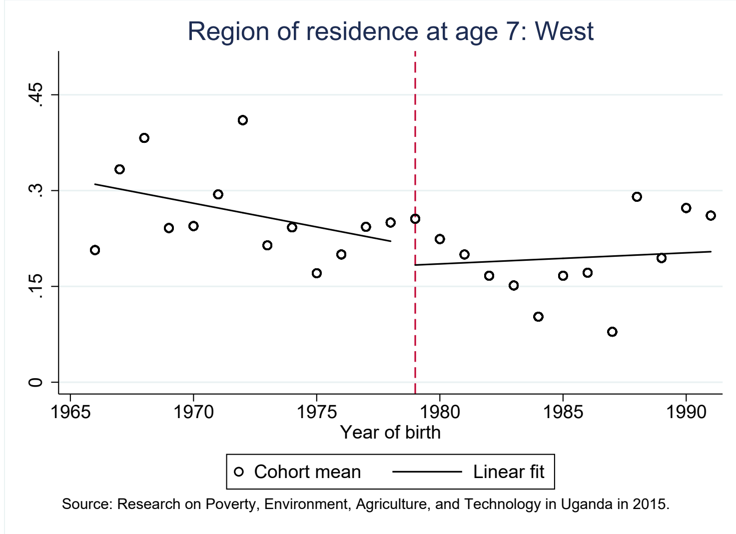


(d) Share of females who were Muslim prior to marriage.

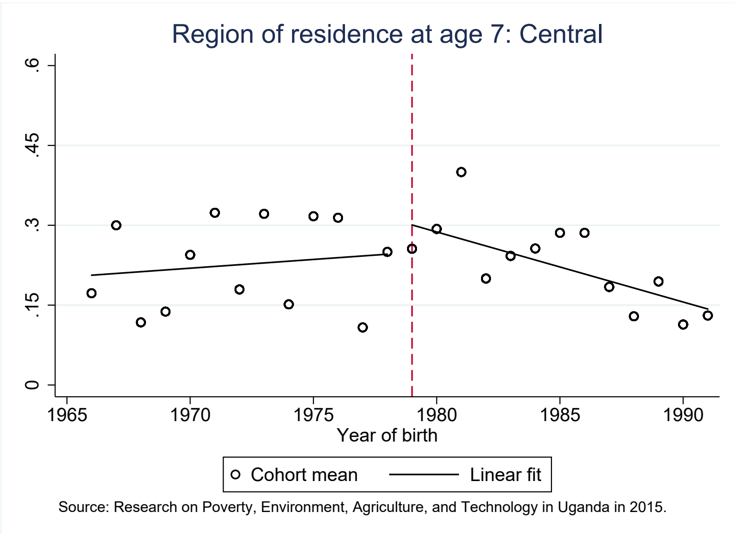
Figure IV: Trends in premarital covariates for females born in years around the cutoff.



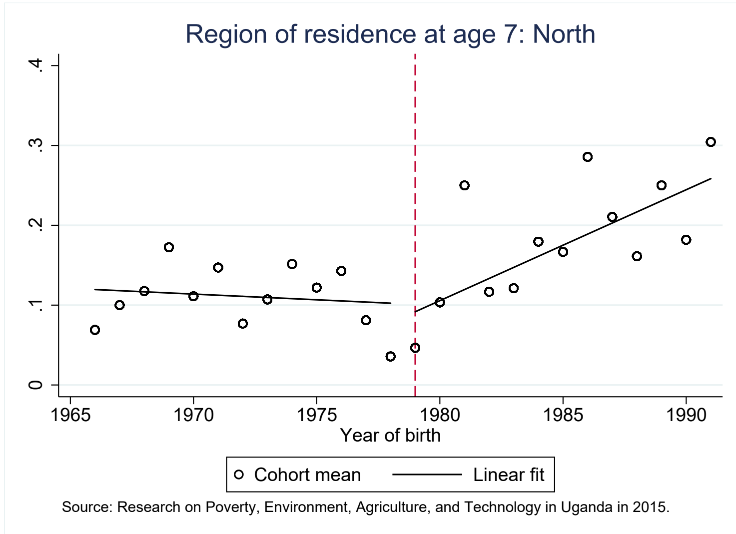
(e) Share of females who lived in the eastern region at age seven.



(g) Share of females who lived in the western region at age seven.

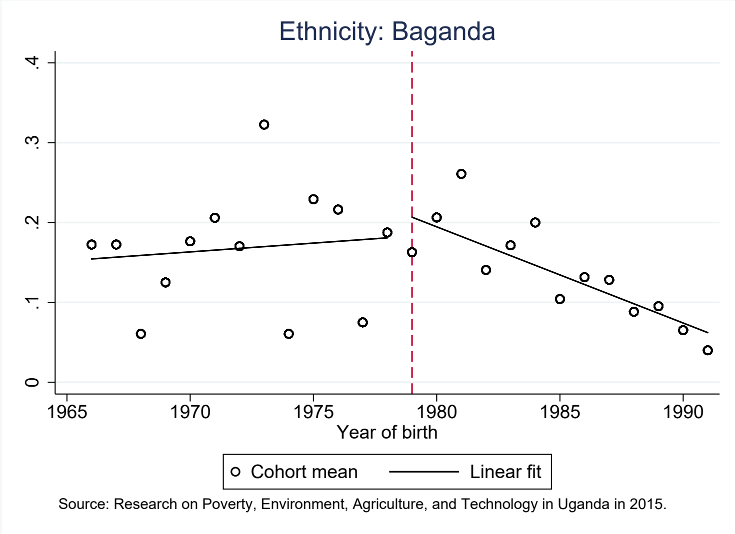


(f) Share of females who lived in the central region at age seven.

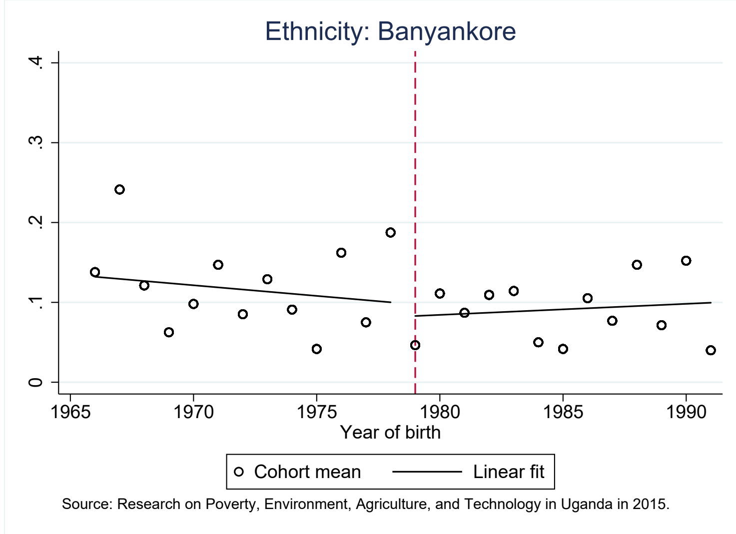


(h) Share of females who lived in the northern region at age seven.

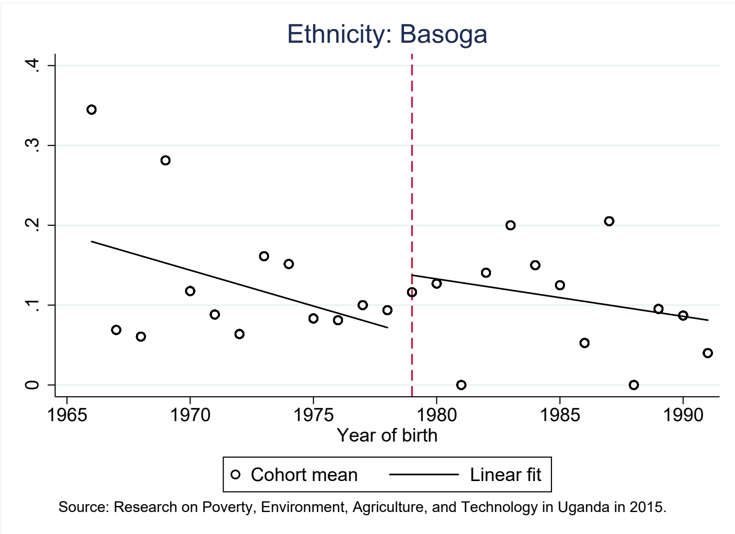
Figure IV: Trends in premarital covariates for females born in years around the cutoff (continued).



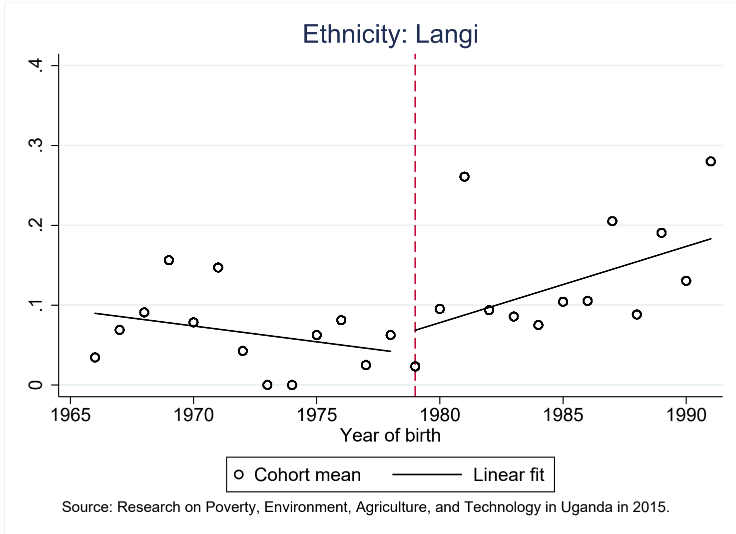
(i) Share of females whose ethnicity is Baganda.



(k) Share of females whose ethnicity is Banyankore.

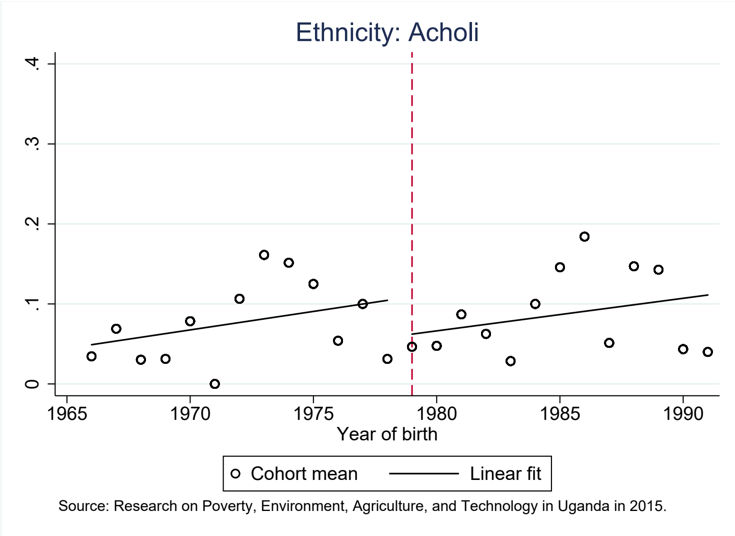


(j) Share of females whose ethnicity is Basoga.

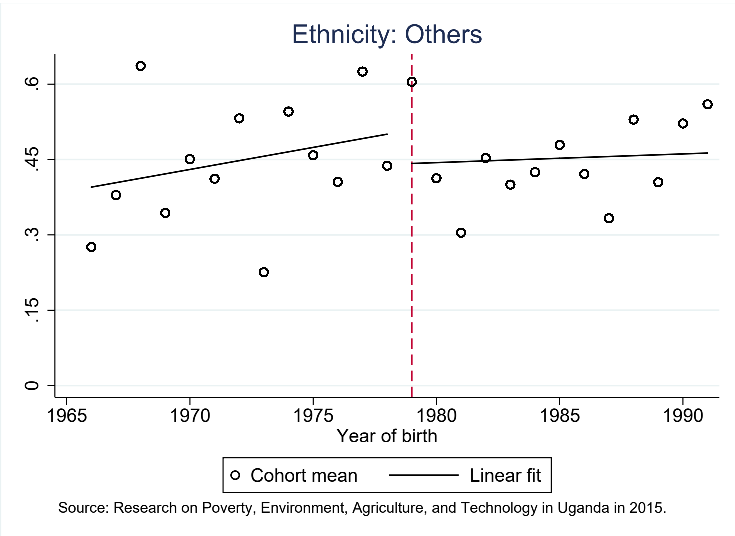


(l) Share of females whose ethnicity is Langi.

Figure IV: Trends in premarital covariates for females born in years around the cutoff (continued).

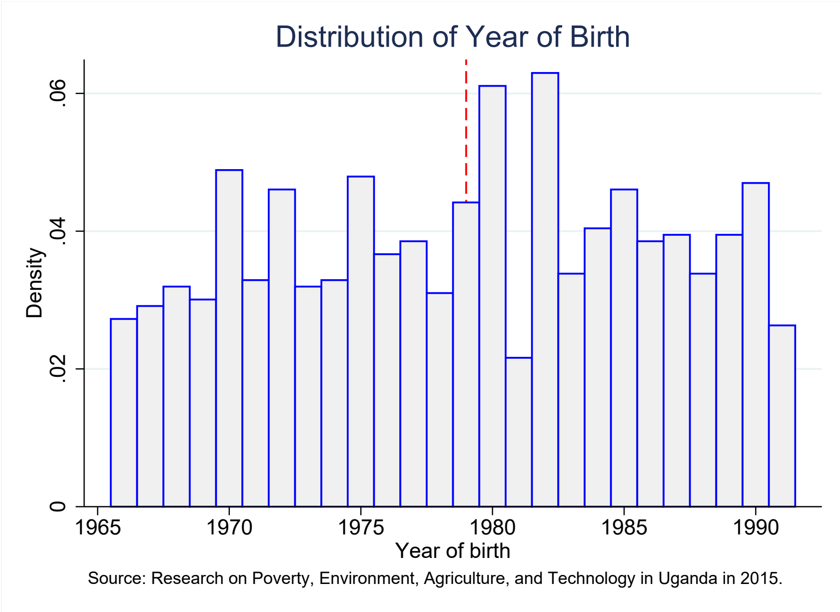


(m) Share of females whose ethnicity is Acholi.



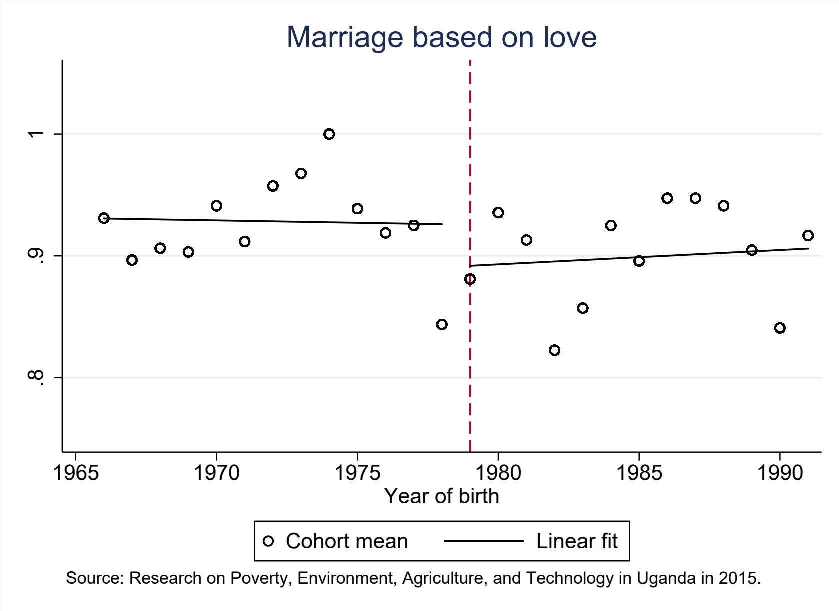
(n) Share of females whose ethnicity is any other.

Figure IV: Trends in premarital covariates for females born in years around the cutoff (continued).



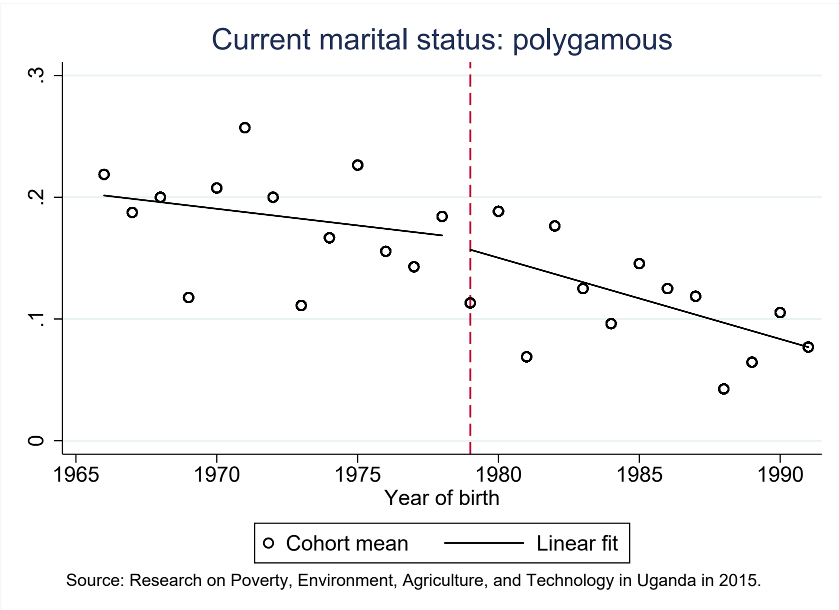
Notes. This figure shows a histogram of the year of birth for those females aged 24 to 49 who have ever married. The dashed vertical line represents the year 1979, the cutoff of our analysis, as explained in detail in Section 5.2.

Figure V: Histogram of year of birth.



Notes. This figure shows the share of females whose first marriage was based on love and its linear fit for females aged 24 to 49 who have ever married.

Figure VI: Share of females whose first marriage was based on love.



Notes. This figure shows the share of females whose current marital union is polygynous and its linear fit for females aged 24 to 49 who have ever married. Polygyny is a type of marital union in which one male marries more than one female.

Figure VII: Share of females whose current marital union is polygynous.

Tables.

Table I: Summary Statistics of the Major Variables.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Sample	Females born in 1966 - 1978.				Females 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	3.62	733	29.71	30	3.84	-60.57***
Region at age 7: Eastern	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: Western	468	0.259	0	0.438	662	0.210	0	0.408	-1.91*
Region at age 7: Northern	468	0.118	0	0.322	662	0.169	0	0.375	2.41**
Own ethnicity: Baganda	504	0.177	0	0.382	702	0.168	0	0.374	-0.39
Own ethnicity: Basoga	504	0.129	0	0.335	702	0.117	0	0.321	-0.64
Own ethnicity: Banyankore	504	0.117	0	0.322	702	0.101	0	0.302	-0.88
Own ethnicity: Langi	504	0.069	0	0.254	702	0.110	0	0.313	2.38**
Own ethnicity: Acholi	504	0.077	0	0.267	702	0.088	0	0.284	0.68
Own ethnicity: Any other	504	0.431	0	0.496	702	0.416	0	0.493	-0.51
	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 enrolment	341	7.639	7	1.500	587	7.305	7	1.567	-3.18***
Primary: Age of leaving school	321	9.330	12	8.302	517	11.53	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***

(Continues to the next page)

Table I: Continued.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Females born in 1966 - 1978.				Females born in 1979 - 1991.				(2) = (6)
Variables	N	Mean	Median	Std. Dev.	N	Mean	Median	Std. Dev.	t statistic
	Panel C. First marriage variables.								
1 if 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
1 if love marriage	474	0.928	1	0.258	532	0.898	1	0.302	-1.67*
Own pre-marital residence: Within LC1	475	0.236	0	0.425	535	0.290	0	0.454	1.94*
Own pre-marital residence: Within subcounty	475	0.251	0	0.434	535	0.245	0	0.430	-0.21
Own pre-marital residence: Within district	475	0.208	0	0.407	535	0.200	0	0.400	-0.33
Own pre-marital residence: Within Uganda	475	0.303	0	0.460	535	0.252	0	0.435	-1.80*
Own pre-marital religion: Christian	474	0.903	1	0.296	533	0.889	1	0.314	-0.71
Own pre-marital religion: Muslim	474	0.095	0	0.293	533	0.111	0	0.314	0.82
1 if current = first marriage	399	0.802	1	0.399	463	0.840	1	0.367	1.46
1 if in polygynous union	521	0.184	0	0.388	731	0.115	0	0.319	-3.46***
1 if brideprice paid	471	0.743	1	0.437	528	0.621	1	0.486	-4.15***

Source. Research on Poverty, Environment, Agriculture, and Technology survey in Uganda in 2015. *Notes.* This table shows the 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 females born from 1966 to 1978, while the treatment group consists of females 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.

Table II: Density Test of Year of Birth.

	(1)	(2)	(3)	(4)
	Smoothing parameter (k)			
Year of birth	0.00	0.01	0.05	0.10
1967	0.961	1.000	1.000	1.000
1968	0.634	0.830	0.836	0.777
1969	0.179	0.172	0.188	0.234
1970	0.014	0.025	0.030	0.047
1971	0.070	0.069	0.079	0.113
1972	0.050	0.079	0.088	0.121
1973	0.346	0.331	0.349	0.450
1974	0.385	0.384	0.400	0.447
1975	0.065	0.089	0.103	0.138
1976	0.443	0.460	0.478	0.523
1977	0.442	0.551	0.565	0.600
1978	0.185	0.178	0.194	0.241
1979	0.891	0.861	0.934	0.941
1980	0.000	0.000	0.001	0.002
1981	0.000	0.000	0.000	0.000
1982	0.000	0.000	0.000	0.000
1983	0.029	0.028	0.035	0.058
1984	0.868	1.000	1.000	1.000
1985	0.341	0.464	0.483	0.483
1986	0.650	0.645	0.657	0.694
1987	0.577	0.698	0.709	0.735
1988	0.502	0.499	0.512	0.556
1989	0.983	1.000	1.000	1.000
1990	0.045	0.066	0.076	0.104

Source. Research on Poverty, Environment, Agriculture, and Technology in Uganda in 2015. *Notes.* This table shows the results of a density test proposed by Frandsen (2017) for the year of birth of females in our dataset. A smaller parameter value of $k \in [0, 1]$ makes the test stricter, where the null hypothesis is that there is no manipulative sorting of the running variable at the cutoff. The computation uses females aged 24 to 49 who have ever married.

Table III: Estimated results of first-stage regression.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Years of education										
Bandwidth	13 years	12 years	11 years	10 years	9 years	8 years	7 years	6 years	5 years	4 years	3 years
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)
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)
Observations	894	847	776	708	651	572	504	426	364	301	206
R-squared	0.261	0.260	0.258	0.259	0.272	0.272	0.288	0.327	0.348	0.365	0.508
F statistics	9.895	8.853	11.41	7.962	12.82	8.887	2.565	7.276	5.314	2.682	0.54

Source. Research on Poverty, Environment, Agriculture, and Technology in Uganda in 2015. *Notes.* This table shows the coefficient estimate of the interaction between the year of birth minus the cutoff of 1979 and an indicator for it being equal to or larger than the cutoff from the regression of female years of education. Reported in parentheses are standard errors robust for heteroscedasticity. Statistical significance is denoted by *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$. The F statistic for the significance of the coefficient for the interaction term is reported in each panel. 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 females aged 24 to 49 who have ever married and were born in years within the indicated bandwidth of the cutoff.

Table IV: The Estimated Impact on Brideprice Receipt Probability with All Available Bandwidths.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Bandwidth.	13 years	12 years	11 years	10 years	9 years	8 years	7 years	6 years	5 years	4 years	3 years
1 if brideprice paid	-0.099**	-0.123**	-0.110**	-0.109**	-0.031	-0.040	-0.081	-0.031	-0.039	-0.082	-0.393
	(0.051)	(0.055)	(0.048)	(0.056)	(0.035)	(0.046)	(0.089)	(0.044)	(0.056)	(0.090)	(0.588)
Observations	878	832	762	695	640	562	495	417	356	294	201

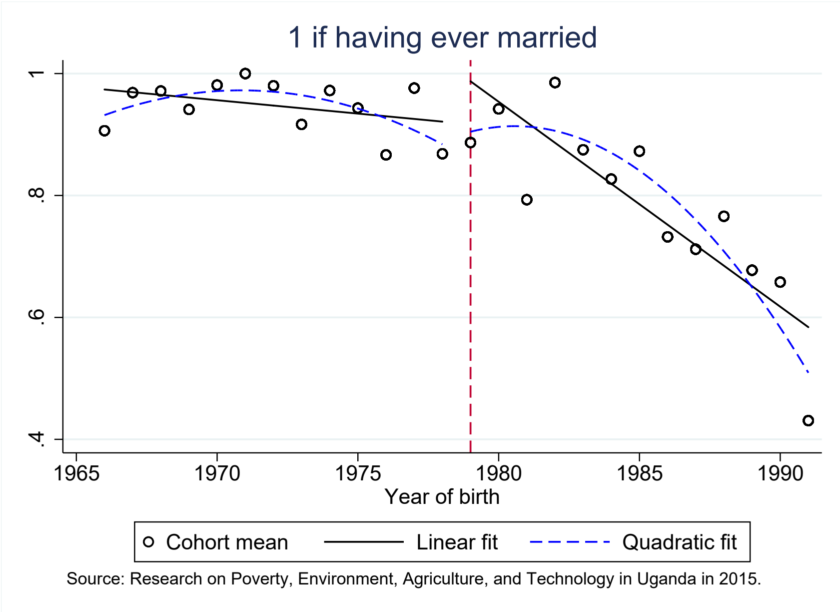
Source. Research on Poverty, Environment, Agriculture, and Technology in Uganda in 2015. *Notes.* This table shows the treatment effect estimate for the probability of receiving brideprice for all the available bandwidths in our data. Reported in parentheses are standard errors robust to heteroscedasticity. Statistical significance is denoted by *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$. 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 females aged 24 to 49 who have ever married and were born in years within the indicated bandwidth of the cutoff.

Table V: The Estimated Impact of Female Education on Marital Characteristics.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Bandwidth	13 years	12 years	11 years	10 years	9 years	8 years	7 years	6 years	5 years	4 years	3 years
	Panel A. Age at marriage										
Treatment effect	0.761**	0.684**	0.456	0.438	0.254	-0.347	-1.643	-0.213	-0.426	-0.556	0.130
	(0.362)	(0.389)	(0.358)	(0.433)	(0.369)	(0.468)	(1.456)	(0.524)	(0.621)	(0.896)	(1.455)
Observations	879	834	764	698	644	567	499	422	361	298	204
	Panel B. 1 if love marriage										
Treatment effect	0.027	0.027	0.043**	0.033	0.018	0.009	-0.013	-0.023	-0.056*	-0.092	0.020
	(0.025)	(0.026)	(0.024)	(0.026)	(0.021)	(0.023)	(0.043)	(0.028)	(0.040)	(0.072)	(0.094)
Observations	886	839	770	703	648	570	502	424	362	300	206
	Panel C. 1 if living in premarital LC1										
Treatment effect	0.056	0.029	0.012	0.021	0.008	-0.012	-0.026	-0.029	0.014	0.076	-0.005
	(0.045)	(0.043)	(0.038)	(0.042)	(0.033)	(0.039)	(0.071)	(0.043)	(0.054)	(0.086)	(0.175)
Observations	889	842	771	705	648	569	501	424	362	299	205
	Panel D. 1 if polygyny										
Treatment effect	0.009	-0.010	-0.026	-0.007	0.008	0.013	-0.022	-0.019	0.003	-0.005	-0.053
	(0.037)	(0.036)	(0.032)	(0.038)	(0.031)	(0.037)	(0.064)	(0.035)	(0.042)	(0.054)	(0.125)
Observations	894	847	776	708	651	572	504	426	364	301	206
	Panel E. 1 if not divorced										
Treatment effect	-0.015	-0.048	-0.047	-0.046	-0.021	-0.013	-0.054	-0.015	-0.036	-0.015	-0.021
	(0.040)	(0.046)	(0.044)	(0.049)	(0.030)	(0.035)	(0.056)	(0.037)	(0.046)	(0.041)	(0.067)
Observations	755	715	655	602	559	493	432	371	319	264	182

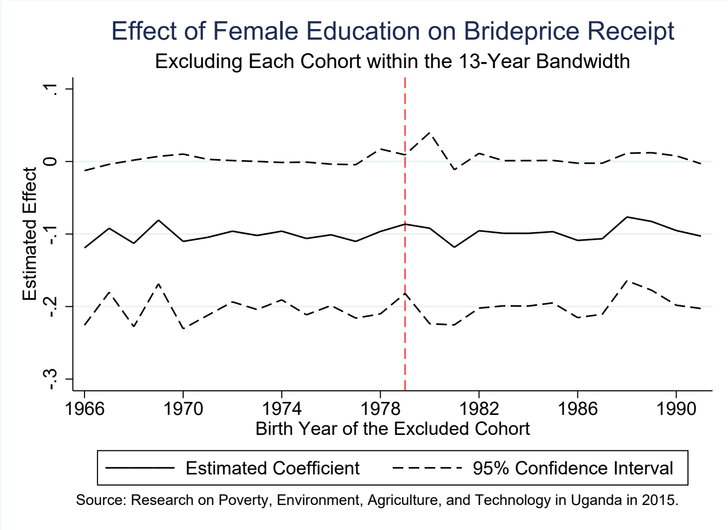
Source. Research on Poverty, Environment, Agriculture, and Technology in Uganda in 2015. *Notes.* This table shows the estimated treatment effect of female education on marital characteristics. Reported in parentheses are standard errors robust to heteroscedasticity. Statistical significance is denoted by *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$. 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 females aged 24 to 49 who have ever married and were born in years within the indicated bandwidth of the cutoff. Due to missing values, the number of observations differs across regressions.

Appendix A Additional figures.

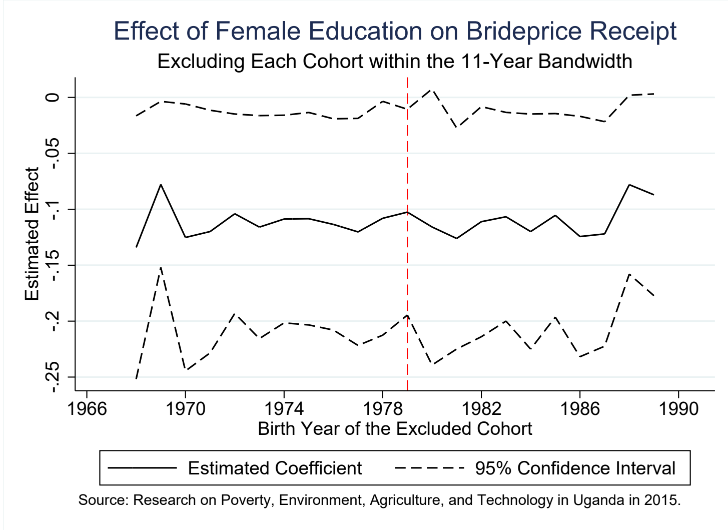


Source: Research on Poverty, Environment, Agriculture, and Technology in Uganda in 2015. *Notes*: This figure plots the share of females who have ever married for each birth cohort and the linear and quadratic fit. The dashed vertical line represents the year 1979, the cutoff of our analysis, as explained in detail in Section 5.2.

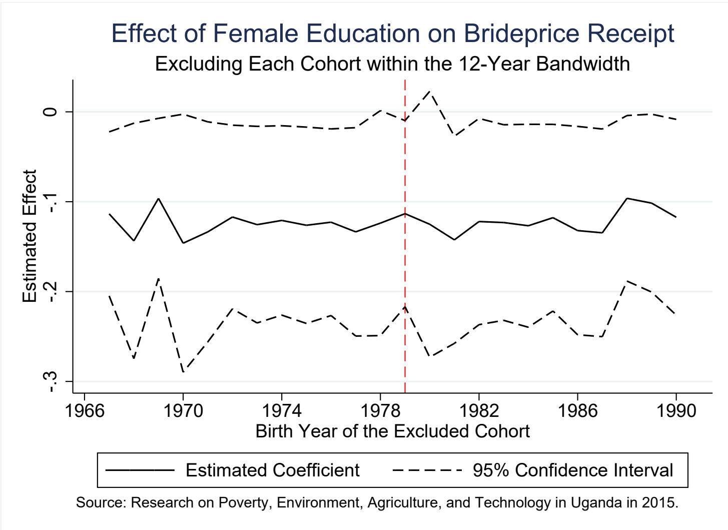
Figure A.1: Share of females who have ever married.



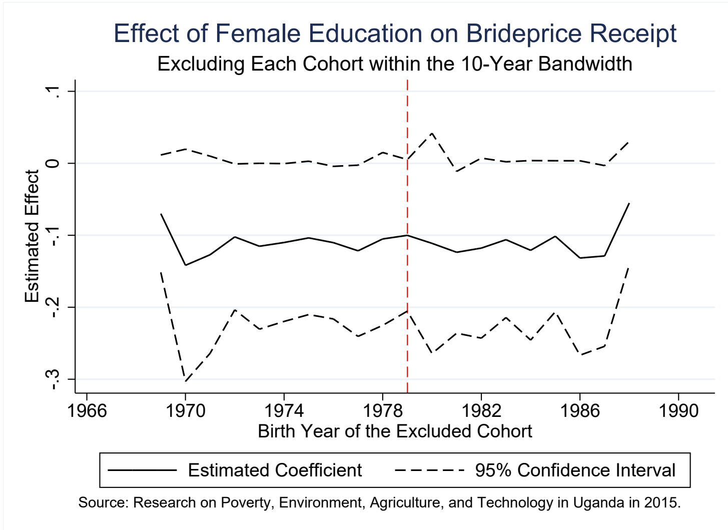
(a) Bandwidth = 13 years.



(c) Bandwidth = 11 years.



(b) Bandwidth = 12 years.



(d) Bandwidth = 10 years.

Source: Research on Poverty, Environment, Agriculture, and Technology in Uganda in 2015. *Notes*: These figures show the estimated effect of female education on brideprice receipt status and its 95% confidence intervals for each bandwidth by excluding each of the birth cohorts from the estimation sample. The dashed vertical line indicates the cutoff year of birth, 1979.

Figure A.2: Robustness check by excluding each of the birth cohorts within the bandwidths.

Appendix B Additional tables.

Table B.1: Matching Ethnicity Codes to Murdock (1967) and Gray (1999).

(1) RePEAT Data	(2) Ethnographic Atlas	(3) Reference	(4) Brideprice practice
Acholi	Luo	O, S	Yes
Alur	Luo	S	Yes
Badama	Luo	J	Yes
Bafumbira	Rwanda-Rundi*	E, W	Yes
Baganda	Ganda	O, S	Yes
Bagisu	Gisu	O, S	Yes
Bagwere	Soga	E	Yes
Bahororo	Nyankole	J, E	Yes
Bakenyi	Soga	E	Yes
Bakiga	Nyankole	E	Yes
Bakonjo	Nyoro	E	Yes
Banyankore	Nyankole	O, S	Yes
Banyarwanda	Rwanda-Rundi*	W	Yes
Banyole	Gisu	O	Yes
Banyoro	Nyoro	O, S	Yes
Baruli	Soga	E	Yes
Barundi	Rwanda-Rundi*	W	Yes
Basoga	Soga	O, S	Yes
Batooro	Nyoro	O, S	Yes
Iteso	Teso	S	Yes
Jopadhola	Luo	S	Yes
Karimojong	Teso	O, S	Yes
Kuman	Luo	O	Yes
Langi	Lango	O, S	Yes
Samia	Gisu	W	Yes
Sebei	Kipsigis	S	Yes
Sabiny	Kipsigis	S, J	Yes
Not Ugandan	-	-	-

Notes. This table relates the ethnic groups that appear in this study's data collected from the Research on Poverty, Environment, Agriculture, and Technology (RePEAT) in Uganda in 2015 to the data from *Ethnographic Atlas*, first written by Murdock (1967) and updated by Gray (1999). Column (3) indicates the source of information that is used to match the names of ethnic groups in the RePEAT and *Ethnographic Atlas*; O stands for Olson et al. (1996), S for Stokes (2009), J for the Joshua Project (Retrieved on the 5th of July, 2019 at https://joshuaproject.net/), E for Ethnologue (retrieved on the 5th of July, 2019 at https://www.ethnologue.com/), and W for Wikipedia. *Since the discussion still continues as to where the ethnic groups in the area that is now contemporary Rwanda and Burundi come from, we assign a new code, 'Rwanda-Rundi'; this area traditionally employs brideprice practice according to Wikipedia.

Table B.2: Kink Coefficient Estimates from Regressions of Predetermined Covariates.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Bandwidth	13 years	12 years	11 years	10 years	9 years	8 years	7 years	6 years	5 years	4 years	3 years
	Panel A. Father's years of education										
I{Year of birth$\geq$ 1979}	-0.058	-0.028	0.095	0.166	0.122	0.189	0.189	0.254	0.055	-0.315	-1.467*
$\times$(Year of birth$-$1979)	(0.085)	(0.093)	(0.108)	(0.118)	(0.136)	(0.161)	(0.205)	(0.296)	(0.395)	(0.446)	(0.882)
Observations	689	655	604	557	514	452	395	330	278	237	159
R-squared	0.020	0.024	0.022	0.031	0.016	0.017	0.010	0.003	0.006	0.003	0.021
	Panel B. Mother's years of education										
I{Year of birth$\geq$ 1979}	-0.016	0.012	0.073	0.133	0.145	0.214	0.270	0.408*	0.155	0.164	-0.624
$\times$(Year of birth$-$1979)	(0.068)	(0.076)	(0.087)	(0.099)	(0.115)	(0.140)	(0.170)	(0.245)	(0.308)	(0.398)	(0.645)
Observations	716	678	625	572	528	464	408	336	281	238	155
R-squared	0.025	0.028	0.030	0.037	0.029	0.037	0.034	0.016	0.007	0.002	0.006
	Panel C. Pre-marital religion: Christian										
I{Year of birth$\geq$ 1979}	0.010*	0.009	0.010	0.011	0.008	0.014	0.024*	0.022	0.047**	0.024	0.084*
$\times$(Year of birth$-$1979)	(0.005)	(0.006)	(0.007)	(0.008)	(0.010)	(0.011)	(0.013)	(0.018)	(0.021)	(0.031)	(0.048)
Observations	1007	953	878	804	737	648	576	481	411	344	234
R-squared	0.004	0.004	0.003	0.003	0.002	0.003	0.006	0.003	0.014	0.003	0.010
	Panel D. Pre-marital religion: Muslim										
I{Year of birth$\geq$ 1979}	-0.009*	-0.008	-0.009	-0.011	-0.007	-0.014	-0.023*	-0.022	-0.047**	-0.026	-0.095**
$\times$(Year of birth$-$1979)	(0.005)	(0.006)	(0.007)	(0.008)	(0.010)	(0.011)	(0.013)	(0.018)	(0.021)	(0.031)	(0.047)
Observations	1007	953	878	804	737	648	576	481	411	344	234
R-squared	0.004	0.004	0.002	0.003	0.002	0.003	0.006	0.004	0.016	0.005	0.012

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Table B.2: Continued.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Bandwidth	13 years	12 years	11 years	10 years	9 years	8 years	7 years	6 years	5 years	4 years	3 years
	Panel E. Premarital region of residence: Eastern										
I{Year of birth≥ 1979}	-0.006	-0.010	-0.010	-0.010	-0.014	-0.037**	-0.018	0.006	0.027	0.015	-0.168**
×(Year of birth−1979)	(0.009)	(0.009)	(0.011)	(0.013)	(0.014)	(0.017)	(0.021)	(0.027)	(0.035)	(0.046)	(0.076)
Observations	944	892	818	748	688	605	536	455	388	322	221
R-squared	0.000	0.002	0.001	0.001	0.003	0.008	0.002	0.001	0.002	0.000	0.033
	Panel F. Premarital region of residence: Central										
I{Year of birth≥ 1979}	-0.018**	-0.016**	-0.017*	-0.013	-0.002	0.005	-0.005	-0.002	-0.017	0.003	0.084
×(Year of birth−1979)	(0.007)	(0.008)	(0.009)	(0.011)	(0.013)	(0.016)	(0.018)	(0.024)	(0.030)	(0.040)	(0.079)
Observations	944	892	818	748	688	605	536	455	388	322	221
R-squared	0.007	0.005	0.004	0.002	0.000	0.000	0.001	0.000	0.001	0.000	0.013
	Panel G. Premarital region of residence: Western										
I{Year of birth≥ 1979}	0.010	0.012	0.006	0.000	-0.008	0.000	-0.004	-0.037*	-0.037	-0.053	-0.049
×(Year of birth−1979)	(0.008)	(0.008)	(0.009)	(0.011)	(0.012)	(0.015)	(0.018)	(0.022)	(0.029)	(0.038)	(0.072)
Observations	944	892	818	748	688	605	536	455	388	322	221
R-squared	0.009	0.013	0.014	0.010	0.018	0.014	0.014	0.010	0.006	0.006	0.002
	Panel H. Premarital region of residence: Northern										
I{Year of birth≥ 1979}	0.016***	0.016**	0.021***	0.022***	0.025***	0.030***	0.021*	0.033**	0.038*	0.048*	0.137**
×(Year of birth−1979)	(0.006)	(0.006)	(0.007)	(0.008)	(0.010)	(0.012)	(0.013)	(0.017)	(0.020)	(0.026)	(0.055)
Observations	944	892	818	748	688	605	536	455	388	322	221
R-squared	0.019	0.014	0.017	0.014	0.017	0.017	0.008	0.009	0.007	0.009	0.034

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Table B.2: Continued.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Bandwidth	13 years	12 years	11 years	10 years	9 years	8 years	7 years	6 years	5 years	4 years	3 years
	Panel I. Ethnicity: Baganda										
I{Year of birth≥ 1979}	-0.015**	-0.015**	-0.015**	-0.010	-0.006	-0.004	-0.004	0.012	-0.014	0.007	0.0584
×(Year of birth−1979)	(0.006)	(0.006)	(0.007)	(0.009)	(0.010)	(0.013)	(0.015)	(0.021)	(0.025)	(0.034)	(0.067)
Observations	1,016	962	887	812	746	656	584	489	418	350	238
R-squared	0.010	0.008	0.005	0.004	0.003	0.002	0.001	0.001	0.001	0.001	0.008
	Panel J. Ethnicity: Basoga										
I{Year of birth≥ 1979}	0.003	-0.002	0.000	0.005	0.005	-0.002	0.007	0.023	0.022	-0.003	-0.061
×(Year of birth−1979)	(0.005)	(0.006)	(0.007)	(0.008)	(0.009)	(0.010)	(0.013)	(0.018)	(0.024)	(0.029)	(0.042)
Observations	1,016	962	887	812	746	656	584	489	418	350	238
R-squared	0.003	0.000	0.000	0.001	0.002	0.001	0.003	0.005	0.005	0.002	0.007
	Panel K. Ethnicity: Banyankore										
I{Year of birth≥ 1979}	0.004	0.006	-0.001	-0.001	-0.004	-0.003	-0.012	-0.005	-0.001	-0.003	0.035
×(Year of birth−1979)	(0.005)	(0.006)	(0.006)	(0.007)	(0.008)	(0.010)	(0.011)	(0.015)	(0.020)	(0.025)	(0.047)
Observations	1,016	962	887	812	746	656	584	489	418	350	238
R-squared	0.002	0.002	0.000	0.000	0.001	0.001	0.002	0.000	0.001	0.000	0.005
	Panel L. Ethnicity: Langi										
I{Year of birth≥ 1979}	0.013**	0.012**	0.015**	0.013*	0.013	0.005	-0.003	-0.007	0.005	0.028	0.127**
×(Year of birth−1979)	(0.005)	(0.006)	(0.006)	(0.007)	(0.008)	(0.009)	(0.010)	(0.012)	(0.015)	(0.022)	(0.050)
Observations	1,016	962	887	812	746	656	584	489	418	350	238
R-squared	0.018	0.011	0.012	0.008	0.012	0.005	0.012	0.013	0.012	0.012	0.052

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Table B.2: Continued.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Bandwidth	13 years	12 years	11 years	10 years	9 years	8 years	7 years	6 years	5 years	4 years	3 years
	Panel M. Ethnicity: Acholi										
I{Year of birth$\geq$ 1979}	0.000	0.003	0.008	0.010	0.012	0.022**	0.028**	0.029**	0.022	0.027	0.028
$\times$(Year of birth$-$1979)	(0.004)	(0.005)	(0.006)	(0.006)	(0.007)	(0.009)	(0.011)	(0.014)	(0.017)	(0.022)	(0.040)
Observations	1,016	962	887	812	746	656	584	489	418	350	238
R-squared	0.002	0.002	0.006	0.004	0.003	0.008	0.012	0.017	0.014	0.008	0.002
	Panel N. Ethnicity: Others										
I{Year of birth$\geq$ 1979}	-0.006	-0.004	-0.007	-0.016	-0.021	-0.018	-0.017	-0.052**	-0.034	-0.056	-0.187**
$\times$(Year of birth$-$1979)	(0.009)	(0.009)	(0.011)	(0.012)	(0.014)	(0.017)	(0.020)	(0.026)	(0.034)	(0.045)	(0.080)
Observations	1,016	962	887	812	746	656	584	489	418	350	238
R-squared	0.001	0.000	0.001	0.002	0.004	0.002	0.002	0.008	0.007	0.006	0.028

Source. Research on Poverty, Agriculture, Environment, and Technology in Uganda in 2015. *Notes.* This table shows the coefficient estimate of the interaction between the year of birth minus the cutoff of 1979 and an indicator for it being equal to or larger than the cutoff from the regression of female years of education. Reported in parentheses are standard errors robust for heteroscedasticity. Statistical significance is denoted by *** for p < 0.01, ** for p < 0.05, and * for p < 0.1. 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 females aged 24 to 49 who have ever married and were born in years within the indicated bandwidth of the cutoff. Due to missing values, the number of observations differs across regressions.

Table B.3: Estimated Effects of Female Education on Marital Probability.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Bandwidth	13 years	12 years	11 years	10 years	9 years	8 years	7 years	6 years	5 years	4 years	3 years
	Panel A. Treatment effect estimates with linear specification										
Treatment effect	-0.111***	-0.080***	-0.062***	-0.061***	-0.058***	-0.039**	-0.034	-0.011	0.023	0.047	-0.084
	(0.022)	(0.023)	(0.020)	(0.026)	(0.023)	(0.021)	(0.035)	(0.023)	(0.030)	(0.039)	(0.099)
Observations	1077	991	895	806	736	641	558	472	399	331	232
	Panel B. Treatment effect estimates with quadratic specification										
Treatment effect	-0.011	-0.018	-0.009	-0.007	0.035	-0.057	0.018	-0.050	0.073	0.051	-0.150
	(0.030)	(0.029)	(0.045)	(0.030)	(0.056)	(0.047)	(0.029)	(0.054)	(0.255)	(0.076)	(0.134)
Observations	1077	991	895	806	736	641	558	472	399	331	232
	Panel C. Akaike information criterion (AIC) from the reduced-form regression of marital probability										
Linear model	669.2	572.4†	475.6†	397.9†	309.8	222.1	176.0†	155.7	81.7†	79.9†	105.5
Quadratic model	653.1†	573.9	477.1	400.5	309.2†	218.0†	177.4	154.6†	83.5	82.1	102.6†

Source. Research on Poverty, Environment, Agriculture, and Technology in Uganda in 2015. *Notes.* Panels A and B show the treatment effect estimate of female years of education on the indicator for ever having married. Panel C shows the AIC values from the reduced-form regressions of the marital indicator on the first-stage regressors. Reported in parentheses are standard errors robust to heteroscedasticity. Statistical significance is denoted by *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$. † indicates that the AIC is smaller relative to the other model specification; *i.e.*, the focal model specification is preferred to the other. All regressions include dummies for ethnicity and region of residence at age seven as covariates. The regressions are run for females aged 24 to 49 who married at the age of 24 or below and for females born in years within the indicated bandwidth of the cutoff.

Table B.4: Robustness Check Using Only Females Who were 24 Years or Younger at Their First Marriage.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Bandwidth	13 years	12 years	11 years	10 years	9 years	8 years	7 years	6 years	5 years	4 years	3 years
1 if having brideprice paid	-0.084**	-0.121**	-0.105**	-0.116**	-0.024	-0.030	-0.084	-0.024	-0.035	-0.075	-0.715
	(0.050)	(0.060)	(0.052)	(0.066)	(0.042)	(0.047)	(0.121)	(0.059)	(0.069)	(0.114)	(2.309)
Observations	822	778	711	647	598	532	468	393	340	280	189
Age at marriage	0.761**	0.684**	0.456	0.438	0.254	-0.347	-1.643	-0.213	-0.426	-0.556	0.130
	(0.362)	(0.389)	(0.358)	(0.433)	(0.369)	(0.468)	(1.456)	(0.524)	(0.621)	(0.896)	(1.455)
Observations	879	834	764	698	644	567	499	422	361	298	204
1 if love marriage	0.027	0.027	0.043**	0.033	0.018	0.009	-0.013	-0.023	-0.056*	-0.092	0.020
	(0.025)	(0.026)	(0.024)	(0.026)	(0.021)	(0.023)	(0.043)	(0.028)	(0.040)	(0.072)	(0.094)
Observations	886	839	770	703	648	570	502	424	362	300	206
1 if living premarital LC1	0.056	0.029	0.012	0.021	0.008	-0.012	-0.026	-0.029	0.014	0.076	-0.005
	(0.045)	(0.043)	(0.038)	(0.042)	(0.033)	(0.039)	(0.071)	(0.043)	(0.054)	(0.086)	(0.175)
Observations	889	842	771	705	648	569	501	424	362	299	205
1 if polygynous union	0.009	-0.010	-0.026	-0.007	0.008	0.013	-0.022	-0.019	0.003	-0.005	-0.053
	(0.037)	(0.036)	(0.032)	(0.038)	(0.031)	(0.037)	(0.064)	(0.035)	(0.042)	(0.054)	(0.125)
Observations	894	847	776	708	651	572	504	426	364	301	206
1 if not divorced	-0.015	-0.048	-0.047	-0.046	-0.021	-0.013	-0.054	-0.015	-0.036	-0.015	-0.021
	(0.040)	(0.046)	(0.044)	(0.049)	(0.030)	(0.035)	(0.056)	(0.037)	(0.046)	(0.041)	(0.067)
Observations	755	715	655	602	559	493	432	371	319	264	182

Source. Research on Poverty, Environment, Agriculture, and Technology in Uganda in 2015. *Notes.* This table shows the treatment effect estimate for the probability of receiving brideprice for all the available bandwidths in our data. Reported in parentheses are standard errors robust to heteroscedasticity. Statistical significance is denoted by *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$. 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 females aged 24 to 49 who married at the age of 24 or below and were born in years within the indicated bandwidth of the cutoff. Due to missing values, the number of observations differs across regressions.

Table B.5: First-Stage Estimation Results of Partner's Years of Education.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Outcome	Male years of education										
Bandwidth	13 years	12 years	11 years	10 years	9 years	8 years	7 years	6 years	5 years	4 years	3 years
$\mathbf{I}\{$Year of birth$\geq$1979$\}$	-0.041	-0.016	-0.021	0.010	0.069	0.045	0.073	-0.071	-0.038	-0.013	0.083
$\times$(Year of birth$-$1979)	(0.068)	(0.077)	(0.087)	(0.101)	(0.119)	(0.145)	(0.182)	(0.218)	(0.285)	(0.409)	(0.942)
Observations	747	705	660	612	563	506	452	369	315	249	179
R-squared	0.200	0.199	0.196	0.214	0.221	0.230	0.279	0.342	0.404	0.425	0.425
F statistic	0.370	0.043	0.059	0.009	0.339	0.097	0.162	0.106	0.018	0.001	0.008

Source. Research on Poverty, Environment, Agriculture, and Technology in Uganda in 2015. *Notes.* This table shows the coefficient estimate of the interaction between the year of birth minus the cutoff of 1979 and an indicator for it being equal to or larger than the cutoff from the regression of male years of education. Reported in parentheses are standard errors robust for heteroscedasticity. Statistical significance is denoted by *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$. All regressions include a constant, the year of birth minus the cutoff, and the dummies for the ethnicity and region of residence at age seven as covariates. The regressions use males born in years within the indicated bandwidth of the cutoff.

Table B.6: Robustness Check of Main Results Allowing for a Potential Jump at the Cutoff.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Bandwidth	13 years	12 years	11 years	10 years	9 years	8 years	7 years	6 years	5 years	4 years	3 years
1 if brideprice paid	-0.107**	-0.126**	-0.111**	-0.111**	-0.033	-0.047	-0.086	-0.031	-0.026	-0.080	0.010
	(0.052)	(0.056)	(0.048)	(0.056)	(0.035)	(0.045)	(0.078)	(0.044)	(0.053)	(0.089)	(0.094)
Observations	878	832	762	695	640	562	495	417	356	294	201

Source. Research on Poverty, Environment, Agriculture, and Technology in Uganda in 2015. *Notes.* This table shows the treatment effect estimate for the probability of receiving brideprice for all the available bandwidths in our data. Reported in parentheses are standard errors robust to heteroscedasticity. Statistical significance is denoted by *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$. 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 females aged 24 to 49 who have ever married and were born in years within the indicated bandwidth of the cutoff.

Table B.7: Akaike Information Criterion (AIC) for the Reduced-Form Regression of Brideprice Receipt.

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. 1 if having brideprice paid										
Linear model	1118.4†	1055.3†	964.7†	898.1†	827.0†	740.6†	669.3†	550.4†	475.3†	392.5†	266.4
Quadratic model	1119.4	1057.8	966.9	901.6	829.7	743.3	672.2	552.1	476.4	395.2	265.6†
	Panel B. 1 if having brideprice paid (0 if unmarried)										
Linear model	1363.6†	1275.8†	1159.8†	1074.2†	987.3†	870.3†	777.9†	647.1†	549.0†	460.5†	318.4
Quadratic model	1364.9	1278.2	1161.6	1076.4	989.5	872.3	781.1	649.5	551.1	463.0	315.6†
	Panel C. 1 if having brideprice paid (1 if unmarried)										
Linear model	1324.5	1231.3†	1122.2†	1032.2†	934.6†	831.5†	747.2†	621.9†	540.1†	445.5†	321.2†
Quadratic model	1322.7†	1234.8	1126.0	1035.7	937.1	833.4	750.0	623.5	542.7	449.2	322.5

Source. Research on Poverty, Environment, Agriculture, and Technology in Uganda in 2015. *Notes.* This table shows the values of the AIC for the reduced-form regression of brideprice receipt status. † indicates that the AIC is smaller relative to the other model specification, *i.e.*, the focal model specification is preferred to the other. Panel A use the females aged 24 to 49 who have ever married and were born in years within the indicated bandwidth of the cutoff. Panels B and C use all the females in our data, regardless of marital status, born in years within the indicated bandwidth of the cutoff, where the brideprice receipt status for never married females are all coded as one in Panel B and zero in Panel C. All the 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.

Table B.8: Robustness Check of Main Results Using the LIML Estimation Method.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Bandwidth	13 years	12 years	11 years	10 years	9 years	8 years	7 years	6 years	5 years	4 years	3 years
1 if brideprice paid	-0.099*	-0.123**	-0.110**	-0.109*	-0.031	-0.040	-0.081	-0.031	-0.039	-0.082	-0.393
	(0.051)	(0.055)	(0.048)	(0.056)	(0.035)	(0.046)	(0.089)	(0.044)	(0.056)	(0.090)	(0.588)
Observations	878	832	762	695	640	562	495	417	356	294	201

Source. Research on Poverty, Environment, Agriculture, and Technology in Uganda in 2015. *Notes.* This table shows the treatment effect estimate for the probability of receiving brideprice for all the available bandwidths in our data, using the LIML estimation method. Reported in parentheses are standard errors robust to heteroscedasticity. Statistical significance is denoted by *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$. 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 females aged 24 to 49 who have ever married and were born in years within the indicated bandwidth of the cutoff.

Table B.9: Robustness Check of the Main Results with the Alternative Cutoff of the Year 1983.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Bandwidth	9 years	8 years	7 years	6 years	5 years	4 years	3 years
	Panel A. First-stage estimation results						
I{Year of birth$\geq$ 1983}	0.175	0.085	0.014	-0.152	-0.064	0.102	-0.738
	(0.106)	(0.121)	(0.137)	(0.192)	(0.265)	(0.359)	(0.585)
Observations	635	585	503	434	372	311	238
R-squared	0.250	0.260	0.282	0.307	0.333	0.395	0.464
F statistics	2.697	0.492	0.010	0.625	0.059	0.080	1.595
	Panel B. Treatment effect estimates						
1 if brideprice paid	-0.158*	-0.451	-1.255	0.180	-0.364	-0.040	0.061
	(0.110)	(0.651)	(6.237)	(0.320)	(1.743)	(0.470)	(0.111)
Observations	623	574	494	425	364	304	235
Age at marriage	0.600	1.200	3.124	-2.821	5.525	0.760	1.093*
	(0.588)	(1.310)	(6.879)	(7.095)	(25.619)	(2.975)	(0.769)
Observations	628	578	498	429	368	310	237
1 if love marriage	0.048	0.050	0.927	-0.197	-1.477	0.467	-0.112*
	(0.046)	(0.100)	(4.271)	(0.293)	(11.020)	(1.192)	(0.086)
Observations	629	580	500	431	370	310	237
1 if living in pre-marital LC1	0.017	0.097	2.407	-0.183	-0.238	0.045	-0.003
	(0.080)	(0.258)	(32.338)	(0.288)	(1.544)	(0.323)	(0.093)
Observations	631	581	499	431	369	308	235
1 if in polygynous union	0.000	-0.020	-0.863	0.044	0.044	0.235	-0.059
	(0.065)	(0.149)	(7.853)	(0.136)	(0.442)	(0.806)	(0.076)
Observations	635	585	503	434	372	311	238
1 if not divorced	-0.002	-0.061	-0.435	0.020	-0.010	-0.068	-0.024
	(0.075)	(0.132)	(3.340)	(0.147)	(0.123)	(0.283)	(0.095)
Observations	543	502	429	374	320	269	207

Source. Research on Poverty, Environment, Agriculture, and Technology in Uganda in 2015. *Notes.* This table shows the treatment effect estimate for the probability of receiving a brideprice for all the available bandwidths in our data using the year of birth of 1983 as the alternative cutoff. Reported in parentheses are standard errors robust for heteroscedasticity. Statistical significance is denoted by *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$. 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 females aged 24 to 49 who have ever married and were born in years within the indicated bandwidth of the cutoff.

Table B.10: Robustness Check of Main Results Excluding the Langi and Those from the Northern Region.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Bandwidth	13 years	12 years	11 years	10 years	9 years	8 years	7 years	6 years	5 years	4 years	3 years
	Panel A. Excluding the Langi										
1 if brideprice paid	-0.126**	-0.150**	-0.136**	-0.147**	-0.043	-0.053	-0.104	-0.037	-0.048	-0.084	-0.280
	(0.064)	(0.070)	(0.063)	(0.080)	(0.045)	(0.054)	(0.108)	(0.045)	(0.058)	(0.082)	(0.280)
Observations	813	773	709	651	602	532	473	397	337	278	191
	Panel B. Excluding the northern region										
1 if brideprice paid	-0.142**	-0.175**	-0.154**	-0.173**	-0.041	-0.050	-0.097	-0.048	-0.060	-0.095	-0.239
	(0.072)	(0.082)	(0.072)	(0.098)	(0.051)	(0.067)	(0.132)	(0.050)	(0.066)	(0.105)	(0.209)
Observations	758	720	660	606	561	495	441	371	317	264	182

Source. Research on Poverty, Environment, Agriculture, and Technology in Uganda in 2015. *Notes.* This table shows the treatment effect estimate for the probability of receiving a brideprice for all the available bandwidths in our data excluding the females of the Langi (Panel A) and from the Northern region (Panel B) from the estimation sample. Reported in parentheses are standard errors robust to heteroscedasticity. Statistical significance is denoted by *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$. 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 females aged 24 to 49 who have ever married and were born in years within the indicated bandwidth of the cutoff.

Table B.11: Robustness Check of Main Results Conditioned on Parental Education.

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. Controlling for mother's years of education										
1 if brideprice paid	-0.028	-0.065	-0.052	-0.035	0.019	0.033	0.112	0.077	0.071	0.179	-0.143
	(0.062)	(0.070)	(0.060)	(0.085)	(0.055)	(0.068)	(0.184)	(0.074)	(0.098)	(0.319)	(0.273)
Observations	625	591	539	492	457	401	350	291	244	203	133
	Panel B. Controlling for mother's years of education and its missingness										
1 if brideprice paid	-0.098**	-0.119**	-0.112**	-0.113*	-0.030	-0.042	-0.094	-0.028	-0.044	-0.090	-0.309
	(0.049)	(0.052)	(0.049)	(0.058)	(0.036)	(0.051)	(0.115)	(0.052)	(0.065)	(0.111)	(0.363)
Observations	878	832	762	695	640	562	495	417	356	294	201
	Panel C. Controlling for father's years of education										
1 if brideprice paid	-0.034	-0.053	-0.039	-0.029	0.033	0.001	0.010	0.028	0.039	0.027	0.041
	(0.059)	(0.059)	(0.052)	(0.059)	(0.052)	(0.053)	(0.116)	(0.075)	(0.061)	(0.069)	(0.444)
Observations	602	570	519	477	441	387	334	282	237	199	133
	Panel D. Controlling for father's years of education and its missingness										
1 if brideprice paid	-0.091**	-0.114**	-0.107**	-0.118*	-0.033	-0.046	-0.095	-0.030	-0.037	-0.076	-0.606
	(0.045)	(0.048)	(0.046)	(0.061)	(0.036)	(0.051)	(0.107)	(0.051)	(0.058)	(0.093)	(1.512)
Observations	878	832	762	695	640	562	495	417	356	294	201
	Panel E. Controlling for both parents' years of education										
1 if brideprice paid	-0.028	-0.073	-0.055	-0.020	0.021	0.009	0.050	0.086	0.099	0.265	-0.342
	(0.073)	(0.082)	(0.068)	(0.085)	(0.061)	(0.071)	(0.156)	(0.105)	(0.143)	(0.637)	(1.026)
Observations	536	508	461	421	394	346	298	253	210	175	114
	Panel F. Controlling for both parents' years of education and their missingness										
1 if brideprice paid	-0.094**	-0.118**	-0.112**	-0.123*	-0.033	-0.048	-0.098	-0.027	-0.045	-0.096	-0.444
	(0.047)	(0.050)	(0.048)	(0.063)	(0.037)	(0.054)	(0.120)	(0.054)	(0.066)	(0.123)	(0.745)
Observations	878	832	762	695	640	562	495	417	356	294	201

Source. Research on Poverty, Environment, Agriculture, and Technology in Uganda in 2015. *Notes.* This table shows the treatment effect estimate for the probability of receiving brideprice for all the available bandwidths in our data, controlling for the years of education of the sample females' parents: mothers' (Panels A and B), fathers' (Panels C and D), and both (Panels E and F). Since parental education variables have relatively high shares of missing values, we tried an additional specification with an indicator for parental education being missing (Panels B, D, and F). Reported in parentheses are standard errors robust to heteroscedasticity. Statistical significance is denoted by *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$. All regressions include dummies for ethnicity and region of residence at age seven as covariates. The regressions are run for females aged 24 to 49 who have ever married and were born in years within the indicated bandwidth of the cutoff.

Table B.12: Robustness Check of Main Results Conditioned on Religion.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Bandwidth	13 years	12 years	11 years	10 years	9 years	8 years	7 years	6 years	5 years	4 years	3 years
	Panel A. Controlling for own pre-marital religion										
1 if brideprice paid	-0.097*	-0.120**	-0.108**	-0.107*	-0.029	-0.037	-0.081	-0.026	-0.018	-0.084	-0.468
	(0.052)	(0.055)	(0.049)	(0.057)	(0.036)	(0.047)	(0.096)	(0.046)	(0.051)	(0.087)	(0.858)
Observations	873	827	757	691	636	559	492	414	354	293	200

Source. Research on Poverty, Environment, Agriculture, and Technology in Uganda in 2015. *Notes.* This table shows the treatment effect estimate for the probability of receiving brideprice for all the available bandwidths in our data, controlling for own pre-marital religion dummies. Statistical significance is denoted by *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$. 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 females aged 24 to 49 who have ever married and were born in years within the indicated bandwidth of the cutoff.

Table B.13: Robustness Check of Main Results Allowing for a Differential Trend in and after 2007.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Bandwidth	13 years	12 years	11 years	10 years	9 years	8 years	7 years	6 years	5 years	4 years	3 years
1 if brideprice paid	-0.126*	-0.138**	-0.104**	-0.112*	-0.021	-0.034	-0.070	-0.032	-0.038	-0.084	-3.72
	(0.079)	(0.076)	(0.060)	(0.073)	(0.044)	(0.053)	(0.080)	(0.048)	(0.054)	(0.094)	(54.72)
Observations	867	823	754	688	635	558	491	414	354	292	199

Source. Research on Poverty, Environment, Agriculture, and Technology in Uganda in 2015. *Notes.* This table shows the treatment effect estimate for the probability of receiving a brideprice for all the available bandwidths in our data, allowing for a differential trend for females whose first marriage took place before or after the year of 2007. Reported in parentheses are standard errors robust to heteroscedasticity. Statistical significance is denoted by *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$. 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 females aged 24 to 49 who have ever married and were born in years within the indicated bandwidth of the cutoff.

Table B.14: The Estimated Impact of Female Education on Whether Women Have a Non-agricultural Job.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Bandwidth	13 years	12 years	11 years	10 years	9 years	8 years	7 years	6 years	5 years	4 years	3 years
1 if the female has a non-agricultural job	-0.002	0.024	-0.018	-0.039	-0.016	0.002	-0.047	0.046	0.028	0.069	0.061
	(0.043)	(0.045)	(0.040)	(0.047)	(0.036)	(0.042)	(0.083)	(0.045)	(0.052)	(0.066)	(0.150)
Observations	891	844	773	705	648	570	502	424	362	300	205

Source. Research on Poverty, Environment, Agriculture, and Technology in Uganda in 2015. *Notes.* This table shows the estimated treatment effect of female education on whether females have a non-agricultural job for selected bandwidths. Reported in parentheses are standard errors robust to heteroscedasticity. Statistical significance is denoted by *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$. 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 females aged 24 to 49 who have ever married and were born in years within the indicated bandwidth of the cutoff.

Table B.15: Robustness Check of Main Results Excluding a Few Control Cohorts.

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. Excluding 1 youngest control cohort										
1 if brideprice paid	-0.096*	-0.124*	-0.108**	-0.105*	-0.031	-0.039	-0.067	-0.02	-0.013	-0.044	0.194
	(0.058)	(0.064)	(0.053)	(0.061)	(0.036)	(0.048)	(0.088)	(0.049)	(0.07)	(0.117)	(0.407)
Observations	852	806	736	669	614	536	469	391	330	268	175
	Panel B. Excluding 2 youngest control cohorts										
1 if brideprice paid	-0.112*	-0.14**	-0.122**	-0.123*	-0.041	-0.05	-0.084	-0.032	-0.044	-0.121	0.338
	(0.063)	(0.071)	(0.058)	(0.069)	(0.038)	(0.05)	(0.095)	(0.053)	(0.083)	(0.148)	(0.846)
Observations	818	772	702	635	580	502	435	357	296	234	141
	Panel C. Excluding 3 youngest control cohorts										
1 if brideprice paid	-0.115*	-0.142**	-0.128**	-0.128*	-0.049	-0.059	-0.079	-0.038	-0.044	-0.083	
	(0.062)	(0.07)	(0.059)	(0.068)	(0.037)	(0.048)	(0.081)	(0.045)	(0.069)	(0.117)	
Observations	785	739	669	602	547	469	402	324	263	201	
	Panel D. Excluding 4 youngest control cohorts										
1 if brideprice paid	-0.128*	-0.15**	-0.13**	-0.125*	-0.045	-0.053	-0.069	-0.03	-0.024		
	(0.069)	(0.073)	(0.061)	(0.067)	(0.037)	(0.046)	(0.072)	(0.042)	(0.069)		
Observations	747	701	631	564	509	431	364	286	225		
	Panel E. Excluding 5 youngest control cohorts										
1 if brideprice paid	-0.119*	-0.143**	-0.124**	-0.122*	-0.049	-0.053	-0.078	-0.03			
	(0.061)	(0.068)	(0.057)	(0.063)	(0.036)	(0.045)	(0.083)	(0.049)			
Observations	716	670	600	533	478	400	333	255			

Source. Research on Poverty, Environment, Agriculture, and Technology in Uganda in 2015. *Notes*. *Notes*. This table shows the treatment effect estimate for the probability of receiving brideprice for all the available bandwidths in our data, when excluding females born in years before but close to 1979. The estimation cannot be done when exluding a large number of cohorts and using a small bandwidth as this results in a perfect collinearity between the treatment indicator and the intercept due to a lack of control females in the estimation sample. Reported in parentheses are standard errors robust to heteroscedasticity. Statistical significance is denoted by *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$. 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 females aged 24 to 49 who have ever married and were born in years within the indicated bandwidth of the cutoff.

Table B.16: Simple Regression for Assortative Matching.

	(1)	(2)	(3)
Outcome	Partner's years of education		
Female years of education	0.466***	0.467***	0.432***
	(0.089)	(0.091)	(0.103)
$\mathbf{I}\{\text{Year of birth} \geq 1979\}$	0.075	-0.063	0.110
	(0.652)	(0.712)	(0.971)
$\mathbf{I}\{\text{Year of birth} \geq 1979\}$	0.033	0.054	0.102
$\times$(Years of education)	(0.112)	(0.120)	(0.139)
Observations	397	354	353
R-squared	0.218	0.452	0.529
Premarital controls	N	Y	Y
Year of marriage	N	N	Y

Source. Research on Poverty, Environment, Agriculture, and Technology in Uganda in 2015. *Notes.* This table shows the selected coefficient estimate of female years of education from the regression of their partners' years of education. Reported in parentheses are standard errors robust for heteroscedasticity. Statistical significance is denoted by *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$. Premarital controls include the dummies for ethnicity and for the region of residence at age seven. The regressions are run for females aged 24 to 49 who have ever married and were born in years within the indicated bandwidth of the cutoff. Due to missing values, the number of observations differs across regressions.