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

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Abstract

We estimate the effect of female education on brideprice using own collected data from rural Uganda. With the fuzzy regression kink design and instrumental variable estimations exploiting the universal primary education policy in the country, we show that female education reduces brideprice payment probability, but does not increase amounts paid or female labour supply. Our finding suggests that a development policy may change a cultural practice, contributing to the growing literature on culture and institutions. We then explore the mechanism of the cultural shift and consider factors such as the trade-off between future quality marital life and immediate brideprice payment, and the changing pattern of mating such that better educated females marry younger males with less earnings as of marriage.

Keywords: brideprice, female education, culture and institution, Uganda.
JEL classification codes: I21, I25, O55, Z13.

1 Introduction

In much of Sub-Sahara Africa, marriage is established when the families of the groom and the bride agree upon brideprice payment—transfers from the groom and his family to the bride's family at marriage. Brideprice payment is practiced widely, but Uganda stands out for the debate on the negative consequences of the practice (Wendo (2004) and Thiara and Hague (2009)). In Uganda, where marriage is typically virilocal (the wife leaves her natal family and moves into the husband's household), it appears that the

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brideprice is really the price for which the husband purchases a female, which is referred to as 'commoditization' of females. Further, the divorced husband at times requests reimbursement of brideprice that he has paid by then, referred to as 'collateralisation' of females. The debate finally led to the decision by the country's Supreme court that reimbursement of brideprice at dissolution of a customary marriage is unconstitutional, while the practice of brideprice payment per se is not (Library of Congress, 2015).

As to the role of brideprice, economics literature takes a somewhat different point of view. If, for instance, brideprice has a role to compensate the bride's family for her labour or income that she would earn and contribute to her family if not married, it would positively respond to female education to the extent that human capital matters for her foregone economic activities (Anderson (2007), Becker (1991)). In addition, a positive response of brideprice to female education is not just posited in economics but similar hypotheses are posed in other social science fields as well (e.g. Goody and Tambiah (2011), Bell (1998)). Several studies have pointed to this hypothesis, but to the best of our knowledge, papers that investigate this causal link empirically are few, if not null.

We study whether higher educational attainments of females translate into higher brideprice that they receive at their marriage using own collected data in Uganda. To estimate the effect causally, we exploit the introduction of the Universal Primary Education (UPE) in Uganda, which abolished school fees for all pupils who were enrolled at primary school in 1997 and onwards. We find that the reform increased females' years of education significantly, while its impact on male education was not statistically distinguishable from zero. The insignificant effect for males may be due to the baseline educational attainments of males which had already been high enough to complete primary school, and consistent with previous studies that report smaller effects for males than for females (Deininger (2003), Nishimura et al. (2008)). This reform is used in our estimation strategy to measure the impact of female education on brideprice.

Our estimation strategy is to use the educational reform in a parametric version of the fuzzy regression kink design, as well as in a conventional instrumental variable estimation. The former estimates the treatment effect on the treated, including always takers and the treated compliers, whereas the latter estimates that of the compliers. This means that the two estimates are not directly comparable. However, we take advantage of this difference, and discuss how heterogeneous the effects would be for different populations

through their comparison.

Our findings show that Ugandan females are less likely to have brideprice paid at their marriage when they achieve higher grades of education. However, we could not conclude that there was any increase in amounts of brideprice. In addition, we do not find evidence that better educated females with brideprice payment at marraige are more likely to engage in non-farm employment, nor evidence that there was any change in the assortative matching in marriage market in terms of male and female education—the feature that marital partners tend to have similar traits.

Our findings suggest, first, that the culture is now changing due to an educational policy, adding a new piece of evidence to the literature on culture and institutions (Alesina and Giuliano, 2013). In particular, our study contributes the evidence that an institution may change culture in a quasi-experimental setting in the interdependent relationship between the two. It is also suggested, second, that the conventional interpretation of brideprice, either human capital compensation or the result of assortative matching, may not directly apply in our data. Third, the decline of polygyny,[1] as proposed by some previous studies (e.g., Tertilt (2005) and Fenske (2015)) may not fully explain our findings as some key variables do not show the movements in the way they predict.

We provide two hypotheses for the cultural shift. One is that better educated females and their family may become more aware of potential downsides of brideprice, such as sexual infidelity (Bishai and Grossbard, 2010) and domestic violence (Kaye et al., 2005). Then, they may choose not to receive brideprice: a choice over the trade-off between the immediate payment of brideprice and the future sound marital life. Another is based on our finding that better educated females marry younger males. If the conventional age-earnings profile applies to rural Uganda, such males earn less on average than older males who would have married the females had it not been for the increase in their years of education. Then, the less ability of younger males to pay brideprice may have led to the decline in the cultural change.

The rest of this paper is structured as follows. The next section provides a brief overview of economic literature on brideprice and female education. After a discussion on the universal primary education reform in Uganda, we describe our data set and present estimation results. The last section concludes with implications for the marriage

[1]Polygyny refers to the type of marriage where one man marries multiple wives. Polygamy is a wider concept, and so it also includes polyandry in which a woman marries multiple men.

institution in Uganda and for future studies.

2 Literature of Brideprice and Female Education

Wealth transfers upon a marriage can be directed in various ways. Ones from the groom's side are referred to as either brideprice if the bride's parents receive them or dower if the newly married couple, particularly the bride, does. Likewise, transfers from the bride's side are called dowry if the groom receives them or groom price if the groom's family do (Papps et al., 1983). Since the practice of brideprice payments is the most typical case in Sub-Saharan Africa (Fafchamps and Quisumbing, 2007) and other channels are reportedly nil (Fafchamps and Quisumbing, 2005a), we direct the focus of this research towards brideprice only.

Economic research presents marriage market models in which women and men search their marrying partner originated from the seminal work by Becker (1991). His model implies that three major factors would affect brideprice: assortative matching, marriage squeeze, and compensation for human capital. In the literature, the first two factors have been extensively investigated for in South Asia (see, for instance, Rao (1993) and Anderson (2003) for India) followed by works on brideprice in Sub-Saharan Africa (see, for instance, Fafchamps and Quisumbing (2002) for Ethiopia). The implications of brideprice as a means of compensation for human capital investment of the bride, on the other hand, have been relatively less analyzed using data from Africa until Platteau and Gaspart (2007) and Ashraf et al. (2016). This is the literature to which we aim to provide empirical evidence.

In theory, the brideprice payments would positively reflect human capital investments of girls. Marriage market models predict that brideprice would arise when there is a mismatch in the number of males and females in the market and parents would have an incentive to invest in human capital accumulation of girls, because the brideprice could serve as compensation for the bride's family for letting go of family labour (Becker, 1991). Anderson (2007) takes into consideration the Sub-Saharan African context and claims that brideprice payments would be larger, and observed more often, given the importance of the wives' labour input in household production and agriculture. In an empirical setting, however, the causality is difficult to establish: in the presence of assortative

mating in the marriage market, a better educated woman may be more likely to marry a better educated man who, at the same time, may come from a richer household in the first place. Either way, from the woman's perspective, a higher brideprice may be correlated with a higher amount of brideprice.

As the first empirical studies on the relationship between the level of education of women and brideprice in Sub-Sahara Africa, Platteau and Gaspart (2007) examined data from Senegal. One downside with their study is that their data are small in sample size (176 observations with brideprice payments). Another is that, due to the very low levels of education completed by women in their sample, they construct an index variable that equals one if the woman has completed the primary schooling (21 observations) and zero otherwise. Nonetheless, they find that the brideprice was significantly higher for educated women for arranged marriage. Although their analysis is later extended to study divorce Gaspart and Platteau (2010), this is one of the first empirical results explicitly discussing the relationship between female education and brideprice in Sub-Saharan Africa.

Another piece of evidence for the impact of education on brideprice is presented by Ashraf et al. (2016). They use the Indonesia Family Life Survey data in 2000 and 2007, and Demographic and Health Survey data from Zambia in 1996, 2001, 2007, and 2013. In Indonesia and Zambia, these years overlap each country's period of school construction boom. Exploiting the fact that there are ethnic groups that practice brideprice payment and others that do not, their triple difference regressions show that the amount of brideprice payments increases as does the bride's level of education. They conclude that the brideprice tradition incentivise parents to invest in girls' education, and this leads to higher amounts of brideprice.

Polygyny, or the type of marriage that one male marries more than one females, is also arguably related to brideprice. The first paper of the kind may be Jacoby (1995) that analysed the link between polygyny and female agricultural productivity. His results indicate that polygyny (demand for wives) intensifies when, conditional on the man's wealth, the price for obtaining additional wives is cheap given female agricultural productivity. Tertilt (2005) considers a scenario opposite to this. She sets up an overlapping generations model with males and females, choosing the number of children and wives, and how much to invest in productive activities. Her model implies that, when polygyny is allowed, there always exists positive payment of brideprice, and the economy has

slow growth. This is because that, when there is positive brideprice, raising a girl has a positive return and having a boy has a zero return (since brideprice is paid by the son, not the parent), children become a good investment good. This leads to high fertility and demand for multiple wives in equilibrium. Her simulation shows that, when polygyny is simply banned, brideprice takes a negative value (i.e. the direction of payment reverses), the population growth slows down, and the economy boosts its growth, due to the allocation of investment shifted from children to production.

However, empirical evidence is arguably little when one looks at factors related to brideprice in Uganda. As briefly discussed in Introduction, brideprice has been criticised in the country. In a virilocal society where brides leave their natal household out to the groom's, brideprice flows in the opposite direction to females. This is criticised since it looks as if men purchase women. The criticism extends to refundability of brideprice ever paid as of divorce: if brideprice is refundable, then husbands may more likely exercise domestic violence or extra-marital infidelity, while wives may be pushed to a vulnerable position (Wendo (2004); Thiara and Hague (2009)). Bishai and Grossbard (2010) document some evidence on such differential behaviours between husbands and wives. Similarly, Kaye et al. (2005) report that wives seem to fear the possibility of such violence or faithlessness.

On the contrary, Alesina et al. (2016) use Ugandan data in their cross-country study and show that brideprice tradition may reduce violence against women, suggesting the effect potentially preferable to women. Using data from Senegal, Mbaye and Wagner (2016) also show women's increased say in household decision on fertility. However, we do not know the extent to which this study has relevance to rural Uganda, since violence and murder related to brideprice is still reported in local newspaper[2] even after the Supreme Court made the decision about brideprice practices in the country (Library of Congress, 2015).

3 Universal Primary Education in Uganda

Uganda has an education system that consists of 7 years of primary, 6 years of secondary, and 3 to 5 years of tertiary education. Household survey data in 1992 reveal that 62.1%

[2]Examples include Daily Monitor (2016a), Daily Monitor (2016b), New Vision (2016a), and New Vision (2016b).

of children aged 6 to 12 years attended school at primary school, while the percentage varies from 45.7% to 81.7% for the lowest and highest per capita household expenditure quintiles, respectively (Deininger, 2003). Table 1 of Deininger (2003) shows that the attendance rate for girls was almost always lower than that for boys, except for those aged 6 to 8 years from households in the highest quintile. The lowest rate, 28.1%, was recorded by girls aged 6 to 8. Similar observation is presented by Uganda Bureau of Statistics (UBoS) and ORC Macro (2004), which reports that 66% of girls and 68% of boys in primary schooling ages attended school in 1995. They also report that the attendance rate is higher in urban areas (78% of children aged 6 to 12) than in rural areas (66%). Overall, Uganda had an issue on its education system functioning.

A major impediment was then said to be the costs of schooling, both direct and indirect, borne by parents and family. Nishimura et al. (2008) note that the share of private resources in the total direct cost was more than 80% and it was higher than other low income countries. Other costs of education paid by parents and family include school uniforms, textbooks, and contributions to Parents and Teachers Association.

Given the notion on heavy financial burden on households with school age children, a reform to the education system took place in January, 1997, and payments of school fees were eliminated under the name 'Universal Primary Education' (UBoS, 2003). Uganda had, by then, put into practice a variety of educational reforms such as curriculum improvements, teachers training, and primary completion examination criteria (Grogan, 2008). However, Grogan (2008) states that it was not until December 1996 that the abolition of school fees was announced by President Museveni who was elected in the year's spring. The announcement was followed by advertising campaign, which made nearly all parents and guardians of school age children hear of the reform in 2001 (MOES, 2001).

The change brought about by Uganda UPE was monumental. The number of enrolled children aged 6 to 8 increased from 2.7 million in 1996 to 5.3 million in 1997 and, steadily, further to 7.3 million in 2002 (UBoS, 2003; Riddell (2003)). Deininger (2003) finds a positive impact of UPE on primary enrolment, more favourable to girls and poor households. Nishimura et al. (2008) also find that UPE significantly pushed up educational attainments of children, and the effects were higher for girls than boys. They provide more evidence on the impact of the UPE on reduction in delayed enrolment and completion of higher grades. Although there are still quite a few people who never attend school, drop

out of school, or discontinue in higher levels of education (UBoS and ICF International Inc., 2012), it would not be exaggeration to say that Uganda UPE was successful.

A caution needs to be paid to the process of implementation of the UPE in Uganda. In the original planning, up to four children per household should be provided with the UPE scholarship.[3] However, the scholarship was said to be eventually provided to everyone who were then enrolled in primary school (Grogan, 2008). This raises a difficulty in how to split the control and treatment groups, which is discussed in more detail in the next section. Here we conclude that this educational reform in Uganda can be thought of as an exogenous intervention. That is, the reform serves as an instrument to identify the causal effects of women's educational attainments on the brideprice practices.

4 Estimation Strategy

Here we discuss the methodology used in our study, fuzzy regression kink design, and its identifying assumptions. It is followed by an extension to the instrumental variable approach. We also discuss the differences between the two estimations.

4.1 Regression Kink Design Estimation

The institutional background above raises an important point in the educational reform. That is, the reform did not introduce free primary education to particular cohorts, but to all those who were enrolled in primary school in 1997 and onwards. Since the older cohorts are less likely to remain enrolled in primary school, they were less likely to benefit from the reform. This is clearly illustrated in Figure 1 where the share of females who were enrolled in primary school is zero for many older cohorts whereas it shows a somewhat S-shaped increase for younger cohorts. It is found in the institutional background that everyone in primary school in 1997 were eligible for the treatment, and thus it is reasonable to interpret that the share plotted in Figure 1 as the probability of treatment. This created the kinked increase in years of education as in Figure 2 where the educational attainment of the older females was stable over years while the younger cohorts exhibited a steady increase in years of education.[4]

[3]The scholarship was paid to school, and the amount was no more than the tuitions and the PTA fees.

[4]Figure 2 has a vertical line indicating the cutoff year, 1981, which is discussed soon below.

Given this, we consider the following esitimation that mimics the fuzzy regression kink design (RKD) in order to identify the causal effect of education on brideprice, where the treatment is the years of education and the running variable is the year of birth. Let y be some outcome, z the years of education, x the year of birth, and c the cutoff. Then, in the fuzzy RKD, the treatment-on-the-treated (TOT) parameter τ is expressed (Card et al., 2015) as

$$\tau = \frac{\lim\limits_{x_0 \to +c} \left. \frac{d\,\mathrm{E}[y|x]}{dx} \right|_{x=x_0} - \lim\limits_{x_0 \to -c} \left. \frac{d\,\mathrm{E}[y|x]}{dx} \right|_{x=x_0}}{\lim\limits_{x_0 \to +c} \left. \frac{d\,\mathrm{E}[z|x]}{dx} \right|_{x=x_0} - \lim\limits_{x_0 \to -c} \left. \frac{d\,\mathrm{E}[z|x]}{dx} \right|_{x=x_0}} \tag{1}$$

where the change in the first-order derivative of the conditional expectation of the outcome at the cutoff is evaluated by the corresponding change in the years of education. For this, we estimate the following parametric model with two equations

$$y_i = \sum_{k=0}^{p_y} \alpha_k (x_i - c)^k + \mathbf{I}\{x_i \geq c\} \sum_{k=0}^{q_y} \beta_k (X_i - c)^k + W_i \phi + u_i \tag{2}$$

$$z_i = \sum_{k=0}^{p_z} \gamma_k (x_i - c)^k + \mathbf{I}\{x_i \geq c\} \sum_{k=0}^{q_z} \delta_k (X_i - c)^k + W_i \psi + v_i \tag{3}$$

for observations i: $x_i \in [c - h_-, c + h_+)$, where p_y, q_y, p_z, and q_z are some integer orders of polynomials, $\mathbf{I}\{\cdot\}$ an indicator function that takes the value 1 if the condition in the bracket holds and 0 otherwise, and W pre-marital controls. From the two estimated equations, we compute the following parameter for the treatment effect of interest

$$\tau_{\text{FRKD}} = \frac{\beta_1}{\delta_1}. \tag{4}$$

4.2 Choice of the Cutoff

Whether our estimation strategy can effectively estimate the causal parameter hinges on the exogeneity of the introduction of the UPE in Uganda. Here we argue that it should have been difficult, if not impossible, to precisely predict when the UPE would be implemented.

First, electoral results are *ex ante* uncertain. In the presidential election in May, 1996, two more candidates were running for the presidential office along with Yoweri

Museveni: Paul Ssemogerere and Kibirige Mayanja. Although Museveni won over 3 times more votes than any other candidate in total, he lost in quite a few districts in the Northern region and some in the Central and Eastern regions to the second place Ssemogerere (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 1996, even those voters who supported Museveni in his winning constituencies would not have been able to predict how popular he would be in other places, let alone his overall victory in the race. In addition, there was another election in June 1996 for the members of the parliament. These two elections in a year should have created large uncertainty over the politics of Uganda thereafter.

Second, Museveni was said to be reluctant to implement the UPE. He arguably put a larger emphasis on infrastructure development in his economic development planning. Furthermore, the government as a whole, and not just the newly-elected President himself, showed little to no interest in pursuing the removal of primary school tuition at all, despite the call for it by the international society (Stasavage, 2005). All these facts support that the reform was indeed introduced all of a sudden.[5]

Third, our study makes a conservative choice of the cutoff. Unlike a sharp RK/RD design, the choice of the cutoff is somewhat arbitrary. It is particularly so in this study as the delayed entrance and grade repetition are prevalent in Uganda and, moreover, the implementation did not filter out any child. Keats (2014) chooses the year 1983 as the cutoff in his study with the RD estimation. Given this cutoff of 1983, the oldest treated cohort was 14 years old in 1997 when the reform was implemented. That implies that, if the youngest control cohort (born in 1982) started schooling at the age 8 and completed primary education, any grade repetition contaminates the control group. Thus, we choose

[5]His manifesto (Museveni, 1996a) states that he was planning to initiate a reform to allow parents to send four children per household for free in 1997. However, the year 1997 was noted only in the written manifesto: it was never discussed in his oral speech (Museveni, 1996b). That is, the information about 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. According to World Bank's database, the literacy rate for those aged 15 or above was only 56% in 1991 and 68% in 2002. The literacy rate statistics may well be driven by those in urban areas, while our data come from the rural areas. Thus we assume that the proportion of the politically literate in our data is small. This is likely true, judging from the fact that the government launched a massive political campaign to publicise the UPE reform after its announcement in December 1996 (Grogan, 2008); if the reform had already been well known to the public, the government would not have needed a massive campaign to make the citizens aware of the reform.

1981 as the cutoff, allowing the controlled children to have up to 4 years of delay in total due to delayed entry, leave of absence, or grade repetition. Even with this conservative cutoff choice, Figure 1 shows some treated females in the control group. Nevertheless, as the contamination is small in fraction, we believe our conservative choice is better than 1983 at least. We then use the year 1978 as an alternative cutoff that ensures no contamination in the control group and confirmed that our results are robust to the cutoff choice.

Choosing the cutoff conservatively further strengthens the validity of the identifying assumption. Of the foremost importance in terms of the cutoff choice is whether or not the population of interest—females born in years around the cutoff—had precise manipulative power to determine their own treatment status (Lee and Lemieux, 2010). Intuitively, the opportunity cost of schooling should be higher for older cohorts whose foregone labour activities would be more expensive. This suggests that, if any, manipulative receipt of the treatment may be more difficult for older cohorts.

4.3 Identifying Assumptions

In order to identify the treatment effect, we need to examine two identifying assumptions: (1) impossibility of precise manipulation of treatment status, and (2) non-existence of a kink at the cutoff in pre-determined covariates. While we leave the examination of the second assumption to the results section below, it is noteworthy to discuss the first, about the selection into the treatment group.

A conventional procedure to check the possibility of manipulative treatment take-up is to perform the density test proposed by McCrary (2008). The test, however, builds on the non-parametric local linear regression (LLR), but LLR is inappropriate when the forcing variable is discrete (Lee and Card, 2008) which applies to our setting, where year of birth is used as the forcing variable. In addition, as reviewed above, the eligibility of the UPE scholarship was not based on year of birth of each pupil, and the cutoff is chosen by us. This means that the population of interest, either the treated or the compliers, would not have had an incentive to manipulatively report their year of birth. Therefore, we give up performing the density test in this study.

4.4 Instrumental Variable Estimation

Using the same treatment, a local average treatment effect (LATE) parameter can also be estimated from the 2-stage estimation of the form

$$y_i = \sum_{k=0}^{p_y} \zeta_k (x_i - c)^k + \tau_{\text{IV}} \hat{z}_i + W_i \kappa + \epsilon_i \tag{5}$$

$$z_i = \sum_{k=0}^{p_z} \eta_k (x_i - c)^k + \mathbf{I}\{x_i \geq c\} \sum_{k=0}^{q_z} \theta_k (x_i - c)^k + W_i \lambda + \xi_i \tag{6}$$

for observations i: $x_i \in [c - h_-, c + h_+)$. The interaction terms involving $\mathbf{I}\{x_i \geq c\}$ serve as instruments in the first-stage estimation, and the estimate of τ_{IV} represents the treatment effect of interest.

Technically, τ_{FRKD} and τ_{IV} are not directly comparable as populations of interest differ: RKD estimates the treatment effect of the treated subjects, while IV estimates that of the compliers. They give the same estimate if there is no always-taker—those who would proceed to higher grades no matter what—and all compliers—those who would change the probability of going to higher grades with the UPE—take up the treatment. In our setting, however, always takers may exist, as some females had completed primary education before the introduction of the Ugandan UPE. On the other hand, the take-up rate by the compliers may be high, and the effect may be large for them particularly if many gifted girls could not go to higher grades because of poverty. We turn to this notion at times in the results section below when we discuss interpretation and implications of the estiamtes.

In addition, the two populations of interest may not be similar for smaller bandwidths, but may be similar for larger bandwidths. This is because the treated females close to the cutoff received the treatment at a higher grade, and such people would have completed primary education even in the absence of the reform. In other words, they are likely always takers, and their share in the treated ones is high when the bandwidth is small. On the contrary, the share of compliers in the treated population possibly increases for larger bandwidths. This suggests that the parameter estimates from the two estimations may become similar for larger bandwidths, while they may differ for smaller bandwidths, implying the possibility of composition effect. Its magnitude depends upon how heterogeneous the effect of the UPE on the outcome is for the always takers and

the compliers, and we at times note this possible composition effect in the results section below.

5 Data

This study uses the data obtained by the fifth wave of the Research on Poverty, Environment, and Agricultural Technologies (RePEAT) survey in Uganda undertaken in September through December, 2015, by National Graduate Institute for Policy Studies in collaboration with Makerere University. Up to the fourth wave in 2012, randomly chosen 10 households from each of randomly chosen 94 rural villages from the Eastern, Western and Central districts were surveyed and structured as a panel data set (Yamano et al., 2004). In 2015, the RePEAT survey is extended to cover 5 more random households in each of the formerly surveyed villages with 23 additional villages, randomly chosen, from the Northern districts, which in total constitutes a 1755-household data set. In earlier waves of the RePEAT survey, questionnaires were designed to collect data on agricultural activities of rural households. In the fifth wave, we added a questionnaire specially prepared to ask about individual characteristics at the time of marriage, including the amount and mode of payments of their brideprice. The amount of brideprice was queried in three separate modes: pecuniary payments, cattle transfers, and all thes other gifts.

Data collection on the brideprice payments was restricted to each individual's first marriage only. One of the reasons for this is that the decision on the transfer should have been made at the first marriage (Arunachalam and Logan, 2016). Another is that a re-entry into marriage market after divorce, separation, or widowhood, may have such impact on brideprice payments that could be complex and different, in both observable and unobservable ways, from the first marriage. In fact, Fafchamps and Quisumbing (2005b) find that men and women remarry with significantly different likelihoods in Ethiopia. In order to eliminate such complications from the analysis, this study uses the data on women's first marriage only.

Another restriction of data collection was placed in terms of the age of respondents. A larger sample size is always preferred, but it comes with the costs associated with human resources and time spent for the entire questionnaire completion. The sample of

this study consists of those who had ever married at the time of the interview survey and were of ages between 24 and 49. For the lower bound, 24, a report (Uganda Bureau of Statistics and ICF International Inc., 2012) shows that the median age of first marriage is 17.9 for women: furthermore, it also reports that women who, among those aged 45 to 49, have never married are only 0.6%. Therefore, the sample should well represent the rural population of Uganda even with this restriction.

Table 1 presents summary statistics of major variables in thus collected data. In Panel A., the proportions of females from the north region and from the Langi ethnic group are significantly larger. This may be related to the conflict involving the Ugandan government and the Lord's Resistance Army, which displaced people in the north region including the Langi. If they are coming back to their original place of residence after the conflict, this may be explained. Other variables appear to be balanced between the treatment and control groups.

The education variables in Table 1 show that the treatment females are indeed more educated than the control counterparts. The years of completed education increased by about two years. The age of enrolment and the rate of repetition of grades decreased while years spent at primary school increased, all significantly. Although we do not explicitly use the information, the UPE treated females are more likely to proceed to high school and tertiary education. All these indicate that the UPE in Uganda indeed increased the educational attainments of women.

First marriage variables in Panel C of Table 1 show that the marriage-related behaviours of sample females did not change much for the two groups. It may seem that the treatment females are more likely to marry locally for a reason other than love, but it is left till the regression analysis to conclude whether this is due to education. No significant difference in the age at marriage may be counter-intuitive. In the context of our study, it may be that females are enrolled in primary at a younger age, and even though they are more likely to go to higher grades, such females are small in proportion and the average years of staying in the primary became smaller, so that they do not need to delay their marriage by a statistically significant length of time.

Brideprice variables are presented in Panel D of Table 1. Notable is the proportion of females who had the brideprice payment at their first marriage, which shows a statistically significant reduction for the younger females. The amounts of brideprice, conditional

upon the agreement of payment at marriage,[6]shows a substantial decrease in all three modes of payment, and statistically significant in cash payment. This is possibly because the table contains all females, including those the oldest cohorts who married in the late 1980's when the inflation rates were extremely high.[7] The total amount of brideprice, the sum of the above three, also shows a substantial decrease for the treatment cohorts, probably driven by the large change in cash brideprice relative to cattle or gift transfers. In brief, the amounts of brideprice paid in cash, and total, may not be very meaningful in estimation below: however, the descriptive statistics pushes the argument forward to examine whether or not the change in payment probability is truly due to increased female education.

6 Results

Now we summarise major findings both from graphical and estimation results, and then discuss their implications and additional works at the end.

6.1 Graphical analyses

We first check one of the identifying assumptions. Figures 3 through 10 show the trend of several pre-marital covariates. For most of the variables that appear in the figures, we cannot find a slope change that is statistically significant before and after the cutoff. With these, we conclude that the pre-determined covariates are smoothly moving around the cutoff.

We also check the graphs of some selected key outcome variables. Figure 11 plots the share of females who had brideprice payment at their first marriage. It shows that the control cohorts did not have much change in the cultural practice, while a kinked

[6]Cattle and gifts transfers were queried in real terms, as "how much it would cost to buy the same amount of cattle (or gifts) now," the cash payment was asked in nominal terms to suppress the recall bias. In order to make the three payments comparable, we used the World Bank's data of GDP deflator to adjust for inflation. We understand that it should be better to use the consumer price index (CPI), but CPI data could not cover the sample period of our study.

[7]Since females of the same cohort can marry at a different age and thus face a different inflation rate, a kink or discontinuity of the inflation rate at any year would not violate the identifying assumption. Yet, we do not know the extent to which the inflation rate prepared by the central bank of Uganda has relevance to the rural areas with which we are concerned, and we cannot tell how reliable the cash brideprice. We at least consider the possible variation of inflation rates across regions of Uganda as a robustness check.

downward trend is found for the treatment group. This may indicate that there may have been a behavioural change in the cultural practice. The conclusion should, however, wait for the regression analyses.

As for amounts paid, the cash payment in Figure 12 shows a downward trend for the control cohorts but a slight increase for the treated cohorts in the neighbourhood of the cutoff. If this is due to the kinked increase in years of education, the interpretation may be that the years of education pushed up the amount paid in cash, reverting back the long-term trend up. However, this analysis may be difficult, since the high inflation rate in the late 1980's was faced largely by older cohorts. As it is evident, the old cohorts' real values of cash brideprice are, therefore, extremely large *vis-à-vis* the young cohorts for whom the variation in the variable is negligible. In the meantime, Figures 13 and 14 do not provide similar evidence as to cattle and gifts transfers. These graphs suggest that the analyses for the cash brideprice may be complicated by the inflation, whereas the other two variables may not have had much change.

6.2 1st stage estimation

Table 2 is the first stage results in equations (3) and (6), which are essentially the same in both RKD and IV estimations. Estimations are performed for two types of females: those who had some brideprice payment upon the first marriage (paid sample, Panel A.), and those who have ever married regardless of the brideprice payment status (married sample, Panel B).

It shows that the interaction of the treatment indicator with the first order term of the year of birth minus the cutoff has the positive and significant coefficient estimate nearly throughout. It is also found that the smaller bandwidths, particularly 3 to 5 years, would not produce reliable estimates. Given the trade-off between the bias and variance in estimation, and the results here, estimates for bandwidths of 6 to 8 years may be preferable hereafter, although all the results are shown for completeness.

6.3 Brideprice variables

The RKD and IV estimation results as to the brideprice variables are shown in Tables 3 and 4.[8] The probability of paying brideprice decreased statistically significantly by about 10% for a year increase in female education. The effect seems stronger for the IV estimation, which suggests that better educated compliers whose years of education is subject to the UPE introduction are more likely to abandon the cultural practice than as educated always takers. This may be because, on one hand, always takers may have already decreased payment probability regardless of the UPE. It is also possible that compliers changed their attitudes towards brideprice more radically. Another finding from the comparison of the two estimations is that the estimated effects resemble for larger bandwidths, but not for smaller bandwidths, as discussed in relation to the composition effect. This suggests that the effect on the brideprice payment probability may be diferent for the always takers and the compliers.

Panel B's of Tables 3 and 4 show the effect of female education on amounts of brideprice paid in cash. Both estimations give large and positive treatment effect estimates, sometimes statistically significant. The interpretation of the estimates is that, for the case of the RKD estimate with the bandwidth of 11 years around the cutoff as an illustration, a year increase in female education leads to nearly 63 million Ugandan Shillings increase in the cash payment of brideprice. If this is the case, then the effect is extremely large.[9] However, the estimates are highly sensitive to the bandwidth choice. This may be due to the high inflation rate in the late 1980's faced by the older, control cohorts when they married. Since we cannot tell whether it is the female education or the inflation rate, or both, that is driving these estimates, the results may not be reliable enough. We reconsider this again in the discussion section below.

Both estimations show positive and significant, though slightly weak, coefficients while the point estimates do not differ much. This may again reflect the difference between the always takers and compliers, since the effect of education may be larger for financially disadvantaged but potentially talented females who thus could not attend school for higher grades without the educational reform. The similar point estimates suggest the

[8]The bandwidth "All" from IV estimates represents the global estimate for all the sample available in the data, and the same applies to all the subsequent tables.

[9]According to the World Databank by World Bank, the per capita GDP was only 79,000 Ugandan Shillings in 1990 and 428,000 in 2002.

composition effect may not at least change the magnitude of the effect. On the contrary, no such relationship is found for cattle and other payments of brideprice, shown in Panels C and D.

6.4 Marriage behaviours

Tables 5 and 6 present the RKD and IV estimates of the treatment effect on marriage behaviours. It appears that, for the RKD estimation, the probability of love marriage rose due to increased female education, whose effect is not found for IV estimation. This may contradict the observation that the larger the bandwidth, the more alike the two estimations. One possible explanation may be that the effect is well driven by the always takers and those compliers who are close to always takers, and these people's attitude towards love marriage is largely different from those marginal compliers who really would not have had access to higher grades of education had it not been for the UPE: the marginal compliers may still have higher probability of arranged marriage, for instance.

Other marriage behaviour variables, local marriage and age at marriage, do not seem to have had much change for better educated women. The fact that there was no effect, in particular, on age at marriage may be surprising, as this relationship is widely discussed in the literature (e.g., Field and Ambrus (2008) and Keats (2014)). This may be related to the fact that the years of education for the treatment group is about 7 and, given delayed entrance and graduation, age at the primary completion may be around 15, whereas the median age at marriage was 17. That is, if the socially desired age at marriage had not been changed in rural communities—where it may be binding—the increased female education may not necessarily lead to a rise in marital age by having a shorter period between primary graduation and marriage.

In addition, we estimate the educational effect on the age gap between spouses (husband minus wife) as well as polygamy. Panel D's of Tables 5 and 6 report that one year increase in female education reduces the age gap between spouses by about 1 to 2 years, and it does not appear that the RKD and IV estimates differ much. This may appear contradictory to the descriptive finding in Table 1 where the spousal age gap is shown to be larger for the younger treatment group. It becomes clearer by Figure 15 that reveals that the age gap was increasing for the control group whilst it started to decline for younger females, so that the slope change at the cutoff was negative. By contrast, female

education does not seem to have changed the probability of polygamy, shown in Panel E's of the two tables.[10] It is plotted in a graph as in Figure 16 that shows no sign of kink at the cutoff, suggesting that the decrease in polygamy may not be related to female education but rather a mere long-term trend.

6.5 Economic variables

Effects on several economic variables are reported in Tables 7 and 8. If the compensation hypothesis causes the positive effect of female education on brideprice, it may be that the better educated women are expected *ex ante*, at the time of marriage, to make better use of the accumulated human capital. One possible outcome to see this is a dummy for working status in non-agricultural sector, regressed in Panel A's of the two tables. We, however, did not find much effect: it may be either that the *ex post* measurement of this variable do not well represent the *ex ante* expectation, or that there may not be enough job opportunities for females in rural areas in Uganda. At least, we do not find supportive evidence of the compensation hypothesis in our data.

On the other hand, the effect on productivity is strongly evident in Panel B's where the better educated women spend fewer hours on household chore work. Yet, it does not seem to lead to longer hours spent of non-agricultural work, which is consistent with the above finding on job status. It may be possible to at least say that females can shorten their house work with longer education.

6.6 Supplementary analyses

We consider a scenario in which the RKD, as well as the IV, estimation strategy would entirely fail. It is possible that men, not just women, benefit from the UPE and better educated males have more wealth to give to their bride's family at marriage. It is also possible that such males have better negotiation skills to convince lower amounts of brideprice to pay. If these stories were ever in place, the treatment indicator would not satisfy the identifying assumption. To examine this, Table A.2 shows the first stage regression with the variables replaced by partners' respective counterparts. Surprisingly, the estimated coefficients are statistically insignificant almost throughout. This may be

[10]The dummy for polygamy is about the current marital union, and it may not necessarily indicate that the first marriage was already polygamous, which is a limitation of our data set.

related to previous studies that report a smaller impact of the UPE on male education (Nishimura et al., 2008), and in the neighbourhood of the cutoff, the effect could be insignificant. This supports our estimation strategy based on the assumption that the UPE caused the brideprice-related cultural change only through female education.

We also consider an alternative choice of the cutoff. In Figure 2, the year 1978 appears to also serve as a valid cutoff ensuring no treatment receipt in the control group. This choice of the cutoff may be seen as a fully data-driven approach. We replicated all the estimation results using the year 1978 as the cutoff, and confirmed that the conclusion is qualitatively much the same.[11] Therefore, our results can be said to be robust to the cutoff choice.

6.7 Discussion

To summarize the findings so far, the increased female education caused a drop in payment probability but not much is found for amounts of brideprice. The first finding is, to the best of our knowledge, the first evidence of the kind in economic literature. This may be due to females' perception of the adverse effect of brideprice: studies have shown that women in Uganda are worried about domestic violence, infidelity of husbands who have paid brideprice (Kaye et al. (2005), Bishai and Grossbard (2010), Wendo (2004), Thiara and Hague (2009)).[12] Another study also reports a higher probability of divorce due to brideprice payment (Gaspart and Platteau, 2010): divorce per se may not be a bad consequence, but given the differential treatment of men and women after divorce (Fafchamps and Quisumbing, 2005b), it may be the last resort for females, to say the least. That is, better educated women may have averted brideprice if the increase in years of education made them aware of possible drawbacks of the cultural practice.

The high inflation rate in Uganda in the past few decades raises one additional research question. That is, people (particularly the groom's side) prefer to agree on cash payment to cattle or other transfers and delay the actual payment, expecting the real value of brideprice to pay would decline over time. Then, there may be some strategic

[11] Results are not shown due to space limitation, but are available upon request.

[12] Alesina et al. (2016) show the negative correlation between brideprice culture and decreased violence against women, an appreciable effect of brideprice. However, we heard more stories of downsides of brideprice in our field survey amd thus consider their academic finding would not be shared at the grassroots level. Indeed, there still are some news reports in Uganda about violence, or even murder, related to brideprice (e.g., Daily Monitor (2016b)).

behaviours that may lead us to conclude differently. We examine this possibility by regressing the probability of paying in cash, cattle, and others as in Table A.1. There, all the payment modes appear to have declined due to increased female education, and no concrete evidence of modal choice is found. This implies that the effect on cash brideprice was not driven by strategic response by male partners.

Further we consider the possibility that the inflation rate in Uganda was actually different across geographical regions. In particular, the northern region had long been affected by the civil conflict, possibly facing different money and other goods supply, and thus a different inflation rate. To account for this, we repeat the regression analyses with marriage year dummies interected by region-of-residence-at-age-7 dummies. The results are shown in Table A.3.[13] Compared to the original estimation results in Tables 3 and 4, we could not find much difference: the point estimates appear sensitive to the bandwidth, but are always positive and sometimes significant. The fact that the point estimates become much smaller implies that the adjustment for inflation in the past years may not be perfect. We could not draw a convincing conclusion on whether the female education really increases the brideprice amount in our study.

In the applied microeconometric literature, there are at least two major hypotheses as to the relationship between female education and brideprice. One is that brideprice may compensate the bride's family for the foregone labour of the bride (Anderson, 2007). Another possibility is the assortative matching in marriage market, which postulates that marriage market participants find a partner with similar traits. The first compensation hypothesis is, however, not supported by the estimates on the non-agricultural work probability. This may be due to the measurement of the variable: we observe the current (or *ex post*) job status of females, while the hypothesis may be more related to the *ex ante* expectation of the husband on his wife's future job status. It may also be due to limited non-agricultural job opportunities for females in rural areas of Uganda. Either way, we cannot tell whether brideprice really compensates the bride's family for her earnings after marriage.

Or, the compensation may be a correct prediction, but it may not be for economic activities of females outside the household. Instead, brideprice may compensate for house-

[13]Because of the sample size and the number of interations, the estimations are performed only for bandwidths of 4 years or longer. This should not create much problem, though, as the 1st stage estimates in Table 2 show that the estimates for small bandwidths are already unreliable.

hold production of females. Although we cannot fully investigate the time allocation of females, our results show that females spent shorter hours for domestic work. If this means an increase in, say, family labour hours for agricultural work, then the compensation motive may be in play. Indeed, other studies show an appreciable change due to female education, such as health investment (e.g., Masuda and Yamauchi (2017) and McCrary and Royer (2011)) and many other aspects of family life (Duflo, 2012). Unfortunately, data limitation does not allow us to fully investigate this hypothesis, and even if this hypothesis was correct, it would not explain the cultural change that we found. Still, our results are at least indicative of the possibility that brideprice may remunerate the bride's family for increased productivity in household production.

Concerning assortative matching in marriage market, the correlation coefficient of own and partner's years of education became larger (0.408) for the treated females than the control females (0.312). However, a simple regression of partner's education on the treatment indicator, female education, and their interaction does not produce a significant coefficient estimate for the interaction term (Table A.4), suggesting that the matching of males and females in the marriage market may not have changed much. Overall, our data do not provide conclusive evidence as to the mechanism behind the positive effect on cash amount of brideprice, neither the compensation for female education nor the assortative matching in marriage market.

As a third alternative hypothesis, let us also discuss the decline of polygyny (Tertilt, 2005). As Figure 16 shows, the share of polygamous marriage has been decreasing over years. This may be due to the long-term increase in the share of Christians (as in Table 1) that is shown to be related to the decrease in polygyny in Sub-Sahara Africa (Fenske, 2015). However, as the figure and the regression results show, this may not be related to female education, since there does not seem any kinked change in the trend. Therefore, the model proposed by Tertilt (2005) may not fully account for our findings.

Thus, we turn back to the previous studies in Uganda. Past studies suggest that brideprice may be linked to some unfavourable consequences such as the husband's extramarital affaires (Bishai and Grossbard, 2010) and domestic violence (Kaye et al., 2005). Related news stories have been reported, even after the judicial decision of the ban of brideprice refunding, such as harassment and murder of the bride and her natal family members (e.g., Daily Monitor (2016a), Daily Monitor (2016b), New Vision (2016a), and

New Vision (2016b)). We understand that brideprice may also have positive impacts (Alesina et al. (2016), Mbaye and Wagner (2016)), but as the long debate in the court of justice implies (Thiara and Hague, 2009), the dominant discussion in the field of Uganda is arguably about the negative aspects of brideprice. If better educated females become more aware of such consequences of brideprice and averse to the cultural practice, they may choose not to receive brideprice at their marriage in the first place. This may be seen as the tradeoff between brideprice, or the immediate transfer of wealth, and her sound marital life as well as her family members' safety in the future. If this is the case, brideprice may play the role of the price for the husband's future reckless behaviours in conjugal life.

One more possible story may be related to the lower age of the husband at marriage. We find that more educated females are likely to marry younger males and hence reduce spousal age gap. If Uganda's rural population also has a conventional age-earnings profile—people earn a small amount at a younger age but increase their earnings with longer work experience—then, at the time of marriage, younger males have smaller earnings, on average, than the males who would have married the females if not treated. The younger males, thus, may appear to be less able to pay than their older counterparts, and this may have caused the change in brideprice payment probability. All these discussions are, however, merely speculative at best, and thus we call for further studies to address these empirical questions.

7 Conclusion

In this paper, we discuss the relationship between female education and brideprice payment practices in Uganda. We find that the culture is disappearing due to the increase in female education. We do not find evidence of compensation for females' productivity or assortative matching in our data. We hypothesize that the change in brideprice culture is triggered by female education where the basis for the change has been nurtured by the decline in polygamy, or that the compensation by way of brideprice payment is for the already-paid cost of female education.

One implication to Uganda's marriage systems may be that the government needs to effectively enforce its laws. In economics literature, brideprice is associated with both

positive and negative aspects of female marital life, such as reduced violence against females (Alesina et al., 2016) and sexual infidelity of the husband (Bishai and Grossbard, 2010). This way, there hardly exists a consensus on the consequences of brideprice: however, the judicial ban of reimbursement of brideprice (Library of Congress, 2015) may not at least increase the husband's moral hazard-like behaviours in marital life. Thus we call for its effective enforcement in Uganda, as well as the conventional criminal laws.

Our study, like any other, is not free of limitations. First, the way to collect brideprice amounts data needs more elaboration. It may be difficult to fully account for the past inflation rate, but our strategy may not be said well prepared enough. Second, our findings may be confined to rural areas of Uganda, since the urban areas including Kampala, the capital, may have different evaluation of female education in marriage market, as well as different attitude towards the cultural practice. Third, our estimation strategy is heavily dependent on the partial equilibrium nature of the UPE implementation, or the fact that the sudden introduction of free primary education created, over cohorts, a kinked increase in female education in the small neighbourhood of the cutoff, although this may be partly addressed through the comparison of the RKD and IV estimations. Future research are, thus, required to overcome these challenges in order to explore the relationship between culture and education in more depth.

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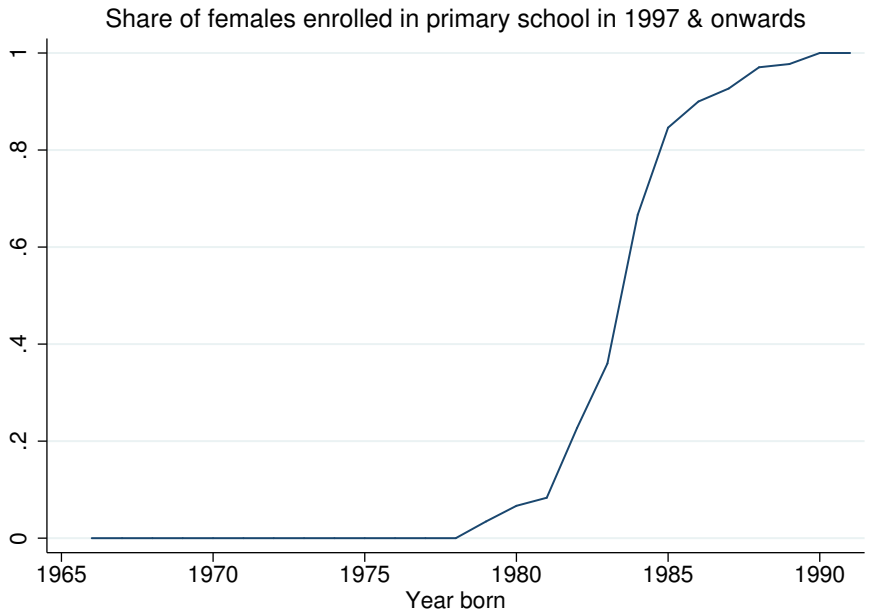

Figure 1: Share of females enrolled in primary school in 1997 and onwards.

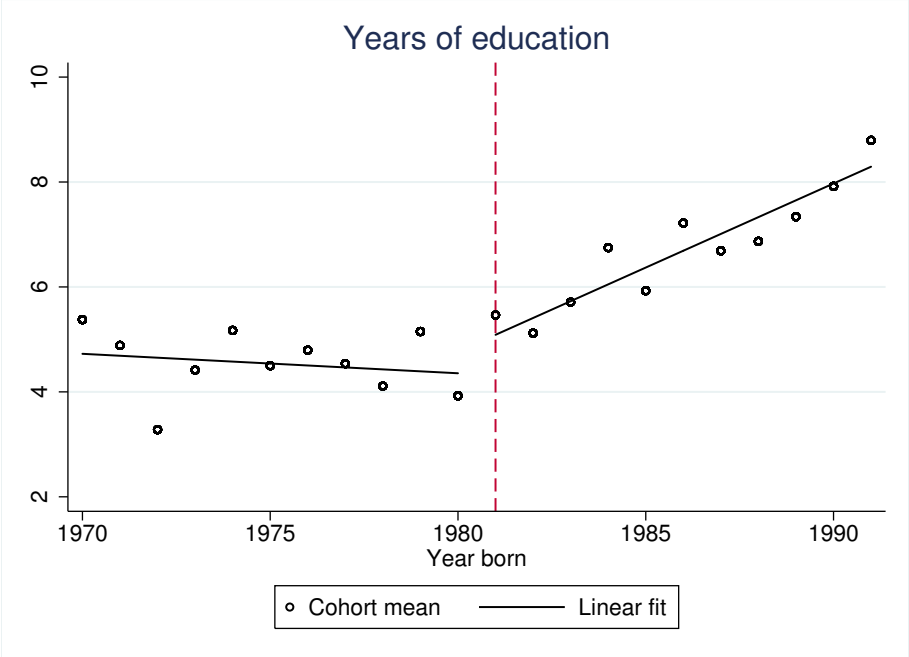

Figure 2: Years of education of females.

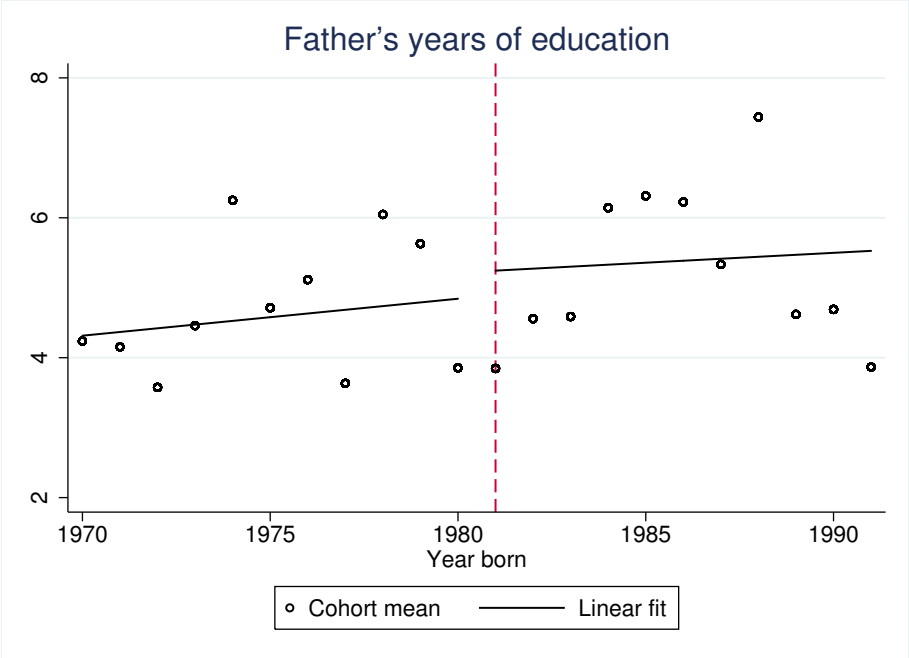

Figure 3: Father's years of education.

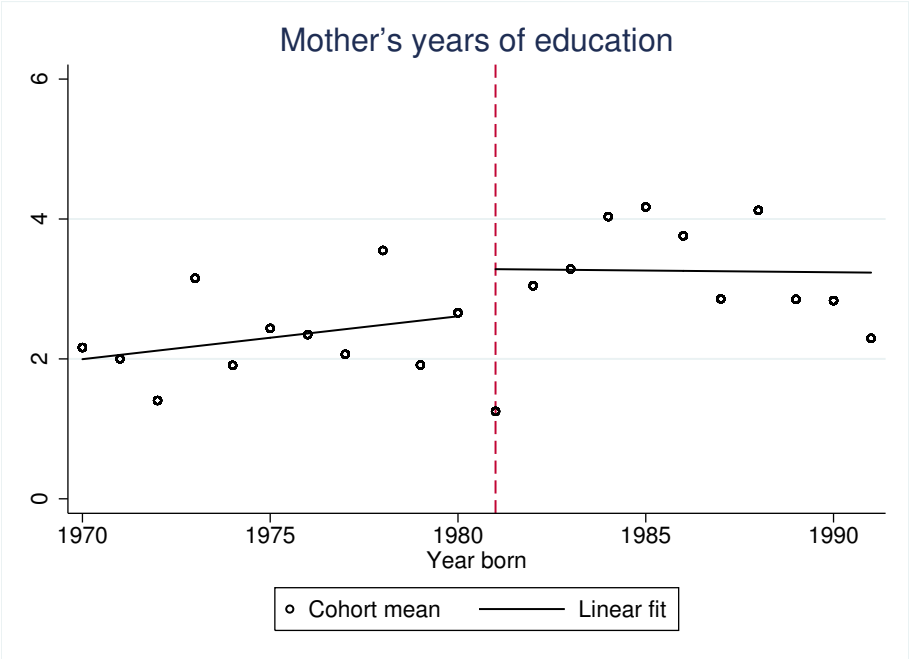

Figure 4: Mother's years of education.

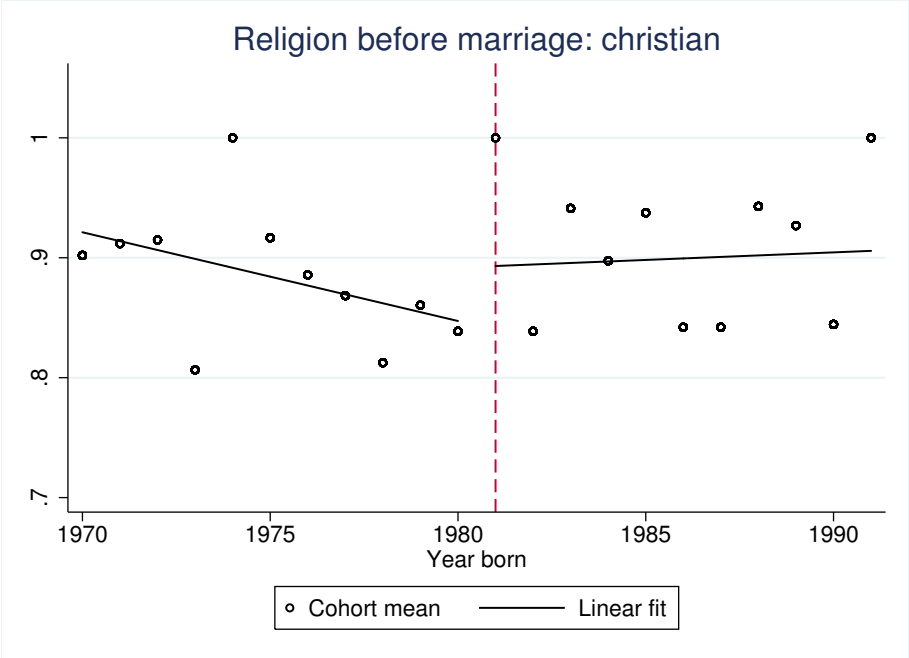

Figure 5: Share of females whose pre-marital religion was some Christianity.

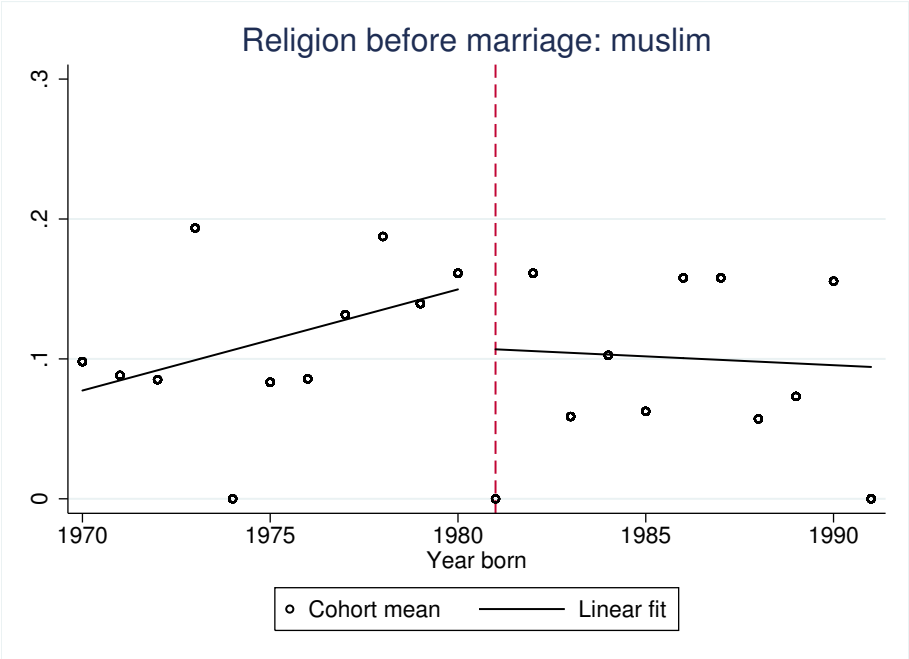

Figure 6: Share of females whose pre-marital religion was Muslim.

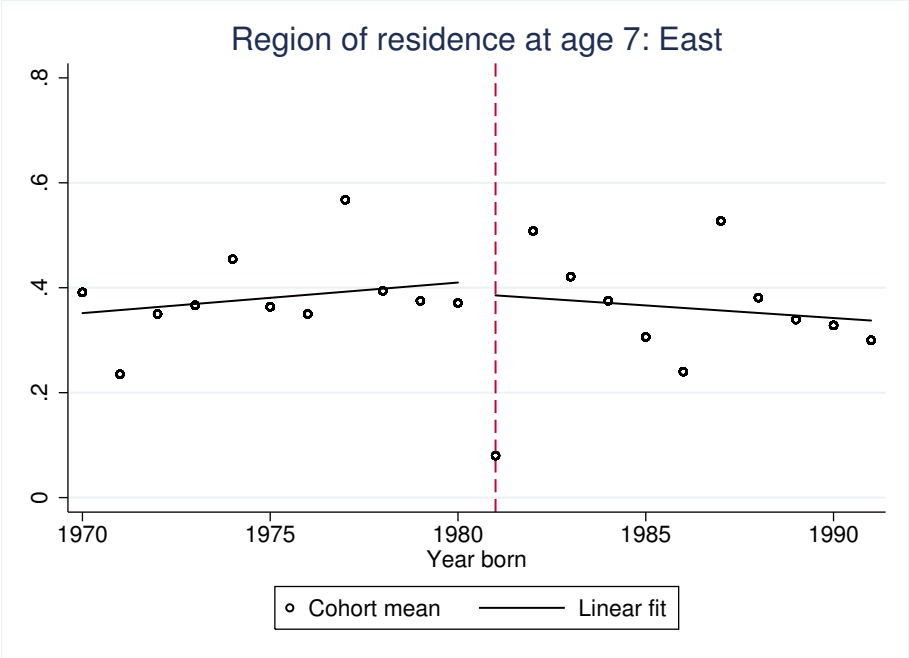

Figure 7: Region of residence at age 7: East.

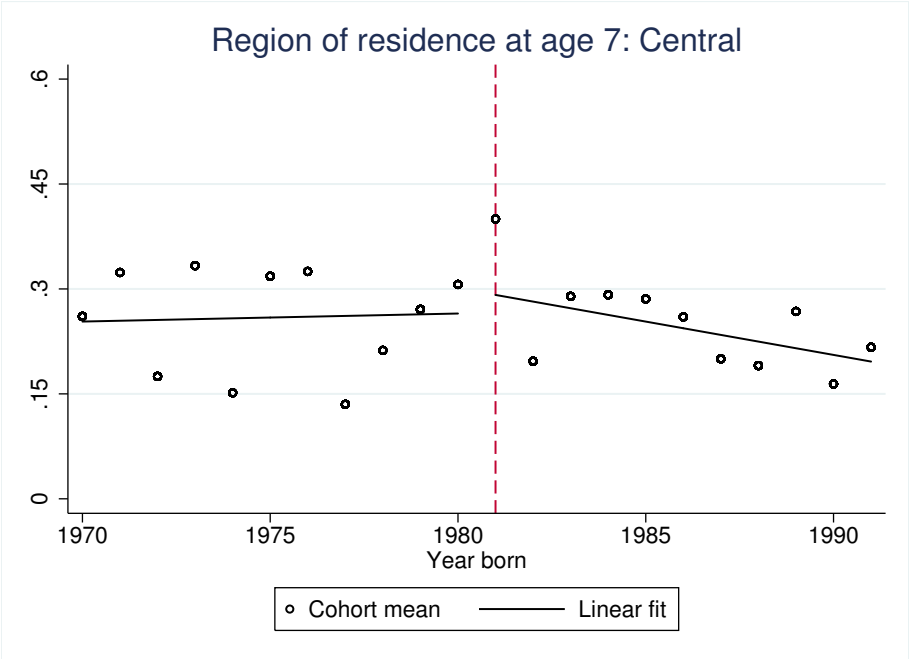

Figure 8: Region of residence at age 7: Central.

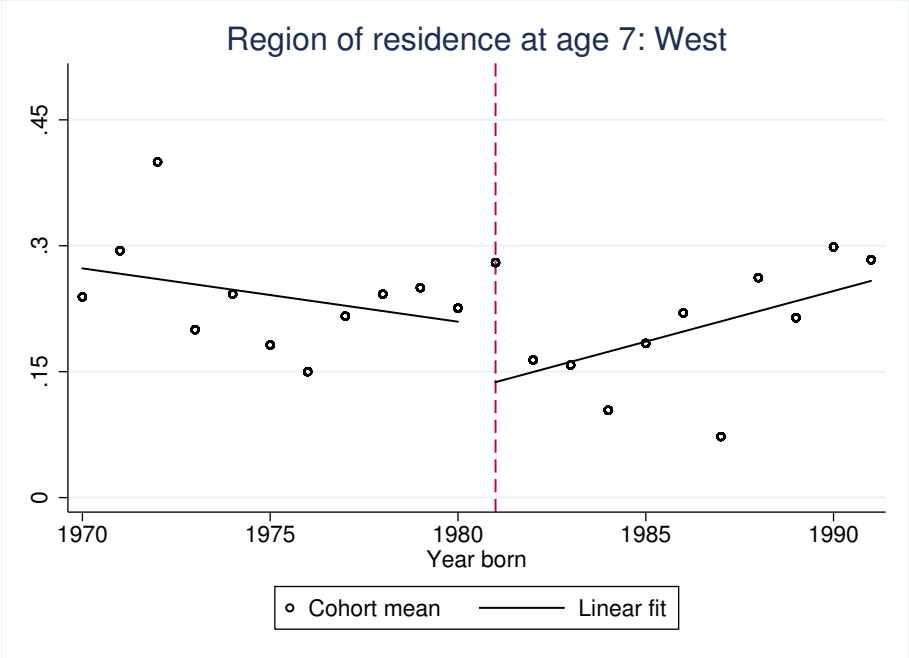

Figure 9: Region of residence at age 7: West.

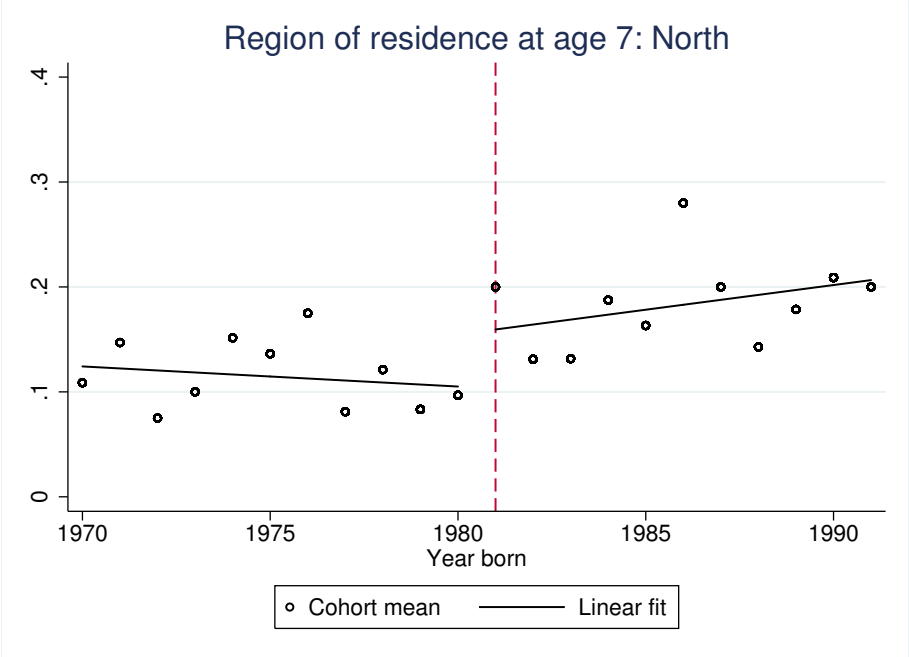

Figure 10: Region of residence at age 7: North.

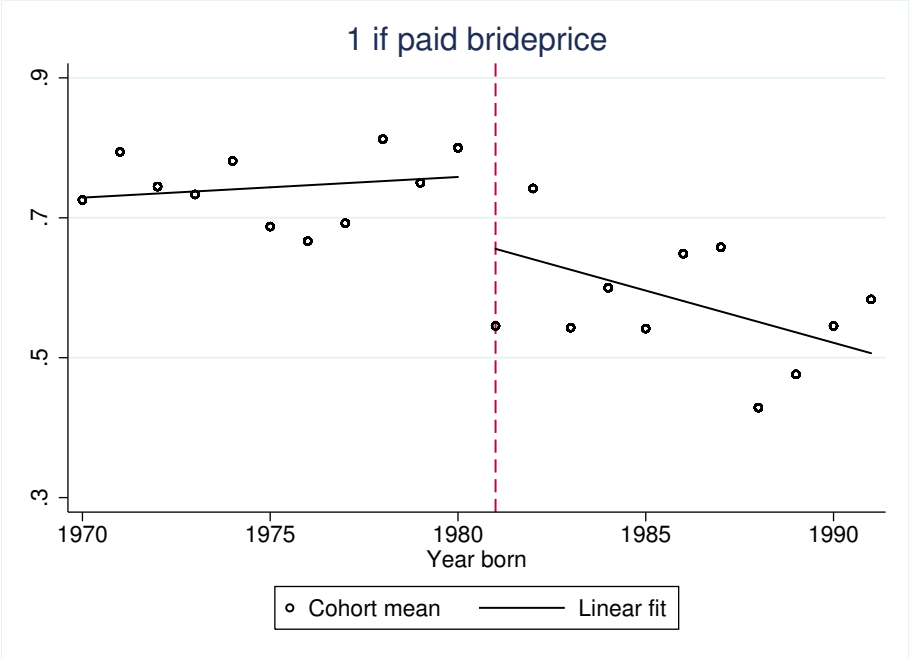

Figure 11: 1 if paid brideprice.

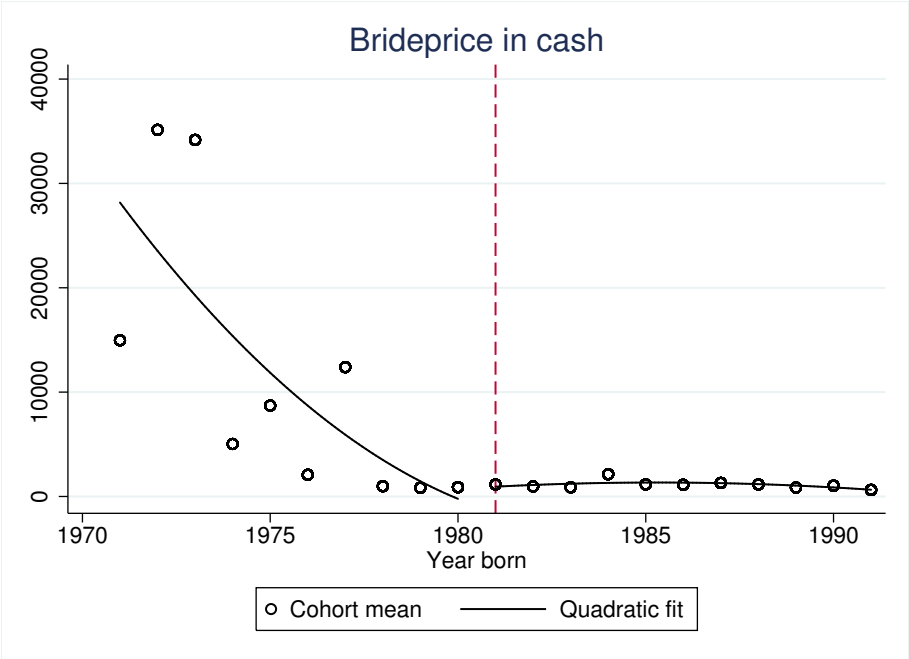

Figure 12: Brideprice paid in cash.

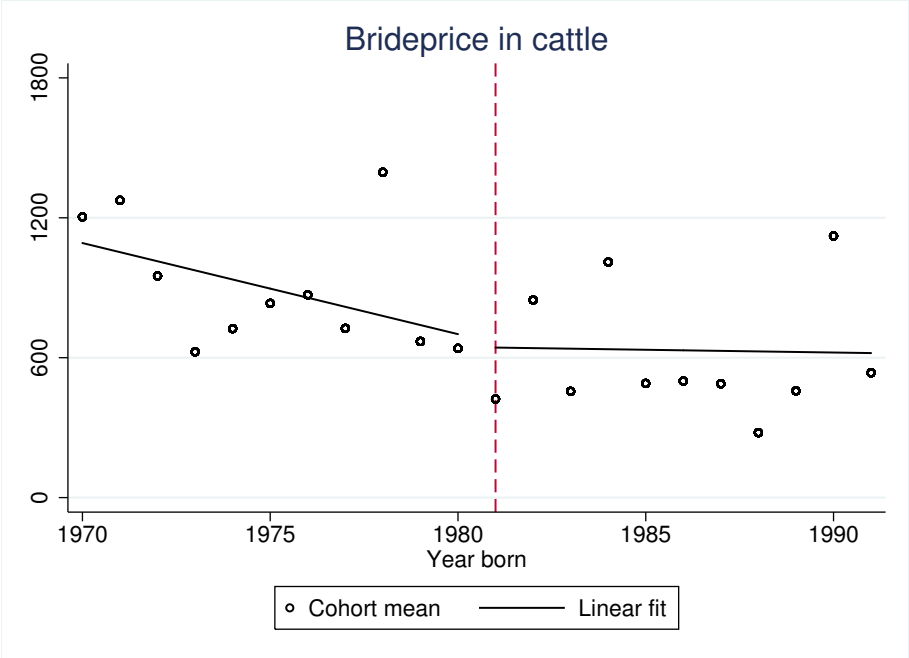

Figure 13: Brideprice paid in cattle.

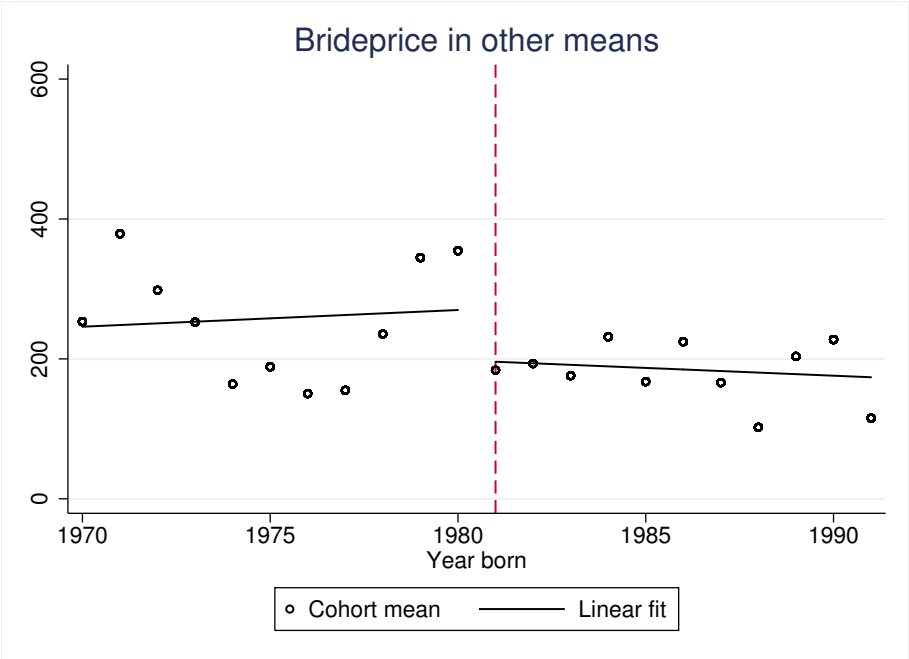

Figure 14: Brideprice paid in others.

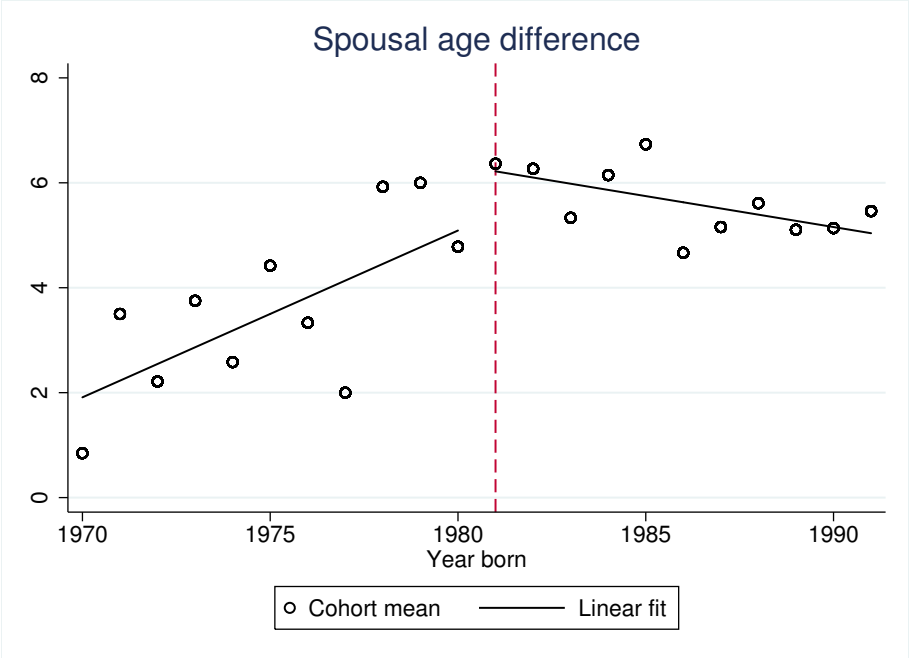

Figure 15: Spousal age difference.

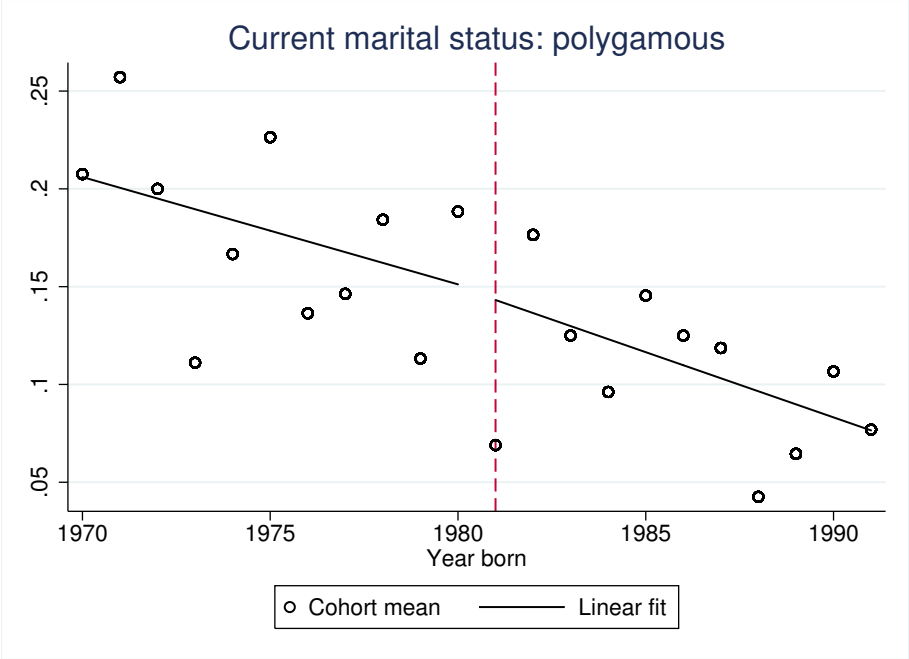

Figure 16: Current marital status: polygamous.

Table 1: Summary statistics of major variables.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Treatment group.				Control group.				(2) = (6)
Variables.	N	mean	median	sd	N	mean	median	sd	t-test
Panel A. Demographic characteristics.									
Age	610	28.57	28	3.129	510	39.74	40	3.319	−57.57***
Region of residence at age 7: East	551	0.359	0	0.480	447	0.383	0	0.487	−0.78
Region of residence at age 7: Central	551	0.240	0	0.427	447	0.260	0	0.439	−0.72
Region of residence at age 7: West	551	0.203	0	0.403	447	0.239	0	0.427	−1.36
Region of residence at age 7: North	551	0.185	0	0.389	447	0.114	0	0.318	3.17***
Own ethnicity: Baganda	585	0.159	0	0.366	488	0.193	0	0.395	−1.45
Own ethnicity: Basoga	585	0.118	0	0.323	488	0.111	0	0.314	0.36
Own ethnicity: Banyankore	585	0.103	0	0.304	488	0.105	0	0.306	−0.11
Own ethnicity: Langi	585	0.120	0	0.325	488	0.064	0	0.244	3.25***
Own ethnicity: Acholi	585	0.094	0	0.292	488	0.084	0	0.278	0.57
Own ethnicity: Bagisu	585	0.080	0	0.272	488	0.100	0	0.301	−1.12
Own ethnicity: Any other	585	0.326	0	0.469	488	0.344	0	0.476	−0.62
Panel B. Education variables.									
Own years of education	598	6.834	6	3.989	500	4.532	5	3.341	10.41***
1 if currently enroled	598	0.065	0	0.247	500	0.002	0	0.045	6.14***
Primary: if attended in any grade	598	0.918	1	0.275	500	0.790	1	0.408	5.97***
Primary: Age of enrolment	512	7.285	7	1.574	331	7.562	7	1.445	−2.62***
Primary: Years spent in school	409	6.056	6	2.148	259	5.382	6	2.283	3.80***
Primary: 1 if repeated any grade	512	0.412	0	0.493	331	0.323	0	0.468	2.64***
Primary: 1 if attended UPE school[1]	530	0.640	1	0.481	278	0.245	0	0.431	11.88***
Primary: 1 if received UPE scholarship[2]	530	0.528	1	0.500	278	0.068	0	0.253	17.35***
Secondary: 1 if attended in any grade	598	0.329	0	0.470	500	0.128	0	0.334	8.26***
Tertiary: 1 if attended in any grade	598	0.074	0	0.261	500	0.008	0	0.089	5.76***

(Continues to the next page.)

Table 1: Continued.

Variables.	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Treatment group.				Control group.				(2) = (6)
	N	mean	median	sd	N	mean	median	sd	t-test
Panel C. First marriage variables.									
1 if ever married	300	0.857	1	0.351	298	0.916	1	0.278	−2.28**
Age at first marriage	246	17.49	17	4.192	257	17.40	17	4.052	0.24
Spousal age difference (husband - wife)	210	5.652	5	3.981	185	3.724	4	3.720	4.97***
Marriage type: Love	246	0.886	1	0.318	260	0.912	1	0.285	−0.97
Marriage type: Arranged	246	0.102	0	0.303	260	0.086	0	0.279	0.67
1 if Married within kinship	245	0.090	0	0.286	260	0.081	0	0.273	0.36
Own pre-marital residence: Within LC1	244	0.279	0	0.449	260	0.238	0	0.427	1.05
Own pre-marital residence: Within Subcounty	244	0.258	0	0.439	260	0.242	0	0.429	0.41
Own pre-marital residence: Within District	244	0.205	0	0.404	260	0.242	0	0.429	−1.00
Own pre-marital residence: Within Uganda	244	0.258	0	0.439	260	0.269	0	0.444	−0.28
Own pre-marital religion: None	243	0	0	0	258	0.004	0	0.062	−1.00
Own pre-marital religion: Christian	243	0.897	1	0.304	258	0.864	1	0.343	1.14
Own pre-marital religion: Muslim	243	0.103	0	0.304	258	0.132	0	0.339	−1.01
1 if current = first marriage	214	0.818	1	0.387	230	0.813	1	0.391	0.14
1 if husband's current = his first marriage	324	0.648	1	0.478	297	0.626	1	0.485	0.57
Panel D. Brideprice variables.									
1 if paid brideprice	427	0.583	1	0.494	449	0.744	1	0.437	−5.10***
Brideprice in cash[3] (1,000 USh)	248	1,139	577.1	1,844	334	28,981	804.3	188,026	−2.706***
Brideprice in cattle[3] (1,000 USh)	247	1,086	700	1,998	334	1,201	800	1,602	−0.745
Brideprice in others[3] (1,000 USh)	248	317.2	150	499.6	334	343.9	200	567.7	−0.601
Brideprice in total[3] (1,000 USh)	247	2,541	1,757	2,853	334	30,526	2,245	188,470	−2.713***

Notes. [1]This question was asked to those who were born in 1972 or after and had completed at least some grade of primary education. [2]This question was asked those who were born in 1972 or after, had completed at least some grade of primary education, and had attended a UPE-targeted school. [3]These variables are defined for only those who reported having agreed to paying some as bride price in the previous question.

Table 2: 1st stage estimation results.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Outcome variable.	Years of education								
Bandwidth.	11 years	10 years	9 years	8 years	7 years	6 years	5 years	4 years	3 years
Panel A. Paid sample.									
$\mathbf{I}\{x \geq 1981\}$	0.376	0.552	0.437	0.769	0.921	0.655	0.275	0.243	−0.593
	(0.568)	(0.611)	(0.648)	(0.706)	(0.728)	(0.783)	(0.911)	(1.210)	(1.626)
$\mathbf{I}\{x \geq 1981\} \cdot (x - 1981)$	0.406***	0.324***	0.379***	0.518***	0.547***	0.749***	0.502	0.845	−0.13
	(0.090)	(0.108)	(0.132)	(0.154)	(0.187)	(0.257)	(0.328)	(0.551)	(0.999)
F(instruments)	10.58	5.097	4.657	6.803	5.429	4.836	1.248	1.235	0.0738
Observations	700	619	544	477	432	369	301	247	193
R-squared	0.38	0.364	0.368	0.353	0.362	0.373	0.382	0.378	0.429
Panel B. Married sample.									
$\mathbf{I}\{x \geq 1981\}$	0.964**	0.889*	0.800	1.098*	1.043	0.878	1.450*	1.719**	1.661
	(0.482)	(0.517)	(0.539)	(0.608)	(0.636)	(0.710)	(0.775)	(0.870)	(1.216)
$\mathbf{I}\{x \geq 1981\} \cdot (x - 1981)$	0.247***	0.207**	0.164	0.278**	0.427***	0.469**	0.422	0.626	0.246
	(0.078)	(0.088)	(0.103)	(0.131)	(0.160)	(0.203)	(0.274)	(0.407)	(0.658)
F(instruments)	6.99	4.031	2.292	3.708	5.016	3.35	2.951	3.49	0.977
Observations	773	711	635	562	508	441	368	295	226
R-squared	0.258	0.257	0.265	0.272	0.292	0.308	0.358	0.448	0.494

Notes. Standard errors are calculated by the delta method. Statistical significance is denoted by *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$. All regressions include ethnicity dummies and dummies for region of residence at age 7 as covariates. Used orders of polynomials are $p_z = q_z = 1$. Here the "paid sample" refers to those females who are married and paid the brideprice upon the first marriage, whereas the "married sample" refers to all those females who have ever married regardless of brideprice payment.

Table 3: RKD estimation results for brideprice variables.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Bandwidth.	11 years	10 years	9 years	8 years	7 years	6 years	5 years	4 years	3 years
Panel A. 1 if paid brideprice.									
τ_{FRKD}	−0.093**	−0.121**	−0.168*	−0.082*	−0.030	−0.052	−0.042	−0.036	0.371
	(0.047)	(0.068)	(0.119)	(0.062)	(0.045)	(0.059)	(0.078)	(0.082)	(1.171)
Observations	760	698	623	550	498	433	362	289	220
Panel B. Brideprice in cash.									
τ_{FRKD}	62938.6*	40158.6	51420.8	12323.2*	3062.4*	5645.3*	7039.5	41829.7	850.4
	(47034.5)	(51100.0)	(76219.1)	(8411.1)	(2020.3)	(3597.9)	(8036.2)	(152000.0)	(2978.7)
Observations	499	456	406	362	333	288	242	201	158
Panel C. Brideprice in cattle.									
τ_{FRKD}	515.31	838.71	−540.94	−132.07	−43.86	46.47	533.97	2916.65	−1216.73
	(424.52)	(1188.6)	(1058.64)	(238.57)	(156.31)	(217.75)	(649.08)	(8587.82)	(4665.45)
Observations	498	455	405	361	332	287	241	200	158
Panel D. Brideprice in others.									
τ_{FRKD}	51.88	172.32	−135.53	−100.18	−60.84	−13.35	−13.69	90.52	387.07
	(101.45)	(293.86)	(320.99)	(98.07)	(60.09)	(82.29)	(176.23)	(766.31)	(1489.82)
Observations	499	456	406	362	333	288	242	201	158
Panel E. Brideprice in total (cash + cattle + others).									
τ_{FRKD}	61944.3*	39565.9	47102.1	11601.0*	2886.4*	5486.8*	7006.8	30453.8	137.2
	(45950.1)	(49021.7)	(65714.9)	(7906.0)	(1949.2)	(3424.7)	(7430.7)	(74726.2)	(3073.3)
Observations	495	452	403	359	330	285	239	198	156

Notes. Standard errors are calculated by the delta method. Statistical significance is denoted by *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$. All regressions include ethnicity dummies and dummies for region of residence at age 7 as covariates. The regressions for Panel A are run for those who have ever married and the variable is thus observed, while the regressions for the other panels are run for those who have ever married and had the brideprice payment agreed upon marriage and the brideprice amounts are thus observed.

Table 4: IV estimation results for brideprice variables.

Bandwidth.	(1) All	(2) 11 years	(3) 10 years	(4) 9 years	(5) 8 years	(6) 7 years	(7) 6 years	(8) 5 years	(9) 4 years	(10) 3 years
Panel A. 1 if paid brideprice.										
τ_{IV}	−0.108**	−0.104**	−0.132*	−0.185*	−0.108*	−0.079*	−0.138**	−0.177*	−0.213	−0.148
	(0.052)	(0.053)	(0.08)	(0.133)	(0.067)	(0.048)	(0.083)	(0.109)	(0.188)	(0.162)
Observations	874	760	698	623	550	498	433	362	289	220
Panel B. Brideprice in cash.										
τ_{IV}	70483.2**	64183.7*	19347.3	30537.3	12183.5	2968.9**	5892.0*	3894.9	-5362.3	-320.0
	(40756.9)	(41879.5)	(15427.1)	(29822.6)	(9560.9)	(1613.2)	(3887.4)	(4183.3)	(8049.4)	(698.6)
Observations	586	499	456	406	362	333	288	242	201	158
Panel C. Brideprice in cattle.										
τ_{IV}	296.66	336.31	170.19	−137.11	−64.52	−31.17	52.89	387.49	513.32	−472.43
	(339.81)	(304.86)	(335.69)	(390.19)	(160.27)	(113.80)	(154.86)	(442.34)	(728.37)	(802.13)
Observations	585	498	455	405	361	332	287	241	200	158
Panel D. Brideprice in others.										
τ_{IV}	0.440	−5.231	−38.62	−171.63	−131.16	−85.57	−34.98	122.72	334.19	237.56
	(109.89)	(88.25)	(149.51)	(243.58)	(108.74)	(66.68)	(82.44)	(157.98)	(447.93)	(307.46)
Observations	586	499	456	406	362	333	288	242	201	158
Panel E. Brideprice in total (cash + cattle + others).										
τ_{IV}	69024.9**	62916.1*	18419.2*	28251.5	11392.2	2766.7**	5719.3*	4780.5	−1993.2	−590.6
	(39421.0)	(40717.2)	(14201.9)	(26356.5)	(8875.6)	(1520.7)	(3686.9)	(4779.2)	(4016.6)	(1350.3)
Observations	582	495	452	403	359	330	285	239	198	156

Notes. Statistical significance is denoted by *** for p < 0.01, ** for p < 0.05, and * for p < 0.1. All regressions include ethnicity dummies and dummies for region of residence at age 7 as covariates. The regressions for Panel A are run for those who have ever married and the variable is thus observed, while the regressions for the other panels are run for those who have ever married and had the brideprice payment agreed upon marriage and the brideprice amounts are thus observed.

Table 5: RKD estimation results for marriage variables.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Bandwidth.	11 years	10 years	9 years	8 years	7 years	6 years	5 years	4 years	3 years
Panel A. 1 if married on love (paid sample).									
τ_{FRKD}	0.032*	0.036	0.103*	0.043	0.025	−0.011	−0.049	−0.073	−0.543
	(0.024)	(0.031)	(0.069)	(0.035)	(0.026)	(0.031)	(0.054)	(0.066)	(1.588)
Observations	767	705	631	558	505	440	367	294	225
Panel B. 1 if married within LC1 (paid sample).									
τ_{FRKD}	0.015	0.012	0.051	0.031	0.009	0.010	0.062	−0.012	0.375
	(0.039)	(0.053)	(0.089)	(0.058)	(0.043)	(0.049)	(0.077)	(0.071)	(1.148)
Observations	769	707	631	559	505	438	365	293	224
Panel C. Age at marriage.									
τ_{FRKD}	0.410	0.382	−1.411	−0.334	−0.258	−1.016	−0.999	−0.719	−7.661
	(0.502)	(0.687)	(2.169)	(0.782)	(0.486)	(0.835)	(1.275)	(2.271)	(38.488)
Observations	765	703	628	556	502	437	365	292	225
Panel D. Spousal age gap.									
τ_{FRKD}	−1.258**	−1.405**	−2.343*	−1.265*	−1.082**	−0.808*	−0.749	−1.620	−2.053
	(0.613)	(0.824)	(1.786)	(0.866)	(0.549)	(0.563)	(0.810)	(2.299)	(3.080)
Observations	348	328	293	264	241	212	181	152	111
Panel E. Current marital status: polygamous.									
τ_{FRKD}	−0.001	−0.003	−0.043	−0.035	−0.007	−0.001	−0.011	−0.026	0.012
	(0.020)	(0.031)	(0.044)	(0.038)	(0.033)	(0.035)	(0.053)	(0.048)	(0.198)
Observations	949	850	750	656	589	507	416	330	251
Panel F. Age at marriage of the first marriage partner.									
τ_{FRKD}	−0.480	−0.932	−2.985	−1.444*	−0.778*	−0.595	−0.456	−0.516	−2.260
	(0.573)	(0.832)	(2.451)	(1.076)	(0.559)	(0.580)	(0.775)	(1.607)	(3.463)
Observations	348	328	293	264	241	212	181	152	111

Notes. Standard errors are calculated by the delta method. Statistical significance is denoted by *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$. All regressions include ethnicity dummies and dummies for region of residence at age 7 as covariates. The regressions are run for those who have ever married and these variables are thus observed.

Table 6: IV estimation results for marriage variables.

Bandwidth.	(1) All	(2) 11 years	(3) 10 years	(4) 9 years	(5) 8 years	(6) 7 years	(7) 6 years	(8) 5 years	(9) 4 years	(10) 3 years
Panel A. 1 if married on love (paid sample).										
τ_{IV}	0.013	0.008	0.008	0.017	0.001	0.007	−0.030	−0.073*	−0.162	−0.142
	(0.024)	(0.023)	(0.031)	(0.045)	(0.027)	(0.020)	(0.033)	(0.052)	(0.146)	(0.172)
Observations	882	767	705	631	558	505	440	367	294	225
Panel B. 1 if married within LC1 (paid sample).										
τ_{IV}	0.057	0.037	0.046	0.090	0.077	0.035	0.011	0.136*	0.200	0.095
	(0.046)	(0.043)	(0.062)	(0.102)	(0.063)	(0.042)	(0.052)	(0.099)	(0.191)	(0.143)
Observations	885	769	707	631	559	505	438	365	293	224
Panel C. Age at marriage (paid sample).										
τ_{IV}	0.517*	0.186	0.028	−1.259	−0.370	−0.355	−0.915	−0.336	0.217	−0.185
	(0.348)	(0.429)	(0.575)	(1.393)	(0.631)	(0.453)	(0.743)	(0.660)	(0.825)	(1.003)
Observations	875	765	703	628	556	502	437	365	292	225
Panel D. Spousal age gap.										
τ_{IV}	−1.69**	−0.951**	−1.169*	−1.444	−1.005*	−1.038**	−0.796*	−0.658	−0.716	−2.165*
	(0.814)	(0.492)	(0.712)	(1.176)	(0.740)	(0.517)	(0.588)	(0.769)	(0.992)	(1.612)
Observations	362	348	328	293	264	241	212	181	152	111
Panel E. Current marital status: polygamous.										
τ_{IV}	−0.010	−0.003	−0.006	−0.041	−0.036	−0.012	−0.007	−0.018	−0.031	−0.111
	(0.018)	(0.021)	(0.031)	(0.042)	(0.032)	(0.029)	(0.034)	(0.046)	(0.048)	(0.149)
Observations	1072	949	850	750	656	589	507	416	330	251
Panel F. Age at marriage of the first marriage partner.										
τ_{IV}	−0.889	−0.384	−0.880	−2.343	−1.395*	−0.798*	−0.603	−0.277	0.281	−0.988
	(0.745)	(0.521)	(0.797)	(1.898)	(1.037)	(0.555)	(0.638)	(0.738)	(0.925)	(1.082)
Observations	362	348	328	293	264	241	212	181	152	111

Notes. Statistical significance is denoted by *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$. All regressions include ethnicity dummies and dummies for region of residence at age 7 as covariates. The regressions are run for those who have ever married and these variables are thus observed.

Table 7: RKD estimation results for economic variables.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Bandwidth.	11 years	10 years	9 years	8 years	7 years	6 years	5 years	4 years	3 years
Panel A. 1 if having non-agricultural job.									
τ_{FRKD}	0.004	0.045	0.034	0.043*	0.017	0.023	0.058	0.004	−0.528
	(0.024)	(0.037)	(0.037)	(0.032)	(0.036)	(0.033)	(0.058)	(0.051)	(3.388)
Observations	681	602	528	463	419	358	292	238	185
Panel B. Hours spent for domestic work.									
τ_{FRKD}	−0.359**	−0.591**	−0.668**	−0.410**	−0.517**	−0.495**	−0.666*	−0.218	1.437
	(0.157)	(0.315)	(0.359)	(0.212)	(0.238)	(0.222)	(0.407)	(0.341)	(7.586)
Observations	619	559	493	437	399	342	281	228	178
Panel C. Hours spent for non-agricultural work.									
τ_{FRKD}	−0.142	−0.191	−0.447	−0.218	−0.202	0.076	0.784*	0.331	−3.190
	(0.165)	(0.279)	(0.354)	(0.227)	(0.242)	(0.209)	(0.49)	(0.433)	(16.065)
Observations	619	559	493	437	399	342	281	228	178

Notes. Standard errors are calculated by the delta method. Statistical significance is denoted by *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$. All regressions include ethnicity dummies and dummies for region of residence at age 7 as covariates. The regressions are run for those who have ever married and these variables are thus observed.

Table 8: IV estimation results for economic variables.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Bandwidth.	All	11 years	10 years	9 years	8 years	7 years	6 years	5 years	4 years	3 years
Panel A. 1 if having non-agricultural job.										
τ_{IV}	0.005	0.011	0.043	0.031	0.024	0.020	0.041	0.060	0.064	−0.050
	(0.023)	(0.025)	(0.038)	(0.043)	(0.039)	(0.038)	(0.044)	(0.072)	(0.066)	(0.121)
Observations	778	681	602	528	463	419	358	292	238	185
Panel B. Hours spent for domestic work.										
τ_{IV}	−0.535***	−0.450**	−0.684**	−0.919*	−0.549**	−0.551**	−0.573*	−0.395	0.184	0.412
	(0.206)	(0.200)	(0.367)	(0.617)	(0.326)	(0.274)	(0.359)	(0.493)	(0.440)	(1.401)
Observations	715	619	559	493	437	399	342	281	228	178
Panel C. Hours spent for non-agricultural work.										
τ_{IV}	−0.045	−0.046	0.021	−0.243	−0.053	−0.087	−0.177	0.503	0.444	−0.444
	(0.199)	(0.205)	(0.307)	(0.435)	(0.286)	(0.271)	(0.334)	(0.567)	(0.536)	(1.693)
Observations	715	619	559	493	437	399	342	281	228	178

Notes. Statistical significance is denoted by *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$. All regressions include ethnicity dummies and dummies for region of residence at age 7 as covariates. The regressions are run for those who have ever married and these variables are thus observed.

A Appendix tables.

Table A.1: Modal choice of payment.

Bandwidth.	(1) All	(2) 11 years	(3) 10 years	(4) 9 years	(5) 8 years	(6) 7 years	(7) 6 years	(8) 5 years	(9) 4 years	(10) 3 years
Panel A. 1 if paid brideprice in cash.										
τ_{FRKD}	–	−0.08*	−0.082	−0.132	−0.052	−0.021	−0.068	−0.027	−0.046	0.317
	–	(0.049)	(0.065)	(0.111)	(0.062)	(0.048)	(0.065)	(0.081)	(0.089)	(1.041)
τ_{IV}	−0.10**	−0.081*	−0.080	−0.117	−0.075	−0.046	−0.103*	−0.099	−0.098	−0.057
	(0.056)	(0.053)	(0.073)	(0.115)	(0.065)	(0.047)	(0.076)	(0.091)	(0.136)	(0.123)
Observations	873	760	698	623	550	498	433	362	289	220
Panel B. 1 if paid brideprice in cattle.										
τ_{FRKD}	–	−0.079**	−0.111**	−0.124	−0.126**	−0.047	−0.064	0.018	0.029	0.749
	–	(0.046)	(0.067)	(0.104)	(0.073)	(0.046)	(0.059)	(0.075)	(0.083)	(2.235)
τ_{IV}	−0.063*	−0.039	−0.049	−0.039	−0.076	−0.067*	−0.106*	−0.081	−0.019	−0.022
	(0.048)	(0.046)	(0.064)	(0.088)	(0.062)	(0.048)	(0.073)	(0.081)	(0.106)	(0.109)
Observations	872	759	697	622	549	497	432	361	288	220
Panel C. 1 if paid brideprice in others.										
τ_{FRKD}	–	−0.111**	−0.131**	−0.188*	−0.123*	−0.039	−0.041	−0.032	−0.091	0.102
	–	(0.054)	(0.076)	(0.135)	(0.077)	(0.05)	(0.064)	(0.085)	(0.098)	(0.549)
τ_{IV}	−0.12**	−0.11**	−0.126*	−0.186*	−0.143**	−0.082*	−0.113*	−0.175*	−0.182	−0.128
	(0.058)	(0.057)	(0.083)	(0.14)	(0.08)	(0.052)	(0.077)	(0.111)	(0.169)	(0.15)
Observations	873	760	698	623	550	498	433	362	289	220

Notes. Standard errors for RKD estimation are calculated by the delta method. Statistical significance is denoted by *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$. All regressions include ethnicity dummies and dummies for region of residence at age 7 as covariates.

Table A.2: 1st stage estimation results of partner's years of education.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Outcome variable.	Partner's years of education.								
Bandwidth.	11 years	10 years	9 years	8 years	7 years	6 years	5 years	4 years	3 years
$\mathbf{I}\{x^{\text{partner}} \geq 1981\}$	1.382	1.336	0.928	1.121	0.617	0.782	−0.489	−0.576	1.243
	(0.924)	(0.933)	(0.970)	(1.057)	(1.136)	(1.229)	(1.419)	(1.510)	(1.807)
$\mathbf{I}\{x^{\text{partner}} \geq 1981\} \cdot (x^{\text{partner}} - 1981)$	0.0851	0.0450	0.0348	0.323	0.296	0.505	0.465	−0.531	−0.700
	(0.148)	(0.152)	(0.169)	(0.240)	(0.276)	(0.352)	(0.472)	(0.635)	(1.111)
Observations	269	240	224	189	172	145	118	90	67
R-squared	0.091	0.125	0.138	0.192	0.195	0.202	0.291	0.337	0.376
F(instruments)	1.460	1.136	0.503	1.783	0.870	1.331	0.510	0.377	0.427

Notes. Standard errors are calculated by the delta method. Statistical significance is denoted by *** for p < 0.01, ** for p < 0.05, and * for p < 0.1. All regressions include ethnicity dummies and dummies for region of residence at age 7 as covariates.

Table A.3: Cash and total brideprice with dummies for marriage year and region of residence.

Bandwidth.	(1) All	(2) 11 years	(3) 10 years	(4) 9 years	(5) 8 years	(6) 7 years	(7) 6 years	(8) 5 years	(9) 4 years	(10) 3 years
Panel A. Brideprice in cash.										
τ_{FRKD}	–	8586.5	-2303.0	1019.7	1980.6	1466.7*	1304.6*	132.7	422.4	–
	–	(48151.3)	(22437.4)	(9933.7)	(1931.2)	(1058.4)	(897.6)	(399.3)	(479.7)	–
Observations	–	496	453	404	360	331	287	241	200	–
τ_{IV}	24945.8	14964.1	1135.2	1638.4	2010.8**	1807.8*	1378.9*	-167.0	257.5	–
	(21010.9)	(12522.3)	(2285.3)	(2451.5)	(1196.7)	(1095.4)	(955.6)	(374.1)	(702.2)	–
Observations	582	496	453	404	360	331	287	241	200	–
Panel B. Brideprice in total (cash + cattle + others).										
τ_{FRKD}	–	8173.5	-415.8	796.4	1573.8	1231.2	1248.7*	439.7	634.9	–
	–	(44293.3)	(19572.0)	(9005.6)	(1638.9)	(965.1)	(852.9)	(466.2)	(574.9)	–
Observations	–	492	449	401	357	328	284	238	197	–
τ_{IV}	23759.3	14095.9	1239.4	1678.5	1936.8**	1601.1*	1337.5*	3.5	442.0	–
	(19755.5)	(11720.1)	(2193.4)	(2364.9)	(1113.1)	(988.3)	(910.5)	(400.2)	(687.5)	–
Observations	578	492	449	401	357	328	284	238	197	–

Notes. Standard errors for RKD estimation are calculated by the delta method. Statistical significance is denoted by *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$. All regressions include ethnicity dummies and dummies for the interaction of marriage year and region of residence at age 7 as covariates.

Table A.4: Simple regression for assortative matching.

Variables	(1) Partner's years of education
$\mathbf{I}\{x \geq 1981\}$	−0.296
	(0.490)
Own years of education	0.420***
	(0.057)
Own years of education × $\mathbf{I}\{x \geq 1981\}$	0.0496
	(0.082)
Constant	6.900***
	(1.763)
Observations	689
R-squared	0.208

Notes. Robust standard errors are reported in parentheses. Statistical significance is denoted by *** for p < 0.01, ** for p < 0.05, and * for p < 0.1. All regressions include ethnicity dummies and dummies for region of residence at age 7 as covariates.