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Abstract

Early pregnancy poses serious medical risk and economic burden to mother and neonatal children. While Economics literature generally explains negative relationship between female schooling and early fertility, it remains unclear whether this reflects a causal relationship. To fill in such a gap in literature, this paper examines the impact of female education on adolescent fertility, health investment behavior and the health status of their children in Uganda, focusing on the fully treated cohorts whose fees were abolished by Universal Primary Education policy (UPE) just before they entered schools. Education is instrumented by the interaction between across-cohorts differences in exposure to UPE and the differences in its effective benefits across districts with varying pre-program rates of completing primary education. We show that attending an additional year of schooling reduces the probability of marriage and that of giving birth before age 18 by 7.0-7.2 percentage points. Among those who become mothers, educated women use maternal care and infant immunization more often, and had lower probability that their child dies before 12 months after the birth. These results indicate that promoting the access to primary education among girls is an effective program to reduce adolescent pregnancy. It also shows the important role of maternal education in breaking the cycle of intergenerational transmission of the poor health in least developing countries by reducing child mortality. This in turn underscores the importance of considering the widespread benefits of female education in shaping the policy and institution influencing educational attainment.

JEL Classification Codes: J13; J12; D10; O10

Keywords: Teenage Pregnancy, Girls' Education, Uganda, Fertility, Universal Primary Education, Child health

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1 Introduction

Education for girls has long been considered to be one of the most important development goals for developing countries. It was advocated at the World Conference on Education For All in 1990, which led to the objective of universal primary education as one of the 2000 Millennium Development Goals and 2016 Sustainable Development Goals. In this context, many developing countries have abolished school fees to promote schooling under the name of the Universal (or Free) Primary Education (UPE) policy (UNICEF, 2008, Avenstrup, et al., 2004). The literature has also found female education to be associated negatively with fertility and positively with the health status of children (e.g, Caldwell, 1979; Cochrane, 1979; Shultz, 1997; Thomas and Strauss, 1991), which has been used as the rationale to invest in female education (Shultz, 2002). Female education can also reduce adolescent marriage and pregnancy, which poses a serious medical risk for mothers and children,[1] and is often accompanied with domestic violence (Jensen and Thornton, 2003). Despite the links to these abominable outcomes, there is still a significant number of adolescent marriage and pregnancies in developing countries.[2] Recently, several studies which use experiments or natural experiments have confirmed the causal impact of female education on delaying the first pregnancy. However, as discussed later, evidence is more mixed for the impact on child health, and there is only sporadic empirical evidence for the mechanisms through which education affect fertility and child health.

This paper provides a new set of evidence on the impact of female education on these outcomes,

[1] For instance, Finlay, et al. (2011) shows that the risks of infant mortality, as well as other risks such as stunting and diarrhea, are significantly higher for first-born children in low- and middle-income countries if mothers are aged 17 and below, compared to those aged 27-29, after controlling for socio-economic characteristics. Using the U.S. data, Myrskylä and Fenelon (2012) also find the health of offspring adults is significantly worse among those born when mothers were aged 24 or below, controlling for similar socio-economic characteristics. Raj and Boehmer (2013) show the association between adolescent pregnancy and HIV prevalence as well as maternal and infant health for 97 countries.

[2] For example, in many African and South Asian countries, close to a half of women marry by the age of 18 (UNICEF, 2005). Regarding pregnancies, the United Nations Population Fund (2013) estimates that 19 out of 100 women give a live birth before the age of 18 in developing countries, whereas only six do in the U.S. The highest rates of adolescent pregnancy are found in sub-Saharan Africa, which range between 25 and 28 out of 100.

and comprehensively explores the pathways through which female education affects them. In order to address the endogeneity of educational attainment, we utilize the exogenous variation caused by UPE in Uganda, which abolished primary education fees in 1997. In particular, we focus on the cohorts which were fully benefited from the policy – i.e., those who were not yet aged for primary school when the policy started. The fully treated cohorts are compared with cohorts not exposed to the policy, and we also take advantage of the fact that districts which historically had low primary school completion rates had more to gain from the policy. This methodology in turn enables us to provide the new evidence for the effectiveness of the UPE policy (hereafter referred as UPE), which have been introduced in many countries in the recent decades.

Our results show that an additional year of schooling effectively reduces the probability of giving a live birth before the age of 18 by seven percentage points, but it does not delay the onset of sexual activities or increase abortions. Weak evidence is found for increased use of modern contraceptive methods. Among those who gave birth by the age of 19 or 20, educated mothers invest more in the health of their children by delivering babies at a formal facility assisted by a medical professional and having the child vaccinated. These results suggest that female education not only decreases adolescent pregnancy but also improves the investment in the health of her children. As the pathways through which education makes these changes, we find that educated women are more likely to be literate and prefer to have fewer children. They also exhibit better knowledge about reproductive issues. Weak evidence is found for an increase in the probability of working in the non-agricultural sector. On the other hand, no evidence is found for assortative mating, and evidence for improved bargaining power is mixed.

The results for adolescent pregnancy are consistent with recent quasi-experimental studies, which use the reduction in the costs of school attendance through school construction and fee abolition, as well

as changes in compulsory education as the source of identification variation.[3] Also, randomized control trials providing educational subsidies show that a reduction in the schooling costs decreases school drop-out and early fertility and marriage (Duflo, et al., 2006; Baird, et al., 2010; Duflo, et al, 2015).

The positive education effects on child health are also consistent with recent previous studies for developing countries. An increase in compulsory schooling duration from 5 to 8 years is utilized by Dincer, et al. (2014) and Günes (2015) to show that female education reduced fertility and increased early prenatal visits in Turkey.[4] Large-scale primary school construction programs were used by Breierova and Duflo (2004) to reveal that maternal education reduced child mortality in Indonesia. Similar effects are found in Taiwan for infant mortality using junior high school construction program (Chou et al., 2010). However, evidence is somewhat mixed for developed countries. For instance, using changes in the proximity to colleges, Currie and Morretti (2003) show that female education improves infant health in the U.S. through increased use of prenatal care, reduced smoking, better marital status and education of the spouse. On the other hand, McCrary and Royer (2006) find no impact on observable inputs to infant health and only small effects on infant health. Lindeboom, et al. (2009) also suggests that an increase in the school leaving age in the U.K. had little effect on the health of their offspring, though schooling reduced financial difficulties. As discussed in McCrary and Royer (2006), these differences might be related to the nature of compliers. When the instruments are related to the improvement of the supply of educational facilities, the results seem to indicate the positive impact of female education on child health.

[3] For example, the negative education effect on early/first pregnancy is found in many developing countries (Breierova and Duflo, 2004; Osili and Long, 2008; Berthelon and Kruger, 2011; Chicoine, 2012; Keats, 2014; Ozier, 2015 and Tequame and Tirivayi, 2015) as well as developed countries (Black et al., 2008; Gronvist and Hall, 2013; Cygam-Rehm, 2013; Clark et al., 2014). The only exception is McCrary and Royer (2011), which did not find significant effects of education on the complete fertility and timing of the first birth in the U.S., though they discuss the possible negative selection of the compliers to which their estimates are applicable.

[4] The two studies for Turkey have use different instruments to identify the impact of the same reform (the number of classrooms and the number of teachers), and look at different sets of outcomes. Dincer (2014) reports an increase in contraceptive use and improvement in knowledge of the ovulation cycle, and Gunes (2015) finds improvements in infant and child health.

Compared to our results for the mechanism through which female education reduces early pregnancy and improves child health, the literature provides only sporadic evidence on it. This is unfortunate as various pathways have been theoretically put forward to explain the relationships. Regarding the negative effect on fertility, first, it is suggested that higher educational attainment increases human capital and thus raises the opportunity cost of childbearing and childrearing, which in turn decreases early pregnancy and increases labor force participation (Becker 1981). However, relatively few studies have examined the impact on labor force participation together with that on fertility or child health, and among those which did, evidence is mixed.[5] Second, schooling might merely create incarceration effects. That is, girls have less time, opportunity and desire to commit risky reproductive health behavior while attending school with adult supervision. However, the empirical evidence for the incarceration effect is limited.[6] Third, education may change fertility preference. Since educated women often face the trade-off between the quantity and quality of children (Becker and Lewis, 1974), improvement in educational attainment might lead women to want to have fewer children. Fourth, educated girls may become more knowledgeable about the access to and importance of contraceptives and preventative health care. While this can be considered to be part of human capital improvement, specific knowledge on medical and reproductive health issues can increase health investment demand separately from any potential income or substitution effect through the labor market. Available evidence supports the pathways through knowledge improvement and the decrease in fertility preference (Keats, 2014; Lavy and Zablotsky, 2011; Mocan and Connonier, 2012), which is consistent with our results.

[5] Among available evidence, education is found to increase the labor force participation rate in Ecuador (De Paoli, 2009) and the share of workers paid in cash in Uganda (Keats, 2014), while no impact is found in Israel (Lavy and Zablotsky, 2011) and impact is limited to men in Kenya (Ozier, 2015). Cygam-Rehm and Maeder (2013) also finds that education improves occupation type and job prestige in Germany.

[6] Only Berthelone and Kruger (2008) provides the direct evidence for the incarceration effect by showing the decline in adolescent pregnancies after the school day was lengthened from half to full day shifts in Chile. Several studies indirectly test the incarceration effect (as opposed to the human capital hypothesis) by looking at the effect of education on fertility after completing school. Some suggest that education has lasting impact on fertility (Black, et al., 2008), while others find the negative effect is limited to the time when women are kept in school (Grönqvist and Hall, 2011).

Fifth, on the mating front, educated women are likely to marry educated men, who might also want fewer children (Behrman and Rosenzweig, 2002). Among studies which examined this assortative mating channel, some have found that partners are indeed more educated (Breierova and Duflo, 2004; Clark et al., 2014; Fort, et al., 2016; Lavy and Zablotzky, 2011, Tequame and Tirivayi, 2015), but others do not (Cygam-Rehm and Maeder, 2013; Keats, 2014). Finally, once women are partnered, the bargaining model of a household suggests that decision making including fertility and health investment hinges on the relative bargaining power of partners. Available evidence on this is mixed, with more liberal attitudes towards husbands found in Sierra Leone (Mocan and Connonier, 2012), while no change is found in Uganda (Keats, 2014).

Similar pathways are considered for the mechanism through which female education improves health investment and health status. First, education can lead to better jobs and income, which increases the amount of resources available for health care and inputs. Two other factors can also augment the income effect: the mating in terms of educational attainment and lowered fertility or delay in the timing of the first pregnancy. Second, education can also increase the ability to acquire and utilize information as well as specific reproductive/health knowledge, which might promote better health investment (Thomas, et al., 1991; Glewwe, 1999). Our results suggest that better reproductive knowledge helps women to invest in their children, and delayed pregnancy is likely to increase the amount of resources available per child to the extent income increases with age.

Our results also reveal that fee abolition from grade one, as opposed to after paying for some years, can be crucial to have the poorest girls to attend school. Keats (2014) examines the impact of UPE in Uganda on similar outcomes, using the regression discontinuity design.[7] That is, he compares the

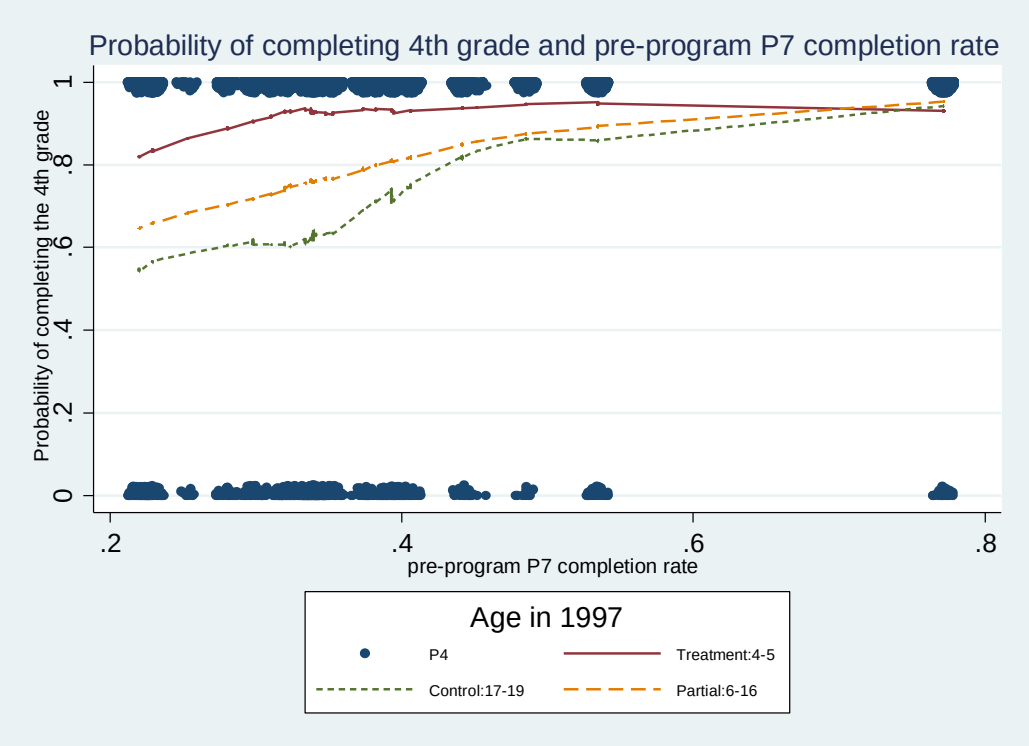
[7] Another study using a similar method as Keats (2014) finds UPE decreased the likelihood of being infected with HIV (Behrman, 2014), which is consistent with both our study and Keats (2014), indicating the improvement in health-related knowledge.

children just above and below the age of 14 when the policy started. However, since some girls in his treatment group had already paid fees up to a certain grade or dropped out of school when the policy began, the estimate is likely to serve as a lower bound, and it is still unknown how large the effect of UPE is when children are fully treated. This is important because girls from the poorest families in the "partially" treated were unlikely to benefit from it if they had already decided not to attend school at all or drop out of it permanently before the policy began. In our study, we focus on the cohorts who were not yet aged for primary school at the start of the policy (aged 4 or 5), who were likely to have benefited from fee abolition from grade one. Hence, while Keats (2014) provides the short-term impart of the policy, we provide the evidence on the full impact of the policy, which is likely to apply to the future cohorts.[8]

In addition, we reveal the policy reduced geographic inequality in educational attainment, by disproportionately benefiting historically disadvantaged areas. Figure 1 indicates the relationship between the historical primary education completion rate for older cohorts of women (aged 27-31 in 1997) and the proportion of girls completing Grade 4 for the fully treated (age 4-5 in 1997) and controlled cohorts (age 17-19). It exhibits that relatively disadvantaged areas became to benefit from UPE for the fully treated cohorts. That is, between the fully treated and partially treated cohorts, a relatively large improvement in educational attainment occurred in areas with the historical completion rate of 0.3-0.35, which contain about a half of the sample. We also find the negative education impact on infant mortality, which is not found among the partially treated cohorts (Keats, 2014). This underscores the importance of financial support from grade one, rather than a higher grade, particularly for girls from very poor families.

[8] For this reason, we mainly use the most recent wave of the 2011 Demographic Health Survey, while Keats (2014) uses the previous waves.

Figure 1: The correlation between the historical primary completion rate and the current probability of Grade 4 completion



Data sources: 2001 and 2011 UDHS.
Notes: The lowess estimates are shown for the bivariate relationship between individual years of education and the district-level pre-program primary completion rate for the control (aged 17-19 in 1997), partially treated (age 6-16) and fully treated (age 4-5) cohorts. The estimates are trimmed for the districts with the largest and smallest completion rates. Age at the time of interview is 19-26 and 18-19 for the controlled and fully treated cohorts in the UDHS 2001 and 2011, respectively. For the partially treated cohorts, we use data when they were 18-20 years old from UDHS 2001 for those aged 14-16 in 1997, while we use data when they were 20-27 from UDHS 2011 for those aged 6-13 in 1997, to balance the age at interview across the three groups. The points for the scattered plots are perturbed to describe the density of observations.

The remainder of this paper is organized as follow. Section 2 provides the background of the educational system and UPE in Uganda, followed by the conceptual framework for the household decision on educational investment. Section 3 describes the data and identification strategy. In Section 4, we present our empirical results. Finally, Section 5 provides concluding remarks.

2. Policy Background

Uganda is one of the low income countries with per capita GDP of about $434 in 2014. The

country however is one of the fastest growing economies in Africa. Its average growth rate between 2000 and 2012 was 7.0 percent.[9] Together with this economic growth, the country's education system has been experiencing a rapid change. Before UPE started in 1997, public primary schools had financially relied on private resources. Parents contributed to the majority of school inputs such as tuitions, Parents Teacher Association (PTA) fees, and uniform costs.[10] Although the idea of free primary education has been discussed since the 1970s and clearly recommended in the 1989 Education Policy Review Commission report, political unrest and lack of resources prevented its implementation (Avenstrup, et al., 2004; MOES, 1999). Also, there was no well-functioning education financing system even in the mid-1990s. The transfers from the central government to public primary schools were mostly diverted by the local government (Reinikka and Svensson, 2004). As a result, 83 percent of the primary education cost, which was 18.6 percent of GDP per capita per pupil, was privately funded (Mehrotra and Delamonica, 1998). This financial burden made it difficult for some, particularly poor, parents to afford school costs (Deininger, 2003). Reflecting this, delayed school entry and grade repetition were not uncommon. Children mostly started schooling by the age of seven or eight, though the national law stipulates that children start primary education at age six.[11] The age of individuals attending primary school ranged between 6 and 22. The enrolment rate among children aged 6-13 was 85 percent (UDHS, 1996).

The issue of free primary education received a high-level political support when the then candidate for the first president, Museveni, made it one of his platform issues during the campaign for the election in May 1996. After being elected, fee abolition was written into a government manifesto in

[9] Based on the World Bank Development Indicators (http://databank.worldbank.org/data/reports.aspx?source=world-development-indicators) and the GDP per capita is in terms of the constant 2005 U.S. dollars.

[10] PTA fees were collected to complement the low salaries of teachers (ODI, 2006).

[11] According to the Uganda National Household Survey (UNHS) 1996, about 17 percent of children aged seven and 11 percent of those aged eight were not in school. The formal education system consists of seven years of primary education, four years of junior high school education, two years of senior high school education, and three years of tertiary education.

December 1996, and it was announced that implementation would begin in January 1997 (Avenstrup, et al., 2004; MOES, 1999). The government abolished tuition and PTA fees in public primary schools, which amounted to 62 percent of the average annual expenditure per child at public primary school before UPE.[12] To supplement education costs, the government provided capitation grants of Ush5,000 per pupil per annum for grade one through three, and Ush8,100 for grade four through seven. In addition, textbooks and construction materials were provided and new teachers were trained (MOES, 1999). Only four children per household were eligible for this cost reduction initially, but the eligibility was expanded to all the children in 2003.[13] This nation-wide education subsidy effectively boosted the primary-level gross enrollment ratio from 70 percent in 1996 to 120 percent in 2009 (UNESCO, 2014), and this increase was particularly large among girls (Deininger 2003; Nishimura et al. 2008).

However, the national-level advancements greatly mask the regional variation in the growth in educational attainment. UPE is likely to have increased the number of girls completing primary education in areas with lower completion rates prior to UPE, as those areas had many more girls to benefit from it. We use this regional variation in the expected level of UPE benefit, together with the across-cohort difference in the exposure to UPE, in order to study the effect of female educational attainment on early fertility, health investment and child health.

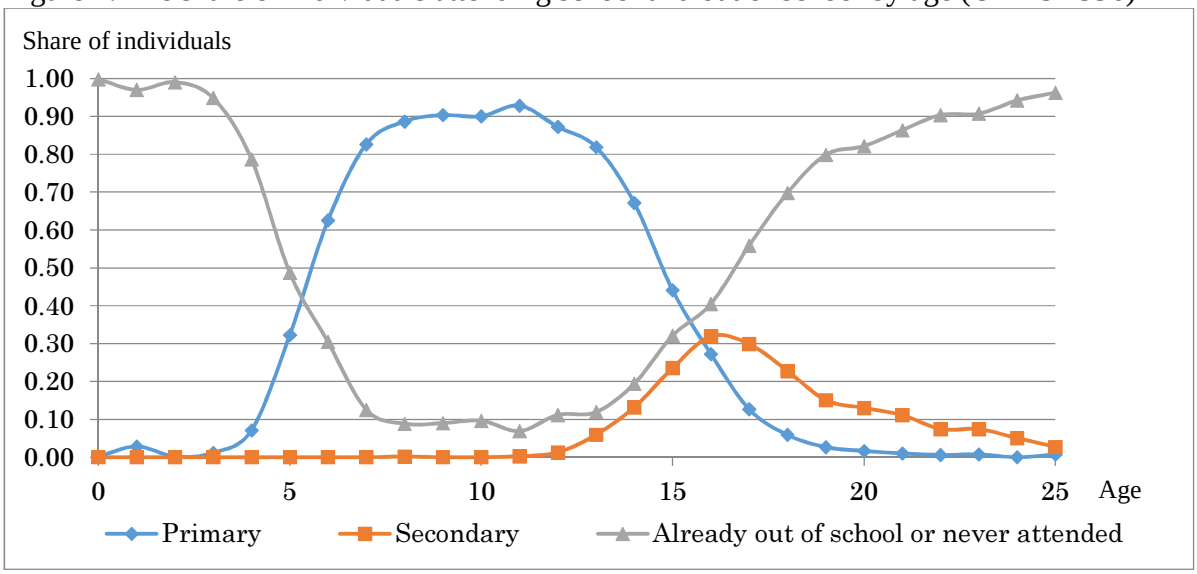
Also, the impact for the fully treated cohorts can be substantially larger than that for the partially treated cohorts from several reasons. First, a large number of children in the partially treated cohorts had already paid fees before 1997. Over 80 percent of girls aged 7-13 in 1996 were attending primary school, paying the annual tuition and PTA fees (Figure 2). Particularly, children who were in higher grades had a shorter period of time to benefit from the UPE. On the other hand, most of children aged 4-5 had not yet

[12] Other expenses include boarding fees, uniforms, books, transportation, food and coaching. Based on the Uganda Integrated Household Survey 1991.
[13] Even in the beginning, the registration limit of four children per family turned out to be problematic because the definition of a family was unclear (ODI, 2006).

started school. Second, fee abolition could have had positive impact on study environments if parents became able to afford educational materials due to a potential income effect. Improvement in the quality of education might have in turn increased an incentive to continue studying. The fully treated children are likely to receive such income effects from grade one. Third, UPE is found to enhance entry to primary education before age nine among those who were 8 years of age or younger when the policy started, and particularly strongly for those who were aged 4 or 5 (Grogen, 2009), which is our fully treated group. Since delayed entry is associated with a lower probability of primary completion (Grogen, 2009), the strong effect on on-time entry for the fully treated cohorts can help them to finish higher grades.[14] Fourth, about 10 percent of the partially treated cohorts never attended school or already dropped out of school at a primary-school age (Figure 3). Among the fully treated cohorts, children who could have become like these children might have attended school, or attended it longer thanks to UPE. In fact, the share of women who *never attended* school dropped from 13 percentage points for the controlled cohorts to 6 percentage points for the fully treated cohorts, though the drop between controlled and the partially controlled cohorts was more limited (Figure 3). This suggests that, only when fees are abolished from grade one, UPE induced girls who are least likely to enter school to do so, thereby making an important step to achieve truly universal primary education.

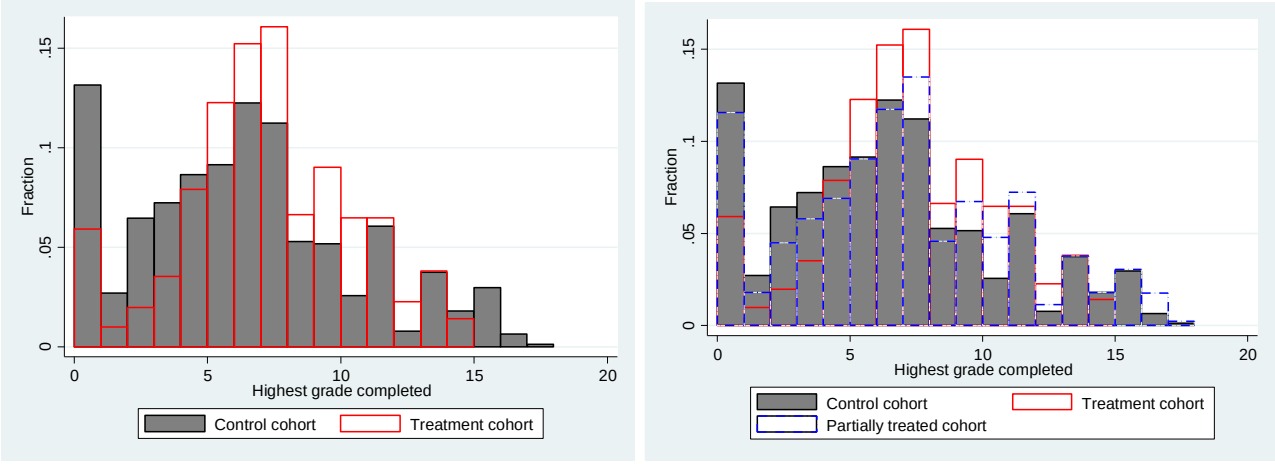
[14] This is particularly the case if girls are pressured to stop schooling due to their age of menarche (Fields and Ambrus, 2008). According to the authors' fieldwork, several district officials and village chairmen stated that girls from Muslim families receive this kind of pressure, as some of them are affected by the traditional view that girls are supposed to marry and leave home once they experience menarche.

Figure 2: The share of individuals attending school and out of school by age (UNHS 1996)



Source: Uganda National Household Survey 1996.

Figure 3. The distribution of the completed years of education for the control and treatment groups



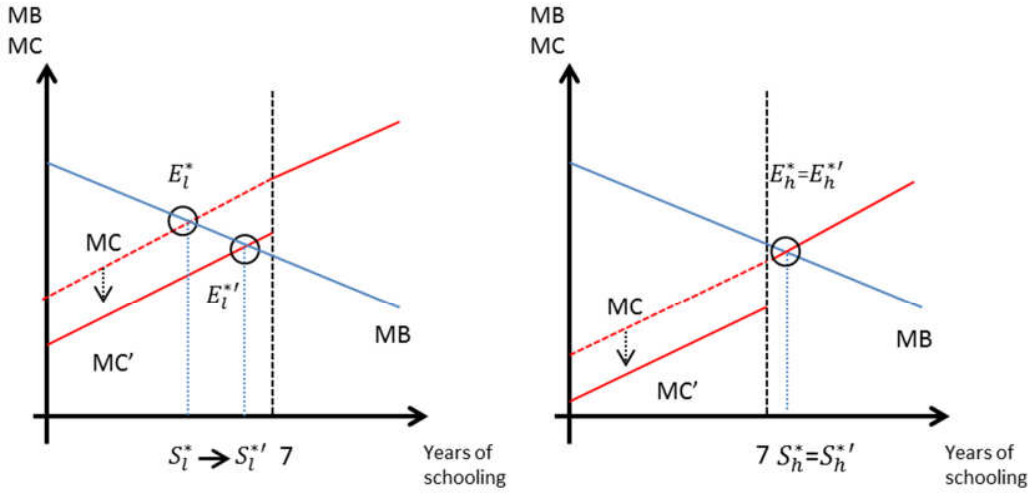
Note: Authors' calculation using UDHS 2001 and 2011. The height of bar shows the share of women completing the respective grade shown on the horizontal axis.
Primary education consists of grade 1-7, lower secondary (Ordinary level) education consists of grade 8-11, higher secondary (Advanced level) education consists of grade 12-13, and grade 14 and higher indicates tertiary education.

3. Conceptual Framework

How has UPE changed schooling decisions? In this section, we discuss a conceptual framework describing how parents decide the optimal level of education for their child. Using this framework, we

further discuss how UPE differently affects the optimal choice across districts with varying initial levels of education. Suppose that parents try to maximize the net benefit from sending their child to school. The optimal level of schooling is achieved by equating the marginal benefit and marginal cost of attending another year of schooling (MB and MC in Figure 4, respectively).[15] We assume that marginal benefit is decreasing in schooling years as basic skills such as numeracy and literacy tend to yield a higher rate of return (Psacharopoulos, 1981, 1994, 2004).[16] That is, $MB_i = F(S_i),\ where\ \frac{\partial F}{\partial S} < 0$ and S represents the number of years of schooling. On the other hand, we assume that marginal cost curve is increasing in schooling years. That is, $MC_i = G(S_i), where\ \frac{\partial G}{\partial S} > 0$. As a girl attends school for a long time, she becomes more able to work and her opportunity cost of attending school increases. The optimal level of schooling, S^*, is chosen so that $F(S^*) = G(S^*)$.

Figure 4. Household Optimization over a Child's Educational Attainment



A: Low pre-program educational attainment (High intensity)

B: High pre-program educational attainment (Low intensity)

Figure 4 illustrates this optimization process in two cases with different initial conditions. Without

[15] For simplicity, assume that parents make educational investment decision of each child independently. We also assume that financial/credit constraint does not bind investment decision making at primary education level.

[16] Our discussion will hold even if the marginal benefit curve is constant over educational level.

UPE, girls in Panel A face a higher marginal cost schedule, which is depicted by a dotted line; MC. For example, they might live in a place that is far from a primary school. These girls may also face a lower marginal benefit schedule, which is depicted by a solid line, MB, because non-farm job opportunities are limited in the surrounding areas. In Panel A, at the intersection between MC and MB, point E_l^* indicates the equilibrium level of educational attainment without UPE.

The optimal level of schooling is chosen in a similar manner in Panel B, but the level of optimal schooling in this case, E_h^*, is higher than E_l^* because the marginal cost is lower or marginal benefit is higher than the case in Panel A. In particular, girls in Panel B complete primary education even in the absence of UPE. This situation resembles relatively developed areas in Uganda before UPE.

UPE is likely to shift the marginal cost line downward from MC to MC' in Figure 4 only between the first and seventh grade since it abolished school fees only for primary education. As a result, the equilibrium point shifts from E_l^* to $E_l^{*\prime}$ in Panel A, increasing the optimal level of schooling from S_l^* to $S_l^{*\prime}$. In contrast, a similar downward shift of the marginal cost curve hardly affects the optimal level of schooling in Panel B ($S_h^* = S_h^{*\prime}$). Therefore, these Panels indicate that the areas which initially had a lower rate of completing primary education are likely to demonstrate a greater catch-up in educational attainment. In other words, the intensity of UPE benefit, or the scope for improvement, is larger in districts whose conditions resemble Panel A. Those districts are likely to exhibit a lower counter-factual level of schooling in the absence of UPE, which is equivalent to E_l^* in Panel A, compared to E_h^* in Panel B. We assume that this variation in the counterfactual level of schooling can be approximated by the share of older women born in each district who completed primary education. Intuitively, if few older women completed primary education prior to UPE in a district, young women in the district would have also attained a low level of education had there not been UPE. We use this regional variation in *pre-program completion rates*, together with across-cohort difference in the exposure to the policy, in order to identify the effect of female

education on their adolescent pregnancy, health investment and child health.

4. Data and Identification Strategy

4.1 Data Sources and Treatment Cohorts

Our analysis mainly draws on the Uganda Demographic and Health Survey (UDHS) 2001 and 2011. The UDHS is nationally representative, repeated cross section data containing rich reproductive, health and demographic information of women aged 15 to 49. This includes the history of pregnancy, health investment and child health. These pieces of information are extracted for women who were young enough to be fully affected, i.e., in pre-school age (four and five), and girls who were too old (aged between 17 and 19) in 1997 when UPE started. Out of 15,921 observations in UDHS 2001 and 2011, 1476 women fall in one of the treated or control cohorts and live in one of the 35 districts existing in 1991 (702 treated girls born in 1992 and 1993 from the 2011 UDHS and 774 control girls born in 1978 to 1980 from the 2001 UDHS). When they were surveyed, the treated girls were aged 18 and 19, while the control girls were aged 21-23.[17] One might be concerned the positive effect of UPE is spuriously produced because the differential censoring issue is likely to be more severe in the districts with higher pre-program completion rate, where many women tend to attain more education. To ease this concern, we tested the robustness of our findings by using the dummy variable for grade 4 completion. This is less likely to be an issue when we use the dummy variable for Grade 4 completion because over 98% of the women in the treated cohorts were no longer attending Grade 4 or below at the time of the interview.[18] Our estimates are qualitatively same under this specification, and thus we maintain the continuous educational

[17] We would ideally like to measure the outcomes for the treated group when they were older. However, in the available DHS, the current treated group is the only cohorts that were fully benefited from UPE. Thus, we chiefly examine the adolescent outcomes such as pregnancy before the age of 18.

[18] Respondents answered that they were "attending school" even if she went to school only one day. Thus, this number is likely to be overstated. The proportion of the fully treated attending Grade 5 or below was 3.6%, and the proportion attending primary schools was 7%.

attainment as the main variable of our interest. The cohorts aged between six and 16 in 1997 are omitted from the main sample because they were only partially treated.[19]

In addition to this main sample, in order to proxy the primary-school completion rate that would have realized for these cohorts without UPE, we use the completion rates among the cohorts that are several years older than the main sample: women aged 27-31 years when the policy started. The information on these older women is based on the 1991 Uganda Population and Housing Census (UPHC). We group them by the district of birth, and compute the completion rates. Given their age, it is highly unlikely UPE affected their educational attainment. On the other hand, their primary-level completion rate is likely to be correlated with the potential gain in educational attainment for girls aged 4-5 in 1997.[20] The older women in the UPHC were aged 21-25 at the time of interview. We assign this district-level pre-program completion rate to the UDHS data using the district of *current* residence. While it is ideal to use the district of residence around the age to enter school, the UDHS does not provide such information. However, over 92% of women aged 18-23[21] stay at the same district as where they were at age seven. Hence, the degree of attenuation bias due to a possible measurement error in the completion rate stemming from migration is likely to be fairly small.

4.2 Identification Strategy: Difference in Differences

We first study the impact of UPE on girls' schooling. For this, we employ a variant of the difference-in-difference approach where one of the differences is represented by a continuous, instead of

[19] We exclude women age 14 to 16 in 1997 even though they were not aged for primary school because a non-negligible number of them attended primary school when UPE started, most likely due to grade repetition and delayed entry. More than a quarter of women aged 15 and 11 percent of those aged 16 were enrolled in primary school, while less than 8 percent of women aged 17 were enrolled in primary school (UDHS 1996).

[20] Later, we show that our results are robust to the use of the other age cohorts to calculate the counterfactual pre-program completion rate

[21] Based on the Research on Poverty, Environment and Agricultural Technology Study 2012, which is a household survey covering the major regions except for North. The age range of 18-23 is chosen as that is when women in the main sample (born in 1992-93 and 1978-80) were interviewed in the UDHS.

dichotomous, treatment variable.[22] In other words, the inter-temporal change in educational attainment between the cohorts that were and were not exposed to UPE is compared across the districts that differ in terms of the intensity of expected UPE benefit, measured by the pre-program primary-level completion rate. Similar approaches have been used by Duflo (2001), Osil and Long (2008), and Dincer, et al. (2014) for example.[23]

Specifically, we use the following model as a baseline:

$$S_{ijkt} = \alpha_0 + \alpha_1 Young_t + \alpha_2 preUPE\ comp.\ rate_j + \alpha_3\left(Young_t * preUPE\ comp.\ rate_j\right) + \alpha_4 X_{ijkt} + \alpha_5 Z_{jkt} + \epsilon_{ijkt} \quad (1)$$

where S_{ijkt} denotes the number of schooling years of girl i, in district j, in region k, born in year t, $Young_t$ takes the value of one if the girl was aged 4 or 5 in 1997 (as opposed to 17-19 for the control group), and $preUPE\ comp.rate_j$ is the share of women born in 1966-70 who were born in district j. The average across-cohort change in educational attainment is captured by α_1, and the effect of differential program intensity is captured by α_2. While simultaneously controlling for these, the coefficient of the interaction term between the two variables, α_3, reflects whether changes across cohorts are systematically correlated with the pre-program primary-level completion rate.

[22] Theoretically it is possible to conduct the regression discontinuity method comparing the partially and fully treated cohorts. However, since only two cohorts are available in the treated cohorts, and also large changes are concentrated among the historically disadvantaged areas, with the existing data we cannot have enough power to detect any significant change between the two groups. It would be good to examine this when future waves of the DHS become available.

[23] As the instrument for educational attainment, the number of schools relative to the size of school-aged population (Duflo, 2001), the value of federal funds disbursed for primary school classroom construction per capita (Osili and Long, 2008), and the number of teachers relative to the number of children of relevant age (Dincer, et al., 2014) have been used. We have the data on the number of schools at age 6, but it did not accelerate its increasing trend around the time of UPE introduction. This might be because the government did not commit to new school construction. It committed to the provision of "construction of basic physical facilities in form of classrooms, laboratories, libraries, and teachers houses" and expected that "local authorities and communities would make additional input especially in the form of labour for construction" (MOES, 1999). Unfortunately, data on the number of classrooms built or governmental transfer for them are not available. Even if they are, they are likely to be jointly determined with the level of local contributions.

Figure 1 illustrates the intuition behind this identification strategy. The dotted line for the control cohort (called "Control 17-19") shows the positive relationship between the district level pre-program completion rates and individual educational attainment for girls aged between 17 and 19 in 1997. This implies that, prior to UPE, a smaller proportion of girls aged 17-19 (control group) completed Grade 4 in the districts with lower pre-program completion rate. This slope corresponds to coefficient α_2 in equation (1). Secondly, the solid line for the fully treated (called "Treatment 4-6") cohorts also shows a positive slope, but it becomes flatter than the slope for the control cohorts. The correlation between the size of the gap between the two lines and the pre-program primary completion rate is captured by coefficient α_3 in equation (1). Since the gap tapers as the pre-program completion rate increases, it confirms the theoretical prediction from Section 2 that improvement in educational attainment is concentrated in districts with higher program intensity (lower completion rates). As a result, the post-UPE cohorts exhibit a smaller inequality in educational attainment across districts.[24]

In addition to the intensity variables, Equation (1) includes other factors which could have affected educational attainment. At the household level, we control for the religion of the household head (X_{ijkt}). We also control for the district-cohort level covariates, Z_{jkt}. This includes the number of governmental primary schools which existed within the district when each cohort was at the age of six.[25] Thus, while access to schools can vary across districts and cohorts, and it can particularly improve after the launch of

[24] The figure for the years of education (not shown) shows a similar picture, with an indication of possible underestimation in only one district with the highest historical completion rate (the capital city of Kampala) where the years of education is higher for the partially treated.

[25] The availability of schools is unlikely to capture the impact of UPE package as the government did not commit to increasing the number of schools when it introduced UPE. In fact, the trend of the total number of schools neither accelerated nor slowed down after UPE started. Including the availability of schools enables us to control for the differences in transportation costs across areas and cohorts. The results however do not change qualitatively with and without controlling for this. The number of schools is based on the School Census 2006, which is the census of all the schools in Uganda and contains information on each school. Using the information on the year of establishment, we created the panel data of the number of public schools. While this does not include schools which existed when the sample girls were aged 6 but closed down by 2006, a public school closure is highly unlikely given the high population growth rate.

UPE in previously disadvantaged areas, its impact on educational attainment is controlled. Also, such disadvantaged areas might experience disproportionate economic growth due to mean reversion, which can boost educational investment. To control for this, we include the district-level share of communities (*Local Council 1,* or LC1) with a bank branch within its boundary when each cohort was around the age of 14. In addition, the access to health services could improve particularly in the disadvantaged areas over time, promoting the health and thus schooling of children there. We control this factor by including the district-level share of LC1s with a public hospital or health center when each cohort was around the age of 14.[26] Finally, the effort level of health and family planning workers might have changed differently in areas with high and low primary completion rates, which is likely to affect fertility and schooling. To address this concern, we include the share of girls in the same district who were interviewed in the previous wave of UDHS and reported to have been visited by family planning workers in the previous 12 months. This serves as a proxy for the frequency of visits by family planning workers for the treated cohorts when they were aged 13 or 14 (using the 2006 wave) and when the controlled were aged 16-18 (using the 1996 wave).[27]

Alternative to the basic specification, in Equation (2), we replace the dummy for the young cohort with λ_t, the set of birth year fixed effects. We also include μ_j, the set of 34 district dummy variables that existed in 1991 as well as the interaction between λ_t and $Region_k$, the set of four regional dummy variables.[28] These controls absorb any possible differences in educational attainment across cohorts and districts. For example, the nation-wide investment in education is likely to have increased over years, but this

[26] We use Uganda National Household Survey (UNHS) 1993 and 2006 to measure these district-level variables, and merge them with the main data. The 1993 data is assigned to the control cohorts born between the years of 1978 and 1980, while the 2006 data is assigned to the treated cohorts born in 1993 and 1994.

[27] This last control variable cannot be included when we have a small sample (Table 6) or when the previous wave is unavailable (Table 2).

[28] Four regions include North (Apac, Arua, Gulu, Kitgum, Nebbi, Kotido, Lira, Moroto, Moyo); East (Iganga, Jinja, Kamuli, Kapchorwa , Mbale, Pallisa , Tororo); West (Hoima, Kabale, Kabalore, Bushenyi , Kasese, Kibaale, Masindi, Mbarara, Kumi , Soroti, Bundibugyo, Kisoro , Rukungiri); and Central (Kampala, Kiboga, Luwero , Masaka, Mubende, Mpigi, Mukono, Rakai, Kalangala).

is captured by the cohort effects. Also, local public efforts to promote education could have been stronger in places with historically low levels of education. This is captured by the district effects. In addition, historically disadvantaged regions might have strengthened their public investment in education over time. Such heterogeneous trends are also captured by the region-specific cohort dummy variables. Thus, the identification assumption is that there was no other time-variant unobserved factors that started to affect educational attainment more favorably in the districts with lower pre-program primary completion rates at the same timing as UPE, after controlling for the district and cohort fixed effects, regional trends, and district-level indicators for general economic development and improvement in the access to schools and health facilities. We later shed light on the validity of this assumption by conducting the same regression exercise for two groups of girls who were both not exposed to UPE as a placebo test. The results indicate no effect, supporting the assumption. Finally, the standard errors are clustered at the level of districts that define the variation of the pre-program completion rate (Bertland, Duflo, and Mullainathan, 2004).

$$S_{ijkt} = \beta_0 + \beta_1\left(Young_t * preUPE\ comp.\ rate_j\right) + \beta_2 X_{ijkt} + \beta_3 Z_{jkt} + \mu_j + \lambda_t + Region_k * \lambda_t + \tau_{ijkt} \quad (2)$$

In Equation (2), the coefficient for the uninteracted terms cannot be estimated, but the estimate for the interaction term is likely to be more reliable. We use this as our preferred specification. The estimated coefficient, β_1, is likely to largely reflect the impact of UPE.[29]

4.3 Identification: Instrumental Variable Approach

The analysis of the impact of UPE on educational attainment can be regarded as the first stage regression in the instrumental variable method where educational attainment is treated as an endogenous

[29] The treatment group benefited from Universal Secondary Education (USE) policy in addition to UPE. However, as discussed in Appendix 4, only one out of three actually benefited from USE, and our estimates generally reflect the impact of UPE.

variable. We can simultaneously estimate the second stage where the effects of education on fertility, health investment and child health are investigated with the interaction term in Equation (2), $\left(Young_t * preUPE\ comp.\ rate_j\right)$, as an instrument. Specifically, the second stage equation is:

$$Y_{ijkt} = \gamma_0 + \gamma_1 S_{ijkt} + \gamma_2 X_{ijkt} + \gamma_3 Z_{jkt} + \mu_j + \lambda_t + Region_k * \lambda_t + u_{ijkt} \quad (3)$$

where Y_{ijkt} is an outcome variable, say, the dummy variable indicating whether a girl gives a live birth before age 18. The instrumental variable method requires that the instrument is correlated with the endogenous variable but not with the error term in Equation (3), u_{ijkt}. The first condition is likely to be met; as shown later, Craig Donald F-statistics indicate that our instrument explain sufficient variation in the endogenous variable (Stock and Yogo, 2005). The second condition is also likely to be satisfied. As we control for the district and cohort fixed effects, the region-specific cohort trends and time-variant district-level controls capturing the level of economic development, access to health and education facilities and the level of health outreach activities, there is unlikely to be a remaining time-variant factor which is correlated with both the historical primary completion rate and within-region variation in changes in outcomes. We will show later that a placebo test comparing two control cohorts indicates results consistent with this identification assumption.

5. Results

5.1. The Effect of UPE on the Years of Education

5.1.1 Fixed Effects Model

We start with the results for the effect of UPE on female educational attainment. Column 1 of Table 1 shows the results of estimating Equation (1). The results are consistent with Figure 1, which depicts the concentrated improvement in educational attainment after the introduction of UPE in areas with a historically low level of education. First, the coefficient for the uninteracted pre-program

completion rate, 10.19, suggests that a 10-percentage-point increase in the pre-program completion rate for older women is associated with a 1.02-year increase in the years of schooling for the control group. This association dramatically falls from 10.19 to 4.97 (=10.19-5.22) for the fully treated cohorts.

Table 1. The effects of UPE on the years of schooling

Outcome	Years of Schooling					
	(1)	(2)	(3)	(4)	(5)	(6)
1 if Born in 1992-1993 (young enough to fully treated under UPE)	3.19***					
	(0.70)					
pre-program primary education completion rate	10.19***					
	(1.68)					
1 if Born in 1992-1993*pre-program primary education completion rate	-5.22***	-4.65***	-5.01***	-8.88***	-9.46***	
	(1.37)	(1.02)	(1.49)	(2.03)	(2.06)	
1 if Born in 1980-1991*pre-program primary education completion rate						-0.75
						(1.21)
Craig Donald F-Statistics	14.57	20.84	11.26	19.10	21.13	0.38
Number of Observation	1421	1421	1421	1421	1421	4172
District Fixed Effect	No	Yes	Yes	Yes	Yes	Yes
Year of Birth Fixed Effect	No	Yes	Yes	Yes	Yes	Yes
4 Regions*Year of birth Fixed Effect	No	No	Yes	Yes	Yes	Yes
Time variant district characteristics	No	No	No	Yes	Yes	Yes
Cohort providing information on pre-program primary education completion rate (Age in 1991)	21-25	21-25	21-25	21-25	21-30	21-25

Note: Authors' calculation using UDHS 2001 and 2011. All specifications control for religion; muslim, catholic, protestant, and other religion serve as a reference group. Column 4-6 controls for the number of government primary school within the district when and where a woman was at the age of six, district level share of LC1 which has public hospital or health center within its boundary and that of LC1 which has bank branch within its boundary when each age cohort was around the age of 14, and the district share of women in the previous wave of UDHS who reported to have been visited by family planning workers in the previous 12 months (as the indicator for the level of outreach activities when the sample girls were aged around 14). District fixed effect refers to 35 districts existing in 1991. Standard errors are clustered at district level. Control cohort is women born 1978-1980. *** indicates significance at 1% level. ** indicates significance at 5% level. * indicates significance at 10% level.

In Column 2, we show the results based on our preferred specification in Equation (2). The inclusion of these controls hardly changes our results qualitatively, and our instrument consistently indicates the significant decline in the magnitude of the positive correlation between the years of schooling and the pre-program primary completion rate. This relationship is robustly found even when

we additionally control for the full interactions between the four regional dummies and birth year fixed effects (Column 3) and the time-variant controls (Column 4), and also when the measure of pre-program completion rate is calculated from different cohorts of women in the 1991 UPHC (Column 5). When we estimate the same regression equation with the partially treated cohort, the coefficient for them also indicates a decline in the positive relationship between the pre-program completion rate and the years of education (Column 6). However, the decline is only marginally statistically significant, and thus, the IV method cannot be used to identify the impact of UPE for the partially treated. Taken together, these results suggest that UPE improved girls' schooling particularly in the districts with low educational attainment prior to UPE, and as a result, it equalized educational attainment across districts in Uganda.

5.1.2 Robustness Check

However, one might wonder if the results merely reflect mean reversion. In other words, even without UPE, girls living in the districts with low pre-program completion rates might have shown the same improvement in educational attainment. To address this possibility, we conduct a placebo experiment. That is, we check whether we find a similar pattern between the two groups of cohorts that are both unaffected by UPE, which should be the case if the results are in fact due to mean reversion. For this exercise, we use the data for girls aged 17-18 in 1997 (the control cohorts in the main analysis) as the placebo treatment cohorts,[30] and girls aged 30-32 in 1997 as the placebo control cohorts. Both groups are unlikely to have benefited from UPE since both were 17 years of age or above when it started.

[30] As a placebo control cohort, women aged 30-32 in 1997 were used so that the age gap between this group and the younger group in the placebo analysis (12 years) is the same as the gap between the control and fully treated groups in the main analysis. We extract information on the two groups used in the placebo analysis from the UDHS 2001. Ideally we would like to use information for women who were aged 30-32 in 1997 from a previous wave so that they are in similar ages at the time of interview as the younger group. However, survey rounds of UDHS prior to 2001 cannot be used as they lack information on the district of current residence. The district level pre-program completion rates are calculated using the educational attainment of women born in 1959-1963, or aged between 34 and 38 in 1991.

Table 2. Placebo test for mean reversion

Placebo Test (Younger = born in 1979-80, Control = born in 1965-67)		Main Analysis (Younger = born in 1992-93, Control = born in 1978-80)		
Dependent Variable: years of schooling				
	(1)		(2)	(3)
1 if Born in 1979-80* pre-program primary education completion rate	-1.13	1 if Born in 1992-93* pre-program primary education completion rate	-9.22***	-10.27***
	(2.11)		(1.83)	(1.9)
Craig Donald F-Statistics	0.3		25.39	29.15
Number of Observation	956		1475	1475
Cohort providing information on pre-program primary education completion rate (age in 1991)	34-38		21-25	34-38

Source: UDHS 2001.

Notes: All specifications control for district fixed effect, single year of birth fixed effect, four-region-specific cohort trends, the number of government primary school within the district when and where a woman was at the age of six, district-level share of communities (Local Council 1) which has public hospital or health center, and that of communities with a bank branch when each age cohort was around the age of 14. Three dummies for the household head's religion are also controlled, indicating Muslim, catholic, and protestant. Other religions serve as a reference group. District fixed effect refers to 35 districts existing in 1991. Standard errors are clustered at district level. *** indicates significance at 1% level. ** indicates significance at 5% level. * indicates significance at 10% level.

The placebo results suggest no significant change in the relationship between the district level pre-program completion rate and individual educational attainment across the two cohort groups (Table 3, Column 1). This contrasts the main results, which are reported again in Column 2. This implies that the equalization in girls' educational attainment did not occur until the launch of UPE, which in turn provides suggestive evidence that UPE enhanced equal education opportunities. The results in Column 3 show consistent results even when the cohorts providing the information on pre-program completion rate is changed to women aged 34-38 in 1997.

5.2 The Effect of Education on Adolescent Pregnancy

5.2.1 The Effect of the Years of Education

We now turn to the impact of educational attainment on adolescent pregnancy. We take advantage of the exogenous change in educational attainment caused by UPE and instrument it by the interaction term between the pre-program primary completion rate and the indicator for the treatment cohorts. Thus,

our discussion thus far serves as the analysis of the first stage regression in the IV method. In Table 3, we start with the OLS estimates for comparison, which does not control for the endogeneity of girls' schooling (Column 1). The results show that an additional year of schooling is associated with 4.2 percentage point reduction in the probability of giving a live birth before the age of 18. The 2SLS estimates are shown with and without the region-specific cohort effects in Columns 2 and 3, respectively. Both results indicate that an additional year of schooling significantly reduces the probability of having adolescent pregnancy. In particular, the results based on our preferred specification (Column 3) show that attending another year of schooling reduces probability of having a birth before the age of 18 by 7.2 percentage point. This fall is as large as 18 per cent of pre-program mean of the outcome. The Craig Donald F-statistic for the test for weak instrument is 23.5, fairly large compared to the rule-of-thumb critical value.

Table 3. The effects of the education on the probability of having the adolescent pregnancy

Outcome: 1 if woman given birth before the age of 18	OLS	TSLS	TSLS
	(1)	(2)	(3)
Educational attainment in years	-0.042***	-0.065**	-0.072***
	(0.003)	(0.029)	(0.021)
Craig Donald F-Statistics	N/A	18.97	23.47
4 Regions*Year of birth Fixed Effect	Yes	No	Yes
Number of Observation	1472	1472	1472

Sources :UDHS 2001 and 2011.

Notes: All specifications control for the number of government primary school within the district when a woman was at the age of six, district-level share of communities (Local Council 1) which has public hospital or health center, that of communities with a bank branch when each age cohort was around the age of 14, the district share of women in the previous wave of UDHS who reported to have been visited by family planning workers in the previous 12 months (as the indicator for the level of outreach activities when the sample girls were aged around 14). The dummies for the household head's religion are also controlled, indicating Muslim, catholic and protestant. Other religions serve as a reference group. District fixed effect refers to 35 districts existing in 1991. Treatment and control cohorts were aged 4-5 and 17-19 in 1997 when UPE started, and their data were extracted from the UDHS2011 and 2001, respectively. Standard errors are clustered at district level. *** indicates significance at 1% level. ** indicates significance at 5% level.

Comparing Columns 1 and 3, the IV estimate is 42 percent larger in the absolute term than the OLS estimate.[31] This suggests the response among compliers – girls who would increase educational

[31] This might be unexpected if one is concerned about the unobserved heterogeneity in factors such as household wealth, academic ability and risk averseness. For instance, if a girl from a wealthy household is more risk averse, she is likely to complete primary education without financial constraints and also avoid adolescent pregnancy. Also, if a girl

attainment if they resided in districts with higher potential benefit from UPE - is larger than the rest of girls who would not change schooling behavior by the intensity of UPE benefit. Several previous studies also have found the IV estimates larger than the OLS estimates (for example, Breierova L. and Duflo E. (2004), Osili and Long (2008) and Cygam-Rehm Maeder (2013)). Altogether, the results suggest that girls' schooling significantly reduces adolescent pregnancy in Uganda.

5.2.2 The Effect of Grade Completion on Adolescent Pregnancy

The analysis in the previous section has assumed the linear relationship between the years of education and the incidence of adolescent pregnancy. However, there might be a critical grade from which the education effect kicks in. In order to explore this possibility, we examine how the completion of each grade in formal education affects the likelihood of adolescent pregnancy. This exercise also allows us to examine the robustness of our results to a possible bias caused by the selective censoring in the completed years of schooling. Specifically, instead of the number of education years, we use the dummy variable indicating the completion of each grade in primary education (P1-P7 completion), and the first three grade in lower secondary education (S1-S3 completion). The results are shown in Table 4. The results in Column 1 in Panel A suggest that a girl who completed the first year of primary education exhibits a lower probability of giving birth before the age of 18 compared to those who failed to complete it. The first stage results, shown in the second raw, indicate the equalizing effect of UPE, which is consistent with the results in Table 1. The results are similar for the P2-P7 completion (Column 2-5 in Panel A and Column 6-7 in Panel B). This consistency therefore provides suggestive evidence that a bias caused by the selective censoring, if any, is unlikely to be qualitatively alter our main findings

with a high innate ability is more ambitious regarding her career, she is more likely to complete primary education without much effort and also avoid pregnancy. These scenarios point to the possibility that the OLS over-estimates the true, negative effect of education on adolescent pregnancy.

Table 4. The effects of the grade completion on the probability of having adolescent pregnancy

Panel A: Effects of completion of P1, P2, P3, P4, and P5					
Endogenous variable	*1 if complete P1*	*1 if complete P2*	*1 if complete P3*	*1 if complete P4*	*1 if complete P5*
	(1)	(2)	(3)	(4)	(5)
Marginal effect on the probability of giving a birth before 18	-1.05** (0.47)	-0.98** (0.38)	-0.74*** (0.25)	-0.55*** (0.18)	-0.60*** (0.19)
Coefficient of instrument in 1st stage	-0.64*** (0.16)	-0.65*** (0.16)	-0.81*** (0.22)	-0.99*** (0.20)	-0.94*** (0.19)
Craig Donald F-Statistics	14.38	13.96	10.28	21.11	22.73
Number of observations	1422	1422	1422	1422	1422
Panel B: Effects of completion of P6, P7, S1, S2, and S3					
Endogenous variable	*1 if complete P6*	*1 if complete P7*	*1 if complete S1*	*1 if complete S2*	*1 if complete S3*
	(6)	(7)	(8)	(9)	(10)
Marginal effect on the probability of giving a birth before 18	-0.74** (0.32)	-0.60** (0.28)	-0.66** (0.32)	-1.27* (0.76)	-1.42* (0.73)
Coefficient of instrument in 1st stage	-0.75*** (0.18)	-0.90*** (0.22)	-0.86*** (0.25)	-0.47** (0.21)	-0.40** (0.16)
Craig Donald F-Statistics	16.24	15.34	10.07	3.97	5.35
Number of observations	1422	1422	1422	1422	1422

Sources: UDHS 2001 and 2011

Note: This Table shows the 2SLS estimates with the instrument of the interaction between the dummy variable indicating being born in 1992-93 (young enough to be fully treated by UPE) and the pre-program primary education completion rate. For more details about the specification, see the notes for Table 3. *** indicates significance at 1% level. ** indicates significance at 5% level. * indicates significance at 10% level.

In addition, the analysis of the completion of the secondary level education sheds light on whether UPE affects the entry into secondary schooling. The results based on the completed years of schooling in Table 2 suggest that UPE induces some girls to attain some secondary education.[32] The results in Table 4 provide similar implications; that is, UPE promotes the enrolment in the first year of lower secondary school, which reduces the probability of having adolescent pregnancy (Column 8). However, the impact of S2 and S3 completion is unlikely to be identified as the first stage results are weak (Columns 9 and

[32] Based on the coefficient in Column 3 in Table 2 (-9.22), a one-standard-deviation increase in the pre-program primary education completion rate (0.18) is associated with a 1.66 year increase in girls' years of schooling. Adding this increment to the mean pre-program educational attainment (5.92), the average number of years of schooling reaches 7.58 years for the treated, which corresponds to the middle of the first grade in secondary education. With some variance around the mean, these results are likely to reflect that some of the treated girls completed the first few grades of secondary education.

10).[33]

5.2.3 The Effects of the Years of Education on Age at Marriage and Contraceptive Use

The results thus far have shown that female education reduces adolescent pregnancy. Next, we analyze whether this was achieved through an increase in contraceptive use, abstinence and/or miscarriage and abortion. First if education promotes abstinence, the onset of sexual activities is likely to delay. However, there is no evidence for the probability of having the first sexual intercourse by the age of 15, …, 18 to decline with education (Panel A, Table 5). On the other hand, improvement in educational attainment can delay the age at the first marriage, and the results in Panel B suggest that this is in fact the case. The decline in the probability of marrying by the age of 15-18 is almost identical to the decline in the probability of giving a birth before the age of 15-18 (Panels B and C, Table 5). In addition, we find that education increases the current use of modern contraceptive methods, in particular pills but not condoms (Columns 1-3, Panel D).[34] While we do not have information on the past use of contraceptives, to the extent that current use is correlated with past use, the results are indicative of the roles contraceptives played in delaying the first pregnancy. On the contrary, education does not reduce the chance of abortion or miscarriage (Column 4, Panel D). If we assume the incidence of miscarriage was unchanged, the results suggest that abortion was not the major pathway through which female education reduced adolescent pregnancy.[35] Taken together, the results indicate that female education reduces adolescent pregnancies by

[33] Results using the completion of grade S4 or higher as the outcome are qualitatively same as those in columns 9 and 10.

[34] Modern contraceptives include 11 methods – Pill, IUD, Injections, Diaphragm, Condom, Female Sterilization, Male Sterilization, NorplantTM or implants, Lactational amenorrhea, Female condom, and Foam/jelly. Other methods include Periodic Abstinence (Rhythm), Withdrawal, Abstinence, Folkloric methods and others. We assume that women who have never had sexual intercourse have never used a condom. Even when we limit the sample to the women who have ever experienced sexual intercourse, our results remain qualitatively unchanged. Also, the results remain insignificant when the outcome is changed to the use of any method of modern contraception.

[35] There was another factor which could have caused the reduction in adolescent pregnancies. In 2007, the amendment to the Penal Code Act introduced more stringent punishments to performing a sexual act with a person who is below 18 years of age. For instance, performing a sexual act with such under-aged persons can lead to life imprisonment, and if the person is below the age of 14 years, death penalty. Since our treated cohorts were 14 and 15 years old in 2007, their chance of pregnancy due to defilement could have been reduced thanks to this amendment. However, if this law change

delaying marriage and possibly promoting modern contraceptive use.

Table 5. The effects of education on the timing of the first pregnancy, marriage and sexual intercourse

A: Effect on the onset of sexual intercourse				
	1 if a woman had the first sexual intercourse			
	before the age of 18	*before the age of 17*	*before the age of 16*	*before the age of 15*
Educational attainment in years	-0.03 (0.02)	-0.04 (0.03)	-0.02 (0.02)	-0.03 (0.02)
Craig Donald F-Statistics	26.74	26.74	26.74	26.74
Number of Observation	1417	1417	1417	1417
B:Effects on the age at the first marriage				
	1 if a woman got married			
	before the age of 18	*before the age of 17*	*before the age of 16*	*before the age of 15*
Educational attainment in years	-0.07*** (0.02)	-0.07*** (0.02)	-0.04*** (0.01)	-0.04*** (0.01)
Craig Donald F-Statistics	23.65	23.65	23.65	23.65
Number of Observation	1472	1472	1472	1472
C: Effects on the age at the first pregnancy				
	1 if a woman gave birth			
	before the age of 18	*before the age of 17*	*before the age of 16*	*before the age of 15*
	(1)	(2)	(3)	(4)
Educational attainment in years	-0.072*** (0.021)	-0.045*** (0.016)	-0.034*** (0.014)	-0.021** (0.010)
Craig Donald F-Statistics	23.47	23.47	23.47	23.47
Number of Observation	1472	1472	1472	1472
D: Effects on contraceptive use				
	1 if used modern contraceptive method during last sexual intercourse	*1 if used condom during last sexual intercourse*	*1 if used pill during last sexual intercourse*	*1 if ever terminated pregnancy by abortion or miscarriage*
Educational attainment in years	0.050* (0.023)	-0.010 (0.016)	0.030*** (0.007)	-0.016 (0.019)
Craig Donald F-Statistics	21.8	21.8	21.8	23.47
Number of Observation	1235	1235	1235	1472

Sources: UDHS 2001 and 2011

Note: This table shows the 2SLS estimates for the effect of the number of years of completed schooling, where the excluded instrument is the interaction between the dummy variable indicating being born in 1992-93 (young enough to be fully treated by UPE) and the pre-program primary education completion rate. In Column 1-3, Panel D, sample is limited to women who have ever had intercourse. Mean of age at the first period is 14.5. For more details about the specification, see the notes for Table 3. *** indicates significance at 1% level. ** indicates significance at 5% level. * indicates significance at 10% level.

These results are somewhat different from Keats (2014) which does not find change in the age of

is the only factor reducing the total number of adolescent pregnancies, it is likely that a larger reduction is found among very young girls and a smaller reduction for older girls. Nevertheless, the effects on the probability of pregnancy before the age of 14 and 13 are smaller (with the coefficients of 0.031 and 0.015, respectively), and larger for pregnancies under 19 years of age (with the coefficient of 0.076). These estimates are all significant (not reported).

marriage. This might be that adolescent marriage is concentrated among girls who never attend school or drop out of it early on, thereby failing to benefit from UPE in the partially treated cohorts. Equivalent girls in the fully treated cohorts however are more likely to be induced to attend school or continue schooling longer as fee abolishment applies from grade one. On the other hand, similar results are found in the other studies from Kenya and Malawi (Duflo, et al., 2006; Baird, et al., 2010; Duflo, et al, 2015).

5.3 The Effects on Maternal and Infant Care Use and Child Health

While we have shown that education reduces adolescent pregnancy and marriage, still one third of the treated women gave birth before the age of 18. Can education help those mothers to provide better health investment for their children? We now examine the impact of female education among those who gave birth by the age of 18 on health investment behavior measured by the utilization of maternal and infant care as well as on the health status of children measured by the infant mortality and current incidence of illness. Since the age of the first pregnancy is lower for the controlled and in areas with higher pre-program completion rates, there are many more sample children in these groups. In order to balance the sample size across these comparison groups, we use only the first-born children.

The results in Panel A, Table 6 show that educated mothers are more likely to receive delivery assistance by a doctor (Column 1), and deliver at a hospital (Column 2).[36] They are also more likely to have their child vaccinated for BCG, but not Polio 0 (Columns 3 and 4).[37] This might because BCG

[36] We define medical professionals to include (1) doctor, (2) nurse/midwife, (3) private medical assistant and (4) private nursing aide. The rest of the response options are: (5) traditional birth attendant, (6) relative, (7) friend, and (8) the others. We define hospital to include (1) government hospital, and (2) private hospital/clinic. The rest are (3) respondent's home, (4) traditional birth attendant's home and (5) other home, (6) government health center, (7) government health post, (8) other public, (9) other private medical facility and (10) others.

[37] Among several types of vaccinations, we focus on BCG and the first dose of polio (Polio 0), which are supposed to be taken at a very early stage of infants' lives (at birth or first contact for BCG, and at birth or within the first two weeks for the first dose of Polio drops), in order to keep a reasonable sample size. The other vaccinations specified by the Uganda National Expanded Programme on Immunization are: DPT combined with Hepatitis B and Haemophilus influenza type B (DPT-HepB+Hib), Measles, and later doses of Polio. DPT-HepB+Hib is supposed to be taken at 6 weeks or first contact after that age and then two more times with a 4-week interval. Measles is supposed to be taken

vaccination is recommended to be given at birth, which can be facilitated as more women choose to deliver at modern facilities.

Table 6. The effects of maternal education on health investment and child health

Panel A: Effects on formal maternal care use and infant vaccination

Outcome	*1 if delivery was assisted by a medical professional*	*1 if deliver at modern health facility*	*1 if child had polio vaccination*	*1 if a child received BCG vaccination*
	(1)	(2)	(3)	(4)
Year of schooling	0.15**	0.15**	-0.043	0.086***
	(0.07)	(0.07)	(0.05)	(0.03)
Craig Donald F-Statistics	11.47	11.76	10.88	10.87
Number of Observation	630	626	554	553

Panel B: Effects on child mortality and illness incidence

Outcome	*1 if died before 1 month*	*1 if died before 12 months*	*1 if dispose of youngest child's stools to latrines*	*1 if suffered from diarrhea during last 2weeks*
	(1)	(2)	(3)	(4)
Year of schooling	-0.024	-0.054**	0.129***	-0.07
	(0.02)	(0.03)	(0.04)	(0.05)
Craig Donald F-Statistics	10.67	11.01	17.26	11.79
Number of Observation	623	496	801	555

Sources: UDHS 2001 and 2012

Note: This Table shows the 2SLS estimates with the instrument for female education being the interaction between the dummy variable indicating being born in 1992-93 (young enough to be fully treated by UPE) and the pre-program primary education completion rate. The sample is limited to the first-born children. In Panel B, it is further limited to children aged 1 month or older (Column 1), 12 months or older (Column 2) and alive at the time of interview. Column 3 is based on the sample of mothers living with a child under the age of 5. For more details about the specification, see the notes for Table 3. Standard errors are clustered at district level. *** indicates significance at 1% level. ** indicates significance at 5% level. * indicates significance at 10% level.

The probability of death before the age of 12 months also falls by 5.4 percentage points for the children of women with an additional year of completed education (Column 2, Panel B). Since the probability of death within one month remains unaffected (Column 1), maternal education is likely to reduce deaths between the 2nd and 12th month. Since BCG vaccination is shown to have "non-specific effects," or the impact of reducing overall mortality on top of its role of reducing the case of tuberculosis in the first 6-12

once at 9 months or first contact after that age. Polio 1 is supposed to be taken at six weeks or first contact after that age, then two more doses with a 4-week interval (Republic of Uganda, 2011). When we analyze the impact on these other types of vaccinations for children old enough to take them, no significant results are found or sample is too small to estimate the IV model.

months of life (Higgins, et al., 2014), a possible explanation for these results is that improved immunization take-up protected the infants. Also, more educated mothers dispose of the stools of their children to latrines or toilets (Column 3).[38] Though the incidence of diarrhea is unaffected (Column 4), this might indicate a better sanitary condition for children. On the other hand, education is not found to reduce other types of sickness (cough and fever). Also, while educated women's households are more likely to own a mosquito net, their children are no more likely to sleep under it (not reported).[39] Taken together, promoting female education not only reduces adolescent pregnancy but also promote some health investments when they become pregnant, and as a result, their children benefit from improved survival rates.

5.4 Pathway through Which Education Improves the Reproductive and Health Outcomes

A natural next question is how education delays marriage and pregnancy and improves child survival. As discussed in Introduction, the literature has suggested at least six channels through which female education affect the reproductive outcomes: (1) human capital effect, (2) incarceration effect, (3) knowledge improvement, (4) decline in fertility preference, (5) assortative mating, and (6) improved bargaining power within couples. Regarding the channels affecting child health, (1) human capital effect and (5) assortative mating in terms of education are likely to increase the amount of resources available to children. Also, (1) human capital effect and (4) knowledge improvement are likely to help mothers to better investment in the health of their children as well. This section examines each of these pathways.

First, the results in Table 7 show that education improves literacy (Column 1, Panel A). While the

[38] The outcome takes the value of one if the child used toilet or latrine, or stools were put/rinsed into toilet or latrine. Other options include that stools were put/rinsed into drain or ditch, thrown into garbage, buried, let in the open, and others.

[39] This might be partly due to a still low probability of sleeping under a net. It is at 2 percent for the whole sample and 7 percent for children living in households with a bed net. The impact on anthropometry (e.g., height for age and weight for height) is difficult due to a high rate of missing values.

impact on the probability of working in any sector or being employed by someone other than family member is insignificant (Columns 2 and 4), the impact on the probability of working in the non-agricultural sector is of marginal significance with the p-value of 0.11 (Column 3).[40] Thus, education seems to improve human capital measured by literacy, but it has not led to significantly different jobs, which might be due to the fact that the treated are still young at ages of 18 and 19. This suggests the education effect on child health through an increase in own income is limited.

Second, while we cannot directly test the incarceration effects due to the lack of data on time allocation or the age at school completion/drop-out, since Ugandan students must stay school from 7am to 5pm every weekday with adult supervision, it is natural that a girl who spends longer years in school has much less time, opportunity and desire to take a risk of pregnancy. Together with the fact the majority of women attended school when they were 13-17,[41] the incarceration effects of education are likely to be one of the probable channels through which education delayed marriage. However, to the extent the negative effects on pregnancy and marriage last till the age of 19 when most girls are out of school, other channels are likely to have played roles as well.

Third, there is some evidence that women became to gather information from media and became more knowledgeable about reproductive issues. The results in Panel B, Table 7 show that educated women are more likely to read newspaper and listen to a radio (Columns 1 and 2). They are also more likely to have heard about family planning in the previous six months through these media (results not shown). In addition, educate girls are more likely to know the timing of ovulation and the source of condoms (Columns 3 and 4). While these outcomes are far from the comprehensive description of knowledge which

[40] The impact of primary grade completion on work in the non-agricultural sector is significant (not reported). Non-agricultural job categories include professional/technical/managerial, clerical, sales and services, skilled manual, and unskilled manual, and exclude the two other categories: "agricultural-self employed" and "agricultural-employee." The excluded categories other than working for someone else are working for family member and being self-employed.
[41] About 90 percent of the treated were attending school at the age of 13 and 14, and about 60-70 percent of them were at the age of 16 and 17 (Panels A and B, Appendix Table 4).

can be improved by education, the results might be taken to indicate that education enables girls to avoid adolescent marriage and pregnancies through improved knowledge.

Fourth, we find that education alters female fertility preference. The results show the ideal number of children declines by 0.13 with an additional completed year of schooling (Column 1, Panel C). This is consistent with the theory that an educated mother prefers fewer births or delays the start of reproduction as their children tend to survive longer (Becker and Lewis, 1973). Thus, lower fertility preference is likely to be one of the important factors contributing to the delay in the first marriage and pregnancy. Together with the knowledge of reproductive issues, it is also likely to contribute to the increase in the amount of resources available per child.

Fifth, assortative mating could augment the income effect on child height if educated women are married to educated men. It can also affect the link between education and pregnancy if educated women are married to men with lower fertility preference, which can overwhelm the possibly positive income effect of the partner's education on fertility (Behrman and Rosenzweig, 2002). The results indicate that improvement in female education has no significant impact on her partner's age, education and fertility preference (Columns 2-4, Panel C). Hence, unlike the case for Indonesia (Breierova L. and Duflo E., 2004), assortative mating is unlikely to be the main channel through which education affects early fertility and child health in Uganda.[42]

[42] Most of the partners belong to the partially treated group as they are six years older than the treatment women on average, and this group did not experience a significant increase in education (Keats, 2014).

Table 7. The effects of education on labor supply, knowledge, fertility preference, bargaining power and assortative mating

A: Effects on literacy and work				
	(1)	(2)	(3)	(4)
	1 if literate	*1 if currently working*	*1 if working in non-agricultural sector*	*1 if employed by someone other than family*
Schooling Year	0.076***	0.017	0.062	-0.001
	(0.018)	(0.03)	(0.04)	(0.04)
Craig Donald F-Statistics	20.00	23.47	18.53	18.62
Number of Observation	1446	1472	883	884
B: Effects on knowledge and information gathering				
	1 if listened to radio in last 12 months	*1 if read newspaper in last 12 months*	*1 if knows the timing of ovulation in ovulatory cycle*	*1 if knows the source of male condom*
Schooling Year	0.075**	0.058***	0.047***	0.035**
	(0.03)	(0.02)	(0.02)	(0.02)
Craig Donald F-Statistics	26.75	23.77	23.30	23.38
Number of Observation	1472	1467	1469	1471
C: Effects on fertility preference and partner's characteristics				
	Ideal number of children	*1 if partner wants the larger number of children than respondent*	*Partner's age*	*Partner's educational attainment*
Schooling Year	-0.12*	-0.058	-0.006	0.33
	(0.07)	(0.04)	(0.35)	(0.33)
Craig Donald F-Statistics	21.27	12.44	13.96	19.15
Number of Observation	1447	552	836	902
D: Effects on bargaining power				
	Acceptance of domestic violence by a husband (0-5)	*1 if a woman can make a decision on her medical expense alone*	*1 if woman can make a decision to visit her family or relatives*	*1 if woman thinks that refusing sex with husband is justified if he has other woman*
Schooling Year	0.004	0.017	0.001	-0.077***
	(0.15)	(0.035)	(0.04)	(0.03)
Craig Donald F-Statistics	19.59	18.84	18.84	23.98
Number of Observation	886	906	906	912

Sources: UDHS 2001 and 2011

Note: This table shows the effects of female education on factors which can eventually improve reproductive and health outcomes, using 2SLS with the instrument for female education being the interaction between the dummy variable indicating being born in 1992-93 (young enough to be fully treated by UPE) and the pre-program primary education completion rate. For more details about the specification, see the notes for Table 3.

In Panels C and D, we limit the sample to women who have ever been in union. In Panel D, to measure acceptance of domestic violence committed by her partner, respondents were asked whether beating by her partner is justified if she: goes out without telling him, neglects the children, argues with him, refuses to have sex with him, and burns the food. We use the number of cases in which "Yes" was chosen. To measure women's decision making power, we use the question of whether she has a final say on the following matters: her health care, making large household purchases, making household purchases for daily needs, visits to family or relatives. All the outcomes indicate no impact, and the estimate in the case of her health care is shown as an example.

Standard errors are clustered at district level. *** indicates significance at 1% level. ** indicates significance at 5% level. * indicates significance at 10% level.

Finally, the reduction in female fertility preference may not realize perfectly unless women have a final say on contraceptive use or the timing and frequency of sexual intercourse. To address this last channel, we examine the education effects on several proxies for the wife's bargaining power among those who have married: acceptance of domestic violence committed by her partner, whether she can make a decision on various household matters, and whether she can refuse sex with him under certain circumstances. To the extent that these proxies represent female bargaining power, our results are mixed (Panel D). Also, it is found that education does not encourage the use of condoms, which is usually decided by men, but increases the use of pills, over which woman have more control (Panel D, Table 5). Thus, it is somewhat inconclusive whether education reduces the likelihood of adolescent pregnancy by allowing women to say no to sex or to ask her partner to use contraceptive.

In sum, we find some evidence for both human capital and incarceration effects. We also find that education lowers fertility preference and equip women with better knowledge about reproductive issues. On the other hand, it is unlikely in the Ugandan case that education promotes assortative mating. Evidence is mixed for the education impact on female bargaining power in marriage.

5. Conclusion

In this paper, we have investigated whether promoting female schooling affects adolescent fertility, health investment behavior and the health status of their children. In order to overcome the endogeneity of educational attainment, we instrument it with the across-district and inter-cohort variation in the intensity of expected benefit from Uganda's UPE, which abolished a substantial part of the schooling costs in 1997. We have shown that an additional year of schooling reduced the probability of giving a live birth before the age of 18 by 18 percent (7.2 percentage points) in Uganda. This reduction was achieved through the delay in marriage and possibly also through an increased use of

pills, though not condoms. On the other hand, neither the timing of sexual debut nor the likelihood of miscarriage or abortion changed. We have also shown that educated women are more likely to practice better health investments such as the use of formal delivery care and the vaccination for infants. As a result, their children are less likely to die between the 2nd and 12th month of their lives.

In addition, we have comprehensively investigated the mechanism through which education make these changes. First, we find that female education reduces their ideal number of children and improves knowledge about reproductive issues. These factors are likely to have contributed to the decrease in adolescent pregnancy and improvement of health investment and thus child health. Second, we find some evidence suggesting that both incarceration and human capital effects were at work. For the incarceration effects, the majority of women attended school when they were in the age range for which the reduction of early pregnancy was found. For the human capital effects, education is found to raise literacy. On the other hand, evidence is inconclusive whether education influences female bargaining power within marriage, and no evidence is found for assortative mating. As Dincer et al. (2014) discusses, it might take more time to change social customs such as attitudes towards spouses and marriage patterns. These results imply that, in addition to the two effects (incarceration and human capital effects) that have been discussed mainly as the pathways through which education lowers fertility, its impact on fertility preference and knowledge gain are likely to be important channels explaining the influence of female education. This is likely because the partners are on average six years older than the fully treated group.

Taking a broad view of the findings, they imply that the benefit of female education is not limited to the advancement in human capital and future labor force, but includes the reduced risk of adolescent pregnancy, better health practices and healthier babies. This underscores the importance of considering the widespread benefits of female education in shaping the policy and institution influencing

educational attainment. It would be fruitful future research to examine how these benefits evolve as the children of these women grow up and whether their social attitudes change in the long run. It would be also important to investigate the external validity of these findings, particularly given the recent trend of developing countries adopting UPE.

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Appendix Table 1: Summary statistics for Pregnancy, marriage, abstinence and educational attainment

	Control group		Treatment group	
Variable	Mean	S.D.	Mean	S.D.
Timing of the first birth				
1 if gave live birth before the age of 18	0.40	0.49	0.26	0.44
1 if gave live birth before the age of 17	0.27	0.44	0.14	0.35
1 if gave live birth before the age of 16	0.15	0.36	0.08	0.28
1 if gave live birth before the age of 15	0.06	0.24	0.03	0.18
Timing of marriage				
1 if got married before the age of 18	0.49	0.50	0.32	0.47
1 if got married before the age of 17	0.38	0.49	0.22	0.41
1 if got married before the age of 16	0.25	0.43	0.11	0.31
1 if got married before the age of 15	0.14	0.34	0.06	0.23
Timing of first sexual intercourse				
1 if had the first sexual intercourse before the age of 18	0.69	0.46	0.57	0.50
1 if had the first sexual intercourse before the age of 17	0.55	0.50	0.42	0.49
1 if had the first sexual intercourse before the age of 16	0.38	0.49	0.26	0.44
1 if had the first sexual intercourse before the age of 15	0.20	0.40	0.14	0.35
Educational Attainment				
Years of education	5.92	4.10	6.82	3.21
1 if completed primary education	0.40	0.49	0.52	0.50
Individual level characteristics				
1 if muslim	0.17	0.37	0.17	0.38
1 if catholic	0.41	0.49	0.40	0.49
1 if protestant	0.37	0.48	0.27	0.45
District level characteristics				
Share of women born 1966-70 and complete primary education	0.39	0.13	0.39	0.18
*District-age cohort level characteristics**				
Number of government primary school at the age of six	323.98	135.51	362.94	181.41
Share of LC1 with public hospital/health center within its boundary	0.08	0.09	0.07	0.07
Share of LC1 with bank branch within its boundary	0.04	0.03	0.01	0.03
Number of observation	770		702	

Note: Authors' calculation using UDHS 2001 and 2011.
* All the district-cohort level characteristics are taken from the 1993 and 2006 UNHS. The 1993 data provides the information when the controlled were aged 13-15, while the 2006 data provides the information when the treated were aged 13-14. The availability of a bank was asked together with the availability of government agencies, cooperatives and money lenders in 1993, while it was asked together with the availability of microcredit institution.

Appendix Table 2: Summary statistics for pathway outcomes

	Control group		Treatment group	
Variable	Mean	S.D.	Mean	S.D.
Contraceptive and abortion				
1 if currently using modern contraceptive method	0.23	0.42	0.12	0.32
1 if currently using condom	0.07	0.25	0.05	0.22
1 if currently using pill	0.05	0.22	0.01	0.08
1 if ever terminated pregnancy by abortion or miscarriage	0.16	0.37	0.06	0.25
Human capital effects				
1 if literate	0.25	0.43	0.37	0.48
1 if currently working	0.67	0.47	0.53	0.50
1 if employed by other	0.26	0.44	0.26	0.44
1 if working in non-agricultural sector	0.52	0.50	0.59	0.49
Fertility preference				
Ideal number of children	4.14	1.47	4.20	1.58
Ideal number of boys	1.67	1.03	1.78	1.00
Ideal number of girls	1.79	1.12	1.90	1.07
Ideal number of children of either gender	0.69	1.64	0.52	1.55
Women's bargaining power				
Number of conditions under which hitting by her husband is considered to be justified (0-5)*	1.97	1.54	1.60	1.60
1 if wife can refuse sex if her partner has other women	0.82	0.39	0.76	0.43
1 if wife can make a decide on:	0.60	0.49	0.52	0.50
health care expenses for herself	0.29	0.45	0.49	0.50
visits to family or relatives	0.52	0.50	0.46	0.49
Knowledge				
1 if listened to radio in last 12 months	0.64	0.48	0.71	0.46
1 if read newspaper in last 12 months	0.17	0.41	0.21	0.41
1 if knows the timing of ovulation in ovulatory cycle	0.22	0.41	0.12	0.32
1 if knows source of male condom	0.69	0.46	0.76	0.43
Partner's characteristics				
Partner's age	28.55	5.60	24.72	4.60
Partner's educational attainment	7.23	3.91	6.99	3.80
1 if partner wants fewer children	0.07	0.27	0.11	0.31

Note: Authors' calculation using UDHS 2001 and 2011. * See Appendix 3 for more details.

Appendix Table 3: Summary statistics for child health

	Control group		Treatment group	
Variable	Mean	S.D.	Mean	S.D.
Delivery care and neonatal care use				
1 if a mother received delivery care by a medical professional	0.66	0.47	0.73	0.44
1 if a mother gave a delivery at health facility	0.66	0.47	0.73	0.45
1 if a child received				
BCG vaccination after birth	0.88	0.32	0.94	0.23
Polio vaccination after birth	0.45	0.50	0.67	0.47
Child mortality				
1 if a child died before				
the age of 1 month	0.06	0.23	0.04	0.21
the age of 12 months	0.11	0.31	0.08	0.27
1 if dispose youngest child's stools to latrines	0.74	0.44	0.68	0.47
1 if a child suffers from diarrhea during last 2 weeks	0.13	0.34	0.28	0.45
Number of observations	356		274	

Note: Authors' calculation using UDHS 2001 and 2011.
Samples are the first birth of respondent in the main analysis.

Appendix: The distribution of girls by the grade attended in 2005, 2009, and 2012

As the number of children completing primary education increased, Ugandan government launched Universal Secondary Education (USE) policy in 2007, abolishing the tuition in public secondary schools. In order to be eligible for this, primary school pupils had to sit in the final examination conducted at the end of primary school called Primary Leaving Exam (PLE) after 2006, and obtain the aggregate score of less than 32 (smaller scores indicate better performance). Without delay in school entry and grade repetition, the fully treated cohorts are supposed to have sat in the PLE in or before 2006, and thus they are not eligible for USE.

While some of them could have sat in the PLE after 2006 due to delayed entry and/or grade repetition, the descriptive statistics of the grade attended show the probability of benefiting from USE policy is at most one in three. Table A3 indicates that at least two thirds of the treated did not benefit from USE policy. Panel A, B and C depict the share of women born in 1992 and 1993 by the grade attended in 2006, 2009 and 2012, respectively. In 2006, 16 percent of those born in 1992 were already

attending secondary school and 14 percent were not attending any school. Since USE policy affected students who sat in the PLE in 2006 or later, those 30 percent of women were unlikely to have benefited from USE policy. While the remaining 70 percent of women who were still in primary school could have proceeded to secondary school after 2007, Panel B shows that only about 23 percent of women born in 1992 and 1993 were in grade S1-S3 in 2009. Some of these girls are likely to have entered secondary school before USE policy started and repeated grade at the lower secondary level. Hence, the share of the treated exposed to the USE policy by 2009 is likely to be at most 23-24 percent. For about 29 percent of women born in 1992 and 47 percent of those born in 1993 who were still attending primary school, Panel C indicates that only 5 percent and 13 percent of the two respective cohorts were in grade S1-S3 in 2012. Therefore, the share of women entering lower secondary school by 2012 is likely to be at most 28 and 37 percent for women born in 1992 and 1993, respectively. Though there is a small fraction of women still in primary school in 2012, it is likely to be uncommon for them to proceed to secondary school later because they would be over 19 years of age, and opportunity costs of attending school would be very high. In summary, the average share of USE beneficiaries among the treated is at most 33 percent, or one in three.

One might also wonder whether the expected benefit from UPE and USE could partially offset each other. While areas with high pre-program primary-level completion rate have lower potential to gain from UPE, they might benefit more from the USE policy because of job opportunities which reward secondary education in the areas. If this is the case, the net benefit from UPE and USE combined becomes similar across areas with differential pre-program primary completion rates. As a result, the impact of UPE on the number of years of schooling, or the first stage coefficient, is likely to be biased towards zero. This tendency is likely to be larger for outcomes indicating the completion of higher secondary grades. This is indeed confirmed by the first stage results in Table 4 (Columns 9 and 10).

Thus, the combined impact of UPE and USE policies is unclear on the completion of higher secondary grades. However, the negative and significant effect of the IV on S1 completion (Column 8) indicates that, for this grade, the equalizing effect of UPE dominates the possibly opposite effect of the USE policy. It might be that UPE induced girls to secondary school, though it did not lead to continued secondary education. Thus, our results are likely to reflect the impact of the UPE, not USE, policy.[43]

Appendix Table 4. Share of treated women who attended each level of education in 2006, 2009, 2012

Panel A: Distribution of women born in 1992 and 93 by attendance status in 2006							
Birth year	Age in 2006	P1-P7	S1-S3	S4-S6	College	Not attending	Total
1992	14	394	88	-	-	80	562
		(0.70)	(0.16)	-	-	(0.14)	(1.00)
1993	13	517	47	-	-	58	622
		(0.83)	(0.08)	-	-	(0.09)	(1.00)
Panel B: Distribution of women born in 1992 and 93 by attendance status in 2009							
Birth Year	Age in 2009	P1-P7	S1-S3	S4-S6	College	Not attending	Total
1992	17	95	75	31	-	126	327
		(0.29)	(0.23)	(0.09)	-	(0.39)	(1.00)
1993	16	198	100	19	-	103	420
		(0.47)	(0.24)	(0.05)	-	(0.25)	(1.00)
Panel C: Distribution of women born in 1992 and 93 by attendance status in 2012							
Birth Year	Age in 2012	P1-P7	S1-S3	S4-S6	College	Not attending	Total
1992	20	17	22	43	27	351	460
		(0.04)	(0.05)	(0.09)	(0.06)	(0.76)	(1.00)
1993	19	19	27	47	17	103	213
		(0.09)	(0.13)	(0.22)	(0.08)	(0.48)	(1.00)

Note: Authors' calculation using Uganda National Household Survey 2006, 2009, 2012.
Share of women born in the same year and attending each level of education is shown in bracket.

[43] One might consider separately estimating the impact of UPE and USE, using the share of women completing secondary education together with the share of women completing primary education prior to UPE as the instruments. However, results become unreliable because of the strong colinearity between the two instruments. Although it would be interesting, distinguishing the effects of UPE from USE policy is out of the scope of this study.