ACCESS TO EDUCATION AND REPRODUCTIVE HEALTH:
EVIDENCE FROM FREE EDUCATION POLICY IN UGANDA

A Dissertation

Submitted to the Faculty of the National Graduate Institute for Policy Studies (GRIPS)

in Partial Fulfillment of the Requirements for the Degree of

DOCTOR OF PHILOSOPHY IN DEVELOPMENT ECONOMICS

by

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March, 2016

Abstract

Access to Education and Reproductive Health: Evidence from Free Education Policy in Uganda

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Improving access to education is a key to economic development in Africa. Governments have attempted to make education more accessible by reducing user costs. However, rigorous empirical literature on the effectiveness of such attempts is particularly scarce. Thus, this dissertation aims to bridge the gap in the literature, examining the free education policy of Uganda in terms of its effects on education and health outcomes.

The present examination of the effects of free secondary education on students' access, student body composition, learning environment, and achievement indicates that it is effective in boosting secondary school graduates. It also emphasizes that private schools play an important role in expanding access. Although the fall in average test score of 11th grade students was achieved by those in the participating private secondary school mostly, this was likely due to the change in the composition of the students taking the exam, and free secondary education had few negative effects on the academic achievement of students in traditional secondary schools that existed prior to the program.

The non-monetary benefits of free primary education for females are also examined here in the context of reproductive health. The analysis reveals that an increase in years of education effectively reduces the probability of adolescent pregnancy. Furthermore, the findings suggest that educated mothers make more effective use of delivery and neonatal care, resulting in lower infant mortality rates.

The primary policy implication is that the USE is a welfare improving measure, in that it increased the number of secondary school graduates with few negative effects on their academic achievement in traditional secondary schools, and that it allowed more students to complete secondary education in new secondary schools. A secondary implication is that the benefits of a free education policy are not limited to educational attainment, but also extend to a reduced risk of adolescent pregnancy and healthier babies. This underscores the importance of considering the widespread benefits of female education in shaping policy and institutions influencing educational attainment.

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Acknowledgements

Firstly, I would like to express my sincere gratitude to my advisor Prof. Chikako Yamauchi for her continuous support of my Ph.D. study and related research, and for her patience, motivation, and immense knowledge. Her guidance helped me over the entire period of research and writing this dissertation. I could not have imagined having a better advisor for my Ph.D. study.

In addition to my advisor, I would like to gratefully thank my thesis committee, including Prof. Tetsushi Sonobe for his insightful comments, guidance, and encouragement, which urged me to widen my research and career from various perspectives. I could not have imagined having a better mentor throughout my study at graduate school.

My sincere thanks also go to Prof. Ryuichi Tanaka, Prof. Alistair Munro, and Prf. Hideo Akabayashi, who enthusiastically supported my research throughout the program. Without their precious support, it would not have been possible to complete my dissertation.

I would like to thank the professors who educated me at the International Development Studies program at GRIPS: Prof. Keijiro Otsuka, Prof. Takashi Yamano, Prof. Tomoya Matsumoto, Prof. Kei Kajisa, Prof. Yukichi Mano, Prof. Aya Suzuki, and Prof. Hideaki Goto. I learnt from their profound knowledge and strong passion for research from beginning to end, and they have been my role models.

I am also particularly thankful for strong support and encouragement from collaborators in Ministry of Education and Sports in Uganda and Uganda National Examination Board, Michael Ssozi, Nsabusa Mathias, and Peter Wakabi. Without their help, this dissertation would not have been possible.

I thank my fellow classmates and football teammates for the stimulating discussions, the sleepless nights we worked together before deadlines, and all the fun we have had in the last five years.

I am indebted to the personnel of GRIPS for their generous administrative support. I am also grateful for the sincere support of the manager at GRIPS international house.

Last but not the least, I would like to thank my parents for supporting me throughout my long journey of 21 years of study, and in my life in general.

The findings and conclusions of this dissertation are all mine, and I am responsible for all the mistakes in this dissertation.

Dedication

To my parents, Ikuo and Yuko

Chapter 1: Introduction

Improving access to education is a key to economic development and poverty alleviation in developing countries. A range of macroeconomic evidence indicates a strong association between education and economic growth. Furthermore, microeconomic theory explains that obtaining an education improves not only individual productivity and income, but also health and other non-productive outcomes. Based on such findings, governments in developing countries have taken to spending large portions of their annual budgets on education. However, access to basic education remains insecure in certain regions of Africa and South Asia. Empirical evidence suggests that schools are largely inaccessible in such regions due to parents having to pay tuition. To break this barrier, one of the most promising policies is to offer education free of charge. A number of African countries have introduced free primary education policies during the last two decades, and studies have shown that this succeeded in expanding access. However, two important questions remain unanswered in terms of the effectiveness of free education policies in Africa. First, relatively few studies have examined whether abolishing school fees for secondary education would also effectively boost enrolment. Second, evidence is limited as to whether such a free education policy changes not only educational outcomes but also rates of early pregnancy and infant mortality. Against this background, the present dissertation examines the free education policy of Uganda, aiming to provide initial evidence of its effects on the number of secondary school graduates and their academic achievement. Furthermore, I propose that the benefits of such a policy are not limited to the improvement of educational outcomes, but also to improvements in reproductive health, reducing risky immature pregnancy and child mortality.

1.1 Background and significance of the issue

The past century has seen dramatic improvement in primary school enrolments rates in developing countries, but the magnitude of this change differs greatly across regions. For example, primary school enrolment rates in sub Saharan Africa remain as low as 77%, although almost all primary school aged children currently attend school in Southeast Asia and Latin America. As many children in Africa do not complete even primary education, approximately four in ten adults remain illiterate in this region (World Bank, 2015). Against this background, achieving universal access to basic education in Africa attracts particular attention amongst policy makers and researchers in the 21st century.

Why do children in such regions not attend school? The literature on developing countries generally attributes the limited access to basic education to its high user costs (for example, Deininger, 2003). To overcome this constraint, more than 10 African countries have attempted to increase the accessibility of government primary schools by abolishing fees during last two decades, including Malawi in 1993, Uganda in 1997, and Kenya in 2003. Such policies have dramatically increased primary school enrolments and numbers of graduates (Lucas and Mbiti, 2012).

However, in order to sustain economic development in Africa, promoting primary education is a prerequisite, but not the ultimate goal, as only a population educated at secondary or a higher level allows a country to produce highly skilled workers, such as physicians, engineers, and academics. However, according to the United Nations Educational, Scientific, and Cultural Organization (UNESCO, 2011), 40% of all lower secondary school-aged girls and 33% of boys remain out of school in Africa. Given that most children in Africa now complete primary education, these statistics suggest that improving access to secondary education is the next goal for governments of this region. To this end, Uganda eliminated secondary school fees in 2007, a first in sub Saharan Africa. Many other African countries are considering introducing such a policy in order to sustain educational development (Lewin, 2009). Clear understanding of both the positive and negative effects of such a policy on students' access and achievement will allow policy makers to institute such policies appropriately. However, to the best of my knowledge, the effectiveness of free secondary education in Africa has not yet been studied.

A further important question that remains unanswered is whether such a free education policy improves not only educational outcomes, but also reproductive health outcomes. This dissertation examines the role of female education in reducing the incidence of early pregnancy and infant mortality.

Early pregnancy poses serious medical risks for both mothers and children. Infant mortality and low birth weight occurs more often among the children of teenage mothers, and such children often suffer poorer health in later years. Despite such negative effects, significant numbers of adolescent pregnancies still occur in developed countries like the U.S. and U.K., and even larger numbers are reported in developing countries. The United Nations Population Fund (2013) estimates that 19 out of 100 women experience a live birth before the age of 18 years in developing countries, whereas only six do so in the U.S. The highest rates of adolescent pregnancy are found in sub Saharan Africa, ranging between 25 and 28 out of every 100 women.

Schooling for girls has been shown to reduce the incidence of adolescent pregnancy. However, comprehensive evidence is limited regarding the pathways by which girls' education reduces early pregnancy.

In addition, even if a young mother has given birth as an adolescent, promoting maternal education may improve her infant's heath, as such mothers may have a better understanding of the risks of delivery and child morbidity. However, research has not yet investigated how the promotion of maternal education reduces infant mortality in Africa, where the problem is most severe.

Policy makers in the education sector generally consider the benefits of a free education policy in the context of educational outcomes. Hence, understanding the reproductive health returns of such a policy might provide useful insights in shaping such policy and the institutions influencing educational attainment.

1.2 Objectives

In this context, the objectives of the study reported in this dissertation were two-fold. The first objective was to determine whether a free secondary education policy in Uganda improved students' access to and achievement in lower secondary school education. I also aimed to examine the effects of such a policy on student body composition, private school entry, and learning environment, in order to develop a comprehensive picture of changes in the education market. The second objective of this study was to determine the effect of the free education policy in Uganda on promoting female educational attainment and thereby reducing the likelihood of adolescent pregnancy and infant mortality. I further investigated the mechanism behind infant causalities by examining the educational effects on a range of reproductive health outcomes.

1.3 Main findings

The main findings, reported in chapter 4, suggested that Uganda's free secondary education policy promotes students' access to education, with few negative effects on the academic achievement of secondary school graduates. The results also illustrate that private schools play important role in expanding access to secondary education by allowing larger number of students to complete 11th grade. The results further reveal that Uganda's free secondary education policy has increased secondary school enrolment and encouraged private schools to enter the education market. Despite the desirable effects on students' access, such a policy is likely to reduce the "average" academic achievement of 11th grade students by changing the composition of those who take secondary school exit exam. However, a free secondary education policy is unlikely to reduce the average test score in secondary schools that existed prior to the implementation of the policy.

Further findings are discussed in chapter 5, showing that an increase in years of education reduces the probability of early marriage and adolescent pregnancy, primarily due to the delayed first marriage. While the literature discusses the incarceration and human capital effects of education as the mechanism through which it lowers fertility, the evidence from this study suggests that both were at work in Uganda. The results also show that, among those who do become mothers, educated women make more use of maternal and infant care, and mortality rates among their children decline between the 2nd and 12th months.

Given such findings, the first policy implication is that the elimination of secondary school fees improves the welfare of a country, as it increases number of secondary school graduates with few negative effects on their academic achievement in traditional secondary schools. It also increases the number of secondary school graduates in newly established secondary schools, although that their academic achievement may be relatively lower than the former graduates.

The second implication is that a free education policy improves female educational attainment, and promoting female education can be effective in reducing adolescent pregnancy, which is associated with adverse consequences, such as premature delivery and maternal and neonatal death. Thus, this dissertation emphasizes the great importance of considering the widespread benefits of girls' educational attainment in shaping the policies and institutions that influence the education system.

1.4 Organization of this dissertation

This dissertation is structured as follows: Chapter 2 provides a review of relevant literature and clarifies gaps in the literature. Chapter 3 describes the Ugandan education system and policy. Chapter 4 examines the effects of Uganda's free secondary education policy on the number of secondary school graduates, student body composition, number of private schools, learning environment, and students' academic achievement. Chapter 5 assesses the effects of female education on adolescent pregnancy and child health in terms of free primary education policy. Lastly, Chapter 6 sets out the conclusions drawn on the basis of the findings.

Chapter 2: Literature review

About the first ten years of basic education teach not only fundamental knowledge and skills, but also ethics and social norms. Such basic education not only improves individual future productivity and earning power, but also promotes individual communication skills and reduces the incidence of crime. The existence of such social returns for basic education provides an incentive to governments to enable all children to complete at least primary education, and preferably also secondary education.

However, in sub Saharan Africa, even primary school completion rates fail to reach 100%, and furthermore, fewer than 50% of children are enrolled in secondary education. Therefore, for governments to achieve their educational objectives, they must design effective policies to boost enrolment without harming the quality of education.

To understand how a government can help children to complete more years of education, it is useful to develop a framework to explain how parents decide on their consumption of and investment in children's education. Against this background, the first part of this chapter introduces a theoretical framework of education production function. Following the introduction of this framework, I present a review of empirical studies examining the causal relationship between education policy and relevant outcomes.

In addition to individual productivity gains and the abovementioned social returns, a further meaningful private return on basic education discussed in the growing body of literature is that promoting female basic education changes early pregnancy patterns and reducing infant mortality. As women achieve more years of education, and acquire more skills and knowledge through education, several effects occur. First, the opportunity cost of childbearing and childrearing rises. Second, women become more knowledgeable about the risks of child morbidity. Third, women earn more, and thus can invest more in their children's health. Thus, mothers are likely to raise fewer children, and invest more in their health and education.

Such non-productive returns on education are especially important in the context of sub Saharan Africa, where the rates of risky adolescent pregnancy and infant mortality are the highest in the world. Thus, the second part of this chapter develops a framework that predicts the trade-off between quantity and quality of children, this framework being followed by a review of empirical research.

2.1 Factors affecting years of schooling and learning

A growing body of literature has investigated the causal relationship between education policy and education outcomes in developing countries. To evaluate the parameters such researchers have estimated, and how much we can trust these, this section begins by providing a baseline framework of the production function for education.

2.1.1 An education production function

Parents determine the optimal level of a child's education by maximizing their utility under certain constraints. Two central constraints include the education production function and income constraints; this section focuses on the former.

The education production function for the extent of skills and knowledge obtained through education, comprises both random exogenous factors and endogenous factors that a family can choose. Examples of exogenous factors include the quality of education available, related to such things as the distance to the nearest primary school, the quantity and quality of available teachers, and so forth. A child's innate ability cannot be chosen by the parents or child, and neither can certain household characteristics, including income level. Such factors are usually determined either randomly or by the government, leaving the family no power to choose. On the other hand, parents can and must decide how many years their children will attend school, and how much the family will invest in the necessities of education, such as the purchase of textbooks.

Glewwe and Kremer (2006) established a theoretical framework for the education production function in terms of five sets of factors, as follows:

$$A = a(S, Q, C, H, I) ----(1)$$

In the above formula, A stands for skills obtained through education, and is a function of the completed year of education (S), the quality of the school (Q), exogenous child and household characteristics (C and H), and household investment in educational materials (I).

In this model, S and I are the variables whose utility parents can choose to optimize given the four exogenous factors. Hence, S and I may be described as follows:

$$S = f(Q, C, H, P) ----(2)$$
$$I = g(Q, C, H, P) ----(3)$$

In the above equations, parents decide the optimal levels of S and I, given Q, C, H, and the exogenous price of schooling (P).

Finally, combining the three equations (1)-(3) above yields the following reduced form of the relationship:

$$A = b(Q, C, H, P) ----(4)$$

Given that the relationship in equation (2) and S are normal good, one of the most promising education policies by which to improve the accessibility of basic education is to reduce its price (P). In section 2.1.2, I review research examining policies that have reduced the price of basic education. In chapter 4, I explore the exogenous change in P in secondary education in Africa on the basis of Uganda's free secondary education policy, and estimates the $\delta S / \delta P$ in (2), using the number of secondary school graduates as proxy for S.

In the context of Uganda's free secondary education policy, I deal with the quality of education (Q) in equations (1)-(4) as a function of its price (P) because such a nation-wide education policy is likely to change the quality of the secondary education available. For example, it may increase the number of students per school, leading to crowded classrooms. Thus, I define Q as follows:

$$\mathrm{Q} = \mathrm{c}(\mathrm{P}) - - - (5)$$

Using equation (5), equation (2) becomes:

$$\mathrm{S} = \mathrm{h}(\ \mathrm{C}, \mathrm{H}, \mathrm{P}\ ) - - - (6)$$

By this equation (6), I determine in chapter 4 the total derivative of S with respect to a change in P, in order to determine whether a free secondary education policy will allow more children to complete secondary education.

In chapter 4, I also use equation (5) to determine whether and how the government responded to maintain the quality of education at its pre-policy implementation level. For example, the government may build more government schools in order to maintain effective class sizes over time.

Finally, again using equation (5), the education production function in (4) becomes:

$$\mathrm{A} = \mathrm{d}(\ \mathrm{C}, \mathrm{H}, \mathrm{P}\ ) - - - (7)$$

In chapter 4, I determine $\delta\,\mathrm{A}/\,\delta\,\mathrm{P}$, the total derivative of the learning (A), using the average secondary education exit exam score as a proxy, with respect to the change in price of secondary education (P), unconditional on the level of Q.

This approach emphasizes that a government might promote access to basic education, mainly in government schools, in two ways, namely by reducing its cost and improving its quality (equation (2)).

Equations (4), (5), and (6) also imply that a change in the price of education (P) may affect learning achievement, A, through a change in the quality of education (Q). Subsection 2.1.3. reviews factors affecting learning achievement in order to understand how a change in the private cost of secondary education may hinder or improve students' skills and knowledge.

2.1.2 Factors and policy influencing access to education

As shown in the section above, the quality of education (Q) and its price (P) are the major determinants of the quantity (S) and quality (A) of children's learning outcomes. In this context, a government is able to improve school enrolment in two ways, namely making school attractive by improving the learning environment and making it accessible by reducing the cost. This sub section sheds light on the important role of price in determining children's access to basic education in developing countries.

Under the assumption that an education (S) is a non-inferior goods, $\delta S / \delta P$ is non-negative by definition. In other words, a fall in the price of education loosens the income constraints of a household, and such income effects may allow a child to attend school for longer, or parents may simply allocate the surplus income to other consumption. In this sense, price elasticity of schooling is debatable.

Recent empirical literature has investigated the causal relationship between a primary education cost reduction policy and children's educational attainment. The most reliable way to determine the causal relationship between an intervention and the outcome of interest is to rely on a randomized controlled trial. There is one study that did so, namely that of Duflo et al. (2015) in Kenya, which showed that the provision of free school uniforms in a public primary school significantly reduced the dropout rate.

Many other studies in developing counties have investigated the effects of changes in public primary school fees. One simple way to reduce the cost of education is to make primary education free. Early studies evaluating such a policy have compared children given free primary education to those without in terms of age, reporting that such a policy increased students' enrolment and timely enrolment (Al-Samarrai and Zaman, 2007 (Malawi); Deininger, 2003; Grogan, 2009; Nishimura et al., 2008 (Uganda)). Furthermore, a recent quasi-experimental study by Lucas and Mbiti (2012a) examined the effects of Kenya's free primary education policy on the number of primary school graduates. Their results showed that fee abolishment effectively increased the number of primary school graduates, suggesting that schooling costs for parents hindered school enrolment.

Various policies have reduced the cost of schooling in different ways. One such policy is to build more primary schools, reducing the distance required to travel to school, thereby reducing the transportation and opportunity costs of attending school. In her seminal work, Duflo (2001) examined the effects of a large school construction program in Indonesia on educational attainment. She explored regional variation in program intensity and found that new primary school construction significantly increased the number of years of schooling children completed. This finding also suggests that the cost of schooling is a major constraint on school enrolment in developing countries.

In addition to such a policy of reducing schooling costs, some policies even "pay" households for children's school enrolment, such as a conditional cash transfer (CCT) program. Schultz (2004) investigated the random assignment of the PROGRESSA CCT program and found that participation in this program increased the probability of enrolment among all primary grade students. A recent study by Glewwe and Kassouf (2012) further confirmed that such a program in Brazil also increased enrolment and lowered dropout rates in primary school. Such empirical evidence once again suggests that reducing the cost of schooling is an effective way to improve children's access to education and to increase the amount of education obtained. However, one caveat of such policies is that enforcement of conditionality is costly and difficult for the government. Thus, a child in a program such as those described above may enroll on a school roster, but not participate in the class. Indeed, De Brauw and Hoddinott (2011) relies on the fact that some households in the PROGRESSA program were not subject to school attendance monitoring, and found that the absence of monitoring significantly reduced the likelihood of school enrolment among children from cash transfer recipient household.

School feeding program are one of the ways of addressing this issue. Such programs provide free meals at school in the middle of the day, and hence a child must physically attend school to receive the meal. Studies in India found that having such a program at primary school increased student attendance rates (Afridi 2011; Jayaraman and Simroth, 2015; Vermeesch and Kremer, 2004). In summary, all of the studies reviewed above are consistent in suggesting that children are likely to obtain more years of education if the cost of primary schooling is reduced or non-existent.

However, at the level of secondary education, few studies have examined the effects of a change in the cost of education, either in developed or developing countries.

In developed countries, public secondary schools have generally been free in the 21st century, and thus researchers cannot examine the effects of a change in cost. One study by Riphahn (2012) explored regional variation in the timing of secondary school fee abolishment in the states of post war Germany, reporting that the probability of attaining an upper secondary education improved significantly in districts without fees.

In contrast to developed countries, most public secondary schools in developing countries currently charge parents for tuition, particularly in sub Saharan Africa. Furthermore, the cost of secondary school has remained somewhat stable, as governments have spent the financial budget on the primary sector during the last two decades. Thus, once again, opportunities for naturalistic experimentation are rare, and there is little empirical evidence. Two studies examined the CCT program, namely Schultz (2004) in Mexico and Hermida (2014) in Ecuador, both reporting that CCT is effective in reducing secondary school dropout rates. Such evidence suggests that more children will attend secondary school if the government rewards such attendance with cash.

However, to the best of my knowledge, no study has investigated whether more children remain in secondary school if the government reduces the cost. Furthermore, no such study has been conducted in Africa, where approximately 20% of the world’s youth live, with this number growing rapidly. Chapter 4 of this dissertation fills this gap in the literature by utilizing Uganda's free secondary education policy in a natural experiment.

Thus far, I have discussed factors and policies that encourage children to attend school for longer in terms of the role of the cost of primary and secondary education. Such education policies are often implemented simultaneously across an entire country, thereby changing the education market dramatically. It is especially worth noting that a school may become congested if basic education becomes less costly and more children enter the school. This may harm the learning environment, and thus the academic achievement of the students. To understand how such policies may affect students' achievement, the following sub section reviews factors influencing the knowledge and skills obtained by students.

2.1.3 Factors and policy influencing skills obtained through education

In this sub section, I review literature examining factors affecting students' test scores in the middle and/or at the end of primary and secondary education, as most empirical studies use students' test scores as a proxy for their knowledge.

In equation (1), the quality of education (Q) entails various school characteristics, such as class size, quality of teachers, learning materials, and peers. Recent quasi-experimental studies have addressed the question of whether and how much such school characteristics affect students’ academic achievement in developing countries.

Moses Maimonides wrote in the 12th century that a “class size should not exceed 40,” and class size has since then been heavily discussed in the empirical literature. If a teacher is required to teach too many students, each individual student may be paid less attention, and the manner of instruction may not be suited to each student’s demand. Thus, a high number of pupils per teacher is likely to harm the classroom learning environment and hinder academic achievement.

Building on this discussion, recent quasi-experimental studies have addressed the effect of class size on academic achievement. Angrist and Lavy (1999) exploited Maimonides’ rule which creates exogenous variation in class size, and found that it was negatively associated with test scores in Mathematics and Reading among Israeli 5th grade students. Most other studies were conducted in developed countries (e.g., Browning and Heinesen (2007) for Denmark; and Boozer and Rouse (2001); Krueger (2003); Krueger and Whitmore (2001) for the U.S.), and report results consistent with those of Angrist and Lavy (1999). The only published study in a developing country was by Urquiola (2006), who showed consistent class size effects in Bolivian primary school data.[1]

[1] In contrast, a few studies have provided conflicting results, including Hoxby (2000) for the U.S. and Leuven et al. (2008) for Norway.

These results suggest that increasing class size is likely to harm academic achievement. To determine whether such change occurred in Uganda, chapter 4 examines whether the free secondary education policy increased the pupil-teacher ratio, and whether it affected 11th grade graduates' academic performance in Mathematics and English.

Not only quantity, but also the quality of teachers may affect students' learning. Teachers with better understanding of the subjects they teach may offer clearer instruction. Furthermore, quality of instruction may improve through teaching experience, allowing more experienced teachers to add greater value to students' skills and knowledge. Clotfelter et al. (2007) studied a rich data set of individual test scores, and found that teachers' level of experience, test scores, and qualification were all related to the Mathematics and Reading scores of grade 3 to 5 students. Thus, even if a government maintains an existing teacher-pupil ratio by recruiting new teachers when school enrolment increases, students' academic achievement may suffer if newly employed teachers are not as effective as existing ones.

In Chapter 4, I examine the effect of the implementation of free secondary education in Uganda on the secondary school learning environment and students' test scores by means of equations (5) and (7). In doing so, this dissertation addresses the issue of a deteriorating learning environment and poorer student achievement as a results of the introduction of a free secondary education policy.

Thus far, I have reviewed factors affecting students' access to education and their learning, as well as the effects of government interventions in government schools. A further way for a government to make education more accessible is to rely on private institutions; the role of private institutions in improving access to education is discussed in the final sub section below.

2.1.4 Institutions: The role of private schools

Positive externality of educating population rationalizes the government to intervene in the education market. Thus, in most countries, both the government and private sectors provide primary and secondary education services. Among these two types of schools, government schools often play an important role in offering equitable access to basic education to the entire population, especially for the poor. Governments in developed countries are generally able to use substantial tax revenue to build sufficient government schools and employ teachers in order to achieve this goal. However, as mentioned above, governments in developing countries have limited budgets, leading to limited access to primary and secondary education. To address such financial constraints, governments in such regions are increasingly forming partnerships with private schools to make school more accessible. The details of such public-private partnerships (PPPs) varies, but the most common type of agreement in developing countries is a voucher program, by which the private sector provides education services and receives grants from the government. Such a program provides a per capita subsidy for a certain number of voucher bearing students to attend a private school. This sub section summarizes the rationale for a PPP program in the education market, and reviews literature on whether it is effective in promoting enrolment and improving student achievement in primary and secondary school.

Proponents of PPPs emphasize that private schools are more productive than public schools because their school managements are less regulated and they deploy school resources more efficiently (Andrabi et al., 2007). Furthermore, private schools are likely to be more transparent than public schools because of their direct financial transactions with parents. In addition, the existence of private schools in the education market may improve the quality of public school services by means of competition. Specifically, in a setting in which a government school's annual budget is tied to per capita grants based on enrolment, the school must compete with private schools to increase its enrolment, leading it to improve the quality of its services to make it more attractive (Patrinos et al., 2009).

On the other hand, concern exists that the expansion of private schools may promote economic segregation into a top tier of private schools and more poorly performing public schools. In fact, MacLeod and Urquiola (2012) reported that a large voucher program in Chile promoted changes in the composition of student bodies across schools in terms of academic performance and socioeconomic status.

A small but growing body of literature has examined the effects of PPPs and/or an increase of private schools in the education market on education outcomes and the quality of private school management. Studies in Pakistan (Alderman et al., 2003;Barrera-Osorio and Raju, 2011) and Uganda (Barrera-Osorio et al., 2015) have found that having PPP schools in the market effectively increased the number of enrolments in primary and secondary schools. Also, Barrera-Ossorio et al. (2011) showed that the public per student subsidy for low cost PPP schools in Pakistan increased school resources, such as the number of teachers and classrooms. Such evidence suggests that forming a partnership with a private school may promote students' access to basic education.

On the other hand, the effects of PPP schools' penetration of the education market on learning achievement is inconclusive. In private schools, flexibility in school management and efficient productivity may improve the learning environment and students' academic achievement. Furthermore, the abovementioned competition between public and PPP private schools may force public schools to provide better education, with students' understanding of the subjects improving. However, if private schools become more accessible due to government subsidies, such schools may become congested, affecting the quality of the learning environment and the academic achievement of the students who would be enrolled in such a private school regardless of the PPP program.

The empirical evidence in this regard is also mixed. One study particularly relevant here is that conducted by Barrera-Osorio et al. (2015) in Uganda, using randomized experiments to show that changing rural low-cost private secondary schools into PPP schools increased enrolment and improved students' test scores. They suggest that having a PPP increased school input, such as laboratory, and promoted students with better ability, thus improving the test scores in the PPP secondary school. On the other hand, MacLeod and Urquiola (2013) and Hsieh and Urquiola (2006) investigated a voucher program in Chile and showed that a rapid increase in the number of private schools did not change the average test scores of all the students in the education market.

These findings relate to a policy whereby the government forms explicit partnerships with private institutions, but a free education policy that makes public schools congested may also drive the demand for private schools. Lucas and Mbiti (2012) investigated the effects of free primary education policy in Kenya, which eliminated school fees in government primary schools, on private schools. They found that the policy promoted the entry of more private school into the market, whereas the number of primary school graduates from private primary schools remained at pre-policy implementation levels. This suggests that, even without developing an explicit PPP program, a free education policy for public schools may lead private institutions to help expand access to basic education in developing countries.

However, although private schools often absorb the larger share of students in the secondary sector than in the primary sector, no study has examined the effects of a free secondary education policy in public schools on private schools. Chapter 4 bridges this gap in the literature by examining whether public secondary school fee abolishment in Uganda increased the number of private secondary schools and the number of private secondary school graduates.

2.2 Education, fertility, and child health

The second part of this chapter discusses non-academic productive benefits of additional years of schooling. Among such benefits, this dissertation explores the returns of female education on fertility and child health. I emphasize educational returns in this context because both birth and child mortality rates in Africa are the highest in the world, and thus of interest to policy makers, but evidence is scarce for this region. Subsection 2.2.1 reviews a predictive model by which the effect of parental education on the quantity of children and child health can be understood. Sub section 2.2.2 presents empirical finding on these effects of education.

2.2.1 A predictive model

This sub section discusses how female education affects the quantity of children and child health in terms of the conceptual framework of the quantity-quality model of fertility, relying on the seminal work of Becker and Lewis (1973). According to this quantity-quality model of fertility, the quantity and quality of children depends largely on parental education, where quality refers to children's' better health and cognitive skills. In this model, the father and mother together maximize their utility by determining the number of children (N), the quality of the children (Q), and their standard living (Z) under the following full budget constraint:

$$R = \pi_z Z + + \pi NQ + \pi_N N + \pi_Q Q - - - (8)$$

where π_Z is the set price of Z, π is the price of one unit of NQ, π_N is the fixed cost of N, π_Q is the fixed cost of Q.

$\pi_N N$ includes the time and cost of pregnancy and delivery. $\pi_Q Q$ incorporates the costs that are independent of the number of children, πNQ shows that the cost depends on both N and Q. Hence, the marginal costs of N and Q are derived as follows:

$$P_N = \pi_N + \pi Q$$
$$P_Q = \pi_Q + \pi N$$

The marginal cost of another child rises with Q, as raising a high quality child is more costly. The marginal cost of Q increases with N because raising the quality of a child is higher if the total number of children who receive the quality increase is larger.

Based on this framework, I incorporate the role of parental education in determining the optimal N and Q. For the sake of simplicity, this section discusses the role of maternal education, rather than that of both parents, as mothers often spend more time with their children than fathers do. Assuming, for example, that an educated women is more efficient in producing quality children because she knows the medical risks of child disease and how to prevent these, the skill and knowledge obtained through a longer education lower her π_Q. This change in the marginal cost of improving child quality produces substitution effects. As improving child quality is now cheaper than before, and mother chooses to raise fewer children with better human capital.

Furthermore, given that an educated mother is more likely to have skills by which to earn a higher wage, a longer education raises the opportunity cost of delivering and raising another child π_N. Again, this increase in π_N induces substitution effects from having more children to having higher quality children. This model predicts that obtaining a longer education will reduce the number of children and increase those with better health and cognitive skills. In the following section, I review recent empirical literature examining the causal relationship between education and pregnancy rates and child health.

2.2.2 Empirical evidence

Numerous studies have investigated the impact of female education on fertility. While earlier studies have tended to report cross-sectional correlations, recent quasi-experimental evidence suggests that female education indeed reduces both early pregnancies (for example, Berthelon and Kruger, 2011; Black et al. 2008; Breierova and Duflo, 2004; De Paoli, 2011; Grönqvist and Hall, 2013; Humlum et al., 2014; Keats, 2014; Osili and Long, 2008; Ozier, 2015) and total fertility (Black et al., 2008;Cygan-Rehm and Maeder, 2012; De Paoli, 2011; Monstad et al., 2008). Furthermore, randomized control trials providing educational subsidies show that a reduction in schooling costs decreases drop-out rates, as well as early pregnancy and marriage rates (Baird et al., 2010; Duflo et al., 2015). As many quasi-experimental studies have focused on a reduction in the costs of school attendance through construction of additional schools or fee abolition, these findings can be taken as robust evidence for the impact of female education on early pregnancy.

Various theoretical pathways have been put forward as an explanation for the negative relationship between female education and fertility. First, higher educational attainment increases human capital, and thus raises the opportunity cost of time for childbearing and childrearing. This is likely to decrease early pregnancy and increase labor force participation (Becker, 2009). However, the empirical evidence for this human capital effect is mixed. Second, schooling might merely create incarceration effects, by which girls have less time, opportunity, and desire to engage in risky reproductive health behavior while attending school with adult supervision (Berthelone and Kruger, 2008). Third, education may change female fertility preferences. Since educated women often face a trade-off between the quantity and quality of children (Becker and Lewis, 1973), improvement in educational attainment might lead women to want to have fewer children. Fourth, educated girls may become more knowledgeable about the medical risks of unprotected sexual activity and/or giving birth at an early age. While this can be considered a 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. Fifth, educated women are more likely to marry educated men, who might also want fewer children (Behrman and Rosenzweig, 2002). Among studies examining this assortative mating channel, some have found that partners are indeed more educated (Breierova and Duflo, 2004; Clark and Royer, 2013; Lavy and Zablotsky, 2011), but others have not (Cygam-Rehm and Maeder, 2013; Keats, 2014). Finally, once women are partnered, the bargaining model of a household suggests that decision making, including that regarding pregnancy and health investment, hinges on the relative bargaining power of each partner.

Evidence is scarce with regard to these pathways, and Keats (2014) suggested they are unlikely to have an effect. In sum, evidence on the importance of such pathways remains limited and mixed. More importantly, few studies have comprehensively investigated all of the pathways. Chapter 5 provides comprehensive evidence regarding early pregnancy, and further explores the pathways through which female education affects this.

In terms of the impact of female education on child health and health investment behavior, rigorous evidence is more limited. For instance, published studies have indicated that maternal education reduces child mortality (Breierova and Duflo, 2004; Chou et al., 2010) and anemia and stunting (Keats, 2014). Moreover, relatively sporadic evidence exists for the mechanism through which female education reduces early pregnancy and improves child health. Chapter 5 bridges this gap in literature by examining the effects of maternal education on the utilization of delivery and neonatal care, and on infant mortality before age of 12 months.

Chapter 3: Education system and policy in Uganda

As education systems vary across countries, the relevant institutional structure must be clarified in order to understand the impact of policies and the external validity of research findings. Therefore, this chapter explains the institutional context of education in Uganda. Section 3.1 explains primary education and school finance systems, the primary school exit exam, and secondary school admission process. Section 3.2 describes the context of secondary education. Section 3.3 illustrates the free education policies in Uganda. In the all sections, reference is made to relevant literature.

3.1 Primary education in Uganda

3.1.1 Primary education system

Primary education in Uganda consists of seven years of schooling, which may be followed by four years of junior high school, two of senior high school, and three of tertiary education. National law stipulates that children should begin primary education at the age of six years. However, nearly one third of children aged six years delay school entry by at least one year, and grade repetition is not uncommon (Uganda Demographic and Health Survey (UDHS), 1996). As a result, the age of children attending primary school ranged between six and 23 years in 1996.

Before the Ugandan universal primary education (UPE) policy was launched, public primary schools had relied financially primarily on parental contributions in the form of tuition and parent teacher association (PTA) fees. The PTA was the main decision making body at school level and parental contributions were the primary source of school income, which accounted for over 60% of total school expenditure[2]. Central government made three types of financial contribution to public primary schools. First, the Ministry of Public Service paid teachers' salaries directly into their individual bank accounts or through district education officers or head teachers. Second, it paid capitation grants to finance instructional materials and other non-wage expenditure[3]. In 1991, this per capita grant was set at a nominal rate of USh2,500 per enrolment in grades 1 to 3 and USh4,000 per enrolment in grades 4 to 7. Third, the government was responsible for financing school rehabilitation. The source of the remaining school income was parental contributions in the form of PTA levies, top-ups for teachers' salaries, and tuition fees.

According to the Uganda Integrated Household Survey of 1991, the average household expenditure for primary education was approximately Ush16,000 (approximately US$5 based on an exchange rate of USh3287=US$1) per student, which

[2] According to Reinikka and Svensson (2004), this relative size of the parental contribution to school income is likely due to the fact that the majority of capitation grants were captured by local government officials.

was 2% of the average annual household expenditure. These costs precluded many children from receiving primary education. Furthermore, over 70% of children aged five to 12 years who either never attended or dropped out of primary school attributed this to the costs (Deininger, 2003).

3.1.2 Primary Leaving Exam

In terms of primary school completion, Ugandan students are required to pass an exit exam, namely the Primary Leaving Exam (PLE). This exam has been administered annually by the Uganda National Examination Board (UNEB) since 1966, and all students in grade 7 are eligible. The PLE score is used for admission to public secondary schools, and is therefore a requirement for students who wish to pursue post-primary education.

In 2013, all candidates were required to take four mandatory subjects, namely Mathematics, English, Science, and Social Studies. Each subject carries the same weight and is graded on a nine point scale, with top performers receiving one point and the poorest nine points. The aggregate score for the four subjects is used to determine whether a student passes or fails[4] and to classify successful candidates into four groups, namely

[4] The minimum requirement to meet the national standard is Division IV. In 2013, the pass rate was approximately 90% (Ministry of Education and Sport (MOES), 2014)

Divisions I, II, III, and IV[6]. Top students obtain a total of four points and belong to Division I. The selection of students for public secondary schools is mainly based on this aggregate PLE score.

3.1.3 Admission process to secondary schools

The admission of students to public secondary schools follows a specific pattern, as follows (Gould, 1974):

Period 1: Students submit to the UNEB an ordered list of up to four preferred schools on the PLE entry form by the end of May.

Period 2: Students take the PLE, usually in November.

Period 3: Students are assigned to secondary schools based on their PLE scores and preferences at a national meeting of head masters.

Most traditional and prestigious public secondary schools set limited quotas, and more students generally wish to enroll in these. In such cases, the selection of students proceeds as follows:

Step 1: The preferences and PLE scores of the students are collected.

Step 2: Students are ordered in terms of their PLE scores.

Step 3-1: Beginning with the top-ranked student, each is assigned a place in his/her

[6] An average total test score of 25 places a student in Division III.

preferred secondary school.

Step 3-2: Once all places in a certain school are taken, that school is removed from assignation process.

According to Liang (2002), cut-off points for elite public secondary schools are generally below six points, and almost 40% of PLE candidates have the opportunity for post-primary education. In contrast to public schools, private secondary schools have varying admission processes. However, in general, they admit students who can pay the fees.

3.1.2 Universal primary education policy

3.2 Secondary education system in Uganda

As mentioned above, Uganda offers four years of lower secondary education after seven years of primary education. In 2006, approximately 64% (404,935) of children completed primary education, whereas only 37% (166,372) completed lower secondary education[12]. These statistics suggest that improving access to secondary education is the logical next goal for the government.

[12] Author's calculation using the population aged between 20 and 25 years in the Uganda National Household Survey (UNHS) of 2006.

The size of the private sector in secondary education is substantial compared to that in primary education. Total private school enrolments accounted for approximately 57% of secondary students in 2000, but only 23% of primary students (Liang, 2002). Among the three possibilities for secondary school by ownership, namely government, private, and community, the distinction between private and community is unclear, as neither receive any government subsidy. Therefore, this dissertation describes both private and community secondary schools as "private."

Before the universal secondary education (USE) policy was introduced in 2007, public secondary schools had similar financing schemes as those described above for the primary education sector. Public secondary schools received government subsidies in two forms, namely teacher salaries and capitation grants. The level of the per capita grant was set by the MOES, and was USh65 per student per day in 2002 (Liang, 2002)[13]. The per student government subsidy for secondary education was US$148, far higher than that for primary education (US$22), suggesting that financing secondary education is costlier for the government.

Families bore almost half of secondary school costs in the form of PTA levies, tuitions fees, boarding fees, and other expenses. According to Liang's (2002) calculation,

[13] Given a secondary school operate 9 months annually (270 days), this amount is about USh 17,550 per student per year.

the average personal cost of having a single household member in secondary school was about USh240,000 per year. Given that 47% of Ugandan households earned less than USh620,500 annually in 2002, this personal cost accounted for over 40% of that amount.

In terms of the completion of lower secondary (Ordinary level (O'level)) education, students are required to take the Uganda Certificate of Education (UCE) exam after the completion of 11th grade in order to be eligible for higher secondary (Advanced level (A'level)) education.

Candidates enrolled in 11th grade must be registered for the UCE exam by the head teacher of the school they attend. The UNEB also allows students to take the UCE exam at public centers in special cases, such as if a school lacks the capacity to manage the UCE exam. However, almost all candidates sit in the exam at the school they attend.

Candidates must take at least eight but not more than ten subjects. As of 2006, these must include five compulsory subjects, namely English, Mathematics, Biology, Chemistry, and Physics. Depending on performance, the UNEB grades each student for each single subject on a scale of 1 to 9[14], where 1 indicates a "pass with distinction." The classification of successful candidates is based on the aggregate grade for the best eight subjects. For example, in Division 1, the best performing cohort, are candidates with an

[14] The grading for each subject out of a total of 100 full marks is as follows: 90-100=1; 80-89=2; 70-79=3; 65-69=4; 60-64=5; 50-59=6; 45-49=7; 35-44=8; and 0-34=9.

aggregate of 32 or less.

Chapter 4 of this dissertation uses school and district level data on the number of UCE exam candidates to determine the number of secondary school graduates, and their average test scores to measure their academic achievement.

3.3 Free Education policies in Uganda

3.3.1 Universal Primary Education policy

In December 1996, the year of the first presidential election, the government announced the introduction of the universal primary education (UPE) policy, and implementation followed in January 1997 (Bategeka, 2005). Aimed at providing universal access to primary education, the UPE policy abolished tuition and PTA fees in public primary schools. Initially, only four children per household were eligible for this cost reduction. However, the government expanded eligibility to all children in 2000[15]. Uganda was one of the first African countries to introduce free primary education in the 1990s. Malawi did so in 1994, Uganda in 1997, Lesotho in 2000, and Kenya in 2003.

Under the UPE policy, government subsidies to a UPE school comprise mainly

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[15] It would be of interest to determine whether this expansion had any additional impact over and above that of the initial introduction of the UPE. However, in the present data, even the youngest cohort was not exposed to the full coverage UPE. Thus, it is beyond the scope of this study to assess the impact of the full UPE policy on those who entered primary school after 2000.

three forms, namely teacher salaries, UPE capitation grants, and school facility grants. UPE capitation grants were intended to cover tuition and PTA fees, and to shift the parental burden onto the government. The level of the per capita annual grant was fixed by the Ugandan MOES at approximately Ush5,000 (about US$3) per enrolment for grades 1 to 3, and USh8,100 (about US$4.75) per enrolment for grades 4 to 7 (Essama Nssah, 2011). The MOES also contributed school facility grants to assist public primary schools to build and maintain school infrastructure in the neediest communities.

Parental contributions under the UPE policy fell dramatically, as tuition and PTA levies were covered by the government subsidy[16]. This nationwide education subsidy program effectively boosted gross primary enrollment from 70% in 1996 to 120% in 2009 (UNESCO, 2014), without even making primary education compulsory.

Some early studies examined the impact of the UPE policy in Uganda by comparing the educational attainment of children who were exposed to it with that of those who were not (Deininger, 2003; Grogan, 2009; Nishimura et al., 2008). Such research found that the introduction of the free primary education policy increased primary school attendance, timely entry into grade 1, and the probability of grade completion, and also reduced gender inequality. These findings are consistent with those

[16] However, primary education was not completely free, as parents still had to shoulder other schooling costs, such as uniforms and textbooks.

of studies conducted in neighboring countries that implemented free primary education, such as Malawi (Al-Samarrai and Zaman, 2007) and Kenya (Bold et al., 2010). However, these early studies depended on the strong assumption that nothing other than free primary education policy affected those concerned during that time.

The only available quasi-experimental studies examining the effects of free primary education on education outcomes in Africa were those of Lucas and Mbiti (2012a, 2012b), who found that, after controlling for other time trends, free primary education increased the number of primary school graduates and gender gap, with little effect on their academic achievement.

A few studies further investigated the effects of free primary education on the fertility and health outcomes among females in Malawi (Behrman, 2015), Uganda (Behrman, 2015; Keats, 2014), and Nigeria (Osili and Long, 2008). These studies showed that the probability of an adult woman testing positive for HIV was lower among those exposed to free primary education (Behrman, 2015),and that an increase in female education reduced early pregnancy (Keats, 2014; Osili and Long, 2008) and increased health investment in children (Keats, 2014).

In summary, a number of studies have examined the effects of free primary education on educational outcomes, but evidence using a quasi-experimental design remains scarce. Furthermore, only a limited number of the studies examined the effects on fertility and child health. Chapter 5 examines the effects of the UPE policy in Uganda in a naturalistic experiment to determine whether an increase in female education reduces adolescent pregnancy and child mortality.

3.3.2 Universal secondary education policy

Aiming at improving access to secondary education, the Ugandan government announced the introduction of the USE policy in 2005, and implemented it in February 2007[17]. This policy allows eligible students to attend government USE secondary schools without paying school fees or PTA fees. Although most government schools were listed as USE schools from the beginning, some stayed out of the program during the initial stage. For example, in 2007, only 791 out of 845 government aided schools benefited from the first phase of the program, but the number of USE government secondary schools ultimately increased to 1,024 in 2012 (Barungi et al., 2014).

The benefit of the USE policy applied only to students who sat the PLE in 2006 or after, and students must attain an aggregate score of 28 or below[18]. According to the

[17] In January 2006, the Education Minister clarified that the USE policy would begin in February 2007 (New vision, 2006).
[18] PLE candidates must take four core subjects, namely English, Mathematics, Integrated Science, and Social Studies and Religious Education. Each subject is graded on a scale of 1 to 9, where 1

UNEB, about 70-80% of PLE candidates in 2006 were eligible to study at secondary schools without paying tuition.

Government subsidies to USE public secondary schools mainly take the form of teacher salaries, capitation grants and infrastructure subsidies. Government secondary schools receive USh41,000 per student per term to cover tuition fees (Daily Monitor, 2016).

Before the USE policy started, parents paid approximately USh126,000 (approximately US$38 based on an exchange rate of USh3344=US$1) per year per secondary student[19]. This payment was a major constraint on access to lower secondary education in Uganda[20]. The introduction of the USE policy was expected to break this barrier, and MoES reported that the total enrolment in lower secondary education improved from 814,087 in 2006 to 1,362,739 in 2013.

In addition, a key feature of this policy was that it was implemented through PPPs. Under this scheme, an owner of a private secondary school can enter a partnership

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indicates a "Pass with distinction." Classification of successful candidates is by an aggregate score of these four core subjects. The best performing cohort, Division I, includes students whose aggregate scores are 12 or lower. Division II includes those with aggregate scores of 24 or lower. Divisions III and IV do not have minimum aggregates, but require students to score at least a pass with a score of 8 in three subjects. All candidates in Divisions I, II, II, and IV pass the PLE.

[19] Author's calculation using UNHS (2006) data. School fees in private schools are higher than those in government schools. The former charges USh156,100, and the latter charges USh103,100, on average.

[20] According to UNHS (2006) data, 76% of children who drop out after completing P7 attribute this to the high cost of O'level education.

with the government and enroll eligible students for lower secondary education[21]. According to Barungi et al. (2014), 90% of PPP private schools chose to apply for their PPP, and a few were approached by the MOES to be USE private schools. Most PPP private secondary schools reported applying for the program in order to have access to government funding and material support.

The requirements for a private school to participate in the USE policy are as follows (Barungi et al., 2014):

1. Schools must charge less than USh75,000 per term per student.

2. Schools must have or establish a Board of Governors to manage the implementation of the USE program.

As a result, not only government schools[22], but also approximately 50% of private schools currently implement the USE policy in Uganda.

PPP participating private schools receive the similar government subsidies, namely USE capitation grant, and material support from the government, but teacher's salary is not covered. The rate of per capita grant is fixed at USh 47,000 per term per student eligible under USE policy to cover non-boarding fee. PPP private school also can

[21] MOES declared that schools charging less than UGX75,000 per term per student were eligible to participate in the USE program.
[22] Approximately 24% of government schools are non-USE schools.

enroll non-USE (private) students for fees. In addition, by 2012, about 64% of PPP participating private schools had received instructional materials, such as textbooks.

In contrast to the UPE policy, only one study by Asankha and Yamano (2011) has ever assessed the effects of the USE policy. These researchers studied repeated cross sectional data collected in 2005 and 2009 to determine whether the probability of secondary school enrolment increased after the USE policy was introduced, finding that the policy increased public secondary school enrolment, especially among girls from poor households. However, their study relied on the strong assumption that there no factors other than the USE policy affected secondary school enrolment at the time, potentially failing to isolate policy effects from other trends over time.

Chapter 4 of this dissertation fills this gap in the literature by employing a quasi-experimental design to determine whether the USE policy increased the number of secondary school graduates, and affected their academic achievement.

Chapter 4: Short Run Effects of Free Secondary Education Policy on Access, Sorting, Learning Environment and Achievement in Uganda

4.1 Introduction

As mentioned above, improving access to secondary education is a key to economic development and poverty eradication in Africa. Literature in developing countries generally attributes limited access to basic education to its high costs, such as the costs of tuition, uniforms, and transportation. To break such constraints, the Ugandan government instituted the USE policy, which abolished school fees in participating public and private secondary schools, and was the first to do so in sub Saharan Africa in the 21st century. Some other African countries have implemented similar policies, such as Malawi, Kenya, and Tanzania, and others are also interested in introducing such a policy in order to sustain educational development (Lewin, 2009). A thorough understanding of the positive and negative effects of such a policy on access and achievement will allow policy makers to implement such changes appropriately. In this chapter, I assess the effects of Uganda's USE policy on access and achievement among 11th grade students (i.e., the final year of lower secondary school (O'level)). This chapter also examines changes in school resources, such as teachers and facilities, and the general equilibrium effects on the supply of private secondary schools.

As mentioned above, Ugandan government abolished school fees in participating public and private secondary schools in February 2007, preceded by the introduction of the UPE policy in 1997. Despite the radical fall in direct schooling costs for secondary education, its price elasticity and the effects on student access remains unclear. One reason for this may be the high opportunity cost for boys who can work to earn a wage outside of school. If parents think that working in the labor market may be more profitable than attending secondary school, a free secondary education policy may have little effect. Another reason for a lack of clear effects among girls may be the cultural tradition for approximately 50% of girls to get married before the age of 18 years (UDHS, 2006). Again, parents may be unwilling to invest in their daughter's secondary education, regardless of its price, if they cannot reap the benefits.

Furthermore, even if a free secondary education policy boosts enrolment in secondary schools, participating secondary schools may face challenges in accepting a large number of students with a limited number of teachers and limited facilities. Without increasing the available resources, a single classroom and teacher may be expected to accommodate too many students. Indeed, Chapman et al. (2010) indicated that teachers were concerned about heavy workloads after the USE policy was implemented, and De Jaeghere et al. (2009) report that one in four head teachers considered other head teachers to be unprepared to implement the USE policy. Decreased quality of the learning environment may lead to sacrifices in terms of students' learning achievements, allowing them to "attend school without learning."

In addition, such a nationwide education policy may have general equilibrium effects on the education market. For example, an increased number of new entrants to secondary schools may lead to congestion in public schools, and facilitate the demand for private schools with better learning environments. Indeed, Lucas and Mbiti (2014) found that Kenya's free primary education policy increased the number of private primary schools operating in the market. Such effects would be reflected in the number and composition of secondary school exit exam candidates and their scores.

This chapter assesses the effects of Uganda's USE policy by exploiting cross-cohort differences in exposure to the policy and cross-district differences in the effective benefits of the policy in terms of varying numbers of pre-program dropouts following primary school graduation. Intuitively, the USE policy should increase the number of secondary school graduates by less in districts where no one attended secondary school before the policy due to major remaining barriers to even primary education access, itself not affected by the USE policy. Furthermore, the policy is likely to have only minor impact where most primary school graduates also completed lower secondary school even before the policy, as school fees were not a major barrier for them. On the other hand, in districts where many students graduated primary school but dropped out before completing 11th grade, the USE policy ought to lead to more students completing lower secondary education. Under this assumption, I identify the effects of the USE policy by employing a difference in differences (DID) approach, comparing changes in educational outcomes in districts with many pre-program dropouts to those with few pre-program dropouts.

The results suggest that the USE policy successfully allowed larger numbers of students to complete lower secondary education in both public and private secondary schools. However, the changes in their learning achievement varied by school type, with graduates of public schools maintaining their pre-program performance levels while the average scores of private secondary school graduates decreased. The analysis of effects on school quality suggests that the number of teachers and school facilities increased in participating secondary schools in accordance with the induced demand for school resources. Finally, the results suggest that the USE policy had few negative effects on UCE exam scores of 11th grade students in all five core subjects.

The rest of this chapter continues as follows: section 4.2 discusses the data and identification strategies employed in this part of the study, section 4.3 presents the results, and section 4.4 offers conclusion and mentions certain policy implications.

4.2 Data and Identification

4.2.1 Data

In this chapter, I assess the effects of Uganda’s free secondary education policy on the basis of three unique data sets.

1. Uganda Certificate of Education

Firstly, I evaluate whether the introduction of the USE policy increased the number of secondary school graduates, and/or changed their learning achievement using district level data on the UCE exams in the years from 2006 to 2012. These data reveal the number of students who took the UCE exams and their average scores in the five core subjects (English, Mathematics, Biology, Chemistry, and Physics). As the UNEB registers all UCE exam candidates every year, this data offers the most reliable measure of the number of students completing lower secondary education. Furthermore, the rich data on test scores allows an assessment of whether the introduction of the USE policy has affected student’s learning performance.

Figure 4-1 shows the change in the number of students who took the UCE exam between 2006 and 2012. The first USE treated cohort took the PLE in 2006 and entered the first grade of secondary school, i.e., 8th grade, in 2007. Thus, they completed 9th grade in 2008, 10th grade in 2009, 11th grade in 2011, and took the UCE exam in that last year. In contrast, the UCE test takers of 2009 and earlier took the PLE in 2005 and earlier, before the advent of the USE, thus were untreated[23]. As can be seen in the Figure, the number of candidates increased dramatically in 2010, suggesting the significant USE policy effects on access to lower secondary education.

Although the UCE exam data provide rich information on learning outcomes by which to assess the effects of the USE policy, this policy should affect learning outcomes through changes in their learning environment, such as the number of teachers per student. In order to recognize such characteristics at each school in certain year, I used data from the Annual School Census (ASC).

2. Annual School Census

The ASC, conducted by the Ugandan MOES, gives detailed data on the number of teachers and their qualifications, number of classrooms, amount of seating, and administrative school information for each school. While access to such information since

[23] It may be suggested that the cohort who entered secondary education in 2006 and took the UCE in 2009 are also partially affected by the USE policy, as the government announced the implementation of the USE policy in the middle of 2005. For example, forward looking parents may have taken into account this anticipated USE policy. This would lead to an increase in the number of UCE candidates in 2009, leading to an underestimation of the policy effects, providing the lower bound of the true magnitude of policy effects. Furthermore, a concern may be that some students who took the PLE in 2006 may have delayed entry into secondary education by one year. However, the government announced the UCE policy in January 2006, after admission decisions had already been made. Thus, a decision to delay entry would have been costly. Indeed, as Figure 4-1 shows, the number of students who delayed entry, if any, is likely to be small.

2001 would be ideal, the MOES has only conducted the ASC since 2006. Thus, I merged the UCE data with the ASC data between the years of 2006 and 2012. Another important feature of the ASC is that the response rate from government schools is almost 100%, as answering the ASC is a condition upon which USE schools receive capitation grants from the government. However, the response from private schools is approximately 60%, according to the MOES. In order to avoid the issue of non-random selection in terms of ASC data from private schools, I focus mainly on government[24] secondary schools when assessing the effects of the USE policy on school characteristics. However, I present the results of the same analysis using private school data so as to provide suggestive evidence. Table 4-1 provides the summary statistics of the UCE and ASC data.

3. Uganda Housing and Population Census

In addition, the Uganda Housing and Population Census (UHPC) of 2003 allowed the measurement of regional variation in the effective benefits of the USE policy based on the number of post-7th grade dropouts in each district. The UHPC offers nationally representative cross sectional data, and the calculation and measurement of program intensity on the basis of these data is discussed in the following section.

[24] Among government schools that administered the UCE exam, 93% were USE schools, whereas only 30% of the private schools were.

4.2.2 **Identification**

In order to isolate the effects of the USE policy from other unrelated trends over time, and to assess whether the introduction of the USE policy allowed more students to complete lower secondary education, this study employed the DID approach by exploiting two variations in the effective benefits of this policy. The first difference entails the varying levels of exposure to the USE policy by test candidate cohorts in each year. During the surveyed period from 2006 and 2012, 11th grade students who took the UCE exam in 2010 and later are taken as the treated group, as discussed in the preceding section.

The second variation in the DID approach is the district level program intensity, for which Intensity_{dt} is derived from the number of post-primary school dropouts in the UHPC (2003). Equation (8) below is the model to be estimated, with Intensity_{dt} a time variant intensity measure with regional variation. It takes the value of 0 between the years of 2006 and 2009 because 11th grade students in those years were untreated by the USE policy, and it takes the potential proportional increase in 11th grade graduates for the years 2010, 2011, and 2012. Thus, Intensity_{dt} is defined as follows.

$$\text{Intensity}_{dt} \begin{cases} =0 & \text{for } t=2006,\ 2007,\ 2008,\ \text{and } 2009 \\ =(\text{N7}_{d} - \text{N11}_{d}) / \text{N11}_{d} & \text{for } t=2010,\ 2011,\ \text{and } 2012 \end{cases}$$

N7_{d}: Number of individuals interviewed for the 2003 UHPC who completed 7th grade in district d

$N11_d$: Number of individuals interviewed for the 2003 UHPC who completed 11th grade in district d

For the numerator, by taking the difference between the number of people who completed 7th grade and 11th grade in each district, I calculated the number of people who entered lower secondary education but dropped out before completing 11th grade in each district, and I regard these dropouts as the main beneficiaries of free secondary education, who potentially become able to complete 11th grade thanks to the free secondary education policy. One caveat of using this measure as one of intensity is that larger districts with larger populations potentially have larger numbers of dropouts, and therefore intensity. To standardize this measure by district size, I divided it by the number of 11th grade graduates in the same district in the 2003 Uganda Housing and Population Census (UPHC) (see Figure 4-2).

Intuitively, this innovative policy should allow more students to complete lower secondary education in districts where many students complete primary school but drop out before completing 11th grade. On the other hand, such a policy might have little effect in districts where no one attends secondary school, irrespective of the USE policy, because major barriers remain for access even to primary education. Furthermore, the effect is likely to be small where most primary school graduates also complete secondary school, irrespective of fee abolishment. Appendix Table 4-2 describes the calculation of these intensity measures in a district with median intensity, namely Iganga.

Ideally, the 2006 data should be used to measure the number of dropouts just before the USE policy started, but as the closest approximation, I rely on the 2003 UPHC to calculate this intensity measure. Thus, this strategy assumes that this relative variation in the cohort size of 7th and 11th grade graduates across districts remains the same over the four years between 2003 and 2007[26]. Figure 4-3 shows the positive correlation between the district intensity measure and a change in the number of UCE test candidates between 2006 and 2010.

Using this intensity measure, I derived the following equation (8):

$$\mathrm{Y_{dt}} = \beta_0 + \beta_1\ \mathrm{Intensity_{dt}} + \beta_2 \mathrm{X_{dt}} + \lambda_\mathrm{d} + \lambda_\mathrm{t} + \epsilon_\mathrm{dt} \quad --- (8)$$

$\mathrm{Y_{dt}}$ is the outcome in district d in year t. This set includes the number of students who took the UCE exam, and the average score for the secondary school exit exam. $\mathrm{X_{dt}}$ is the time variant district characteristics, including age-district cohort size[27]. λ_d is district

[26] The UNHS (2006), with a far smaller number of observations, verifies that the regional variation in the intensity measure across districts did not change significantly from 2003 to 2006.
[27] This variable is calculated using the 2003 census data. By assuming that the relative cohort size across districts remains stable over seven years, I calculated the number of children aged 10 years in each district as a proxy for the cohort size aged 17 (school entry at age 6+11 years) in 2010. In the same manner, I calculated the age-district cohort size for test candidates in the other test years.

fixed effects, λ_t is year fixed effects, and the error term ϵ_{cdt} is clustered at district level, as Bertrand et al. (2004) suggest.

This intensity measure is likely to be valid for the analysis when the policy effects are observed for 2010 and later. Thus, I used this intensity measure when examining the effects of the free secondary education policy on the test taking cohort size, and their academic achievement. However, the USE policy affected certain outcomes, such as the number of secondary school enrolments, the number of public and private schools, and the quality of the learning environment, soon after its introduction in 2007. When assessing the effects on such outcomes, I used the variant of the abovementioned intensity measure. Figure 4-3 describes this idea. For example, when assessing the effects on 8th grade enrolments, I used the following time variant intensity measure.

$$\text{Intensity}'_{dt} \begin{cases} =0 \text{ for } t=2006 \\ =(N7_d - N8_d)/(N8_d) \text{ for } t=2007, 2008, 2009, 2010, 2011, \text{ and } 2012 \end{cases}$$

$N8_d$: Number of individuals interviewed for the 2003 UHPC who completed 8th grade in district d

Firstly, $\text{Intensity}'_{dt}$ takes the value of 0 in the year 2006, as all grades in all schools were unaffected by the USE policy. However, in 2007 and later, it takes the value of the potential proportional increase in the 8th grade cohort in each district, as the first

USE treated cohort entered the first year of secondary school, i.e., 8th grade, in 2007[28]. I followed the same strategy in the analysis of the effects on 9th, 10th, and 11th grade enrolment[29].

When assessing whether the introduction of the USE policy increased the total enrolment in lower secondary school over the 8th to 11th grades, I used $\text{CumulativeIintensity}_{dt}$, which is defined as follows:

$$\text{CumulativeIintensity}_{dt} \begin{cases} = 0 \text{ for t=2006} \\ = (N7_d - N8_d) / (N8_d + N9_d + N10_d + N11_d) \text{ for t=2007} \\ = [(N7_d - N8_d) + (N7_d - N9_d)] / (N8_d + N9_d + N10_d + N11_d) \text{ for t=2008} \\ = [(N7_d - N8_d) + (N7_d - N9_d) + (N7 - N10_d)] / (N8_d + N9_d + N10_d + N11_d) \text{ for t=2009} \\ = [(N7_d - N8_d) + (N7_d - N9_d) + (N7_d - N10_d)] + (N7_d - N11_d)]] / (N8_d + N9_d + N10_d + N11_d) \text{ for t=2010, 2011, and 2012} \end{cases}$$

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28 Intensity=(potential increase in 8th grade)/(pre-USE 8th grade cohort size)

29 In the analysis of the effects on 9th grade enrolment, Intensity_{dt} takes the value of 0 between 2006 and 2007, and the potential proportional increase in 9th grade in 2008 and later. Similarly, in the analysis of the effects on 10th grade enrolment, Intensity_{dt} takes the value of 0 between 2006 and 2008, and the potential proportional increase in 10th grade in 2009 and later. Lastly, in the analysis of the effects on 11th grade enrolment, Intensity_{dt} takes the value of 0 between 2006 and 2009, and the potential proportional increase in 11th grade in 2010 and later.

$N9_d$: Number of individuals interviewed for the 2003 UHPC who completed 9th grade in district d

$N10_d$: Number of individuals interviewed for the 2003 UHPC who completed 10th grade in district d

$CumulativeIintensity_{dt}$ takes the same value as in the previous analysis for 2006. In contrast, in 2007, $CumulativeIntensity_{dt}$ takes the potential proportional increase in 8th grade compared to pre-program total enrolment. In other words, its numerator is the potential absolute increase in the 8th grade cohort, but divided by the sum of the pre-USE cohort size in 8th, 9th, 10th, and 11th grades[30]. In 2008, its numerator is the sum of the 8th grade potential increase and the 9th grade potential increase, holding the denominator as pre-program total enrolment size. Then, the same strategy is employed to calculate the district level cumulative intensity in 2009, 2010, and later. Table 4-2 describes the calculation of these intensity measures in a district with median intensity, namely Iganga.

In summary, I used these regional variations in the effective benefits of exposure to the USE policy in and across cohort variation to examine policy effects on access, private school supply, learning environment, and achievement. The results are discussed

[30] Intensity=[(potential increase in 8th grade)+(potential increase in 9th grade)]/[(pre-USE 8th grade cohort size)+(pre-USE 9th grade cohort size)]

in the following section.

I further examined the heterogeneous impact of the USE policy by disaggregating observations into government and private schools, and estimate the following equation (9):

$$Y_{cdt} = \gamma_0 + \gamma_1(\text{Intensity}_{dt} \times \text{Public}_c) + \gamma_2(\text{Intensity}_{dt} \times \text{Private}_c) + \gamma_3 X_{dt} + \lambda_c + \lambda_d + \lambda_t + \mu_{cdt} --- (9)$$

where Y_{cdt} is the outcome of type c (public USE, public non-USE, private USE, private non-USE), in district d, in year t, λ_c is school type fixed effects, Public_c takes the value of 1 if the school i in type c is a public school, whereas Private_i takes the value of 1 if the school is a private school. In order to further examine the heterogeneous effects in terms of school USE status, I also disaggregated schools into USE and non-USE schools.

Then, by limiting the sample to public and private schools, respectively, I estimated the following equations (10) and (11):

$$Y_{cdt} = \gamma_0 + \gamma_1(\text{Intensity}_{dt} \times \text{Public}_c) + \gamma_2(\text{Intensity}_{dt} \times \text{PublicUSE}_c) + \gamma_3 X_{dt} + \lambda_c + \lambda_d + \lambda_t + \epsilon_{cdt} --- (10)$$

$$Y_{cdt} = \gamma_0 + \gamma_1(\text{Intensity}_{dt} \times \text{Private}_c) + \gamma_2(\text{Intensity}_{dt} \times \text{PrivateUSE}_c) + \gamma_3 X_{dt} + \lambda_c + \lambda_d + \lambda_t + \epsilon_{cdt} --- (11)$$

where PublicUSE_c takes the value of 1 if the school type c is public and a USE school, whereas PrivateUSE_c takes the value of 1 if the school type is private and a USE school.

Lastly, this chapter also examines changes in the number of students taking the UCE exam at school level by calculating the variant of equation (8) as follows:

$$Y_{icdt} = \gamma_0 + \gamma_1 \text{Intensity}_{dt} + \gamma_2 X_{dt} + \eta_i + \lambda_t + \mu_{icdt} - - - (12)$$

where Y_{isdt} is the outcome of school i, in type c, in district d, in year t, and η_i is school fixed effects. γ_1 indicates the change in the test taking cohort size at each school. If it is positive, this suggests that one school registered more 11th grade students than pre-program levels.

Table 4-3 shows the results of OLS regression, which regresses the intensity measures on district level characteristics using the data from the UHPC of 2003. The results suggest that high intensity districts are more likely to have populations with high educational attainment and more valuable assets. In the following sections, I determine whether the USE policy improved students' access to secondary school and harmed academic achievement more in such districts.

4.3 Results

The objective of this section is six-fold. First, section 4.3.1 studies whether free secondary education increased the number of secondary school graduates. Secondly, I examine changes in secondary students' characteristics in section 4.3.2. Third, section 4.3.3 tests whether the USE policy increased the number of schools in the market. Fourth, the effects of the policy on school quality, in terms of physical and human resources available per student, is examined in 4.3.4. Fifth, changes in average 11th grade test scores are discussed in section 4.3.5. Lastly, all the estimates are submitted to a robustness check in section 4.3.6.

4.3.1 Effects on access

This section examines whether Uganda's free secondary education policy increased the number of students who took the UCE exam. Simple supply and demand analysis predicts that a fall in the price of education in participating secondary schools would fuel the demand for secondary education. Hence, the USE policy was expected to increase the number of secondary school graduates.

Indeed, Table 4-4 shows significant positive effects of the USE policy on the number of students who took secondary school exit exam. Using the value of intensity in median districts, 0.328, the results in Column 1 suggest that, in Iganga, the number of UCE candidates increased by an average of 869 students, of which 112 (=345*0.328) were from government secondary schools and 757 from private secondary schools, in 2010 as opposed to 2009 and earlier. This increase in the number of candidates from private schools was significantly different from zero, whereas that in government school was imprecisely estimated. The size of the increase is as large as approximately 18% relative to the number of 11th grade graduates in 2009. This analysis suggests that free secondary education was effective in making secondary school accessible. However, the results in Column 1 mask whether the number of students increased in USE participating secondary schools.

In order to examine whether the effects differed across school type, Column 2 disaggregates both government and private schools into USE and non-USE participating schools. The results show that the effects are heterogeneous. Among the government secondary schools, the number of lower secondary school graduates increased only in USE participating government schools by 186 [=(-223+791)*0.328] in median districts, whereas that in non-USE schools fell slightly by an average of 73 students. In other words, the number of graduates increased only where the USE policy made schooling less costly, as expected.

In contrast, amongst private schools, the number of UCE candidates increased not only in USE participating (PPP) private schools, by an average of 117 [=(1950-1592)*0.328], but also in non-USE participating private schools, by an average of 640 (=0.328*1950) in median districts. In short, the results in Table 4-4 suggest that the free secondary education policy effectively increased the number of lower secondary school graduates in the districts with high intensity, and improved access to both participating government and private secondary schools, and surprisingly also to non-PPP private schools.

The change in the number of UCE candidates in non-participating private secondary schools may be due to two reasons. First, an increase in the demand for secondary education in participating secondary schools may have exceeded the limited supply of secondary schools and other physical and human resources. Low cost non-participating private secondary schools may satisfy this demand. Secondly, as discussed in section 4.3.4, the quality of participating secondary schools may have suffered, as such schools became more accessible than they were pre-program. Changes in class size and student body composition may have encouraged middle class students to flee from participating secondary schools to non-participating private secondary schools. These interpretations will be further discussed below.

The results in Table 4-4 reveal that the number of UCE candidates increased at district level in both public and private secondary schools. I now focus on whether the USE policy changed the number of secondary school graduates at school level. As USE participating secondary schools abolished school fees and became more accessible, each participating secondary school would be expected to accept more students. However, the change in non-participating private secondary schools is unclear, because its price was unchanged by the USE policy, and the relative price, compared to participating schools, became even higher after USE implementation. If the number of students in traditional non-participating private secondary schools was maintained at pre-program level, the increase in the number of graduates in the private sector may be due to these students entering and graduating from new secondary schools established after the USE started.

To understand the school level changes that occurred in Uganda, Table 4-5 examines the changes in the test taking cohort size at school level[31]. The results show that the size of the test taking cohort remains at pre-program level in public schools (Column 1). However, among government schools, the size of the change differs in terms of USE status, and non-USE government secondary school had fewer test takers, by an average of 15 students per school (=-45.3*0.328) in the district with median intensity, whereas USE participating government secondary schools graduated more 11th grade students after free secondary education started, by 13.2 students [=(-45.3+85.5)*0.328] (Column 2).

On the other hand, the size of the test taking cohort shrank in private secondary

[31] In order to study the change in the same schools over time, I included school fixed effects as an explanatory variable, and thus the schools that entered the market after 2007 were excluded from this analysis.

schools (Column 1), and this fall was sharp in non-participating private secondary schools (Column 2). Taken together with the sharp increase in the number of secondary school graduates in private schools at district level, this implies that the new private secondary schools are likely to have entered the market after the USE policy started. This possible general equilibrium effect will be further discussed in section 4.3.4. Altogether, the introduction of the USE policy improved the access to government USE secondary schools by making each school accommodate more children. In contrast, it increased private secondary school enrolment possibly because new private schools entered the market.

Before concluding this sub section, Table 4-6 confirms the above finding by relying on a different data set, namely the ASC conducted by the MOES, which provides the number of enrolments in government secondary schools by grade. I expected that the increase in enrolments would be similar to that in UCE candidates. The results show that the number of 8th grade enrolments increased by an average of 253 (=2088*0.121) in median districts in 2007 (Column 1), that of 9th grade increased by 309 (=2553*0.199) in 2008 (Column 2), 10th grade by 269 (=1009*0.267) in 2009 (Column 3), and 11th grade by 227 (=693*0.328) in 2010 and later (Column 4).

Table 4-7 presents the change in total enrolment in government secondary schools between 8th grade and 11th grade by student type. The results in Column 1 with the value of cumulative intensity in Iganga show that total enrolment in public secondary schools increased by 196 (=5936*0.033) in 2007, 499 in 2008, 879 in 2009, and 1134 in 2010 and later. Once this total increase is disaggregated for students' gender, the results suggest that the free secondary education policy benefited female more than male students (Columns 2 and 3). This finding is in line with the empirical evidence from an earlier study in Uganda (Ashanka and Yamano, 2011).

Furthermore, amongst the total increase in enrolments, approximately 15% may be explained by an increase in orphan enrolment (Column 4). These results imply that the free secondary education policy made school accessible especially for students who were historically marginalized, and hence such a policy potentially addresses the domestics disparity in access to secondary education.

In summary, this section found that the USE policy in Uganda increased the number of 11th grade graduates in USE participating secondary schools and profit seeking non-USE private schools. Furthermore, females and orphans were more likely to benefit from the policy.

4.3.2 Effects on student's sorting

The preceding section showed that Uganda's free secondary education policy improved the access to secondary education in USE participating schools and non-participating private schools. A natural next question is whether such a policy promotes changes in student body composition across schools and in the composition of students taking the secondary school exit exam in each type of school. For example, if the USE policy makes government schools more accessible and hence congested, students from wealthier households may leave participating secondary schools and enter non-participating private schools in order to avoid the damaged learning environment. Another example of a possible composition changing mechanism may be based on competition, by which fee abolishment may make participating USE secondary schools more accessible, with more primary graduates applying for admission. In such a context, the owners of participating USE schools with limited available places may be selective, choosing applicants who perform well in the PLE. Hence, such competition-based composition changes may force less able students, who would have entered a public secondary school without the USE policy, to attend a non-participating private school, in which competition is less severe.

These changes in student body composition in participating secondary schools and non-participating private secondary schools may affect the average secondary school exit exam scores in each type of school. I further discuss this issue in section 4.4.5. In order to shed light on how such composition changes may occur, and how they may affect academic achievement, this section focuses on data of students attending the first year of secondary school, i.e., 8th grade, according to the Uganda National Household Survey (UNHS) of 2006 and 2012.

Table 4-8 shows the changes the characteristics of students attending the first grade of secondary education by school type. If participating secondary schools accepted applicants from less wealthy household after the USE policy, the level of per capita expenditure would fall in such a secondary school. As most public schools participated in the USE policy, I predicted that such a change was likely to occur in public secondary schools. However, the results in the first Column show that the per capita household expenditure is unlikely to have changed after the USE policy started. On the contrary, the results suggest that the average per capita household expenditure of those in private schools fell in the district with high intensity (Column 2).

These findings may imply that two possible changes occurred in the private sector. First, by developing partnerships between the government and private schools, low cost rural private schools participated in the USE and became accessible to children from less wealthy households. Secondly, as implied by section 4.4.1, the free secondary education policy may have allowed smaller non-participating private schools to enter the market to satisfy the demands of the students fleeing congested participating secondary schools. If the level of wealth of those students was indicated by a lower than average per capita household expenditure for children attending elite non-participating private secondary schools regardless of USE policy, the average per capita household expenditure of all students attending private schools after the USE policy would become lower.

Columns 3 and 4 use educational attainment of the household head as a proxy for a student's innate ability, to account for change in the ability of students attending each type of school. The results show that the average educational attainment of the household heads of students attending private school became shorter in the district with high intensity (Column 4), whereas this was more likely to remain at pre-program level in public schools (Column 3). These estimates suggest that, under the USE policy, private secondary schools may have enrolled students who were less able than incumbent students, and such a change in peer quality may have harmed the academic achievement of students in private school.

In summary, these results suggest that the free secondary education policy changed the characteristics of students taking the secondary school exit exam in private schools. Two pieces of evidence discussed in this section might predict that average test scores in private secondary schools were likely to fall in the district with high intensity. This interpretation is further discussed in section 4.4.5.

4.3.3 Effects on school supply

In section 4.3.1, I showed that Uganda's free secondary education policy increased the number of secondary school graduates not only in public schools but also in private schools in the district with high USE program intensity. Furthermore, it was found that a given private school educated a similar number of students even after the USE policy started, implying that the large increase in secondary school graduates was due to the entry into the market of new private schools. In order to reveal the picture behind this change in the education market, this section examines whether private secondary schools become more accessible by increasing the number of private schools at the same time as the USE policy implementation.

Table 4-9 shows the change in the number of schools by type since the free secondary education policy started in Uganda. First, the total number of secondary schools increased in the districts where the USE policy enabled more students to enter secondary school (Column 1). This increase started in 2007, when the USE policy was implemented, and by 2012, approximately 7.3 private schools entered the market in the district with median intensity. However, the number of government schools remained at pre-program level over the years (Column 2). On the other hand, the number of private schools rose rapidly in the district where program intensity was high. Furthermore, once the private schools are disaggregated into PPP and non-PPP participating private schools, most of the increase is again explained by the entry of non-PPP participating private schools (Column 4 and 5).

These findings imply that fee reduction made USE participating secondary schools both attractive and congested in spite of limited supply, creating the excess demand for new schooling space, and thus profit seeking private secondary schools entered the market to satisfy this demand. Table 4-10 applies the same analysis as Table 4-8 with outcomes in terms of the distance between students' homes and the secondary schools they attended. According to these data, the average distance to a government secondary school did not change after the USE policy started, but that to private schools fell by approximately 4.2 (=-13*0.328) km in the district with median intensity. This finding is consistent with an increase in new private schools in the district with high intensity, suggesting that private schools became physically more accessible after the USE policy started.

The private sector sometimes plays an important role in expanding access to schools in developing countries. One typical example is the extensive voucher program introduced in Chile in 1980. This government program provided a voucher to any child, which allowed them to attend either a public or a private school. Under this scheme, both types of school received equal government subsidy per enrolled student. This innovative program increased new entrants into the market place, and private school attendance increased from 15% in 1980 to 42% in 2005 (Hsieh and Urquiola, 2006). I suggest that the rapid expansion of the private secondary school market in Uganda is in line with the Chilean experience.

4.3.4 Effects on learning environment

It was shown above that a given public secondary school enrolled a larger number of students after the free secondary education policy was initiated. In this context, without increasing the available school resources, such schools may have become congested and the learning environment may have suffered. To examine whether this hypothesis is plausible, this section assesses whether the government built more facilities and employed more teachers in the district in which the introduction of the USE policy increased the number of school enrolments. Comparing such a change in school inputs to the change in total enrolments allows a discussion of whether the policy reduced the school resources available per student, and/or harmed learning environment at government secondary schools.

Panel A in Table 4-11 shows the change in the physical resources available at USE participating public secondary schools after the USE policy started in 2007. The results show that the number of classrooms (Column 1) and seating (Column 3) increased more in the district with high program intensity. These results suggest that the government prepared additional seats by building new classrooms and also opening seating within existing classrooms in the district where enrolment increased dramatically. As a result, although the number of classrooms available per student fell slightly (Column 2), the number of seats per student remained at pre-program level (Column 4).

Panel B further shows the change in the quantity and quality of human resources available at USE participating government secondary schools. The results illustrate that schools employed new teachers (Column 1) and also trained existing teachers (Column 3) more intensively in the district with high intensity. As a result, despite the number of teachers per student slightly worsening (Column 2), the number of teachers with credentials per student improved (Column 4). Thus, the quality of instruction in participating public secondary schools was unlikely to suffer under the USE policy.

Panel C describes the change in the number of teachers leaving schools for various reasons, providing suggestive evidence on the effects of the USE policy on teaching body composition in Uganda. Column 1 shows that the number of teachers transferred out from participating public secondary schools increased significantly in the district with high intensity after the USE policy started in 2007. Most of these teachers left to other schools (Column 2), rather than due to death (Column 3) or retirement (Column 4).

Given that a number of private schools entered the market in such districts, these results may imply that a non-negligible number of teachers in public schools might have moved to newly opened private schools to avoid the expected heavy work load in public secondary schools under the USE scheme. Indeed, such an interpretation is in line with that of Chapman et al. (2011), who mentioned that teachers in participating public secondary schools in Uganda were concerned about the heavy workload after the USE policy started. If such sorting occurred, especially among experienced teachers which private schools are likely to demand, the quality of instruction in public schools may have been harmed, with students having to learn from younger and less experienced teachers. Thus, although the present data lacks such information, examining what types of teachers transferred, and to what school they transferred, may be of great interest for future study.

Furthermore, Table 4-12 shows the change in the learning environment in non-

USE participating government secondary schools. In contrast to the case of participating government secondary schools, changes in physical and human resources were not statistically significant (except for the number of teachers). This is consistent with the finding in section 4.3.1. that, in non-participating government secondary schools, the number of secondary school graduates changed only slightly after the USE policy started. Thus, if the academic achievement of UCE candidates in this type of school changed, if at all, it is likely to have been due to composition effects rather than class size effects.

Before concluding this section, I discuss the change in available school resources in private schools on the basis of the available data. In interpreting the results, readers should note that ASC response rates from private secondary schools are far lower than those from government schools, and results are thus sensitive to endogenous selection in answering the questionnaire. If response rates were higher from private secondary schools with efficient head teachers, and lower from those with less efficient head teachers, the results below are likely to provide lower bounds of true negative effects on the learning environment in private schools.

Table 4-13 shows the change in available school resources per school in PPP participating USE private secondary schools. Panel A shows that the change in the number of classrooms and seating space was qualitatively similar to that which occurred in USE participating public schools, but the size of the increase was smaller (Columns 1 and 3) despite a larger increase in enrolment, as shown in section 4.3.1. Columns 2 and 4 suggest that available physical resources per student in PPP participating private schools worsened more markedly than in government schools.

The results in Panel B are in line with these findings, with the number of teachers in PPP participating private schools increasing in the high intensity district but by less (Column 1), reducing the number of teachers available per student more dramatically than in participating public secondary schools (Column 2). These schools employed more teachers with credentials in such a district, but this increase is again relatively modest compared to that in government schools (Column 3 and 4). Given that these estimates provide only the lower bound of the true negative effects in private schools, the results imply that the introduction of the free secondary education policy is likely to have damaged the learning environment in PPP participating private schools when free secondary education started.

Panel C shows that outbound teacher transfers from PPP participating private schools was qualitatively similar to that from participating public secondary schools, and was also high in the district in which the total number of enrolments and of private schools increased rapidly (Column 1). Again, most such transfers were by teacher leaving to other schools (Columns 2-4).

Lastly, Table 4-14 presents the change in the learning environment in PPP non-participating private secondary schools. Although a number of new non-participating private secondary schools entered the market after the USE policy started (see section 4.3.3.), readers should note that this analysis limited the sample to traditional private schools that existed in 2006, as it included school fixed effects to determine the change in a given school over time.

The results show that the change in non-participating private schools was also qualitatively similar to that in USE participating secondary schools. One difference was that the number of seats per student fell significantly in non-participating private secondary schools (Panel A), although pre-program levels were far higher than in other school types. The human resources available per student fell quantitatively after the USE policy started (Panel B), and thus class size effects may have placed downward pressure on test scores in non-participating private secondary schools.

The outbound transfer of teachers was observed in non-participating private schools (Panel C). However, less than one sixth of such transfers are explained by teachers leaving to other teaching posts (Columns 2-4). This implies that teachers in non-participating private secondary schools were less likely to leave their schools, possibly because their workloads were lighter than those in USE participating schools.

In sum, this section has found that the free secondary education policy affected the learning environment differently across school types. The results suggest that, in participating secondary schools, both physical and human resources available per student were likely to be reduced after the policy started. In particular, the magnitude of these change was larger in PPP participating private secondary schools. Furthermore, in non-participating private secondary schools, the policy was likely to damage the learning environment. Given these findings, the following section examines the effects of the free secondary education policy on learning achievement among 11th grade students as reflected by secondary school exit exam scores.

4.3.5 Effects on achievement

This section examines the “average” scores for the secondary school exit exam to determine whether the free secondary education policy harmed students’ learning achievements. A free secondary education policy may reduce such an average score for two reasons.

Firstly, class size effects entail that the physical and human resources available per student may decrease if the policy increases the number of enrolments without simultaneously increasing school resources. Class size effects are likely to place downward pressure on average test scores, and according to above findings, such effects may have occurred in participating secondary schools and non-participating private secondary schools.

Secondly, composition effects follow from changes in the composition of the students taking secondary school exit exam after the USE policy started. If free secondary education makes secondary school accessible for students from less wealthy households, and their ability is positively correlated with the household wealth, their test scores are likely to be lower than the average before the USE policy started. Such composition effects also put downward pressure on the average score, as is likely to occur in participating secondary schools. Given that the average educational attainment of household heads became lower in private secondary schools (see section 4.3.2.), composition effects are more likely to have occurred in private secondary schools.

However, the direction of policy effects on average test scores in certain types of secondary school is inconclusive because the changes in learning environment and student body composition across schools occurred simultaneously. In other words, isolating the effects of the former change from those of the latter is potentially difficult in the context of this study. In such a context, Hsieh and Urquiola (2006) propose examining the change in the average test score of all students in one education market. In this way, this approach net out the change in average test scores derived from the student sorting across schools [33]. I begin by following this strategy, recognizing a district of Uganda as an individual education market. I then present the changes in average test scores for each district. If class size and composition effects occurred in Uganda, the average test scores are likely to have decreased in the high intensity district in which more children took the UCE exam after the USE policy was introduced.

Panel A in Table 4-15 shows the effects of free secondary education on the district level mean score for the secondary school exit exam by subject. The results in Column 1, with the value of median intensity 0.328, show that this innovative policy reduced the average test score in English by 0.2 standard deviations (=-0.62*0.328) in the district with median intensity. The results in Mathematics, Physics, Chemistry, and Biology were qualitatively the same, and those coefficients are statistically different from zero for all subjects.

[33] This approach nets out the effects of changes in across-school composition of students who would have completed 11th grade even in the absence of free secondary education. However, it is worth noting that free secondary education may change the overall composition of students who take the UCE exam, by allowing more students with diverse backgrounds to complete 11th grade. Thus, what I calculate in this section is the gross change in average test score derived from the following two effects: one is the effect of free secondary education on the academic achievement of students who would have completed secondary education even without the USE policy, and the other is the effect of the policy on the composition of students who complete 11th grade thanks to school fee abolishment.

Section 4.3.1 showed that free secondary education increased the number of students who took the UCE exam in such districts by 879 students, 122 from public school and 757 from private school. Thus, the fall in the overall average test score may be due to this high enrolment harming the learning environment, and to the test scores of students who would have taken the UCE exam even without the USE policy (class size effects). Alternatively, it may be because these 879 students were potentially less able than other students, and simply attained lower than pre-program average test scores (composition effects). Thus, although the average test score decreased after free secondary education started, it is too early to conclude that the USE policy failed in this regard.

Secondly, to determine in which school types these falls in average test score occurred, I disaggregated the secondary schools in each district by ownership and USE participation. The results in Panel B show that, in USE participating government secondary schools, the fall in the average test score for all five subjects was far smaller than that in Panel A, and not statistically different from zero. Section 4.3.1 showed that, in participating public schools, the number of secondary school exit exam candidates increased, and section 4.3.4 that both physical and human resources available per student fell after the USE policy started. Thus, class size effects were likely to occur, but the effects on test scores were, if any, likely to be small. This finding is also consistent with the results in 4.3.2, which suggested that the characteristics of students attending public secondary schools were unlikely to have changed, and hence composition effects were also unlikely to have occurred. The reason why participating public secondary schools maintained the characteristics of their students is possibly because admission was competitive. In principle, the fall in the price of education in participating schools would increase the demand for its services. If the supply or available positions in participating secondary schools did not increase enough to satisfy the increased demand, the demand for admission would exceeds the supply. Such competition may have allowed participating government secondary schools to be selective, admitting abler children to maintain the quality of their students.

Secondly, in non-participating public secondary schools, the change was qualitatively similar to that in participating public schools, and the average test score did not fall significantly. This finding is consistent with my expectations, as non-participating public schools hardly increased their numbers of secondary school graduates, and changed their learning environments. Thus, neither class size and composition effects were less likely to be at work.

On the other hand, the average test scores in participating private secondary schools suffered to a far greater extent than those in government secondary schools.

Indeed, the change in average test scores for four subjects were negative and statistically different from zero (Column 1-5), suggesting that their academic achievement worsened significantly. These findings are consistent with those in sections 4.3.1 and 4.3.4, which imply that, in participating private schools, free secondary education reduced school resources available per student, harming the learning environment and students' achievement (class size effects). Furthermore, as the policy abolished school fees in this type of secondary school, such schools were likely to become more accessible for children from less wealthy households with less educated household heads. This expectation is indeed supported by section 4.3.2. Thus, composition effects may have put downward pressure on average test scores in participating private secondary schools. In contrast, if the demand for admission to participating private schools outweighed supply, as in USE participating government secondary schools, competition may have allowed only abler students to enroll in PPP secondary schools. However, PPP private secondary schools located in rural areas with limited outreach, as well as the scale of rural low-cost private secondary schools, may have been too small to provide an adequate range of subjects (Winkler and Sondergaard, 2008), and hence the increase in the demand for admission was likely to be limited.

Lastly, in non-participating private secondary schools, the change in average test scores for four subjects were positive and imprecisely estimated. Sections 4.3.1 and 4.3.4 show that the number of students graduating from non-participating private secondary schools increased rapidly after the USE policy started, and available physical and human resources per student were reduced. These findings suggest that class size effects were likely to occur, but the effects on the average test scores were marginal.

In summary, Table 4-15 shows that free secondary education decreased district level average test scores of 11th grade students in all five subjects. This change differs largely by school type, and the greatest fall in average test scores was likely in PPP participating private secondary schools due to both class size and composition effects.

A natural next question would be whether the fall in average test score in participating private secondary schools was due to the free secondary education policy harming the academic achievement of students who would have graduated from secondary school even in the absence of the USE policy. Alternatively, it may be simply because the test scores of students who became able to graduate secondary education were lower than the pre-program average. Ideally, individual data should be used to determine the counterfactual test scores of students who would have graduated from secondary education if the USE policy was not implemented. However, the context and data of this study does not allow such an analysis. Instead, I used school level data to examine the change in average test scores in the same school over time by including school fixed effects. In doing so, I limited the sample to the secondary schools that existed before the USE policy started, determining whether the average test scores in such schools suffered, or they fell at district level fell simply because the average in new participating private secondary schools was lower than that in traditional participating schools.

Table 4-16 shows the change in academic achievement in traditional secondary schools which existed before the USE policy started. Section 4.3.3 shows that the number of government secondary schools hardly increased after the USE policy started, and hence the results for these are qualitatively very similar to those in Table 4-14. Thus, this part sheds light on the results for PPP participating private secondary schools. The results show that the fall in test scores was smaller in traditional participating private schools, and not statistically different from zero for four subjects. This result suggests that free secondary education was less likely to harm academic achievement in traditional PPP participating private secondary schools that existed before the USE policy started. In other words, the fall in the average test score in Table 4-14 was likely due to the average test score in new PPP participating secondary schools being lower than the pre-program average among PPP participating private secondary schools.

These findings imply that the USE policy improved welfare, in that the number of secondary school graduates in traditional secondary schools increased, with few negative effects on their academic achievement, and further that it allowed more students to complete secondary education in new secondary schools, although their academic achievement may have been relatively poorer.

4.3.6 Robustness check

Before concluding this section, I submitted the estimated results to a robustness test by controlling for additional covariates. Table 4-16 presents the number students who took the secondary school exit exam as a dependent variable to examine the robustness of the findings, and Column 1 shows the results from the baseline specification for ease of comparison. Column 2 shows the interaction between the region fixed effects and year fixed effects, and shows that the main findings are robust with this inclusion. This set of variables control for the unobserved heterogeneous change in the four regions across the years, including heterogeneous trends in general economic development. Another concern leading to potential bias in the estimates is the effect of a government program targeting districts with high unemployment. As the timing of the introduction of the USE policy was almost same as the presidential election held in 2005 and the global economic recession, isolating the effects of such a government program from those of the USE policy is a major challenge. To address this concern, Column 3 controls for the interaction between pre-program unemployment rates and year fixed effects, but again the estimates remain similar. Finally, reconstruction following the civil war in the northern regions of Uganda may have affected access and achievement among secondary school students at the time of the USE policy implementation. In order to provide conservative estimates to address this concern, Column 4 excludes the sample from the north, and again the estimates remain robust. The same robustness test was conducted using the average test score as dependent variable, as shown in Table 4-17. These results suggest that the findings reported above are not spurious.

4.4 Conclusion

The study reported in this chapter examined whether the introduction of the USE policy promoted access to lower secondary education, changed the student body composition, led to the opening of private schools, or sacrificed quality of education and students' learning achievement. I exploited the variation across districts in the effective benefits of the USE policy in terms of the number of dropouts prior to the USE program. The findings suggest that the USE policy was effective in boosting the number of secondary school graduates in both public and private schools. Although I found that the policy decreased the average test scores in the secondary school exit exam, this change differed by school type and academic achievement was maintained in all types except PPP participating private secondary schools. Furthermore, the average test scores in traditional PPP participating private secondary schools existing before the USE policy started were far less likely to fall. Instead, the fall in the average test score was likely due to new PPP participating private schools becoming accessible for students from less wealthy households, as the new average test score was lower than the pre-program level.

Thus, this chapter concludes that the USE is a welfare improving measure for Uganda, in that it increased the number of secondary school graduates with few negative effects on their academic achievement in traditional secondary schools that existed before the policy started, and that it allowed more students to complete secondary education in new secondary schools, although their academic achievement may have been relatively lower than that of other graduates.

Chapter 5: Effects of education on adolescent pregnancy and child health

5.1 Introduction

There has long been debate on the relationship between female education and various outcomes, such as fertility, health investment, and child health status (Cochrane et al., 1979; Schultz, 1988, 2002; Strauss and Thomas, 1995). Numerous studies have investigated the impact of female education on fertility. While earlier studies tend to report cross-sectional correlations, recent evidence based on quasi-experimental studies has found that female education indeed reduces early pregnancy (for example, Berthelon and Kruger, 2011; Breierova and Duflo, 2004; Keats, 2014; Osili and Long, 2008; Ozier, 2015). Furthermore, randomized control trials providing educational subsidies show that a reduction in schooling costs decreases school drop-out and early pregnancy and marriage rates (Baird et al., 2010; Duflo et al., 2006, forthcoming). As many quasi-experimental studies focus on the reduction of school attendance costs through school construction or fee abolition, the findings may be taken as robust evidence for the impact of female education on early pregnancy.

However, rigorous evidence is more limited regarding the impact of female education on child health and health investment behavior. For instance, available studies indicate that maternal education reduces child mortality (Breierova and Duflo, 2004;

Chou et al., 2010), as well as anemia and stunting (Keats, 2014). Moreover, relatively sporadic evidence exists for a mechanism by which female education reduces early pregnancy and improves child health.

Various theoretical pathways have been put forward as an explanation for the negative relationship between female education and fertility. First, higher educational attainment increases human capital, thereby raising the opportunity cost of time for childbearing and childrearing. This is likely to decrease early pregnancy and increase labor force participation (Becker, 1981). However, the empirical evidence for this human capital effect is mixed. Second, schooling might merely create incarceration effects, by which girls have less time, opportunity, and desire to behave in a manner that outs their reproductive health at risk while attending school under adult supervision (Berthelone and Kruger, 2008). Third, education may change female pregnancy preferences. As educated women often face a trade-off between the quantity and quality of children (Becker and Lewis, 1973), improvement in educational attainment may lead them to want fewer children. Fourth, educated girls may become more knowledgeable about the medical risks of unprotected sexual activity and/or of giving birth at an early age. While this can be considered a 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.

Fifth, educated women are more likely to marry educated men, who might also want fewer children (Behrman and Rosenzweig, 2002). Among studies examining this assortative mating channel, some have found that partners are indeed more educated (Breierova and Duflo, 2004; Clark et al., 2014; Lavy and Zablotsky, 2011), but others have not (Cygam-Rehm and Maeder, 2013; Keats, 2014). Finally, once women are partnered, the bargaining model of a household suggests that decision making, including that around pregnancy and health investment, hinges on the relative bargaining power of the partners. Evidence is scarce regarding this pathway, and Keats (2014) has shown that this is unlikely to be at work. In sum, evidence remains limited or mixed regarding the importance of these pathways. More importantly, few studies have comprehensively investigated all of the pathways.

This chapter presents fresh and comprehensive evidence on early pregnancy, health investment, and child health, and further explores the pathways by which female education affects these outcomes. In order to address the endogeneity of educational attainment, I utilized the exogenous variation in the level of schooling over time caused by the UPE policy in Uganda. This policy abolished primary school fees across the country in 1997, aiming at improving access to basic education. I compare changes in the above outcomes between the cohorts fully exposed and not exposed to the UPE policy across districts that were likely to have had different levels of potential gain from the policy due to their differential pre-intervention primary education completion rates.

The results show that an additional year of schooling effectively reduced the probability of giving a live birth before the age of 18 years, and the probability of getting married by seven percentage points. This was not due to delays in the onset of sexual activity or an increase in abortion. There was also weak evidence for the increased use of modern contraceptive methods. Thus, education reduced pregnancy within marriage, which is consistent with the experimental effect of school subsidies reported by Duflo et al., (2015). Among those who had given birth by the age of 19 or 20 years, the more educated mothers invested more in the health of their children by delivering babies at formal facilities assisted by medical professionals, breastfeeding, and owning a mosquito net. Perhaps due to such investments, the probability of a child of an educated mother dying between the age of two and 12 months declined. These results suggest that female education not only decreases adolescent pregnancy, but also improves the health of children through mothers' better health investment. In terms of the pathways by which education leads to these changes, the data show that educated women were more likely to be literate and prefer to have fewer children. They also exhibited better knowledge about certain health and reproductive issues. Weak evidence was found for an increase in the probability of working in the non-agricultural sector and for incarceration effects. On the other hand, no evidence was found for assortative mating or improved bargaining power among more educated women.

The study most relevant to the present one is that of Keats (2014), which also examined the effects of the free primary education policy in Uganda on fertility, health investment, and child health. Keats (2014) study employed a regression discontinuity design, comparing women who were just above and below the age of 14 years when the policy was implemented. This means the findings are applicable to the partially treated cohort of women, some of whom were already enrolled in a higher grade of primary school, or had dropped out when the policy started. In the present study, I focused on the fully treated cohorts of those who were not yet of primary school attending age at the start of the policy. Any improvement in educational attainment is likely to have been larger among such fully treated women. As shown in Figure 5-1, the average years of schooling in fact increased as the cohorts became younger. Thus, this study supplements the findings of Keats (2014) by providing evidence on the full impact of the UPE policy, which is likely to apply to future cohorts in the long term.

In addition, as the present identification strategy exploited regional and temporal variation in policy intensity, this study shows that the policy reduced inequality in educational attainment, by disproportionately benefiting historically disadvantaged areas. A simple break-down of Figure 5-1 into areas with different levels of pre-policy educational attainment (Figure 5-2) shows that the greatest improvement occurred in areas in which the years of schooling among older cohorts of women (aged 27-31 years in 1997) fell in the 1st and 2nd quartiles. On the other hand, there was very little improvement among those in the 4th quartile.

In sum, this study contributes to the literature by providing comprehensive evidence on the full impact of education on fertility and child health, as well as the pathways of such impact. In addition, the focus on not only temporal but also regional variation in UPE policy intensity allows an examination of its equalizing impact. The present findings differ somewhat from those of Keats (2014). While the evidence for human capital effects is weaker in the present results, the negative impact of education on child mortality rates is clear, which was not found in the Keats (2014) study. Another study using a similar method as Keats (2014) found that the UPE policy decreased the likelihood of being infected with HIV (Behrman, 2014).

The remainder of this chapter is organized as follow: section 5.2 describes the data and analysis strategies; in section 5.3, I present the empirical findings; finally, section

5.4 provides concluding remarks.

5.2 Data and identification

5.2.1 Data and treatment cohorts

This empirical exercise draws mainly on the UDHS data of 2001 and 2011. The UDHS offers nationally representative, repeated cross sectional data rich with information on reproductive health and demographics of women aged 15 to 49 years. This includes information on their pregnancy history, health investment, and child health. This information was extracted for all cohorts that were young enough to be fully affected by the UPE policy, from its inception, as well as cohorts that missed the UPE opportunities by being too old at its onset.

More specifically, the treatment group includes girls who were aged four and five years (i.e., pre-school age) in 1997 when the UPE policy started, and the control group includes girls who were aged between 17 and 19 years at that time. The treated cohorts were likely to have benefited from UPE policy from grade one. On the other hand, the control cohorts were likely to have already finished or dropped out of primary schooling when UPE policy started. Out of 15,921 observations of UDHS data from 2001 and 2011, 1476 women fell into either the treated or control cohort, and lived 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 years, whereas the control girls were aged 21 to 23 years. The cohorts aged between six and 13 in 1997 were omitted from the main sample because they were only partially treated. Specifically, some may have already dropped out of school, or spent some years paying school fees before the UPE policy started. Thus, they form neither a pure control nor a pure treated cohort. I also excluded women aged 14 to 16 years in 1997, as a non-negligible number of women in these cohorts attended primary school when the UPE policy started, despite not being correctly aged for that level of education, due to the high prevalence of grade repetition and delayed primary school entry in Uganda. The summary statistics are described in Table 5A-1.

In addition to this main sample, as a proxy for the primary school completion rate that would have been realized for these cohorts without the UPE policy, I used the completion rates among cohorts several years older than the main sample, namely women aged 27 to 31 years when the policy started. The information on these older women is based on the 1991 UPHC. I grouped these women by their district of birth, and computed the completion rates. Given their age, the UPE policy is highly unlikely to have 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 four to five years in 1997. The older women in the UPHC were aged 21 to 25 years at the time of interview. In assigning this district level pre-program completion rate to girls in the main sample, I had to use the district of current residence, as the UDHS did not provide information on the district of birth. However, over 92% of women aged 18 to 23 years remain in the same district as at age seven in Uganda. Thus, the degree of attenuation bias due to possible measurement error in the completion rate stemming from migration is likely to be fairly small.

5.2.2 Identification strategy: Difference in differences

I focused first on the impact of the UPE policy on girls' schooling. For this purpose, I employed a variant of the DID approach, in which one of the differences is represented by a continuous, rather than a dichotomous, treatment variable. In other words, the temporal change in educational attainment between the cohorts exposed and not exposed to the UPE policy was compared across the districts that differ in terms of the intensity of expected UPE benefit, as measured by the pre-program primary-level completion rate. Similar approaches have been used by Duflo (2003) and Osili and Long (2008), for example. Specifically, I used the following model as a baseline:

$$S_{ijkt} = \alpha_0 + \alpha_1 Young_t + \alpha_2 preUPE\ comp.\ rate_j$$
$$+ \alpha_3 (Young_t * preUPE\ comp.\ rate_j)$$
$$+ \alpha_4 X_{ijkt} + \alpha_5 Z_{jkt} + \epsilon_{ijkt} \qquad (13)$$

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 four or five years in 1997 (as opposed to 17 to 19 years for the control group), and $preUPE\ comp.rate_j$ is the share of women born in 1966 to 1970 who completed primary education in district j. The across cohort change in the number of schooling years is captured by α_1, and the effect of differential program intensity is captured by α_2. While simultaneously controlling for these two sources of variation, the interaction term between the two variables reflects whether the changes across cohorts are systematically correlated with the pre-program primary-level completion rate.

Figure 5-3 illustrates the intuition behind this identification strategy. The dotted and solid lines indicate the lowess fitted lines for the control and treated cohorts, respectively. The line for the control cohort shows the positive relationship between the district level pre-program completion rates and individual educational attainment for girls aged between 17 and 19 years in 1997. This implies that, prior to the UPE policy, girls aged 17 to19 years (the control group) attained a lower level of education in districts with a lower pre-program completion rate. This slope corresponds to coefficient α_2 in equation (13). Secondly, the solid line for the treated cohorts also shows a positive slope, but 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 (13). Since the gap tapers as the pre-program completion rate increases, it confirms the theoretical prediction in section 5.2 that improvement in educational attainment is concentrated in districts with higher program intensity (i.e., lower completion rates). Thus, the post-UPE cohorts exhibit less inequality in educational attainment across districts.

In addition to the intensity variables, in equation (13), I controlled for the religion of the household head (X_{ijkt}) and a number of district cohort level covariates, Z_{jkt}, including the number of governmental primary schools that existed within the district when each cohort was aged six years. Thus, while access to schools could vary across cohorts, particularly following the launch of the UPE policy in previously disadvantaged areas, its impact on educational attainment was controlled for. Furthermore, such disadvantaged areas may have experience disproportionate economic growth due to mean reversion, which can boost educational investment. To control for this, I included the district level share of communities (Local Council 1, or LC1)[34] that had a bank branch within their boundaries when each cohort was around the age of 14[35]. In addition, access to health services could be another time-variant factor that improved relatively greatly in disadvantaged areas, promoting health and thus schooling of children. I controlled for this factor by including the district level share of LC1s with public hospitals or health centers around the age of 14 years.

Alternative to the basic specification, in equation (14) below, I replaced the dummy for the young cohort with μ_j, the set of 35 district dummy variables that existed in 1991. I could then also include λ_t, the set of birth year fixed effects, as well as the interaction between λ_t and $Region_k$, the set of four regional dummy variables.[36] These controls absorb possible unobserved heterogeneity across districts, which might arise from, for example, varying levels of local public efforts to promote education or reduce the local capture of public funds[37], which could be associated with the pre-program primary completion rate. While these efforts may have become stronger, particularly in

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[34] A village refers to the LC1 in the Ugandan geographical category.

[35] I used UNHS 1993 and 2006 data to measure these district level variables, and merged them with the main data. The 1993 data was assigned to the control cohorts born between the years 1978 and 1980, and the 2006 data to the treated cohorts born in 1993 and 1994.

[36] The four regions are the 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).

[37] According to Reinikka and Svensson (2005), the Ugandan government initiated a newspaper campaign and reduced local capture after the first public expenditure tracking survey conducted in 1996.

places with historically low levels of education, such heterogeneous trends are also captured by the region-specific cohort dummy variables. Thus, the identification assumption is that there were 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 time as the UPE policy, after controlling for district and cohort fixed effects, regional trends, and district level indicators of general economic development and improvement in access to schools and health facilities[38]. I address later the validity of this assumption by conducting the same regression exercise for two groups of girls both not exposed to the UPE policy as a placebo test. The results indicated no effect, supporting this assumption. Finally, the standard errors were clustered at the level of district, which defined the variation in pre-program completion rates (Bertland et al., 2004).

$$S_{ijkt} = \beta_0 + \beta_1\left(Young_t * preUPE\ comp.\ rate_j\right) + \beta_2 X_{ijkt} + \beta_3 Z_{jkt} + \mu_j + \lambda_t + Region_k * \lambda_t + \tau_{ijkt} \qquad (14)$$

In equation (14), the coefficient for the un-interacted terms cannot be estimated, but the

[38] Some readers may be concerned that boys, not only girls, in the same cohort also benefited from the UPE policy, and the outcomes of interest, such as fertility and child health, may have been affected through the improvement of male education. Although boys indeed improved in educational attainment after the UPE started, the UNHS data show that, mainly because the average years of education among boys was above seven even before the UPE policy started, there was no regional variation in the magnitude of improvement among boys. The effects through such change were isolated by the birth year fixed effects, allowing us to isolate the exogenous change in female education from that in male education.

estimate for the interaction term is likely to be more reliable. I used this as the preferred specification. The estimated coefficient β_1 is likely to reflect largely the impact of the UPE. As mentioned in 2.1., the treatment group was too old to benefit fully from the UPE policy[39].

5.2.3 Identification: Instrumental variable approach

The analysis of the impact of UPE policy on educational attainment can be regarded as the first stage regression in the instrumental variable method in which educational attainment is treated as an endogenous variable. Specifically, I used the interaction term $(Young_t * preUPE\ comp.\ rate_j)$ in equation (14) to instrument the endogenous variable in order to identify the impact of education on fertility, health investment, and child health. The second stage equation is specified as follows:

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

(15)

where Y_{ijkt} is an outcome variable, for example, the dummy variable indicating whether a girl gave a live birth before the age of 18 years. The coefficient of interest is γ_1. In this

[39] To assess the full impact of the UPE policy, one needs the cohort of girls born after 1994. Given no delay or repetition, these girls would have been supposed to have entered primary school after 2000, taken the PLE after 2006, and entered junior secondary school after 2007.

case, I expect its sign to be negative if girls' education effectively reduced adolescent pregnancy.

As shown below, Craig Donald F-statistics indicated that the instrument explained sufficient variation in the endogenous variable (Stock and Yogo, 2005). The instrument was also likely to satisfy the exclusion restriction. As I controlled for district and cohort fixed effects, the interaction between the region dummies and cohort dummies, and time-variant district level controls, there was unlikely to be a remaining time-variant factor that correlated with both the historical primary completion rate and within-region variation in changes in outcomes, and that started to have an impact around the same time as the UPE implementation.

5.2.4 Measures of educational attainment

While one would ideally like to know the completed years of education, as the treated cohorts were aged 18 and 19 years at the time of the survey, some of the girls in these cohorts may have progressed to obtaining more schooling later. This censoring issue was likely to be more severe among the treated cohorts who were younger, and also in the districts with higher pre-program completion rates, in which many women tended to obtain more education. This could produce a spurious positive effect of the UPE on the level of education completed by the age of 18 or 19 years[40]. This selective censoring was less likely to be an issue when the outcome variable was the completion of primary school, as over 92% of the women in the treated cohorts were no longer attending a primary school at the time of the interview. Since an adult woman is less likely to enroll in a low grade of primary education, the lower the grade I employed as the outcome, the less susceptible were the results to bias caused by the censoring[41]. Thus, if the results for completion of grades at primary level were qualitatively consistent with the results for years of schooling, that would indicate the robustness of the finding that fee abolition for primary education reduced adolescent pregnancy.

Comparing the distribution of the number of completed years of education between the control and treatment groups (Figure 5-4), it is clear that the share of women who *never attended* school dropped sharply across the two cohorts from 13 to six percentage points. This suggests that the UPE policy particularly promoted the enrolment of women who would not have entered primary school in its absence. Furthermore, the share of women who dropped out of school between the first and fourth grades halved.

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[40] As I controlled for birth fixed effects and the interaction terms between birth year dummies and region dummies, for such a selective censoring to bias my estimates, if any, it must arise within a region.

[41] In my data set, about 7% of treated women attended primary school sometimes in the year of interview. The proportion dropped to 3.6% for girls who attended grades lower than P6, and further down to 1.8% for girls who attended grades lower than P5. Given that they answered with "attending school" even if attending school only for one day, this number is likely to be overstated.

Indeed, the share of women attaining the fifth, sixth, and seventh grades was two to three percentage points higher in the treatment group. This suggests that the UPE policy also promoted the completion of higher grades among primary school entrants.

5.3 Results

5.3.1 Effects of UPE policy on years of schooling

I start with the results for the effect of the UPE policy on girls' educational attainment. Column 1 of Table 5-1 shows the results from the baseline specification, given in equation (1). The results are consistent with Figure 5-3, which depicts the concentrated improvement in educational attainment after the introduction of the UPE policy in areas with historically low levels of education. First, the coefficient for the un-interacted pre-program completion rate, 11.78, suggests that a 10 percentage point increase in the pre-program completion rate for older women was associated with a 1.18 year increase in the years of schooling for the controlled in the same district. This association fell dramatically from 11.78 to 8.05 (=11.78-3.73) for the treated cohorts. These results are consistent with the conceptual framework that suggests the equalizing effect of the UPE policy.

In Column 2, I show the results based on the preferred specification in equation (2). The inclusion of these controls barely changes the results qualitatively, and the instrument consistently indicates a significant decline in the magnitude of the positive correlation between years of schooling and pre-program primary completion rates. This finding remained robust, even when I additionally controlled for the full interactions between the four regional dummies and birth year fixed effects (Column 3), and when the measure of pre-program completion rates was calculated from different cohorts of women in the 1991 UPHC who were aged between 26 and 30 years, and between 21 and 30 years in 1991, respectively (Columns 4 and 5). Taken together, these results suggest that the UPE policy improved girls' education, particularly in the districts with lower educational attainment prior to the UPE policy, and as a result, it equalized educational attainment across districts in Uganda.

5.3.2 Falsification test: Placebo experiment

However, one might wonder whether the results merely reflect mean reversion. In other words, even without the UPE policy, girls living in districts with lower pre-program completion rates may have shown the same improvement in schooling. To address this possibility, I conducted a placebo experiment. Specifically, if the results were in fact due to mean reversion, I would find a similar pattern between two groups of cohorts both unaffected by the UPE policy. For this exercise, I used the data for girls aged 17 to 18 years in 1997 (the control cohorts in the main analysis) as the placebo treatment cohorts[42], and girls aged 30 to 32 years in 1997 as the placebo control cohorts. Both groups were highly unlikely to have benefited from the UPE policy, as both were 17 years of age or older when it started.

The results suggest the placebo experiment did not detect any significant change in the relationship between the district level pre-program completion rate and individual educational attainment across the two cohorts (Table 5-2, Column 1). This contrasts with the results of the main analysis, which are presented again in Column 2. This implies that the equalization in girls' educational attainment did not occur until the launch of the UPE policy, which in turn provides suggestive evidence that the equalization effect may be attributed to the policy's fee abolishment. The results in Column 3 are consistent, even when the cohorts providing the information on pre-program completion rates were changed to women aged 34 to 38 years.

[42] As a placebo control cohort, women aged 30-32 years were used to maintain the same age differences across two cohorts as a main analysis. I extracted information on the placebo treatment and control groups from the UDHS 2001 data because prior surveys lacked information on the district of current residence. The district level pre-program completion rates were calculated using the educational attainment of women born between 1959 and 1963, or aged between 34 and 38 years in 1991.

5.3.3 Effects of schooling on adolescent pregnancy

I turn now to the impact of educational attainment on adolescent pregnancy. I take advantage of the exogenous change in educational attainment caused by the UPE policy and instrument it by the interaction term between the pre-program primary completion rate and the indicator for the treatment cohorts. Thus, the discussion thus far serves as the analysis of the first stage regression in the IV method. As shown in Table 5-3, I started with the OLS estimates for comparison, which did not control for the endogeneity of girls' schooling (Column 1). The results show that an additional year of schooling was associated with a 4.2 percentage point reduction in the probability of giving a live birth before the age of 18 years. The instrumental variable estimates using the 2SLS are shown in Columns 2 and 3. Column 2 shows the results based on equations (2) and (3) without the region-specific cohort effects, while Column 3 additionally controls for the regional trends. Both results indicate that an additional year of schooling significantly reduced the probability of having an adolescent pregnancy. In particular, the results based on the preferred specification (Column 3) show that attending another year of schooling reduced the probability of giving birth before the age of 18 years by 6.7 percentage points. This fall was as large as 17% of the pre-program mean of the outcome. The Craig Donald F-statistic for the test for a weak instrument was 25.4, rather larger than the rule-of-thumb critical value.

An interesting finding is that the IV point estimates were almost 50% larger in the absolute term than the OLS estimates. This might be unexpected if one is concerned about the unobserved heterogeneity of factors such as household wealth, academic ability, and risk averseness[43]. The fact that the absolute size of the negative effect was larger in the IV estimate suggests that the response among compliers (i.e., girls who would have completed more years of schooling had they resided in districts with low pre-program completion rates, but would not had they resided in districts with high pre-program completion rates) is likely to be larger than that among the remainder of girls who did not change their schooling behavior depending on the intensity of UPE benefit. The fact that the IV estimates showed larger effects in absolute value is consistent with previous studies examining the effects of education on early motherhood using school construction or reforms as the sources of instruments (for example, Breierova and Duflo, 2004; Cygam-Rehm Maeder, 2013; Osili and Long, 2008). Altogether, the IV analysis suggests that girls' schooling significantly reduced the probability of adolescent pregnancy in Uganda.

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

5.3.4 Effects of grade completion on adolescent pregnancy

The analysis in the previous section assumed a 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, I examine how the completion of each formal education grade affected the likelihood of adolescent pregnancy. This exercise also allowed me to examine the robustness of the results in terms of a possible bias caused by selective censoring by the completed years of schooling. I estimate equations (2) and (3) using the dummy variable indicating a girl i in district j and region k born in year t who had completed a certain grade. Specifically, I considered the completion of each grade in primary education (P1 to P7), as well as the first three grades in lower secondary education (S1 to S3). The results are shown in Table 5-4. The results in Column 1 of Panel A suggest that a girl who completed the first year of primary education had a lower probability of giving birth before the age of 18 years, in comparison to those who failed to complete grade 1. The first stage results, shown in the second row, indicate the equalizing effect of the UPE policy, which is consistent with the results in Table 5-1. The results are similar for the P2 to P7 completion data (Columns 2-7). This consistency provides suggestive evidence that a bias caused by selective censoring, if any, is unlikely to qualitatively affect the main findings.

In addition, the analysis of the completion of secondary level education sheds light on whether the UPE policy affected entry into secondary schooling. The results based on the completed years of schooling in Table 5-2 suggest that the UPE policy induced some girls to attain some secondary education[44]. The results in Table 5-4 mirror these implications and indicate that the UPE policy promoted enrolment in the first year of lower secondary school, reducing the probability of adolescent pregnancy (Column 8). However, the impact of S2 and S3 completion is unlikely to be traceable, as the first stage results become weak (Columns 9 and 10)[45].

One might also wonder whether the expected benefits of the UPE and the USE could partially offset each other. While areas with high pre-program primary-level completion rates had less potential to gain from the UPE policy, they might have

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[44] Based on the coefficient in Column 3 of Table 5-2 (-6.21), a one-standard-deviation increase in the pre-program primary education completion rate (0.18) is associated with a 1.12 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 7.03 years for the treated cohort, which corresponds to the end of primary education. With some variance around the mean, these results are likely to reflect that some treated girls completed the first few grades of secondary education. This is based on the following computation:
$\left.\frac{\partial S}{\partial preUPE\ comp.rate}\right|_{Young=1} = -6.21$, and thus, $\Delta S|_{Young=1} \cong -6.21 \times \Delta preUPE\ comp.rate$. Similarly, by construction, $\left.\frac{\partial S}{\partial preUPE\ comp.rate}\right|_{Young=0} = 0$, and thus $\Delta S|_{Young=0} \cong 0 \times \Delta preUPE\ comp.rate$. Therefore, $(\Delta S|_{Young=1} - \Delta S|_{Young=0}) = -6.21 \times \Delta preUPE\ comp.rate$.

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

benefitted more from the USE policy because of job opportunities that reward secondary education in such areas. If this were the case, the net benefit would become similar across areas with differential pre-program primary completion rates, and as a result, the impact of the UPE on the number of years of schooling, or the first stage coefficient, would be likely to be biased towards zero. This tendency would likely be larger for outcomes indicating the completion of higher secondary grades. This was indeed confirmed by the first stage results in Table 5-4 (Columns 9 and 10). Thus, the combined impact of the 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 the UPE policy dominated the possibly opposite effect of the USE policy. It might be that the UPE policy induced girls to enter secondary school, even though it did not lead to continued secondary education.

5.3.5 Effects of education on safe delivery, neonatal care, and infant health

While I have shown that education reduced adolescent pregnancy, one third of treated women had given birth before the age of 18 years. The question arises as to whether education might help those mothers to provide better health investment for their children. I next examined the impact of female education among those who had given birth by the age of 18 years on health investment behavior, measured by the utilization of maternal and infant care, as well as on the health status of children, measured by current incidence of infant mortality and illness.

The results in Panel A of Table 5-5 show that educated mothers were more likely to receive delivery assistance by a medical professional (Column 1), and to deliver at a modern health facility, such as a hospital or health center (Column 2). In addition, they were more likely to breastfeed their children and to have them receive a BCG vaccination after birth (Column 3 and 4), which helps prevent disease.

Perhaps reflecting these health investments, the probability of death before the age of 12 months fell by 5.4 percentage points among the children of women with an additional year of completed education (Column 2, Panel B, Table 5-6). Since the probability of death within one month remained unaffected (Column 1), maternal education is likely to have reduced the incidence of death between the 2nd and 12th months. An explanation may be that educated mothers receive better information on infant care, such as preventative inoculation and breastfeeding, which boost infants' immunity, either when they deliver their babies at formal facilities or from other sources. On the other hand, the incidence of diarrhea and fever did not fall among the children of more highly educated mothers (Columns 3 and 4). Taken together, the results indicate that the promotion of female education not only reduced adolescent pregnancy but also promoted better health investments when they become pregnant, and as a result, their children benefited from improved health.

5.3.6 Pathway: Early marriage, abstinence, and contraception

Thus far, together with favorable effects on infant health, I have shown that increases in girls' years of schooling reduced the probability of giving birth before the age of 18 years in the present data from Uganda. Next, I analyze whether this reduction in adolescent pregnancy could be ascribed to increases in contraceptive use, abstinence, and/or miscarriage and abortion. First, if education promoted abstinence, the onset of sexual activity is likely to be delayed. However, there is no evidence of the probability of first sexual intercourse by the age of 15, 16, 17, or 18 years declining with education (Panel A, Table 5-7). On the other hand, the results in Panel B suggest that improvement in educational attainment delayed the age at first marriage. The decline in the probability of getting married by the age of 15 to 18 years is almost identical to the decline in the probability of giving birth before the age of 15 to 18 years (Panels B and C, Table 5-7). These results suggest that the reduction in adolescent pregnancy is likely to be attributed to delayed marriage. In addition, I found a significant education effect on the use of modern contraceptive methods, particularly pills but not condoms (Columns 1-3, Panel D)[46]. While I lacked information on the past use of contraceptives, to the extent that more highly educated women also started using these early, the results are indicative of the role played by contraceptive use in delaying the age at first pregnancy. In contrast, increased education level did not reduce the chance of having an abortion or miscarriage (Column 4, Panel D). If the incidence of miscarriage did not change or was reduced, this would suggest that abortion was not a major pathway by which female education reduced adolescent pregnancy. Taken together, the results suggest that female education reduced adolescent pregnancy by delaying marriage, and possibly by promoting modern contraceptive use.

A natural next question is why education delays marriage and promotes the use of contraceptives. As discussed in the Introduction, the literature has suggested at least six channels through which female education affects these mediating factors, namely (1) human capital effects, (2) incarceration effects, (3) change in fertility preferences, (4) knowledge improvement, (5) assortative mating, and (6) improved bargaining power within couples. I examine each of these pathways below.

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[46] In the analysis, I assumed that women who had never had sexual intercourse had never used a condom. Even when I limited the sample to women who had ever experienced sexual intercourse, the results remained qualitatively unchanged. Furthermore, the results remained insignificant when the outcome was changed to the use of any method of contraception.

First, the results in Table 5-8 show that education improved literacy (Column 1). While the impact of education on the probability of working in any sector or being employed by someone other than a family member was insignificant (Columns 2 and 4), its impact on the probability of working in the non-agricultural sector was of marginal significance with a p-value of 0.11 (Column 3). These results contradict those of Keats (2014), who found evidence for an increase in the probability of earning a cash income. A possible reason for the difference might be that the present treatment group was still young, aged 19 to 20 years at the time of the interview. Within this age range, only weak evidence was found for the human capital effects of education.

Second, there was some evidence that women became more knowledgeable about the medical risks of unprotected sexual activity and premature pregnancy with an increase in education. I used the rate of correct answers to questions on HIV infection and the source of the male condom as a measure of reproductive knowledge. I also used the probability of using a mosquito net as a proxy for knowledge of malaria prevention, malaria being one of the most common fatal diseases in Uganda. The results in Panel B of Table 5-9 show that educated women were more likely to know the source of condoms (Column 2) and to have mosquito nets (Column 4), although their HIV knowledge remained unchanged (Column 1). While these outcomes offer a far from comprehensive description of the knowledge base that may be improved by education, the results may be taken as weak evidence for the possibility that education promoted female knowledge about reproductive and health issues, which in turn reduced the incidence of adolescent pregnancies[47]. The lack of improvement in knowledge regarding HIV infection is interesting given that the UPE policy reduced the probability of being HIV positive (Behrman, 2015). This might indicate that increased contraceptive use might have played a more important role than education in reducing the infection rate.

Third, I found that education altered female pregnancy preferences. The results showed that the perceived ideal number of children declined by 0.13 with an additional completed year of schooling (Column 4). This is consistent with the theory that an educated mother might prefer fewer births, or delay the onset of reproduction, as their children tend to survive longer (Becker and Lewis, 1973). Thus, a preference for fewer pregnancies is likely to have been an important factor contributing to the delay of first marriage. However, such a reduction in fertility preference may not come to be fully realizes unless women have the final say on contraceptive use and/or the timing and frequency of sexual intercourse. As a proxy for female bargaining power within marriage, I used women's reported acceptance of domestic violence committed by partners and the

[47] Unfortunately, data on knowledge about other issues, such as the risks of adolescent pregnancy and the importance of preventative care, were not available.

likelihood of women making their own decisions on medical expenses. To the extent that these proxies represent female bargaining power, the results did not suggest that this was strengthened by educational attainment (Columns 1 and 2, Panel C). Thus, it is unlikely that education reduced the likelihood of adolescent pregnancy by allowing women to say no to sex or to ask partners to use contraception. This finding is consistent with the fact that education does not encourage the use of condoms, which is usually decided by men, but does increase the use of pills, over which woman have more control (Table 5-6).

Fifth, assortative mating could potentially explain the link between education and delayed pregnancy if educated women tended to marry men with lower fertility preferences. On the one hand, educated women might marry educated men, who are likely to have higher earnings, which could increase their fertility preferences due to income effects. However, educated partners may also prefer to have fewer children (Behrman and Rosenzweig, 2002). Thus, the direction of this effect remains undetermined. However, the present results indicate that improvement in female education had no significant impact on the age or educational attainment of partners (Columns 3-4). Thus, unlike the case in Indonesia (Breierova and Duflo, 2004), assortative mating is unlikely to have been the main channel through which education affected early pregnancy in the present Ugandan data.

While I was unable to test incarceration effects directly due to the lack of data on time allocation, as Ugandan students must stay at school from 7am to 5pm every weekday with adult supervision, it seems natural that a girl who spends longer years in school has far less time, opportunity, and desire to risk pregnancy. Together with the fact the majority of women attended school when they were aged 13 to 17 years[48], the incarceration effects of education are likely to have been a probable channel through which improvement in education delayed marriage in Uganda.

In sum, I found weak evidence for both human capital and incarceration effects. I also found that education lowered pregnancy preferences and equipped women with better knowledge about certain reproductive and health issues. On the other hand, it is unlikely in the Ugandan case that education promoted assortative mating or female bargaining power within marriage.

5.4 Conclusion

In this chapter, I reported on an investigation of whether promoting female education affected adolescent pregnancy, health investment behavior, and the health status of children among Ugandan women. In order to overcome the endogeneity of educational

[48] About 90% of the treated cohorts were attending school at the age of 13 and 14 years, and about 60-70%were aged 16 or 17 years (Panels A and B, Appendix Table A3).

attainment, I instrument it with across-district and inter-cohort variation in the intensity of the expected benefit from Uganda's UPE policy, which abolished a substantial part of schooling costs in 1997.

One of our major findings was that an additional year of schooling reduced the probability of giving a live birth before the age of 18 years by 6.7 percentage points, which amounted to a 17% decline compared to the pre-program mean. This implies that promoting female education can be an effective means to reduce the incidence of adolescent pregnancy, which is associated with adverse consequences, such as maternal and neonatal death, low birth weight, and premature birth. I also found that this reduction was achieved through a delay in marriage throughout the ages of 15 to 18 years, and possibly also through the increased use of pills but not of condoms. On the other hand, neither the timing of sexual debut nor the likelihood of miscarriage or abortion was affected by education. Another major finding was that educated women were more likely to practice better health investment, such as by the use of formal delivery care, breastfeeding, and infant vaccination. As a result, their children were less likely to die between the 2nd and 12th months.

While the literature discusses the incarceration and human capital effects of education as mechanisms through which it lowers fertility, I found some evidence suggesting that both were at work. In terms of incarceration effects, the majority of women attended school when they were in the age range for which the reduction of early pregnancy was found. In terms of human capital effects, education was found to raise literacy rates and the probability of working in the non-agricultural sector, the latter with a p-value of 0.11. In addition, I found that female education reduced the perceived ideal number of children. Weak evidence was found that education also improved knowledge about certain health and reproductive issues. On the other hand, no evidence was found that education influenced female bargaining power within marriage or the characteristics of their partners. These results imply that, in addition to the two effects discussed as main pathways through which education lowers fertility, its impact on fertility preferences and knowledge are likely to be important channels explaining the influence of female education. However, other debated possibilities, such as assortative mating and bargaining power gain, did not have a discernable effect in the case of Uganda.

The pathways through which education improved health investment behavior remained less clear. While I lacked data on knowledge about health services, as educated women are more likely to know the source of condoms, it may also be the case that they know more about formal maternal care services, increasing their utilization of such. This may have provided them with further information on the importance of breastfeeding, vaccination, and mosquito nets; alternatively, educated women were better at translating information into action.

Taking a broad view of the findings, they imply that the benefits of the UPE policy were not limited to advancement in educational attainment, but also extended to 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 policy and institutions influencing educational attainment.

Appendices for Chapter 5

Appendix 5.1. Conceptual discussion

How has the UPE policy changed parents' schooling decisions? In this appendix, I discuss a conceptual framework describing how parents decide on the optimal level of education for their child. Using this framework, I further discuss how the UPE policy differentially affects the optimal choices across districts with varying initial levels of education.

Suppose that parents try to maximize the net benefit of sending their child to school. The optimal level of schooling would be determined by comparing the marginal benefit and marginal cost of attending another year of schooling (MB and MC in Figure 5A-1, respectively). I assume that marginal benefit decreases over years of schooling as basic skills such as numeracy and literacy tend to yield higher rates of return (Psacharopoulos, 1981, 1985, 1994, 2004). That is, $\mathrm{MB_i} = \mathrm{F(S_i)}$, where $\partial \mathrm{F} / \partial \mathrm{S} < 0$ and S represents the number of years of schooling. On the other hand, I assume that the marginal cost curve increases over years of schooling. That is, $\mathrm{MC_i} = \mathrm{G(S_i)}$, where $\partial \mathrm{G} / \partial \mathrm{S} > 0$. As a girl attends school for a longer time, she becomes abler to work and her opportunity cost of attending school increases. The optimal level of schooling, S*, is chosen so that $\mathrm{F(S^*)} = \mathrm{G(S^*)}$.

Figure 5A-1 illustrates this optimization process in two cases with different initial conditions. Without the UPE policy, girls in Panel A face a higher marginal cost schedule, which is depicted by the dotted line (MC). For example, they might live in a place that is far from a primary school. Such girls may also face a lower marginal benefit schedule, which is depicted by the solid line (MB) because non-farm job opportunities are limited in the surrounding areas. In Panel A, at the intersection of MC and MB, point E_l^* indicates the equilibrium level of educational attainment without the UPE policy.

The optimal level of schooling is determined 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 is the case in Panel A. In particular, girls in Panel B complete primary education even in the absence of the UPE policy. This situation resembles relatively developed areas in Uganda before the UPE policy.

The UPE policy is likely to shift the marginal cost line downward from MC to MC' in the figure only between the first and seventh grades, as 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 barely affects the optimal level of schooling in Panel B $(S_h^* = S_h^{*\prime})$. Therefore, these panels indicate that areas that initially had lower rates of primary education completion are likely to demonstrate a greater catch-up effect in educational attainment with the UPE policy. In other words, the intensity of UPE benefits, or the scope for improvement, is greater in districts whose conditions resemble those in Panel A. Such districts are likely to exhibit a lower counter-factual level of schooling in the absence of the UPE policy, which is equivalent to E_l^* in Panel A, compared to E_h^* in Panel B. I 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 the UPE policy in a given district, young women in that district would also have attained a lower level of education had the UPE policy note been implemented. I use this regional variation in pre-program completion rates, together with across-cohort differences in exposure to the policy, in order to identify the effect of female education on adolescent pregnancy, health investment, and child health.

Appendix 5.2: Impact of the UPE policy on education for partially treated cohorts

The sample of women born between 1981 and 1991 was excluded from the main analysis in section 5.3. These women were aged between six and 17 years when the UPE policy started, and were thus either partially treated by the UPE or had already stopped schooling by then. While Keats (2014) shows that such partially treated cohorts benefited from the UPE policy, the results based on equation (2) fail to detect any impact in the present data (Table 5A-2). That is, a significant decline in the relationship between pre-program primary completion rate and educational attainment compared to the reference group (women born in 1978 and 1979) occurred only among the fully exposed cohorts (born in 1992 and 1993) in the first row, and not among the partially treated (rows 2 to 7). Figure 5A-2 also indicates that a disproportionate growth in educational attainment concentrated in areas with low pre-program primary completion rates started to emerge only amongst the fully treated. A possible explanation for these results might be that many women were already out of school in those areas with low pre-program primary completion rates when the policy started, and thus UPE benefits were found only among fully exposed cohorts. Furthermore, only four children per household were eligible for fee abolishment between 1997 and 1999. Thus, parents in such areas might have preferred to make younger children eligible so that they could avoid fees from grade one.

Appendix 5.3: Data appendix

Regarding Table5-6, to measure the use of delivery care, respondents were asked who had assisted in the delivery, selecting from the following list: (1) doctor, (2) nurse/midwife, (3) private medical assistant, (4) private nursing aide, (5) traditional birth attendant, (6) relative, (7) friend, and (8) other. I defined (1) to (4) as medical professionals for present purposes. The respondents were also asked to indicate the place of delivery, selecting from the following list: (1) respondent's home, (2) traditional birth attendant's home, (3) other home, (4) government hospital, (5) government health center, (6) government health post, (7) other public facility, (8) private hospital/clinic, (9) other private medical facility, and (10) other. For present purposes, I defined all but (1) to (3) as modern health facilities. Regarding a child's vaccination history, mothers were asked whether a child had received BCG, DPT, Polio, and/or measles vaccinations. Table 5-6 suggests that maternal education increased the take-up of the BCG vaccination, but there was no significant effect on other types of vaccination. To measure child morbidity, mothers were asked whether a child was currently alive. Those with a living child were asked whether s/he had suffered from a fever, cough, and/or diarrhea in the past two weeks. The child's height and weight were also asked to determine whether the child was stunted, underweight, or wasted. I found none of these acute morbidity and chronic health status indicators to be significantly associated with maternal education level.

Turning to Table 5-7, regarding contraceptive usage, respondents were asked whether they had ever used any type of contraceptive from a list of 16 methods. I classified these into three types, namely modern methods (including the pill, an IUD, injections, a diaphragm, condoms, female sterilization, male sterilization, NorplantTM or other implant, lactational amenorrhea, female condom, and foam/jelly), traditional methods (periodic abstinence (rhythm method), withdrawal, and Abstinence), and the remainder (folkloric methods and others). While I found a positive impact of education on modern contraceptive use (Table 5-7), this was not the case for traditional or folkloric methods. Regarding labor supply, respondent were asked whether they were working at the time of the interview. Those who were working were asked the type of job, selecting from following list: professional/technical/managerial, clerical, sales, agricultural – self-employed, agricultural – employee, sales and services, skilled manual, and unskilled manual. The non-agricultural category included professional/technical/managerial, clerical, sales, sales and services, skilled manual, unskilled manual. Respondents were also asked whether they were working for a family member, someone else, or were self-employed.

As for Table 5-8, t measure wives' autonomy, respondents with partners were asked whether they had a final say in the following household decisions: own health care, large household purchases, household purchases for daily needs, visits to relatives. To measure the acceptance of domestic violence committed by partners, respondents were asked whether beating by a partner was justified if one: went out without telling one's husband, neglected one's children, argued with one's husband, refused to have sex with one's husband, and burned the food. I used the number of affirmative answers as an indication of the acceptance of domestic violence. To measure knowledge regarding HIV/AIDS infection, respondents were asked whether the following reduced their chances of getting AIDS: always using condoms during sex, and having only one sex partner. Respondents were also asked whether AIDS could be transmitted by any of the following: being bitten by a mosquito, or sharing food with a person who has AIDS. I used the number of correctly answered questions as an indication of knowledge regarding AIDS infection.

Appendix 5.4: Distribution of girls by grade attended in 2005, 2009, and 2012

The impact of education identified in this dissertation may be taken to largely reflect that of the UPE policy, because only approximately one third of the treated cohorts benefited from the USE policy. Table 5A-3 indicates that at least two thirds of the treated cohorts did not benefit from the USE policy. Panels A, B, and C show the share of women born in 1992 and 1993 by grade attended in 2006, 2009, and 2012, respectively. In 2006, 16% of those born in 1992 were already attending secondary school and 14% were not attending any school. Since the USE policy affected students who sat in the PLE in 2006 or later, that 30% of women were unlikely to have benefited from the USE policy. While the remaining 70% of women who were still in primary school could have proceeded to secondary school after 2007, Panel B shows that only about 23% of women born in 1992 and 1993 were in grades S1 to S3 in 2009. Some of these girls are likely to have entered secondary school before the USE policy started, and to have repeated a grade at the lower secondary level. Thus, the share of the treated cohorts exposed to the USE policy by 2009 is likely to be at most 23-24%. For about 29% of women born in 1992 and 47% of those born in 1993 who were still attending primary school, Panel C indicates that only 5% and 13% of the two respective cohorts were in grades S1 to S3 in 2012. Therefore, the share of women entering lower secondary school by 2012 is likely to be at most 28 and 37%

for women born in 1992 and 1993, respectively. Though there was 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 the opportunity costs of attending school would be very high. In summary, the average share of USE beneficiaries among the treated is at most 33%, or one in three.

Chapter 6: Conclusion

The primary aim of this dissertation was to determine the effects of Uganda's free secondary education policy on students' access to education, student body composition, and achievement. Given that such a free education policy promoted female educational attainment, the secondary goal was to estimate the non-monetary returns of such a policy and of female education in the context of early fertility and child health.

Chapter 4 presented some insightful findings. First, eliminating secondary school fees appears to have been effective in boosting the number of secondary school graduates, whereas the magnitude of the change differs largely by school type. As participating secondary schools become more accessible due to reduced cost, the number of 11th grade graduates increased in participating public and private secondary schools. In addition, in non-participating private schools, that number rose even more rapidly after the USE policy started. This surprising increase in private school graduates is likely to have derived from the entry of new private schools into the market. Indeed, the number of private schools increased in the districts in which the free secondary education policy promoted school enrolment to a greater extent. These results imply that the private sector played an important role in expanding access to secondary education in Uganda.

Secondly, in analyzing the change in student body characteristics in public (and private) secondary schools, the results illustrated that, after the policy change, private schools enrolled more students from lower socio-economic status households, in terms of wealth and educational attainment of the household head. On the other hand, these characteristics in public schools remained stable over time. These results suggest that some students who would have attended public secondary school might have moved to private secondary school under the USE policy scheme, and/or that children of lower socio-economic status entered USE participating private schools.

Third, I found that the learning environment, in terms of physical and human resources available per student in participating secondary schools and non-participating private secondary schools, decreased due to rapid increases in enrolment. The change was greatest in participating private secondary schools. The free secondary education policy was also likely to produce class size effects in these schools, possibly placing downward pressure on the average test score of secondary school graduates.

Fourth, free secondary education reduced the average test score of 11th grade graduates in the secondary school exit exam. This is likely to have been due to two changes in the education market. First, the characteristics of students who took the secondary school exit exam changed, by allowing more students from lower socio-economic households to complete 11th grade. Secondly, the learning environment may have suffered from increased enrolment caused by the USE policy. The former change is inevitable as long as such policy aims to provide equitable access to the whole population, and is therefore not a key problem. On the other hand, the government must exercise caution to mitigate the negative effects of the latter change.

Furthermore, I investigated the heterogeneous change in test scores by school type, and the results suggested that the average test scores in public secondary schools and non-participating private schools were likely to remain at pre-program levels, whereas, in participating private secondary schools, the average test scores fell to far greater extent. This is consistent with the abovementioned finding of the change in learning environment and student body characteristics by school type. The results also showed that the fall in the average test scores did not occur in the traditional participating private schools that existed prior to the policy implementation, whereas the average test scores of graduates from new participating private schools were lower than those of other graduates.

Against the general background of limited pre-program access to secondary education, this study concludes that a free secondary education policy is welfare improving for a country, in that it increases the number of secondary school graduates with few negative effects on their academic achievement in traditional secondary schools existing prior to the program, and it further allows more students to complete secondary education in new secondary schools, although that their academic achievement might be relatively lower than that of other graduates.

Chapter 5 offered the important new finding that Uganda's free education policy improved female education more in historically disadvantaged districts, and effectively reduced the incidence of early pregnancy. The results illustrated that such change is produced through the frequent use of modern contraceptive methods, whereas the timing of sexual debut was unaffected by female education. While the literature discusses the incarceration and human capital effects of education as the mechanism through which it lowers fertility, I found some evidence suggesting that both were at work.

The second finding was that the likelihood of child death before 12 months fell among those of more educated mothers. The results illustrated that such educated women are more likely to practice better health investment, such as the use of formal delivery care, breastfeeding, and infant vaccination. As a result, their children are less likely to die between the 2nd and 12th months. Given such findings, I propose that the benefits of a free education policy are not limited to advancement in educational attainment, but also extend to the reduced risk of adolescent pregnancy, better health practices, and healthier babies. Thus, policy makers instituting such policies should explicitly acknowledge such positive externality, not underestimating its true benefits to the country, and not making unwise policy decisions.

One limitation of this study is that the available data allowed only the examination of short term effects of a free education policy. Chapter 4 examined the effects on students who entered secondary school just after the policy change. However, such effects may differ in the longer term. For example, such a policy allows a larger number of students to complete secondary education, and secondary school graduates increasingly enter the labor market. This increased labor supply with secondary education certificates may change the wage premium of attaining secondary education, and hence students' incentive to attend school up to this level. Chapter 5 discusses the effects of female education on giving birth before the age of 18 years and the health of children younger than 12 months. However, the data did not allow an examination of the longer term effects on outcomes such as total fertility, and child human capital, including their educational attainment. Thus, comparing the long term effects of such a policy with the results of this study might be even more insightful for policy makers, and is thus an important avenue for future research.

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Table 4-1 Change in number of test takers, schools, and test scores before and after 2010

Variables	Pre-USE	Post-USE	Change
Number of school	2113	2654	541
Government school	751	861	110
Private school	1362	1793	431
Number of test takers per year	184661	260384	75723
Government school	70045	100685	30640
Private school	114616	159699	45083
Standardized English test score	-0.00	-0.00	0.00
Government school	-0.19	-0.10	0.09
Private school	0.21	0.10	-0.11
School resources in government school			
Number of enrolment per school	449	543	94
Number of classroom per school	9.9	10.0	0.1
Number of sitting space per school	394	461	66
Number of teachers per school	24.4	25.3	0.9
Number of teachers with certificate per school	15.6	20.4	4.8

Note: Author's calculation using UCE and ASC data between 2006 and 2012. Values for the pre-USE period were calculated by taking the average of the values in 2006, 2007, 2008, and 2009. Those for the post-USE period were calculated by taking the average of the values in 2010, 2011, and 2012.

Table 4-2 Calculation of program intensity measure in the median district Iganga

Number of population who completed					Potential increase in the number of			
7th grade (A)	8th grade (B)	9th grade (C)	10th grade (D)	11th grade (E)	UCE taker (F)=(A)-(E)	8th grade (G)=(A)-(B)	9th grade (H)=(A)-(C)	10th grade (I)=(A)-(D)
10831	9661	9034	8550	8155	2676	1170	1797	2281

Variable	Definition	2006	2007	2008	2009	2010	2011	2012
Intensity (S4 intensity)	=(F)/(E) after 2010, 0 otherwise	0.000	0.000	0.000	0.000	0.328	0.328	0.328
S1 intensity	=(G)/(B) after 2007, 0 otherwise	0.000	0.121	0.121	0.121	0.121	0.121	0.121
S2 intensity	=(H)/(C) after 2008, 0 otherwise	0.000	0.000	0.199	0.199	0.199	0.199	0.199
S3 intensity	=(I)/(D) after 2009, 0 otherwise	0.000	0.000	0.000	0.267	0.267	0.267	0.267
Cumulative intensity	see section 3.2 for calculation	0.000	0.033	0.084	0.148	0.191	0.191	0.191

Table 4-3 Characteristics of high intensity districts

	(1) Intensity
Adult population	0.000
	(0.000)
Child population	-0.000
	(0.000)
Schooling year	0.040***
	(0.011)
unemployment20s	0.343
	(0.264)
Share of household which owns radio	0.152***
	(0.035)
Share of household which resides in Urban	-0.066*
	(0.039)
Share of household which has access to electricity	-0.066
	(0.368)
N	109
R^2	0.610

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Author's calculation using the UHPC of 2003.

Table 4-4 Effects of the USE policy on the number of 11th grade graduates by school type (district school type level)

Dependent variable: Number of students who took the secondary school exit exam

	(1)	(2)
Intensity*Government	344.964	-223.125**
	(263.185)	(102.880)
Intensity*Private	2307.673***	1949.650**
	(839.976)	(838.599)
Intensity*Government*USE		791.215***
		(117.468)
Intensity*Private*USE		-1591.627*
		(864.522)
N	1526	3052
R^2	0.099	0.108

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Author's calculation using UCE data between 2006 and 2012. All specifications control for district fixed effect, year fixed effect, school type fixed effects, and district-year cohort size of population in 2003. Standard errors are clustered at district level.

Table 4-5 Effects of the USE policy on completion of lower secondary education by school type (school level)

Dependent variable: Number of students who took the secondary school exit exam at each test center

	(1)	(2)
Intensity*Government	6.833	-45.302***
	(11.604)	(11.363)
Intensity*Private	-37.871***	-34.298***
	(9.621)	(9.169)
Intensity*Government*USE		85.474***
		(9.107)
Intensity*Private*USE		29.410***
		(9.416)
N	9912	9912
R^2	0.186	0.212

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Author's calculation using UCE data between 2006 and 2012. All specifications control for school fixed effect, year fixed effect, and district-year cohort size of population in 2003. Standard errors are clustered at district level.

Table 4-6 Effects of the USE policy on school enrolment (district cohort level)

Dependent variable: Number of students attending...

	(1)	(2)	(3)	(4)
	8th grade(S1)	9th grade(S2)	10th grade(S3)	11th grade (S4)
S1Intensity	2088.417***			
	(646.529)			
S2Intensity		1552.522***		
		(471.690)		
S3Intensity			1008.897***	
			(331.077)	
S4Intensity				692.664***
				(241.430)
N	783	783	783	783
R^2	0.398	0.508	0.503	0.524

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Author's calculation using ASC data between 2006 and 2012. All specifications control for district fixed effect, year fixed effect, school type fixed effects, and district-year cohort size of population in 2003. Standard errors are clustered at district level.

Table 4-7 Effects of the USE policy on enrolment by gender and orphan status (district school type level)

Dependent variable: Total number of enrolments between 8th and 11th grades.

	(1) Total enrolment	(2) Male	(3) Female	(4) Orphan
Cumulative intensity	5936.327***	2468.390***	3467.937***	913.669***
	(1759.711)	(898.097)	(893.186)	(332.496)
N	783	783	783	783
R^2	0.511	0.481	0.501	0.214

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Author's calculation using ASC data between 2006 and 2012. All specifications control for district fixed effect, year fixed effect, school type fixed effects, and district-year cohort size of population in 2003. Standard errors are clustered at district level.

Table 4-8 Change in student and household characteristics of those attending 8th grade by school type

Change in the household per capita expenditure and educational attainment (1 if completed primary school) of household head

	(1) ln[PCE] (Gov.)	(2) ln[PCE] (Pri.)	(3) Primary (Gov.)	(4) Primary (Pri.)
Intensity	0.725	-1.332	0.484	-0.988**
	(1.616)	(1.067)	(0.652)	(0.468)
N	418	403	355	353
R^2	0.090	0.164	0.038	0.019

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Author's calculation using Uganda National Household Survey data in 2006 and 2012. All specifications control for district fixed effect, year fixed effect, and dummy variable which takes value of 1 if student is female. Standard errors are clustered at district level.

Table 4-9 Effects of the USE policy on school supply by school type

	(1) Total	(2) Government	(3) Private	(4) PPP	(5) non-PPP
Cumulative intensity	38.157**	1.091	37.066**	4.355	32.711**
	(18.682)	(2.653)	(17.085)	(2.885)	(15.509)
N	777	777	777	777	777
R^2	0.363	0.302	0.317	0.371	0.223

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Author's calculation using UCE data between 2006 and 2012. All specifications control for district fixed effect, year fixed effect, school type fixed effects, and district-year cohort size of population in 2003. Standard errors are clustered at district level.

Table 4-10 Change in distance to the school currently attended by school type

Dependent variable: Distance to the school in km

	(1) Distance (Gov.)	(2) Distance (Pri.)
Intensity	0.188	-13.022***
	(5.289)	(4.651)
N	235	229
R^2	0.000	0.067

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Author's calculation using Uganda National Household Survey data in 2006 and 2012. All specifications control for district fixed effect, year fixed effect, school type fixed effects, and district-year cohort size of population in 2003. Standard errors are clustered at district level.

Table 4-11 Effects of the USE policy on characteristics of USE-participating government secondary schools (school level)

Panel A: The change in the number of classroom and adequate sitting space per school

	(1) Classroom	(2) Classroom per pupil	(3) Sitting space	(4) Sitting per pupil
Cumulative intensity	8.219***	-0.008*	618.760***	-0.007
	(1.393)	(0.004)	(52.988)	(0.070)
N	6224	6223	6239	6238
R^2	0.040	0.012	0.129	0.006

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Panel B: The change in the number of teacher and teacher with certificates per school

	(1) Teacher	(2) Teacher per pupil	(3) Trained teacher	(4) Trained per Pupil
Cumulative intensity	20.449***	-0.044***	44.142***	0.053***
	(1.761)	(0.010)	(2.401)	(0.009)
N	6232	6231	6268	6266
R^2	0.122	0.045	0.319	0.122

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Panel C: The change in the number of teacher left school per school by reasons

	(1) Outbound Transfer	(2) To other school	(3) For death	(4) For retirement
Cumulative intensity	3.035***	2.746***	-0.065	0.231**
	(0.586)	(0.483)	(0.130)	(0.099)
N	6239	6239	6239	6239
R^2	0.024	0.035	0.002	0.003

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Author's calculation using Uganda National Household Survey data in 2006 and 2012. All specifications control for school fixed effect, year fixed effect, and district-year cohort size of population in 2003. Standard errors are clustered at school level.

Table 4-12 Effects of the USE policy on the characteristics of non-participating government secondary schools (school level)

Panel A: The change in the number of classroom and adequate sitting space per school

	(1)	(2)	(3)	(4)
	Classroom	Classroom per pupil	Sitting space	Sitting per pupil
Cumulative intensity	-3.664	0.007	214.068	0.208
	(4.777)	(0.013)	(138.222)	(0.156)
N	1537	1537	1551	1551
R^2	0.061	0.017	0.048	0.008

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Panel B: The change in the number of teacher and teacher with certificates per school

	(1)	(2)	(3)	(4)
	Teacher	Teacher per pupil	Trained teacher	Trained per Pupil
Cumulative intensity	10.245*	0.040*	17.520	0.024
	(6.054)	(0.023)	(11.512)	(0.022)
N	1550	1550	1566	1565
R^2	0.127	0.072	0.155	0.110

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Panel C: The change in the number of teacher left school per school by reasons

	(1)	(2)	(3)	(4)
	Outbound Transfer	To other school	For death	For retirement
Cumulative intensity	7.297***	6.518***	0.130	0.037
	(2.329)	(2.175)	(0.286)	(0.270)
N	1551	1551	1551	1551
R^2	0.036	0.044	0.006	0.012

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Author's calculation using Uganda National Household Survey data in 2006 and 2012. All specifications control for school fixed effect, year fixed effect, and district-year cohort size of population in 2003. Standard errors are clustered at school level.

Table 4-13 Effects of the USE policy on the characteristics of participating private secondary schools (school level)

Panel A: The change in the number of classroom and adequate sitting space per school

	(1) Classroom	(2) Classroom per pupil	(3) Sitting space	(4) Sitting per pupil
Cumulative intensity	3.105*	-0.033***	827.064***	-0.024
	(1.720)	(0.008)	(74.704)	(0.108)
N	3832	3828	3853	3849
R^2	0.013	0.028	0.189	0.011

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Panel B: The change in the number of teacher and teacher with certificates per school

	(1) Teacher	(2) Teacher per pupil	(3) Trained teacher	(4) Trained per Pupil
Cumulative intensity	16.200***	-0.078***	34.297***	0.047***
	(2.158)	(0.013)	(2.708)	(0.010)
N	3843	3839	3880	3876
R^2	0.122	0.120	0.170	0.073

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Panel C: The change in the number of teacher left school per school by reasons

	(1) Outbound Transfer	(2) To other school	(3) For death	(4) For retirement
Cumulative intensity	1.772**	1.623***	-0.097	-0.055
	(0.860)	(0.608)	(0.127)	(0.129)
N	3852	3852	3852	3852
R^2	0.005	0.009	0.004	0.002

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Author's calculation using Uganda National Household Survey data in 2006 and 2012. All specifications control for school fixed effect, year fixed effect, and district-year cohort size of population in 2003. Standard errors are clustered at school level.

Table 4-14 Effects of the USE policy on the characteristics of non-participating private secondary schools (school level)

Panel A: The change in the number of classroom and adequate sitting space per school

	(1) Classroom	(2) Classroom per pupil	(3) Sitting space	(4) Sitting per pupil
Cumulative intensity	5.648	-0.044*	118.667***	-0.422**
	(7.475)	(0.026)	(28.678)	(0.183)
N	9351	9315	9474	9438
R^2	0.002	0.019	0.010	0.004

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Panel B: The change in the number of teacher and teacher with certificates per school

	(1) Teacher	(2) Teacher per pupil	(3) Trained teacher	(4) Trained per Pupil
Cumulative intensity	7.462***	-0.078***	21.355***	0.024*
	(1.444)	(0.020)	(1.990)	(0.015)
N	9427	9391	9524	9488
R^2	0.071	0.024	0.112	0.034

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Panel C: The change in the number of teacher left school per school by reasons

	(1) Outbound Transfer	(2) To other school	(3) For death	(4) For retirement
Cumulative intensity	1.981***	0.291	0.224**	-0.031
	(0.626)	(0.327)	(0.088)	(0.073)
N	9472	9472	9472	9472
R^2	0.012	0.006	0.002	0.001

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Author's calculation using Uganda National Household Survey data in 2006 and 2012. All specifications control for school fixed effect, year fixed effect, and district-year cohort size of population in 2003. Standard errors are clustered at school level.

Table 4-15 Effects of the USE policy on standardized secondary school exit exam scores by subject (district level)

Panel A: Overall effects on students' achievement

	(1) English	(2) Math	(3) Physics	(4) Chemistry	(5) Biology
Intensity	-0.61903***	-0.38225***	-0.46527***	-0.53794***	-1.33135***
	(0.22511)	(0.13949)	(0.16136)	(0.16651)	(0.20599)
N	1441	1441	1441	1441	1441
R^2	0.681	0.500	0.519	0.535	0.573

Panel B: Heterogenous effects by school type

USE participating government secondary school

	(1) English	(2) Math	(3) Physics	(4) Chemistry	(5) Biology
Intensity	-0.13965	-0.14261	0.12070	0.08601	-0.20991
	(0.24300)	(0.19101)	(0.21974)	(0.19642)	(0.28437)
N	751	751	751	751	751
R^2	0.834	0.832	0.821	0.794	0.773

Non participating government secondary school

	(1) English	(2) Math	(3) Physics	(4) Chemistry	(5) Biology
Intensity	-0.21191	-0.26746	-0.37558	-0.74417	-1.27262***
	(0.44998)	(0.40152)	(0.43614)	(0.47101)	(0.37897)
N	288	288	288	288	288
R^2	0.922	0.917	0.896	0.887	0.900

USE participating PPP private secondary school

	(1) English	(2) Math	(3) Physics	(4) Chemistry	(5) Biology
Intensity	-0.78099***	-0.80658***	-0.48912*	-0.30134	-0.72604***
	(0.24428)	(0.22932)	(0.25000)	(0.25560)	(0.23247)
N	620	620	620	620	620
R^2	0.756	0.735	0.674	0.636	0.673

Non participating private secondary school

	(1) English	(2) Math	(3) Physics	(4) Chemistry	(5) Biology
Intensity	0.05243	0.25674	0.12675	0.28407	-0.41005*
	(0.25899)	(0.18619)	(0.19532)	(0.19779)	(0.24486)
N	615	615	615	615	615
R^2	0.911	0.855	0.856	0.861	0.860

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Author's calculation using UCE data between 2006 and 2012. All specifications control for district fixed effect, year fixed effect, school type fixed effects, and district-year cohort size of population in 2003. Standard errors are clustered at district level.

Table 4-16 Effects of the USE policy on standardized secondary school exit exam scores by subject (school level)

USE participating government secondary school

	(1) English	(2) Math	(3) Physics	(4) Chemistry	(5) Biology
Intensity	-0.05448	-0.16019	0.09422	0.13421	-0.11981
	(0.13819)	(0.13928)	(0.15578)	(0.15211)	(0.15925)
N	4728	4728	4728	4728	4728
R^2	0.818	0.798	0.795	0.771	0.762

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Non participating government secondary school

	(1) English	(2) Math	(3) Physics	(4) Chemistry	(5) Biology
Intensity	0.15865	-0.07086	0.18447	-0.27426	-0.73187*
	(0.25967)	(0.32260)	(0.42589)	(0.54342)	(0.40933)
N	681	681	681	681	681
R^2	0.946	0.950	0.926	0.912	0.923

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

USE participating PPP private secondary school

	(1) English	(2) Math	(3) Physics	(4) Chemistry	(5) Biology
Intensity	-0.31828	-0.54848***	-0.38882	-0.23206	-0.33226
	(0.19925)	(0.19931)	(0.24342)	(0.27539)	(0.24023)
N	2079	2079	2079	2079	2079
R^2	0.742	0.735	0.658	0.600	0.679

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Non participating private secondary school

	(1) English	(2) Math	(3) Physics	(4) Chemistry	(5) Biology
Intensity	-0.25588	-0.52471*	-0.20038	-0.33405	-0.64720*
	(0.19499)	(0.28886)	(0.27987)	(0.64728)	(0.36370)
N	2405	2405	2405	2405	2405
R^2	0.958	0.947	0.924	0.911	0.921

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Author’s calculation using UCE data between 2006 and 2012. All specifications control for school fixed effect, year fixed effect, and district-year cohort size of population in 2003. Standard errors are clustered at school level.

Table 4-17 Robustness check with additional control (1)

Dependent variable: Number of students who took UCE exam

	(1) Baseline	(2) Region*year	(3) Unemployment	(4) Without North
Intensity*Government	344.964	402.034	287.111	287.326
	(263.185)	(285.218)	(296.771)	(329.115)
Intensity*Private	2307.673***	2364.743***	2249.820***	2410.731**
	(839.976)	(813.743)	(796.629)	(1004.330)
N	1526	1526	1526	1134
R^2	0.099	0.102	0.101	0.112

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Author's calculation using UCE data between 2006 and 2012. All specifications control for district fixed effect, year fixed effect, school type fixed effects, and district-year cohort size of population in 2003. Standard errors are clustered at district level.

Table 4-18 Robustness check with additional control (2)

Dependent variable: Average standardized English test score

	(1) Baseline	(2) Region*Year	(3) Unemployment	(4) Without North
Intensity*Government	-0.12363	-0.25919	-0.26953	-0.29039
	(0.31524)	(0.34963)	(0.34388)	(0.30811)
Intensity*Private	-0.51976**	-0.66088**	-0.68874**	-0.70367***
	(0.23153)	(0.27191)	(0.32603)	(0.23148)
N	1441	1441	1441	1083
R^2	0.779	0.784	0.780	0.779

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

	(1) Baseline	(2) Region*Year	(3) Unemployment	(4) Without North
Intensity	-0.61903***	-0.70440**	-0.73494**	-0.79602***
	(0.23455)	(0.27944)	(0.32031)	(0.23726)
N	759	759	759	563
R^2	0.955	0.962	0.957	0.961

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Author's calculation using UCE data between 2006 and 2012. All specifications control for district fixed effect, year fixed effect, school type fixed effects, and district-year cohort size of population in 2003. Standard errors are clustered at district level.

Table 5-1 Effects of the UPE policy on years of schooling

Outcome	Years of Schooling				
	(1)	(2)	(3)	(4)	(5)
1 if Born in 1992-1993	2.46***				
	(0.78)				
pre-program primary education completion rate	11.78***				
	(1.43)				
1 if Born in 1992-1993*pre-program primary education completion rate	-3.73**	-5.85***	-9.22***	-10.35***	-9.74***
	(1.43)	(1.22)	(1.83)	(1.80)	(1.82)
Craig Donald F-Statistics	6.8	22.7	25.4	33.1	28.5
Number of Observation	1472	1472	1472	1472	1472
District Fixed Effect	No	Yes	Yes	Yes	Yes
Year of Birth Fixed Effect	No	Yes	Yes	Yes	Yes
4 Regions*Year of birth Fixed Effect	No	No	Yes	Yes	Yes
Cohort providing information on pre-program primary education completion rate (Age in 1991)	21-25	21-25	21-25	26-30	21-30

Note: Author's calculation using UDHS 2001 and 2011 data. All specifications control for the number of government primary schools within the district when and where a woman was at the age of six, district level share of LC1 with public hospital or health center within its boundary and that of LC1 with bank branch within its boundary when each age cohort was around the age of 14, and religion (Muslim, Catholic, Protestant, and Other as reference groups). District fixed effect refers to 35 districts existing in 1991. Standard errors are clustered at district level. Control cohort is women born in 1978-1980. *** indicates significance at 1% level. ** indicates significance at 5% level. * indicates significance at 10% level.

Table 5-2 Placebo test for mean reversion

False Treatment; Born 1979-80 ; Control Born 1965-67		Treatment; Born 1992-93 ; Control Born 1978-80		
Dependent Var.	Year of Schooling		Year 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	-9.00***	-6.21***
	(2.11)		(2.08)	(1.63)
Craig Donald F-Statistics	0.3		18.7	14.5
Number of Observation	956		1475	1475
pre-program P7 completion rate	34-38		21-25	34-38

Note: Author's calculation using UDHS 2001 data. All specifications control for district fixed effect, single year of birth fixed effect, a set of interactions between four regions and birth year fixed effects, the number of government primary schools within the district when and where a woman was at the age of six, and religion (Muslim, Catholic, Protestant, and Other as reference groups). District fixed effects refer 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.

Table 5-3 Effects of education on the probability of adolescent pregnancy

Outcome: 1 if woman given birth before the age of 18	OLS	TSLS	TSLS
	(1)	(2)	(3)
Educational Attainment in year	-0.042***	-0.051**	-0.067***
	(0.003)	(0.024)	(0.020)
Craig Donald F-Statistics	N/A	22.7	25.4
4 Regions*Year of birth Fixed Effect	Yes	No	Yes
Number of Observation	1472	1472	1472

Note: Author's calculation using UDHS 2001 and 2011 data. All specifications control for district fixed effect, single year of birth fixed effect, the number of government primary schools in the district of a woman's residence at the age of six, district level share of LC1s with a public hospital or health center within its boundary and the district level share of LC1s with a bank branch within its boundary when each age cohort was around the age of 14, and the dummy variables indicating Muslims, Catholics, and Protestants. Those of 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.

Table 5-4 Effects of grade completion on the probability of adolescent pregnancy

Panel A: Effects of completion of P1, P2, P3, P4, and P5

Explanatory 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	-0.89***	-0.89***	-0.73***	-0.60***	-0.64***
	(0.33)	(0.29)	(0.21)	(0.15)	(0.16)
Coefficient of instrument at 1st stage	-0.69***	-0.69***	-0.84***	-1.03***	-0.95***
	(0.17)	(0.17)	(0.22)	(0.20)	(0.18)
Craig Donald F-Statistics	16.17	17.43	15.27	27.38	29.63
Number of Observation	1473	1473	1473	1473	1473

Panel B: Effects of completion of P6, P7, S1, S2, and S3

Explanatory 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	-0.76***	-0.65***	-0.67**	-1.23**	-1.39**
	(0.28)	(0.25)	(0.28)	(0.59)	(0.60)
Coefficient of instrument at 1st stage	-0.80***	-0.95***	-0.91***	-0.50*	-0.44**
	(0.17)	(0.22)	(0.25)	(0.21)	(0.17)
Craig Donald F-Statistics	22.23	17.89	13.44	5.55	6.81
Number of Observation	1473	1473	1473	1473	1473

Note: Author's calculation using UDHS 2001 and 2012 data. Table shows the results using 2SLS with instrument of "1 if born in 1992-93*pre-program P7 completion rate." All specifications limit the sample to the first birth of sample women. In Column 5, I limit the sample to children aged 1 month or older, in Column 6, to children e aged 12 months or older. In Columns 3, 4, 7, and 8, I limit the sample to those alive at the time of the interview. All specifications control for the set of dummy variables indicating the mother's age at birth, district fixed effect, mother's single year of birth fixed effect, a set of interactions between four regions and birth year fixed effects, and the number of government primary schools within the district when a mother was aged six, district level share of LC1 with public hospital or health center within its boundary and that of LC1 with bank branch within its boundary when each age cohort was around the age of 14, and mother's religion (Muslim, Catholic, Protestant, and Other as reference groups). District fixed effects refer to 35 districts existing in 1991. *** indicates significance at 1% level. ** indicates significance at 5% level. * indicates significance at 10% level.

Table 5-5 Effects of maternal education on health investment and child health

Panel A: Effects of education on the safe delivery and neonatal care use

Outcome	*1 if received delivery assistance by a medical professional*	*1 if gave a delivery at modern health facility*	*1 if a child has ever breastfed*	*1 if a child received BCG vaccination*
	(1)	(2)	(3)	(4)
Year of schooling	0.15**	0.15**	0.071**	0.86***
	(0.07)	(0.07)	(0.03)	(0.03)
Craig Donald F-Statistics	11.47	11.76	11.47	10.87
Number of Observation	630	626	630	553

Panel B: Effects of education on child mortality and morbidity

Outcome	*1 if die before the age of 1 month*	*1 if die before the age of 12 months*	*1 if a child suffered from diarrhea during last 2weeks*	*1 if a child suffered from fever during last 2weeks*
	(5)	(6)	(7)	(8)
Year of schooling	-0.024	-0.054**	-0.07	-0.04
	(0.02)	(0.03)	(0.05)	(0.03)
Craig Donald F-Statistics	10.67	11.01	11.79	11.79
Number of Observation	623	496	555	555

Note: Author's calculation using UDHS 2001 and 2011 data. Columns 1-4 in Panels A-C are the results using 2SLS with instrument of "1 if born in 1992-93*pre-program P7 completion rate." All specifications control for district fixed effect, single year of birth fixed effect, a set of interactions between four regions and birth year fixed effects, the number of government primary schools within the district when a mother was aged six, district level share of LC1 with public hospital or health center within its boundary and that of LC1 with bank branch within its boundary when each age cohort was around the age of 14, and religion (Muslim, Catholic, Protestant, and Other as reference groups). District fixed effects refer to 35 districts existing in 1991. In Columns 1-3, Panel E, I limit the sample to women who had ever had intercourse. In Columns 2-4, Panel F, I limit the sample to women who had ever been in union. Standard errors are clustered at district level. Mean of age at the first period is 14.5. *** indicates significance at 1% level. ** indicates significance at 5% level. * indicates significance at 10% level.

Table 5-6 Effects of education on timing of first pregnancy, marriage, and sexual intercourse

A: Effects on 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*
Schooling Year	-0.010	-0.22	-0.009	-0.011
	(0.019)	(0.028)	(0.024)	(0.020)
Craig Donald F-Statistics	25.40	25.40	25.40	25.40
Number of Observation	1417	1417	1417	1417
B:Effects on age at 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*
Schooling Year	-0.069***	-0.063***	-0.034***	-0.035***
	(0.015)	(0.016)	(0.012)	(0.009)
Craig Donald F-Statistics	25.40	25.40	25.40	25.40
Number of Observation	1472	1472	1472	1472
C: Effects on age at 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)
Schooling Year	-0.067***	-0.047***	-0.035***	-0.020**
	(0.020)	(0.015)	(0.014)	(0.010)
Craig Donald F-Statistics	25.40	25.40	25.40	25.40
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*
Schooling Year	0.041*	-0.015	0.029***	-0.016
	(0.02)	(0.17)	(0.07)	(0.02)
Craig Donald F-Statistics	24.04	24.04	24.04	25.40
Number of Observation	1235	1235	1235	1472

Note: Author's calculation using UDHS 2001 and 2011 data. This Table shows the 2SLS results for the effect of the number of years of completed schooling, where the excluded instrument is the interaction term between the dummy for women born in 1992-93 and the pre-program completion rate for primary education. All specifications control for district fixed effects, birth cohort fixed effects, a set of interactions between four regional dummies and birth cohort fixed effects, the number of government primary school within the district when a mother was aged six, district level share of villages (LC1s) with public hospital or health center within its boundary and that of LC1s with bank branch within its boundary when each age cohort was around the age of 14, and a set of dummy variables indicating the religion of women (Muslim, Catholic, Protestant, and Other as reference groups). District fixed effects refer to 35 districts existing in 1991. In Column 1-3, Panel D, I limit sample to women who have ever had intercourse. Mean of age at the first period is 14.5. *** indicates significance at 1% level. ** indicates significance at 5% level. * indicates significance at 10% level.

Table 5-7 Effects of education on labor supply, knowledge, pregnancy preferences, bargaining power, and assortative mating

A: Effects on literacy and labor market participation				
	1 if a woman is literate	*1 if a woman is currently working in any sector*	*1 if a woman is working in non-agricultural sector*	*1 if a woman is employed by someone other than family*
Schooling Year	0.088***	0.001	0.068	-0.001
	(0.19)	(0.03)	(0.04)	(0.04)
Craig Donald F-Statistics	25.84	25.40	19.77	19.91
Number of Observation	1467	1472	883	884
B: Effects on fertility preference, knowledge and behavior on disease infection				
	HIV Knowledge (0-4)	*1 if a woman knows the source of male condom*	*1 if household has mosquito net*	*Ideal number of children*
Schooling Year	0.014	-0.032*	0.093***	-0.13*
	(0.04)	(0.02)	(0.02)	(0.07)
Craig Donald F-Statistics	30.14	25.32	25.40	23.52
Number of Observation	1402	1471	1472	1447
C: Effects on acceptance of domestic violence, autonomy, partner's age, and educational attainment				
	Acceptance of domestic violence by a husband (0-5)	*1 if a woman can make a decision on her medical expence alone*	*Partner's age*	*Partner's educational attainment*
Schooling Year	0.026	0.019	-0.051	0.41
	(0.15)	(0.32)	(0.31)	(0.36)
Craig Donald F-Statistics	23.01	21.41	15.82	24.40
Number of Observation	886	906	836	902

Note: Author's calculation using UDHS 2001 and 2011 data. Columns 1-4 in Panels A-C are the results using 2SLS with instrument of "1 if born in 1992-93*pre-program P7 completion rate." All specifications control for district fixed effect, single year of birth fixed effect, a set of interactions between four regions and birth year fixed effects, the number of government primary schools within the district when a mother was aged six, district level share of LC1 with public hospital or health center within its boundary and that of LC1 with bank branch within its boundary when each age cohort was around the age of 14, and religion (Muslim, Catholic, Protestant, and Other as reference groups). District fixed effects refer to 35 districts existing in 1991. In Columns 1-3, Panel E, I limit the sample to women who had ever had intercourse. In Columns 2-4, Panel F, I limit the sample to women who had ever been in union. Standard errors are clustered at district level. Mean of age at the first period is 14.5. *** indicates significance at 1% level. ** indicates significance at 5% level. * indicates significance at 10% level.

Table 5A-1a Summary statistics (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 first sexual intercourse before the age of 18	0.69	0.46	0.57	0.50
1 if had first sexual intercourse before the age of 17	0.55	0.50	0.42	0.49
1 if had first sexual intercourse before the age of 16	0.38	0.49	0.26	0.44
1 if had 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
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: Author's calculation using UDHS 2001 and 2011 data.

* All the district-cohort level characteristics were taken from the 1993 and 2006 UNHS data. The 1993 data provides the information when the controls 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 in 2006.

Table 5A-1b Summary statistics (pathway outcomes)

	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 ever	0.88	0.32	0.94	0.23
Polio vaccination ever	0.45	0.50	0.67	0.47
Measles vaccination ever	0.71	0.46	0.58	0.49
vitamin A vaccination ever	0.10	0.29	0.34	0.47
1 if ever breastfed	0.98	0.15	0.48	0.50
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
Child morbidity				
1 if a child suffered from				
diarrhea during last two weeks	0.13	0.13	0.28	0.28
fever during last two weeks	0.29	0.29	0.35	0.35
cough during last two weeks	0.33	0.33	0.44	0.44
Number of observations	356		274	

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

Table 5A-2 Effects of the UPE policy on years of schooling among partially treated cohorts

Outcome: Schooling year	Coefficient			
	(1)	Age in 1997	Observation	Data source
1 if born in1992-93	-5.19***	4,5	709	DHS2011
* pre-program P7 completion rate	(1.56)			
1 if born in1990-91	-0.96	6,7	659	DHS2011
* pre-program P7 completion rate	(1.07)			
1 if born in1988-89	0.066	8,9	589	DHS2011
* pre-program P7 completion rate	(1.05)			
1 if born in1986-87	-1.61	10,11	633	DHS2011
* pre-program P7 completion rate	(1.19)			
1 if born in1984-85	-0.71	12,13	541	DHS2011
* pre-program P7 completion rate	(1.43)			
1 if born in1982-83	-1.14	14,15	570	DHS2001
* pre-program P7 completion rate	(1.26)			
1 if born in1980-81	0.15	16,17	588	DHS2001
* pre-program P7 completion rate	(0.83)			
1 if born in1978-79 (Reference group)	-	18,19	510	DHS2001
* pre-program P7 completion rate	-			
Number of Observation	4457			

Note: Author's calculation using UDHS 2001 and 2011 data. All specifications control for district fixed effects, single year of birth fixed effects, a set of interactions between four regions and birth year fixed effects, the number of government primary schools within the district when a mother was aged of six, and religion (Muslim, Catholic, Protestant, and Other as reference groups) Standard errors are clustered at district level. Control cohort is women born 1978-1979. *** indicates significance at 1% level. ** indicates significance at 5% level. * indicates significance at 10% level.

Table 5A-3 Share of treated women attending each level of education in 2006, 2009, and 2012

Panel A: Share of women born in 1992 and 93 and attending primary and secondary school in 2006

Birth year	Age in 2006	P1-P7	S1-S3	S4-S6	College	Not attending	N
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: Share of women born in 1992 and 93 and attending primary and secondary school in 2009

Birth Year	Age in 2009	P1-P7	S1-S3	S4-S6	College	Not attending	N
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)

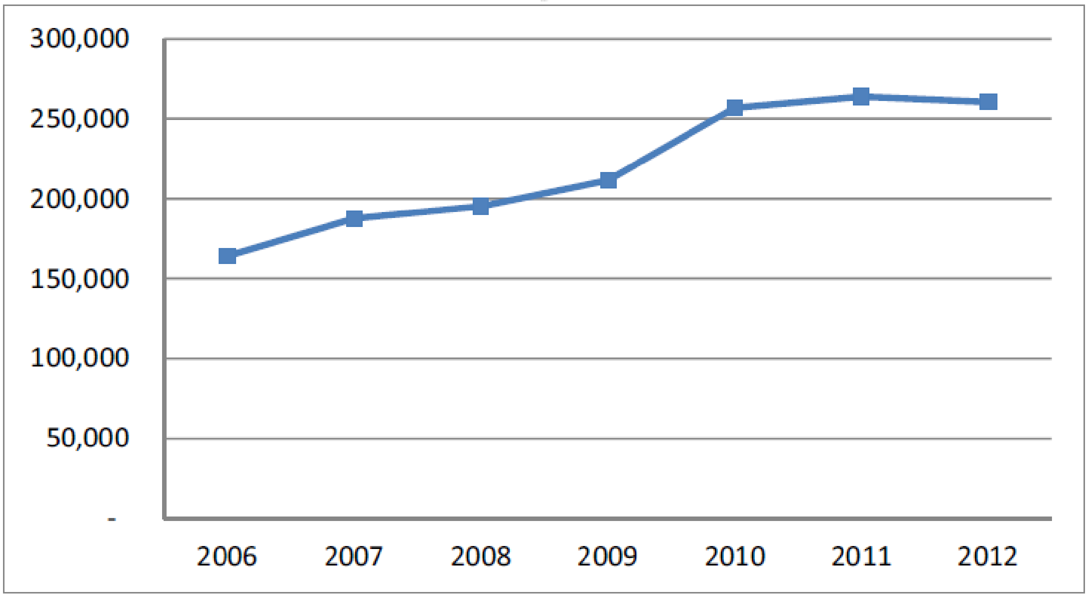
Panel C: Share of women born in 1992 and 93 and attending primary and secondary school in 2012

Birth Year	Age in 2012	P1-P7	S1-S3	S4-S6	College	Not attending	N
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.

Note: Author's calculation using UNHS 2006, 2009, and 2012 data. Share of women born in the same year and attending each level of education is shown in brackets.

Figure 4-1 Number of UCE candidates by year



Note: Author's calculation using UCE data between 2006 and 2012.

Figure 4-2 Calculation of intensity measure to assess the effects on the number of lower secondary school graduates and their learning achievement

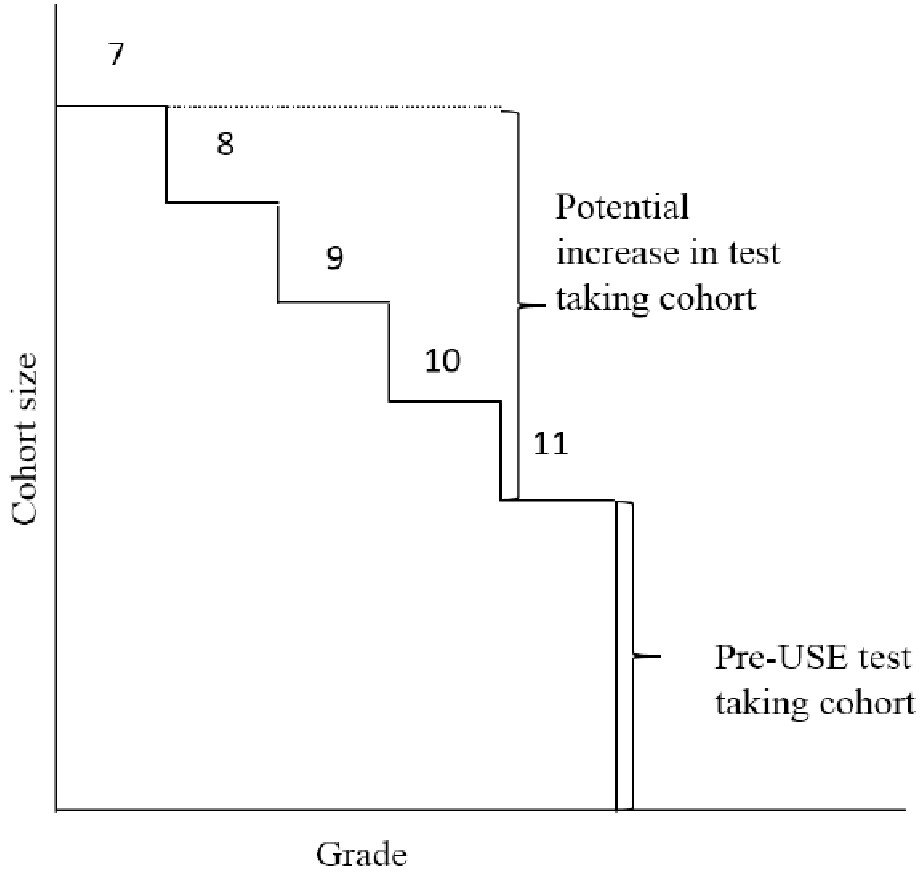
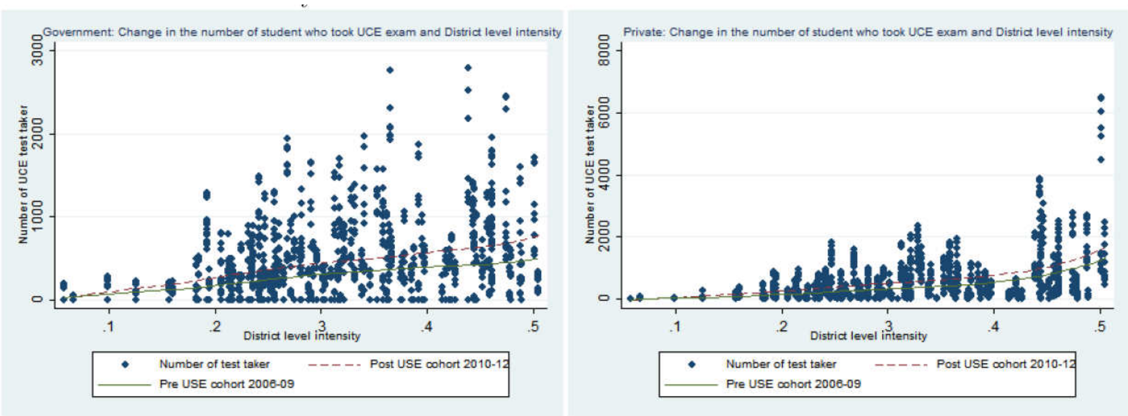
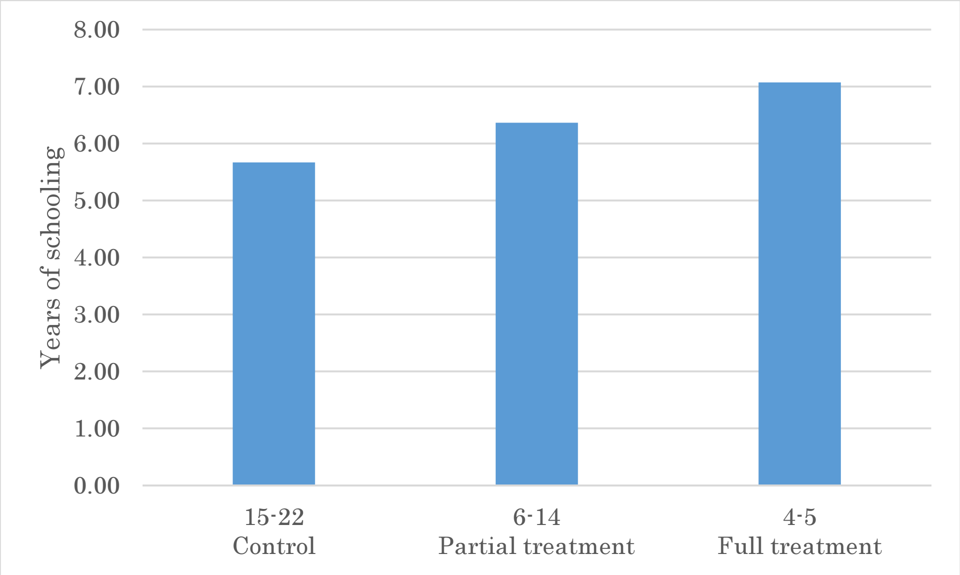


Figure 4-3 Positive correlation between an increase in the number of UCE candidates and district level intensity



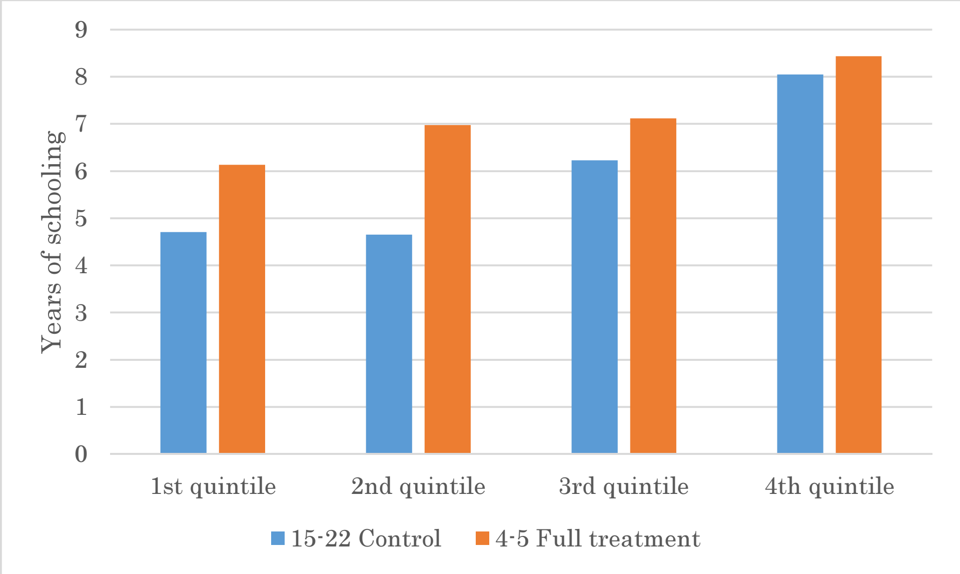
Note: Author's calculation using UCE data between 2006 and 2012.

Figure 5-1 Average years of schooling by age at the start of the UPE (1997)



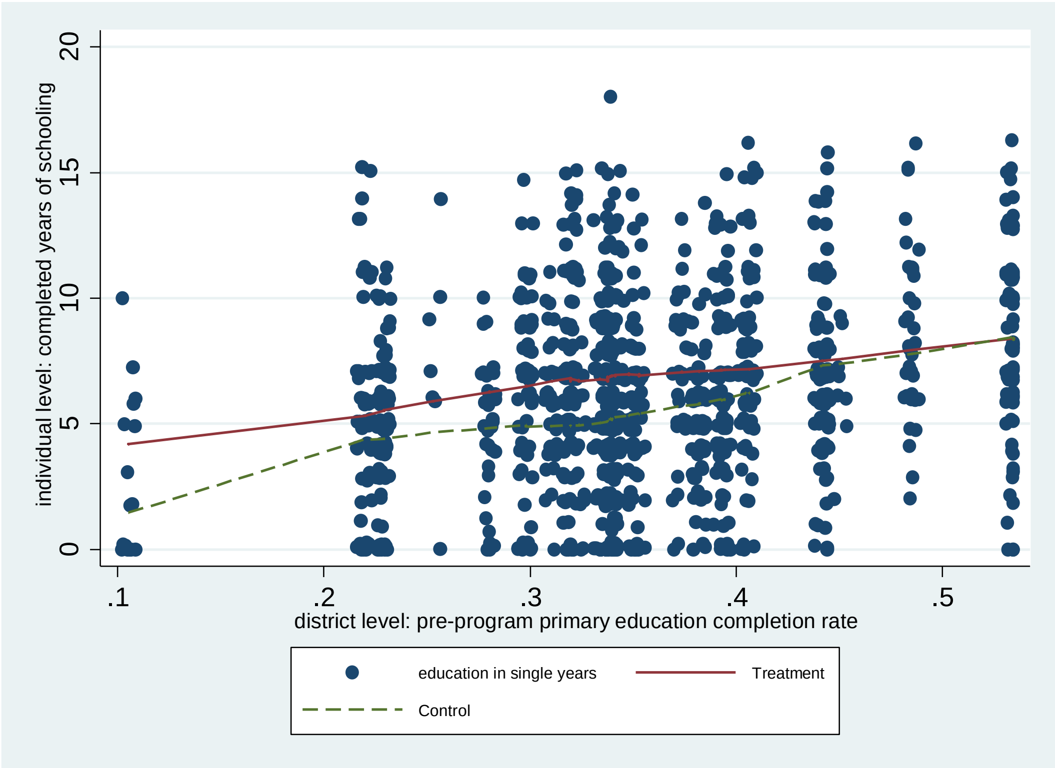
Note: Author's calculation. The number of completed years of schooling was asked at the ages of 19-26, 15-23, and 18-19 for the controls, partially treated, and fully treated from the UDHS 2001, 2006, and 2011 data, respectively.

Figure 5-2 Change in years of schooling by quartile of pre-program educational attainment



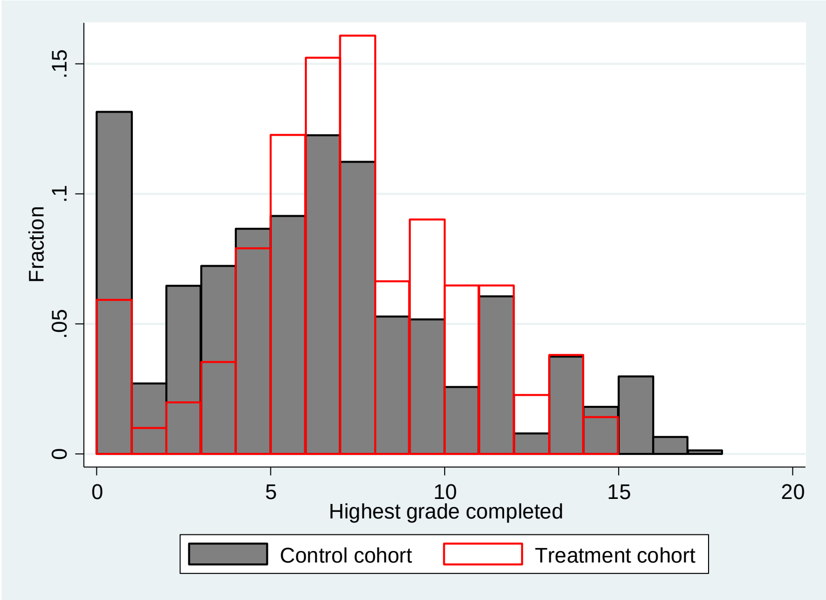
Note: Author's calculation using UDHS 2001, 2006, and 2011 data.

Figure 5-3 Negative correlation between pre-program primary completion rate and change in girls' educational attainment



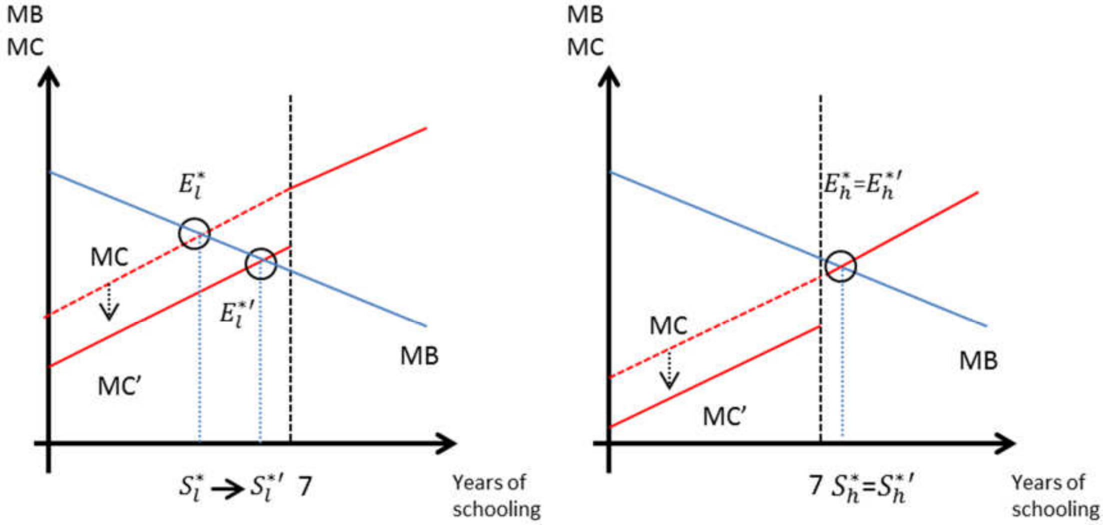
Note: Author's calculation using UDHS 2001 and 2011 data.

Figure 5-4 Distribution of completed years of education for control and treatment groups



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

Figure 5A-1 Household optimization of a child’s educational attainment



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

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

Figure 5A-2 Change in educational attainment among partially treated cohorts across regions