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The Long-Term Effects of Universal Primary Education: Evidence from Ethiopia, Malawi, and Uganda

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Abstract

This paper exploits the roll out of universal primary education (UPE) policies in Ethiopia, Malawi, and Uganda as natural experiments to assess their long-term causal effects on schooling attainment, adolescent sexual and reproductive health behavior, and economic outcomes. We use data from the Living Standards Measurement Survey (LSMS) and Demographic and Health Surveys (DHS) to exploit plausibly exogenous variation in UPE eligibility as determined by individuals' year of birth. Using the UPE rollout as an exogenous shock, we implement an instrumental variables (IV) design to identify the effect of primary education expansion on schooling completion and the consequent long-term effects on adolescent behavior, adult labor force participation, and socioeconomic status. Our findings confirm that the UPE policies were effective in increasing educational attainment in all three countries, while narrowing the pre-existing gender attainment gap. Additionally, we show that UPE is also successful in lowering the rates at which adolescents engage in sexual behavior, child birth, and marriage/cohabitation. However, in terms of labor market outcomes we find that the effects of schooling increases are moderate at best in improving employment, salaried employment, and poverty status among the affected populations.

JEL Classification: J16, O12, I24, I25

Keywords: Universal primary education, natural experiment, instrumental variables, educational attainment, gender gap, sexual and reproductive health, labor supply.

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1 Introduction

Two goals of the United Nations Millennium Development Goals (MDGs) are to achieve Universal Primary Education (UPE) and achieve gender equality and increased female empowerment by 2015. Over the past four decades, school participation rates for primary school age children has steadily increased in the developing world from approximately 74 percent to over 90 percent, as of 2014. In addition, although Sub-Saharan Africa has made similar strides in improving net attendance among its primary age population, the attendance rate still lags the rest of the developing world where net attendance rates have increased from only 44 percent to about 79 percent with girls still exhibiting lower attendance rates than boys (UIS, 2015). This roughly translates to about 34 million primary school age children who are out of school across all Sub-Saharan African countries. Figure 1 exhibits compelling evidence that the push toward universal primary education has been successful in providing access to schooling to millions of children with net attendance rates increasing by about 30 percentage points between 1995 and 2014. At the same time, survival rates to the last grade of primary school have not shown the same levels of progress as they have lingered at around 55 percent since the mid-1980s.

[INSERT FIGURE 1 HERE]

The literature on the effectiveness of providing tuition-free primary education on long-term outcomes such as educational attainment, health, and labor market outcomes is sparse. This is in part due to the relative recency in the adoption of UPE policies in Sub-Saharan African countries and the lack of adequate data to measure any potential impacts of UPE. For instance, Cameroon, Lesotho, Tanzania, Zambia, Kenya, and Timor-Leste eliminated primary school tuition fees between 2000 and 2003, making it difficult to properly evaluate long-term effects of UPE provisions (Kattan, 2006). In this paper, we focus on three East African countries who were early implementers of UPE--Ethiopia and Malawi implemented UPE policies in 1994, and Uganda, which eliminated primary school fees in 1997. This enables us to evaluate the long-term UPE effects as the first eligible cohorts are now at least 25 years of age, as of the most recent Living Standards and Measurement Survey (LSMS) and Demographic and Health Survey (DHS) data collections.

Langsten (2014) makes the argument that participation rates are not necessarily sufficient to evaluate UPE policies, but rather to ensure that the increased access to primary education also translates to at least a higher completion rate in primary school. We take this argument further and investigate whether investments made in ensuring universal access to primary education have translated to improved health and behavioral outcomes as well as improved economic and socioeconomic status. This research follows from Behrman (2015) who shows that increased access to primary schooling has had a beneficial effect on women's educational attainment and HIV prevalence in Malawi and Uganda. We add to this research by evaluating the long-term effectiveness of UPE in increasing attainment of women relative to the increase experienced by men as well as the consequent effects of schooling on marriage, fertility, and sexual activity as

adolescents. As adults, we examine the long-term effects of schooling on labor force activity, employment, and poverty status.

This paper contributes to the existing research literature in three ways. First, this paper adds to the education finance literature by examining the impacts of abolishing tuition fees, among other associated costs, on household decisions to enroll eligible children in primary school. Second, we examine the direct effects of UPE on educational attainment measured as years of schooling completed among men and women and the consequent effects on gender parity. Lastly, we formally evaluate the causal effects of schooling increases resulting from UPE in three East African countries almost 20 years post-implementation and examine changes in adolescent sexual and marital behavior, and adult labor force participation as a result of the estimated changes in educational attainment. This expands upon previous research which has mostly been constrained to examining immediate and short-run impacts of UPE on enrollment and at best primary school completion or attainment.[1]

We exploit the timing of the implementation of UPE policies in three countries as an instrument to determine, first, the effect of the policy on years of schooling completed, and, second, the consequent effect of schooling on long-term outcomes such as adolescent sexual and marital behavior, labor market outcomes, and poverty status. The timing of the UPE policies create a natural break in UPE take-up using year of birth-based eligibility criteria for primary school enrollment. The essence of the UPE policies was the abolition of tuition fees in primary school, which means that children who were of primary school age at the time of the implementation of the policy were eligible for free primary schooling, whereas those who were not of primary school age were not. However, as is common in many developing countries over-age children may still enroll in primary school now that it is tuition-free Riddell (2003), which means that compliance with UPE eligibility was imperfect.

Our empirical strategy follows Oreopoulos (2006), who evaluates the causal links between changes in the minimum compulsory attendance age in the United Kingdom using birth cohort membership as an instrument for years of schooling. Using a similar strategy, we instrument for years of schooling with UPE age eligibility determined by their birth cohort membership to determine the causal effects of schooling on fertility, marriage, labor market, and socioeconomic outcomes. Because we use UPE as the exogenous shift variable for schooling in our estimating equation, we can thus compute the net effect of UPE on outcomes through the schooling mechanism. We draw upon microdata from the Living Standards Measurement Surveys (LSMS) and Demographic and Health Surveys (DHS), where our study shows that UPE policies in Ethiopia, Malawi, and Uganda were successful in increasing mean years of schooling for women by between 1.2 and 1.8 years, on average. Further, we find that the effects on schooling were larger in magnitude for women than men, thus lowering the attainment gap by between 13 and 29 percent depending on the country. We also find that the consequent increase in years of schooling completed among women

[1] See Deininger (2003), Al-Samarai and Zaman (2007), Grogan (2009), Nishimura et al. (2008), and Langsten (2014).

because of UPE increases female employment by 2.1 to 8.4 percentage points, lowers the probability of teenage sexual activity by 6.6 to 7.6 percentage points, lowers the probability of teenage marriage and/or cohabitation by 3.5 to 13.7 percentage points, and lowers the probability of teenage births by 4 to 18.6 percentage points. In addition, we find that women in Malawi who were eligible for UPE are approximately 3 percentage points less likely to belong to the bottom 40 percent in terms of household wealth.

2 Country and Policy Context

2.1 Ethiopia

School fees in primary education and the first two years of lower secondary school were eliminated starting in 1994 in an effort to ameliorate student enrollment and literacy rates. Prior to the abolishment of school fees, the cost of attending public schools in Ethiopia included registration, maintenance, textbook, and certificate fees as schools were seen as a source of revenue to the government. At the same time, Ethiopia experienced a government regime change in the early 1990s that paved the way to a nation-wide policy to decentralize the public education system to woredas (districts) and to abolish school fees. However, World Bank (2009) states that initial challenges to fee abolition were that not all woredas had complied at the time of the passing of the new legislation and complete fee abolition did not reach the entire country until 1996. In view of the delay in full implementation of the fee elimination policy, this means that there is potentially a substantial proportion of two cohorts of children who did not receive tuition-free schooling despite being eligible. Although the planned roll out of UPE in Ethiopia was to be national and simultaneous across all districts, the actual implementation timeline was staggered between different regions.[2] In addition, in the process of eliminating school fees in Ethiopia, schools still required residual fees and/or community assistance to help cover the budgetary needs of the schools.

The immediate effects of the UPE policy in this case was somewhat evident when primary enrollment rates increased by more than 50 percent from 1993 through 1997. This growth in enrollment rates translates to increased access to primary school for almost 2 million children in total. However, at the same time, pupil-teacher ratios experienced similarly sharp increases as a direct result of the policy. Pupil-teacher ratios in Ethiopia between 1993 and 1997 increased from 27:1 to 43:1, this ratio as of 2014 stands at 64 pupils to every teacher.[3] This likely means that with the expansion of primary education access, the resources for primary schools were not expanded in a similar manner and could have implications on the quality of learning/schooling that the children are now gaining access to.

[2] The exact timing of school fee elimination, by woreda, is unknown, whereas we know that public primary schooling across all woredas was fee free as of 1996.

[3] Enrollment and pupil-to-teacher ratios are extracted from the UNESCO Institute for Statistics (UIS) database. $http://data.uis.unesco.org/Index.aspx?DataSetCode=EDULIT

2.2 Malawi

Prior to 1994, school fees were levied in Malawi public primary schools to cover the costs of schooling materials such as textbooks, writing material, and school uniforms with fees being relatively lower in rural than urban areas--tuition also increased with each grade level. Malawi followed a sequential primary school fee elimination approach to implementing UPE between 1991 and 1992. The Malawi government provided fee waivers for the first grade of primary school in 1991 and added a fee waiver for the second grade in 1992 (World Bank, 2009). However, in 1994, Malawi adopted a "big bang"' approach by essentially doing away with all school fees in all of public primary education, nationally. In addition to eliminating school fees, the government provided all primary school students with the required learning materials and removed school uniform requirements.

The abolition of school fees in Malawi had clear direct impacts on enrollment and teacher employment. Enrollment, in general, increased by about 50 percent and provided primary schooling access to over 1 million children. Surprisingly, pupil-teacher ratios were relatively unaffected going from 68:1 prior to the policy to 63:1 afterward. The relative stability of pupil-teacher ratios in Malawi was a result of some 20,000 newly hired teachers, the majority of whom were untrained (World Bank, 2009). Although Malawi took steps to ensure that existing classrooms are able to absorb the increase in demand, it is clear that the policy has implications on the quality of schooling being supplied to the larger population of children since the newly hired teachers were untrained and may not have been qualified instructors. It is however unclear, or unknown, whether these drastic changes to the structure and delivery of primary education (and possibly secondary education) had lasting effects on outcomes of the affected children as youths and adults.[4]

2.3 Uganda

Until the mid-1990s Uganda experienced high rates of out of school children, at around 60 percent, that were a result of expensive direct schooling costs (Kattan and Burnett, 2004). The Ugandan government committed to providing free and compulsory primary education starting in 1996 with the actual legislation being put into effect in 1997. Similar to Malawi, Uganda opted for the "big bang" approach to provide free primary education for all primary school age children in the country at once. However, unlike Malawi, Uganda did not follow a phased approach and instead enforced the fee-free policy from the beginning. Specifically, the policy eliminated tuition fees, provided textbooks free of charge, and school uniforms were no longer mandatory.[5]

Total enrollment in primary education also saw a sharp increase from the pre-policy to the post-policy period by approximately 70 percent. That is, total enrollment in primary education rose from 3.1 million to 5.3 million children. At the same time, pupil-teacher ratios saw similar growth

[4] ibid.

[5] The use of school uniforms remained the decision of the school boards, however, no child is to be excluded for lack of uniform.

rates from 38:1 to 59:1.[6] Again, it is clear that these drastic changes to the structure and delivery of primary education services in Uganda could have adverse effects on the quality of education. However, the long-term effects of UPE are unknown in the existing research literature. The theme that the quality of education may have been affected negatively as a result of nationwide expansion of primary education appears in all three countries. Specifically, Uganda and Ethiopia experience drastic increases in class sizes and seemingly little change in the human resources dedicated to primary education. While Malawi expanded teacher staffing to accommodate the rise in enrollment, the hired teachers were untrained and in this case the quality of learning may have been affected by a substantial proportion of teachers possibly having inadequate qualifications.

3 Theoretical Framework

Early human capital theory lays the foundations to understand educational choice, which posits the decision to attend school and for how long, or in other words invest in education, as a function that maximizes the expected lifetime benefits in relation to the costs of attendance (Becker, 1962). This naturally creates two margins at which determine the amount of schooling investments. The first is whether to attend or not, and the second determines the duration. The costs of attendance can thus be broken down into a direct cost, tuition fees (and other attendance related expenses), and an opportunity cost. The opportunity cost of attendance would be the foregone economic gains from engaging their children in the labor force rather than attending school. In Africa, child labor is not a negligible issue since almost a fifth of all primary age children are in child labor (ILO, 2017).

The classical formulation, thus, predicts that parents will send their child to school if they believe the future lifetime benefits from attendance outweigh the (direct and indirect) costs of attendance.[7] It is straightforward to see that the basic prediction, based on this framework, is that parents will send their child to school if the perceived benefits are higher, or if the costs associated with attendance are lower, holding all other factors fixed (Glewwe and Jacoby, 2004). We include a simple enhancement to this framework by allowing benefits to be a function of attainment, or the duration of schooling following the decision to attend. The prediction, in this case, shows that the decision to attend for a single year relative to none, i.e. the extensive margin, would come about when the benefits of a single year of schooling outweigh the costs of attending for a single year. Further, the model generates predictions for the decision to attend additional years over and above the first, the intensive margin. Using the same logic as at the decision's extensive margin, the higher the perceived returns on expanding the duration of schooling, or the lower the costs of schooling, the more likely parents are to allow their children to attend school for a longer period.

[6] ibid.

[7] In the classical economics literature, benefits have usually been measured in the form of monetary/wage returns to schooling. Black et al. (2007; 2011) and Oreopoulos and Salvanes (2011) show that the benefits of schooling extend beyond wage returns including better health outcomes and self-reported happiness.

As discussed earlier, the existing literature covers the extensive margin of this problem quite extensively.[8] However, the intensive margin is a much less discussed topic, especially in the Sub-Saharan African context.[9] Empirically, to identify the causal effect of investments in education on the amount of associated benefits, health or economic activity, we would require an exogenous change in the household schooling function that would either increase the benefits of education or lower the costs thereof. As such, we test this theory by evaluating the effects of a free tuition policy in primary education in Ethiopia, Malawi, and Uganda that lowered the direct cost of attendance.[10] The UPE policy, in this case, acts as the exogenous variable that shifts the costs of attendance down, thus relaxing the household budget constraint enabling us to measure the actual returns on health and economic outcomes.

4 Data and Empirical Methods

The data in this paper are drawn from the most recent versions of two publicly accessible household surveys, the Living Standards Measurement Survey (LSMS)--administered by the World Bank--and the Demographic and Health Survey (DHS)--administered by the United States Agency for International Development--for Ethiopia, Malawi, and Uganda. Across all the countries included in this paper, we condition the final analytic sample to cohorts of individuals born between 1969 and 1992. As a result, individuals in the LSMS are aged between 20 and 42 years, and the same cohorts from the DHS are aged between 18 and 40.[11] Table 1 details the data sources, the years of available data, the years the UPE policy went into effect, and the birth cohorts who are eligible as of the date of implementation and those who are eligible as of the first year of formal schooling.

[INSERT TABLE 1 HERE]

4.1 Living Standards Measurement Surveys

Data from the LSMS are nationally representative household surveys designed and administered by the World Bank in collaboration with each country's national statistics office. The LSMS data provide a unique source of micro-level data covering detailed information on family and background characteristics, education, labor force participation, income, and household expenditures.[12] For the purposes of this study, the distinct advantage the LSMS holds over other similar household surveys are the detailed labor market outcomes data that include labor force activity, employment, type of employment, and wages. We also collect information on individuals' basic family and demographic background characteristics including marital status, number of

[8] ibid.
[9] See long list in introduiction.
[10] As discussed previously, all countries included in this paper eliminated tuition fees at the primary education level, yet Malawi and Uganda also eliminated other expenses such as textbooks and uniforms.
[11] The difference in age is due to the DHS surveys being two years older than the LSMS. However, the final analytic samples across both datasets are for the same birth cohorts.
[12] Specific administrations of the LSMS will include specific topics that are uncommon such as agriculture, social safety nets, fertility, and household shocks, depending on the country. http://go.worldbank.org/UJ3E1I2850

children, urban or rural residence, subnational region of residence, and educational attainment and literacy.

Educational attainment is central to this paper as the primary and most immediate goal of any UPE policy. To ensure systematic analyses across countries and data sources, we convert categorical educational attainment into a continuous years of schooling variable. The LSMS collects detailed educational attainment information from all survey respondents including highest level as well as grade attained, which enables us to compute exact years of schooling for each individual in the sample.[13] Lastly and most important to this study, the LSMS collects exact date of birth (age) information for all respondent. This enables us to identify birth cohorts as well identify UPE eligibility groups for each country.

[INSERT TABLE 2 HERE]

Table 2 displays the sample statistical summaries for each country grouped by labor force outcomes, educational outcomes, and demographics, disaggregated by gender. As mentioned earlier in this section, we restrict the final analytic sample to observations whose date of birth falls between 1969 and 1991, yielding a final sample size of 7,979 individuals in Ethiopia, 6,365 individuals in Malawi, and 3,809 individuals in Uganda, with an average age of approximately 29 years across all countries in the study. We observe that almost four fifths of respondents in each sample are located in rural locations and at least two thirds are currently married. In terms of education and literacy, the gender gaps in Ethiopia appear to be the most pronounced with only 40 percent of adult females being literate relative to 71 percent of males, whereas the literacy gap in Malawi and Uganda are at most within 12 percentage points.

Labor force outcomes vary somewhat substantially across three East African countries that are considered "least developed" as per United Nations definitions. For instance, the percentage of the population (aged 20-42 years) who are either employed or unemployed in Ethiopia is about 65 percent for females and 78 percent for males, whereas in Malawi that percentage is 80 percent for females and almost 86 percent for males, and 91 percent for females and 97 percent for males in Uganda. However, Ethiopia boasts a lower unemployment-to-population ratio than Malawi despite the lower labor force activity rate. In terms of salaried employment, all three countries have a rather low percentage of the population who are employed in salaried occupations, the highest being 27 percent for males in Uganda and the lowest being just under three percent for females in Uganda.

[13] In most surveys that only collect information on level of education completed such primary, secondary, and tertiary, researchers have to assume that no schooling was pursued following attainment of each level. For instance, if an individual completed primary education but dropped out after one of year of secondary education, the conversion into years of schooling would not account for that additional year post-primary education.

4.2 Demographic and Health Surveys

We draw upon individual level data from the DHS program to supplement the LSMS since the DHS collects information on adolescent sexual and family planning behavior for all adult respondents. We note that we use the DHS datasets for adolescent behavior that are relevant for females, we therefore restrict the DHS analytic samples accordingly. As such, the DHS data enable us to identify the age at which each female respondent first engages in sexual activity, gives birth, or gets married. The DHS datasets collect information on household wealth status as determined by house asset ownership. Central to this paper, the DHS datasets include a variable to indicate continuous years of schooling and exact dates of birth to identify UPE eligibility based on the reported year of birth. Lastly, the DHS program, similar to the LSMS, collects demographic information including marital status, ethnicity/religion, number of own children under 15 years, urban or rural location, and subnational region of residence.

[INSERT TABLE 3 HERE]

Table 3 summarizes the analytic samples from the DHS datasets collected in Ethiopia, Malawi, and Uganda. We note that since not all DHS administrations include both female and male respondents, we restrict the DHS analytic samples to female respondents only. The first panel of Table 3 shows that the proportion of females who engage in sexual activity as an adolescent is between 64 and 70 percent across all the countries in this study. However, women in Ethiopia exhibit a substantially higher rate of marriage/cohabitation prior to adulthood at almost 65 percent, whereas around half of the women in Malawi and Uganda get married before their eighteenth birthday. Lastly, we see that the overall schooling and demographic variables are similar to those in the LSMS except for average age, due to the earlier enumeration period of the DHS relative to LSMS in Ethiopia and Malawi.

4.3 Identification Strategy

The objective of this paper is twofold. First, we examine the effects of the implementation of universal and free primary education policies on educational attainment. Second, we examine whether any changes in attainment as a result of the policies translated into favorable adolescent sexual behavior, gains in the labor market, and improved socioeconomic standing. The analysis presented in this study follows typical instrumental variables design where the timing of the policy along with the age at school entry requirements are used to predict the endogenous variable, years of schooling. We use official primary school age as the determinant for eligibility for tuition-free primary school attendance. Compliance with the policy was not perfect, where individuals from ineligible cohorts could still enroll as over-aged students and those eligible may have not taken up the policy due to other constraints such as distance to nearest school. However, since UPE eligibility serves the purpose of instrument for schooling, the resulting Wald estimate of the impact of schooling on later outcomes accounts for imperfect compliance by differencing the schooling gained from eligible and ineligible cohorts. Thus, our empirical strategy identifies the causal effect of schooling on long-term outcomes and computes the contribution of UPE to those outcomes by via their net effect on attainment.

Consider the structural relationship between schooling and the following long-term outcomes of interest: the likelihood of engaging in sexual behavior, getting married, and childbirth as an adolescent; the likelihood of employment, salaried employment, unemployment, and being active in the labor market; and the likelihood of being in the bottom 40 percent and 20 percent of wealth as an adult. Formally, we estimate the structural relationship between schooling and outcomes as follows.

$$\Pr(Y_{ij}|S, X, \mu_r) = \alpha S_{ij} + X_{ij}\beta + \mu_{rj} + \varepsilon_{ij} \quad [1]$$

where, $\Pr(Y_{ij})$ denotes the probability that individual i, in country j, is employed, active in the labor force, engaged in sexual activity as an adolescent, age at first marriage $\leq$ 18 years, age at first birth $\leq$ 18 years, or is currently among the poorest 20 percent.[14] S_{ij} represents the years of schooling completed by individual i in each country. X is the matrix of observable factors that determine the outcome Y_{ij} which includes marital status, religion, and residence location (urban/rural). μ_{rj} denotes subnational region fixed effects to account for between-region differences in mean outcomes and schooling. Lastly, ε_{ij} is the idiosyncratic error term.

Ordinary Least Squares (OLS) estimation of equation (1) leads to biased estimates of the returns to schooling on the different outcomes because the independent variable of interest, S_{ij} (years of schooling), is not randomly assigned across individuals but could be a function of individuals' exposure to the UPE policies. As a result, OLS would yield upward biased estimates of the returns to schooling since the policies are positively correlated with higher levels of schooling, which is also positively correlated with the outcome Y. To eliminate this bias, we implement an instrumental variables regression approach that exploits the exogenous variation in the exposure to the UPE policies determined by a person's year of birth. Specifically, eligibility for UPE in all three countries is determined by the official primary school age.

As shown in Table 1, the UPE policies were put into effect in Ethiopia and Malawi in 1994, and in Uganda in 1997. This means that the youngest birth cohort eligible for free primary schooling would be 7 year olds born in 1987 in Ethiopia, 6 year olds born in 1988 in Malawi, and 6 year olds born in 1991 in Uganda. These would be considered the first cohort of children to be eligible for full exposure to free primary education, as they would have been eligible for all possible primary grades. The oldest birth cohort eligible for free primary education would be 12 year olds born in 1982 in Ethiopia, 11 year olds born in 1988 in Malawi, and 12 year olds born in 1985 in Uganda. These cohorts, however, were potentially eligible for only one year of free primary education, and can be thought of as the cohort with the least amount of exposure to the UPE policies. Based on these eligibility criteria we identify the unexposed populations as those whose birth year preceded that of the oldest exposed cohort, in each country. Specifically, in Ethiopia and Malawi, whereas

[14] Although the subscript j is represented in equation (1) to denote the different countries in this study, all regression equations will be estimated separately for each country to account for heterogeneity in the policy effect.

the unexposed population in Uganda are those born on or before 1984. Formally, we define exposure to the universal primary education policy (our set of excluded instrumental variables) as follows (Figure 2).

[INSERT FIGURE 2 HERE]

$$UPE_{ij}^{1} = \begin{cases} 1 & if\ 1982 \leq yob \leq 1986\ ; j = Ethiopia \\ 1 & if\ 1982 \leq yob \leq 1987\ ; j = Malawi\ , \quad 0\ otherwise \\ 1 & if\ 1985 \leq yob \leq 1991\ ; j = Uganda \end{cases} \quad [2]$$

$$UPE_{ij}^{2} = \begin{cases} 1 & if\ yob \geq 1987\ ; j = Ethiopia \\ 1 & if\ yob \geq 1988\ ; j = Malawi\ , \quad 0\ otherwise \\ 1 & if\ yob \geq 1992\ ; j = Uganda \end{cases} \quad [3]$$

As such, we estimate the first stage regression with years of schooling as the left-hand side variable as a function of UPE exposure, observable background characteristics, and subnational region fixed effects. The first stage regression equation is represented according to the following.

$$S_{ij} = \sum_{k}^{2} \delta^{k} UPE_{ij}^{k} + X_{ij}\gamma + \mu_{rj} + \nu_{ij} \quad [4]$$

where we use predicted values of S in the second stage regression. The second stage regression equation becomes

$$\Pr\left(Y_{ij} \middle| \hat{S}_{ij}, X, \mu_{r}\right) = \lambda \hat{S}_{ij} + X_{ij}\varphi + \mu_{rj} + \upsilon_{ij} \quad [5]$$

As a result, the parameter estimate λ represents the causal relationship between schooling and the outcome Y_{ij} identified by the shift created by the implementation of the UPE policy in each country under two main conditions to maintain causality. The first necessitates that the exposure to free primary education does in fact influence school attainment to a significant degree, ensuring that the instrumental variables are not weak.[15] The second condition required to yield causal estimates of λ, is that any exposure to the UPE policies does not alter outcomes other than through the change in educational attainment. In other words, birth cohort membership does not affect adolescent behavior or labor market participation except through the shift created in schooling. Lastly, we address the issue of imperfect compliance when UPE eligibility with the use of an instrument to identify the effects of schooling. We illustrate this point by showing the Wald estimate of λ, as follows.

$$\hat{\lambda}^{IV} = \frac{E(Y|UPE = 1) - E(Y|UPE = 0)}{E(S|UPE = 1) - E(S|UPE = 0)} \quad [6a]$$

$$\hat{\lambda}^{IV} = \frac{E(Y|UPE = 1) - E(Y|UPE = 0)}{E(S|A = 1)[E(A|UPE = 1) - E(A|UPE = 0)]} \quad [6b]$$

[15] Staiger et al. (1997) show that first stage instruments are considered weak, and thus lead to exacerbated bias in the coefficient of the endogenous variable, when the joint hypothesis that $UPE_{ij}^{1} = UPE_{ij}^{2} = 0$ yields $F < 10$.

Where A denotes free primary school attendance. We identify λ as the difference in long-term outcomes between eligible and non-eligible cohorts divided by the difference in the compliance rate between the two sets of cohorts. Therefore, λ is the estimated causal effect of schooling on outcomes.

Another concern with our identification strategy is that our estimates may be spurious or a function of regional trends in SRH, employment, and socioeconomic status outcomes. As such, we conduct a falsification test using only pre-UPE data on the original samples and randomize a false UPE implementation in the pre-UPE period to ensure that our IV estimates are not spurious. We can conclude that our estimates are non-spurious if estimates of the false policy implementation date are zero. Lastly, it is not unlikely that estimates of the impacts of the UPE policies in Ethiopia, Malawi, and Uganda could be a function of regional trends, i.e. improvements in these outcome variables that are occurring in the region for reasons other than UPE. To test for this potential source of bias, we implement a placebo test in three neighboring countries that had not yet implemented UPE during our analytic time frame. We introduce data from the DHS for Kenya, Tanzania, and Zambia and assign a false policy date between 1994 and 1997 and replicate the analysis described in equations (1) and (4). If the placebo estimates are not statistically different from zero then we can conclude that our IV impact estimates are not affected by regional trends.

5 Results

To estimate the impact of the universal primary education policies in Ethiopia, Malawi, and Uganda on the gender educational attainment gap, we stratify the analysis by estimating equation (4) for males and females in each country separately. In addition to the effect of UPE policies on attainment, we estimate the impacts on the likelihood of being literate. However, only the results from the attainment regressions will be used as the first stage of the overarching structural relationship between schooling and adolescent behavior, labor market, and socioeconomic outcomes of women. Lastly, since we examine the impacts of UPE policies on labor market outcomes that are available from the LSMS datasets and adolescent behavior and socioeconomic status that are available from DHS datasets, we assess the policy effects on schooling from both sources.

5.1 First Stage Results

Figure 3 plots the unconditional mean years of schooling completed in Ethiopia, Malawi, and Uganda by year of birth cohort and stratifies cohorts by gender and UPE exposure level. It is clear that the UPE policy going into effect had shifted attainment in a positive direction in all three countries by the time all incoming cohorts of children were fully eligible to attend primary school for free. Interestingly, the increase in attainment was somewhat gradual as cohorts directly before and after the first eligible cohort appear to be similar but increase over time. The later cohorts, on the other hand, who were eligible from the start of their schooling exhibit a discrete jump in attainment relative to the last ineligible cohort and the cohort prior to full eligibility. These findings also hold true for both females and males. The second panel of Figure 3 plots the same information

for Malawi, where we find similar trends among the men in our sample, whereas women display a distinct increase in attainment following each of the different eligible cohort cutoffs. Similarly, we again observe a similar trend among Ugandan males, where cohorts who became eligible for free primary schooling did not exhibit a higher level of attainment than ineligible cohorts. However, among women in Uganda, we observe a clear jump in attainment following the implementation of UPE.

[INSERT FIGURE 3 HERE]

Overall, the visual evidence for the UPE on mean years of schooling appears to be much clearer for females than males. To better gauge the impact of implementing free primary education in the study countries, we show the regression results from estimating equation (4) on males and females, using LSMS and DHS household surveys, separately and for each country.

Table 4 shows the results for Ethiopia where males who were exposed to the UPE policy in Ethiopia by age 12 completed an additional 0.63 years of schooling over those who were not exposed to the policy, and those exposed by age 7 completed another 0.63 years over those partially exposed. In other words, the UPE policy in Ethiopia was successful in increasing attainment by 0.63 years of schooling for males partially exposed to the policy and by 1.26 years for those exposed to a full primary education cycle. This results in an F-statistic of 12.6 for the joint significance of the instruments UPE^1 and UPE^2, for the male sample. For females, the UPE policy increases attainment by an average of about 0.26 years for those exposed by age 12 and by about 1.8 years for those exposed from the beginning of primary school eligibility and an F statistic of 68. In other words, the UPE policy in Ethiopia was successful in increasing mean attainment by about 25 percent for males who were exposed to a full primary education cycle and by about 61 percent for their female counterparts. The results are mimicked using the DHS dataset where females exposed by age 12 exhibit a 0.34-year increase in attainment and those exposed by age 7 exhibit a 1.75-year increase in attainment--the joint significance of the instruments yields an F-statistic of 112, in this case.

[INSERT TABLE 4 HERE]

The same analysis in Malawi confirms the finding that males did not make substantial gains in attainment with the onset of the UPE policies where we estimate that UPE at most increased attainment by 0.2 years (Table 5). Additionally, the joint significance of the UPE effect estimates yield an F-statistic of 0.6, which is substantially below the threshold to reject the weak instruments hypothesis. For females, using the LSMS dataset, we estimate that UPE increased attainment for the first group of birth cohorts--those who were eligible for free primary education but not from the start--by about 0.49 years, and by 1.28 years for those who were exposed to a free education for a full primary education cycle. When using the DHS dataset, we find that females from the partial exposure group experience a 1.25-year increase in attainment relative to their control group counterparts, and the full exposure group exhibits a 1.53-year increase in mean years of schooling completed as a result of the UPE policy. Although the point estimates are not equal across data

sources, the results still confirm that females under different levels of exposure to the policy experience substantial increases in attainment relative to cohorts who were not eligible for free primary schooling. In the LSMS sample, the first stage F-statistic is 23.7 for the joint significance of the instrumental variables, and 190.8 in the DHS sample.

[INSERT TABLE 5 HERE]

Lastly, Table 6 presents the first stage regression results for the Uganda sample. Among males, we estimate that mean years of schooling only marginally increases for the first group of eligible cohorts (partial exposure by age 13 years) by 0.28 years. However, our first stage regressions also show that the first cohort to be exposed to the policy for a full primary education cycle experience a sharp decline in mean years of schooling completed by about 1.46 years, relative to pre-UPE cohorts. The effect of the UPE policy in Uganda on female schooling are generally positive for all post-UPE cohorts relative to the pre-UPE cohorts. Moreover, we estimate that mean years of schooling increases by 2.12 years for the cohorts experiencing partial exposure to the policy, and by 1.38 years for the cohorts under full exposure. It is interesting to note that we also observe a sharp decline in schooling for the full exposure group relative to the partial exposure group, similar to their male counterparts. However, when evaluating UPE among females using the DHS dataset, we find that mean years of schooling increases for the partial exposure group by 1.39 years and for the full exposure group by 1.25 years.

[INSERT TABLE 6 HERE]

5.2 Second Stage Results

Using the first stage results, we estimate the effects of schooling, through the implementation of UPE, on the probability of sexual activity as a teenager, cohabitation/marriage, and childbirth using a two-stage least squares (2SLS) estimator. We argue that the set of excluded instruments, UPE exposure, do not influence the outcomes except through the endogenous variable, schooling. We see across the three countries included in the analysis that the first stage results are strong for the female subsamples. However, for the male subsamples, the excluded instruments are somewhat weak with an F-statistic of at most 12.6. It is also important to note that the adolescent sexual behavior outcomes are available only for the female respondents. As such, we are not able to assess the effects of schooling on sexual behavior for males.

5.2.1 Sexual and Reproductive Health

Table 7 shows the second stage results on SRH outcomes for the Ethiopia sample. We find that an additional year of schooling completed lowers the likelihood of adolescent sexual activity by 4.2 percentage points. Relative to 69.6 percent of all females in the sample having engaged in sexual activity as a teenager, the probability of engaging in sexual activity as an adolescent is 10.5 percent lower through UPE increasing the mean years of schooling by 1.75 years. Similarly, we find that an additional year of schooling lowers the probability of cohabitating or getting married before the age of 18 years by 7.5 percentage points. Given that almost 65 percent of the sample have cohabitated or married before the age of 18, this result translates to lowering the rate at which girls

cohabitate or get married as teenagers by 20 percent through the increase in schooling induced by the UPE policy. For adolescent childbearing, we find that an additional year of schooling completed lowers the likelihood by 10.2 percentage points. This means that through UPE, the likelihood of childbearing as an adolescent is lower by 43 percent.

[INSERT TABLE 7 HERE]

The same analysis in Malawi yields similar results to those in Ethiopia where schooling, through the implementation of UPE, significantly lowers all SRH outcomes measured in this paper. Moreover, females in Malawi are less likely to engage in sexual activity by 5.7 percentage points with each additional year of schooling completed (Table 8). With the UPE policy implementation in Malawi increasing attainment by an average of 1.53 years, this finding translates to an 8.7 percentage points decrease in the probability of engaging in sexual activity by age 18 years. Relative to the percentage of females (64 percent) who engage in sexual activity as teenagers, this is equivalent to a 13.6 percent decrease in the probability of engaging in sexual activity. We also estimate that an additional year of schooling completed lowers the likelihood of females getting married before the age of 18 years by 2.9 percentage points, which is equivalent to an 8.1 percent decrease in the same probability. Lastly, the 2SLS results provide evidence that the UPE policy in Malawi is also successful in lowering childbirth among adolescents by about 3.3 percentage points, which is equivalent to almost a 13 percent decrease in the teenage fertility rate, relative to a 39 percent baseline rate through the implementation of the UPE policy in Malawi.

[INSERT TABLE 8 HERE]

Lastly, using the schooling estimates from the first stage regressions on the Uganda sample, we estimate that an additional year of schooling significantly lowers the probability of engaging unfavorable SRH outcomes (Table 9). Specifically, we estimate that an additional year of schooling lowers the probability of engaging in sexual activity before age 18 years by 6.1 percentage points. In the context of the UPE policy, this estimate translates to lowering the likelihood of engaging in sexual activity as an adolescent by about 12 percent as a result of the UPE policy increasing attainment by 1.25 years and a baseline adolescent sexual activity rate of 64.4 percent. Similarly, we estimate that an additional year of schooling results in a lower probability of early marriage and/or cohabitation by 6.4 percentage points. This estimate is equivalent to a 15.6 percent decrease in the probability of having been married or in cohabitation by age 18 years through the increased attainment created by the UPE policy. The 2SLS results also show that the probability of teenage childbirth is lowered by 5.8 percentage points for each additional year of schooling completed. Via the UPE policy, this finding is equivalent to a 35.7 percent decline in the likelihood of giving birth as an adolescent.

[INSERT TABLE 9 HERE]

5.2.2 Labor Market

Table 10 displays the 2SLS results in Ethiopia from estimating the structural relationship between schooling and the likelihood of being active in the labor force (employed or seeking employment), employed, employed in a salaried position, being in the bottom 40 percent of wealth (poor), and being in the bottom 20 percent of wealth (poorest). We note that the second stage results only show the estimates for the female sample since the first stage results for the male sample resulted in weak instruments. The point estimates in this case are not precise enough to reject the null hypothesis at the 95 percent level, but are economically significant with non-negligible effect sizes. We estimate that an additional year of schooling increases the probability of being active in the labor force by 2.3 percentage points. Relative to a labor force activity rate of 64.6 percent for females, this estimate translates to a 6.2 percent increase in labor force activity from the increased attainment generated through the UPE policy.

When examining the returns to schooling in terms of overall employment, we estimate that an additional year of schooling leads to an increase in the female employment rate in Ethiopia by 4.6 percentage points, which is equivalent to a 16 percent increase in the likelihood of being employed.[16] Further, we find that the likelihood of being employed in a salaried position increases modestly by 0.7 percentage points, which translates to a 10 percent increase in the salaried employment rate relative to the mean of 7 percent. Although the point estimate is small, it is important to note that the salaried employment rate is similarly small. Lastly, we examine the relationship between schooling and socioeconomic status. We find that an additional year of schooling lowers the likelihood of being among the poorest 40 percent and the poorest 20 percent by 0.9 and 1.8 percentage points, respectively. That is, through UPE, females are now 4 percent and 15.8 percent less likely to be among the poorest 40 percent and the poorest 20 percent, respectively.

[INSERT TABLE 10 HERE]

Table 11 presents the results from the same analysis in Malawi. We do not find a statistically significant relationship between labor force activity and schooling. However, we find substantial effects on employment, we find that an additional year of schooling leads to a 3.2 percentage point increase in the probability of employment. The effect translates to an increase of 6 percent relative to a baseline employment rate of 68 percent and a 1.3-year increase in schooling completed through the UPE policy. However, the point estimate is not statistically significant. When disaggregating employment by type (salaried or not), we find that an additional year of schooling increases the likelihood of salaried employment by 8 percentage points, which translates to an increase in the probability of salaried employment because of UPE by 57 percent. We also find that an additional year of schooling yields a 2.5 percent decrease in the probability of being among the poorest 40 percent. Similarly, we estimate that women who complete an additional year of schooling are 1.8

[16] All employment and unemployment rates presented in this analysis refer to the employment- and unemployment-to-population rates.

percent less likely to be among the poorest 20 percent. However, the coefficient associated with women being in the lowest poverty status is not statistically significant.

[INSERT TABLE 11 HERE]

Table 12, below, presents the 2SLS results of the effects of schooling on labor market outcomes and socioeconomic status for women in Uganda. Unlike the results for SRH outcomes, we do not find statistically significant effects of schooling on labor force activity, employment, salaried employment, or poverty status. The estimated returns to education in terms of employment and labor market activity, as channeled through increased attainment created by the UPE policy, are insignificant statistically and in magnitude, where we estimate a 0.6 percentage point decrease in labor force activity and a 1 percentage point increase in employment with each additional year of schooling.

Although the point estimates are not statistically significant, we argue that the estimated effects on salaried employment are non-negligible in terms of relative magnitude. Specifically, we estimate that an additional year of schooling induced by the UPE policy leads to a 2.4 percentage point increase in the probability of being employed in a salaried position, and is only marginally insignificant at the 13 percent level. With UPE increasing mean years of schooling by 1.38 years and a salaried employment-to-population ratio of 10.2 percent, this estimate translates to a 32.5 percent increase in the likelihood of having a salaried position. The implications of this finding coupled with the relatively small effect on overall employment means that the UPE policy may have shifted the composition of the employed labor force toward salaried occupations rather than simply increasing employment in general.

Lastly, we estimate the effect of schooling on the likelihood of being among the poorest two quintiles and the poorest quintile, respectively. Similar to the estimated effects on labor force participation and employment outcomes, we find fairly small and statistically insignificant results. We estimate that an additional year of schooling would decrease the probability of being in the bottom 40 percent of household wealth by 1.3 percentage points, and in the bottom 20 percent by 1.2 percentage points. Relative to a 1.38-year increase in mean years of schooling completed due to the UPE policy, this translates to about a 4.5 percent decrease in the likelihood of being among the poorest 40 percent, and about an 8.3 percent decrease in the likelihood of being among the poorest 20 percent.

[INSERT TABLE 12]

5.3 Robustness Checks

In this section, we complete two tests to show that the UPE impact estimates are neither spurious nor subject to regional trends in East Africa. The first is a falsification test that estimates the effects of a false policy date such that a false implementation date is assigned using only pre-UPE data from Ethiopia, Malawi, and Uganda. The purpose of this test is to show that the estimated effects of UPE implementation are not a function of pre-existing trends in the outcome variables prior to

implementation of the policy in each country. As such, we test the null hypothesis that estimates are not spurious or a function of pre-UPE trends, i.e. the false policy effect is zero. The second is a placebo test conducted using data from neighboring countries (Kenya, Tanzania, and Zambia), where the false policy implementation date is assigned around the same time UPE was implemented in Ethiopia, Malawi, and Uganda.[17] The null hypothesis is such that we are testing whether the placebo effect is zero. If so, then we argue that the UPE estimates and consequently the second stage estimates of the effect of schooling in the original study countries are not a function of regional trends in sexual and reproductive health or labor market outcomes.

5.3.1 Falsification Test

Results of the falsification experiments are presented in Table 13 to provide further support on the robustness of the estimated UPE impacts. The falsification samples from Ethiopia is restricted to birth cohorts born on or prior to 1981, 1980 for Malawi, and 1984 for Uganda. We then assign a random start date for the false policy, under the condition that there are enough birth cohorts in the post-false policy period to include individuals who would have been eligible from the start of primary school and from the last year of primary school. This is to mimic the UPE implementation structure on a hypothetical timeline. For instance, if the false policy were to start in 1987 in Ethiopia, then eligible cohorts would be those born between 1975 and 1981. As such, since UPE was not implemented during those time periods, we would expect to find zero schooling effects of the false policy.

[INSERT TABLE 13]

Table 13, shows that all false policy effects are statistically insignificant with magnitudes ranging from -0.7 years to 0.48 years of schooling, although most estimates are smaller and range between -0.15 and 0.16 years. The falsification tests also show that the false policy had a net zero effect on schooling in Malawi and Uganda and a negative effect in Ethiopia, which leads us to conclude that the estimated effects of UPE in the original samples are not spurious or a function of pre-policy trends in each of the study countries.

5.3.2 Test for Regional Effects

The final robustness check tests whether UPE estimates in Ethiopia, Malawi, and Uganda are affected by regional trends in sexual and reproductive health outcomes among adolescent and young adult females. An argument that would introduce bias to our UPE estimates would be that SRH outcomes have been improving due to regional level factors other than UPE, e.g. programs and intervention explicitly targeting SRH outcomes among females. To test whether the UPE estimates are biased due to the regional effect on SRH outcomes, we replicate the original analysis in three neighboring countries, Kenya, Tanzania, and Zambia that had not implemented UPE over the same analytic time-period. The false policy is therefore assigned around the same time as actual

[17] Although UPE was eventually implemented in Kenya, Tanzania, and Zambia, the false treatment is assigned prior to actual implementation. Further, the false policy date assigned to each of these countries was randomized between 1994 and 1997.

UPE implementation in Ethiopia, Malawi, and Uganda. In addition to testing the effect of the placebo on schooling, we also run the reduced form relationship between the placebo policy and the adolescent sexual and marital behavior outcomes in each of the three non-UPE countries.

[INSERT TABLE 14]

Table 14 displays the regression coefficient estimates for the placebo policy. Of the 36 regression coefficients, only six estimates are statistically significant, five of which are in the opposite direction of the expected effect had there been a regional effect on SRH outcomes at the time of actual UPE implementation in the study countries. The only placebo estimate that showed a statistically significant and beneficial effect was on the probability of teen births in Kenya among one of the two false policy groups, those who would be eligible at six years old. However, this finding may not be enough to prove that teen births may have been trending favorably in the region since the false policy effect is found to be negligible and statistical insignificant among the second false policy group in Kenya and all false treatment groups in Tanzania and Zambia. We conclude that regional effects have had little influence on UPE estimates in the original study countries.

6 Policy Implications

6.1 Early Marriage and Adolescent Sexual and Reproductive Health

This paper shows that the UPE policies implemented in the study countries are an effective tool in increasing schooling attainment. We are thus able to estimate the causal relationship between schooling and lowering sexual activity rates, marriage rates, and fertility rates among adolescents. Consequently, this enables us to compute the indirect effect of UPE via the increase in schooling. Our estimated effects are also consistent with those from evaluations of various interventions implemented in Sub-Saharan Africa. Erulkar and Muthengi (2009) evaluate the Berhane Hewan program, a school-level intervention aimed at reducing the prevalence of child marriage in rural Ethiopia through combination of school support for girls to remain in school and community awareness. The program was implemented among 1,386 girls between the ages of 10 and 19 years and showed that the program lowered the early marriage rate by 10 percent--more so for 10-14 year olds than for 15-19 year olds. Whereas our paper shows similar effect sizes for Ethiopia where UPE lowered early marriage rates by about 11.6 percent per year of additional schooling completed. However, the differences between this study and the former is that this study is evaluated at a national scale and the mechanism by which outcomes are influenced is through reduction in the cost of school attendance rather than an after-school intervention.

Baird et al. (2010) evaluate the Zomba conditional cash transfer program that was aimed at subsidizing schooling for girls in the Zomba district of Malawi and show that the program was successful in lowering early marriage, teen pregnancy, and sexual activity among 13-22 year old women. The study estimates that subsidizing young women's education led to a 40 percent decline in early marriage rates, 12 percent decline in teen pregnancy, and an 11 percent decline in sexual activity. The Baird et al. (2010) study is similar to this paper in that the policy mechanism relaxes

the household education budget constraint and keeps young women in school for a longer duration otherwise. However, our study evaluates the UPE policy at a national scale, whereas the Zomba conditional cash transfer program was implemented only in one district in Malawi. As such, we find that our estimates are similar or at least within the bounds of the Baird et al. (2010) study where the UPE policy lowers early marriage rates by about 8 percent, lowers sexual activity by 13.5 percent, and lowers teen pregnancy by 13 percent.

Lastly, Bandiera et al. (2010) evaluate the effects of BRAC's adolescent development program (ADP), which provides 14-20 year old women with life skill and microfinance training. The program was implemented in a sample of villages across Uganda including an analytic sample of 5,133 adolescent girls. The study finds that the program had no effect on sexual activity or marriage rates among the study sample, but was successful in lowering childbirth by about 28 percent. In contrast, the UPE policy in Uganda lowered teen sexual activity by about 11.3 percent, early marriage/cohabitation by 15 percent, and adolescent childbirth by 34 percent. It is important to note that the mechanisms behind both evaluations are different, thus making the comparison less than ideal. However, both evaluations provide grounds for discussion on the possibility of different mechanisms that can induce more favorable adolescent outcomes.

The findings from the three countries included in this paper, in addition to evaluations of different programs and interventions implemented in the same countries, contribute to the discussion that subsidizing young women's education is an effective policy tool to influence non-educational outcomes through the education vehicle. Additionally, the findings highlight that even though early marriage, pregnancy, and sexual activity may not be the explicit aim of the education subsidies, its effects are comparable to programming whose main objective is. However, an important question remains.

6.2 Labor Market

Pritchett (2013) argues that increased access to education and increased schooling, in general, is not necessarily the same as increased learning or skill acquisition. Our paper, to some degree, provides some support to that notion. We find that the UPE policy implemented in each of Ethiopia, Malawi, and Uganda increased schooling by 1.2 to 1.7 years among women. However, we find somewhat small and imprecise long-run effects on labor force participation and employment. This could be the result of increasing access to schooling while the overall quality and content may not have changed between pre- and post-UPE implementation periods. Alternatively, it is possible that delivery of educational services and instruction have improved but were ultimately offset by the drastic increase in pupil-teacher ratios as experienced in the study countries, except for Malawi.

To that end, although the UPE policy in Malawi did not affect overall employment, the composition of the employed labor force has shifted toward salaried occupations as a result. Malawi was the only country, of the three studied in this paper, to foresee a dramatic increase in enrollment and preemptively prepare the educational infrastructure by hiring an additional 20,000

teachers to absorb the increase in demand. Additionally, Malawi implemented what became a two-year piloting stage with its phased roll-out of the free primary education policy. Although this cannot be viewed as evidence of Malawi's superior effectiveness in implementing the UPE policy, it does provide an interesting policy implication that could apply to not only reducing tuition in primary education, but also secondary. The evidence provided to support the notion that Malawi was better prepared for the UPE policy is anecdotal at best. However, it raises the question of whether anticipating such a demand shock and investing in the educational infrastructure yield better results?

Finally, we cannot rule out other mechanisms that could explain our findings. It is possible that an increase in schooling in this context does not necessarily equate to an increase in human capital formation that would make individuals more employable, or at least more active in the labor force. However, it is also likely that the economy may not be well equipped to absorb the increase in educated labor supply. Alternatively, the increase in human capital formation induced by increased access to primary education may not be enough to induce increased production in the labor market. Regardless, further research into the mechanisms that link education to labor market production in Sub-Saharan African countries would shed light into the inner workings of the causal links between education and growth in economic opportunity, especially since education is normally viewed as a conduit of economic growth (Lucas,1988; Mankiw and Weil, 1992; Hanushek and Kimko, 2000; Sala-i-Martin et al.,2004). Aghion et al. (2009) also argue that the use of years of schooling as a proxy does not accurately capture human capital accumulation. As such, we cannot rule out this explanation when interpreting our 2SLS results.

7 Conclusion

The results of this paper provide evidence that the UPE policies implemented in Ethiopia, Malawi, and Uganda increased educational attainment. We also show that the UPE policies were successful in bridging the gender attainment gap as years of schooling completed for males and females have somewhat converged following the implementation of free primary education in the three countries. This paper also demonstrates that the increase in schooling translated into mitigating unfavorable adolescent outcomes including engaging in sexual activity, entering marriage or cohabitation, and child bearing prior to reaching adulthood (18 years of age). However, we are not able to completely support the notion that the increase in schooling had a significant impact on human capital accumulation by way of yielding increased labor force activity, employment, or higher socioeconomic status among women affected by the implementation of a free primary education policy in each of the three study countries. Finally, Ethiopia, Malawi, and Uganda represent three countries from East Africa with a similar economic and political climate to other Sub-Saharan African countries that are considered least developed (LDCs). While our findings may not be applicable to LDCs in elsewhere around the globe, they insight into the implications of such policies in in Sub-Saharan African education systems with similar economic and demographic characteristics.

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Tables and Figures

Table 1. Universal Primary Education policy implementation timeline and eligibility

Country	Policy Year	Eligible Age	First Eligible Cohorts	LSMS Year	DHS Year
Ethiopia	1994	7 - 12 years	1982 - 1987	2013	2011
Malawi	1994	6 - 13 years	1981 - 1988	2013	2010
Uganda	1997	6 - 12 years	1985 - 1991	2011	2011

Table 2. LSMS Sample Summary, by Gender and Country

	Ethiopia		Malawi		Uganda	
	Female	Male	Female	Male	Female	Male
Labor Force Outcomes:						
Active in Labor Force	0.646	0.783	0.801	0.857	0.908	0.974
Employed	0.504	0.683	0.620	0.681	0.826	0.866
Employed - Salaried	0.070	0.156	0.065	0.180	0.102	0.271
Unemployed	0.117	0.074	0.157	0.124	0.027	0.020
Education:						
Years of Schooling	2.89	4.91	8.04	9.30	6.13	7.59
Literate	0.396	0.711	0.839	0.922	0.766	0.884
Demographics:						
Age in years	29.41	29.29	29.57	29.19	29.45	29.17
Married	0.751	0.610	0.700	0.608	0.653	0.590
Number of Children	2.84	2.46	3.42	3.01	4.42	3.89
Rural	0.830	0.827	0.821	0.821	0.797	0.801
Urban	0.170	0.173	0.179	0.179	0.203	0.199
Observations	4,266	3,713	3,238	3,127	2,034	1,775

Table 3. DHS Sample Summary, by Country

	Ethiopia	Malawi	Uganda
Adolescent Behavior:			
Age at first sex < 18	0.696	0.644	0.644
Age at first cohabitation < 18	0.646	0.551	0.511
Age at first birth < 18	0.413	0.386	0.203
Poverty Status:			
Bottom 40% - Wealth	0.355	0.374	0.358
Bottom 20% - Wealth	0.175	0.183	0.177
Education:			
Years of Schooling	3.07	5.66	6.40
Demographics:			
Age in years	26.61	26.87	28.79
Married	0.636	0.67	0.769
Number of Children	2.49	2.90	3.98
Rural	0.747	0.805	0.782
Urban	0.253	0.195	0.218
Observations	11,689	15,858	7,674

Table 4. First Stage Results on Years of Schooling – Ethiopia

	Male	Female	
	LSMS	DHS	
Universal Primary Education:			
Eligible for UPE by age 7	0.625**	1.570***	1.412***
	(0.268)	(0.189)	(0.129)
Eligible for UPE by age 12	0.627***	0.257	0.339***
	(0.236)	(0.161)	(0.117)
Control Variables:			
Married	-1.217***	-1.531***	-1.465***
	(0.238)	(0.189)	(0.124)
Orthodox	0.643	0.748**	0.884***
	(0.526)	(0.313)	(0.259)
Protestant	1.827***	1.413***	0.916***
	(0.514)	(0.303)	(0.236)
Muslim	0.529	0.124	0.218
	(0.533)	(0.308)	(0.245)
Rural	-5.358***	-5.159***	4.109***
	(0.236)	(0.233)	(0.189)
Constant	8.708***	6.787***	-1.395***
	(0.644)	(0.432)	(0.472)
IV - F statistic	12.6	68.0	112.1
Observations	3,357	3,951	11,688

Notes: All regression specifications include subnational regional fixed effects. Other control variables include quadratic age, number of own children under 15 years, ethnicity, and location (urban or rural). Parentheses denote standard errors clustered at the cohort level. Asterisks denote statistical significance as follows.
*** p < .01, ** p < .05, * p < .10

Table 5. First Stage Results on Years of Schooling – Malawi

	Male	Female	
	LSMS	DHS	
Universal Primary Education:			
Eligible for UPE by age 6	-0.196	0.795***	0.283***
	(0.221)	(0.189)	(0.091)
Eligible for UPE by age 11	0.215	0.485**	1.253***
	(0.208)	(0.196)	(0.087)
Control Variables:			
Married	-1.088***	-1.096***	-0.645***
	(0.203)	(0.190)	(0.079)
Catholic	1.542***	1.416***	2.260***
	(0.392)	(0.482)	(0.309)
Muslim	0.778*	0.368	1.037***
	(0.461)	(0.529)	(0.329)
Rural	-0.701**	-1.249***	3.075***
	(0.282)	(0.301)	(0.112)
Constant	9.432***	9.129***	4.431***
	(0.631)	(0.647)	(0.327)
IV - F statistic	0.6	23.7	190.8
Observations	2,902	2,813	15,846

Notes: All regression specifications include subnational regional fixed effects. Other control variables include quadratic age, number of own children under 15 years, and ethnicity. Parentheses denote standard errors clustered at the cohort level. Asterisks denote statistical significance as follows. *** p < .01, ** p < .05, * p < .10

Table 6. First Stage Results on Years of Schooling – Uganda

	Male	Female	
	LSMS	DHS	
Universal Primary Education:			
Eligible for UPE by age 6	-1.734***	-0.741	-0.145
	(0.615)	(0.466)	(0.303)
Eligible for UPE by age 11	0.275	2.120***	1.393***
	(0.331)	(0.369)	(0.244)
Control Variables:			
Married	-0.379	-0.579*	-0.515*
	(0.355)	(0.331)	(0.285)
Baganda	0.604	0.510	1.490**
	(0.583)	(0.536)	(0.619)
Basoga	0.423	1.190	2.147**
	(0.813)	(0.801)	(0.857)
Iteso	-0.192	-0.243	1.064
	(0.680)	(0.833)	(0.663)
Langi	1.064*	0.737	0.664
	(0.605)	(0.640)	(0.608)
Lugbara	0.104	0.454	1.560***
	(0.827)	(0.974)	(0.466)
Rural	-2.209***	-2.500***	3.465***
	(0.402)	(0.402)	(0.422)
Constant	10.770***	9.411***	4.449***
	(0.814)	(1.018)	(0.845)
IV - F statistic	4.0	19.0	17.3
Observations	1,465	1,853	7,674

Notes: All regression specifications include subnational regional fixed effects. Other control variables include quadratic age, number of own children under 15 years, and ethnicity. Parentheses denote standard errors clustered at the cohort level. Asterisks denote statistical significance as follows. *** p < .01, ** p < .05, * p < .10

Table 7. Second Stage Results on Sexual and Reproductive Health Outcomes – Ethiopia

	Teen sex	Teen cohab.	Teen birth
Effect of Schooling:			
Schooling (IV)	-0.042**	-0.075***	-0.102***
	(0.019)	(0.024)	(0.026)
Control Variables:			
Married	-0.290***	-0.212***	-0.235***
	(0.032)	(0.039)	(0.042)
Orthodox	0.095**	0.084*	0.104**
	(0.044)	(0.050)	(0.050)
Protestant	0.078*	0.028	0.103**
	(0.043)	(0.048)	(0.048)
Muslim	0.074*	0.021	-0.003
	(0.041)	(0.045)	(0.044)
Rural	0.161**	0.304***	0.506***
	(0.081)	(0.100)	(0.106)
Constant	2.708***	2.675***	3.150***
	(0.181)	(0.256)	(0.315)
Observations	11,688	9,120	8,175

Notes: All regression specifications include subnational regional fixed effects. Other control variables include quadratic age, number of own children under 15 years, and ethnicity. Parentheses denote standard errors clustered at the cohort level. Asterisks denote statistical significance as follows. *** p < .01, ** p < .05, * p < .10

Table 8. Second Stage Results on Sexual and Reproductive Health Outcomes – Malawi

	Teen sex	Teen cohab.	Teen birth
Effect of Schooling:			
Schooling (IV)	-0.057***	-0.029*	-0.033**
	(0.015)	(0.015)	(0.015)
Control Variables:			
Married	-0.106***	-0.070***	-0.072***
	(0.014)	(0.015)	(0.015)
Catholic	0.049	-0.035	0.034
	(0.065)	(0.062)	(0.060)
Muslim	0.020	-0.031	0.014
	(0.060)	(0.056)	(0.053)
Rural	0.164***	0.034	0.122**
	(0.049)	(0.049)	(0.048)
Constant	3.269***	2.690***	2.288***
	(0.178)	(0.200)	(0.211)
Observations	15,846	14,276	14,029

Notes: All regression specifications include subnational regional fixed effects. Other control variables include quadratic age, number of own children under 15 years, and ethnicity. Parentheses denote standard errors clustered at the cohort level. Asterisks denote statistical significance as follows. *** p < .01, ** p < .05, * p < .10

Table 9. Second Stage Results on Sexual and Reproductive Health Outcomes – Uganda

	Teen sex	Teen cohab.	Teen birth
Effect of Schooling:			
Schooling (IV)	-0.061***	-0.064***	-0.058***
	(0.017)	(0.018)	(0.018)
Control Variables:			
Married	-0.052***	-0.108***	-0.027
	(0.017)	(0.020)	(0.017)
Baganda	0.085**	0.064*	0.079**
	(0.035)	(0.038)	(0.035)
Basoga	0.043	0.043	0.040
	(0.030)	(0.033)	(0.027)
Iteso	0.220***	0.277***	0.158***
	(0.050)	(0.053)	(0.049)
Langi	0.133***	0.219***	0.139***
	(0.049)	(0.049)	(0.045)
Lugbara	-0.014	0.019	0.033
	(0.039)	(0.044)	(0.037)
Rural	0.151**	0.198***	0.229***
	(0.066)	(0.072)	(0.069)
Constant	1.929***	2.080***	1.092***
	(0.265)	(0.298)	(0.288)
Observations	7,485	6,743	6,006

Notes: All regression specifications include subnational regional fixed effects. Other control variables include quadratic age, number of own children under 15 years, and ethnicity. Parentheses denote standard errors clustered at the cohort level. Asterisks denote statistical significance as follows. *** p < .01, ** p < .05, * p < .10

Table 10. Second Stage Results on Labor Market Outcomes – Ethiopia

	Active	Employed	Salaried	Poor	Poorest
Effect of Schooling:					
Schooling (IV)	0.023	0.046	0.007	-0.009	-0.018
	(0.030)	(0.035)	(0.016)	(0.019)	(0.015)
Control Variables:					
Married	-0.036	0.030	-0.032	0.027	-0.003
	(0.050)	(0.057)	(0.028)	(0.032)	(0.025)
Orthodox	0.041	0.054	0.100***	-0.073*	-0.089**
	(0.046)	(0.050)	(0.035)	(0.044)	(0.038)
Protestant	0.102	0.084	0.105**	-0.059	-0.076**
	(0.067)	(0.074)	(0.044)	(0.043)	(0.037)
Muslim	-0.143**	-0.116*	-0.034	-0.061	-0.095***
	(0.069)	(0.064)	(0.039)	(0.041)	(0.036)
Rural	0.215	0.308*	-0.129	-0.396***	-0.134**
	(0.151)	(0.174)	(0.081)	(0.080)	(0.063)
Constant	0.072	-1.181*	-0.118	0.941***	0.798***
	(0.552)	(0.637)	(0.310)	(0.200)	(0.163)
Observations	3,951	3,951	3,934	11,688	11,688

Notes: All regression specifications include subnational regional fixed effects. Other control variables include quadratic age, number of own children under 15 years, and ethnicity. Parentheses denote standard errors clustered at the cohort level. Asterisks denote statistical significance as follows. *** p < .01, ** p < .05, * p < .10

Table 11. Second Stage Results on Labor Market Outcomes – Malawi

	Active	Employed	Salaried	Poor	Poorest
Effect of Schooling:					
Schooling (IV)	-0.019	0.032	0.080***	-0.025*	-0.018
	(0.053)	(0.061)	(0.031)	(0.014)	(0.012)
Control Variables:					
Married	-0.068	0.065	0.013	-0.076***	-0.087***
	(0.061)	(0.072)	(0.035)	(0.013)	(0.011)
Catholic	0.022	-0.129	-0.073	-0.106*	-0.110**
	(0.104)	(0.120)	(0.048)	(0.057)	(0.056)
Muslim	-0.085	-0.175*	-0.028	-0.082	-0.119**
	(0.080)	(0.091)	(0.029)	(0.051)	(0.051)
Rural	-0.051	0.053	0.101**	-0.309***	-0.135***
	(0.076)	(0.089)	(0.045)	(0.044)	(0.037)
Constant	1.163	-0.758	-1.546***	0.818***	0.495***
	(0.801)	(0.947)	(0.483)	(0.173)	(0.148)
Observations	3,951	3,951	3,934	11,688	11,688

Notes: All regression specifications include subnational regional fixed effects. Other control variables include quadratic age, number of own children under 15 years, and ethnicity. Parentheses denote standard errors clustered at the cohort level. Asterisks denote statistical significance as follows. *** p < .01, ** p < .05, * p < .10

Table 12. Second Stage Results on Labor Market Outcomes – Uganda

	Active	Employed	Salaried	Poor	Poorest
Effect of Schooling:					
Schooling (IV)	-0.006	0.010	0.024	-0.013	-0.012
	(0.026)	(0.028)	(0.016)	(0.015)	(0.012)
Control Variables:					
Married	0.022	0.048	-0.105***	-0.012	-0.022*
	(0.036)	(0.038)	(0.033)	(0.014)	(0.011)
Baganda	0.006	0.000	-0.006	-0.083***	-0.025
	(0.042)	(0.044)	(0.041)	(0.029)	(0.022)
Basoga	0.211*	0.187	0.080*	-0.135***	-0.078***
	(0.121)	(0.120)	(0.048)	(0.024)	(0.019)
Iteso	0.119	0.151*	0.023	-0.068	-0.074*
	(0.073)	(0.078)	(0.028)	(0.046)	(0.041)
Langi	-0.035	-0.024	-0.095	-0.017	-0.119***
	(0.035)	(0.041)	(0.058)	(0.044)	(0.042)
Lugbara	-0.029	0.008	-0.001	-0.002	-0.037
	(0.056)	(0.060)	(0.076)	(0.038)	(0.037)
Rural	0.103	0.153*	-0.001	-0.294***	-0.104**
	(0.075)	(0.081)	(0.059)	(0.058)	(0.047)
Constant	0.909	0.501	0.372	0.641***	0.580***
	(0.600)	(0.670)	(0.488)	(0.239)	(0.194)
Observations	1,853	1,853	1,853	7,485	7,485

Notes: All regression specifications include subnational regional fixed effects. Other control variables include quadratic age, number of own children under 15 years, and ethnicity. Parentheses denote standard errors clustered at the cohort level. Asterisks denote statistical significance as follows. *** p < .01, ** p < .05, * p < .10

Table 13. Falsification Test Results, by Country

	Ethiopia	Malawi	Uganda
Placebo - Early Start	-0.576	0.158	0.481
	(0.408)	(0.409)	(0.936)
Placebo - Late Start	-0.072	-0.148	-0.720
	(0.332)	(0.356)	(0.738)
Observations	4,000	5,468	4,134
IV - F statistic	1.1	0.1	0.8

Notes: Estimated parameters in this table refer to the estimation of the first stage regression as described in equation (4). Reported F-statistics refer to the joint significance of the placebo assignments. All falsification test regressions follow the same specifications as Tables 4 through 12. Asterisks denote statistical significance, as follows. *** p < .01, ** p < .05, * p < .10

Table 14. Placebo Test Results, by Country

	Schooling	Teen sex	Teen cohab.	Teen birth	Poor	Poorest
Kenya:						
Eligible by age 6	0.034	-0.015	-0.029	-0.067**	0.025	0.006
	(0.139)	(0.027)	(0.019)	(0.027)	(0.018)	(0.012)
Eligible by age 11	-0.166	0.018	0.037***	-0.013	0.035*	0.020*
	(0.133)	(0.021)	(0.010)	(0.011)	(0.018)	(0.010)
Observations	11,163	11,146	10,207	10,517	11,163	11,163
IV - F-statistic	0.8					
Tanzania:						
Eligible by age 7	0.068	-0.034	0.035	-0.002	0.018	0.006
	(0.327)	(0.036)	(0.027)	(0.038)	(0.029)	(0.022)
Eligible by age 13	0.064	-0.007	0.068	0.027	0.022	0.001
	(0.225)	(0.053)	(0.047)	(0.047)	(0.042)	(0.037)
Observations	3,990	3,991	3,750	3,781	3,991	3,991
IV - F-statistic	0.1					
Zambia:						
Eligible by age 7	-0.334*	-0.008	0.016	0.018	0.040	0.009
	(0.176)	(0.017)	(0.032)	(0.029)	(0.022)	(0.025)
Eligible by age 13	0.058	0.031	0.063**	-0.015	-0.002	0.013
	(0.233)	(0.024)	(0.027)	(0.037)	(0.021)	(0.015)
Observations	5,643	5,642	5,195	5,363	5,651	5,651
IV - F-statistic	1.8					

Notes: Estimated parameters in this table refer to the estimation of the first stage regression as described in equation (4). Reported F-statistics refer to the joint significance of the placebo assignments. All falsification test regressions follow the same specifications as Tables 4 through 12. Asterisks denote statistical significance, as follows. *** $p < .01$, ** $p < .05$, * $p < 0.10$

Figure 1. Trend in Net Attendance Rate in Sub-Saharan Africa, by Gender

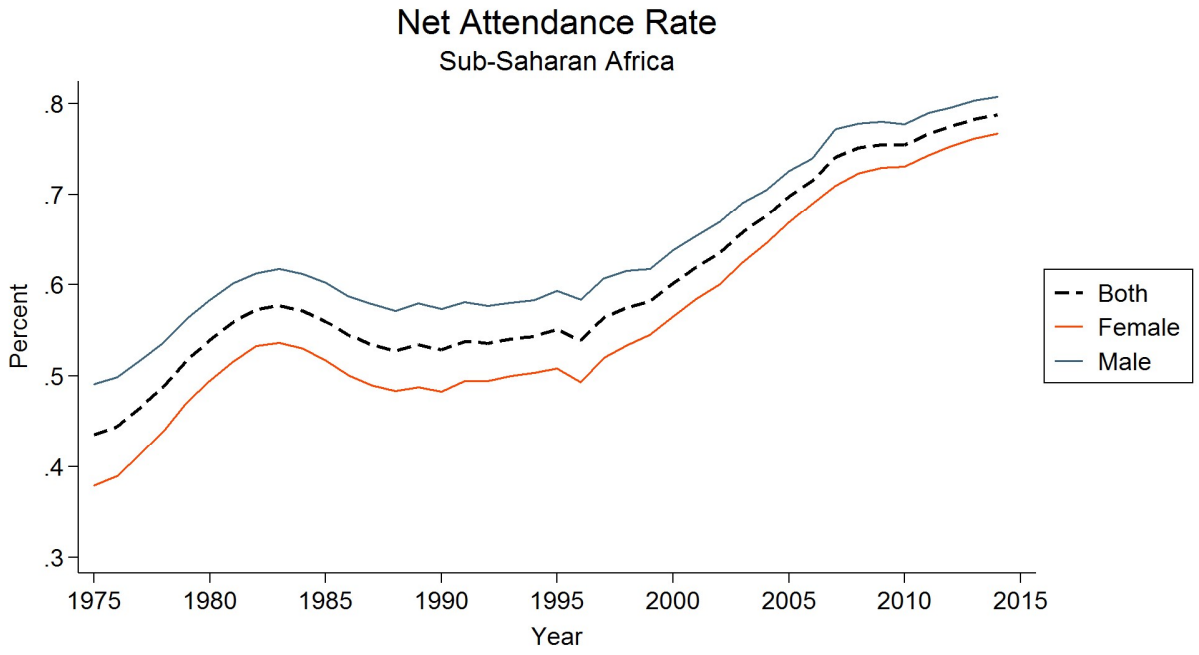

Figure 2. Universal Primary Education Eligibility, by Birth Cohort and Country

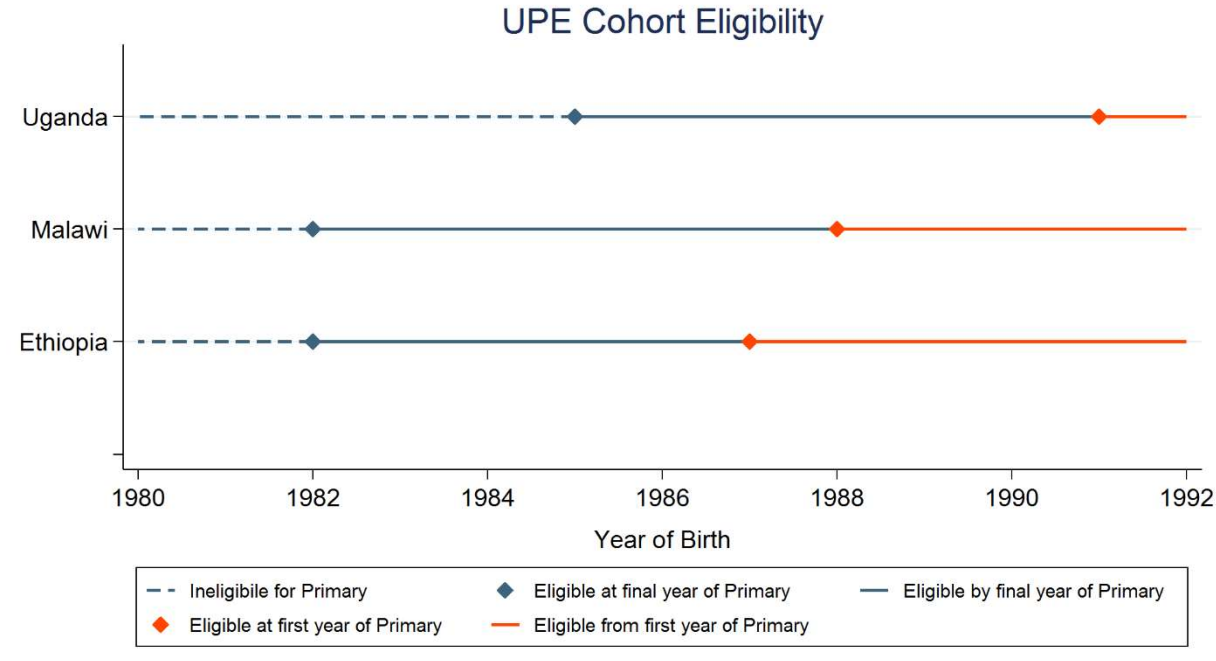

Figure 3. Mean Years of Schooling Completed in Ethiopia, by Gender and Birth Cohort

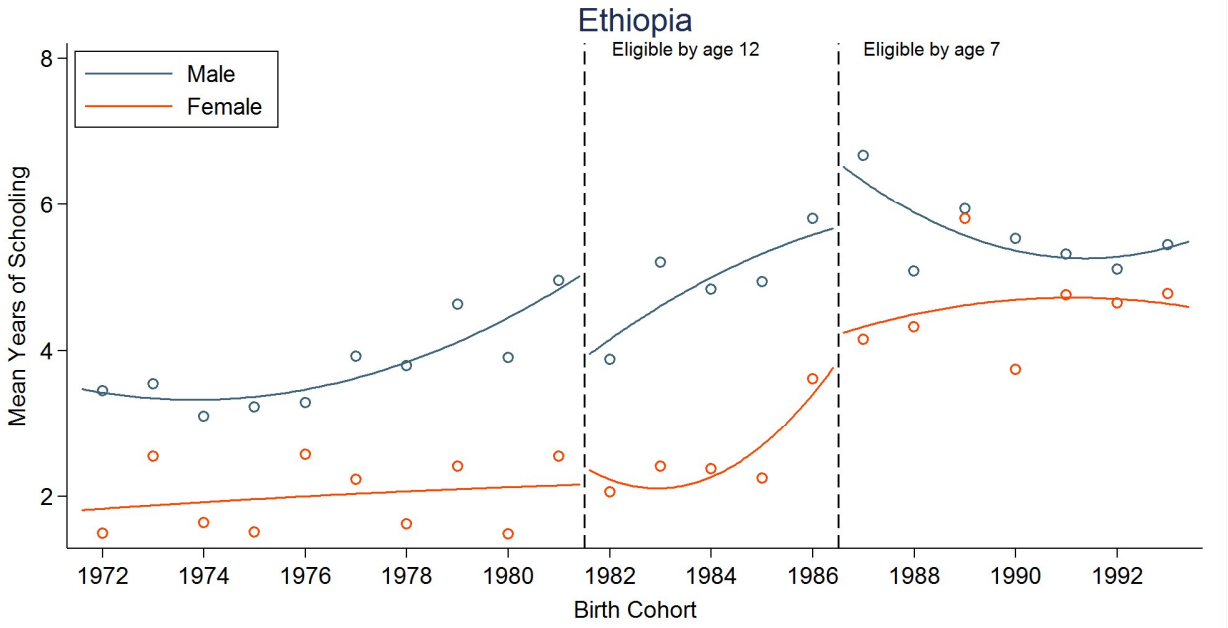

Figure 4. Mean Years of Schooling Completed in Malawi, by Gender and Birth Cohort

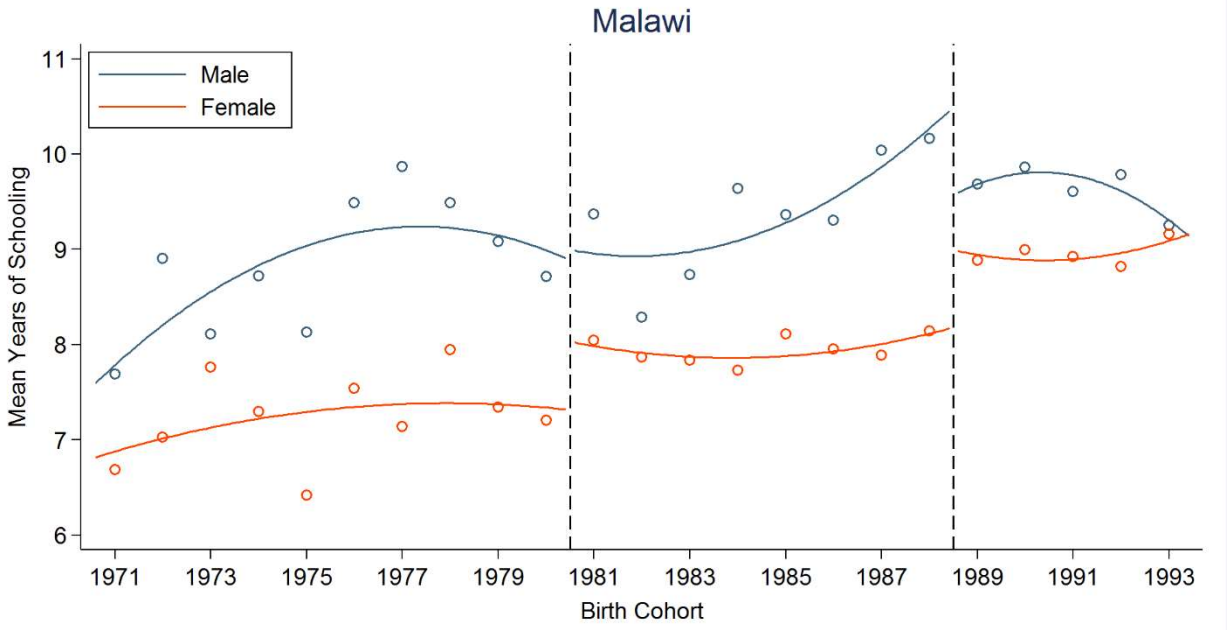

Figure 5. Mean Years of Schooling Completed in Uganda, by Gender and Birth Cohort