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Evaluating UPE in Uganda: school fee abolition and educational outcomes

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Abstract

This paper analyzes the effect of lifting primary school fees on educational attainment in Uganda. After the abolishment of school fees in 1997, the enrollment rate more than doubled. Two decades later, however, we know little about the effect of the policy on educational attainment. With recent data on eight cohorts exposed to free education, we analyze the impact of the policy on years of completed primary school, completion of primary school, and transitioning to secondary school. We use a straightforward regression analysis with cohort dummies and household fixed effects to control for unobserved heterogeneity. We find that lifting school fees had no effect on the years of primary school achievement and the likelihood of primary school completion. We find some weak evidence that the likelihood of those who completed primary education to start secondary school increased after UPE.

JEL Classification: I21, I28, O55
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1. Introduction

We analyze the impact of lifting primary school fees on educational outcomes in Uganda. Since the World Declaration on Education for All in Jomtien in 1990 and the conclusion of the Millennium Development Goals (MDGs) in 2000 called for Universal Primary Education (UPE), the number of children out of school worldwide has been reduced by nearly half and net primary enrollment has increased to 91 percent (United Nations, 2015). Despite the progress, in Sub-Saharan Africa, many of the toughest challenges and disparities remain, such as meeting demand for teachers (UNESCO-BREDA, 2009), and ensuring that students not only enroll but also progress well and are promoted to the next grade level (UN Economic Commission for Africa, 2015). In order to improve educational attainment, many African countries, Uganda among them, lifted user fees for primary school in the 1990s and early 2000s (Bhalotra, Harttgen, & Klasen, 2015). Given the political will and financial resources invested to enact UPE, as well as to help policymakers shape future education policy, we should first understand whether UPE achieved its intended outcomes. While there have been a number of studies examining the impact of UPE on enrolment rates (e.g. Deininger, 2003; Essama-Nssah, Leite, & Simler, 2008; Grogan, 2008; Nishimura, Yamano, & Sasaoka, 2008), studies on the impact of UPE on educational attainment have been scarce.

In this paper, we estimate the relationship between lifting primary school fees and educational attainment in Uganda. Educational attainment is defined as years of completed primary school and two binary indicators: completion of primary school and starting secondary school (conditional on having completed primary school). To estimate these effects, we conduct a cohort analysis of children in households where some received free education (treated), and others did not (control) across cohorts, followed by a host of robustness checks. We find that, surprisingly, lifting primary school fees had a generally insignificant effect on educational attainment. Compared with the pre-UPE reference cohort, the cohorts after UPE did not complete more years of primary schooling and were not more likely to complete primary school. We provide some weak evidence that they were more likely to start secondary school.

This paper makes two key contributions. First, we fill a gap in the existing literature by providing one of the first evaluations of the effect of the 1997 Ugandan primary school fee reduction policy on educational attainment, and in doing so, moving the discourse beyond enrollment rates. While enrollment rates exploded from 3.1 million in 1996 to 6.3 million by 1999 (World Bank, 2014), little is known about the impact of UPE on educational attainment. Part of the reason for this knowledge gap is due to data limitations. Until recently, insufficient time had passed to analyze the effect of the intervention.[3] Second, we suggest that UPE had an insignificant effect on educational attainment. This is an important contribution to the literature and for various policy stakeholders, considering the enormous expense of such interventions.

In terms of methodology, to analyze the effect of lifting school fees, we use a straightforward linear estimation technique, comparing cohorts before and after the intervention. We also take advantage of the generally large household sizes in Uganda and use household fixed effects to control for differences between households. Doing so allows us to compare siblings, one pre- and one post-UPE, while controlling for various types of household characteristics such as income, parents' education, and other unobserved household-level variables.

[3] Because school fees were lifted only in 1997, one would have to wait until data became available for at least several cohorts that were fully exposed to all seven years of free primary school.

This paper is structured as follows. In Section 2 we describe the context for our study by briefly introducing the Ugandan education system. We also review the existing studies on school fee reduction in Uganda. In Section 3 we present the data and describe the estimation strategy. We construct a large pooled cross-section and control for household effects to tease out the impact of free education across cohorts. In Section 4 we outline the methodology, and in Section 5, present the results and robustness checks. We then conclude in Section 6 by summarizing policy implications and areas for future research.

2. Background on the Ugandan education system and Universal Primary Education

Universal Primary Education (UPE) is a political initiative and an umbrella term for a number of policies targeted at primary school interventions. In Uganda, UPE aims to ensure that every child enters and completes primary school, thereby reducing inequities in education and eventually, reducing poverty. As part of the UPE agenda, in 1997, the government of Uganda formally abolished primary school tuition, Parents and Teachers Association fees, and textbook fees for up to four children per family[4]. This no-fee policy was rolled out across all primary school grades, rather than being introduced cohort-by-cohort. At the time the policy was enacted, the gross primary school enrollment rate in 1996 was 71% (World Bank, 2016). For parents, the financial burden of enrolling their children in school was not trivial, as they were expected to finance around 60 percent (Kattan & Burnett, 2004). When tuition was lifted, within the first year, the gross enrollment rate[5] increased from 71% to 125%, and peaked at 138% in 2003 (World Bank, 2016), suggesting that many younger and/or older children entered primary school. While lifting school fees had an immediate and notable impact on enrollment rates, its effect on educational attainment has not been sufficiently investigated.

In Uganda, primary education consists of seven years, followed by two years of lower secondary school, and two years of upper secondary school. The official primary school enrollment age is six, but many children start school later, delaying enrollment, which is common in Uganda (Moyi, 2011). Delayed enrollment is problematic, because it correlates with higher dropout rates (Wils, 2004). A cross-section of students from the 2013-14 Uganda National Household Survey shows substantial age-inappropriate enrollment. Table 1 illustrates this situation, showing the percentage of children enrolled in a given grade by age. The first column lists ages, and the rows represent the percentage of children of different ages at a given grade level. The cell that corresponds to the official enrollment age is shaded. For example, by the final year of primary school (grade 7), only 13.8% of students are of official enrollment age, with many children as much as four years behind. Many students are in the 15-20 age category, which is most notable at grade six and above.

[4] Initially, only four children per household could receive free education, of which two were required to be female if the household had at least two female children. This policy was difficult to implement in practical terms, and was modified to include all children (Bategeka & Okurut, 2006). Parents also had to continue to pay for uniforms, writing materials, meals, and transportation (Tamusuza, 2011).

[5] The gross enrollment rate represents all enrolled primary school students relative to the number of students of official enrollment age. Because students can be of different ages and include children beyond primary school age, these ratios may exceed 100%.

Table 1. Age-appropriate enrollment, the percentage of children by age per grade level

	Grade level												
Age	1	2	3	4	5	6	7	8	9	10	11	12	13
4 & under	38.7	2.3	-	-	-	-	-	-	-	-	-	-	-
5	22.3	8.2	0.6	-	-	-	-	-	-	-	-	-	-
6	17.9	17.4	4.7	0.5	-	-	-	-	-	-	-	-	-
7	10.9	25.5	13.5	4.0	-	-	-	-	-	-	-	-	-
8	5.8	18.2	18.9	10.3	2.7	-	-	-	-	-	-	-	-
9	2.2	14.6	23.1	17.1	8.4	2.4	-	-	-	-	-	-	-
10	1.0	7.0	15.2	17.0	14.5	9.2	2.7	1.3	-	-	-	-	-
11	0.5	3.3	9.7	18.8	15.8	10.6	8.0	4.5	-	-	-	-	1.1
12	0.5	1.7	8.8	15.1	21.9	19.8	13.8	10.8	6.5	-	-	-	-
13	-	0.6	2.2	9.0	16.1	19.1	17.2	19.3	11.0	6.1	1.0	-	-
14	-	0.3	2.1	4.5	7.5	14.4	12.6	19.3	19.9	11.2	8.3	2.6	-
15	-	0.4	-	1.8	7.1	11.7	16.5	17.0	19.5	19.6	10.7	10.5	3.2
16	-	-	0.5	1.0	2.9	6.4	12.6	15.7	18.7	22.0	18.5	17.1	7.4
17	-	-	-	0.3	2.1	3.5	8.5	5.4	11.0	16.4	22.0	21.1	24.5
18	-	-	-	-	-	2.1	4.1	2.2	8.9	12.6	18.0	26.3	24.5
19	-	-	-	-	0.5	0.2	2.4	3.6	4.1	8.9	14.1	13.2	23.4
20	-	-	-	-	-	-	1.0	0.9	0.4	3.3	7.3	9.2	16.0
n	878	780	772	730	663	575	412	223	246	214	205	76	94

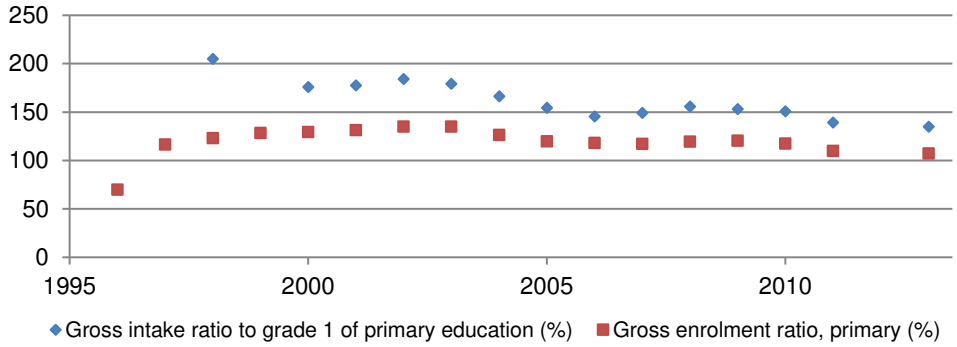
Source: Own calculations based on the 2013-14 Uganda National Panel Surveys for all students currently attending school, aged 20 and younger. The columns show the percentage of children by age, for the given grade level (e.g. in column 2, which is grade 1, 17.9% of children are 6 years old). The official enrollment age of each grade level is highlighted.

Alternatively, we can measure the scale of age-inappropriate enrollment by examining the gross enrollment ratio (total primary school enrollment divided by the number of children in official enrollment age, i.e. 6-12) and the intake ratio (new entrants in grade 1, regardless of age, divided by the number of official primary entrance-age students, i.e. 6-year-olds). Figure 1 shows that the gross enrollment ratio peaked after 1997 and continues to remain above 100%. As shown by the gross intake ratio, age-inappropriate enrollment is persistent, and in 1998, more than half of all new primary school entrants were mostly older (and some younger) than the official enrollment age of six years. Remarkably, the gross intake ratio falls after 1998 but remains far above one until after 2010, suggesting that delayed enrolment into primary school persists and/or many children enrolled in first grade as repeaters.

The existing literature on the impact of lifting primary school fees in Uganda is relatively small and outcomes are mostly measured as enrollment rates. The seminal contribution to this literature is a study by Deininger (2003), who finds that the reduced cost of schooling had a positive and significant effect on school attendance rates. His study used a repeated cross-section to compare households pre- and post-intervention, using two different nationally representative household surveys from 1992 and 1999-2000. Results showed that the impact of reducing school fees was large, leading to a 60 percent increase in the probability of enrollment between the two time periods. Furthermore, controlling for income levels, the impact was especially notable for the poor; and controlling for gender, the study finds a significant increase in the enrollment of girls. While Deininger provides a robust early analysis of the impact of fee reduction on enrollment rates, we are now—given the additional data and time—able to exploit ex-post data for a greater

number of cohorts that have benefited from reduced school fees to estimate educational attainment. By using recently available data, we are also able to control for time trends (in the robustness checks) that could not be controlled for in 2000, as the policy was only enacted three years prior.

Figure 1. Gross intake and enrollment ratios



Note: The gross intake ratio is the number of all students in grade 1 of primary school, relative to the number of official school-entrance age students. The gross enrollment rate is all enrolled primary school students relative to the number of students of official enrollment age. Because students can be of different ages, these ratios may exceed 100%. Blank spaces represent missing data for the respective year. Source: World Bank (2014).

We also found four additional studies on the effect of lifting primary school fees in Uganda. Nishimura, Yamano, and Sasaoka (2008) investigate its effect on enrollment rates. The authors use a dataset collected in 2003, six years after the intervention, allowing for almost one full cohort (seven years) to move through primary school. The study finds that the intervention significantly reduced delayed enrollment (defined as starting primary school later than six years of age), and also increased grade completion rates up to grade 4 for boys, and grade 5 for girls. The authors use a probit analysis with the following outcome variables: delayed enrollment (starting school after the age of six), and whether a child completed school by grades 4 or 5, by gender. Data used for the analysis covers a relatively small sample of 940 households in rural Uganda. While the analysis helps answer some questions about the effect of fee reduction—such as identifying the gender bias in completion rates—data limitations prevent the authors from identifying the effect across all grades and assessing the impact on both rural and urban households.

Grogan (2008) also investigated delayed enrollment and found that the 1988-92 birth cohorts (in 1997, respectively, ages five to nine) were more likely to enter primary school before the age of nine, compared with children born in 1982-87, before the elimination of fees. The study used a regression discontinuity analysis design to identify the probability of attending school before and after the intervention.

Essama-Nssah et al. (2008) employed a difference-in-differences estimation strategy to measure the impact of the 1997 policy on enrollment using cross-sectional data for 1992-2005. They found that reduced fees increased enrollment as well as age-appropriate entry.

Lastly, Lincove (2012) also investigated enrollment rates and modeled school enrollment as a function of price using the 2001 Uganda Demographic Health Survey and EdData. She found that while enrollment increased, price was still a determining factor particularly for male and female foster children. Supply-side factors such as distance to school also hindered enrollment. In short, while enrollment has increased since UPE, existing costs still remain a barrier for some.

In order to expand our understanding of the effect of lifting school fees, we supplement enrollment-focused studies with an analysis of attainment, taking duration and completion into account, i.e. do they stay in school, and for how long? To the best of our knowledge, only Bhalotra et al. (2015) address the effect of lifting tuition on educational attainment using econometric methods. Their study analyzes educational attainment across 67 countries and approximately 1 million children between the ages of 15 to 18 years. The study found that lifting primary school fees increased total education by 0.2 years. For African countries, the effect was larger, at 0.289 years.

In this paper, we focus on one country to gain an understanding of the effect of lifting primary school fees on educational attainment. A nuanced understanding of lifting school fees is also relevant given the high investment level of the intervention, and because of its economic implications. A large number of studies agree that education is a lever for economic growth (Barro, 2001; Benhabib & Spiegel, 1994; Cohen & Soto, 2007), and also explicitly for developing countries context (Baldacci, Clements, Gupta, & Cui, 2008; Hanushek, 2013). A standard argument is that investment in education can generate economic growth through an increase in wages (Duflo, 2001; Psacharopoulos & Patrinos, 2004). This notion is rooted in human capital theory, which argues that education leads to increased productivity, and subsequently, an increase in wages. The magnitude of the rate of return to education, however, is not straightforward as it depends on many country-specific factors. In Uganda, Appleton (2001) finds that education reduces the risk of poverty and increases both the private rate of return (30%) and social rate of return (24%) for primary education. Appleton also finds that in 1999 and 2000, an extra year of schooling led to a 17% increase in wages. Of course, we acknowledge that there exists a wide span of benefits—from improved health outcomes (Jamison, Jamison, & Hanushek, 2007) to the strengthening of civil society—which lie beyond the scope of this paper. Our premise is that education is a valuable asset and we focus on how a fee elimination policy has shaped educational attainment for students in Uganda.

3. Data

Our primary data set combines fives waves of data from the Uganda National Household Survey (UNHS) and Uganda National Panel Survey (UNPS). All surveys are nationally representative household surveys using a stratified random sample design, with enumeration areas as the sampling units. In 2005-06, the UNHS survey covered 3,123 households. The UNPS then re-interviewed 2,975 households in 2009-10. This was followed by re-interviewing 2,716 households in 2010-11, 2,850 households in 2011-12 (additional households were added from the 2012 Uganda Population and Housing Census), and 3,119 households in 2013-14. The left-hand panel of Table 2 provides a decomposition of the household and individual sample sizes by survey year. Surveys were administered over a twelve-month period; and each household was visited twice within the survey period to account for responses affected by seasonality. The strata include Kampala City, other urban areas, Central Rural, Eastern Rural, Western Rural, and Northern Rural. The UNPS surveys are based on the sampling method used in the 2005-06 UNHS survey.

For our analysis, we pooled all five waves. We then constructed our dataset by first only including participants from the 2013-14 survey. Next, we worked backwards, adding individuals from the previous survey years that were not present in the most recent survey year. Therefore, while using data from all five waves, each household member is represented only at one point in time in our final dataset. This approach allows us to capture household members who left the household in previous years and who are no longer living in the household in the most recent survey year. The ability to construct households that include people who left the household in future years is important for our analysis and enables us to control for

household effects to compare siblings within households. The right-hand panel of Table 2 shows the composition of our final data set by survey year.

Table 2. Composition of our data set, before and after combining five survey waves

	Survey Sample Size		Final Data Set		
Year	Individuals	Households	Individuals	Households	Percent
2005-06	16,759	3,123	2,533	1,493	22
2009-10	18,734	2,975	644	440	7
2010-11	19,189	2,716	724	463	7
2011-12	21,487	2,850	3,112	1,543	23
2013-14	17,510	3,119	6,124	2,745	41
Total	93,679	14,783	13,137	6,684	

Source: Own calculations based on the 2005-06 Uganda National Household Survey and the Uganda National Panel Surveys for the years 2009-10, 2010-11, 2011-12, and 2013-14.

To specify the criteria for exposure to the lifting of tuition fees, we define the post-UPE treatment group as cohorts who could benefit from the full seven years of free primary school and the control group as those who received fewer years or none.[6] A visual tool is provided in Appendix A-1, where the first column represents year of birth and the rows show age in the given year. For example, a child born in 1991 is six years old in 1997 (corresponding to grade 1). The table assumes official enrollment age progression, defined as entering primary school at age 6 without grade repetition. Because of the high frequency of delayed enrollment, we allow for a delay of one year, but not more, which we consider a reasonable threshold given that UPE had a significant and positive effect on age-appropriate enrolment (see Grogan, 2008). Therefore, our pre-UPE control group birth cohorts are 1980-89 (10 cohorts), and our post-UPE treatment group cohorts are 1990-1997 (8 years). We also restrict our control sample to people born after 1979 because restricting our sample allows us to avoid comparing people from different generations and different education systems.

To ensure that our sample only includes children who had ample time to complete primary school, we exclude children who were under 15 at the time they were captured in a given survey wave, in either 2005-06, 2009-10, 2010-11, 2011-12, or 2013-14. This also demonstrates the advantage of using 5 waves of the survey. For example, a child might have been 10 in the first wave (2005-06), 15 in the third one (2010-11) and have left the household by the fifth wave (2013-14). That child would be captured in our dataset in the last year of age (15+) when she was still living in the household, in our example, in the 4th wave (2011-12).

For children 15 and older, we expect that they have had sufficient time to complete primary school. While a conservative sample definition would increase the minimum age, it would also exclude more age-appropriate and successful younger children and downward bias our analysis. Therefore, we construct our sample as individuals 15 and older, two years more than the official enrolment age of someone who completed grade 7. Since the reform improved age-appropriate schooling (Essama-Nssah et al., 2008;

[6] In constructing our treatment and control groups, we initially attempted to include spillover effects. Following Bhalotra et al. (2015) who include partial-treatment effects, we considered assigning partial treatment status to children in the second grade and above when school fees were lifted. This would allow us to include children who received partial reduced fees for primary school but had paid the full school fee in at least one prior year. The survey data, however, does not indicate the year in which the household member started school. One would thus have to establish thresholds by cohorts in defining partial exposure. Due to high levels of delayed enrollment and repetition, we instead separated the sample by full exposure (treatment) and no or at most partial exposure (control) groups.

Grogan, 2008; Nishimura et al., 2008), we expect that by age 15, the effect of free primary schooling should be visible.

To control for other factors affecting attainment, we use a household fixed effects approach in the following way. First, we kept only household members whose relationship to the head was identified as a 'child', defined as children, grandchildren, stepchildren, and nephews and nieces. Limiting the sample to 'child' status ensures that we compare children with other children, and not with, for example, their parents. Second, we limit our analysis to households with at least two children. Third, at least one child must meet the cohort criteria of the treatment group, i.e. at least one child must be born after 1989, and at least one child must be born before 1990, our control group. After applying these criteria, we reduce the sample to 3,251 children in 809 households.

Table 3. Descriptive statistics of the control and treatment groups, by birth cohort

	Mean																	
	80	81	82	83	84	85	86	87	88	89	90	91	92	93	94	95	96	97
Gender (male=1)	0.60	0.59	0.63	0.53	0.60	0.58	0.56	0.58	0.52	0.48	0.55	0.63	0.61	0.45	0.55	0.55	0.49	0.58
Attending school	0.24	0.23	0.30	0.40	0.28	0.36	0.42	0.46	0.51	0.51	0.61	0.49	0.54	0.60	0.68	0.75	0.79	0.84
Location (rural=1)	0.62	0.67	0.70	0.62	0.74	0.72	0.71	0.69	0.76	0.75	0.66	0.73	0.72	0.77	0.73	0.76	0.77	0.81
Years of primary edu.	6.06	6.74	6.51	6.60	6.35	6.40	6.24	6.28	6.28	6.26	6.14	6.43	6.32	6.23	6.28	5.88	5.76	5.83
Years of total edu.	8.98	11.38	9.70	10.45	9.18	9.39	8.71	9.01	9.05	9.31	8.77	9.83	8.97	8.13	7.92	7.01	6.70	6.60
Primary school completion	0.70	0.90	0.79	0.84	0.75	0.76	0.66	0.66	0.62	0.66	0.65	0.65	0.73	0.64	0.62	0.48	0.44	0.45
Secondary school entry	0.54	0.87	0.66	0.72	0.64	0.66	0.54	0.57	0.55	0.57	0.55	0.69	0.59	0.53	0.54	0.40		
N	63	39	70	77	114	160	194	222	231	285	257	179	239	232	258	244	248	139

Source: Own calculations based on the 2005-06 Uganda National Household Survey and the 2009-10, 2010-11, 2011-12, and 2013-14 Uganda National Panel Surveys. The final sample totals 3,251 people. For the sample of students who progress to secondary school entry, we use the same selection method as in the main sample (and hence lose data for the 1996-97 cohorts), and then keep only households with two children, one born in the pre-treatment, and the other born in the post-treatment cohort. We then condition the construction of the secondary school entry binary variable on having completed primary school. We therefore remove all observations that have not completed a primary school degree.

Table 3 provides basic summary statistics of our control and treatment groups by cohort. In terms of gender, we see generally more boys. Across cohorts, most of our sample lives in rural areas, and the average years of primary school is slightly more than 6 years. Thus, most children were already close to completing primary education, suggesting that lifting user fees mainly lowers the cost of education to parents that already sent their children to school, with the potential to boost schooling years but attracting more children to primary school being limited. But despite the relatively high average number of years of primary schooling, a significant share of children does not complete primary school, and even fewer transition to secondary school. Lifting fees could improve both those indicators. We also notice that for children born in 1995 and later, their mean years of education are slightly lower, likely reflecting the problem of delayed enrollment. On the one hand, because of delayed enrolment, perhaps these children require more time to complete primary school and should hence be removed from our sample. On the other hand, we argue earlier that by excluding these children, given that they have been fully treated, excluding these younger cohorts would downward bias our analysis. A visual aide showing mean years of primary and total education across birth

cohorts as well as the share of children who completed primary school and started secondary school—also across birth cohorts—is provided in Appendix A-2.

4. Methodology

Our empirical method is a linear regression with household fixed effects and cohort dummies. Our regression specification is as follows,

$$Ed_i = \beta_0 + \sum_t \beta_t Year_{it} + \beta_1 Male_i + \psi_i + \in_i ,$$

where subscript i represents individual i. Ed represents years of completed primary school ranging from zero to seven. $Year$ is a dummy variable which takes the value of 1 if individual i is born in year t, and 0 otherwise. Appendix A-1 shows a table of student enrolment by age. 1991 is the first cohort to be officially treated but because a large number of children delay their enrolment, we assign 1990 as the first treated cohort. The reference category is 1989, which we use to compare years of education before and after treatment. The household fixed effects are represented by ψ. We also control for three characteristics affecting educational access in Uganda, gender, rural-urban location, and region (Gideon & Bemanzi, 2013); the latter two are only included in the robustness checks without household fixed effects. The reference category for the regional dummies is the Central Region, which includes Kampala.

In addition to years of completed primary school, we also estimate the impact of lifting school fees on two additional outcomes: the likelihood of completing primary school and the likelihood of transitioning to secondary school (conditional on having completed primary school). Because our dependent variables are binary, we would ideally employ a probit estimation model. However, we cannot consistently estimate a probit model with fixed effects without a larger time dimension and a larger sample size. We therefore use a linear probability model to predict the effect of UPE.

In a robustness check, we also use a parametric approach to assess the impact of UPE on years of primary schooling, primary completion, and starting secondary education.

5. Results

5.1. Main Analysis

Results obtained from our regressions indicate that cohorts that started school in the first three to four years post-UPE enactment accumulated fewer years of education compared to the reference cohort, although the differences are not significant. In Table 4, we regress years of primary school on cohort dummies and other covariates. The reference cohort category for columns 1 and 2 is 1989. Column 1 controls for rural/urban location and regional dummies, and column 2 controls for household fixed effects. In both columns 1 and 2, we see that relative to the reference cohort, the early cohorts fully exposed to UPE (1990-94) experienced years of primary schooling no different from the reference cohort, while for the 195-97 cohorts, primary schooling years are significantly lower.

Next, we widen the sample of students by removing the restriction that per household, there must be at least one each of pre- and post-UPE cohorts, and that the individual must have 'child' status in the household. This more than triples our sample to 10,163 students. We run the same regressions for this sample as in columns 1-2. Consistent with the previous findings, in columns 3 and 4, we find that the 1995-97 cohorts complete, on average, 0.215-0.472 years less education than the reference cohort, while the first five cohorts exposed to UPE did not have different schooling from the reference cohort.

Table 4. Dependent variable: Years of primary education, with household fixed effects

	(1)	(2)	(3)	(4)
1980	-0.261	-0.137	-0.370***	-0.246
	(0.241)	(0.262)	(0.103)	(0.158)
1981	0.449***	0.370**	-0.357***	-0.143
	(0.153)	(0.172)	(0.121)	(0.170)
1982	0.238	-0.0320	-0.0650	0.0382
	(0.154)	(0.175)	(0.0923)	(0.142)
1983	0.272*	0.165	-0.0449	0.111
	(0.149)	(0.161)	(0.0882)	(0.118)
1984	0.0837	0.147	0.0440	0.136
	(0.148)	(0.156)	(0.0813)	(0.114)
1985	0.103	0.116	-0.0461	-0.0742
	(0.124)	(0.140)	(0.0840)	(0.117)
1986	-0.0288	0.0789	0.0248	0.0580
	(0.116)	(0.132)	(0.0752)	(0.104)
1987	-0.0224	0.0110	0.0313	0.0455
	(0.108)	(0.113)	(0.0750)	(0.104)
1988	0.0238	-0.00345	0.0228	-0.0312
	(0.108)	(0.120)	(0.0767)	(0.108)
1989	ref. year	ref. year	ref. year	ref. year
1990	-0.165	-0.102	-0.152**	-0.134
	(0.115)	(0.128)	(0.0739)	(0.103)
1991	0.149	0.0528	0.0895	-0.0311
	(0.116)	(0.131)	(0.0798)	(0.109)
1992	0.0437	0.0380	0.0732	0.0477
	(0.112)	(0.133)	(0.0735)	(0.102)
1993	-0.0117	-0.0227	0.0333	0.0489
	(0.113)	(0.128)	(0.0736)	(0.101)
1994	0.0154	0.0655	-0.0477	0.0303
	(0.0991)	(0.114)	(0.0711)	(0.0952)
1995	-0.384***	-0.431***	-0.189**	-0.215**
	(0.107)	(0.126)	(0.0736)	(0.104)
1996	-0.487***	-0.439***	-0.367***	-0.404***
	(0.109)	(0.119)	(0.0724)	(0.0988)
1997	-0.405***	-0.405***	-0.402***	-0.396***
	(0.128)	(0.146)	(0.0808)	(0.118)
Gender,	-0.00327	0.123**	0.112***	0.152***
male=1	(0.0488)	(0.0530)	(0.0291)	(0.0411)
Rural=1,	0.0170		-0.521***	
Urban=0	(0.0485)		(0.0346)	
Region: East	-0.505***		-0.147***	
	(0.0601)		(0.0443)	
Region: North	-0.0962		-0.483***	
	(0.0743)		(0.0497)	
Region: West	-0.351***		-0.339***	
	(0.0908)		(0.0484)	
Household fixed effects	NO	YES	NO	YES
Constant	6.768***	6.199***	6.638***	6.024***
	(0.0919)	(0.0788)	(0.0582)	(0.0698)
Observations	3251	3251	10163	10163
R^2	0.071	0.482	0.065	0.588
Adjusted R^2	0.065	0.306	0.063	0.320

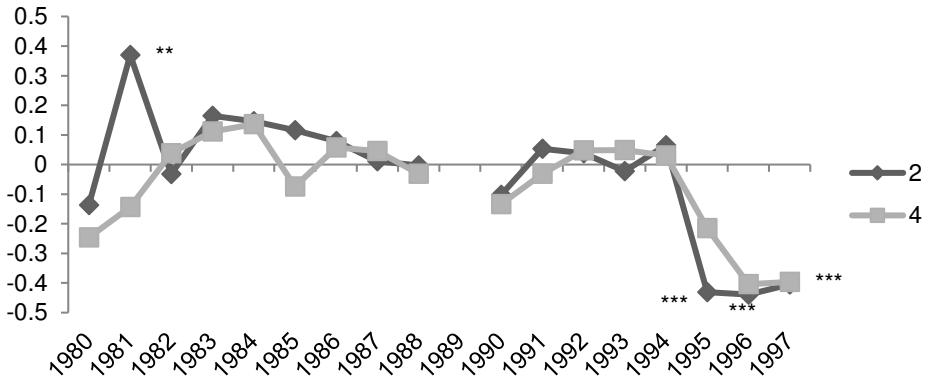
Note: Standard errors are in parentheses and * p < .10, ** p < 0.05, *** p < 0.01. The reference category for region is the Central Region, which includes Kampala.

Figure 2 illustrates the average years of primary education by cohort, plotting the coefficients from Table 4. Each figure compares the restricted sample with the broader sample which includes everyone of a given birth cohort. The statistical significance of each coefficient is marked by stars, corresponding to the p-levels

(* p < .10, ** p < 0.05, *** p < 0.01). Three findings stand out. First, there is no trend towards rising years of primary schooling over time. Children born in the 1980's have no fewer years of primary schooling than those born in the 1990s, particularly in the restricted sample. This is remarkable in and of itself as education was expanding in most Sub-Saharan African countries. Second, after UPE, there is no evidence whatsoever that primary schooling years increased. And third, the youngest cohorts actually have fewer years. This might be due to delayed enrolment and slow progression through the school system, suggesting that UPE did not improve on this either.

Regarding our control variables, the results also indicate gender inequality in education; being male corresponds to greater years of primary school completion. In our most rigorous specification, being male corresponds to 0.123 years of more education than females, statistically significant at the one-percent level. While there are no rural-urban differences in the restricted sample, we see them in the larger sample, and there are substantial regional differences.

Figure 2. Visual plot of the coefficients corresponding to Table 4



Note: The figure plots the coefficients for Table 4, columns 2 and 4, which control for household fixed effects. The reference category is 1989, and line/column 2 is a limited sample of households with at least one child born before 1989, whereas line/column 4 does not restrict the sample to the same criteria. * p < .10, ** p < 0.05, *** p < 0.01

We next examine the trends in primary school completion and entering secondary school by cohort. Table 5 shows the likelihood of both, regressed on cohort dummies and gender. Starting with primary school completion, column 1 regresses years of education on cohort dummies, gender, and rural/urban location, and regional dummies. We limit the sample to individuals with 'child' status, and households with at least two children (one pre- and one post-UPE). Column 2 is a more rigorous specification, in that we also control for household fixed effects. Cohorts born after 1990 are no more likely to complete primary school than those before. Again, the youngest cohorts are less likely to complete primary school, likely due to delayed entry and slow progression.

Gender also plays a role; being male seems to slightly increase the likelihood of completing primary school (by 0.04 years).

Table 5. Completing primary school and starting secondary school, linear probability model

	Primary School Completion		Secondary School Entry		
	(1)	(2)	(3)	(4)	(5)
1980	0.00831	-0.00405	-0.0620	0.0610	-0.0279
	(0.0634)	(0.0626)	(0.0567)	(0.0581)	(0.0623)
1981	0.223***	0.209***	0.145**	0.108	0.0280
	(0.0777)	(0.0769)	(0.0629)	(0.0698)	(0.0750)
1982	0.119*	0.0589	0.0163	0.101*	0.0259
	(0.0608)	(0.0599)	(0.0515)	(0.0539)	(0.0582)
1983	0.155***	0.104*	0.0301	0.0361	-0.0605
	(0.0585)	(0.0578)	(0.0479)	(0.0497)	(0.0546)
1984	0.0830	0.104**	0.0407	0.0809*	0.0107
	(0.0505)	(0.0495)	(0.0429)	(0.0461)	(0.0519)
1985	0.0875*	0.0923**	0.0411	0.0508	-0.0160
	(0.0450)	(0.0448)	(0.0372)	(0.0409)	(0.0472)
1986	-0.00552	0.0492	-0.00520	0.00415	-0.0781*
	(0.0424)	(0.0414)	(0.0366)	(0.0386)	(0.0442)
1987	-0.0233	-0.0160	0.0356	0.0907**	0.00103
	(0.0408)	(0.0397)	(0.0349)	(0.0364)	(0.0430)
1988	-0.0384	-0.0286	0.0577	0.0831**	-0.0000916
	(0.0403)	(0.0401)	(0.0351)	(0.0371)	(0.0430)
1989	ref. year	ref. year	ref. year	ref. year	ref. year
1990	-0.0309	-0.00128	0.0140	0.0738**	-0.000654
	(0.0392)	(0.0386)	(0.0333)	(0.0353)	(0.0426)
1991	0.0888**	0.0640	0.0851**	0.118***	0.0607
	(0.0435)	(0.0415)	(0.0358)	(0.0366)	(0.0404)
1992	0.0585	0.0536	-0.0167	0.0615*	-0.0320
	(0.0400)	(0.0380)	(0.0328)	(0.0343)	(0.0400)
1993	-0.0128	-0.0195	0.00166	0.0270	-0.0274
	(0.0403)	(0.0385)	(0.0346)	(0.0361)	(0.0414)
1994	-0.0401	-0.0286	0.0522	0.0831**	0.0505
	(0.0391)	(0.0377)	(0.0337)	(0.0351)	(0.0425)
1995	-0.178***	-0.175***	-0.00931	0.0427	0.0367
	(0.0397)	(0.0380)	(0.0377)	(0.0393)	(0.0504)
1996	-0.213***	-0.177***			
	(0.0395)	(0.0381)			
1997	-0.205***	-0.239***			
	(0.0472)	(0.0452)			
Gender, male=1	0.00295	0.0386**	-0.00956	-0.00526	0.0101
	(0.0161)	(0.0161)	(0.0158)	(0.0168)	(0.0180)
Rural=1, Urban=0	-0.211***		-0.0989***		
	(0.0187)		(0.0170)		
Region: East	-0.0634***		-0.0134		
	(0.0216)		(0.0205)		
Region: North	-0.155***		-0.0462*		
	(0.0237)		(0.0237)		
Region: West	-0.0469**		-0.00659		
	(0.0231)		(0.0220)		
Household fixed effects	NO	YES	NO	YES	YES
Age restricted to	15+	15+	15+	15+	17+
Constant	0.878***	0.640***	0.910***	0.797***	0.868***
	(0.0322)	(0.0271)	(0.0241)	(0.0211)	(0.0305)
Observations	3251	3251	2088	2088	1623
R^2	0.105	0.511	0.030	0.511	0.536
Adjusted R^2	0.099	0.345	0.020	0.263	0.277

Note: Standard errors are in parentheses and * p < .10, ** p < 0.05, *** p < 0.01. The reference region category is the Central Region, which includes Kampala. The sample of individuals for secondary school entry is conditional on having completed primary school.

For the likelihood of starting secondary school, we restrict our sample to the same group as in Table 5, column 1, and then further restrict our sample to individuals that completed primary school. The last cohort for whom we have data is the 1995 cohort. In our most rigorous specification (column 2), which controls for household fixed effects, four UPE cohorts (1990, 1991, 1992, and 1994) have a statistically significant

positive likelihood of entering secondary school, relative to the reference group. But note that also some cohorts prior to UPE also had a significantly higher positive likelihood to start secondary school, suggesting that UPE did not shift the likelihood upwards, compared to most pre-UPE cohorts. To ensure that all children have been given sufficient time to complete primary school and start secondary, column 5 presents the results from a sample of individuals 17 and older. The post-UPE cohorts in this specification are not statistically different from the reference group. This may indicate that younger students or those whose ages are closer to official enrollment ages may be more successful in advancing to secondary school after UPE. Interestingly, there is no significant relationship between gender and the likelihood of entering secondary school, in contrast to completing primary, where being male has a positive effect. Overall, we take this as weak evidence in favor of UPE having increased the likelihood to enter secondary school, especially among younger children in age-appropriate grades.

5.2. Robustness Checks

In a first robustness check, we varied the age threshold of when we expect children to have completed primary school. While we argue that children aged 15 and older had ample time to complete primary school, we cannot completely dismiss the problems of age-inappropriate enrollment and delayed enrollment. We therefore estimated years of education including both younger and older cohorts to determine whether shifting the threshold to include only children aged 15 and older has a decisive impact on our results. In Appendix A-3, column 1, if we set the sample age threshold to 13, the results are different. Cohorts 1990-94 have greater years of education compared to the reference cohort; but cohorts prior to 1989 also had more years of schooling suggesting that the last cohort prior to UPE had particularly low years of primary schooling at age 13 or older. To the extent these results can be interpreted in favor of an effect of UPE, it suggests that early treated cohorts got through primary school faster than the reference cohort.

Including such young individuals, however, assumes age appropriate enrollment, which based on the descriptive statistics, is not the situation in Uganda and thus gives a biased assessment of the impact of UPE. Columns 2, 4, and 5 are more similar to our main age threshold (column 3), and column 6 likely biases our results as children who leave the household before 18 are excluded. Setting the threshold too high (old), as in column 6 is also problematic it dramatically decreases the sample size.

In a second robustness check, we use a parametric approach to estimate the effect of UPE. In particular, we investigate whether UPE changed a pre-existing time trend. In particular, we estimate the following model:

$$Ed_i = \beta_0 + \beta_1 UPE_i + \beta_2 Year_i + \beta_3 Year_i^2 + \beta_4 Male_i + \beta_5 UPE_i * Year + \psi_i + \varepsilon_i,$$

where subscript i represents individual i. Ed represents years of completed primary school ranging from zero to seven. UPE is a dummy variable that takes the value of 1 if the child was born in 1990 or later, that is, being age 7 or younger in 1997, when the policy came into effect (see Appendix A-1). $Year$ represents the birth year of the individual, and a squared term of birth year is included to allow for a non-linear relationship between cohort and education. $Year$ is coded as a two-digit number, the actual year of birth subtracted by 1900. We also control for an interaction term, $UPE*Year$ to control for the fact that children who were exposed to UPE in the initial years may have been less affected by the program than children from later cohorts, when UPE was more established. The household fixed effect is represented by ψ. As

in the main analysis, we also control for gender, rural-urban location, and region when not including household fixed effects.

Results are presented in Table 6, column 1. Column 1 shows that the coefficients for both UPE and the interaction term are significant. As expected, we also find that living in a rural household and in particular, being from the Northern region has a large negative effect on years of primary education.[7] But these results could be driven by unobserved heterogeneity between households, so we now turn to our preferred fixed effects specification.

In Table 6, column 2, UPE alone has a negative effect (-0.188) on years of primary schooling. We then examine the effect of the year trends, accounting for the possibility of non-linear effects, represented in column 3. The year trends are significant, and the trend shows that there is a slight increase in years of primary education from 1980 to 1987, and a steeper decline thereafter. Interestingly, when we combine birth cohorts and UPE in column 4, UPE becomes insignificant. We next include the interaction term UPE*Year in our most rigorous specification, which yields an insignificant effect of UPE on years of primary school (column 5). This confirms our finding above that UPE did not increase years of primary schooling.

Table 6. Dependent variable: Years of primary education

	(1)	(2)	(3)	(4)	**(5)**	(6)	(7)
UPE, treatment =1	-11.11*	-0.188***		0.138	**-11.99**	-16.25***	-13.47***
	(6.409)	(0.0489)		(0.0935)	**(7.374)**	(5.339)	(3.815)
Year	1.837**		0.689***	0.701***	**2.003****	2.429***	2.375***
	(0.742)		(0.252)	(0.253)	**(0.861)**	(0.586)	(0.413)
Year^2	-0.0109**		-0.00402***	-0.00417***	**-0.0118****	-0.0143***	-0.0138***
	(0.00435)		(0.00140)	(0.00142)	**(0.00504)**	(0.00345)	(0.00243)
UPE*Year	0.126*				**0.136**	0.183***	0.151***
	(0.0716)				**(0.0824)**	(0.0597)	(0.0427)
Gender, male=1	0.0183				**0.123****	0.152***	0.111***
	(0.0480)				**(0.0528)**	(0.0410)	(0.0289)
Household fixed effects	NO	YES	YES	YES	***YES***	YES	NO
Rural=1, Urban=0	-0.499***						-0.522***
	(0.0603)						(0.0346)
Region: East	-0.0968						-0.149***
	(0.0745)						(0.0442)
Region: North	-0.349***						-0.484***
	(0.0909)						(0.0496)
Region: West	-0.184**						-0.338***
	(0.0881)						(0.0485)
Constant	-70.57**	6.309***	-23.15**	-23.15**	**-78.42****	-97.28***	-95.36***
	(31.66)	(0.0270)	(11.25)	(11.25)	**(36.74)**	(24.93)	(17.52)
Observations	3251	3251	3251	3251	**3251**	10163	10163
R^2	0.062	0.467	0.474	0.475	**0.477**	0.586	0.063
Adjusted R^2	0.059	0.290	0.299	0.300	**0.303**	0.318	0.062

Note: Standard errors are in parentheses and * p < .10, ** p < 0.05, *** p < 0.01. The reference category for region is the Central Region, which includes Kampala.

A sensitivity analysis shifting the treatment threshold to 1990 and 1991 (included in Appendix A-4) showed comparable results to our main specification. We also varied the age threshold of our sample composition, as in the main analysis. The results, presented in Appendix A-5, are consistent with our main estimate

[7] Civil unrest starting in the early-1980s gave rise to armed insurgency groups called the Lord's Resistance Army and the Allied Democratic Forces, which not only killed many people but also abducted children. Among the people recruited to the rebel groups for at least one day, 65% were children in the birth cohorts 1976 to 1992, estimated to number 66,000 (Annan, Blattman, & Horton, 2006).

when we set the age threshold to children 15 and above. When we set the age threshold too low (young), at 13 or 14 years, UPE had a significant effect. Setting the threshold too high (old) is also problematic because we then exclude children who may have also benefited from UPE but left the household before 17 or 18, not to mention the decrease in the sample size.

We also estimate the probability of completing primary school and starting secondary school. We use a linear probability model. We find some evidence that UPE increased the likelihood to complete primary school, while it has no significant effect on starting secondary school. But these parametric results have to be treated with caution as the projected effects are quite sensitive to the parametric specification.[8]

In addition to the above robustness checks, it is perhaps important to note that an analysis of the effect of eliminating tuition by income groups would be valuable to gain a more nuanced understanding of fee elimination. Because public schools in poorer regions of Uganda were more dependent on school fees, the effect of lifting school fees on school quality may be more acute for the poor (Grogan, 2008). At the same time, overcrowding in schools after the policy may have also pushed the poorest income groups out of school (Kattan & Burnett, 2004). This is because the poor have the least resources to cope with reductions in education quality, and moreover, if quality is poor, families may opt to employ children at home rather than send them to school. Our study does not untangle these factors to address different income levels. Due to data limitations, we are not able to measure household income at the time the person was enrolled in school. For example, applying income data from 2010 for a 30-year-old tells us little about the role their current household income played in their primary school attainment. One possible method to distinguish the effect of eliminating tuition on different income groups is to study the differences between children attending public school versus private school because in Uganda, the type of school one attends is a strong predictor of income (Grogan, 2008).

An analysis that considers public vs. private schooling may also help identify some spillover effects, such as middle-income children moving to private schools due to overcrowding in the public-school system, which may have a downward bias on the educational attainment of lower-income or public-school students. Despite these reasons, we do not distinguish school type in this paper for two reasons. First, our interest in educational attainment is for all children, including those in private school. Moreover, the majority of children in Uganda attend public schools, at least at the primary level. For example, in the sample of children surveyed in 2012, among students currently attending primary school, 73.4% attended public schools and in 2005, 78.9% attended public schools. The shift to private schools is more frequent at the secondary level. Second, spillover effects may play less of a role in Uganda, where the choice between enrollment at all, enrollment in a public school, and enrollment in a private school tends to be made at once rather than first deciding whether to enroll or not and then deciding to go to a public or to private school (Steiner, 2010).

6. Conclusions and Caveats

For the Ugandan Government, UPE represents a costly commitment to improve access to education and reduce national poverty through education. Monitoring the effect of this investment requires evidence-based data. Our study provides a quasi-experimental impact evaluation of fee elimination on educational attainment, and we find that the intervention had little effect on educational attainment. Lifting school fees had no direct effect on years of completed primary school and did not increase the likelihood to complete primary education. We find weak evidence that UPE had an effect on the likelihood of secondary transition.

[8] Results are available on request.

And we find some evidence that it helped at least some children move through the school system faster (see our results on children 13+).

There are several likely explanations for the lack of effect of UPE on years of primary school attainment. First, the increase in enrollment when UPE was enacted overwhelmed the existing education system and infrastructure in terms of resource availability, capacity, and teachers, with respect to both quality and quantity (Kattan & Burnett, 2004; Mulkeen & Chen, 2008). This crowding due to UPE came on top of the effect of rapid population growth which increased the cohorts of school-age populations by 3-4% per year. This could potentially lead to reduced quality—through the lack of teachers and facilities and high student-teacher ratios—lowering outcomes for all students. Second, overcrowding may have pushed some students out of school, or resulted in parents putting their children to work at home. Third, the remaining cost of education (e.g. uniforms, meals, writing utensils, transportation) may be high enough to pull children back out of school as they get older. Further research in this area is needed to better understand why UPE has not had its intended effect.

Of course, UPE still had the effect of reducing schooling costs, freeing scarce household resources. Since most children were already attending primary school, this was a windfall gain to these parents. For most this did not, however, affect schooling investments. But the lower cost may be one reason some children are now moving through the school system faster.

Our study questions the mid-term effect of lifting primary school fees and suggests that fee elimination alone cannot overcome the challenges of educational attainment. Given the tremendous cost of the policy, it would behoove stakeholders to acquire a nuanced understanding of its effects, and also investigate the effect on the poor, the effect on quality of education, and on progression to secondary school.

Note: There is no conflict of interest

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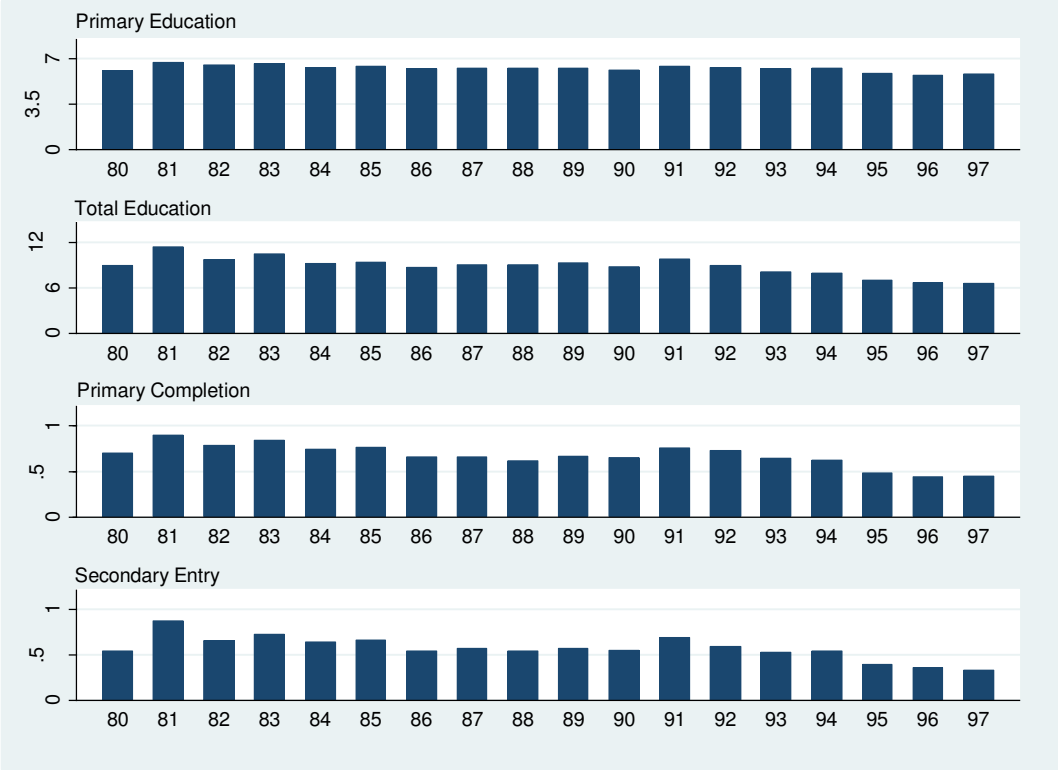
Appendix

Appendix A-1. Visual aide of enrollment by age

		1997	1998	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011
Control (birth cohort)	1980	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31
	1981	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30
	1982	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29
	1983	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28
	1984	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27
	1985	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26
	1986	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25
	1987	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24
	1988	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23
	1989	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22
Treatment (birth cohort)	1990	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21
	1991	**6**	7	8	9	10	11	12	13	14	15	16	17	18	19	20
	1992		6	7	8	9	10	11	12	13	14	15	16	17	18	19
	1993			6	7	8	9	10	11	12	13	14	15	16	17	18
	1994				6	7	8	9	10	11	12	13	14	15	16	17
	1995					6	7	8	9	10	11	12	13	14	15	16
	1996						6	7	8	9	10	11	12	13	14	15
	1997							6	7	8	9	10	11	12	13	14

Note: The first column represents year of birth, and in the rows, age in the given year. A child born in 1991 is six years old in 1997 (corresponding to grade 1). The table assumes age-appropriate progression, defined as entering primary school at the age of 6 without repeating grade levels.

Appendix A-2. Dependent variables plotted by cohort



Note: From top down, in the first panel, mean years of completed primary school, mean years of total education, rate of primary school completion, rate of transition to secondary school.

Appendix A-3. Dep: Years of primary education, with HH dummies

	(1)	(2)	(3)	(4)	(5)	(6)
	13+	14+	15+	16+	17+	18+
1980	0.451**	0.00756	-0.137	-0.194	-0.477	-0.402
	(0.227)	(0.254)	(0.262)	(0.289)	(0.309)	(0.341)
1981	0.940***	0.541***	0.370**	0.400**	0.212	0.137
	(0.159)	(0.158)	(0.172)	(0.160)	(0.168)	(0.175)
1982	0.711***	0.257	-0.0320	-0.0587	-0.310	-0.444**
	(0.176)	(0.175)	(0.175)	(0.187)	(0.192)	(0.208)
1983	0.798***	0.376**	0.165	0.136	-0.0864	-0.123
	(0.149)	(0.150)	(0.161)	(0.170)	(0.169)	(0.191)
1984	0.833***	0.412***	0.147	0.171	-0.143	-0.135
	(0.150)	(0.150)	(0.156)	(0.164)	(0.179)	(0.191)
1985	0.852***	0.445***	0.116	0.0218	-0.216	-0.283
	(0.132)	(0.137)	(0.140)	(0.153)	(0.174)	(0.196)
1986	0.774***	0.353***	0.0789	0.105	-0.158	-0.268*
	(0.124)	(0.127)	(0.132)	(0.143)	(0.149)	(0.162)
1987	0.697***	0.300***	0.0110	0.0110	-0.251*	-0.326**
	(0.105)	(0.110)	(0.113)	(0.120)	(0.130)	(0.139)
1988	0.708***	0.284**	-0.00345	0.0310	-0.235*	-0.179
	(0.109)	(0.114)	(0.120)	(0.127)	(0.139)	(0.157)
1990	0.586***	0.168	-0.102	0.313**	0.0380	-0.00784
	(0.120)	(0.125)	(0.128)	(0.136)	(0.140)	(0.151)
1991	0.265**	-0.111	0.0528	0.0494	-0.206	-0.245
	(0.125)	(0.126)	(0.131)	(0.133)	(0.141)	(0.151)
1992	0.243*	0.300**	0.0380	0.0693	-0.237	-0.258
	(0.131)	(0.130)	(0.133)	(0.138)	(0.148)	(0.159)
1993	0.623***	0.214*	-0.0227	-0.0389	-0.241*	-0.254*
	(0.128)	(0.126)	(0.128)	(0.135)	(0.133)	(0.152)
1994	0.718***	0.336***	0.0655	0.0960	-0.201	-0.0601
	(0.105)	(0.114)	(0.114)	(0.122)	(0.136)	(0.157)
1995	0.178	-0.217*	-0.431***	-0.364***	-0.527***	
	(0.115)	(0.123)	(0.126)	(0.135)	(0.172)	
1996	0.119	-0.195*	-0.439***	-0.174		
	(0.105)	(0.116)	(0.119)	(0.140)		
1997	-0.160	-0.457***	-0.405***			
	(0.120)	(0.132)	(0.146)			
Gender	0.161***	0.113**	0.123**	0.103*	0.114*	0.143**
	(0.0507)	(0.0509)	(0.0530)	(0.0564)	(0.0625)	(0.0694)
Constant	5.473***	5.912***	6.199***	6.242***	6.530***	6.580***
	(0.0656)	(0.0731)	(0.0788)	(0.0850)	(0.0964)	(0.104)
Observations	4082	3657	3251	2752	2219	1738
R^2	0.479	0.488	0.482	0.483	0.503	0.520
Adjusted R^2	0.324	0.325	0.306	0.289	0.297	0.301

Note: Standard errors are in parentheses and * p < .10, ** p < 0.05, *** p < 0.01.

Appendix A-4. Dependent variable: years of primary education, with household fixed effects, varying cohort thresholds for the treatment group

	(1) 1986	(2) 1987	(3) 1988	(4) 1989	**(5) 1990**	(6) 1991
UPE, treatment=1	-3.490	-8.544	-12.32*	-21.67***	**-11.99**	-5.652
	(6.637)	(6.225)	(6.654)	(7.344)	**(7.374)**	(6.925)
Year	1.096	1.555**	2.050**	2.946***	**2.003****	1.524*
	(0.666)	(0.691)	(0.797)	(0.861)	**(0.861)**	(0.786)
Year^2	-0.00638	-0.00918**	-0.0121**	-0.0174***	**-0.0118****	-0.00903**
	(0.00401)	(0.00412)	(0.00471)	(0.00507)	**(0.00504)**	(0.00458)
UPE*Year	0.0386	0.0966	0.139*	0.244***	**0.136**	0.0662
	(0.0774)	(0.0718)	(0.0757)	(0.0828)	**(0.0824)**	(0.0767)
Gender, male=1	0.0944	0.127**	0.119**	0.115**	**0.123****	0.103**
	(0.0620)	(0.0601)	(0.0564)	(0.0550)	**(0.0528)**	(0.0523)
Constant	-40.67	-59.48**	-80.39**	-118.0***	**-78.42****	-58.05*
	(27.70)	(29.03)	(33.69)	(36.55)	**(36.74)**	(33.76)
Observations	1905	2305	2677	2951	**3251**	3436
R^2	0.498	0.489	0.495	0.486	**0.477**	0.480
Adjusted R^2	0.340	0.326	0.329	0.317	**0.303**	0.304

Note: Standard errors are in parentheses and * p < .10, ** p < 0.05, *** p < 0.01.

Effect of UPE, taking interaction terms into account by year (corresponding to above table)

			Year of birth									
Col.	UPE (β)	UPE*Year (β)	88	89	90	91	92	93	94	95	96	97
(1)	-3.49	0.04	-0.09	-0.05	-0.02	0.02	0.06	0.10	0.14	0.18	0.22	0.25
(2)	-8.54	0.10	-0.04	0.05	0.15	0.25	0.34	0.44	0.54	0.63	0.73	0.83
(3)	-12.32*	0.139*	-0.09	0.05	0.19	0.33	0.47	0.61	0.75	0.89	1.02	1.16
(4)	-21.67***	0.244***	-0.20	0.05	0.29	0.53	0.78	1.02	1.27	1.51	1.75	2.00
(5)	-11.99	0.14	-0.02	0.11	0.25	0.39	0.52	0.66	0.79	0.93	1.07	1.20
(6)	-5.65	0.07	0.17	0.24	0.31	0.37	0.44	0.50	0.57	0.64	0.70	0.77

Note: this table presents the estimates of the UPE variable, taking the interaction term into consideration. They are based on the coefficients from the regression in Appendix A-4.

Appendix A-5. Dependent variable: Years of primary education (with household fixed effects), by different age threshold used to define sample

	(1)	(2)	**(3)**	(4)	(5)	(6)
	Age 13+	Age 14+	**Age 15+**	Age 16+	Age 17+	Age 18+
UPE, treatment=1	-24.60***	-22.06***	**-11.99**	1.147	9.621	14.65
	(6.174)	(6.928)	**(7.374)**	(8.268)	(9.202)	(9.890)
Year	3.457***	3.204***	**2.003****	0.736	-0.208	-1.481
	(0.641)	(0.765)	**(0.861)**	(1.034)	(1.236)	(1.382)
Year^2	-0.0204***	-0.0189***	**-0.0118****	-0.00435	0.00131	0.00883
	(0.00376)	(0.00448)	**(0.00504)**	(0.00605)	(0.00723)	(0.00809)
UPE*Year	0.275***	0.247***	**0.136**	-0.00981	-0.106	-0.164
	(0.0688)	(0.0773)	**(0.0824)**	(0.0925)	(0.103)	(0.111)
Gender, male=1	0.150***	0.109**	**0.123****	0.0997*	0.111*	0.145**
	(0.0506)	(0.0509)	**(0.0528)**	(0.0568)	(0.0628)	(0.0691)
Constant	-140.3***	-129.8***	**-78.42****	-24.80	14.61	68.41
	(27.33)	(32.63)	**(36.74)**	(44.15)	(52.72)	(58.97)
Observations	4082	3657	**3251**	2752	2219	1738
R^2	0.490	0.488	**0.477**	0.478	0.498	0.513
Adjusted R^2	0.341	0.328	**0.303**	0.286	0.294	0.297

Note: Standard errors are in parentheses and * p < .10, ** p < 0.05, *** p < 0.01.

Effect of UPE, taking interaction terms into account (corresponding to above table)

			Year of birth									
Col.	UPE (β)	UPE*Year (β)	88	89	90	91	92	93	94	95	96	97
(1)	-24.60***	0.275***	-0.40	-0.13	0.15	0.43	0.70	0.98	1.25	1.53	1.80	2.08
(2)	-22.06***	0.247***	-0.32	-0.08	0.17	0.42	0.66	0.91	1.16	1.41	1.65	1.90
(3)	-11.99	0.14	-0.02	0.11	0.25	0.39	0.52	0.66	0.79	0.93	1.07	1.20
(4)	1.15	-0.01	0.28	0.27	0.26	0.25	0.24	0.23	0.22	0.22	0.21	0.20
(5)	9.62	-0.11	0.29	0.19	0.08	-0.02	-0.13	-0.24	-0.34	-0.45	-0.56	-0.66
(6)	14.65	-0.16	0.22	0.05	-0.11	-0.27	-0.44	-0.60	-0.77	-0.93	-1.09	-1.26

Note: this table presents the estimates of the UPE variable, taking the interaction term into consideration. They are based on the coefficients from the regression in Appendix A-5.