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Evaluating universal primary education 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, 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 Universal Primary Education.

KEYWORDS

international education development, school fees, Uganda, 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 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% (United Nations, 2015). A key driver in increasing enrollment rates was likely the adoption of UPE in many African countries and lifting primary school fees 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 many studies examining the impact of UPE on enrollment 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, referred to as free primary education (FPE), and educational attainment in Uganda. It is important to note that while FPE was the main component of UPE, there were other elements of UPE such as greater budget allocation to the education sector, from 13.7% of the national budget in 1990 to 24.7% by 1998, and an increase in the number of primary schools and teachers, from 8,531 schools in 1996 to 13,353 in 2003, and 81,564 primary teachers in 1996 to 145,587 in 2003 (Bategeka & Okurut, 2006). We do not disentangle the effects of the components, due to limitations in scope and data availability. However, our focus is on FPE as we believe that this likely had the most dramatic effect on a household's decision to send their children to school. However, because we cannot disentangle the effects of different elements of UPE, to be precise, our analysis also likely captures other components of UPE.

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 born between 1980 and 1997, controlling for household fixed effects. We find that, surprisingly, lifting primary school fees had a generally insignificant effect on educational attainment. Compared with older cohorts, younger cohorts who were in school after UPE was enacted did not complete more years of primary schooling and were not more likely to complete primary school. We do, however, find weak evidence that younger cohorts were more likely to transition to secondary school, conditional on having successfully completed primary education. A possible explanation is that the elimination of primary school fees may have changed the composition of students who complete primary school and therefore become candidates for secondary school.

Our main contribution is to 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, thereby moving the discourse beyond enrollment rates. While we know that 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, not enough time had passed to analyze the effect of the intervention.¹ Now, with sufficient data and enough time since UPE was enacted, we can better analyze UPE's impact and find that it had a limited and insignificant effect on educational attainment. This has important implications for various policy stakeholders, especially given the enormous expense and undertaking of such interventions.

In terms of methodology, to analyze the effect of UPE, 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. Therefore, we eliminate the possibility, for instance, that later cohort households behave differently due to an unobserved difference from earlier cohorts rather than because of UPE.

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 how we use the data for our analysis: by constructing 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, we present the results and robustness checks. In Section 6, we then conclude by summarizing policy implications and areas for future research.

2 | BACKGROUND ON UPE AND FPE IN UGANDA

The main goal of UPE is to ensure that every child enters and completes primary school, thereby reducing inequities in education and eventually reducing poverty. The term UPE, therefore, is a political initiative and an umbrella term for several policies targeted at improving primary school access and quality. In Uganda, UPE included not only FPE but also greater investments in the education sector as a whole, including building more schools and hiring more teachers (Bategeka & Okurut, 2006).

In 1997, as part of the UPE agenda, the government of Uganda formally abolished primary school tuition, Parents and Teachers Association fees, and textbook fees for up to four children per family.² This no-fee policy was rolled out across all primary school grades, rather than being introduced cohort-by-cohort. UPE was viewed as an important tool to bring children into the school system. For parents, the financial burden of enrolling their children in school was not trivial as they were expected to finance around 60% of the costs (Kattan & Burnett, 2004), and the burden was large enough to have a direct and negative effect on enrollment rates, the pupil–teacher ratio, and primary completion rates (Işcan, Rosenblum, & Tinker, 2015). Therefore, school fees not only drove down demand but arguably led to less government ownership of the education system.

As mentioned earlier, many countries implemented UPE in the 1990s and 2000s. Most UPE policies have been implemented over the past 30 years albeit at different points of time and in different ways. For example, Malawi abolished school fees for grade 1 students starting in 1991 and then for all primary students in 1994. In Lesotho, UPE was offered starting with grade 1 students in 2000. In Kenya, UPE was rolled out for all students in 2003, in the same manner as in Uganda.

Once UPE was introduced, the policy led to a surge in enrollment rates in most countries, which often overwhelmed and exceeded the capacity of the education system. The severe supply-side constraints such as a shortage of teachers, schools, and school materials led to overcrowding (Avenstrup, Liang, & Nellemann, 2004; Bategeka & Okurut, 2006; Oketch & Rolleston, 2007). In Uganda, the student–teacher ratio for primary school jumped from 37.6 in 1996 to 59.4 in 1997 (World Bank, 2014). Dropout rates also increased and survival rates sunk from 59% to 37% (World Bank, 2014) despite the fact that automatic grade promotion was implemented to offset the large number of students moving through the system after UPE (Al-Samarrai, 2003). While automatic promotion or what was sometimes referred to as "social promotion" was widespread (Muhwezi, 2003; Okuni, 2003; World Bank, 2004), it was not considered official government policy (Uganda Ministry of Finance Planning and Economic Development, 2002), until formally outlined in 2005 (Okurut, 2015).

The supply-side constraint stemmed not only from a lack of capacity (e.g., the availability of trained teachers) but also from a lack of funding; for example, Kenya, Lesotho, Malawi, and Uganda adopted UPE in the middle of their fiscal years but the budget did not include sufficient funds to meet increased demand. Uganda's education expenditure, as a percentage of gross domestic product (GDP), was relatively low compared to countries such as Malawi and Botswana, starting at 2.9% in 1995–1996 and increasing to 4.7% in 1998–1999 (Al-Samarrai, 2003). To finance the gap, Uganda, as with many other countries, relied heavily on donor assistance. An estimated 50% of the education sector budget was funded by donors through technical assistance, grants, loans, and general budget support (Riddell, 2004).

Despite the increase in government funding and donor assistance, supply-side constraints remained, and coupled with the influx of older students, found to have had a negative effect on learning outcomes. A study by Al-Samarrai (2003) shows that, in Botswana, Malawi, and Uganda, learning outcomes after UPE in both numeracy and literacy deteriorated. Existing literature shows that the effects of UPE on learning outcomes and graduation rates have been mixed. Cross-country analyses (Al-Samarrai, 2003, 2006) show that cost and expenditure do not correlate with educational outcomes, enrollment does not lead to attendance and retention, and results have been largely unique to each country. In Nigeria, determinants of educational outcomes include a host of characteristics such as income and school quality, beyond UPE and government expenditure (Lincove, 2009). In Malawi, Kadzamira and Rose (2003) find that additional non-tuition-related costs to households (e.g., uniforms, or better quality uniforms as students progress through school, and learning materials) and opportunity costs of forgoing income-generating activities continued to make education cost-prohibitive for many after UPE.

In Uganda, primary education consists of 7 years, followed by 4 years of lower secondary school, and 2 years of upper secondary school. The official primary school enrollment age is 6 years, but many children start school later, delaying enrollment (Moyi, 2011). When tuition was lifted, within the first year, the primary school gross enrollment rate³ in Uganda increased from 71% to 125% and peaked at 138% in 2003 (see Figure 1 and Appendix Figure A2, World Bank, 2019), largely because many older children (relative to the official enrollment age) entered or reentered primary school (Al-Samarrai, 2003). Consequently, the school composition changed dramatically, and by 2003, 25% of primary students were grade 1 students (Oketch & Rolleston, 2007). Delayed enrollment is problematic for many reasons, such as making teaching difficult for both the teacher and other classmates,



FIGURE 1 Gross intake and enrollment ratios (primary). *Source*: World Bank (2014). 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 [Colour figure can be viewed at wileyonlinelibrary.com]

but also because it correlates with higher dropout rates (Wils, 2004). In the first years after UPE, the dropout rate also increased to around 20%, and usually occurred after the completion of only grade 1 or grade 2. Of the students who enrolled in grade 1 in 1997, only 23% reached grade 7 by 2003 (Bategeka & Okurut, 2006).

Table 1 shows a cross section of students from the 2013–2014 Uganda National Household Survey (UNHS). It illustrates the challenges of age-inappropriate enrollment, showing the percentage of children enrolled in each 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 cells that correspond to the official enrollment age are shaded. For example, by the final year of primary school (grade 7), only 14% of students are of official enrollment age, with many children as much as 4 years behind. Many students are in the 15–21 age category, which is most notable at grade 6 and higher. Overall the table shows that a large percentage of enrolled children are generally older than the official enrollment age.

We can also observe the general scale of age-inappropriate enrollment by examining the gross enrollment ratio and gross 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).⁴ Figure 1 shows that the gross enrollment ratio peaked after 1996 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 6 years. Remarkably, the gross intake ratio falls after 1998 but remains far above 1 until after 2010, suggesting that delayed enrollment into primary school persists and/or many children enrolled in first grade as repeaters. This is consistent with the descriptive statistics in Table 1, which shows that age-inappropriate enrollment is high, where many children are much older than the official age for the given grade level.

Enrollment into lower secondary school is dependent on passing the primary school leaving exam. The exam pass rates from 1999 to 2016 oscillated around 80%–88% and were closer to 80% when the first UPE cohorts took the exam, providing some further evidence that quality of schooling might have dropped. But it is important to note that exam pass rates are much higher than the transition rates to secondary school (Ssewanyana, Geofrey, & Kasiryelbrahim, 2011) so that passing the exam does not appear to be a binding constraint for transitioning to secondary school. Figure 2 shows the gross intake ratio of students entering the first year of secondary school (World Bank, 2019). The rates were 13.4 in 1995, 25.2 in 2000, and 27.9 in 2002. The gross intake ratio had a sharp increase to 35.6 in 2007 and peaked at 38 in 2010. Since then, it has been relatively constant at a ratio of around 35.

The existing literature on the impact of lifting primary school fees in Uganda is relatively small, and outcomes are mostly measured using enrollment rates. Our search led to five key studies that examine this question. In the following text, we summarize the relevant findings of these studies with respect to our own research agenda. The first and 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% 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 timeable 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 enacted only 3 years prior to the study.

| | Grade | e level | | - | 0 | | - | 0 | | | | | | |
|---|--------------------------|---|--|---|---|---|---|--|---|--|--|---|---|--------------------------------------|
| Å ge | Pre.1 | - | ~ | 3 | 4 | v | 9 | - | × | 0 | 10 | = | 2 | 13 |
| 2941 | | • | 1 | • | • | , | • | | 5 | | 2 | 1 | 1 | 3 |
| 4 | 14 | | | | | | | | | | | | | |
| 5 | 27 | 5 | | | | | | | | | | | | |
| 9 | 28 | 15 | б | | | | | | | | | | | |
| 7 | 17 | 27 | 11 | 1 | | | | | | | | | | |
| 8 | 8 | 23 | 21 | 7 | 1 | | | | | | | | | |
| 6 | С | 16 | 24 | 14 | 5 | | | | | | | | | |
| 10 | 1 | 9 | 18 | 23 | 15 | 4 | 1 | | | | | | | |
| 11 | | 3 | 12 | 20 | 16 | 10 | 3 | | | | | | | |
| 12 | | 1 | 9 | 14 | 19 | 16 | 11 | 5 | 1 | | | | | |
| 13 | | 1 | 2 | 10 | 18 | 17 | 13 | 6 | 7 | | | | | 1 |
| 14 | | | 1 | 9 | 13 | 22 | 19 | 14 | 15 | 8 | 1 | | | |
| 15 | | | 1 | 2 | 7 | 14 | 19 | 16 | 19 | 14 | 7 | 2 | | |
| 16 | | | | 1 | 4 | 9 | 15 | 17 | 18 | 22 | 14 | 12 | 4 | 1 |
| 17 | | | 1 | | 1 | 9 | 6 | 14 | 16 | 19 | 22 | 13 | 14 | 5 |
| 18 | | | | | 1 | 3 | 9 | 11 | 14 | 16 | 21 | 21 | 20 | 15 |
| 19 | | | | | | 1 | 3 | 8 | 5 | 11 | 16 | 19 | 20 | 30 |
| 20 | | | | | | | 1 | 4 | 3 | 9 | 13 | 19 | 27 | 25 |
| 21 | | | | | | | | 2 | ю | 3 | 9 | 14 | 15 | 23 |
| и | 822 | 880 | 781 | 772 | 730 | 663 | 575 | 410 | 222 | 245 | 210 | 197 | 74 | 81 |
| <i>Notes</i> : The enrollment were survey | age of each ed toward | Now the perce a grade level in the end of the | entage of chil is highlighted le year (the U | dren by age, fc 1, where two nu gandan school | or the given gr ambers are inc year is from I | rade level (e.g cluded as the (February to D | ., in column 3, official enrollm ecember). Pre- | which is grade nent age to acc 1 represents ki | o 1, 42% of chi ount for being ndergarten and | ldren are of off 6 when the gra 1 all school pro | icial age, bein de 1 school ye grams before _i | g 6–7 years ol ar began but t grade 1. The la | d). The offi urning 7 by tst row show | cial the time they s the total |

Source: Own calculations based on the 2013–2014 Uganda National Panel Surveys for all students currently attending school, aged 21 years and younger.



FIGURE 2 Gross intake ratio (grade 1 of lower secondary). *Source*: World Bank (2019). The gross intake ratio is the number of all students in grade 1 of lower secondary school (grade 8), relative to the number of official school-entrance age students. Blank spaces represent missing data for the respective year

The second study, by Nishimura et al. (2008), investigates the relationship between UPE and enrollment rates. The authors use a data set collected in 2003, 6 years after the intervention, allowing for almost one full cohort (7 years) to move through primary school. The study finds that the intervention significantly reduced delayed enrollment (defined as starting primary school later than 6 years of age) and 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 6) and, whether a child completed school by grades 4 or 5, by gender. The data used for the analysis cover 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.

A third study by Grogan (2008) investigated delayed enrollment and found that the 1988–1992 birth cohorts (in 1997, respectively, ages 5–9) were more likely to enter primary school before the age of 9, compared with children born in 1982–1987, 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. The fourth study (Essama-Nssah et al., 2008) employed a difference-in-differences estimation strategy to measure the impact of the 1997 policy on enrollment using repeated cross-sectional data for 1992 and 2005. They found that reduced fees increased enrollment as well as age-appropriate entry. The final study (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, remaining costs present a barrier for some families.

In addition to the aforementioned studies, several additional studies also indirectly examine UPE and educational achievement. Behrman (2015a, 2015b) and Omoeva and Moussa (2018) use UPE as an instrument for years of primary school attendance to measure the effect of years of primary school on other variables such as adult women's human immunodeficiency virus (HIV) status and desired fertility and labor outcomes. Therefore, their first stage analysis measures the effect of UPE on years of primary education. Behrman (2015a, 2015b) studies only females and finds a positive effect of UPE. Omoeva and Moussa (2018) find a negative effect on males and mixed results for females. In contrast to these papers, we create a data set to enable us to control for household characteristics using household fixed effects. We construct our data set by combining five waves of data, whereas they use one wave. We also implement various robustness checks since the effect on attainment is the focus of our paper, whereas it is secondary for these papers. We also go beyond years of educational attainment and analyze the effect of UPE on the completion of primary school and the secondary school enrollment. Finally, in a global study, Bhalotra et al. (2015) address the effect of lifting tuition on educational attainment across 67 countries and approximately 1 million children between the ages of 15 and 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. To expand our understanding of the effect of lifting school fees, we add a nationally representative analysis of attainment to the regional and enrollment-focused studies, taking duration and completion into account; that is, do they stay in school and for how long?

In this paper, we focus on one country to gain a nuanced understanding of lifting school fees, which is particularly important given the high cost of such interventions 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), especially for developing countries (Baldacci, Clements, Gupta, & Cui, 2008; Hanushek, 2013). A widely-accepted theory is that investments in education can generate economic growth through an increase in productivity that translates into higher wages (Duflo, 2001; Psacharopoulos & Patrinos, 2004). This notion is rooted in human capital theory, which argues that education leads to greater productivity, and subsequently, an increase in wages. However, the magnitude of the rate of return to education 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. 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 an 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 five waves of panel data from the UNHS and the Uganda National Panel Survey (UNPS). The surveys were conducted by the Uganda Bureau of Statistics and cover a wide range of socioeconomic topics such as education, health, agricultural activities, housing, energy sources, employment, sanitation, livestock, assets, and consumption. The surveys include questions at the individual, household, and community levels. All surveys are nationally representative household surveys using a stratified random sample design, with enumeration areas as the sampling units. For the 2005–2006 UNHS survey, the sample was constructed in two stages. In the first, 783 enumeration areas were selected based on the probability proportional to size sampling method (and using the Uganda Population and Housing Census Frame for 2002). Then, households were drawn from the enumeration areas using the simple random sampling method. The UNPS surveys then selected 322 enumeration areas from the 2005–2006 UNHS survey with equal probability stratified by urban and rural areas and districts. The strata include Kampala City, other urban areas, Central Rural, Eastern Rural, Western Rural, and Northern Rural. Areas that were oversampled by UNHS, such as 10 rural districts, were then not included in the UNPS (Uganda Bureau of Statistics, 2010).

In 2005–2006, the UNHS survey covered 3,123 households. The UNPS then reinterviewed 2,975 households in 2009–2010. This was followed by reinterviewing 2,716 households in 2010–2011, 2,850 households in 2011–2012 (additional households were added from the 2012 Uganda Population and Housing Census), and 3,119 households in 2013–2014. Surveys were administered over a 12-month period, and each household was visited twice within the survey period to account for responses affected by seasonality (Uganda Bureau of Statistics, 2010, 2012, 2014).

For our analysis, we pooled all five waves. We then constructed our data set first by including only individuals from the 2013–2014 survey. Next, we worked backward, 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 at only one point in time in our final data set. 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.

To ensure that our sample only includes children who had ample time to complete primary school, we exclude children who were under 15 years of age at the time they were captured in a given survey wave, in either 2005–2006, 2009–2010, 2010–2011, 2011–2012, or 2013–2014. This also demonstrates the advantage of using five waves of the survey in that we are able to keep many more children in our data set. For example, a child could be 9 years old in the first wave (2005–2006), 15 years old in the fourth one (2011–2012) and have left the household by the fifth wave (2013–2014). Due to this method of constructing our data set, that child (who was not in the fifth wave and too young in the first three waves) would now be captured in our data set. For children of age 15 years and older, we expect that they have had sufficient time to complete primary school. While a more restricted 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 of age 15 and older, 2 years greater than the official enrollment age of someone who completed grade 7. Age-inappropriate schooling remains a problem, but since the reform improved age-appropriate schooling (Essama-Nssah et al., 2008; 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 being children, grandchildren, stepchildren, and nephews and nieces of the head. 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, that is, at least one child must have been young enough to be fully exposed to UPE and one child must be old enough to have not been fully exposed to UPE. This way, we are exploiting the within-household variation of exposure to UPE, controlling for other unobserved household characteristics.

To specify the criteria for pre- and post-UPE, we define the post-UPE treatment group as cohorts who could benefit from the full 7 years of free primary school and the control group as those who received fewer years or none. A visual tool is provided in Appendix Table A1, where the first column represents the year of birth and the rows show age in the given year. For example, a child born in 1991 is 6 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 1 year, but not more, which we consider a reasonable threshold given that UPE had a significant and positive effect on age-appropriate enrollment (see Grogan, 2008). Therefore, our pre-UPE control group birth cohorts are 1980–1989 (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. We will vary these cutoffs to deal with delayed enrollment and people who were partially exposed to UPE in robustness checks shown later.

After applying these criteria, we reduce the sample to 3,251 children in 809 households. We believe that this is still a large number of households relative to the total number of around 3,000 households, given our demanding restrictions to be able to use household fixed effects.

Table 2 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

| | Mean | | | | | | | | | | | | | | | | | |
|---|--|---|---|---|---|---|---|---|--|---|--|------------------------------------|------------------------------------|--|--------------------------------------|---------------------------------------|---------------------------------------|------------------------|
| | 80 | 81 | 82 | 83 | 84 | 85 | 86 | 87 | 88 | 89 | 90 | 91 | 92 | 93 | 94 | 95 | 96 | 97 |
| Gender (male = 1) | 09.0 | 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 education | 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 education | 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 |
| <i>Source:</i> Own calculation: totals 3,251 people. For th and then keep only house variable on having compl- | s based or he sample holds with eted prim | n the 2005- e of student h two child ary school. | -2006 Uga s who pro lren, one b . Therefon | unda Natior gress to ser orn in the J e, we remo | nal Househ condary sc pretreatme we all obse | iold Surve shool entr- ant and the ervations t | ey and the y, we use 1 s other bor that have r | 2009–20. the same s in the p tot compl | 10, 2010–7 selection n osttreatme eted a prir | 2011, 201 nethod as ent cohort nary scho | 1–2012, a in the ma . We then ol degree | in 2013– in sample condition | 2014 Uga (and ther the consi | nda Natiol efore lose ruction of | nal Panel data for t the secor | Surveys. 7 he 1996–1 ıdary scho | The final s 997 coho ol entry b | ample tts) inary |

TABLE 2 Descriptive statistics of the control and treatment groups, by birth cohort

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the average years of primary school is slightly more than 6. Thus, many children were already close to completing primary education, suggesting that lifting user fees mainly improves age-appropriate enrollment and lowers the cost of education to parents who already sent their children to school, with the potential to boost schooling years. 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 enrollment, these children require more time to complete primary school and should therefore 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 Figure A1.

4 | METHODOLOGY

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

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

where subscript *i* represents individual *i*. *Ed* represents years of completed primary school ranging from 0 to 7. *Year* is a dummy variable that takes the value of 1 if individual *i* is born in year *t*, and 0 otherwise. Appendix Table A1 shows a table of student enrollment by age. Cohort 1991 is the first to be officially treated, but because a large number of children delay their enrollment, 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 included only 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 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 due to the incidental parameter problem. Therefore, we use a linear probability model to study the effect of UPE. In specifications without fixed effects, we compare the results from our linear probability model with those of a probit model and find them to be extremely similar (results available on request). 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.

Our stated specifications address mainly the substitution effect of UPE as this makes the price of primary school attendance lower. But it could be also argued that our within-household comparison of educational attainment could underestimate the effect of UPE if older siblings also benefited from UPE indirectly through higher household incomes. Such income effects have been found to benefit

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ineligible children in the case of Conditional Cash Transfer (CCT) programs (e.g., Lincove & Parker, 2016). However, compared with CCTs, UPE is different for several reasons. First, the value of the transfer via FPE was very small, especially compared to, for example, a CCT in Nicaragua that found an income effect (Lincove & Parker, 2016), which represented nearly 20% of income. Second, UPE in Uganda was more a partial in-kind transfer than a cash transfer. While some actual cash fees were eliminated, other parental contributions such as materials and labor in school improvement remained. So available household income for those children who attend school as a result of UPE would be lower. But it would be higher for those children who would have gone to school regardless of UPE, as the saved school fees are a windfall and increase household income. To examine the possibility of an income effect, we examined the effect of being an older sibling in a household with a younger sibling benefiting from UPE. The dummy is equal to 1 if the person is an older sibling who (born before 1990) has a younger sibling born between 1990 and 1997. The sample size is larger than in the main specification because we do not limit the sample to being in a household where there is at least one pre-UPE-age child and one UPE-age child. Instead, our sample is composed of households where there is at least one pre-UPE-age child. This is precisely because we want to compare ineligible children in households with an eligible child to those without one.

5 | RESULTS

In Section 5.1, we present our main analysis, which includes different specifications for the effect of UPE on years of primary education (Table 3) and on primary school completion and secondary school enrollment (Table 4). In Section 5.2, we implement three robustness checks. In the first two robustness checks, we changed the age threshold of children of our sample construction and the threshold year of exposure to UPE, respectively.

5.1 | Main analysis

We find that UPE had an insignificant effect on completed years of primary school. This result is shown in Table 3, where we regress years of primary school on cohort dummies and other covariates. The reference cohort 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. We have also replaced regional dummies with 103 district-level fixed effects, but the results did not change and are available upon request. In both columns 1 and 2, relative to the reference cohort, the early cohorts fully exposed to UPE (1990–1994) experienced years of primary schooling no different from the reference cohort, while for the 1995–1997 cohorts, primary schooling years are significantly lower.

Next, we widen the sample of students in our analysis 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 and 2. Consistent with the previous findings, in columns 3 and 4, we find significant results for the 1995–1997 cohorts, and that they complete, between 0.189 and 0.404 years less education than the reference cohort, while the first five cohorts, with the exception of 1990 in column 3, exposed to UPE did not have different schooling from the reference cohort.

Figure 3 shows the average years of primary education relative to the 1989 cohort, plotting the cohort coefficients from Table 3. The figure compares the restricted sample with the broader sample that includes everyone of a given birth cohort, each controlling for household fixed effects. The statistical

| Birth year (age in 1997) | (I) | (2) | (3) | (4) | (5) |
|-----------------------------|------------------------|------------------------|-------------------------|-------------------------|------------------------|
| 1980 (17) | -0.261 (0.241) | -0.137 (0.262) | $-0.370^{***}(0.103)$ | -0.246 (0.158) | -0.164 (0.255) |
| 1981 (16) | $0.449^{***}(0.153)$ | $0.370^{**}(0.172)$ | -0.357^{***} (0.121) | -0.143 (0.170) | 0.329 (0.202) |
| 1982 (15) | 0.238 (0.154) | -0.0320 (0.175) | -0.0650(0.0923) | 0.0382 (0.142) | 0.120(0.180) |
| 1983 (14) | 0.272* (0.149) | 0.165 (0.161) | -0.0449 (0.0882) | 0.111 (0.118) | 0.204 (0.154) |
| 1984 (13) | 0.0837 (0.148) | 0.147 (0.156) | 0.0440 (0.0813) | 0.136(0.114) | 0.193(0.154) |
| 1985 (12) | 0.103 (0.124) | 0.116 (0.140) | -0.0461 (0.0840) | -0.0742 (0.117) | 0.253*(0.143) |
| 1986 (11) | -0.0288 (0.116) | 0.0789 (0.132) | 0.0248 (0.0752) | $0.0580\ (0.104)$ | 0.211 (0.137) |
| 1987 (10) | -0.0224 (0.108) | 0.0110 (0.113) | 0.0313 (0.0750) | 0.0455 (0.104) | 0.166 (0.127) |
| 1988 (9) | 0.0238 (0.108) | -0.00345 (0.120) | 0.0228 (0.0767) | -0.0312 (0.108) | 0.115 (0.127) |
| 1989 (8) | Ref. year | Ref. year | Ref. year | Ref. year | Ref. year |
| 1990 (7) | -0.165 (0.115) | -0.102 (0.128) | -0.152^{**} (0.0739) | -0.134(0.103) | -0.245* (0.131) |
| 1991 (6) | 0.149 (0.116) | 0.0528 (0.131) | 0.0895 (0.0798) | -0.0311 (0.109) | 0 (.) |
| 1992 (5) | 0.0437 (0.112) | 0.0380 (0.133) | 0.0732 (0.0735) | 0.0477 (0.102) | -0.0740 (0.131) |
| 1993 (4) | -0.0117 (0.113) | -0.0227 (0.128) | 0.0333 (0.0736) | 0.0489 (0.101) | -0.0779 (0.125) |
| 1994 (3) | 0.0154 (0.0991) | 0.0655 (0.114) | -0.0477 (0.0711) | 0.0303 (0.0952) | -0.0673 (0.116) |
| 1995 (2) | -0.384^{***} (0.107) | -0.431^{***} (0.126) | -0.189** (0.0736) | -0.215** (0.104) | -0.418^{***} (0.131) |
| 1996 (1) | -0.487^{***} (0.109) | -0.439^{***} (0.119) | -0.367^{***} (0.0724) | -0.404^{***} (0.0988) | -0.558^{***} (0.124) |
| 1997 (0) | $-0.405^{***}(0.128)$ | $-0.405^{***}(0.146)$ | -0.402^{***} (0.0808) | -0.396^{***} (0.118) | -0.618^{***} (0.144) |
| Gender, male $= 1$ | -0.00327 (0.0488) | 0.123 * (0.0530) | $0.112^{***}(0.0291)$ | $0.152^{***}(0.0411)$ | 0.0730(0.0494) |
| Rural = 1, urban = 0 | 0.0170 (0.0485) | | -0.521*** (0.0346) | | |
| Region: East | $-0.505^{***}(0.0601)$ | | -0.147^{***} (0.0443) | | |
| Region: North | -0.0962 (0.0743) | | -0.483^{***} (0.0497) | | |
| Region: West | $-0.351^{***}(0.0908)$ | | $-0.339^{***}(0.0484)$ | | |

TABLE 3 Dependent variable: Years of primary education

(Continues)

| | | .182 (0.129) | S | 13*** (0.0897) | 13 | 51 | 42 | 06 | 1-:-: 3 3- 3- E1:1 |
|--------------|-----------------------------|------------------------|----------------------------|------------------------|--------------|------------|---------|----------------|---------------------------|
| | (5) | 0- | Ye | 3) 6.2 | 571 | 2,2 | 0.6 | 0.4 | J F - 1J : 1 |
| | (4) | | Yes | 6.024^{***} (0.0698 | 10,163 | 3,991 | 0.588 | 0.320 | 9-1 |
| | (3) | | No | $6.638^{***} (0.0582)$ | 10,163 | 3,991 | 0.065 | 0.063 | |
| | (2) | | Yes | $6.199^{***} (0.0788)$ | 3,251 | 809 | 0.482 | 0.306 | · OF *** · O1 V |
| ttinued) | (1) | | No | 6.768^{***} (0.0919) | 3,251 | 809 | 0.071 | 0.065 | ** CF * * F |
| TABLE 3 (Cor | Birth year (age in 1997) | Older sibling dummy | Household fixed effects | Constant | Observations | Households | R^{2} | Adjusted R^2 | N |

Note: Standard errors are in parentheses and *p < .10, **p < .05, ***p < .05, ***p < .01. Years represent cohort dummies, with the reference cohort as 1989, the year before tuition was lifted for a child of official enrollment age in 1997. In parenthesis next to the cohort dummies are the ages of the children of a cohort when UPE was enacted, which was 17 for the 1980 cohort and 1 for the 1996 cohort. The reference category for the region is the Central Region, which includes Kampala.

| Birth vear (age | Primary school compl | etion | Starting secondary sch | lool | |
|---------------------------|--------------------------|-------------------------|--------------------------|-----------------------|---------------------|
| in 1997) | (1) OFS | (2) OLS | (3) OTS | (4) OLS | (2) OTS |
| 1980 (17) | 0.0195 (0.0638) | -0.00145 (0.0628) | -0.0620 (0.0567) | $0.0610\ (0.0581)$ | -0.0279 (0.0623) |
| 1981 (16) | 0.223*** (0.0776) | 0.211*** (0.0768) | $0.145^{**}(0.0629)$ | 0.108 (0.0698) | 0.0280 (0.0750) |
| 1982 (15) | $0.119^{**}(0.0607)$ | 0.0614(0.0598) | 0.0163 (0.0515) | 0.101*(0.0539) | $0.0259\ (0.0582)$ |
| 1983 (14) | $0.155^{***} (0.0585)$ | 0.106*(0.0578) | 0.0301 (0.0479) | 0.0361 (0.0497) | -0.0605 (0.0546) |
| 1984 (13) | 0.0833*(0.0504) | $0.106^{**}(0.0495)$ | 0.0407 (0.0429) | 0.0809*(0.0461) | 0.0107 (0.0519) |
| 1985 (12) | $0.0876^{*}(0.0450)$ | 0.0946^{**} (0.0447) | 0.0411 (0.0372) | 0.0508(0.0409) | -0.0160 (0.0472) |
| 1986 (11) | -0.00516 (0.0424) | 0.0502 (0.0414) | -0.00520 (0.0366) | 0.00415 (0.0386) | -0.0781* (0.0442) |
| 1987 (10) | -0.0231 (0.0408) | -0.0135 (0.0397) | 0.0356 (0.0349) | 0.0907** (0.0364) | 0.00103 (0.0430) |
| 1988 (9) | -0.0382 (0.0403) | -0.0288 (0.0400) | 0.0577 (0.0351) | $0.0831^{**}(0.0371)$ | -0.0000916 (0.0430) |
| 1989 (8) | Ref. Year | Ref. year | Ref. year | Ref. year | Ref. year |
| 1990 (7) | -0.0307 (0.0392) | 0.000802 (0.0385) | 0.0140 (0.0333) | $0.0738^{**}(0.0353)$ | -0.000654 (0.0426) |
| 1991 (6) | $0.0933^{**}(0.0435)$ | 0.0685*(0.0415) | $0.0851^{**}(0.0358)$ | $0.118^{***}(0.0366)$ | 0.0607 (0.0404) |
| 1992 (5) | 0.0615 (0.0400) | $0.0582\ (0.0380)$ | -0.0167 (0.0328) | 0.0615*(0.0343) | -0.0320 (0.0400) |
| 1993 (4) | -0.00695 (0.0403) | -0.0132 (0.0386) | 0.00166 (0.0346) | 0.0270 (0.0361) | -0.0274 (0.0414) |
| 1994(3) | -0.0398 (0.0391) | -0.0276 (0.0377) | 0.0522 (0.0337) | $0.0831^{**}(0.0351)$ | 0.0505 (0.0425) |
| 1995 (2) | -0.178*** (0.0397) | -0.173^{***} (0.0379) | -0.00931 (0.0377) | 0.0427 (0.0393) | 0.0367 (0.0504) |
| 1996 (1) | $-0.213^{***}(0.0395)$ | -0.175^{***} (0.0381) | | | |
| 1997 (0) | $-0.205^{***}(0.0471)$ | $-0.239^{***}(0.0451)$ | | | |
| Gender, male $= 1$ | 0.00232 (0.0161) | $0.0363^{**}(0.0161)$ | -0.00956 (0.0158) | -0.00526 (0.0168) | $0.0101\ (0.0180)$ |
| Rural = 1, urban = 0 | -0.209*** (0.0187) | | -0.0989^{***} (0.0170) | | |
| Region: East | -0.0668^{***} (0.0216) | | -0.0134 (0.0205) | | |
| Region: North | -0.158^{***} (0.0237) | | -0.0462* (0.0237) | | |

TABLE 4 Completing primary school and starting secondary school, linear probability model

(Continues)

| (Continued) | |
|--------------|--|
| 4 | |
| LE | |
| B | |
| \mathbf{A} | |
| F | |

| Birth vear (age | Primary school comple | tion | Starting secondary scho | loc | |
|----------------------------|-----------------------|------------------------|-------------------------|-------------------|------------------------|
| in 1997) | (1) OLS | (2) OLS | (3) OTS | (4) OLS | (2) OTS |
| Region: West | -0.0497** (0.0231) | | -0.00659 (0.0220) | | |
| Household fixed effects | No | Yes | No | Yes | Yes |
| Age restricted to | 15+ | 15+ | 15+ | 15+ | 17+ |
| Constant | 0.879*** (0.0322) | $0.640^{***} (0.0271)$ | $0.910^{***} (0.0241)$ | 0.797*** (0.0211) | $0.868^{***} (0.0305)$ |
| Observations | 3,246 | 3,246 | 2,088 | 2,088 | 1,623 |
| Households | 809 | 809 | 687 | 687 | 567 |
| R^2 | 0.106 | 0.512 | 0.030 | 0.511 | 0.536 |
| Adjusted R^2 | 0.100 | 0.346 | 0.020 | 0.263 | 0.277 |
| | | | • | | |

Note: Standard errors are in parentheses and *p < .10, **p < .05, ***p < .01. Years represent cohort dummies, with the reference cohort as 1989, the year before tuition was lifted for a child of official enrollment age in 1997. In parenthesis next to the cohort dummies are the ages of the children of a cohort when UPE was enacted, which was 17 for the 1980 cohort and 1 for the 1996 cohort. 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. OLS = ordinary least squares.



FIGURE 3 Plot of the coefficients corresponding to Table 3. The figure plots the coefficients for Table 3, 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 < .05, ***p < .01

significance of each coefficient is marked by stars, corresponding to the *p* levels (*p < .10, **p < .05, ***p < .01). Three findings stand out. First, there is no significant difference in years of primary education for the mid-to-late 1980s cohorts and the early 1990s cohorts relative to 1 year before tuition was lifted. 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. Third, the youngest cohorts have fewer years of education relative to the 1989 reference group. This might be due to delayed enrollment and slow progression through the school system, suggesting that UPE did not improve on this either.

In terms of other drivers, our results indicate gender inequality in education, where being male corresponds to greater years of primary school. In our most rigorous specification, being male corresponds to 0.123 years of more education than females, statistically significant at the 1% level. We also looked at the differing effects of UPE on gender. To do this, we included interaction terms of each cohort interacted with gender. However, the results confirmed our findings in Table 3. Other key control variables include geographic variation. While there are no rural–urban differences in the restricted sample, we see them in the larger sample, and there are substantial regional differences.

In column 5, we add the older sibling dummy on a sample that no longer requires households to include one UPE and one non-UPE child. Being an older sibling in a household with an UPE-eligible child does not increase years of primary education. If anything, the effect is negative for the older sibling though not significant. Thus, we find no evidence of a benefit for ineligible children through an income effect.

We next examine the effect of UPE on primary school completion and entering secondary school. Table 4 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. As before, 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 generally 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. When we add the older sibling dummy to the specification in column 2 it is, as before, negative and this time significant, confirming that, if anything, having a younger sibling eligible for UPE hurts older siblings (results available on request). Gender also plays a role; being male seems to slightly increase the like-lihood of completing primary school (by 0.04 percentage points).

For the likelihood of starting secondary school (columns 3–5), we restrict our sample to the same group as in Table 4, column 1, and then further restrict our sample to individuals who completed primary school. The last cohort for whom we have data for is the 1995 cohort. In our most rigorous specification (column 2), which controls for all household-related factors using 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 some cohorts before UPE also had a significantly higher positive likelihood to start secondary school, suggesting that UPE did not shift the likelihood upward, holding all other variables constant, 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 Alternatively, this could potentially be affected by the effect of Universal Secondary Education (USE), which provided grants to secondary schools to reduce the financial burden on students starting in 2007 (benefiting the 1993 and 1994 birth cohorts onward in Table 4). Unfortunately, we are unable to make inferences about USE given our limited sample post-1993 birth cohorts. While the effect of USE lies beyond the scope of this paper, a study by Omoeva and Gale (2016) finds that USE led to only a very marginal increase in attendance from 2005 to 2011, and there was no measurable association between the capitation grant and retention rates. They also find that attendance rates were strongly related to wealth, where the wealthiest income quintile had a 40% net attendance rate versus 5% for the poorest quintile. Another study (Asankha & Takashi, 2011) finds that while there was an increase in the enrollment of poor female students, there seemed to be no other significant increases in enrollment, possibly due to supply-side constraints.

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. Therefore, we 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 Table A2, column 1, if we set the sample age threshold to 13, the results are different. Cohorts 1990–1994 have greater years of education compared to the reference cohort, but cohorts before 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 progressed through primary school faster than the reference cohort although not more than prior cohorts.

However, including such young individuals 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 age 18 are excluded. Setting the threshold too high (old), as in column 6, is also problematic because it dramatically decreases the

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sample size. In all, this robustness check largely confirms our result of no impact of UPE on years of primary schooling.

In a second robustness check, we shift the threshold of exposure to UPE back in time to include those with partial exposure in the treatment group. The results for years of primary schooling are shown in Table 5. Column 5 reproduces our preferred specification from Table 3 with 1989 as the

| Rirth year (age | (1) | (2) | (3) | (4) | (5) |
|-------------------------|-------------------|-------------------|-------------------|-------------------|-----------------------|
| in 1997) | 1986 | 1987 | 1988 | 1989 | 1990 |
| 1980 (17) | -0.276 (0.240) | -0.245 (0.257) | -0.222 (0.248) | -0.143 (0.250) | -0.137 (0.262) |
| 1981 (16) | 0.141 (0.202) | 0.147 (0.209) | 0.278 (0.171) | 0.344** (0.173) | 0.370** (0.172) |
| 1982 (15) | -0.142 (0.171) | -0.136 (0.175) | -0.109 (0.166) | -0.0479 (0.171) | -0.0320 (0.175) |
| 1983 (14) | -0.00548 (0.152) | 0.00214 (0.152) | 0.00569 (0.143) | 0.0817 (0.149) | 0.165 (0.161) |
| 1984 (13) | -0.00267 (0.153) | 0.0206 (0.149) | 0.0500 (0.142) | 0.145 (0.150) | 0.147 (0.156) |
| 1985 (12) | Ref. year | -0.00356 (0.141) | 0.0432 (0.133) | 0.104 (0.136) | 0.116 (0.140) |
| 1986 (11) | -0.0212 (0.163) | Ref. year | 0.0534 (0.126) | 0.116 (0.125) | 0.0789 (0.132) |
| 1987 (10) | -0.190 (0.137) | -0.157 (0.132) | Ref. year | 0.0673 (0.120) | 0.0110 (0.113) |
| 1988 (9) | -0.0429 (0.134) | 0.0377 (0.122) | 0.0652 (0.123) | Ref. year | -0.00345 (0.120) |
| 1989 (8) | -0.227 (0.144) | -0.283** (0.139) | -0.287** (0.129) | -0.184 (0.129) | Ref. year |
| 1990 (7) | -0.346** (0.174) | -0.310* (0.160) | -0.198 (0.129) | -0.122 (0.126) | -0.102 (0.128) |
| 1991 (6) | -0.0115 (0.155) | -0.00476 (0.147) | 0.0416 (0.133) | 0.111 (0.121) | 0.0528 (0.131) |
| 1992 (5) | -0.0177 (0.180) | -0.116 (0.181) | -0.0729 (0.140) | 0.0456 (0.138) | 0.0380 (0.133) |
| 1993 (4) | -0.0644 (0.190) | 0.00397 (0.160) | 0.0377 (0.146) | 0.00888 (0.145) | -0.0227 (0.128) |
| 1994 (3) | -0.114 (0.147) | -0.103 (0.131) | -0.0592 (0.119) | 0.0252 (0.115) | 0.0655 (0.114) |
| 1995 (2) | -0.497*** (0.178) | -0.526*** (0.182) | -0.586*** (0.150) | -0.487*** (0.142) | -0.431*** (0.126) |
| 1996 (1) | -0.500*** (0.170) | -0.507*** (0.158) | -0.464*** (0.132) | -0.436*** (0.128) | -0.439*** (0.119) |
| 1997 (0) | -0.444* (0.236) | -0.479** (0.195) | -0.498*** (0.185) | -0.362** (0.162) | -0.405*** (0.146) |
| Gender, male = 1 | 0.0984 (0.0630) | 0.129** (0.0610) | 0.120** (0.0568) | 0.111** (0.0551) | 0.123** (0.0530) |
| Constant | 6.376*** (0.104) | 6.308*** (0.0997) | 6.259*** (0.0828) | 6.212*** (0.0840) | $6.199^{***}(0.0788)$ |
| Household fixed effects | Yes | Yes | Yes | Yes | Yes |
| Observations | 1,905 | 2,305 | 2,677 | 2,951 | 3,251 |
| Households | 451 | 552 | 656 | 728 | 809 |
| R^2 | 0.505 | 0.496 | 0.503 | 0.492 | 0.482 |
| Adjusted R^2 | 0.344 | 0.331 | 0.336 | 0.321 | 0.306 |

TABLE 5 Years of primary education, with household fixed effects, by varying cohort thresholds for the treatment/control groups

Note: Standard errors are in parentheses and *p < .10, **p < .05, ***p < .01. The columns represent birth cohorts that were used to assign the first year of treatment into UPE to create the sample, where we required each household to have at least one pre-UPE and one post-UPE child. In terms of the variables in the first column, years represent birth cohort dummies, with the reference cohorts specified in each column, the year before tuition was lifted for a child of official enrollment age in 1997. In parenthesis next to the cohort dummies are the ages of the children of a cohort when UPE was enacted, which was 17 for the 1980 cohort and 1 for the 1996 cohort.

reference year. As we move left in the table, the threshold is pushed back in time. For example, in column 2, we assume that people born in 1987 or younger benefitted from UPE even though those born in 1987 were already of age 10 when UPE was implemented.⁶ People partially exposed benefited from UPE for a few years of their primary education. On the contrary, they may have been negatively affected by the crowding of schools brought about by UPE. Table 5 shows that including the partially exposed with the treatment group does not change the result that UPE did not increase years of primary schooling.

We also varied the thresholds for primary completion and starting secondary school for children 15 + and 17+. Appendix Table A3 reports the results for children 15+ (results for children 17 + were insignificant). Including the partially exposed in the treatment group confirms the result that UPE did not promote primary completion rates and the weak evidence that it might have promoted the transition to secondary school among those who had moved quickly through primary education.

In a third robustness check, we use a parametric approach to estimate the effect of UPE. We investigate whether UPE changed a preexisting time trend, using 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 0 to 7. *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 Table A1). *Year* represents the birth year of the individual, and a squared term of birth year is included to allow for a nonlinear 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. The effect of *UPE*, including the interaction term, is calculated as the coefficients of $UPE + (UPE*Year \times Year)$, where *Year* represents the treatment group of the cohorts from 1990 to 1997. As expected, we also find that living in a rural household and, in particular, residing in 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; therefore, 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 nonlinear 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 earlier finding that UPE did not increase years of primary schooling.

In addition to the aforementioned 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

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|--------------------------------|------------------------------------|--------------------------------|---|----------------------------|--------------------------|
| | (1) | (2) | (3) | (4) | (5) |
| UPE, treatment = 1 | -11.11* (6.409) | -0.188^{***} (0.0489) | | 0.138 (0.0935) | -11.99 (7.374) |
| Year | $1.837^{**}(0.742)$ | | 0.689*** (0.252) | $0.701^{***}(0.253)$ | $2.003^{**}(0.861)$ |
| $Year^2$ | -0.0109^{**} (0.00435) | | -0.00402^{***} (0.00140) | -0.00417^{***} (0.00142) | -0.0118^{**} (0.00504) |
| UPE*Year | 0.126* (0.0716) | | | | 0.136 (0.0824) |
| Gender, male $= 1$ | 0.0183 (0.0480) | | | | $0.123^{**}(0.0528)$ |
| Household fixed effects | No | Yes | Yes | Yes | Yes |
| Rural = 1, urban = 0 | -0.499^{***} (0.0603) | | | | |
| Region: East | -0.0968 (0.0745) | | | | |
| Region: North | -0.349^{***} (0.0909) | | | | |
| Region: West | $-0.184^{**}(0.0881)$ | | | | |
| Constant | -70.57** (31.66) | 6.309*** (0.0270) | -23.15** (11.25) | -23.15** (11.25) | -78.42** (36.74) |
| Observations | 3,251 | 3,251 | 3,251 | 3,251 | 3,251 |
| Households | 809 | 809 | 809 | 809 | 809 |
| R^2 | 0.062 | 0.467 | 0.474 | 0.475 | 0.477 |
| Adjusted R^2 | 0.059 | 0.290 | 0.299 | 0.300 | 0.303 |
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Denendent variable. Years of nrimary education **TABLE 6**

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Note: Standard errors are in parentheses and *p < .10, **p < .05, ***p < .01. The reference category for region is the Central Region, which includes Kampala. Abbreviation: UPE, Universal Primary Education. 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 his or her current household income played in his or her 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 versus 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 among 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 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. Previous studies that analyze enrollment rates (Deininger, 2003; Essama-Nssah et al., 2008; Grogan, 2008; Lincove, 2012; Nishimura et al., 2008) have found that UPE has had a positive effect on enrollment. These studies may lead the reader to infer that UPE created positive educational gains, which our study does not support. Instead, we measure the impact of UPE on educational attainment, and our results help demonstrate the importance of assessing outcomes beyond enrollment rate metrics. In fact, the surge in enrollment rates may have led to a downward effect of UPE on the educational attainment of Ugandan students, which we further discuss in the following paragraphs.

In this study, we conducted a quasi-experimental impact evaluation of UPE fee elimination on educational attainment and somewhat surprisingly 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 influenced the likelihood of secondary transition for those who had completed primary education. And we find some evidence that it helped at least some children move through the school system faster (see our results on children age 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 overcrowding due

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to UPE came on top of the effect of rapid population growth which increased the cohorts of schoolage populations by 3%-4% per year. Appendix Figure A2 shows the dramatic increase in the gross enrollment ratio for primary school students. Until 1996, the enrollment was relatively static, with the lowest rate in 1992 (63.7) and the highest in 1988 (74.1). After tuition was lifted, enrollment increased to 118 and steadily increased to 138 in 2002 and then steadily decreased to 99.8 in 2018 (World Bank, 2018). The data on progression to secondary school,⁸ while limited, show an increase from the range of 40%-45% from 2000 to 2004 to the low- to mid-60% range from 2006 onward. Progression was highest in 2007 when USE was introduced (66.4%) but then slowly declined to 53.9% by 2015. This crowding is likely to have led to reduced quality-through the lack of teachers and facilities and high student-teacher ratios-lowering outcomes for all students. Second, dropout rates were high, and overcrowding may have pushed some students out of school or resulted in parents putting their children to work at home. Moreover, for some households, opportunity costs may have been too high, especially since primary education was not compulsory. 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. In some cases, for school construction, parents were expected to contribute both materials and labor (Bategeka & Okurut, 2006). Fourth, there is evidence showing that UPE led to enrollment by a disproportionately larger number of students from the lowest income quintile (Oketch & Rolleston, 2007). Given the relationship between income and academic achievement, one could conclude that overall outcomes are negatively affected by low-income students. 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. However, for most, this did not affect schooling investments. But the lower cost may be one reason some children are now moving through the school system faster.

While our study focuses on the Ugandan case, it is noteworthy that despite the lifting of user fees and associated increases in enrollment rates in many sub-Saharan African countries, progress in promoting literacy there has been very slow, increasing by only 4 percentage points (from 54% to 58%) between 1995 and 2012, by far the slowest increase of any developing region (World Bank, 2019). This suggests that our findings might have relevance for other sub-Saharan African countries as well. Moreover, despite the progress made in enrollment, in sub-Saharan Africa, many of the toughest challenges and disparities remain, such as meeting the demand for qualified teachers (UNESCO-BREDA, 2009) and ensuring that students not only enroll but also progress well and are prepared to enter the next grade level (Beegle, Christiaensen, Dabalen, & Gaddis, 2016; UN Economic Commission for Africa, 2015).

Our study questions the impact of lifting primary school fees and suggests that fee elimination alone cannot overcome the challenges of low educational attainment. Careful planning and implementation of complementary investments to overcome the constraints students and schools face are critical for the success of such a policy.

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DATA AVAILABILITY STATEMENT

The data that support the findings of this study are openly available at the World Bank Microdata Library at https://microdata.worldbank.org/index.php/catalog/lsms, survey ID numbers UGA_2005-

2009_UNPS_v01_M, UGA_2010_UNPS_v01_M, UGA_2011_UNPS_v01_M, and UGA_2013_UNPS_v01_M.

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ENDNOTES

- ¹ 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 7 years of free primary school.
- ² Initially, only four children per household could receive free education, of whom 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).
- ³ 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%.
- ⁴ A comparison of gross enrollment and net enrollment would provide more precise information on the scale of age-inappropriate enrollment, but data on net enrollment is not available at the national level.
- ⁵ Adding the older sibling dummy yields a positive coefficient in the 15+ regression and a negative one in the 17+. Results available on request.
- ⁶ Please note that the sample gets smaller as we push the threshold back in time as fewer pre-UPE children are left to be matched with fully or partially exposed UPE children in households.
- ⁷ Civil unrest starting in the early 1980s resulted in 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 1 day, 65% were children in the birth cohorts 1976–1992, estimated to number 66,000 (Annan, Blattman, & Horton, 2006).
- ⁸ Progression to secondary school is defined as the percentage of new (nonrepeat) grade 8 students with respect to the number of students enrolled in grade 7 in the previous year.

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APPENDIX



FIGURE A1 Dependent variables plotted by cohort. N = 5,958. Sample of all surveyed household members (with "child" status in relation to household heads) born between 1980 and 1997. 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, conditional on primary completion [Colour figure can be viewed at wileyonlinelibrary. com]



FIGURE A2 Gross enrollment ratio and progression to secondary school. *Source*: World Bank (2019), with the exception of the following data points: gross enrollment ratio for 2000-2011 (The Republic of Uganda Ministry of Education and Sports, 2012), and gross enrollment ratio for 2012-2015 (Uganda Bureau of Statistics, 2017) [Colour figure can be viewed at wileyonlinelibrary.com]

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|--------------------------|------|------|------|------|------|------|------|------|------|------|------|------|------|------|------|------|
| | | 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 | 6 | 10 | 11 | 12 | 13 | 14 | 15 | 16 | 17 | 18 | 19 | 20 | 21 | 22 | 23 |
| | 1989 | 8 | 6 | 10 | 11 | 12 | 13 | 14 | 15 | 16 | 17 | 18 | 19 | 20 | 21 | 22 |
| Treatment (birth cohort) | 1990 | 7 | ~ | 6 | 10 | 11 | 12 | 13 | 14 | 15 | 16 | 17 | 18 | 19 | 20 | 21 |
| | 1991 | 9 | 7 | 8 | 6 | 10 | 11 | 12 | 13 | 14 | 15 | 16 | 17 | 18 | 19 | 20 |
| | 1992 | | 9 | 7 | 8 | 6 | 10 | 11 | 12 | 13 | 14 | 15 | 16 | 17 | 18 | 19 |
| | 1993 | | | 9 | 7 | 8 | 6 | 10 | 11 | 12 | 13 | 14 | 15 | 16 | 17 | 18 |
| | 1994 | | | | 9 | 7 | 8 | 6 | 10 | 11 | 12 | 13 | 14 | 15 | 16 | 17 |
| | 1995 | | | | | 9 | 7 | 8 | 6 | 10 | 11 | 12 | 13 | 14 | 15 | 16 |
| | 1996 | | | | | | 9 | 7 | 8 | 6 | 10 | 11 | 12 | 13 | 14 | 15 |
| | 1997 | | | | | | | 9 | 7 | 8 | 6 | 10 | 11 | 12 | 13 | 14 |

TABLE A1 Visual aide of enrollment by age

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.

| AND K | LASE | EN | | | | | | | | | | | | | | | | | | | -V | Vile | EY |
|--------------------|-------|-----------------|-----------------------|-----------------------|-----------------------|-----------------------|------------------|------------------|----------------------|-----------------------|-----------|-----------------------|---------------------|---------------------|------------------|-----------------------|------------------------|------------------------|------------------------|------------------------|------------------------|----------------------------|--------------|
| (9) | 18+ | -0.402 (0.341) | 0.137 (0.175) | -0.444** (0.208) | -0.123 (0.191) | -0.135 (0.191) | -0.283 (0.196) | -0.268* (0.162) | -0.326** (0.139) | -0.179 (0.157) | Ref. year | -0.00784 (0.151) | -0.245 (0.151) | -0.258 (0.159) | -0.254* (0.152) | -0.0601 (0.157) | | | | 0.143** (0.0694) | 6.580^{***} (0.104) | Yes | 1738 |
| (5) | 17+ | -0.477 (0.309) | 0.212 (0.168) | -0.310 (0.192) | -0.0864 (0.169) | -0.143 (0.179) | -0.216 (0.174) | -0.158 (0.149) | -0.251* (0.130) | -0.235* (0.139) | Ref. year | 0.0380 (0.140) | -0.206 (0.141) | -0.237 (0.148) | -0.241* (0.133) | -0.201 (0.136) | $-0.527^{***}(0.172)$ | | | $0.114^{*}(0.0625)$ | $6.530^{***}(0.0964)$ | Yes | 2219 |
| (4) | 16+ | -0.194 (0.289) | $0.400^{**}(0.160)$ | -0.0587 (0.187) | 0.136 (0.170) | 0.171 (0.164) | 0.0218 (0.153) | 0.105 (0.143) | 0.0110 (0.120) | 0.0310 (0.127) | Ref. year | 0.313** (0.136) | 0.0494 (0.133) | 0.0693 (0.138) | -0.0389(0.135) | 0.0960 (0.122) | -0.364^{***} (0.135) | -0.174 (0.140) | | 0.103*(0.0564) | 6.242*** (0.0850) | Yes | 2752 |
| (3) | 15+ | -0.137 (0.262) | 0.370** (0.172) | -0.0320 (0.175) | 0.165 (0.161) | 0.147 (0.156) | 0.116 (0.140) | 0.0789 (0.132) | 0.0110 (0.113) | -0.00345 (0.120) | Ref. year | -0.102 (0.128) | 0.0528 (0.131) | $0.0380\ (0.133)$ | -0.0227 (0.128) | 0.0655 (0.114) | -0.431^{***} (0.126) | -0.439^{***} (0.119) | -0.405^{***} (0.146) | $0.123^{**}(0.0530)$ | 6.199^{***} (0.0788) | Yes | 3251 |
| (2) | 14+ | 0.00756 (0.254) | 0.541^{***} (0.158) | 0.257 (0.175) | $0.376^{**}(0.150)$ | 0.412^{***} (0.150) | 0.445*** (0.137) | 0.353*** (0.127) | $0.300^{***}(0.110)$ | $0.284^{**}(0.114)$ | Ref. year | 0.168 (0.125) | -0.111 (0.126) | $0.300^{**}(0.130)$ | 0.214* (0.126) | 0.336^{***} (0.114) | -0.217* (0.123) | -0.195* (0.116) | -0.457*** (0.132) | $0.113^{**}(0.0509)$ | 5.912*** (0.0731) | Yes | 3657 |
| (1) | 13+ | 0.451** (0.227) | $0.940^{***}(0.159)$ | 0.711^{***} (0.176) | 0.798^{***} (0.149) | 0.833^{***} (0.150) | 0.852*** (0.132) | 0.774*** (0.124) | 0.697*** (0.105) | 0.708^{***} (0.109) | Ref. year | 0.586^{***} (0.120) | $0.265^{**}(0.125)$ | 0.243* (0.131) | 0.623*** (0.128) | 0.718^{***} (0.105) | 0.178 (0.115) | 0.119 (0.105) | -0.160 (0.120) | $0.161^{***} (0.0507)$ | 5.473*** (0.0656) | Yes | 4082 |
| Birth vear (age in | 1997) | 1980 (17) | 1981 (16) | 1982 (15) | 1983 (14) | 1984 (13) | 1985 (12) | 1986 (11) | 1987 (10) | 1988 (9) | 1989 (8) | 1990 (7) | 1991 (6) | 1992 (5) | 1993 (4) | 1994 (3) | 1995 (2) | 1996 (1) | 1997 (0) | Gender | Constant | Household fixed effects | Observations |

TABLE A 2 Dep: Years of primary education, with household fixed effects

| (Continued) |
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| Birth vear (age in | (1) | (2) | (3) | (4) | (5) | (9) |
|-------------------------|----------------------------|---------------------|-------------------|----------------------------------|------------------------------|-----------------------------|
| 1997) | 13+ | 14+ | 15+ | 16+ | 17+ | 18+ |
| Households | 908 | 864 | 809 | 733 | 636 | 531 |
| 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 |
| Mata. Ctondard amous at | in somethood and kn < 10 k | ** > 05 **** > 01 V | more other dumine | h the information achorit of 100 | 20. the user hefers thirting | النفيط فيسم ملائلط مرفقتمنا |

** p < .01. Years represent cohort dummies, with the reference cohort as 1989, the year before tuition was lifted for a child of official enrollment age in 1997. In parenthesis next to the cohort dummies are the ages of the children of a cohort when UPE was enacted, which was 17 for the 1980 cohort and 1 for the 1996 cohort. *Note:* Standard errors are in parentheses and ${}^{*}p < .10$, ${}^{**}p < .05$, *

| Birth year (age in 1997) | 1986 | 1987 | 1988 | 1989 | 1990 |
|--|------------------------|------------------------|------------------------|------------------------|------------------------|
| 1980 (17) | 0.0109 (0.0560) | 0.0401 (0.0562) | 0.00425 (0.0570) | 0.0394 (0.0570) | $0.0610\ (0.0581)$ |
| 1981 (16) | 0.0546 (0.0672) | 0.0865 (0.0686) | 0.0510 (0.0694) | 0.0552 (0.0688) | 0.108 (0.0698) |
| 1982 (15) | 0.0788 (0.0521) | $0.109^{**}(0.0518)$ | 0.0728 (0.0525) | 0.0777 (0.0532) | $0.101^{*} (0.0539)$ |
| 1983 (14) | 0.0431 (0.0463) | 0.0703 (0.0456) | 0.0149 (0.0466) | 0.0235 (0.0476) | 0.0361 (0.0497) |
| 1984 (13) | $0.0823^{*}(0.0449)$ | 0.102^{**} (0.0433) | 0.0674 (0.0448) | 0.0632 (0.0454) | $0.0809^{*} (0.0461)$ |
| 1985 (12) | Ref. year | $0.0694^{*} (0.0409)$ | 0.0293 (0.0406) | 0.0272 (0.0406) | 0.0508 (0.0409) |
| 1986 (11) | 0.0179 (0.0464) | Ref. year | -0.0169(0.0387) | -0.0188(0.0381) | 0.00415 (0.0386) |
| 1987 (10) | $0.0509\ (0.0439)$ | 0.0859^{**} (0.0424) | Ref. year | 0.0640^{*} (0.0364) | $0.0907^{**}(0.0364)$ |
| 1988 (9) | $0.0834^{*}(0.0481)$ | 0.115^{***} (0.0421) | 0.0946** (0.0426) | Ref. year | $0.0831^{**}(0.0371)$ |
| 1989 (8) | -0.00127 (0.0471) | 0.0513 (0.0425) | 0.0143 (0.0402) | 0.0134 (0.0390) | Ref. year |
| 1990 (7) | $0.0469\ (0.0453)$ | 0.0838^{**} (0.0418) | 0.0339 (0.0384) | 0.0485(0.0359) | $0.0738^{**}(0.0353)$ |
| 1991 (6) | 0.137^{***} (0.0458) | 0.155^{***} (0.0442) | 0.0964^{**} (0.0421) | $0.0923^{**}(0.0391)$ | 0.118^{***} (0.0366) |
| 1992 (5) | -0.00146 (0.0470) | 0.0486 (0.0429) | 0.0248 (0.0388) | 0.0323 (0.0360) | $0.0615^{*}(0.0343)$ |
| 1993 (4) | 0.0180 (0.0513) | 0.0316 (0.0464) | -0.0135 (0.0424) | 0.0121 (0.0389) | 0.0270 (0.0361) |
| 1994 (3) | 0.0536 (0.0456) | $0.0953^{**}(0.0423)$ | 0.0618 (0.0407) | $0.0664^{*} (0.0375)$ | $0.0831^{**}(0.0351)$ |
| 1995 (2) | 0.0341 (0.0561) | 0.0589 (0.0505) | 0.0440 (0.0461) | 0.0252 (0.0433) | 0.0427 (0.0393) |
| Gender, male $= 1$ | 0.00687 (0.0203) | 0.00612 (0.0191) | $0.00740\ (0.0181)$ | 0.00440 (0.0172) | -0.00526(0.0168) |
| Constant | 0.816^{***} (0.0269) | 0.787^{***} (0.0246) | $0.815^{***}(0.0237)$ | 0.814^{***} (0.0228) | 0.797*** (0.0211) |
| Household fixed effects | Yes | Yes | Yes | Yes | Yes |
| Observations | 1310 | 1531 | 1747 | 1913 | 2088 |
| Households | 397 | 474 | 556 | 514 | 687 |
| R^2 | 0.486 | 0.488 | 0.503 | 0.511 | 0.511 |
| Adjusted R^2 | 0.250 | 0.247 | 0.261 | 0.271 | 0.263 |
| <i>Note</i> : Standard errors in parentheses | | | - | - | |

Entry to secondary school, age 15+, with different year thresholds for UPE treatment, with household fixed effects TABLE A3

The columns represent birth cohorts that were used to assign the first year of treatment into UPE to create the sample, where we required each household to have at least one pre-UPE and one post-UPE child. In terms of the variables in the first column, years represent birth cohort dummies, with the reference cohorts specified in each column, the year before tuition was lifted for a child of official enrollment age in 1997. In parenthesis next to the cohort dummies are the ages of the children of a cohort when UPE was enacted, which was 17 for the 1980 cohort and 1 for the 1996 cohort.

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* p < .10; ** p < 0.05; *** p < 0.01.