# Closing the Gender Gap: Differential Impacts of Public and Private School Subsidies in Uganda*

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**Abstract**

How can low-income countries expand access to secondary education? In 2007, Uganda subsidized tuition fees in public and private secondary schools if no public school was available within a reasonable walking distance. Using novel administrative and household data, I find that the subsidies had no effects on educational outcomes for both male and female students in public schools. However, educational outcomes significantly improved in subsidized private schools, with female students benefiting approximately five times more than male students. I provide new insights into how a non-targeted fee-reduction policy can produce large, heterogeneous effects, in particular with respect to gender and school location.

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# 1 Introduction

Education is a key determinant of individual earnings, social mobility, and broader economic growth (Akresh et al., 2023; Duflo et al., 2024). Despite the widespread implementation of universal primary education, access to secondary education remains much more limited. In Sub-Saharan Africa secondary school enrollment stood at 45% in 2021, with a gender gap of 5% (WorldBank, 2021). Barriers include, among others, financial constraints, high opportunity costs, overcrowded schools, and teacher absenteeism (Duflo et al., 2021). Access to secondary education is even more difficult for girls: the additional barriers they face include restrictive gender norms, household responsibilities, and risks to personal safety when traveling to school (Burde and Linden, 2013; Muralidharan and Prakash, 2017).

How can low income countries expand access to secondary education? I study the impact of Uganda's Universal Secondary Education (USE) policy, which eliminated tuition and registration fees in public schools, and in selected private schools if no government school was available within a 5 km walk of primary schools.[1] For each enrolled student, the government paid a subsidy to public schools and, through a Public-Private Partnership (PPP) agreement, to private schools contingent on schools not charging any additional tuition or registration fees to students.

I exploit variation in both cohort-and district-specific exposure to the program using a difference-in-differences design. Individuals born before 1993 were not eligible for tuition and registration fee reductions, while those born in 1993 or later could benefit from it. Differences in the availability of public and private USE schools, normalized by the number of primary students per district, made exposure to the policy more or less likely. The key identifying assumption is that educational outcomes for cohorts not eligible for USE would have followed parallel trends across districts with varying levels of exposure, with outcomes diverging for eligible cohorts once the program was introduced.

[1] Similar school fee reduction policies have been implemented in Ghana (2016), Kenya (2008), Tanzania (2015), and Rwanda (2017).

I find that the likelihood of attending secondary education increased by 6 percentage points; the completion rate for lower secondary education by 8 percentage points, and schooling years by 0.31 for respondents eligible for USE and residing in districts with above the median number of USE schools. There are no significant effects on the completion rate of upper secondary education. Heterogeneous treatment effects indicate that the exposure to one additional private USE school resulted in 0.66 more years of schooling for female students, compared to a modest 0.14 increase for male students. In contrast, one additional public USE school had no significant effect on educational outcomes for either gender.

Regarding the underlying mechanisms, I present evidence that suggests that public USE schools had lower capacity to increase student enrollment and probably fewer incentives to expand. This could explain why the exposure to private USE schools resulted in greater improvements in educational outcomes compared to public USE schools, further suggesting that private schools absorbed much of the additional schooling demand generated by the policy. Since the policy benefited women more than men, I also explore whether geographical proximity plays a role. The policy goal was to provide access to tuition-free schools within five kilometers and most educational gains are linked to private USE schools: I examine whether girls living within a five kilometers walk of a private USE school benefited more than those living farther away. Employing a triple difference estimator, I show that there are improved educational outcomes for female students within, but not beyond, this distance. These findings emphasize that distance remains a significant constraint on the effectiveness of fee-reduction policies. Opportunity costs, such as extended travel times that reduce the time that girls are available for household chores, and safety concerns may still have deterred female students from enrolling in secondary education, despite any cost reduction (Evans et al., 2024; Muralidharan and Prakash, 2017).

There are three key threats to identification. One, strategically-located private schools decided to be part of the program. This could introduce selection bias if households migrated close to their private schools of interest. This is unlikely. The participation of private schools in the first year of the program was only announced shortly before the school year began, limiting opportunities for strategic relocation. Once accepted into the USE program, private schools continued to receive USE funding in subsequent years. More importantly, only 13% of respondents migrated, and the majority only did so after reaching secondary school age. Two, USE could have induced composition effects. If more students complete primary education the composition of student populations differs before and after the introduction of USE, making the pre- and post-policy samples non-comparable. I rule this out by showing that the introduction of USE did not increase the likelihood of completing primary education. Three, changes in school quality may explain differences in educational outcomes between public and private USE schools. I find, using test-score and school input data, that educational quality remained similar, suggesting that the observed differences in outcomes are due to the fee reduction policy itself rather than changes in school quality.

My research contributes to the literature on the impacts of school fee reduction policies in low income countries. Previous studies yield mixed evidence. There is a positive impact on access to secondary education in Gambia, Kenya, Colombia, and Ghana (Blimpo et al., 2019; Brudevold-Newman, 2021; Duflo et al., 2021; Angrist et al., 2002, 2006). Conversely, access remained nearly unaffected in Pakistan (Chaudhury and Parajuli, 2010), while it increased at the lower secondary level in South Africa, but decreased at the upper secondary level due to worsening school quality (Garlick, 2019). Importantly, most papers study programs that are targeted to specific groups, such as women or financially constrained households, and designed to address specific barriers to secondary education.

I contribute to the literature by studying the effects of an untargeted intervention, which is not designed to reach a specific group and may, therefore, be less effective. To the best of my knowledge, my study is the first to demonstrate that an untargeted secondary education program can produce significantly heterogeneous responses with respect to gender and in terms of exposure to private and public schools. This research contributes to the debate about whether school fee reduction programs should be targeted to promote equity, or untargeted to promote equality. I conduct a cost-effectiveness analysis and find that the intervention could have been more efficient if the subsidies had been more targeted, such as being directed exclusively toward female students. This approach would have optimized resources by focusing on the group most likely to benefit from the policy.

I also introduce a novel dimension by examining the importance of distance to schools for the effectiveness of fee reduction policies. Existing studies have shown that the provision of bicycles significantly improves educational outcomes for female students in India (Muralidharan and Prakash, 2017) and that constructing schools closer to student populations in Afghanistan increases years of schooling for students living nearby (Burde and Linden, 2013). My research extends previous findings by analyzing how proximity interacts with school fee reduction programs, revealing that fee reductions primarily benefit female students living close to schools. This effect is not entirely straightforward. One could also expect that reducing school fees enables students to spend more on transportation, bringing schools within reach of those living farther away (Evans et al., 2024).

The second strand of literature that this study engages with pertains to public-private partnerships (PPPs) in education. PPPs are seen as having potential to fund the expansion of educational opportunities, particularly when public school capacity is limited. Studies from countries including Pakistan, India, Liberia and Uganda have shown that PPPs can improve educational quality and learning outcomes, primarily by leveraging resources and efficiencies of the private sector (Alderman et al., 2003; Barrera-Osorio and Raju, 2011; Barrera-Osorio et al., 2020; Romero et al., 2022). In Liberia, for example, Romero et al. (2020) find that giving public subsidies to private schools led to improved school quality, with increased student learning and reduced teacher absenteeism.

However, there is limited evidence on how these partnerships affect general access to secondary education. Existing studies have examined changes in enrollment numbers at the school level. For example, Barrera-Osorio et al. (2020) find that USE increased student enrollment in participating private schools. However, they do not explore whether this increase is driven by new students accessing secondary education or by a redistribution of current students towards private USE schools (Patrinos et al., 2009; Crawfurd et al., 2024). In addition, these studies only consider short-term impacts and do not account for long-term enrollment. As a result, it remains unclear to what extent PPPs enhance the access to secondary education for the broader student population. My study addresses this gap by examining how overall secondary education access changes under the PPP in Uganda.

The remainder of this paper is structured as follows. In Section 2, I describe the setting and provide an overview of the policy. I describe the identification strategy in Section 3. In Section 4 and 6, I present results and explore potential mechanisms. Section 6 is the conclusion.

## 2 The school system in Uganda

The educational system in Uganda includes seven years of primary education (ending with the Primary Leaving Examination PLE), followed by six years of secondary education, divided into four years of lower secondary (ending with the Uganda Certificate of Education, UCE), and two years of upper secondary (culminating in the Uganda Advanced Certificate of Education, UACE). Class levels are designated as P1-P7 for primary, S1-S4 for lower secondary, and S5-S6 for upper secondary education.

In 2007, Uganda launched the Universal Secondary Education (USE) program to improve access to secondary schooling. Despite universal access to primary education, many students still faced significant barriers to attend secondary education, such as the associated costs. USE abolished tuition and registration fees, starting with first-year secondary students (S1) in 2007. In 2008, S2 students became eligible as well. By 2012, all secondary levels were included in the program. The government provides schools with a subsidy per student and per term: 41,000 UGX for lower secondary education for public schools, 47,000 UGX for lower secondary education for private schools and 80,000 UGX for upper secondary education for both public and private schools. In return, schools are not allowed to charge any additional tuition and registration fees from the students.

To address the shortage of public school places, a Public-Private Partnership (PPP) agreement was established, enabling certified private schools with low fees and sufficient infrastructure to receive government subsidies. Importantly, private schools could only join the program if no government school was available within a reasonable walking distance for students. The Ministry of Education made the final decision on private schools receiving subsidies (Donoghue et al., 2018). The primary goal was to ensure that every student had ideally access to a low-cost school within 5 kilometers to their primary school (EPRC, 2022; MOES, 2014; MoLHUD, 2022). This objective has largely been achieved. Using data from the Annual School Census, Table A2 shows that the average distance from 16,884 primary schools to the nearest public USE school was 5.8 km by 2014. When considering both public and private USE schools, this average distance decreases to 4.46 km.

Private schools that joined USE were mostly rural, established by local communities or entrepreneurs (Barrera-Osorio et al., 2020). At the time of joining, their average student-to-teacher ratio, student-to-classroom ratio, enrollments and teacher absenteeism (Wane, 2014) was lower than in public USE schools (Table A1).[2] In terms of overall school quality, they performed similar to public schools, as discussed in section 3.5. Table A3 shows that USE decreased schooling costs by approximately 50% for lower and upper secondary education, with similar expenses remaining for students in public versus private schools.[3]

[2]By 2014, seven years after the program's introduction, the student-to-teacher and student-to-classroom ratios had become more similar between public and private USE schools.

[3]Total costs include registration, tuition, exam fees, and expenses for meals, transport, school books, and uniforms. This comprehensive measure ensures that hidden costs, such as increased meal and textbook expenses when tuition is waived, are not overlooked. Despite these reductions, annual expenses per child in USE schools still account for 10-20% of household income, making secondary education financially out of reach for many families in Uganda (Kakuba et al., 2021).

Universal Secondary Education has effectively divided the schooling market in Uganda into private and public schools with USE funding and private and a small portion of public schools without USE-funding.[4] Figure A1 shows that the number of public USE schools increased from 800 to 1000, while the number of private USE schools more than doubled from 450 to 980 between 2007 and 2014. The number of officially reported non-USE private schools remained nearly constant between 2007 and 2011, with approximately 1200 to 1400 schools, before increasing to 1794 schools in 2014.[5] The increase in the number of private non-USE schools may be induced by the USE policy. However, I demonstrate in Section 4.3.5 that the main effects on educational outcomes are driven by private or public USE schools and not by private non-USE schools

The expansion of both private and public secondary schools is also visible in the number of students enrolled in secondary education. This number doubled from 0.7 million in 2006 to 1.4 million in 2014 (Figure A2). Moreover, the share of students attending private or public USE schools rose from 20% to 65% in 2014 (Figure A3). In addition, the difference in the share of students attending private USE versus public USE schools is narrowing over time, with private and public USE schools each enrolling approximately 30% of students in 2014. Meanwhile, the share of students not receiving USE funding dropped from 80% in 2007 to 35% in 2014.

The rise in the number of students attending secondary education must be considered in the context of population growth. Figure A4 shows that the secondary gross enrollment rate GRE increased modestly from 23% in 2006 to 28% in 2016, with female enrollment consistently lagging behind male enrollment (21% versus 26% in 2007 and 26% versus 28% in 2016). Compared to a similar fee reduction scheme in Kenya, where the GER rose from 39%

[4] A few elite public schools, denoted as public non-USE schools, opted out of the program to charge higher tuition and registration fees. They are mostly found around Kampala.

[5] Exact numbers of public USE, private USE, public non-USE schools and private non-USE schools per year and district were provided by the Ministry of Sports and Education (MOES). However, not all private non-USE schools report to MOES, suggesting that the numbers presented are most likely a lower-bound (MOES, 2014).

in 2008 to 68.5% in 2017, the increase in Uganda is much smaller (ETH, 2021; MOE, 2020). While USE may have stabilized and slightly improved access to secondary education in line with population growth, further analysis is needed to understand who actually benefited from the policy and why.

# 3 Empirical set-up

This section first outlines the identification strategy, followed by an assessment of the empirical model, data, and sample selection. Finally, I address potential threats to the identification.

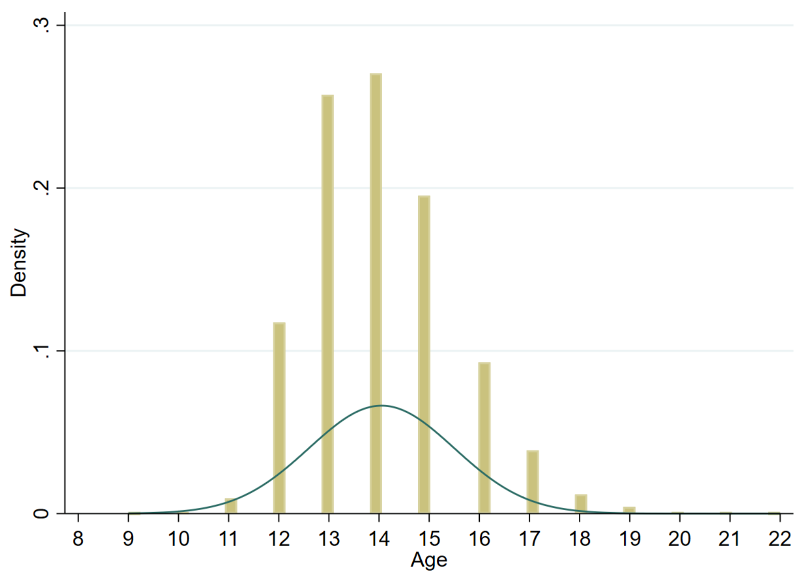
## 3.1 Identification

My identification strategy uses a difference-in-differences design to estimate the causal impacts of USE. I exploit district- and cohort-specific variations in exposure intensity to the program. The key identifying assumption is that educational outcomes for birth cohorts not eligible for USE would have followed parallel trends across districts with varying exposure levels. Divergence in these trends for cohorts eligible for USE allow to capture the program's impact.

Cohort exposure is determined by the timing of USE. Children, who entered secondary education before 2007 were not eligible to USE. As the official age for entering secondary education is 13, students born in 1994 were the first cohort eligible for USE-funding. However, Figure 1 shows that most students entering secondary education were 14 years old. Assuming that the age distribution is constant over time, I consider cohorts born in 1993 or later as treated and those born before as untreated. To test robustness, I will also use different cut-offs and omit different cohorts.

The second variation in the exposure to USE stems from the fact that the density of USE schools varies across districts and over time. To compare districts in terms of school density, I compute the total number of private and public USE schools per 10,000 students that are enrolled in primary education.[6] Given that access to primary education is nearly universal in Uganda (only 5 districts recorded a gross enrollment rate below 100% in 2007 (MOES, 2010)), this serves as a reasonable proxy for the population of children who can center secondary education.

Figure 1: **Age of students entering S1**




**NOTE**: Computed by using test-score data from 305,000 students from the Uganda Examinations Board in 2015 and 2016.

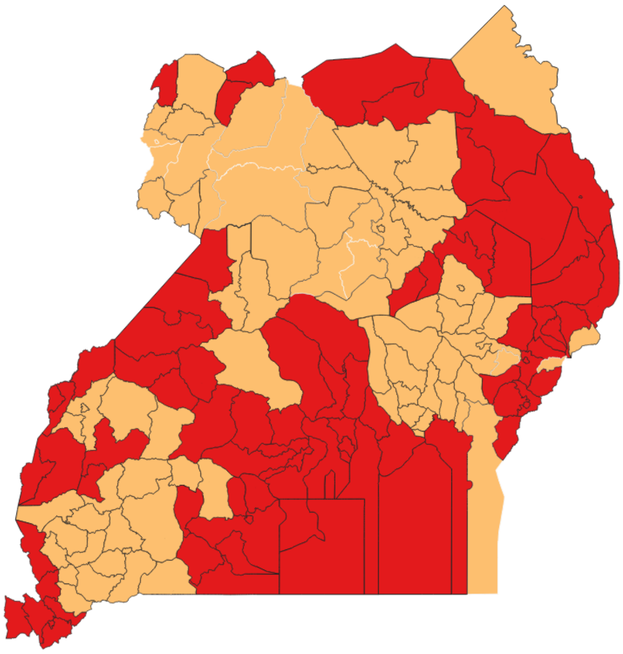
I then use two different approaches to measure district exposure to USE: First, I divide districts into high- and low-exposure districts. For this, I calculate the average number of private and public USE schools per 10,000 primary students in each district between 2007 and 2014. Districts exceeding the median number of total USE schools per 10,000 primary students are categorized as high-exposure, while those below the median are classified as low-exposure. This measure effectively captures the average treatment effect, but does not allow disentan-

[6]More precisely, I divide average number of public and private USE schools between 2007 and 2014 by the total primary student population at the district level in 2004. The primary student population is sourced from the 2004 Annual School Census (ASC). The analysis would, ideally, use the number of Primary 7 students in 2006 to better capture secondary education demand. However, due to the unavailability of grade-specific enrollment data for primary education at the district level before 2010, I rely on total primary enrollment per district in 2004 as a proxy. Despite this limitation, there is a strong correlation of 0.86 between total primary school enrollment in 2004 and total primary school enrollment in P7 in 2010.

gling the differential effects of private and public USE schools. Figure 2 shows which districts are low and high-exposure districts.

Second, instead of categorizing districts as high- or low-exposure, I use the total number of private and public USE schools per 10,000 primary students as a treatment measure for each year from 2007 to 2014. This approach captures absolute differences in the number of USE schools across districts and over time. It, therefore, is a measure of treatment intensity and allows to assess the differential effects between public and private USE schools.

Figure 2: **High and low exposed districts to USE**



**NOTE**: Computed using official data on student enrollment and USE-schools from the Ministry of Education. Red indicates districts with a high density of USE schools, and beige areas are districts with a low density of USE schools.

## 3.2 Specification

I use individual level data to compute the impact of Universal Secondary Education on educational outcomes. I estimate the average treatment effect as following:

$$Y_{irbd} = \beta_0 + \beta_1 \times Cohort_b \times Exposure_d + X_{irbd} + \delta_d + \alpha_b + \gamma_r + \epsilon_{irbd}, \qquad (1)$$

where $Y_{irtd}$ is the outcome of interest for individual *i*, born in year *b*, living in district *d* at the age of 14 and being surveyed in round *r*. My outcomes of interest include the likelihood of attending secondary education, completing lower or upper secondary education, and the increase in total years of schooling. $Cohort_b$ is one if an individual is born in 1993 or later, otherwise 0. $Exposure_d$ equals one if a respondent lived in a district above the median number of USE schools per 10 000 primary students. This measure is time-invariant. It is based on the average average number of USE schools per 10,000 primary students in each district, computed over the period from 2007 to 2014. The coefficient of interest $\beta_1$ captures the causal effect of USE.

The regressions include district fixed effects $\delta_d$, birth year fixed effects $\alpha_b$ and survey round fixed effects $\gamma_r$. I also include household and individual level control variables $X_{irbd}$: the gender of the respondent, ethnicity, the education level of the household head, household size, a wealth index 1-5[7] and whether the respondent lives in an urban area. To account for potential underlying trends within districts that may impact education outcomes, I include region-specific linear trends and an interaction term between the birth cohort of the respondent and pre-program poverty rates at the district level (Brudevold-Newman, 2017; Lucas and Mbiti, 2012; Duflo, 2001). $\epsilon_{irbd}$ are standard errors clustered at the district level.

To account for treatment-intensity across districts and over time, I proceed by interacting each birth cohort with the number of private and public USE schools per 10,000 primary students available in the respondent's district at the age of 14:

$$\begin{aligned} Y_{irbd} = \beta_0 + \beta_1 \times Cohort_b \times Schools_{bd}^{Public} + \beta_2 \times Cohort_b \times Schools_{bd}^{Private} \\ + X_{itbd} + \delta_d + \alpha_b + \gamma_r + \epsilon_{irtd}, \end{aligned} \tag{2}$$

the coefficients $\beta_1$ and $\beta_2$ capture the effect of one additional public or private USE school

[7]The wealth index categorizes respondents based on the household assets they possess, with a score of one representing the "poorest households" and five representing the "wealthiest." For further details, see DHS (2023).

per 10,000 primary students on educational outcomes. To control for differential impacts of private and public USE schools on female and male respondents, I adjust the equation as following:

$$
\begin{aligned}
Y_{irbd} = \beta_0 + \beta_1 \times Male_{itbd} + \beta_2 \times Cohort_b \times Schools_{bd}^{Private} \\
+\beta_3 \times Cohort_b \times Schools_{bd}^{Private} \times Male_{itbd} \\
+\beta_4 \times Cohort_b \times Schools_{bd}^{public} + \beta_5 \times Cohort_b \times Schools_{bd}^{public} \times Male_{itbd} \\
+Male_{itbd} \times X_{itbd} + \delta_d + \alpha_b + \gamma_r + \epsilon_{irtd},
\end{aligned}
\tag{3}
$$

, $\beta_2$ now captures the effect of an additional private USE school on female respondents, while $\beta_3$ represents the variation in this effect for male respondents. Similarly, $\beta_4$ measures the effect of an additional public USE school on female respondents and $\beta_5$ indicates the difference in this effect for male respondents.

I use Uganda's district boundaries as defined in 2006, consisting of 80 districts. For respondents who migrated, I use the district in which they resided at age 14. For 87% of respondents, this corresponds to their birth district, which can be considered exogenous to the USE policy. Approximately 13% of respondents attended school in a different district from their birthplace.

Recent advancements in the literature have raised concerns about the two-way fixed effects (TWFE) estimator in difference-in-differences designs with staggered implementation (de Chaisemartin and D'Haultfœuille, 2023; Gardner, 2022; Roth et al., 2023; Sant'Anna and Zhao, 2020). This issue is less of a concern for this study, as the USE-policy was implemented homogeneously across all districts in 2007 and all students who entered S1 became eligible. Therefore, there is no variation in treatment timing across districts or cohorts.[8] However, USE did vary in terms of treatment intensity, as more and more schools joined after 2007.

[8] In fact, using the de Chaisemartin and D'Haultfœuille (2023) estimator indicates that equation (1) does not possess any negative weights. Therefore the program's effect is not overestimated.

Therefore, I test robustness to an alternative set of difference-in-differences estimators in section 4.3.1 (de Chaisemartin and D'Haultfœuille, 2023; Gardner, 2022; Sant'Anna and Zhao, 2020; Sun and Abraham, 2021).

## 3.3 Data

I employ different datasets, which I detail below:

**School data**: The Ministry of Sports and Education shared a dataset detailing the number of public and private USE schools, as well as public and private non-USE schools, at the district level from 2007 to 2014. In addition, I received access to the Annual School Census (ASC) data from 2004, 2014, 2016, and 2019. The ASC offers comprehensive information on school characteristics, including geo-location, student enrollment, number of classrooms and teachers, and ownership status. The data also indicates whether a school participated in the Universal Secondary Education (USE) program in 2014, 2016, and 2019, though it does not specify the year a school joined the program.

**Test-score data:** I use official test-score data from the Uganda Certificate of Education (UCE) in 2014 and 2015. This is the national examination taken by students at the end of lower secondary education. The dataset also contains the scores of the students with regard to their Primary Leaving Examination, so the national examination at the end of primary school.

**World Bank Living Standards Measurement Survey (LSMS)**: I leverage the LSMS to assess the effects of USE on educational outcomes. I use the second to sixth rounds and treat each survey round as a distinct cross-section by applying cross sectional survey weights.[9][10] Each survey round is representative at the national, regional, urban and rural

[9]Please note that the first, seventh, and eighth rounds do not assess the respondents' migration history or geolocations. Since these variables are essential for my analysis, I exclude these rounds.

[10]The LSMS continually introduces new households into each survey round and surveys the same households for one, two or three survey rounds. Moreover, the household composition is changing for each survey round, with new household members joining and/or old household members leaving the household. For simplicity, I decided to take advantage of the individual cross-sectional snapshot of Uganda for each survey round.

level (WorldBank, 2024). I extract information for each respondent with respect to their educational attainment, their employment, marital status, ethnicity, household size, household assets, parental education and if they reside in an urban or rural area. For individuals still enrolled in school, the survey includes specific questions related to education, such as school fees paid or the type of school the respondents are attending. Importantly, the LSMS documents the migration history of respondents at the district level, detailing any movement from the district of birth to the current district of residence.

**Population census:** I extract from the population census from 2002 and 2014 district and sub-county level characteristics, such as the number of primary students, population size or the poverty rate.

## 3.4 Sample selection and summary statistics

The final sample consists of 6846 respondents born between 1987 and 1998. I exclude respondents born earlier as Uganda was in civil war until 1986.[11] I further restrict the sample to respondents that have completed primary education to ensure that I compare individuals that had the opportunity to pursue secondary education. For all outcomes except upper secondary education, I focus on respondents who are at least 18 years old. For upper secondary education, which most students complete by age 20 or later, I restrict the sample to respondents that are at least 20 years old (MOES, 2014). I also exclude respondents who attended secondary school in Kampala, as the educational landscape in the capital may differ significantly from the rest of the country.

Table A4 presents summary statistics. The respondents are on average 22 years old and have completed 9.74 years of education. 73% have attended some secondary schooling, 41% have completed lower secondary education, and 21% have completed upper secondary edu-

[11] It is important to compare individuals who grew up predominantly during periods of peace, as exposure to conflict can negatively affect educational outcomes and introduce selection bias (Chamarbagwala and Morán, 2011; Valente, 2014).

cation. Approximately 40% of respondents in the sample are still enrolled in secondary or post-secondary education.[12] As the LSMS survey rounds were typically conducted in the first half of the year, coinciding with the last semester of the schooling year for these individuals, I use their last year of schooling as a reference point for completed education. For those who advanced to university or post-secondary studies, I code them as having completed upper secondary education. Consequently, the main estimates for educational outcomes represent lower bounds, as some students may have continued their education after the LSMS survey was conducted.

When comparing respondents from low- and high-exposure districts, there is a slight imbalance in household size and wealth (Table A5). The average household size is 7.97 in high-exposure districts, compared to 8.33 in low-exposure districts. Respondents in high-exposure districts also score higher on the wealth index, with a difference of 0.28 compared to those in low-exposure districts. Additionally, there is a disparity in ethnic representation: 32% of respondents in high-exposure districts are from the Baganda ethnic group, compared to 12% in low-exposure districts. I control for all of these imbalances in the main regressions.

## 3.5 Threats to identification

Before showing the results, I discuss potential threats to my identification. First, while nearly all public USE schools automatically joined USE, private schools could only opt in if their location significantly reduced students' travel distances to government schools with available USE places. This could introduce selection bias if respondents anticipated this and migrated close to private schools, before these schools became part of USE. This is unlikely. First, the Ministry of Education announced the inclusion of private schools in the subsidy program only shortly before the school year began, which limited opportunities for strategic relocation, at least for the first year that private schools started receiving subsidies. Second, there is little evidence of migration before secondary school age, with only 13% of respondents moving

[12] Brudevold-Newman (2017) also reports that half of the respondents in his DHS sample are still enrolled in secondary education at age 18 in Kenya.

across districts between their birth and the start of secondary school.

Second, composition effects are another threat. The USE policy may have encouraged students to complete primary education, who in the absence of the policy would not have done so. This could result in cohorts before and after the introduction of the program that are not comparable to each other. I examine this by estimating whether USE increased the likelihood of completing primary education. The results in Table A7 show this is not the case. Hence, composition effects do not drive the outcomes of USE.

Third, improvements in school inputs or school quality after the introduction of the program can also impact educational outcomes. My evidence, however, suggests that this is unlikely. As shown in Table A1, inputs like the student-to-teacher ratio worsened post-USE. Also, Barrera-Osorio et al. (2020) find no improvements in inputs or test scores for private USE schools based on exams administered by their research team. Similarly, my data depicts minimal differences in test-scores between public and private USE schools. Table A8 shows that students entering private and public USE schools have similar Primary Leaving Examination scores, differing by only 0.8 points out of 36. When they complete lower secondary education, the difference in their Uganda Certificate of Education exam scores is just 0.7 points out of 80. This suggests that students with comparable prior performance enter both private and public low-cost schools, and their scores remain similarly aligned upon completion of lower secondary education. Given these small differences, I can confidently analyze heterogeneous effects between public and private USE schools without concern that variations in school quality are influencing the results, as neither type of USE school is performing significantly better than the other.

Fourth, I measure exposure to USE based on the number of schools rather than their capacity. For example, district A has one USE school that accommodates 1,000 secondary students, while district B has two USE schools with only 200 students each. If I ignore capacity, district B could be incorrectly classified as a high-exposure district to USE simply because it has more schools, even though district A actually has more student capacity. To address this, I test whether the average number of students enrolled in secondary USE schools varies significantly between low and high-exposure districts. As shown in Table A6, the differences in the average school size are minimal (about 40 students). In addition, equations 2 and 3 account for treatment intensity by assessing the effect of having an additional private or public USE school, regardless of their capacity.

# 4 Results

In this section I present the results. First, I show the average treatment effects and their respective pre-trends. Then, I analyze heterogeneous treatment effects, along with their associated pre-trends. To validate the findings, I employ an alternative set of difference-in-differences estimators, conduct a sub-county level analysis, and perform placebo tests and robustness checks. Finally, I rule out the possibility that the main effects of USE on educational outcomes are driven by private non-USE and public non-USE schools.

## 4.1 Average treatment effects

Table 1 shows the average treatment effects using the classical difference-in-differences estimator (equation 1). The first two columns indicate that USE improved the likelihood of transitioning from primary to secondary education by 6 percentage points, while columns (3) and (4) reveal that the probability of completing lower secondary education has increased by 8 percentage points. There appears to be no effect on completing upper secondary education in columns (5) and (6). Overall, USE increased the number of schooling years by 0.30 to 0.31, significant at the 10% level, in columns (7) and (8). This equals, approximately, a 10% increase in secondary school attainment.

To check for pre-trends, I interact each birth cohort with a dummy variable indicating if a district is a high-exposure district. The comparison group is the birth cohort of 1987. The results in Figure A6 show that pre-trends hold in relation to schooling years and attending secondary or lower secondary education. However, most post-1993 coefficients are insignificant suggesting that the average treatment effects may only be weakly identifiable. This indicates the need for an analysis of heterogeneous treatment effects, as the impacts may vary across different groups.

Table 1: **The effects of USE on secondary education outcomes**

| | Attending Secondary Education | | Completing Lower Secondary Education | | Completing Upper Secondary Education | | Years of Schooling | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Cohort × Exposure | 0.06* | 0.06* | 0.08** | 0.08** | 0.00 | -0.01 | 0.30* | 0.31* |
| | (0.04) | (0.03) | (0.04) | (0.04) | (0.03) | (0.03) | (0.16) | (0.16) |
| Region linear trends | no | yes | no | yes | no | yes | no | yes |
| Poverty*birth | no | yes | no | yes | no | yes | no | yes |
| Mean | 0.76 | 0.76 | 0.49 | 0.49 | 0.24 | 0.24 | 9.90 | 9.90 |
| Observations | 6,846 | 6,846 | 6,846 | 6,846 | 6,846 | 6,846 | 4,676 | 4,676 |
| $R^2$ | 0.15 | 0.16 | 0.20 | 0.21 | 0.20 | 0.21 | 0.24 | 0.25 |

**NOTE**: Levels of significance: *<0.10; **<0.05; ***<0.01. This table shows the effects of USE on eligible individuals residing in high-exposure districts compared to low-exposure districts. The outcome variables in columns (1) to (6) are binary indicators, where one indicates that the respondent attended some secondary education, completed lower or upper secondary education. Columns (7) and (8) represent the total number of years of schooling a respondent completed. The sample consists of 6,846 respondents, at least 18 years old, and born between 1987 and 1998. For upper secondary education, the sample consists of 4,676 respondents. Exposure is a dummy that takes the value one if districts have on average more than the median number of USE schools per 10,000 primary students between 2007 and 2014. Cohort equals one if a respondent is born after 1992. All regressions include survey round, birth year, and district fixed effects. Control variables are gender, urban status of the household, household size, a wealth index of the household [1-5], ethnicity, and education of the household head. I also include birth year fixed effects, district fixed effects, and, depending on the specification, regional linear trends and the 2006 poverty rate per district interacted with each birth cohort. There are 79 districts, and standard errors are clustered at the district level. All regressions include cross-sectional DHS survey weights to ensure that the sample is representative at the national level.

## 4.2 Heterogenous treatment effects

I account for treatment intensity by applying equations 2 and 3. The interaction term "Exposure × Schools$^{i}$" captures the effect of increasing the number of private or public USE schools per 10,000 primary students by one. In practice the coefficients present the approximate impact of adding one private or public USE school per 700 P7 students at the district level.[13]

Table 2: **The effects of private and public USE schools on educational outcomes**

| | Attending Secondary Education | | Completing Lower Secondary Education | | Completing Upper Secondary Education | | Years of Schooling | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Cohort × Schools$^{public}$ | 0.05 | 0.05 | 0.01 | -0.01 | -0.06*** | -0.07*** | 0.04 | -0.05 |
| | (0.03) | (0.03) | (0.05) | (0.05) | (0.03) | (0.03) | (0.20) | (0.20) |
| Cohort × Schools$^{private}$ | 0.07** | 0.07* | 0.12*** | 0.13*** | 0.01 | 0.01 | 0.37** | 0.40** |
| | (0.04) | (0.04) | (0.03) | (0.03) | (0.04) | (0.04) | (0.16) | (0.17) |
| Mean | 0.76 | 0.76 | 0.49 | 0.40 | 0.24 | 0.24 | 9.90 | 9.90 |
| Region linear trends | yes | yes | yes | yes | yes | yes | yes | yes |
| Unemp.*birth | yes | yes | yes | yes | yes | yes | yes | yes |
| Poverty*birth | yes | yes | yes | yes | yes | yes | yes | yes |
| Observations | 6,846 | 6,846 | 6,846 | 6,846 | 6,846 | 6,846 | 4,676 | 4,676 |
| $R^2$ | 0.16 | 0.17 | 0.22 | 0.23 | 0.22 | 0.22 | 0.27 | 0.27 |

**NOTE**: Levels of significance: *<0.10; **<0.05; ***<0.01. This table shows the effect of one additional private or public USE school per 10,000 primary students on educational outcomes. The variable Schools$^{j}$ refers to the number of private or public USE schools per 10,000 primary students in the respondent's district at the age of 14 and cohort is a dummy variable that equals one if the respondent is born after 1992. See Table 1 for the explanations of the sample, the outcome and control variables. There are 79 districts, and standard errors are clustered at the district level. All regressions include cross-sectional DHS survey weights to ensure that the sample is representative at the national level.

Table 2 shows one additional private USE school significantly improves the likelihood of transitioning to secondary school and to complete lower secondary education by 0.07 to 0.13

[13] On average, 7% of all primary school students in Uganda are in their final grade. This translates to approximately 700 P7 students for every 10,000 primary school students at the district level.

percentage points. Total schooling years increase further by 0.40, significant at the five percent level. In contrast, there is no (positive) effect of public USE schools on educational outcomes. There appears to be a strong negative impact on the probability of completing upper secondary education. However, the pre-trend for upper secondary education with regard to public USE schools does not hold. I find strong negative effects for birth cohorts that were not eligible to USE with regard to upper secondary education, indicating that the observed treatment effect is likely not driven by the program itself.

Table 3 presents the effect of one additional private or public USE school for male and female students. An increase in the number of public USE schools per 10,000 primary students has no (positive) effect on educational outcomes for both gender.

Table 3: **The gender impact of private and public USE schools on educational outcomes**

| | Attending Secondary Education | | Completing Lower Secondary Education | | Completing Upper Secondary Education | | Years of Schooling | |
|---|---|---|---|---|---|---|---|---|
| Cohort × PublicUSE | 0.06 | 0.06 | 0.02 | 0.00 | -0.11*** | -0.13*** | 0.05 | 0.00 |
| | (0.04) | (0.04) | (0.05) | (0.05) | (0.04) | (0.04) | (0.20) | (0.20) |
| Cohort × PublicUSE × Male | 0.00 | 0.00 | -0.04 | -0.04 | 0.00 | 0.01 | -0.15 | -0.13 |
| | (0.03) | (0.03) | (0.03) | (0.03) | (0.03) | (0.03) | (0.13) | (0.12) |
| Cohort × PrivateUSE | 0.12** | 0.11** | 0.19*** | 0.17*** | 0.12** | 0.11** | 0.66*** | 0.64*** |
| | (0.05) | (0.05) | (0.04) | (0.04) | (0.05) | (0.05) | (0.19) | (0.19) |
| Cohort × PrivateUSE × Male | -0.10** | -0.10** | -0.11** | -0.11*** | -0.07 | -0.08 | -0.52*** | -0.53*** |
| | (0.04) | (0.04) | (0.04) | (0.04) | (0.05) | (0.05) | (0.19) | (0.18) |
| Region linear trends | no | yes | no | yes | no | yes | no | yes |
| Poverty*birth | no | yes | no | yes | no | yes | no | yes |
| Mean | 0.76 | 0.76 | 0.49 | 0.49 | 0.24 | 0.24 | 9.90 | 9.90 |
| Observations | 6,844 | 6,844 | 6,844 | 6,844 | 4,676 | 4,676 | 6,844 | 6,844 |
| $R^2$ | 0.13 | 0.13 | 0.17 | 0.18 | 0.17 | 0.18 | 0.19 | 0.20 |

**NOTE**: Levels of significance: *<0.10; **<0.05; ***<0.01. Please refer to tables 1 and 2 for further explanations.

The effects of private USE schools, on the other hand, differ by male and female. For female respondents, private USE schools increase the transition to secondary education by 12 percentage points, the completion of lower secondary education by 19 percentage points, and of upper secondary education by 12 percentage points. Completed schooling years further rise by 0.66. All point estimates are statistically significant, indicating that private USE schools are particularly effective in boosting educational attainment for female students. The interaction term "Cohort × PrivateUSE × Male" is negative and significant. This implies that the overall effect for male students is considerably smaller: an increase of 0.14 years in schooling, of 2 percentage point in secondary school attendance, of 8 percentage points in completing lower secondary and of 5 percentage points in completing upper secondary education.

To check for pre-trends, I interact each birth cohort in equation 2 with the number of private and public USE schools available at the district level in 2014. A similar approach is applied in equation 3 to examine the effects of private USE schools specifically on female students. Detailed specifications and explanations are provided in the appendix (equations 6 and 7). In Figure A7, I plot the interaction coefficients for each birth cohort with respect to private USE schools. In Figure A8, I plot the interaction coefficients for each birth cohort, focusing on private USE schools and female students. The reference category is the 1987 birth cohort. Reassuringly, all pre-trends hold.

## 4.3 Validation of the results

I conduct different tests to validate the results. First, I test whether the average treatment effects hold when using alternative difference-in-differences estimators. Second, I apply the treatment intensity specification from equation 2 at the subcounty level and apply the difference-in-differences estimator from Gardner (2022). Third, I conduct various placebo tests and robustness checks. Finally, I assess potential contamination effects from private and public non-USE schools.

### 4.3.1 Alternative difference-in-differences estimators

First, I verify that the results hold when using alternative difference-in-differences estimators. Table A9 contains the point estimates derived from the estimators proposed by de Chaisemartin and D'Haultfœuille (2023); Gardner (2022); Sant'Anna and Zhao (2020). The point estimates procured from these estimators are marginally higher and remain significant compared to the main estimates in Table 1.

### 4.3.2 Sub-county level analysis

One potential criticism is that measuring exposure to USE at the district level may be too broad, as schooling markets typically operate at a more localized level, such as sub-counties. To address this, I modify equation 2 by replacing the number of private and public USE schools per 10,000 primary students at the district level with the absolute number of private and public USE schools available in the respondent's sub-county in 2014.[14]

This specification may underestimate the true treatment effect for two reasons. First, I cannot account for migration patterns at the sub-county level. To mitigate this bias, I include a dummy variable indicating whether a respondent relocated districts between the age of 14 and the time the LSMS survey was conducted. Second, respondents who attended secondary school in 2007 may be misclassified as exposed to a private USE school in their sub-county, even if that school only joined USE later.[15] To address the fact that I cannot control for the exact timing at the sub-county level, I use the alternative difference-in-differences estimator from Gardner (2022) that accounts for treatment heterogeneity across cohorts.[16]

The Gardner estimator confirms previous findings, showing that the main treatment effects are driven by private USE schools. For example, one additional private USE school at the

[14]Uganda's administrative structure is organized into districts, which are subdivided into counties, and further into sub-counties. In 2006, Uganda had approximately 958 sub-counties.

[15]The number of available USE schools at the subcounty level is taken from the Annual School Census data, which details whether a school was part of USE in 2014, but not when exactly the school joined USE.

[16]Detailed explanations are to be found in Table A10.

sub-county level increases schooling years by 0.28 (Table A10), which is slightly lower than the estimate in Table 2. There are no significant treatment effects with regard to public USE schools and Figure A9 confirms pre-trends.

#### 4.3.3 Placebo tests

I conduct two placebo tests. I restrict the sample to respondents born prior to the implementation of USE, specifically between 1987 and 1992. The first placebo treatment cohort is comprised of those born between 1991 and 1992, and the second spans from 1989 to 1992. I re-run equation 1 and find that none of coefficients is significant in Table A13. This confirms the previous set of results.

#### 4.3.4 Robustness checks

I run a battery of robustness checks. First, I omit respondents from the analysis that were born in 1991 and 1992. This guarantees that the treatment effects are not driven by cohorts that received partial treatment. Table A14 shows that the point estimates become slightly stronger. Therefore, the results are not attributable to the 1991 and 1992 cohorts.

Second, I restrict the sample to respondents who are no longer enrolled in secondary education. I expect the treatment effects to increase, as younger respondents in the treated cohorts may have benefited from USE but did not yet complete their education. Table A15 shows that the treatment effect increases to 0.46 (as compared to 0.30 in Table 1).

Third, I estimate equation 2 only including households, where at least one sibling was eligible and another was not. This approach enables the inclusion of household fixed effects, and thus accounting for household-specific factors. This results in a substantial reduction in sample size. Reassuringly though, shows the estimated coefficients remain significant for private USE schools in Table A16.

#### 4.3.5 Assessing contamination effects of private and public non-USE schools

I now examine the impact of private non-USE schools entering the market after 2007. Figure A1 demonstrates that the number of private non-USE schools continues to increase starting in 2007. This expansion could potentially bias the effects of USE, as some educational gains might be attributed to the growing supply of high-cost private non-USE schools. However, two factors suggest this is unlikely: first, the rise in the number of non-USE private schools does not correlate with an increase in the proportion of students attending these schools (Figure A3). This suggests that the proliferation of non-USE private schools may primarily be a reaction to the overall growth in secondary student numbers. Second, the accessibility and affordability of secondary education remain critical factors in influencing school choice among families, especially in rural areas. Since the majority of educational gains can be attributed to private USE schools, which are predominantly situated in rural areas, the prospect of more costly private non-USE schools finding a receptive market in these areas should be low (MOES, 2016). Therefore, it is more likely that private non-USE schools cater to a small group of wealthier students, who would have pursued secondary education regardless of the fee reduction policy (Donoghue et al., 2018).

In order to rule out the possibility that private non-USE schools drive the treatment effects, I re-estimate equation 2 with an interaction term between the cohort dummy and the number of private non-USE schools per district at age 14. Table A11 shows the effect on educational outcomes of each additional private non-USE-school per 10,000 primary students after 2007. None of the coefficients are significant and are close to zero, suggesting that private non-USE schools do not drive the treatment effects.

Last, the treatment effects may be influenced by a small number of elite public schools that opted out of USE. To account for this, I exclude respondents from districts with a high proportion of government non-USE schools. This ensures that the results are not skewed by elite public non-USE schools.[17] The results in Table A12 are consistent with previous

[17] The excluded districts, in addition to Kampala, are Wakiso, Mbarara, Jinja, and Mpigi.

estimates, indicating that the effects are not driven by public non-USE schools.

# 5 Mechanisms

The mechanism section addresses two key points: First, I explore why private USE schools achieve greater educational gains than public ones, focusing on both the supply and demand aspects of secondary education in Uganda. Second, given the government's goal of providing every student with access to a low-cost school within a five kilometer walking distance, I analyze the locations where school fee reduction policies are most effective, specifically examining whether students living closer to or farther away from schools benefit more.

## 5.1 Supply and demand side responses of public and private USE schools

Both demand- and supply-side factors may contribute to the different educational outcomes observed with regard to public and private USE schools. At the demand-side, I discuss household preferences for public and private USE schools, while at the supply side I focus on the ability of public and private USE schools to accommodate more students.[18]

Current research suggests that parents perceive the benefits of public secondary education to be lower compared to private secondary education in Uganda. This perception is shaped largely by existing conditions in public schools, such as overcrowded classrooms, inadequate infrastructure, and high teacher absenteeism (Wane, 2014; Bashir et al., 2018). Indeed, Table A1 shows that public USE schools had higher student-to-teacher and student-to-classroom ratios and greater overall enrollment prior to the introduction of USE. Consequently, even with reduced tuition fees, some parents may still decide not to send their children to public USE schools.

[18] Due to a lack of data, I only provide suggestive evidence without establishing causality.

From a supply-side perspective, when fee reduction policies elevate demand for schooling, schools are expected to respond by expanding their capacity. This involves, for example, building additional classrooms or new facilities to accommodate the growing number of students. The literature has shown that private schools relying on enrollment-based funding, are often more responsive to demand fluctuations and exhibit greater flexibility in scaling their operations compared to public schools (Patrinos et al., 2009; Evans and Mendez Acosta, 2021; Lemos et al., 2024). Public schools, on the other hand, often operate under strict budgetary constraints and have fewer incentives to accommodate additional enrollment, as their funding is not primarily student-based but comes largely from the government.

These reasons could explain the limited improvements in access to secondary education with regard to public USE schools in Uganda. Notably, my data shows that private USE schools experienced larger increases in student enrollment, as well as in the number of teachers and classrooms, suggesting they were more flexible in scaling their operations compared to public schools (see Table A1). In addition, there is evidence from the World Bank that public schools struggle with slow procurement processes and insufficient public funding to build additional classrooms and school infrastructure, while private schools are able to do this more rapidly (Tsimpo and Wodon, 2016).

Therefore, both demand- and supply-side factors suggest that private USE schools are better positioned to accommodate more students and attract greater demand. Further research is needed to fully understand the mechanisms driving these differences.

## 5.2 Distance to private USE schools

This section proceeds by analyzing the demand for private USE schools focusing on distance and gender-specific responses. I concentrate on private USE schools rather than public ones, as the majority of educational benefits from USE have been observed in private USE schools.

For this, I exploit the geolocations of respondents' households from the LSMS data to compute the distance to the nearest private USE, but also to the nearest public USE, private non-USE and public non-USE school using data from the 2014 Annual School Census.[19] The distance to the nearest public USE, private non-USE and public non-USE school are used as controls in this exercise.

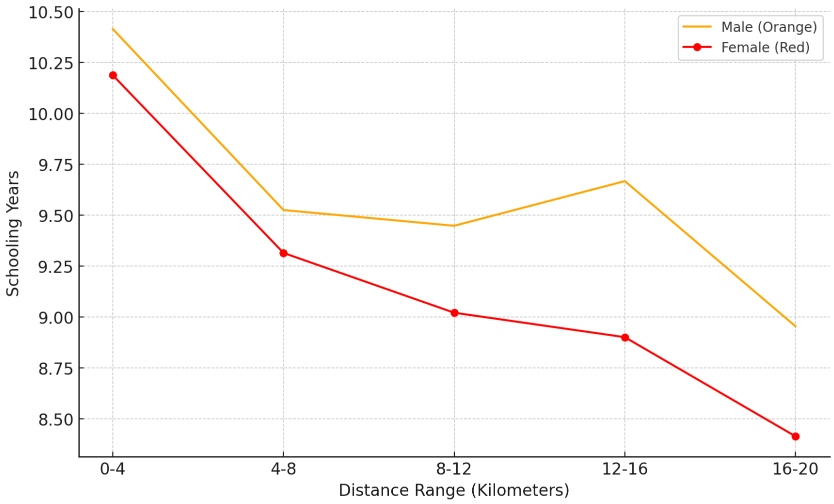
The LSMS data only records household geolocations at the time of the LSMS survey. Therefore, I restrict the sample to respondents who lived at their current residence since the age of 14, ensuring an accurate representation of household location during the period when respondents were attending secondary school. I follow Muralidharan and Prakash (2017) and drop all respondents who live more than 20 kilometers away from a private USE school by arguing that any effect of a private USE school approaches zero above distances of 20 kilometers.[20] The final sample consists of 3321 respondents with a median distance to a private USE school of 5.3 kilometers.

Figure 4 shows that educational attainment improves as the distance to the nearest secondary school, whether public USE, private USE, public non-USE, or private non-USE, decreases. Educational attainment of female respondents consistently falls behind that of males, with both declining as the distance to schools increases. This pattern validates the accuracy of my distance computation and corresponds to the literature showing that an increase in distance correlates with a decrease in educational attainment (Evans et al., 2023, 2024).

[19]While the geolocations in the LSMS data are generally accurate, some households are randomly displaced by up to five kilometers to ensure privacy. This introduces measurement error, which I assume is randomly distributed across households, thereby minimizing any systematic bias in the results.

[20]For 90% of respondents the distance to a private USE school is below 20 kilometers.

Figure 3: **Completed schooling years by distance to closest secondary school**




**NOTE**: Distance refers to the proximity of the nearest private USE, public USE, private non-USE and public non-USE school available to the 3,321 respondents in the LSMS dataset.

I now estimate the threshold effect of the USE policy by calculating the impact of private USE schools on respondents living within five kilometers of a private USE school, given the program's objective of providing access to low-cost schools within a five-kilometer walking distance. I conduct a triple difference regression to examine the impact of distance to private USE schools on educational outcomes, with a focus on gender-specific effects.[21]

$$
\begin{aligned}
Y_{irbd} = \beta_0 + \beta_1 \times Male_{irbd} + \beta_2 \times Distance_{irbd}^{Private} \\
+\beta_3 \times Male_{irbd} \times Distance_{irbd}^{Private} + \beta_4 \times Cohort_b \times Male_{irbd} \\
+\beta_5 \times Cohort_b \times Distance_{irbd}^{Private} + \beta_6 \times Cohort_b \times Distance_{irbd}^{Private} \times Male_{irbd} \\
+\omega_1 \times Distance_{irbd}^{Public} + \omega_2 \times Cohort_b \times Distance_{irbd}^{Public} \\
+\omega_3 \times Distance_{irbd}^{NonUSE} + X_{irbd} \times Male_{irbd} + \delta_d + \alpha_b + \gamma_r + \epsilon_{irtd},
\end{aligned} \tag{4}
$$

where $Y_{irbs}$ measures the number of educational years and "Cohort" is a dummy equal to one if a respondent was eligible for the USE program. Based on Muralidharan and Prakash

[21] Ideally, I calculate the increase in years of education following the introduction of USE, disaggregated by gender and distance from a private USE school for each kilometer. However, the small sample size limits the statistical power needed to obtain reliable coefficients for each kilometer.

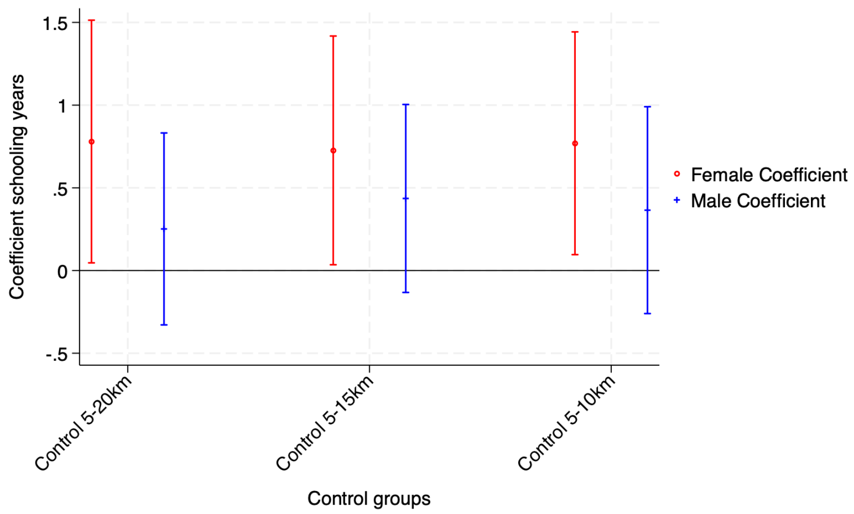
(2017), I code $Distance_{irbd}^{Private}$ as a dummy equal to one if the distance to a private USE school is below five kilometers. Therefore, my treatment group consists of respondents that have access to a private USE school within five kilometers, while the control group includes respondents with a private USE school that is between five and twenty kilometers away. Robustness is tested by further restricting the control group to respondents that have a private USE school within five to fifteen or five to ten kilometers.

In equation 4, $\beta_1$ represents the baseline differences in years of schooling between male and female respondents. $\beta_2$ captures the effect of having a private USE school within five kilometers for female students prior to the implementation of USE on years of schooling, while $\beta_3$ reflects the difference in this effect for male students. $\beta_4$ measures the change in years of schooling for male students before and after USE implementation. The key coefficient of interest is $\beta_5$ capturing the effect of USE on female students with a private USE school within five kilometers, and $\beta_6$ reflecting the difference in this effect with regard to male students.

I control for the distance to public USE schools (in km) both before and after the implementation of USE, as well as for the distance to any public and private non-USE schools, captured by $\omega_1, \omega_2$, and $\omega_3$. I include the same control variables as before: an urban dummy, household size, household head's education, ethnicity and the wealth index. All control variables are interacted with a male dummy to capture gender-specific effects. In addition, I include birth year, district, and survey round fixed effects and cluster standard errors at the district level.

Table A17 presents the complete set of coefficients of the saturated model. Reassuringly, all coefficients remain relatively stable regardless of the control group used or the inclusion of the interacted controls. To provide a visual representation, Figure 4 plots the overall treatment effects, along with 90% confidence intervals, for female and male students living within five kilometers to a private USE school. These estimates are taken from columns (1), (3), and (5) of Table A17 and are compared against three different control groups: students with a private USE school located between five and twenty kilometers, five and fifteen kilometers, and five and ten kilometers away.

Figure 4: **Distance to private USE schools and completed schooling years**




**NOTE**: This Figure is based on columns (1), (3) and (5) of Table A17. It shows the total treatment effects for female students and for male students, as well as the 90 % confidence intervals. All female coefficients are significant at the ten percent level.

The main treatment effects of USE are significant for female students with access to a private USE school within five kilometers. These coefficients are significant at the 10% level and are larger in magnitude compared to those for male students. It is important to note that all coefficients likely represent lower bounds of the true treatment effect.[22] Importantly, treatment effects are consistent across control groups, indicating that the policy's impact is strongest within 5 kilometers to a private USE school. The pre-trends also hold as explained in equation 9 and shown in Figure A10.

Overall, USE has a stronger impact on female students living closer to private USE schools. In this regard, the design of placing private USE schools within 5 kilometers to primary schools, if possible, has been largely successful. However, female students living further away still face considerable challenges. Restrictive gender norms, long travel distances that increase safety

[22]Since the distance to private USE schools was calculated using data from 2014, there is uncertainty regarding the timing when each school joined the USE program. Some female students who were eligible for USE and attended a private secondary school in 2008 within five kilometers to their household may not have profited from the program, as this private school only joined USE later.

concerns, but also reduce the time available for household responsibilities, may limit girls to fully benefit from USE (Evans et al., 2023, 2024). To better support these students, additional measures, such as improved transportation options, may be necessary to enhance their access to education.

### 5.3 Cost-effectiveness analysis

It is important to place USE within the broader context of other school fee reduction initiatives, particularly in low-income countries. The introduction of such programs can present significant financial challenges for governments, highlighting the necessity for a comprehensive assessment of their benefits and costs. Given that over 80% of schools, both public and private, were already established before the implementation of USE, my analysis does not consider the costs of constructing new USE schools. Instead, I assume that the existing school infrastructure is already in place.

The USE program incurred a total cost of approximately USD 41 million during the 2013/2014 school year, with USD 22.62 million allocated to public schools and USD 18.33 million to private schools for lower and upper secondary education (MOES, 2015). This amount covered both the capitation grant and administrative fees. With a total enrollment of 471,726 students in public schools and 335,266 students in private USE schools, the annual cost translates to USD 51 per student. On average USE increased schooling years by 0.30 per student (Table 1). Scaling up, the cost of providing an additional year of schooling would be USD 170 per student annually. This estimate is considerably lower than the scholarship program tested by Duflo et al. (2021), which is approximately USD 380 per additional year, or the fee reduction initiative for public schools in Kenya, which incurred a cost of about USD 425 per additional year (Brudevold-Newman, 2017).

Recent critiques from the World Bank challenge the validity of assessing interventions based exclusively on their impact on schooling years (Angrist et al., 2020; Filmer et al., 2019). For instance, an intervention may yield more learning years for USD 100 in country A compared to country B, but students actually learn more in country B than in country A. Subsequently, policy makers may be interested to evaluate interventions based on schooling years and actual learning gains.

Therefore, I apply a Learning-Adjusted Years of School model (LAYS) that allows to compare educational outcomes in terms of schooling and learning gains with other interventions worldwide against an absolute, cross-country standard. I follow the methodology proposed by Angrist et al. (2020) to compute LAYS and describe the standard-approach below:

$$\text{LAYS}_i = \gamma_i \times L_i^h \times t, \tag{5}$$

where $\gamma_i$ represents additional schooling years per USD 100 that an intervention brings in country $i$. $L_i^h$ measures learning gains for students in country $i$ relative to a high-performing benchmark $h$, where $L_i^h = \frac{L_i}{L_h}$. $L_i$ denotes learning gains in country $i$ and $L_h$ learning gains in country $h$. In this regard, I suppose that USE led to an improvement of 0.59 schooling years per USD 100 ($\gamma_i$), and that students in Uganda learn on average 70% of what students are learning in a high-performing country, such as the United Kingdom ($L_i^h$) in one year.[23] Since the USE grant is paid every year, I also presuppose that any effects of USE last a maximum of one year ($t$).

Plugging the numbers into equation 5 yields 0.41 LAYS. This estimate positions USE in the medium effectiveness range when using LAYS as a metric scale and scores above the provision of scholarships by Duflo et al. (2021).[24] I argue that the cost-effectiveness of the USE program can be enhanced by implementing targeted subsidies, particularly for female students or for schools with adequate capacity to accommodate the increased demand.

[23]The exact methodology and learning values are explained in the paper by Angrist et al. (2020), page 9.

[24]For comparison, please refer to Figure 6, p.22 in Angrist et al. (2020).

# 6 Conclusion

This paper investigates the impact of Uganda's Universal Secondary Education (USE) program, a secondary school fee reduction initiative. By exploiting district- and cohort-level exposure, I find that the USE policy led to an average increase of at least 0.30 years of schooling, as well as an increased likelihood of transitioning to secondary education and completing lower secondary education. However, heterogeneous treatment effects reveal that most educational gains occurred in areas with private low-cost schools compared to public ones, particularly benefiting female students residing in close proximity to these private schools, with an estimated increase of 0.75 years of additional schooling.

My findings show that an untargeted fee reduction program can have wide-ranging heterogeneous effects in a low-income setting. Given the limited impact on male respondents and on students close to public schools, this suggests that targeted interventions could be more effective and cost-efficient. For example, subsidizing schools with higher demand for education based on distance considerations or schools with more capacity may yield better results than subsidizing all schools equally. This approach, for example, could free up resources to further reduce educational costs or improve transportation infrastructure.

In conclusion, this paper provides a pioneering analysis of the impact of a fee reduction policy on both private and public schools and gender responses in Uganda. It highlights the need to better understand the heterogeneous effects of such a policy to improve overall educational outcomes. Notably, the current phasing-out of USE funding to private schools appears to be at odds with the findings of this research (MOES, 2021).

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# Appendix

## A.1 Appendix - descriptive statistics

Table A1: **School characteristics of public and private USE schools**

| School-type | Year: 2004 | Year: 2014 | Total change | School numbers |
|---|---|---|---|---|
| Student-to-teacher ratio: | | | | |
| Public USE | 21.4 | 22.2 | 0.8 | 335 |
| Public non-USE | 21.2 | 18.8 | -2.4 | 43 |
| Private USE | 17.3 | 23.6 | 6.3 | 349 |
| Private non-USE | 18.4 | 17.7 | -0.7 | 142 |
| Student-to-classroom ratio: | | | | |
| Public USE | 52.3 | 61.7 | 9.4 | 355 |
| Private USE | 46.0 | 65.6 | 19.6 | 354 |
| School-size | | | | |
| Public USE | 461 | 692 | 231 | 355 |
| Private USE | 275 | 597 | 322 | 354 |

**NOTE**: This table was computed by using Annual School Census data from 2004 and 2014. I constructed a panel dataset of schools that were available in both datasets. Using fuzzy matching on district and school names, I matched 335 public and 349 private USE schools in both years, resulting in a matching rate of about 35%. The low matching rate is coming from the fact that some schools were founded after 2004 or did not participate in the Annual School Census from 2004 as it was not obligatory for private schools to do so. Overall, this table shows the average student-to-teacher ratio, student-to-classroom ratio and enrollment numbers in 2004 and 2014.

Table A2: **Distance of primary schools to public USE schools (group 1) and to public and private USE schools (group 2) in 2014**

| | Obs. | Group 1 | Group 2 | Difference | Std. Err. | T-value | P-value |
|---|---|---|---|---|---|---|---|
| Distance | 16884 | 5.80 | 4.46 | 1.34 | 0.05 | 26.95 | 0.00*** |

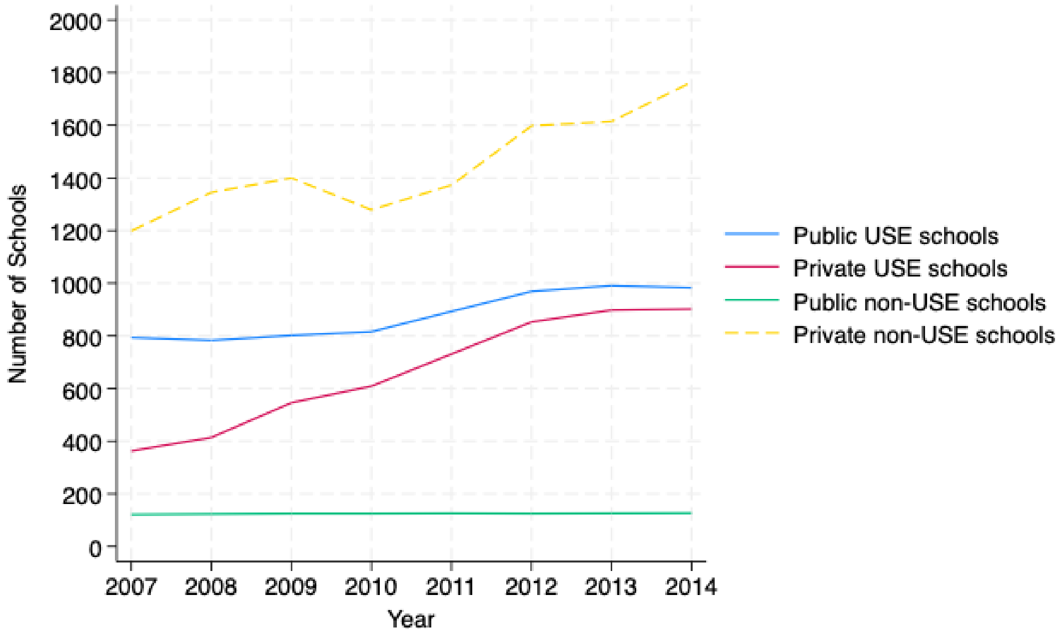
**NOTE**: Levels of significance: *<0.10; **<0.05; ***<0.01. This table presents the results of a two-sided paired t-test comparing the distances of 16,884 primary schools in the 2004 Annual School Census to the nearest public USE school (group 1) and the nearest public or private USE schools (which ever school is closer) in the Annual School Census data in 2014. The table reports the average distance in km, the difference in means, the standard error, the t-statistic, and the corresponding p-value.

Table A3: **Average schooling costs per school-type and year for secondary education in USD**

| School year | Measurement | Private USE | Public USE | Private non-USE | Public non-USE |
|---|---|---|---|---|---|
| Lower secondary education | | | | | |
| 2005-2006 | Mean | - | - | 282 | 266 |
| | (Median) | | | (199) | (211) |
| 2011-2012 | Mean | 124 | 139 | 308 | 290 |
| | (Median) | (118) | (86) | (261) | (216) |
| Upper secondary education | | | | | |
| 2005-2006 | Mean | - | - | 540 | 391 |
| | (Median) | | | (443) | (344) |
| 2011-2012 | Mean | 238 | 248 | 481 | 467 |
| | (Median) | (220) | (207) | (394) | (449) |

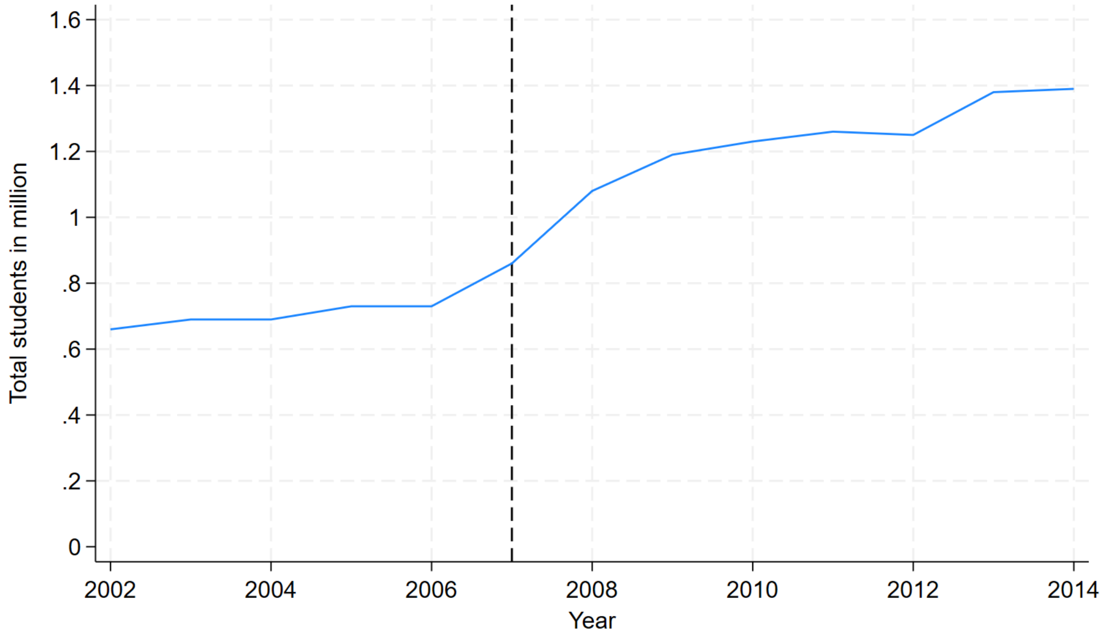
**NOTE**: This table shows the total schooling costs a student paid during the school year 2005/2006 and 2011/2012. The schooling costs were computed by using data on school expenditures from the Living Standards Measurement Survey (LSMS) from 2005/2006 and 2011/2012. Total school expenditures encompass tuition and registrations fees, costs related to transportation, school-uniforms, school books, school meals and other expenses, such as exam fees. All costs are expressed in USD (2012). The schooling costs from 2005/2006 were adjusted with the yearly inflation rate in Uganda. After the adjustment all expenses in UGX were converted into US Dollars using the exchange rate from 2012.

Figure A1: **School numbers after school-type between 2007 - 2014**



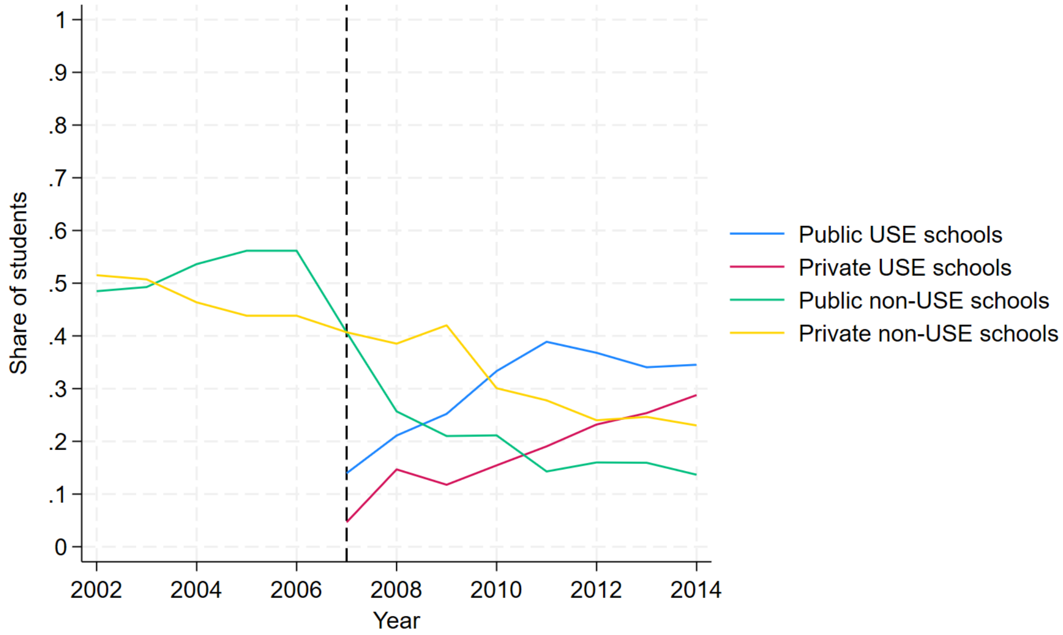

**NOTE**: This graph was computed by using data from the Ministry of Sports and Education. Please note that the number of private non-USE schools may not be accurate, as private non-USE schools were not required to participate in the Annual School Census. Potentially, I miss out on approximately 30 to 40% of all private non-USE schools, particularly in the earlier years: 2007, 2008, 2009 and 2010 (Masuda and Yamauchi, 2018).

Figure A2: **Number of students enrolled in secondary education**



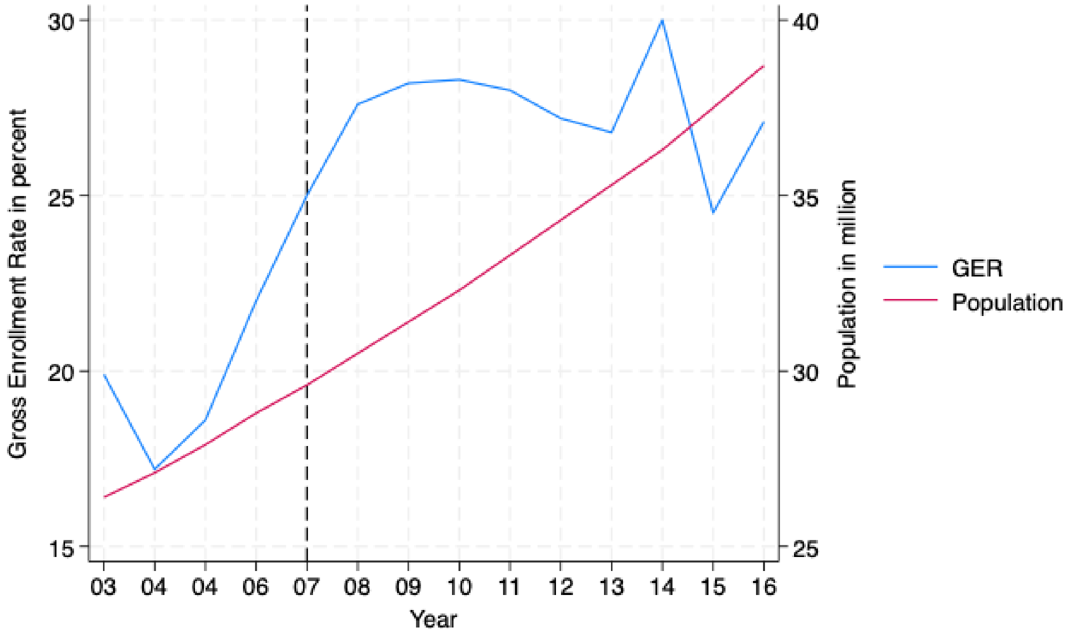

**NOTE**: This graph was computed by using offical data on student numbers from the education reports from the Ministry of Sports and Education (MOES, 2014, 2017).

Figure A3: **Number of students per school-type**



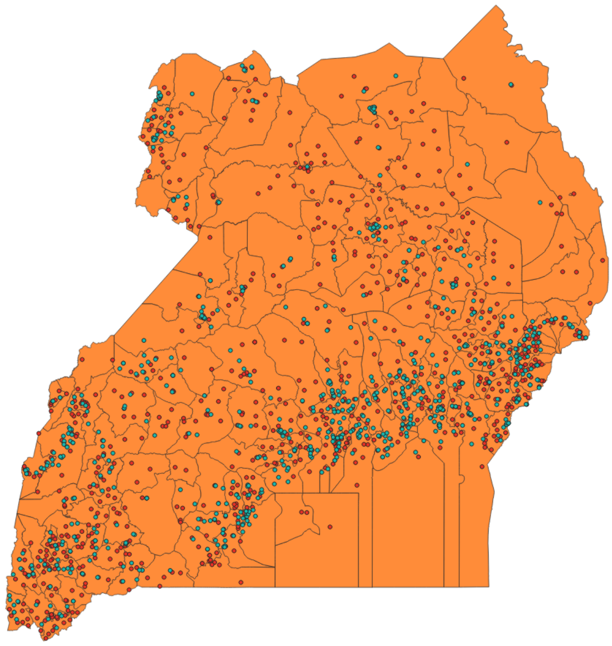

**NOTE**: This graph was computed by using offical data on student numbers from the education reports from the Ministry of Sports and Education (MOES, 2014, 2017).

Figure A4: **Secondary gross enrollment rate and total population in Uganda**




**NOTE**: This graph was computed with official data from the education reports from the Ministry of Sports and Education in Uganda (MOES, 2017).

Figure A5: **Locations of USE schools in 2014**



**NOTE**: This figure was computed with the Annual School Census data from 2014. Red points correspond to public USE schools and green points to private USE schools. District boundaries from 2006 are used.

Table A4: **Summary statistics of the sample**

| Variable | Observations | Mean | Standard deviation |
|---|---|---|---|
| **Sample: Completed primary education, 18-29 years old** | | | |
| Age | 6846 | 21.67 | 3.69 |
| Female | 6846 | 0.48 | 0.50 |
| Years of education | 6846 | 9.74 | 2.20 |
| Some secondary schooling | 6846 | 0.73 | 0.44 |
| Completed lower secondary education | 6846 | 0.41 | 0.49 |
| Completed upper secondary education | 6846 | 0.21 | 0.42 |
| Still in education | 6846 | 0.42 | 0.49 |
| Household head completed primary education | 6846 | 0.39 | 0.46 |
| Household size | 6846 | 7.00 | 3.66 |
| Wealth index [1-5] | 6846 | 3.50 | 1.39 |
| Urban household | 6846 | 0.34 | 0.47 |
| Baganda | 6846 | 0.23 | 0.42 |
| Banyankole | 6846 | 0.11 | 0.31 |
| Basoga | 6846 | 0.09 | 0.28 |
| **Sample: Completed primary education, 20-29 years old** | | | |
| Age | 4676 | 24.12 | 2.76 |
| Female | 4676 | 0.48 | 0.50 |
| Years of education | 4676 | 9.90 | 2.29 |
| Some secondary schooling | 4676 | 0.71 | 0.45 |
| Completed lower secondary education | 4676 | 0.48 | 0.50 |
| Completed upper secondary education | 4676 | 0.24 | 0.44 |
| Still in education | 4676 | 0.21 | 0.40 |
| Migrated | 4676 | 0.12 | 0.32 |
| Household head completed primary education | 4676 | 0.42 | 0.47 |
| Household size | 4676 | 6.11 | 3.73 |
| Wealth index [1-5] | 4676 | 3.40 | 1.50 |
| Urban household | 4676 | 0.32 | 0.47 |
| Migrated | 4676 | 0.12 | 0.32 |
| Baganda | 4676 | 0.21 | 0.41 |
| Banyankole | 4676 | 0.09 | 0.29 |
| Basoga | 4676 | 0.08 | 0.26 |

Table A5: **Sample balancing in high and low exposure districts to USE**

| | (1) | | (2) | | (1)-(2) | |
|---|---|---|---|---|---|---|
| | Low exposed USE-districts | | High exposed USE-districts | | Pairwise t-test | |
| Variable | N | Mean/(SE) | N | Mean/(SE) | N | Mean difference |
| Female | 4059 | 0.46 | 2787 | 0.50 | 6846 | -0.05*** |
| | | (0.01) | | (0.01) | | |
| Urban | 4059 | 0.30 | 2787 | 0.30 | 6846 | 0.00 |
| | | (0.01) | | (0.01) | | |
| Wealth index | 4059 | 3.32 | 2787 | 3.60 | 6846 | -0.28*** |
| | | (0.02) | | (0.02) | | |
| HH size | 4059 | 8.33 | 2787 | 7.97 | 6846 | 0.36*** |
| | | (0.06) | | (0.08) | | |
| HH education | 4059 | 0.37 | 2787 | 0.38 | 6846 | -0.01 |
| | | (0.01) | | (0.01) | | |
| Age | 4059 | 21.39 | 2787 | 21.33 | 6846 | 0.06 |
| | | (0.04) | | (0.05) | | |
| Iteso | 4059 | 0.08 | 2787 | 0.06 | 6846 | 0.02*** |
| | | (0.00) | | (0.00) | | |
| Baganda | 4059 | 0.12 | 2787 | 0.32 | 6846 | -0.20*** |
| | | (0.01) | | (0.01) | | |
| Banyankole | 4059 | 0.18 | 2787 | 0.03 | 6846 | 0.15*** |
| | | (0.01) | | (0.00) | | |
| Basoga | 4059 | 0.14 | 2787 | 0.02 | 6846 | 0.12*** |
| | | (0.01) | | (0.00) | | |
| F-test of joint significance (F-stat) | | | | | | 112.83*** |
| F-test, number of observations | | | | | | 6846 |

**NOTE**: The values displayed for the t-tests are the differences in the means across the groups. Birth cohort fixed effects are included to control for baseline differences across birth cohorts. High exposed districts are districts where the average number of USE schools between 2007 and 2014 exceeds the median number of USE schools per 10 000 students across all districts. Iteso, Baganda, Banyankole and Basoga are dummy variables that equal one if a respondent belongs to this ethnic group. HH size refers to the household size and HH education is a dummy indicating if the household-head has more than primary education.

Table A6: **Average differences in student and teacher numbers in USE schools in low and high exposed districts**

| | (1) Total number of students | (2) Total number of teachers | (3) Private USE |
|---|---|---|---|
| High exposure districts | -42.35* | -1.66** | -0.00 |
| | (23.23) | (0.71) | (0.04) |
| Mean | 301.22 | 19.27 | 0.32 |
| Number of schools | 1254 | 1218 | 1285 |
| Number of districts | 77 | 77 | 77 |
| $R^2$ | 0.01 | 0.01 | 0.00 |

**NOTE**: Levels of significance: *<0.10; **<0.05 ***<0.01. This table uses information on student and teacher numbers in 1254 private and public USE schools from the Annual School Census in 2019. It shows the regression coefficients of a dummy indicating if a district belongs to a high treatment districts on student and teacher numbers in those 1254 USE schools, as well as on their ownership status (public or private USE school). Standard errors are clustered at the district level.

For composition effects, I estimate equations 1 and 2 using a different outcome variable - namely, the likelihood of completing or attending P7 among respondents in the LSMS data. The sample composition is specified in the footnotes of table A7.

Table A7: **Effects of USE on completing primary education**

| | Completing Primary Education | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Cohort × Exposure | 0.03 | 0.03 | | |
| | (0.03) | (0.03) | | |
| Cohort × $Schools^{private}$ | | | 0.00 | -0.00 |
| | | | (0.01) | (0.01) |
| Cohort × $Schools^{public}$ | | | 0.01 | 0.03 |
| | | | (0.01) | (0.03) |
| Mean | 0.35 | 0.35 | 0.35 | 0.35 |
| Region linear trends | no | yes | no | yes |
| Poverty*birth | no | yes | no | yes |
| Respondents | 10,699 | 10,699 | 10,699 | 10,699 |
| $R^2$ | 0.21 | 0.21 | 0.23 | 0.23 |

**NOTE**: Levels of significance: *<0.10; **<0.05; ***<0.01. Cohort is a dummy that equals one if respondents are born in 1993 or later. Exposure is a dummy that equals one if respondents resided in districts above the median number of USE schools per 10,000 primary students (columns 1-2). In columns 3 and 4, cohort is interacted with the number of available private or public USE-schools per 10,000 primary students in the district of the respondent at age 14. The sample consists of 10,699 respondents that were sampled in the LSMS survey rounds 2-6. Respondents are born in 1987 or later and at least 14 years old. The outcome variable is a dummy that equals one if a respondent has at least completed P7 or is currently attending P7. For control variables and fixed effects used please refer to table 1. There are 79 districts, and standard errors are clustered at the district level. All regressions use DHS cross-sectional survey weights.

Table A8: **Summary statistics of PLE and UCE scores after school type**

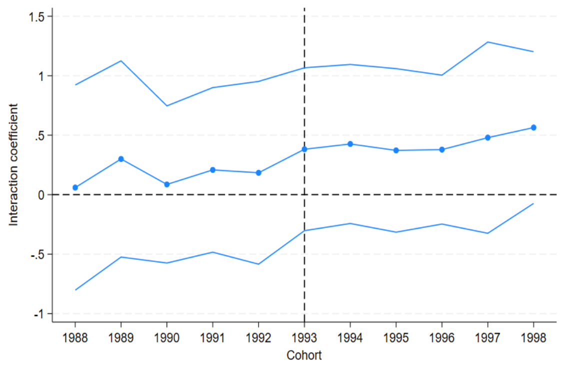
| | Private USE | Public USE | Private non-USE | Public non-USE |
|---|---|---|---|---|
| PLE | 21.9 | 21.1 | 17.9 | 12.0 |
| UCE | 58.9 | 58.2 | 51.5 | 36.7 |
| Observations | 8515 | 29104 | 11845 | 5876 |

**NOTE**: This table uses official test-scores data from the Uganda Examinations Board in 2015 and 2016. The table shows the average score of students taking the lower secondary education exit exam (UCE) in private and public USE and private and public non-USE schools. The table also shows the average score of the Primary Leaving Examination (PLE) exam for the same set of students, once they entered secondary education. The PLE score ranges from 4 to 36 points, with 4 representing the best grade that can be attained, while the UCE score ranges from 8 to 80, with 8 being the best grade.

## A.2 Pre-trends

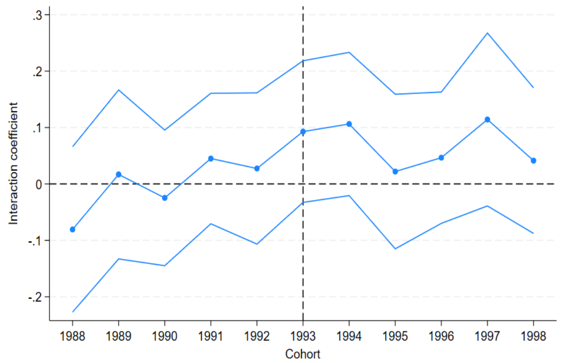
Figure A6: **Pre-trends for the average treatment effects of USE - educational outcomes**

(a) Completed schooling years

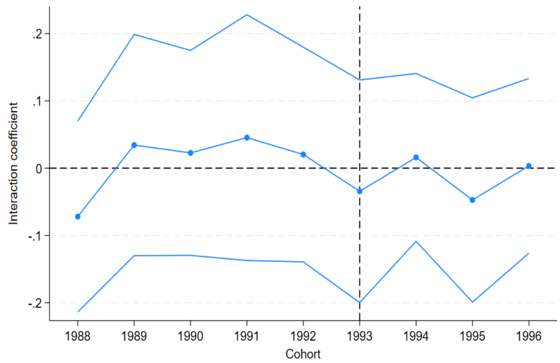



(b) Attending secondary education


(c) Completing lower secondary education




(d) Completing upper secondary education




**Note**: These figures interact each birth cohort with a dummy indicating if the district of the respondent belongs to a high or low exposed USE-district. The comparison group is the birth cohort of 1987. Plotted are the interaction coefficients, along with 95% confidence intervals. Control variables are gender, urban status, wealth index, education of the household head and household size. Further included are birth year fixed effects and district fixed effects. Standard errors are clustered at the district level. There are 79 districts. Respondents born before 1993 were not eligible for USE and those born after were eligible to USE in the main specifications of this paper. For completed schooling years, a joint F-test of pre-program and post-program values does not reject that all values are equal to zero.

I run the following two regressions to examine pre-trends related to treatment intensity with regard to private and public USE schools:

$$
\begin{aligned}
Y^{j}_{irtd} = \beta_0 + \sum_{t=1988}^{1998} \beta_{t1} \times Cohort_t \times PrivateUSE_d \\
+ \sum_{t=1988}^{1998} \beta_{t2} \times Cohort_t \times PublicUSE_d + X_{irtd} + \delta_d + \alpha_t + \gamma_r + \epsilon_{irtd},
\end{aligned} \tag{6}
$$

this equation captures the effect of private and public USE schools available at the district level in 2014 on all cohorts born between 1988 and 1998. Ideally, there should be no significant effect on cohorts born before 1993, with the 1987 cohort serving as the comparison group. I plot the coefficient with regard to private schools $\beta_{t1}$ in figure A7.

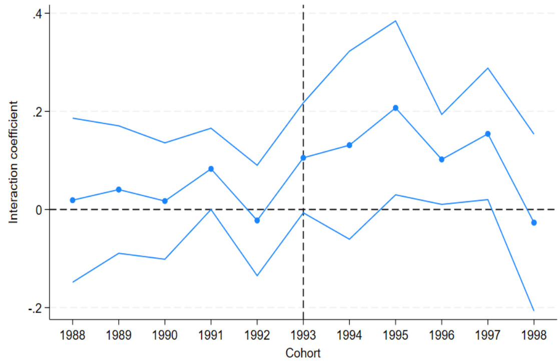
The second regressions analyzes treatment effects of private and public USE schools by gender:

$$
\begin{aligned}
Y^{j}_{irtd} = \beta_0 + \sum_{t=1988}^{1998} \beta_{t1} \times Cohort_t \times PrivateUSE_d + \\
\sum_{t=1988}^{1998} \beta_{t2} \times Cohort_t \times PrivateUSE_d \times Male_{irtd} + \\
\sum_{t=1988}^{1998} \beta_{t3} \times Cohort_t \times PublicUSE_d + \sum_{t=1988}^{1998} \beta_{t4} \times Cohort_t \times PublicUSE_d \times Male_{irtd} + \\
X_{irtd} + \delta_d + \alpha_t + \gamma_r + \epsilon_{irtd},
\end{aligned} \tag{7}
$$

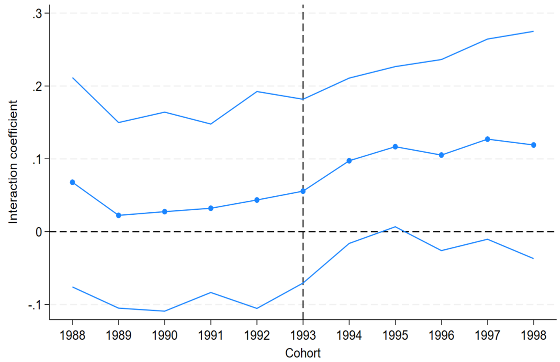
this equation captures the differential effect of private and public USE schools in 2014 at the district level for male and female students across each birth cohort. The coefficients $\beta_1$ and $\beta_3$ represent the main effect of public and private USE schools on female respondents, while $\beta_2$ and $\beta_4$ show the difference in the main effect with regard to male respondents. I plot the coefficients with regard to female students and private schools $\beta_{t1}$ in figure A8.

Figure A7: **Interaction coefficients between each birth cohort and the number of private Use schools in 2014 at the district level**

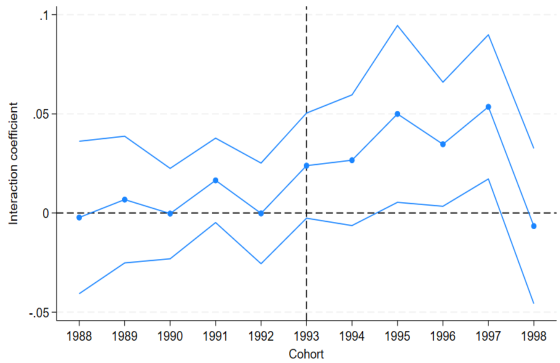
(a) Completed schooling years




(b) Attending secondary education




(c) Completing lower secondary education

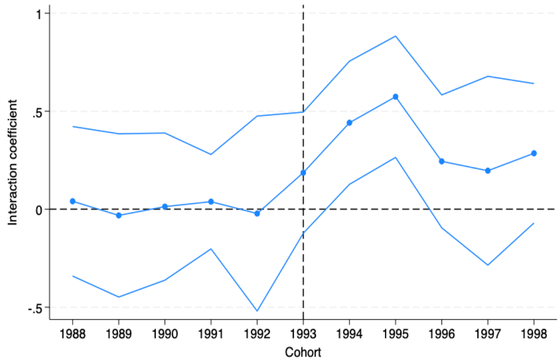



(d) Completing upper secondary education

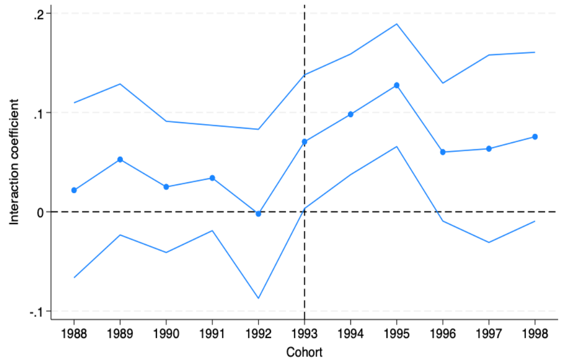

**Note**: Levels of significance: *<0.10; **<0.05 ***<0.01. These figures show the interaction coefficients between each birth cohort and the number of private USE schools per 10 000 primary students at the district level in 2014. For more information please refer to figure A6. For completed schooling years and lower secondary education, a joint F-test of pre-program values does not reject that all values are equal to zero. For both outcomes a joint F-test of post-program values rejects that all values are equal to zero.

Figure A8: **Interaction coefficients with regard to female students**

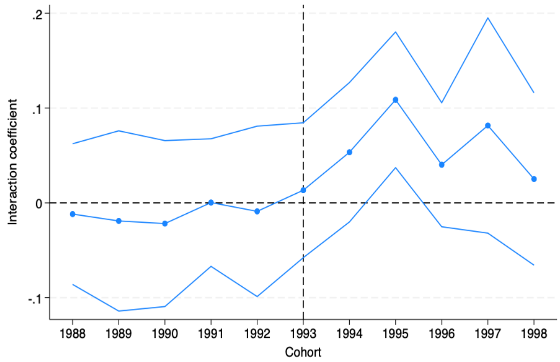
(a) Completed schooling years - female respondents



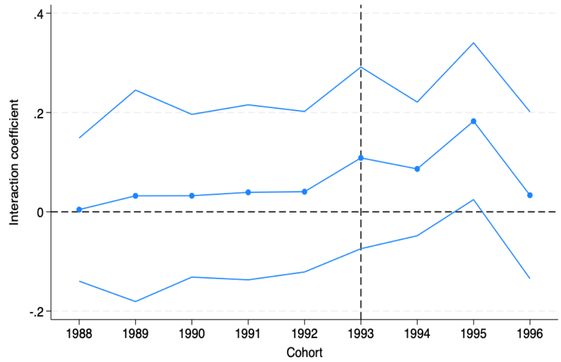

(b) Attended secondary education - female respondents




(c) Completed lower secondary education - female respondents




(d) Completed upper secondary education - female respondents




**Note**: Levels of significance: *<0.10; **<0.05; ***<0.01. The figures display the interaction between the birth cohort and the number of private USE schools per district in 2014, per 10,000 primary students. Given the specification in equation 7 the coefficients depict the treatment effects with regard to female students. For more information please refer to figure A6. For completed schooling years, attending secondary education and completing lower secondary education, a joint F-test of pre-program values does not reject that all values are equal to zero. For all outcomes a joint F-test of post-program values rejects that all values are equal to zero.

## A.3 Appendix - robustness results

Table A9: **Effects of USE on educational outcomes - different DiD estimators**

| | Attended Secondary Education | | | Completed Lower Secondary Education | | | Completed Upper Secondary Education | | | Years of Schooling | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) . | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
| Estimator | Chais. | Gardner | Sant'Anna | Chais. | Gardner | Sant'Anna | Chais. | Gardner | Sant'Anna | Chais. | Gardner | Sant'Anna |
| Cohort × Exposure | 0.05 | 0.06* | 0.11** | 0.07 | 0.09** | 0.09* | 0.05 | -0.03 | -0.02 | 0.42* | 0.33* | 0.46* |
| | (0.24) | (0.05) | (0.03) | (0.04) | (0.06) | (0.04) | (0.05) | (0.04) | (0.04) | (0.05) | (0.25) | (0.17) |
| Mean | 0.76 | 0.76 | 0.76 | 0.49 | 0.49 | 0.49 | 0.24 | 0.24 | 0.24 | 9.90 | 9.90 | 9.90 |
| Observations | 6,846 | 6,846 | 6,846 | 6,846 | 6,846 | 6,846 | 6,846 | 6,846 | 6,846 | 4,676 | 4,676 | 4,676 |

**NOTE**: Levels of significance: *<0.10; **<0.05; ***<0.01. The sample consists of 6,846 respondents, at least 18 years old and born between 1987 and 1998. For upper secondary education, the sample consists of 4,676 respondents, 20-29 years old. For more information please refer to table 1. Chaisemartin refers to the estimator specified in de Chaisemartin and D'Haultfœuille (2023), Gardner refers to the estimator specified in Gardner (2022), and Sant'Anna refers to the estimator specified in Sant'Anna and Zhao (2020).

Table A10: **Effects of USE on educational outcomes with regard to private and public USE schools using Gardner DiD estimator - educational outcomes**

| | Attending secondary education | | Completing lower secondary education | | Completing upper secondary education | | Years of schooling | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Cohort × Schools$^{private}$ | 0.03* | | 0.06** | | 0.07*** | | 0.28*** | |
| | (0.02) | | (0.03) | | (0.02) | | (0.10) | |
| Cohort × Schools$^{public}$ | | 0.03 | | 0.02 | | 0.03 | | 0.18 |
| | | (0.04) | | (0.04) | | (0.04) | | (0.19) |
| Mean | 0.76 | 0.76 | 0.49 | 0.49 | 0.24 | 0.24 | 9.90 | 9.90 |
| Observations | 6,846 | 6,846 | 6,846 | 6,846 | 4,676 | 4,676 | 6,846 | 6,846 |
| $R^2$ | 0.20 | 0.20 | 0.24 | 0.24 | 0.21 | 0.21 | 0.27 | 0.27 |

**NOTE**: Levels of significance: *<0.10; **<0.05; ***<0.01. This table shows the Gardner estimates. I run two separate regressions, where I interact the cohort dummy with the number of private or public USE schools in the subcounty of the respondent at age 14. The Gardner (2022) estimator applies a two-stage framework. In the first stage, group and period effects are identified using untreated observations, which include individuals not eligible for USE and those who were eligible but did not have a private or public USE school in their sub-county. In the second stage, treatment effects are derived by comparing the outcomes for treated and untreated groups accounting for group and period specific effects. This two-stage approach is robust to treatment heterogeneity. Standard errors are clustered at the birth and district level. There are 79 districts. The dashed lines are 95% confidence intervals.

For pre-trends using the Gardner estimator, I run the following estimation:

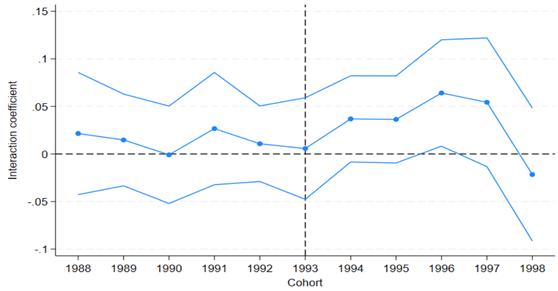
$$Y^j_{irtd} = \beta_0 + \sum_{t=1988}^{1998} \beta_{1t} \times Cohort_t \times PrivateUSE_s + X_{irtd} + \delta_d + \alpha_t + \gamma_r + \epsilon_{irtd}, \qquad (8)$$

where I interact the respondent's birth cohort with the number of private USE schools available in the sub-county of the respondent in 2014. I proceed by plotting the coefficients $\beta_{1t}$ below:

Figure A9: **Pre-trend regarding the interaction between birth cohort and the number of private USE-schools per subcounty in 2014 - Gardner estimator**

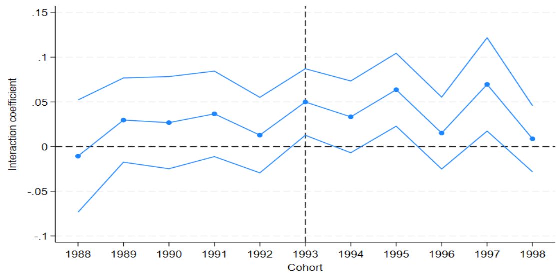
(a) Completed schooling years


(b) Attending secondary education




(c) Completing lower secondary education


(d) Completing upper secondary education




**Note**: These figures show the interaction coefficients between the birth cohort and the number of private USE schools per subcounty in 2014 using the Gardner (2022) estimator. The comparison group is the birth cohort of 1987. Birth cohorts being born before the black reference line are not affected by USE, and those being born after are affected by the USE policy. For the first stage of the Gardner estimator, I use district fixed effects, birth cohort fixed effects and survey round fixed effects, as well as the following control variables: gender, urban status, wealth index, education of the household head and household size. Standard errors are clustered at the district level. There are 79 district. The dashed lines are 95% confidence intervals.

Table A11: **The effects of non-USE private schools on educational outcomes**

| | Attending secondary education | | Completing lower secondary education | | Completing upper secondary education | | Years of schooling | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Cohort × $\text{Schools}^{\text{Nonpriv}}$ | 0.01 | 0.01 | 0.00 | -0.00 | 0.01 | 0.00 | 0.00* | 0.00 |
| | (0.03) | (0.03) | (0.01) | (0.01) | (0.01) | (0.01) | (0.01) | (0.01) |
| Mean | 0.76 | 0.76 | 0.49 | 0.49 | 0.24 | 0.24 | 9.90 | 9.90 |
| Region linear trends | no | yes | no | yes | no | yes | no | yes |
| Poverty*birth | no | yes | no | yes | no | yes | no | yes |
| Observations | 6,846 | 6,846 | 6,846 | 6,846 | 4,676 | 4,676 | 6,846 | 6,846 |
| $R^2$ | 0.17 | 0.17 | 0.22 | 0.23 | 0.22 | 0.22 | 0.26 | 0.27 |

**NOTE**: Levels of significance: *<0.10; **<0.05; ***<0.01. The sample consists of 6,846 respondents, 18-29 years old, and born between 1987 and 1998. For upper secondary education, the sample consists of 4,676 respondents, 20-29 years old. Cohort is a dummy that equals one if a respondent is born after 1992. $Schools^{Nonpriv}$ records the number of private non-USE schools per 10,000 primary students in the district of the respondent at the age of 14. For further explanations, such as the control variables or fixed effects used, refer to table 1.

Table A12: **Effects of USE excluding districts with a large number of public non-USE schools**

| | Attending secondary education | | Completing lower secondary education | | Completing upper secondary education | | Years of schooling | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Cohort × Exposure | 0.07* | 0.05 | 0.11** | 0.10* | -0.00 | -0.01 | 0.35** | 0.32* |
| | (0.03) | (0.03) | (0.04) | (0.04) | (0.03) | (0.04) | (0.16) | (0.17) |
| Mean | 0.80 | 0.80 | 0.51 | 0.51 | 0.19 | 0.19 | 9.95 | 9.95 |
| Region linear trends | no | yes | no | yes | no | yes | no | yes |
| Poverty*birth | no | yes | no | yes | no | yes | no | yes |
| Observations | 6,101 | 6,101 | 6,101 | 6,101 | 4,162 | 4,162 | 6,101 | 6,101 |
| $R^2$ | 0.15 | 0.15 | 0.18 | 0.19 | 0.19 | 0.20 | 0.23 | 0.23 |

**NOTE**: Levels of significance: *<0.10; **<0.05; ***<0.01. Excluded are districts with a high share of government non-USE schools, such as Bushenyi, Kampala, Mpigi, Iganga, and Wakiso. For further explanations refer to table 1.

Table A13: **Effects of USE on educational outcomes with placebo treatment groups**

| | Attending secondary education | | Completing lower secondary education | | Completing upper secondary education | | Years of schooling | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Cohort × Exposure (1991-1992) | 0.07 | | 0.00 | | 0.05 | | -0.00 | |
| | (0.16) | | (0.03) | | (0.04) | | (0.04) | |
| Cohort × Exposure (1989 - 1992) | | 0.13 | | -0.02 | | 0.06 | | 0.06 |
| | | (0.20) | | (0.05) | | (0.04) | | (0.04) |
| Mean | 0.85 | 0.85 | 0.48 | 0.48 | 0.25 | 0.25 | 10.41 | 10.41 |
| Region linear trends | yes | yes | yes | yes | yes | yes | yes | yes |
| Poverty*birth | yes | yes | yes | yes | yes | yes | yes | yes |
| Observations | 4130 | 4130 | 4130 | 4130 | 4130 | 4130 | 4130 | 4130 |
| $R^2$ | 0.28 | 0.30 | 0.39 | 0.22 | 0.27 | 0.27 | 0.22 | 0.23 |

**NOTE**: Levels of significance: *<0.10; **<0.05; ***<0.01. For further explanations refer to table 1.

Table A14: **Effects of USE on educational outcomes without partially treated cohorts in 1991 and 1992.**

| | Attending secondary education | | Completing lower secondary education | | Completing upper secondary education | | Years of schooling | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Cohort × Exposure | 0.06 | 0.06 | 0.10** | 0.11** | 0.00 | 0.01 | 0.33* | 0.35* |
| | (0.04) | (0.04) | (0.04) | (0.02) | (0.03) | (0.03) | (0.20) | (0.22) |
| Region linear trends | no | yes | no | yes | no | yes | no | yes |
| Poverty*birth | no | yes | no | yes | no | yes | no | yes |
| Mean | 0.76 | 0.76 | 0.46 | 0.46 | 0.22 | 0.22 | 9.96 | 9.96 |
| Observations | 5,305 | 5,305 | 5,305 | 5,305 | 3,656 | 3,656 | 5,305 | 5,305 |
| $R^2$ | 0.28 | 0.29 | 0.18 | 0.19 | 0.23 | 0.25 | 0.22 | 0.23 |

**NOTE**: Levels of significance: *<0.10; **<0.05; ***<0.01. For further explanations refer to table 1.

Table A15: **Effects of USE on educational outcomes for individuals being out of school**

| | Attending secondary education | | Completing lower secondary education | | Completing upper secondary education | | Years of schooling | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Cohort × Exposure | 0.07 | 0.08 | 0.12** | 0.12** | -0.00 | 0.00 | 0.43** | 0.46** |
| | (0.05) | (0.05) | (0.05) | (0.05) | (0.03) | (0.03) | (0.20) | (0.19) |
| Region linear trends | no | yes | no | yes | no | yes | no | yes |
| Poverty*birth | no | yes | no | yes | no | yes | no | yes |
| Mean | 0.84 | 0.84 | 0.58 | 0.58 | 0.27 | 0.27 | 10.30 | 10.30 |
| Observations | 3,292 | 3,292 | 3,292 | 3,292 | 3109 | 3109 | 3,292 | 3,292 |
| $R^2$ | 0.20 | 0.21 | 0.23 | 0.24 | 0.20 | 0.21 | 0.29 | 0.30 |

**NOTE**: Levels of significance: *<0.10; **<0.05; ***<0.01. The sample consists of 3,292 respondents, 18-29 years old and born between 1987 and 1998. For upper secondary education, the sample consists of 3,109 respondents, aged 20-29. The respondents in the sample are no longer enrolled in primary, secondary, post-secondary, or tertiary education. Exposure is a dummy variable that equals one if districts have more than the median number of USE schools per 10,000 primary students. Cohort is a dummy variable that equals one if a respondent is born after 1992. For further explanations, such as control variables or fixed effects, refer to table 1.

Table A16: **Effects of USE on siblings with household fixed effects**

| | Attending secondary education | | Completing lower secondary education | | Completing upper secondary Education | | Years of schooling | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Cohort × $\text{Schools}^{\text{public}}$ | 0.09 | 0.08 | -0.00 | -0.04 | -0.15** | -0.17*** | -0.01 | -0.10 |
| | (0.06) | (0.06) | (0.04) | (0.02) | (0.07) | (0.03) | (0.27) | (0.26) |
| Cohort × $\text{Schools}^{\text{private}}$ | 0.09 | 0.08 | 0.17** | 0.17** | 0.14 | 0.14* | 0.63** | 0.62** |
| | (0.07) | (0.07) | (0.06) | (0.02) | (0.09) | (0.09) | (0.30) | (0.30) |
| Poverty*birth | no | yes | no | yes | no | yes | no | yes |
| District linear trends | no | yes | no | yes | no | yes | no | yes |
| Mean | 0.84 | 0.84 | 0.57 | 0.57 | 0.27 | 0.27 | 10.46 | 10.46 |
| Observations | 3,842 | 3,842 | 3,842 | 3,842 | 2,493 | 2,493 | 3,842 | 3,842 |
| $R^2$ | 0.49 | 0.53 | 0.51 | 0.56 | 0.55 | 0.59 | 0.59 | 0.63 |

**NOTE**: Levels of significance: *<0.10; **<0.05; ***<0.01. The sample consists of 3,842 respondents, at least 18 years old and born between 1987 and 1998. The sample is further restricted to siblings living in households where one sibling was born before 1993 and one sibling after 1992. $Schools^j$ measures the number of private or public USE schools per 10,000 primary students available for the respondent during the age of 14 at the district level. Cohort is a dummy variable equal to one if a respondent was born after 1992. For further explanations, such as control variables or fixed effects, refer to table 1.

## A.4 Appendix - mechanisms results

Table A17: **Distance to private USE schools, gender impacts and educational outcomes**

| | Years of schooling | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Cohort × Private USE <5km × Male | -0.38 | -0.27 | -0.40 | -0.29 | -0.53 | -0.39 |
| | (0.43) | (0.40) | (0.46) | (0.43) | (0.52) | (0.48) |
| Cohort × Private USE <5km | 0.74* | 0.69* | 0.77* | 0.73* | 0.78* | 0.74 |
| | (0.39) | (0.40) | (0.41) | (0.42) | (0.45) | (0.46) |
| Male × Private USE <5km | 0.15 | -0.20 | 0.10 | -0.23 | 0.10 | -0.26 |
| | (0.38) | (0.35) | (0.40) | (0.37) | (0.45) | (0.41) |
| Cohort × Male | 0.38 | 0.35 | 0.42 | 0.37 | 0.53 | 0.45 |
| | (0.33) | (0.32) | (0.36) | (0.34) | (0.40) | (0.38) |
| Private USE <5km | -0.14 | -0.04 | -0.14 | -0.07 | -0.16 | -0.09 |
| | (0.37) | (0.38) | (0.38) | (0.39) | (0.41) | (0.43) |
| Male | -0.32 | -2.05*** | -0.27 | -2.01*** | -0.25 | -2.10*** |
| | (0.30) | (0.48) | (0.33) | (0.51) | (0.35) | (0.53) |
| Distance public USE | -0.12** | -0.08 | -0.14*** | -0.10* | -0.15*** | -0.11* |
| | (0.05) | (0.05) | (0.05) | (0.05) | (0.06) | (0.06) |
| Distance public USE × Cohort | 0.06 | 0.03 | 0.07 | 0.05 | 0.09 | 0.06 |
| | (0.05) | (0.05) | (0.05) | (0.05) | (0.06) | (0.06) |
| Control group | 5-20km | 5-20km | 5-15km | 5-15km | 5-10km | 5-10km |
| Mean | 10.3 | 10.3 | 10.1 | 10.1 | 10.2 | 10.2 |
| Interacted controls | no | yes | no | yes | no | yes |
| Observations | 3,321 | 3,321 | 3,156 | 3,156 | 2,868 | 2,868 |
| $R^2$ | 0.18 | 0.22 | 0.18 | 0.22 | 0.17 | 0.22 |

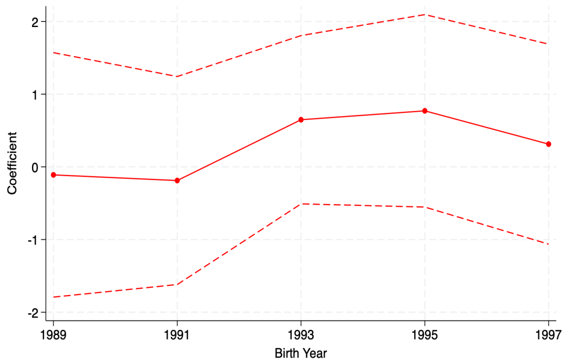
**NOTE**: Levels of significance: *<0.10; **<0.05; ***<0.01. The sample consists of 3221, 3156 or 2868 respondents that were living since the age of 14, 10, 15 or 20km away from a private USE school. All respondents are born between 1987 and 1998 and at least 18 years old. Cohort is a dummy that equals one for respondents born in 1993 or later. Private USE < 5km is a dummy that equals one if a private USE school is within 5km to the respondent. For further explanations, such as control variables or fixed effects, refer to table 1.

To check for pre-trends, I run the following equation and plot the coefficients $\beta_{5i}$ and $\beta_{6i}$ in figure A10:

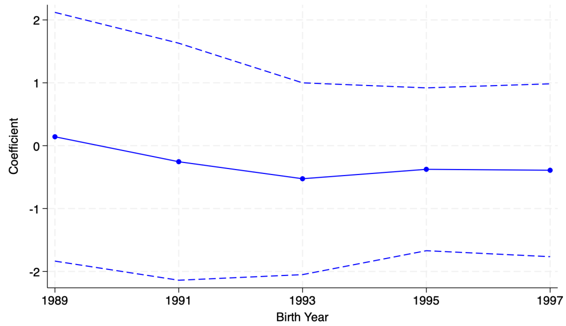
$$
\begin{aligned}
Y_{irbs} = \beta_0 + \beta_1 \times Male_{irbd} + \beta_2 \times Distance^{Private}_{irbd} \\
+\beta_3 \times Male_{irbd} \times Distance^{Private}_{irbd} + \sum_{i=1988}^{1998} \beta_{4i} \times Cohort_i \times Male_{irbd} \\
+ \sum_{i=1988}^{1998} \beta_{5i} \times Cohort_i \times Distance^{Private}_{irbd} + \sum_{i=1988}^{1998} \beta_{6i} \times Cohort_i \times Distance^{Private}_{irbd} \times Male_{irbd} \\
+\omega_1 \times Distance^{Public}_{irbd} + \sum_{i=1988}^{1998} \omega_{2i} \times Cohort_i \times Distance^{Public}_{irbd} \\
+\omega_3 \times Distance^{NonUSE}_{irbd} + \delta_d + \alpha_b + \gamma_r + \epsilon_{irtd},
\end{aligned} \tag{9}
$$

Figure A10: **Pre-trends with regard to distance - completed schooling years**

(a) Treatment effect female respondents within 5km to a private USE schools




(b) Difference in the treatment effects with regard to male students




**Note**: Figure (a) shows the interaction coefficient for each birth cohort with a dummy indicating if a respondent was eligible for USE and in figure (b) the interaction coefficient for each birth cohort with a dummy indicating if a respondent was eligible for USE and a male student. The comparison group is the birth cohort of 1987. To increase power, the birth cohorts of 1988 and 1987 are coded as one, the birth cohorts 1990 and 1989 are coded as one etc.. Control variables are gender, urban status, wealth index, education of the household head and household size, all interacted with a male dummy. Further included are birth year fixed effects and district fixed effects. Standard errors are clustered at the district level. There are 79 districts. All regresions use DHS survey weights.