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**"The Effects of Universal Secondary Education Program
Accompanying Public-Private Partnership on Students' Access,
Sorting and Achievement: Evidence from Uganda"**

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The Effects of Universal Secondary Education Program Accompanying Public-Private Partnership on Students' Access, Sorting and Achievement: Evidence from Uganda

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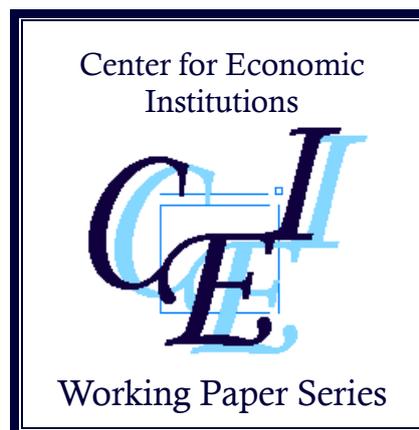
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Abstract

This paper evaluates how the abolition of fees for public secondary education affects the access, sorting and achievement of students when it is accompanied by the Public-Private Partnership (PPP) scheme. In 2007, Uganda introduced the Universal Secondary Education policy, which solicited the participation of private schools to offer fee-free education by receiving public assistance. This has created (1) public schools (fee-free for all), (2) PPP private schools (accepting both fee-free and fee-paying students) and (3) private schools. We investigate the sorting across these types of schools, and further assess the impact on achievement, student composition, and learning environments by the type. In order to identify the effects of the policy, we utilize the across-cohort discontinuity in exposure to the program and across-district variation in the program intensity based on the pre-existing transition and retention rates. Our results suggest that the program increased the overall number of the students taking the secondary school exit exam by 16% in the median intensity district, partly supported by the entry of private schools. Though learning environments worsened, this access improvement was not accompanied by a change in test scores. Across the subsectors, public and the PPP schools particularly increased their enrolments, and the PPP and private schools caught up with public schools in terms of the test scores. Our findings suggest that fee elimination can improve the access to secondary education with few negative effects on learning, and that PPP can provide one of the cost-effective means for financing it.

JEL Classification Codes: J13; J12; D10; O10

1. Introduction

Improving access to secondary education is key for economic development and poverty eradication in Africa. This region experienced a rapid improvement in primary school enrollment rates in the 1990s and 2000s, increasing from 59% in 1999 to 79% in 2012 (UNESCO, 2015). However, gross enrolment rates in secondary education remain low at 42.8% in 2013 (World Bank, 2016). Literature often attributes this limited access to basic education in developing countries to its high user costs, tuition, and transportation fees, and high opportunity costs.[1] Several countries in this region have recently recognized the importance of secondary education by abolishing tuition fees in secondary schools.[2] However, as will be discussed below, relatively scarce empirical evidence exists regarding the impact of such fee abolition on student enrollment and achievements at the secondary education level. This is unfortunate, as there have been concerns about the possible detrimental impact of such a fee abolition, such as poorer learning environments (e.g., congestion and fewer resources per student) and a resulting decline in educational achievement. As the government supply of school resources for secondary education is inelastic,[3] an acute increase in the demand for a public education system may deteriorate the learning environment by reducing the school resources available per student.

Given this increase in demand for secondary education, the number of privately operated secondary schools increased rapidly in many developing countries. In those countries where the private sector plays an important role, it is critical to assess how the fee abolition in the public sector affects the supply of private education and its quality. Furthermore, some countries have adopted a public-private partnership (PPP) in the provision of educational services in order to minimize the public burden of financing secondary education (Patrinos et al., 2009). While this helps governments in absorbing excess demand for secondary education, it has also not been well-understood how the

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[1] For the examples from sub-Saharan Africa, see Lucas and Mbiti (2012) for Kenya, Deininger (2003), Grogan (2009) for Uganda, Al-Samarrai and Zaman, (2007) for Malawi. Benerjee et al. (2013) provides the reviews in the context of developing countries as a whole.

[2] The program started in South Africa in 2007, the Gambia in 2001-2004, Uganda in 2007, Kenya in 2008, Tanzania in 2016, and Rwanda in 2007.

[3] Compared to the primary education, operating new secondary school requires better teachers, more school materials, and so forth. In the country where the stock of secondary school graduates is limited, the supply of secondary school is inelastic to its demand increase (Andravi et al., 2013).

PPP scheme affects access and achievement of students and how they sort across different education sectors at the secondary level.

This study provides new evidence regarding the impact of the secondary fee abolition that accompanied the PPP scheme on students' attainment, sorting, and achievement, using Uganda's Universal Secondary Education (USE) policy as a case study. We also explore the mechanism through which the USE policy affects achievement by focusing on the composition of students and their learning environments, measured by the number of classrooms, seating, teachers, and qualified teachers. Interestingly, Uganda's USE policy utilized the PPP scheme by soliciting private schools' participation to offer fee-free education and receive public assistance. Consequently, the educational sector became comprised of (1) USE public schools (fee-free for all), (2) USE private schools (accepting both fee-free and fee-paying students) and (3) non-USE private schools.[4] By taking advantage of this unique feature of Uganda's USE policy, we investigate the sorting across these types of schools. Further, we assess the impact on achievement, student composition, and learning environments by type.

Methodologically, we use a variant of the difference-in-differences method. Uganda began to offer fee-free secondary schooling in public schools and its partnering private schools for boys and girls in grades 8 to 11 in February 2007. Thus, only children who entered lower secondary schools in 2007 or later benefited from the policy. Additionally, the potential gain from this policy was likely larger in districts with many more children who completed primary schools but failed to transition to secondary schools, or those that lacked the program, but retained students until they completed their final year of secondary education. We utilize this cross-district variation to define the program intensity as the share of those who do not transition or are not retained, over the number of those who completed primary education. This regional variation combined with the cross-cohort variation allows us to identify the USE policy's impact on students' attainment, sorting, and achievement.

Our results suggest that the program successfully promoted a large number of students to complete a lower secondary education. In the median-intensity district, the number of students taking secondary school exit exams increased by 16% compared with the pre-

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[4] A small proportion of public schools opted out of the USE policy and kept charging fees after 2007. These tend to be elite schools. We do not include these schools in our analysis because the sample size is too small to ensure accurate estimation.

program level. These changes were supported by the supply-side response: the policy increased the number of both fee-charging private schools as well as private schools that opted in to the USE's PPP scheme. As a result, private school enrollment increased. USE policy however negatively affected learning environments, as measured by the number of classrooms, seating, and the teachers available per students in all three types of schools. Nevertheless, no significant impact is noted on their performance on secondary school exit exams. These findings suggest that the universal fee-free secondary education program expanded access with few negative effects on student achievements. One possible explanation is that the composition changes among students explain the improved test scores. Specifically, the results indicate the following two kinds of sorting: (1) high-ability children, who could not attend secondary schools before the USE due to credit constraints, began to enter subsidized schools after the USE's implementation; and (2) this crowded out the relatively low-ability students, who were not credit-constrained, to non-subsidized schools, which opted out from the USE policy. This increased the average ability of students attending the two types of subsidized schools (public and PPP-participating private schools), as they caught up with the non-subsidized (non-PPP private) schools. These results also suggest that the PPP scheme helped the policy improve access to secondary education without sacrificing the level of achievement. The response of fee-charging private schools also contributed to the overall gain in enrolment, with no negative effect on learning outcomes.

Our contributions are three-fold; first, we provide the first evidence on the effects of the *universal* fee-free secondary education program on the access and achievement of students in Africa[5]. We also take advantage of the unique features of Uganda's USE policy. Namely, we contribute to literature on the supply response of private schools to public policies by providing additional evidence on the effects of the USE policy on the supply of private schools and on the educational attainment and achievement in the private sector. Finally, we provide the first evidence on the effects of fee abolition accompanying the PPP scheme on the access, sorting and achievement in the entire education market.

Our work is most related to the vast literature on the impact of cost reduction on educational outcomes in developing countries. There is relatively ample evidence for the impact at the primary level (e.g., Grogan, 2009; Hoogeveen and Rossi, 2013; Lucas and

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[5] While the Gambia started the secondary-level fee abolition first in 2001, it was limited to girls only. South Africa started fee elimination in the same year as Uganda did in 2007, but its implementation was gradual and it took until 2010 to eliminate fees in about 75% of schools.

Mbiti, 2012). Our work most relates to the vast literature on the impact of reducing costs on educational outcomes in developing countries. Relatively ample evidence exists to demonstrate an impact at the primary level (e.g., Grogan, 2009; Hoogeveen and Rossi, 2013). However, evidence is still limited regarding how reduced costs affect educational outcomes at the secondary level. One study with available evidence based on an RCT was conducted by Duflo et al. (2017). They find the provision of three-years scholarships in the upper secondary level leads to an increase in the years of education as well as test scores in Ghana. However, as the number of treated individuals is small relative to the entire education market, it might be difficult to assess the possible negative effects of congestion and a decrease in the per-student amount of resources. The other pieces of evidence are more likely to illuminate this issue by investigating the impact of nation-wide reforms to reduce secondary education costs. Blimpo et al.'s (2016) study has evaluated a large-scale female scholarship program at the secondary level in Gambia by using the differential timing of the program's rollout across regions. The authors find that the program increased the number of girls who took the secondary school's exit exam by 55%, and this also significantly increased the exam's average scores. Gajigo (2016) used different datasets, but also reported that this program increased female enrollment.[6] Another pair of studies has investigated the effects of Kenya's fee abolition at the primary and secondary education levels using the strategy similar to ours (Lucas and Mbiti (2012) and Brudevold-Newman (2017)): the difference-in-differences method across cohorts and regions with differential primary retention rates or primary-to-secondary transition rates. The results show that free primary education increased the number of exam takers at the end of primary education, particularly from disadvantaged background. The achievement at the exam is found to have slightly decreased among those who would have completed primary school without the fee elimination (Lucas and Mbiti, 2012). The results for free secondary education show that it increased the educational attainment of children by 0.8 year, with little impact on grades of the secondary school completion exam. The fee abolition at the secondary level has also been found to increase educational attainment in West Germany (Riphahn, 2012). These studies generally suggest that increased access need not be accompanied by a decrease in achievement.[7]   We add on to these pieces of

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[6] In India, the bicycle provision program that reduced girls' traveling costs to their secondary school increased their enrollment by 30%, and decreased the gender enrollment gap by 40% (Muralidharan and Prakash, 2017).

[7] One study however suggests no or slightly negative effects. In South Africa, where fees were eliminated in primary and secondary public schools, Garlick (2013) reports the fee elimination only slightly increased enrollment in the first two grades of secondary school, and it decreased the number of students completing it. This might be due to the low valuation of education as the author notes, or the fact that the

evidence by addressing the issues of how the private sector responds to public fee abolition and how children sort across the education subsectors: public schools, PPP private schools, and fee-charging private schools. We furthermore show evidence on the impact on the completion of secondary education and test scores by the education subsector.

We also contribute to the literature on the supply response of private schools to public policies by providing additional evidence on the effects of the supply of private schools on educational attainment and achievement. The literature on private school entry and exit is relatively limited,[8] and only several studies have examined how the entry and exit change the policy outcome measures. For example, Dinerstein and Smith (2015) studied the New York City's Fair Student Funding (FSF) reform in which the local government provides funding to public elementary and middle schools. They discuss that the private school neighboring the funded public school is likely to exit in response to the reform, and students in private school had to switch to public school. The reform improved the access to public school, however, much of the positive achievement effects of FSF funding were possibly undid by sorting of some students from private school to public school. For developing countries, Menezes-Filho et al. (2014) examined the Bolsa Familia conditional cash transfer program expansion in Brazil, where it increased the attendance of poor children in public education system. They argue that the program induced the private school entry into the market because high ability public students sort to private education to avoid being the peer of the worse students. Hsie and Urquiola (2006) studied the universal voucher program, which financially allow voucher recipients to enroll in private schools. They found that the program results in the tremendous increase in number of private schools, though the communes with more private school entry did not improve the test score. The present paper provides the new evidence on the response of private schools to the public policy in the much more resource-limited setting.

We also contribute to current literature on private schools' supply response to public

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enrolment rate in South Africa was already relatively high compared to the Gambia, Kenya and Uganda.

[8] Literature on entry includes Downes and Greensterin (1996), Barrow (2006), and Ferreyra (2007). Literature on exit includes Pandey et al. (2009). In sub-saharan Africa, Lucas and Mbiti (2012) examined the free primary education program in public school, and argued that it increased the private school opening in the district with higher program intensity than lower program intensity. They, however, did not shed light on the student sorting between public and private school, and its effects on the aggregate test score.

policies by providing additional evidence on the effects of private schools' supplies on educational attainment and achievement. The literature on private school entries and exits is relatively limited,[9] and only a few studies have examined how these entries and exits change policy outcome measures. For example, Dinerstein and Smith (2015) studied the New York City's Fair Student Funding (FSF) reform, in which the local government provides funding to public elementary and middle schools. The authors note that a private school neighboring a funded public school is likely to exit in response to the reform, and students in a private school had to switch to public school. The reform improved the access to public school, but much of the positive achievement effects of FSF funding were possibly reversed by sending some private school students to public school. For developing countries, Menezes-Filho et al. (2014) examined the Bolsa Familia conditional cash transfer program's expansion in Brazil, which increased poor children's attendance in the public education system. The authors argue that the program induced private school entry into the market because high-ability public school students were sorted to private education to avoid exposure to less able students. Hsieh and Urquiola (2006) studied the universal voucher program, which financially permits voucher recipients to enroll in private schools. They found that the program results in a tremendous increase in the number of private schools, although locales with more private school entries did not improve their test scores. The present paper provides new evidence on private schools' response to public policies in a much more resource-limited setting.

Our work also relates to a smaller but growing body of literature on the public-private partnership (PPP) in the education market (Hoxby, 2007; Patrinos, et al, 2009). Several studies have found that PPP programs were effective in increasing enrollment in Tanzania, Colombia, and Pakistan (Alderman, Kim, and Orazem, 2003; Barrera-Osorio and Raju, 2011; Barrera-Osorio and de Galbert, 2015; Patrinos et al., 2009). Of these, however, only Barrera-Ossorio et al. (2015) address the effects on test scores. They evaluated the same PPP program in Uganda to find that the developing a partnership with the government increases the enrollment and test scores in low-cost private schools, compared to other comparable low-cost private schools. They also found that the USE induced students with strong academic backgrounds to enroll in PPP schools. However, it is still unclear not

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[9] Entry literature includes works by Downes and Greenstein (1996), Barrow (2006), and Ferreyra (2007). Exit literature includes Pandey et al.'s (2009) study. In sub-Saharan Africa, Lucas and Mbiti (2012) examined the free primary education program in public schools, and argued that it increased private school openings in the districts with higher program intensity than lower program intensity. However, they did not highlight the student sorting between public and private schools or its effects on aggregate test scores.

only how the sorting of students occurred across public schools, PPP private schools, and private schools that opted out of the USE program, but also how the test scores compare among these types of schools after the program's introduction. We address these questions by studying the program's impact across the entire education market.

The rest of the paper is as follows: Section 2 explains Uganda's secondary education system and its free secondary education program. Section 3 discusses the data and identification strategy employed in this study, Section 4 reveals the results, and Section 5 concludes and provides policy implications.

2. Secondary Education system and Free Secondary Education policy in Uganda

2.1. Secondary education in Uganda

Uganda provides seven years of primary education, followed by four years of lower secondary education. In 2006, approximately 64 percent of children (404,935) completed primary education, while only 37 percent (166,372) completed a lower secondary education. [10] These statistics suggest that improving students' access to secondary education should be the next goal for the government.

The private sectors in secondary education are substantial in size compared to those in primary education; total private secondary enrollment accounted for approximately 57 percent in 2000, while this was only 23 percent in primary education (Liang, 2002). Among the three types of secondary school by ownership—the government, private, and the community—the distinction between private and community is unclear, as neither have received any government subsidies. Hence, this paper describes both private and community secondary schools as "private."

Before the USE policy was introduced in 2007, public secondary schools received government subsidies in two forms: teacher salaries and capitation grant. The Ministry of Education and Sports (MoES) established the per capita grant level, which was US$65 per student per day in 2002 (Liang, 2002).[11] The per-student government subsidy in secondary education was US$148, which was substantially higher than that in primary education (US$22). This suggests that financing secondary education is more costly for the government.

Families bore almost half of secondary school costs in the form of Parent-Teacher Association (PTA) levies, tuition fees, boarding fees, and other expenses. According to Liang's (2002) calculation, the average private cost of a household member in secondary school was approximately USh240,000 per year. Given that 47 percent of Ugandan households earned less than USh620,500 annually in 2000, this private cost accounted for over 40 per cent of this income.

2.2. Uganda Certificate of Education exam

Students who complete a lower secondary education and wish to study at a higher secondary level must take the Uganda Certificate of Education (UCE) exam after completing the 11th grade.

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[10] The authors' calculation incorporated a population aged between 20 and 25, from the Uganda National Household Survey (2006), or UNHS.

[11] Given a secondary school operates 9 months annually (270 days), this amount is approximately USh17,550 per student, per year.

Candidates enrolled in the 11th grade must be registered for the UCE exam by the head teacher of the school which they attend. While nearly all candidates take the examination at the school they attend, the Uganda National Examination Board (UNEB) also allows students to take the UCE exam at public centers under special exceptions, for example, if the school lacks the capacity to manage the UCE exam.

Further, candidates must take at least eight subjects in school, but no more than ten. These subjects have included five compulsory subjects since 2006: English, Mathematics, Biology, Chemistry, and Physics. Depending on the student's performance, the UNEB grades each student on each subject using a scale from 1 to 9,[12] where grade 1 indicates the student "passed with distinction." Successful candidates are classified based on the aggregate grades of their best eight subjects. For example, the best-performing division I includes candidates with an aggregate of 32 or less.

This study uses school- and district-level data on the number of UCE exam participants to measure the number of secondary school graduates, and incorporates test score averages to measure their academic achievement.

2.3   Universal Secondary Education policy

The Ugandan government aimed to improve students' access to secondary education by introducing the USE policy in 2005; this policy was first implemented in February 2007,[13] and allows eligible students to attend government USE secondary schools without paying school and PTA fees. Although most government schools were listed as USE schools from the beginning, some schools stayed out of the program during its initial stage. For example, only 791 out of 845 government-aided schools benefited from the first phase of the program in 2007, but the number of USE government secondary schools eventually increased to 1,024 in 2012 (Barungi et al., 2014).

The USE benefit only applies to the students who have taken the Primary Leaving Exam (PLE) since 2006, and students must perform better than an aggregate score of 28.[14] According to the UNEB, approximately 70 to 80 percent of PLE students

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[12] Each subject is graded with 100 full marks, as follows: 90–100 = 1; 80–89 = 2; 70–79 = 3; 65–69 = 4; 60–64 = 5; 50–59 = 6; 45–49 = 7; 35–44 = 8; and 0–34 = 9.

[13] In January 2006, the education minister clarified that the USE policy would begin in February 2007 (New Vision, 2006).

[14] All PLE candidates must take four core subjects: English, Mathematics, Integrated Science, and Social Studies and Religious Education. Each subject is graded on a scale between 1 and 9, where 1 stands for "pass with distinction." Successful candidates are classified by an aggregate score of four core subjects. The best-performing division I includes students with aggregate scores of 12 or lower. Division II includes those with aggregate scores of 24 or lower. Divisions III and IV do not have minimum aggregates, but require students to score at least an 8 in 3 subjects. All candidates in

in 2006 were eligible to study at a secondary school without paying tuition.

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

Prior to the USE policy's implementation, students' parents paid secondary schools approximately USh126,000 (or US$38, based on an exchange rate of USh3344 = US$1) per year per student.[15] This payment was a major constraint on Ugandan students' access to a lower secondary education.[16] The USE policy's introduction is expected to break this barrier, and the MoES indicated that total enrollment in lower secondary education improved from 814,087 in 2006 to 1,362,739 in 2013.

Additionally, a critical feature of this policy was that it was implemented through a public-private partnership (PPP). Under this scheme, an owner of a private secondary school can partner with the government and allow eligible students to receive a lower secondary education.[17] According to Barungi et al. (2014), 90 percent of PPP private schools chose to apply for PPP, but a few schools were approached by the MoES to become USE private schools. Most PPP private secondary schools mentioned that they applied for the program to access governmental funding and material support.

The requirements for private schools' participation in the USE policy are as follows (Barungi et al., 2014):

1. Schools must charge less than USh75,000 per term per student
2. Schools must have or establish a Board of Governors to manage the USE program's implementation.

As a result, government schools[18] as well as approximately one-third of private schools have implemented USE policies in Uganda.

In contrast to the free primary education policy, one study by Asankha and Yamano (2011) is the only one to assess the effects of this free secondary education policy. They used repeated cross-sectional data collected in 2005 and 2009 to study whether the probability of secondary school enrollment increased after the USE policy was introduced.

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divisions I, II, II, and IV pass the PLE.

[15] The authors' calculation using the UNHS (2006). Private school fees are higher than those in government schools. The former charges USh156,100, and the latter charges USh103,100 on average.

[16] According to UNHS (2006), 76 percent of children who drop out after completing P7 attribute the reason for stopping is due to the high secondary education costs.

[17] The MoES mandated that schools charging less than UGX75,000 per term per student are eligible to participate in the USE program.

[18] Approximately 24% of government schools are also non-USE schools.

The authors discovered that the USE policy increased public secondary school enrollments, and especially for girls from poor households. However, their study relies on the strong assumption that no change simultaneously affected the child's secondary enrollment other than the USE policy, and they potentially failed to isolate the policy effects from other time trends.

This study fills this gap in literature by employing a quasi-experimental design to examine whether the USE policy increased the number of secondary school graduates and affected their academic achievement.

3. Data and Identification
3.1 Data
This study assesses the effects of a free secondary education policy by relying on three unique data sets:

1. Uganda Certificate of Education

First, this study evaluates whether the USE policy's introduction increased the number of secondary school graduates, and/or changed the students' learning achievement, using the Uganda Certificate of Education (UCE) exam's district-level data from 2006 to 2012. This data provides the number of students who took the UCE exam and their average test scores in five core subjects: English, Mathematics, Biology, Chemistry, and Physics. As the UNEB registers all the UCE exam candidates every year, this dataset gives the most reliable measure of the number of students who have completed a lower secondary education. Further, substantial data on these test scores allows me to assess whether the introduction of the USE policy affects students' learning performance.

Figure 1 describes the change in the number of students who took the UCE exam between 2006 and 2012. The first USE students took the PLE in 2006, and entered the first grade of secondary school (eighth grade), in 2007. Hence, they reached ninth grade in 2008, tenth grade in 2009, and the eleventh grade in 2011, and took the UCE exam in the same year. On the contrary, UCE test-takers before 2009, who took the PLE in 2005 or prior when the USE policy had not yet been implemented, were untreated by USE.[19]

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[19] Some may posit that the students who entered secondary education in 2006 and took the UCE in 2009 are also partially affected by the USE policy because Uganda's government announced the USE policy's implementation in the middle of 2005. For example, forward-looking agents, such as parents, who anticipated the USE policy's implementation might consider this future policy change. In this case, the number of 2009 UCE examinants increased; therefore, this study's estimate of the policy effects would be underestimated, providing a lower bound for the true size of policy effects. In another case, readers may be concerned that some students who took the PLE in 2006 might delay their secondary education entry by one year. However, the government announced the UCE policy

As Figure 1 illustrates, the number of examinants increased dramatically from 2009 to 2010, suggesting the USE policy's significant effects on students' access.

Although UCE exam data provides rich information on learning outcomes to assess the USE policy's effects, this policy should affect students' learning outcomes through the changes in their learning environment, such as the number of teachers available per student. Annual School Census (ASC) data is used to recognize such characteristics at each school in a certain year.

2. Annual School Census

The ASC is conducted by Uganda's MoES, and contains detailed data on each school's number of teachers and their qualifications, classrooms, adequate seating, and administrative school information. Although it would be ideal to obtain such information since 2001, the MoES has only performed the ASC since 2006. Thus, I merged UCE and ASC data between the years of 2006 and 2012. Another important feature of the ASC is that the response rate from government schools is almost 100 percent, as answering the ASC is one condition under which USE schools receive capitation grants from the government. However, private schools' response rate is approximately 60 percent, according to MoES. To avoid the issue of non-random selection among private schools' ASC responses, this study primarily focuses on government secondary schools[20] when assessing the USE policy's effects on school characteristics. However, the same analysis results using private school data are provided as suggestive evidence. Table 1 provides the summary statistics using UCE and ASC data.

Table 1-Summary Statistics

Variable	pre-USE (2006-2009)		post-USE (2010-2012)		change
Number of test takers per year	189,677		260,375		70,698
USE participating government schools	54,260	[879]	84,799	[7,379]	30,539
Non participating government schools	15,743	[12]	15,829	[192]	86
USE participating private schools	32,453	[4,093]	52,904	[19,019]	20,451
Non participating private schools	87,221	[8,644]	106,843	[28,969]	19,622
Number of secondary schools	2,115		2,655		540
USE participating government schools	651	[21]	761	[117]	110

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introduction in January 2006, timed after admission decisions had been already made. Hence, changing the decision to delay their entry would have been too costly. Indeed, as Figure 1 indicates, the number of students who delayed entry was likely to be small, if any.

[20] Among the government schools that administered the UCE exam, 93% are USE schools, while 30% of private schools are.

Non participating government schools	100 [0]	102 [3]	0
USE participating private schools	416 [69]	593 [270]	177
Non participating private schools	948 [135]	1,201[424]	253
Standardized score at UCE exam	0.000	0.000	0.000
USE participating government schools	-0.057	-0.045 [-0.099]	0.012
Non participating government schools	0.111	0.053 [-0.065]	-0.059
USE participating private schools	-0.074	-0.046 [-0.108]	0.028
Non participating private schools	0.020	0.038 [-0.028]	0.018
Number of classroom per student			
USE participating government schools	0.005	0.004	-0.001
Non participating government schools	0.006	0.005	-0.001
USE participating private schools	0.007	0.005	-0.002
Non participating private schools	0.009	0.008	-0.001
Number of teachers per student			
USE participating government schools	0.012	0.011	-0.002
Non participating government schools	0.017	0.015	-0.002
USE participating private schools	0.015	0.011	-0.004
Non participating private schools	0.019	0.017	-0.002

Note: Author's calculation from Uganda Certificate of Education data and Annual School Census data

Values are calculated as annual average over pre and post USE policy period. Statistics for new

schools which were established in 2007 or after in bracket.

3. The Uganda Housing and Population Census
4. Additionally, the Uganda Housing and Population Census 2002, or "UHPC," allows for the measurement of the regional variations in the effective benefit from the USE policy, based on each district's number of dropouts after completing the seventh grade. The UHPC is nationally representative cross-sectional data, and the following section will further discuss this program intensity's calculation and measurement.

Figure 1. Change in the total number of test taker
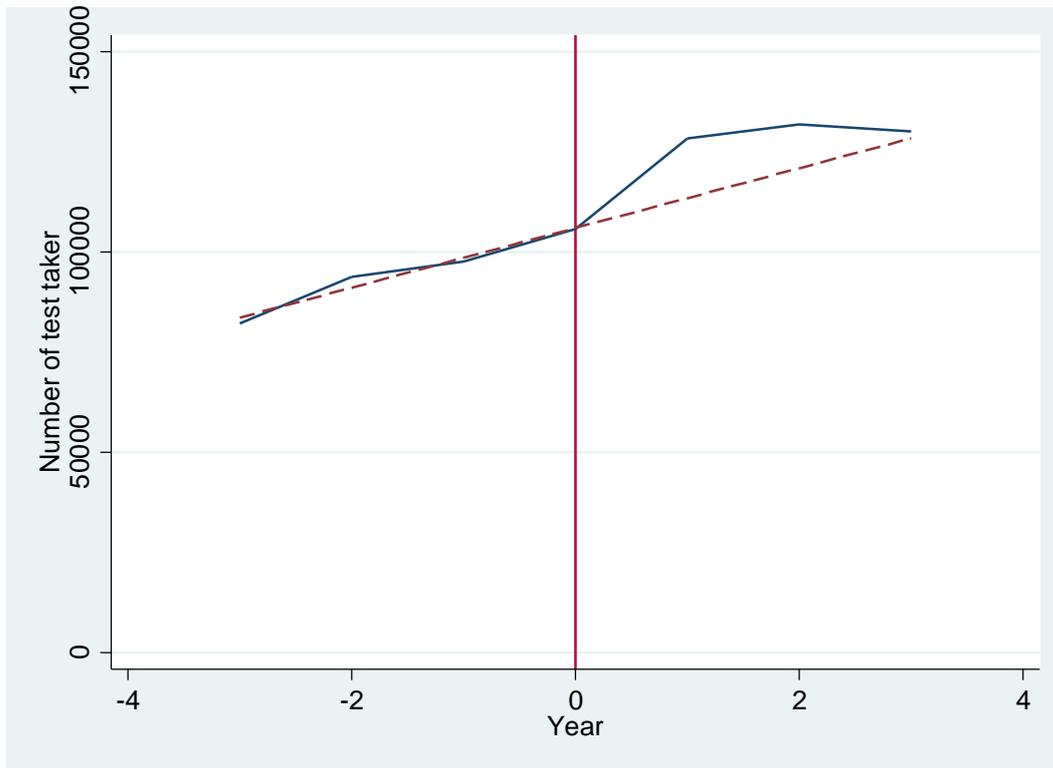
Note: Authors' calculation from UCE data

3.2. Identification

This study isolates the program's effects from other unrelated time trends—and assesses whether the program's introduction allowed more students to complete a lower secondary education—by employing a difference-in-differences approach in exploiting two variations in the program's effective benefit. The first difference in this strategy is each year's varying level of exposure to the program by the test taker. During the survey period between 2006 and 2012, 11th grade students who took the UCE exam in 2010 or later are handled disproportionately by the program, as discussed in the preceding section.

The second variation is the district-level program intensity Intensity_d, derived from the number of children who completed primary education but did not transition to or complete a lower secondary education in the program's absence. The counterfactual information is calculated from the UHPC's (2002) information.

$$Y_{dt} = \gamma_0 + \gamma_1 \text{Intensity}_d * \text{Post}_t + \gamma_2 X_{dt} + \lambda_d + \lambda_t + \mu_{dt} --- (1)$$

Equation (1) is the model to be estimated, where Y_{dt} is the outcome from district d, in year t. This set includes the number of students who took the UCE exam, and

the average test scores of the secondary school exit exams. Further, X_{dt} signifies the time-variant district characteristics, including the age-district cohort size;[21] λ_d denotes the district fixed effects; λ_t is the year fixed effects; and the error term ϵ_{cdt} is clustered at the district level as Bertrand et al. (2004) suggest.

Intensity$_d$ is the time invariant intensity measure with the regional variation. It takes the program's resulting potential and proportional increase in those who complete the 11th grade, compared to the level in 2009 and before. Specifically, Intensity$_d$ is defined as follows:

$$\text{Intensity}_d = (N7_d - N11_d)/N11_d$$

where

$N7_d$ is the number of individuals interviewed in the UHPC (2002) and who completed the 7th grade in district *d*; and $N11_d$ is the number of individuals interviewed in the UHPC (2002) who completed the 11th grade in district *d.*

Regarding the numerator, the quantity of each district's population is then calculated that completed a primary education but did not transition to a secondary education, or that transitioned but dropped out before completing the 11th grade. This is determined by taking each district's differences between the population that completed 7th grade and those completed the 11th grade. Further, this population margin is considered the main beneficiary of (or compliant with) the free secondary education program.[22] One caveat of using this measure as intensity is that the districts with larger populations potentially have larger sub-populations, and thus, a high measured level of program intensity. We standardize this measure by district size by dividing it by the number of 11th grade completers at the same district as noted in the UPHC (2002),[23] which Figure 2 further illustrates.

Figure2-Calculation of intensity measure

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[21] This variable is calculated using 2002 census data. By assuming the relative group size across district remains constant over 7 years, we calculated the number of children aged 10 in each district to proxy the group's size aged 17 in 2010 (school entry at age 6, + 11 years). Similarly, we calculated the age-district group size for test takers in the other test years.

[22] In other words, this intensity measure reveals the program's maximum impact on 11th grade completion rates, assuming that all of this sub-population is induced to complete the 11th grade due to the program.

[23] Ideally, the 2006 data would be used to measure the number of possible compliers just before the program started; however, the 2006 data is unavailable. As the closest approximation, I rely on the 2002 Housing and Population Census to calculate this intensity measure. Hence, this strategy assumes that this relative variation in the 7th and 11th grade completers across districts remains the same over the 5 years between 2002 and 2007.
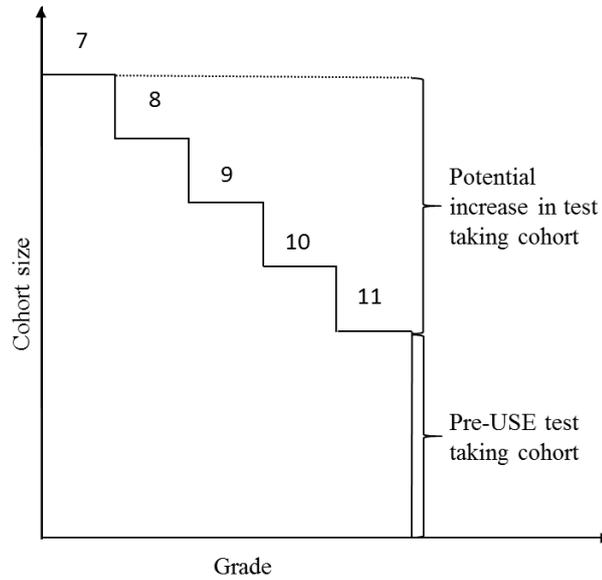
Intuitively, and on the one hand, the program would promote more students to complete lower secondary education in the districts in which many students completed primary school, but did not transit to secondary school, or dropped out before completing the 11th grade. On the other hand, the program has little effect in districts in which no one completed primary school in the absence of the program, because a major barrier exists even in the access to primary education. Further, the effects are likely to be small in the event that all primary school graduates transition to and complete a secondary education, even without the program. Table A-1 in the Appendix describes the calculation of these intensity measures in a district with median intensity: Iganga.

Finally, this paper also examines the change in the number of students who took the UCE exam at the school level by estimating the variant of Equation (1), as follows:

$$Y_{icdt} = \gamma_0 + \gamma_1 \text{Intensity}_{dt} + \gamma_2 X_{dt} + \eta_i + \lambda_c + \lambda_d + \lambda_t + \mu_{icdt} --- (2)$$

where Y_{isdt} is the outcome of school i, in type c, in district d, in year t; and η_i is the school fixed effects. Further, γ_1 indicates the change in each school's test-taking group size: if its sign is positive, this suggests that one school registered more 11th grade students than at the pre-program level. In summary, these regional variations—in the effective benefit from and across group variations after exposure to the USE policy—to examine its effects on students' access, private school supply, learning environment, and achievement. The results will be discussed in the following section.

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Table 2 displays the estimates from the OLS regression, which regresses the intensity measure of district-level characteristics using the data from the UHPC (2002). The results suggest that high-intensity districts are likely to have a highly educated population with more valuable assets. This paper's following sections reveal whether the USE policy improved students' access to secondary education, and whether this academic achievement was adversely impacted more in such districts.

Table 2. The characteristics of high-intensity districts

Outcome	Intensity
Adult population (20 years old and older)	-0.000
	(0.000)
Child population (19 years old and younger)	0.000
	(0.000)
Average educational attainment in year	0.091***
	(0.010)
Unemployment rates among the population aged 20-29	0.417*
	(0.244)
Share of household which owns radio	0.015
	(0.055)
Share of household which resides in Urban area	-0.045
	(0.066)
Share of household which has access to electricity	0.086
	(0.304)
Obsevation	109
R-sq	0.758

Notes: Author's calculation using the Uganda Housing and Population Census (2002). Standard errors are noted in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

4. Results

This section has six objectives. First, Section 4.1 studies whether free secondary education programs increased the number of secondary school graduates. Second, Section 4.2 examines the change in the students' characteristics in secondary education. Third, Section 4.3 will test if the USE policy increased the number of schools in a market. Fourth, Section 4.4 examines its effects on school quality in terms of the physical and human resources available per student. Fifth, Section 4.5 discusses the change in the 11th grade students' average test scores. Finally, Section 4.6 provides a robustness check of these estimates.

4.1 Effects on Students' Access

This section examines whether a free secondary education policy increased the number of students who took the UCE exam. A simple supply-demand analysis predicts that a decrease in the price of education in participating secondary schools will fuel the demand

for secondary education. Hence, the USE policy is likely to increase the number of secondary school graduates.

Indeed, Table 3 indicates the program's significant, positive effects on the number of students taking the secondary school exit exam. The first column results depict the value of the intensity measure in median districts, 0.328, to suggest that the number of test-takers in Iganga increased by 783 students in 2010, compared to the levels in 2009 and before. The size of this increase is as large as approximately 16 percent relative to the number of 11th grade graduates in 2009. This analysis suggests that financial constraints remain an important barrier to accessing secondary education, and the fee-free secondary education program effectively makes secondary school accessible.

To examine whether the effects differ across school type, columns 2 through 5 disaggregate secondary schools into USE-participating government schools, non-USE-participating government schools, PPP-participating private schools, and non-participating private schools. Consistent with our expectations, the results indicate the effects are heterogeneous. Of the 783 total increase in the number of secondary school graduates, 248 (757 * 0.328) are from USE-participating government secondary schools, and 104 (319 * 0.328) are from PPP-participating private schools, in which the program abolished school fees. The remaining 450 (1,373 * 0.328), however, are from non-participating private secondary schools. This result suggests that free secondary education effectively makes secondary schools accessible in both program-participating secondary and non-participating private schools. An interpretation of the results on participating secondary schools is straightforward: the number of graduates increased where the program made schooling financially accessible. However, the reason for the change in non-participating private schools is not straightforwad.

Table 3. Students' access to secondary schools and changes in the number of graduates

Outcome: Number of secondary school exit exam takers per district	All types of secondary school	USE participating Government	Non participating Government	PPP participating Private	Non participatng Private
	(1)	(2)	(3)	(4)	(5)
Intensity	2387.78***	756.916***	-61.232	318.604**	1373.489**
	(791.37)	(198.234)	(89.413)	(129.906)	(604.530)
Observation	763	763	763	763	763
R-sq	0.516	0.566	0.027	0.432	0.213

Notes: Author's calculation using annual UCE data between 2006 and 2012. All specifications control for district-fixed effects, year-fixed effects, and district-year group sizes of the populations in 2003. Standard errors are clustered at the district level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

The change in the number of exit test-takers in non-participating private secondary schools might occur for two reasons. First, consistent with the theoretical prediction in Section 2, the increase in the demand for secondary education in a public secondary education system might exceed its supply, as the quantity of secondary schools and other physical and human resources are inelastic and therefore limited. Then, low-cost, non-participating private secondary schools may enter the market to satisfy this excess demand. Second, as Section 4.4 later indicates, the quality of participating secondary schools might suffer because they became more accessible and congested than at the pre-program level. This change in the class size and student composition might promote middle-class students to exit the public education system to non-participating private secondary schools. These interpretations will be further extrapolated in the following sections.

In summary, this section finds that the program increased the number of 11th grade graduates in both public and private program-participating secondary schools, and as somewhat unanticipated, in profit-seeking, non-participating private schools.

4.2 Effects on School Supply

Section 4.1 revealed that the program increased the number of secondary school graduates in both the public education system and fee-charging private schools. This subsection aims to provide a possible explanation for this change, in that new private schools entered the market, as discussed in the previous section and Section 2, to satisfy the excess demand for secondary education created by the program. This section reveals the reasons behind this seemingly unexpected change in the education market by examining how the

number and types of secondary schools in the market changed after the program.

Table 6 illustrates the change in the number of schools by type since the program began in 2007. First, the total number of secondary school increased in high intensity districts, in which the program enabled more students to graduate from secondary school (column 1). With the value of cumulative intensity in Iganga, the program increased its number of schools in 2007, when it was implemented; by 2012, it had increased the number of secondary schools by approximately 8.2 (43 * 0.191) in its median-intensity district. However, the number of government secondary schools remained at a pre-program level over the years, in contrast to this general change (column 2). Alternatively, the number of private schools more rapidly increased in the high-program intensity district. Further, once we disaggregate private schools into PPP-participating and non-PPP-participating private schools, most of the increase is again explained by the entry of non-PPP-participating private schools (columns 4 and 5). This finding is consistent with the theoretical prediction and the results in Table 3.

Table 4. School supplies

Outcome: Number of secondary school per district	Total number of secondary school	Government	Private	PPP participating private	Non-participating private
	(1)	(2)	(3)	(4)	(5)
Cumulative intensity	42.575**	2.668	39.907**	5.462**	34.444**
	(17.958)	(2.038)	(16.758)	(2.568)	(15.464)
N	763	763	763	763	763
R-sq	0.412	0.374	0.348	0.412	0.242

Note: The authors' calculation using annual UCE data between 2006 and 2012. All specifications control for district-fixed effects, year-fixed effects, and district-year groups' population size in 2002. Standard errors are clustered at the district level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

A possible explanation for this massive private school entry is that the program made the public secondary education system more financially accessible in spite of a limited supply of available seats, thus creating an excess demand for new space for secondary schooling. Thus, profit-seeking private secondary schools entered the market to satisfy such a demand. In summary, the program was likely to indirectly cause private schools to enter the market (Table 4), and more students graduated from fee-charging private schools (Table 3) in high-intensity districts.

The private sector sometimes plays an important role in expanding the access to education in developing countries. One typical example is Chile's substantial voucher program, introduced in 1980. This government program provided a voucher to any child to allow them to attend either public or private school. Under this scheme, both types of

schools received equal government subsidies per the number of students enrolled. This innovative program increased the entrants into the marketplace, and consequently, private school attendance increased from 15% in 1980 to 42% in 2005 (Hsieh and Urquiola, 2006). This study interprets the rapid expansion of Uganda's private secondary school market as paralleling the Chilean experience, and a later section discusses the effects of such a change on the overall educational outcome.

4.3 Effects on students' achievements

This section uses the secondary school exit exam's average test scores to discuss whether the students' learning achievements deteriorated as a result of the program, which may decrease average test scores for the following two reasons:

1. Composition effects: As predicted by the model in Section 2, the composition of students who took the secondary school exit exam may have changed after the program began. If the program makes secondary school accessible to students from less wealthy households, and their abilities positively correlate with household wealth, the "average" abilities of students in secondary education may decline after the program. Such composition effects place downward pressure on average test scores, and this is likely to occur in participating secondary schools. Alternatively, if the program removed the credit constraint on poor but able students, the average ability of the students receiving a secondary education may even improve after the program. In summary, the program's effects on secondary students' average abilities and test scores are both theoretically and empirically ambiguous.

2. Class size effects: The physical and human resources available per student may decrease if the program increases the number of students enrolled without simultaneously increasing the available school resources, such as the number of teachers and classrooms. These class size effects are likely to place downward pressure on the average test scores; according to the preceding sections' findings, such effects may occur in participating secondary schools as well as non-participating private secondary schools.

In summary, the program effects' direction of impact on students' learning achievements is theoretically unclear, as the two effects may work in opposite directions.

The program effect's direction of impact on students' test scores in certain types of secondary schools is also inconclusive, as students' sorting across schools and changes in the learning environment may possibly occur simultaneously. In other words, this study's setting makes it potentially difficult to isolate the changes in students' test scores as the former changes due to the latter. In such a context, Hsieh and Urquiola (2006) proposed an examination of the change in all students' average test scores across one

education market. In doing so, this approach nets the change in test scores as derived from student sorting across schools.[24] This paper first follows the authors' strategy and recognizes Uganda's "districts" as one education market. Thus, the first part of this sub-section presents the changes in average test scores at each district level.

Table 5 reveals the free secondary education program's effects on the secondary school exit exam's district-level mean scores. The results in column 1 have a median intensity value of 0.328, and indicate that the program increased its average test scores by 0.1 standard deviations (-0.32 * 0.328) in the median-intensity district, although this coefficient is imprecisely estimated. This result suggests that at a minimum, the program on average did not decrease the secondary school graduate's academic achievement.

Table 5. Effects on students' achievements and changes in test scores per district

		by school type		
	Aggregate	Participating Government	Participating Private (PPP)	Non participatng Private
Outcome: standardized mean test score per district	(1)	(2)	(3)	(4)
Intensity	0.32	0.03	0.25***	0.03
	(0.22)	(0.08)	(0.07)	(0.07)
Observation	756	751	625	616

Note: The authors' calculation using annual UCE data between 2006 and 2012. All specifications control for district-fixed effects, year-fixed effects, and the district-year group population size in 2002. Standard errors are clustered at the district level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Columns 2 to 5 in Table 5 disaggregate the secondary schools into different school types, which reveal that learning gains are evident in PPP-participating private schools (column 4). These results are consistent with existing experimental research in Uganda. Barrera-

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[24] This approach nets the effects of the change in the across-school composition of students who would have completed the 11th grade even in the absence of a free secondary education policy. However, it is noteworthy that the free secondary education policy may change the overall composition of students who take the UCE exam by allowing more students with diverse backgrounds to complete the 11th grade. Hence, this section estimates the gross change in average test scores, as derived for the following two reasons: One, the free secondary education policy impacts the academic achievement of students who would have completed a secondary education even without the USE policy; two, such a policy influences the composition of students who complete 11th grade due to abolishing school fees.

Ossorio et al. (2015) used a randomized control trial with low-cost private secondary schools to find that developing government partnerships improved students' test scores in low-cost private schools by a 0.2 to 0.3 standard deviation in both English and mathematics. The authors posit that such a learning improvement is driven by student selection; the students in the treatment school had performed better in their primary school exit exams, and came from more well-educated households. Therefore, this suggests that the program removes credit constraints on poor but able children, and that the composition effects outweigh class size effects, at least in PPP-participating secondary schools. However, it may be noteworthy that the students' test scores in PPP private schools prior to the program were the worst among the four types of secondary schools (Table 1). Therefore, the program helped PPP private schools to catch up with the national average test scores, rather than labeling them as elite secondary schools. Alternatively, the test scores in other types of secondary schools remain at values similar to the pre-program level (columns 1, 3, and 4).

Naturally, a subsequent question involves how the program changed the students' average achievements on the secondary school exit exam. First, we discuss whether the increase in average test scores in participating private secondary schools occurred because the program improved the academic achievement of the students who would have graduated from secondary school even before or in the absence of the program. Otherwise, this may simply be because the program removed the credit constraints on poor but able students, and students who could graduate secondary education have better abilities than the pre-program average test scores.

Ideally, we would use individual-level test score data to observe the counterfactual test score distribution of students who would have graduated secondary education even in the absence of the program. However, this study's setting and data do not allow for such an analysis. Instead, we use school-level data to examine the changes in average test scores in the same school over time by including school-fixed effects. In doing so, we limit the sample to the secondary schools in existence since the pre-program period. This allows us to determine whether the average test scores improved in such schools, or if the increase in average test scores at the district level (Table 5, column 1) occurred because the test scores in the newly established secondary school were superior to those in traditional schools.

Table 6. Change in traditional schools in existence since 2006 or before

		by school type			
	Aggregate	Participating Government	Non participating Government	Participating Private (PPP)	Non participatng Private
Outcome: standardized test aggregate score	(1)	(2)	(3)	(4)	(5)
Intensity	0.09***	0.00	0.03	0.06**	0.04*
	(0.01)	(0.02)	(0.06)	(0.03)	(0.02)
Observation	13116	4464	697	2362	5593

Note: The authors' calculation was obtained using annual UCE data between 2006 and 2012. All specifications control for school fixed effects, year fixed effects, and the district-year group population size in 2002. Standard errors are clustered at the district level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 6 illustrates the change in academic achievement in the traditional secondary schools that have existed since the pre-program period. The results in column 1 suggest that the test scores in such a traditional school indeed increased after the program, although the point estimate and effect size of 0.03 in the median intensity district is smaller than in Table 5. Once we disaggregate schools into different types, the government schools' results are qualitatively similar to Table 5 (columns 2 and 3). This is consistent with Section 4.2, which notes that the number of government secondary schools symmetrically increased across districts after the program. The results for private secondary school indicate that, consistent with Table 5, the program increased the average test score in PPP private schools, but the size of change was smaller in traditional schools (Table 5, column 3). This result suggests that the program is likely to have changed academic achievement in the traditional PPP-participating private secondary schools, which have been in operation since the pre-program period. However, the average test score in newly founded PPP-participating secondary schools is simultaneously higher than the pre-program average among PPP-participating private secondary schools.

It is also noteworthy that the coefficient for fee-charging private schools changed from 0.03 to 0.04, and was precisely estimated (column 5, Table 6). These results suggest that the program improved the performance of students in traditional private schools. This is possibly because traditional private schools are likely to cater to wealthy households, and the program may induce some wealthy and able students to transfer to traditional private schools to avoid the post-program student heterogeneity in the public education system. This also suggests that the average test scores of students in newly established private schools are less than those from a traditional private school. This is consistent with our explanation of sorting in Section 4.2. In summary, this section found that at a minimum, the program did not deteriorate the students' learning achievements; it also suggests that the program improves welfare, in that the program increased the number of

secondary school graduates with few negative effects on their academic achievement.

4.4 Effects on student composition

Preceding sections revealed that the program increased the number of students taking secondary school exit exams as well as their test scores. The natural next question involves how the program changed students' test scores. The previous section briefly offered two possible explanation: composition and class size effects. The subsequent two sections explicitly examine whether the program changed either the composition of students in each type of secondary school, or the school resources available per student.

First, we focus on the change in student composition, and the number of orphan enrollments. Orphans are most likely to face financial constraints in the program's absence, and the program is expected to increase their enrollment. The results in the upper panel of Table 7 suggest that orphans possibly switched from fee-charging secondary schools (columns 2 and 4) to a participating secondary education system (columns 1 and 3). If credit constraints force orphans to work to earn and pay for direct educational costs, the program may allow them more hours to study. These time allocation effects from the program would place upward pressure on the test scores in participating secondary schools. On the contrary, depending on the average ability of orphans, the direction of the program composition's effects on test scores in each type of secondary school is theoretically and empirically ambiguous.

Table 7-Effects on student's composition by school type

The effects of free secondary education	USE participating government	PPP participating private	Non participating private
Outcome	(1)	(2)	(3)
Orphan enrolment per district	1230.8***	596.9**	-730.3**
	(330.7)	(258.2)	(353.2)
Observation	784	784	784

Note: The upper panel is derived from the authors' calculation using annual UCE data between 2006 and 2012. Standard errors are clustered at the school level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

4.5 Effects on the learning environment

This section focuses on the class size effects, and assesses whether the

government has sustained the resources available per student by building facilities and employing more teachers in the districts in which the program most increased secondary school enrollments.

The first two rows in Table 8 reveal the change in the physical resources available at secondary schools after the program began. The results demonstrate that the number of classrooms per student decreased in all four secondary school types (row 1). This change may reflect the fact that classrooms were congested after the program began. The number of adequate seating per students (row 2) stayed relatively close to the pre-program level, except in PPP-participating private schools. Hence, the class size effects may place downward pressure on the test scores in this type of secondary school.

The last two rows further indicate the change in the quantity and quality of human resources available per student. The results illustrate that the schools that employed new teachers retained the same number of teachers per student as before the program was implemented (row 3). However, teachers with proper training are not easily available in the market; hence, the number of teachers with certificates per student decreased after the program in all types of secondary schools (row 4). Increasing the number of teachers may be more important for the government than classrooms, as participating secondary schools used a double-shifting system—or using the same classroom twice per day—after the program. Hence, any class size effects may be mitigated by this system.

Table 8. School characteristics

Effects of free secondary education	USE participating Government	Non participating Government	PPP participating Private	Non participatng Private
Outcome	(1)	(2)	(3)	(4)
Classroom per student	-0.018*	-0.033*	-0.048**	-0.184***
	(0.009)	(0.020)	(0.022)	(0.035)
Adequate sitting space per student	-0.060	0.273	-0.503**	-0.166
	(0.148)	(0.319)	(0.233)	(0.164)
Teachers per student	-0.025	0.112***	0.020	-0.026
	(0.022)	(0.041)	(0.034)	(0.032)
Teachers with credential per student	-0.035*	-0.063*	-0.050**	-0.100***
	(0.020)	(0.034)	(0.025)	(0.024)
N	5317	1433	3601	8744

Note: The coefficient of cumulative intensity values have a median of 0.328. The authors' calculation uses annual ASC data between 2006 and 2012. All specifications control for school-fixed effects, year-fixed effects, and district-year group population size in 2002. Standard errors are clustered at the district level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Collectively, this section discovered that free secondary education programs may

increase the number of students per classroom. This change may place downward pressure on students' test scores. However, class size effects might be mitigated by employing more teachers and using existing classrooms twice a day.

4.6 Robustness check

Before concluding these sections, the estimated results are subject to a robustness test by controlling for additional covariates. The upper panel of Table 9 uses the number of students who took secondary school exit exams as a dependent variable to examine the findings' robustness, and column 1 notes the results from the baseline specification for the ease of comparison. Column 2 includes the interactions between the region-fixed effects and year-fixed effects, but the results demonstrate that our primary findings are robust to its inclusion. This set of variables control for the unobserved heterogeneous change in four regions across years, including heterogeneous trends in general economic development. Another concern leading to bias in our estimates involves the effects of government programs that target high unemployment districts. As the USE policy's introduction is timed nearly the same as the 2005 presidential election and global economic recessions, one major challenge involves isolating the effects of such government programs from the USE policy's effects. To address such concerns, column 3 controls for the interaction between pre-program unemployment rates and year-fixed effects, but the estimates remain similar. Finally, a reconstruction from the civil war in Uganda's northern regions would affect secondary school students' access and achievements at the same time as the USE policy. To provide conservative estimates to address this concern, column 4 excludes the sample from the northern region; thus, our estimates are robust for this specification. The same robustness test is conducted by using the average test score as a dependent variable, as noted in the lower panel of Table 9. These results conclude that our preceding sections' results are not spurious.

Table 9. Robustness check

Outcome: Number of secondary school	Baseline	Region*Year	Unemployment	Without north
exit exam takers per district	(1)	(2)	(3)	(4)
Intensity	2387.78***	2456.45***	2282.71***	2385.20**
	(791.37)	(761.50)	(735.69)	(1003.20)
N	763	763	763	567
R-sq	0.516	0.567	0.548	0.556
Outcome: Standardized aggregate	Baseline	Region*Year	Unemployment	Without north
test score per district	(1)	(2)	(3)	(4)
Intensity	0.32	0.42*	0.39*	0.11
	(0.22)	(0.26)	(0.20)	(0.14)
N	756	756	756	560
R-sq	0.954	0.963	0.958	0.962

Note: The authors' calculation uses annual UCE data between 2006 and 2012. All specifications control for district-fixed effects, year-fixed effects, and the district-year group population size in 2002. Standard errors are clustered at the district level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

5. Conclusion

This study has examined how the elimination of public secondary school fees, which comes in tandem with Public-Private Partnership scheme, affects the educational outcomes and the education market as a whole. Exploiting the across-cohort discontinuity in exposure to the program and across-district variation in the effective benefit from the program, we have shown that the program substantially boosted the number of secondary school graduates in public, PPP private and non-PPP private schools. These increases were particularly supported by the large entries of non-PPP private schools. We have also found that the program improved the test scores in secondary school exit exam in PPP private and non-PPP private schools, though these schools experienced deterioration in the amount of resources per student. There is also evidence for sorting: that is, children from lower socioeconomic background, who are unlikely to have been able to attend secondary education, became able to complete it in subsidized sectors (public and PPP schools), while their share dropped in the fee-charging sector.

These results suggest that the financial constraint remains the important barrier to the access to secondary education in Africa. They also indicate that free secondary education program is likely to improve access without sacrificing students' learning outcomes very much. While this could be a substantial burden on the government budget, in the context with a relatively active private education sector in the market, the results imply that PPP is likely to provide one of the cost-effective ways to finance a free secondary education program.

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Appendix A. The definition of cumulative intensity

This intensity measure is likely to be valid for an analysis in which the policy effects were only observed in 2010 and after. Hence, this intensity is used when examining the effects of a free secondary education policy on the test-taking group size and its academic achievement. However, the USE policy affected some outcomes after its 2007 introduction, such as the number of those enrolled in secondary school, the number of public and private schools, and the quality of the learning environment. When assessing these effects, I use the variant of the aforementioned intensity measurement to assess the policy's effects on these outcomes. For example, when assessing whether the USE policy's introduction increases the total enrollment in the lower secondary school from 8th to 11th grade, $\text{CumulativeIintensity}_{dt}$ is used, which is defined as follows:

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

$N8_d$: The number of individuals interviewed in the 2002 UHPC and who have completed the 8th grade in district *d*

$N9_d$: The number of individuals interviewed in the 2002 UHPC and who have completed the 9th grade in district *d*

$N10_d$: The number of individuals interviewed in the 2002 UHPC and who completed the 10th grade in district *d*

$\text{CumulativeIintensity}_{dt}$ takes the same value as in the previous 2006 analysis. On the contrary, in 2007, $\text{CumulativeIntensity}_{dt}$ takes the potential proportional increase in the 8th grade compared to the pre-program total enrollment. In other words, its numerator is the potential absolute increase in the 8th grade group, divided by the sum of the pre-USE group sizes in the 8th, 9th, 10th, and 11th grades.[25] Its numerator for 2008 is the sum of the 8th and 9th grade potential increases, holding the denominator as the

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[25] Intensity = [(potential increase in the 8th grade) + (potential increase in the 9th grade)] / [(pre-USE 8th grade group size) + (pre-USE 9th grade group size)]

pre-program total enrollment size. The same strategy is then employed to calculate the district-level cumulative intensity in 2009 and after. The Appendix describes the calculation of these intensity measures in Iganda, the median-intensity district.