Secondary school access raises primary school achievement in Tanzania

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Abstract

Do investments in education today depend on access to education tomorrow? In 2015, Tanzania abolished fees in public secondary schools, creating an opportunity to study the effect of increased secondary school access on primary student learning at scale. Using the universe of Tanzanian standardized test results, I first confirm that the reform increased secondary school access: secondary enrollments rose, primary scores became a better predictor of secondary transition, and the elite advantage in transition rate disappeared. I then examine the impact of the secondary fee abolition on primary learning, using data on exam fee non-payments to define “treated” students as those from ex-ante financially constrained families. Difference-in-difference estimation using ward and year fixed effects shows that the reform increased pass rates by 2 percentage points and transition to secondary school by 3 percentage points. This demonstrates that primary education decisions in this context are forward-looking, and that free secondary education programs may have greater benefits than previously understood.

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1 Introduction

Does it make sense for developing-country governments to expand secondary school access even as primary school pupils face a learning crisis? The world is nearing universal primary school enrollment, but students in many countries still fail to acquire basic literacy and numeracy (Barro & Lee, 2013; Glewwe & Kremer, 2006; Pritchett & Beatty, 2015). Recent policies in parts of the developing world to provide free secondary education (FSE) have many detractors. Opponents see FSE as a regressive transfer to the rich (Doe-Glah, 2017; O’Malley, 2015). Others point to research suggesting a trade-off between access and quality (Garlick, 2019; Bold, Kimenyi, Mwabu, & Sandefur, 2015). Others prefer to focus expansion efforts on pre-primary education, citing research suggesting that early educational investments matter most (Attanasio, Meghir, & Nix, 2020; Cunha & Heckman, 2007; Heckman, 2006).

A crucial-yet-absent question in these debates is whether secondary access itself affects primary students’ learning. FSE’s direct effect is to raise secondary enrollment by removing financial constraints for qualified students (Das, Singh, & Yi Chang, 2022). But it may also indirectly raise secondary enrollment by motivating a wider set of high-ability poor students who, sans access to secondary school’s greater returns, rationally disinvest in primary education (Becker, 1964). The presence of these cross-age spillover effects of access would enrich researchers’ understanding of the inter-temporal aspects of education production functions. It could also alter the cost-benefit calculus for FSE programs. But the lack of granular data on learning outcomes, and the dearth of exogenous sub-national variation in the reach of at-scale FSE programs, have made it difficult thus far to test for a reduced-form effect of secondary school access on primary student learning.

In theory, there are many reasons to suspect secondary access might affect the decisions of a forward-looking primary student. Some potential channels include option value (Stange, 2012), continuation value (Heckman, Humphries, & Veramendi, 2018), dynamic complementarities (Foster & Gehrke, 2017), and cumulative returns (Navarro-Sola, 2019).¹

This paper examines the effect of secondary school access on primary student learning in the context of Tanzania’s 2016 abolition of public secondary school fees. Tanzania in 2016 had a wide gap between net primary enrollment (83.5%) and net secondary enrollment (23.9%). Prior to this time, public secondary schools charged official school fees of about 20,000 TZS (9 USD). But other compulsory contributions – for services such as building maintenance and security, and supplies such as desks and exams – could easily

¹Decisions about individual investments in primary schooling may be taken by some combination of students and their parents or guardians. For simplicity, I refer to this set of makers of decisions relevant to students as “students.”
rise to the order 300,000 TZS (139 USD).\textsuperscript{2} One month before the January 2016 start of the school year, the president announced that starting that year, all basic education through the fourth year of secondary school would be free and compulsory. A notable feature of the program as announced was that all fees, official and unofficial, were to be covered, and school attendance was intended to be truly fee-free at the point of sale. Schools’ foregone revenues were to be replaced by capitation grants of USD 5 per student (Taylor, 2016).

Credible measures of learning come from the universe of administrative data on high-stakes exams from mainland Tanzania for primary and secondary school. The data span 2013-2021, a period which brackets the 2016 FSE reform. The Primary School Leaving Examination (PSLE) is taken at the end of the last year of primary school. The Form Two National Assessment (FTNA) is the first exam of secondary school, taken at the end of the second year. Both exams are high-stakes for students: they must be passed to advance to the next grade. To measure primary students’ learning, I use PSLE exam performance. To measure primary students’ transition to secondary school, I test whether PSLE takers sat the FTNA two years later, by matching on name (97% of names are unique within cohort).\textsuperscript{3}

To pinpoint the students most likely to have been affected by the reform’s alleviation of financial constraints, I use student-level microdata on exam fee payments. Exam fee payment status is available for the 2014 (pre-reform) cohort of secondary school FTNA takers. Because I am interested in primary student outcomes, I seek to link these FTNA takers to their likely younger siblings (in Tanzania, siblings tend to share a last name). I limit attention to students with a unique last name within their cohort / times ward\textsuperscript{4}. I match the 2014 fee payers and non-payers to later cohorts of PSLE takers by last name and ward. I consider these matched students to be sibling sets, and limit my main analysis to PSLE takers who match to an older sibling. I then define each PSLE student as financially constrained – “treated” – if their older sibling failed to pay exam fees. I argue that students from these families are more likely to have been affected by FSE’s easing of financial constraints. Prior to the reform, primary students from these families were 2 percentage points less likely to transition from primary to secondary school than students from other families in their ward, suggesting it is a good measure of financial constraints.

To identify the reform’s causal effect, I compare how the difference between the performance of financially-constrained students and their less-constrained peers changed after the reform. To make com-

\textsuperscript{2}See Habyarimana, Opalo, and Schipper (2020) for one example of how Tanzania paid for national government public goods by requiring contributions from local resident users.

\textsuperscript{3}This measure is likely an undercount, as I explain further in Section 3.

\textsuperscript{4}Wards (kita in Kiswahili) are the third administrative division, a very granular geographic division, of which there are 2303 in my sample.
parisons as clean as possible, all specifications include year and ward fixed effects. My identification rests on the assumption that in the absence of the reform, the gap in outcomes between these two groups of students would have remained constant. I show that the gap did not shrink at all in the two years prior to the reform. This treatment definition identifies only those families who were sufficiently constrained that they failed to pay exam fees; other families who made sacrifices in order to pay exam fees may also be considered financially constrained. My empirical strategy identifies the differential effect of the policy on the most financially constrained families, but may be considered a lower bound on the overall effect insofar as it fails to capture the effect of the alleviation of less severe constraints.

I first show that the FSE policy, which was intended to increase access to secondary school, did so. Raw descriptive measures are illuminating: the number of secondary test takers began to rise after the policy’s implementation (so did the number of primary test takers) (Figure 2). A contemporaneous household survey carried out by a Tanzanian NGO found that people widely believed that the government would make good on its promise to abolish fees (Twaweza East Africa, 2016a). After the policy’s implementation, PSLE score suddenly became a much stronger predictor of transition to secondary school, suggesting that many clever kids who were previously too poor for secondary school could now attend (Figure 3). Perhaps most strikingly, the elite advantage in secondary transition disappeared. I measure eliteness by whether a student shares a last name with a candidate for local District Councillor (diwani) in her ward in the 2015 election. Prior to the reform, elite children were about 3 percentage points more likely to attend secondary school conditional on their primary school scores. After the reform, this difference fell to zero (Figure 4).

My main diff-in-diff results show that increased secondary school access raised both the exam pass rate and the transition rate for primary students. After the reform, students from financially-constrained families began to catch up to other students in both outcomes. By the third year of the reform, the pass rate gap had disappeared completely (Figure 5). The reform raised pass rates for treated students by 3% (a 2 percentage point increase from a base of 74%). It also increased the transition rate by 6% (a 3 percentage point increase from a base of 49%). Because these estimates rely on within-school variation in treatment exposure, I can rule out alternative explanations involving changes to staffing or resources at the school level. I check the robustness of these findings using alternate methods for defining the set of “treated” students likely to be constrained ex ante by school fees: whether they had an older sibling who failed to transition to secondary school, and whether they had an older sibling who failed the PSLE (conditional on being close to the passing threshold). These results broadly confirm the results from my
main specification.

I also test whether the reform affected primary school choice and school entry at the school market (ward) level. The years following the reform saw a modest overall increase in the number of new primary schools and the number of students taking the PSLE. I test whether these increases were concentrated in places with more financially constrained families. I define “treated” wards as those with a non-zero number of 2014 exam fee non-payers. A diff-in-diff at the ward level shows that the influx of primary students was concentrated in “treated” wards which had high pre-reform pass rates, but that primary school entry occurred uniformly across wards. This implies 1) the reform increased primary students’ educational investment through the selection of higher-quality schools; and 2) primary school supply did not respond, at least in the short term, to the reform. This further suggests that my main results could represent a lower bound on the reform’s true effect on learning, since they are identified only off of variation within wards. Indeed, the ward-level analysis shows larger effects on pass rates and transition rates than the individual-level analysis does.

This paper is the first to empirically examine the effect on primary student learning of a secondary school fee abolition at scale in Africa. Despite the proliferation of FSE programs across the developing world, and studies about their direct effects, no existing work documents their impact on younger student outcomes (Brudevold-Newman, 2019; Garlick, 2019; Blimpo, Gajigo, & Pugatch, 2019). Previous studies from other contexts offer precedents for increased access to higher levels of schooling affecting student outcomes at lower levels. Mukhopadhyay and Sahoo (2016) shows that secondary school construction increases nearby primary enrollment, and Jagnani and Khanna (2020) finds that elite public college construction increases educational attainment among school-age children. A related literature shows that educational investments respond to increases in the returns to education (Khanna, 2020; Jensen, 2012). To this literature I am able to add micro data on high-stakes learning outcomes in Africa.

This paper contributes to the body of research evaluating free education programs. In light of that research, FSE was far from guaranteed to raise learning: many of the Free Primary Education (FPE) programs of recent decades have failed to create measurable learning gains (Kremer, Brannen, & Glennerster, 2013; Bold et al., 2017; Pritchett, 2013).5 Tanzania abolished primary school fees in 2002, with uneven results: large increases in pupil-teacher ratios and reductions in teacher quality, no big overall test score gains, and even some reduced test scores in urban areas (Valente, 2015). The FPE capitation grants which

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5This is not to diminish the extraordinary and rapid progress in enrollments in recent decades: the average developing country resident now has more years of education than the average developed country resident did in the 1960s (Barro & Lee, 2013).
were supposed to replace school fees often failed to materialize \citep{TwawezaEastAfrica2013}. Garlick \citep{Garlick2019} shows that a South African FSE program reduced pass rates and school quality by inducing overcrowding, and this tension between access and quality is a common feature in the chronicles of efforts to expand enrollment.

Most existing evidence on the effect of higher-level access on lower-level learning comes from the literature on scholarships. \citet{DufloDupasKremer2017} shows that secondary school scholarships improve secondary school learning (and many other outcomes). \citet{KremerMiguelThornton2009} show that offering secondary school scholarships to girls in Kenya increases effort and achievement in primary school. \citet{LaajajMoyaSanchez2018} examines a similar question in the context of higher education, showing that the introduction of university merit scholarships in Colombia led to improved test scores for high school students. While these results come from relatively small-scale RCTs (with the exception of \citet{Laajajetal2018}), my results show that a similar dynamic holds for a policy which increases secondary access at scale nationwide \citep{MuralidharanNiehaus2017, BoldKimenyiMwabuNg‘ang’aSandefur2018}.

The rest of this article is structured as follows: Section 2 provides background on education in Tanzania, including the FSE policy and its reception. Section 3 details and contextualizes the test score data. Section 4 shows descriptive evidence that FSE did in fact increase secondary school access. Section 5 lays out the empirical strategy; Section 6 presents the results. Section 7 examines student sorting and school entry at the level of the school market (ward). The conclusion is Section 9.

\section{Background}

Primary schooling in Tanzania begins officially at age seven, and consists of seven years ("standards"). At the end of Standard 7, students sit the Primary School Leaving Examination (PSLE). This is a national standardized test, and passing it is a requirement for being admitted to a public secondary school (private secondary schools are not bound to require a passing grade on the PSLE). Primary net enrollment in Tanzania was 81\% as of 2018 \citep{TheWorldBank2021a}. Public primary schools have been fee-free since 2001 \citep{Valente2015}.

Secondary school consists of 4 years ("forms") of "ordinary level" followed by two years of "advanced

\footnote{Other FPE programs which failed to increase test scores include \citep{KadzamiraRose2003, SamarraiZaman2007, Deininger2003, MbitiLucas2012}, although it should be noted that given the large influx of previously underschooled pupils caused by these programs, a very reasonable case can be made that a null effect on test scores is a big success.}
level”. At the end of year 2 of ordinary level, students sit the Form Two National Assessment (FTNA), which must be passed in order to progress to the next grade. As of 2018, secondary net enrollment stood at 26% (The World Bank, 2021a, 2021b).

Primary schools and ordinary secondary schools begin the school year in January. The PSLE is administered in September; the FTNA is administered in November.

Descriptive data suggest there may be high returns to secondary education in Tanzania. As of 2015, those who had finished secondary school were 32 percentage points more likely to report having been an employee for most of the last year, and 12 percentage points more likely to report earning a wage. Among wage earners, median wages were three times higher for secondary school finishers ($10 vs $3 USD).²

### 2.1 2016: Free Secondary Education

In December 2015, the government abolished fees in public secondary schools. This policy was announced by the newly-elected president John Magufuli in December 2015, after that year’s students year had already taken their standardized tests.

Officially announced in government Circular No. 5, the program forbade schools from soliciting fees or contributions from students or parents, declaring that “provision of free education means pupils or students will not pay any fee or other contributions that were being provided by parents or guardians before the release of new circular.” In his speech announcing the policy, President John Magufuli emphasized the comprehensive nature of the fee abolition, and underscored his commitment to it by injecting a note of menace toward would-be fraudsters: “When I say free education, I indeed mean free . . . The funds for providing free education are being set aside, already we have TSh 131bn. We have planned to transfer these funds directly to all the relevant schools, with copies sent to the Regional and District Commissioners, and to the council Director. This is why we say they will study for free. All the money for capitation grants, money for chalk, money for examinations, money for everything, we are sending it. We will send it each month starting this December. Money for food. I am certain that those being sent the money will use it well, I warn them not to use it badly.” (Taylor, 2016) The government also put its money where it mouth was: the policy coincided with an increase in the education budget from 3.9 trillion to 4.77 trillion TZS – an increase of 870 billion TZS (≈ 375 million USD). (Twaweza East Africa, 2016b)

Out of the gate, a common worry among the commentariat was that the program would lead to

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²From the nationally representative Tanzania National Panel Survey 2014-2015; \( N = 5,319 \). I use the exchange rate of 2164.55 TZS = 1 USD, the rate as of 1 January 2016 according to https://www.xe.com/currencycharts/?from=USD&to=TZS&view=10Y
overcrowding in schools. At the same time FSE was likely increasing demand for schools, the government decided to simultaneously expel foreigners working without proper permits, including thousands of teachers from neighboring Kenya, further exacerbating worries of a supply crunch (The Economist, 2016).

But the available evidence is that the program was seen favorably by ordinary Tanzanians. In a nationally-representative phone survey conducted by the NGO Twaweza from December 2015 to January 2016, 88% of the 1,894 respondents said it was “likely” that the promise of free education would be implemented on the announced date; 76% reported that they expected fee-free education to improve quality by improving the teaching environment (Twaweza East Africa, 2016a). A few months later, in August 2016, 50% of 1,806 respondents reported that the quality of education had “become better” since becoming fee-free, with 35% saying it had stayed the same and only 15% saying it had become worse (Twaweza East Africa, 2016b).

3 Data

My primary outcome data comes from the universe of administrative-date high-stakes exam scores which determine whether and how students can continue in their schooling. The agency responsible for administering these examination is the National Examinations Council of Tanzania (NECTA). For each of these exams, the exam papers are gathered to a central location to be graded, rendering remote the possibility of systematic manipulation in grading.

Tanzanians have seven years of primary school, called Standard 1 through Standard 7. The school year goes from January to November, and at the end of Standard 7 – around age 13 for students who remain on-track – students sit the Primary School Leaving Examination (PSLE). Students must pass the PSLE to be admitted to government secondary schools; private secondary schools have the option of admitting whomever they choose, but many also consider the PSLE, especially the most selective.

There are six years of secondary school, called Form 1 through Form 6. The first national standardized test of secondary school comes at the end of Form 2: the Form Two National Assessment (FTNA), and it determines whether students can continue forward in their schooling. At the end of Form 4, students sit the Certificate of Secondary Education Examination (CSEE) – roughly corresponding to the “O-level” in the British system. Students who continue through the sixth year of secondary school take the Advanced Certificate of Secondary Education Examination (ACSEE) – the equivalent of the British “A-level.” Note
that Tanzania’s 2016 FSE policy covered schooling only from Standard 1 to Form 4.

I focus on two of these exams: the Primary School Leaving Examination (PSLE), the last exam of primary school; and the Form Two National Assessment (FTNA), the first exam of secondary school.

### 3.1 The PSLE

NECTA’s website describes the PSLE as “a selection test which enables the government to select form one entrants for its schools,” and specifies that any pupil who has completed Standard 7, whether in a government or a private school, may sit the exam.

The PSLE consists of five sections: Mathematics, Science, English, Kiswahili, and Social Studies. Each section has a maximum score of 50 marks, with letter grades corresponding to A, B, C, D, and E corresponding to minimum marks of 40, 30, 20, 10, and 0 respectively. The overall PSLE score is the sum of marks earned on each of the five sections, yielding a maximum overall score is 250. The passing threshold is 100 marks overall. For the sake of legibility I convert these scores to a 100-point scale, so the passing threshold is 40%.

NECTA provides noisy measures of students precise PSLE scores. Outcome data for each student include their letter grade on each subject of the exam, as well as the letter grade of their overall marks. To convert these coarse categories into proxy scores, I let each grade correspond to the midpoint of its range. I.e., I code an “A” as 90%, and a “C” as 50%, etc. I then create a continuous measure of students’ “combined score” by adding together their five subject scores and dividing by five. NECTA’s indicator for whether a student passed the exam is precise: this consists of achieving a “C” or better letter grade in the “Student Avg”. In the period from 2013-2018, between 760,000 and 944,000 pupils sat the PSLE each year. Over the same period, the nationwide pass rate fluctuated between 50% (in 2013) and 70% (in 2018).

### 3.2 The FTNA

The Form Two National Assessment (FTNA) is the first major test students take in secondary school. It is taken at the end of a student’s second year of secondary school, and is required to continue on to the third year of secondary school.

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8Figure A.1 shows some example questions from the English and Math portions of the PSLE.

9Section B details efforts to validate this proxy measure.
3.3 Matching students

While neither PSLE nor FTNA data include unique student identifiers, they do include student names – typically first, middle, and last. Of the 6 million primary school pupils who sat the PSLE from 2013 to 2019, 98% have a name that uniquely identifies them within their cohort nationwide.

3.3.1 The transition rate

A key quantity of interest is the transition rate – the fraction of students who make the transition from primary school to secondary school.\(^\text{10}\) NECTA does not publicly report direct measures of transition rates, so I construct a proxy measure.\(^\text{11}\) I match the names of PSLE students in a given district to the names of FTNA takers from the same district two years later. This measure likely undercounts the true number of transitioners for at least three reasons: 1) it misses pupils who transition to secondary school but drop out sometime in the first two years before taking the FTNA; 2) it misses pupils who skip ahead or fall behind the normal grade progression schedule; and 3) it misses pupils whose name changes between PSLE and FTNA, including misspellings.\(^\text{12}\)

3.3.2 Financially constrained families

NECTA data are not linkable to census measures of household income, but a quirk of data reporting permits the identification of students whose families likely faced financial constraints to educational investments. Data is available indicating which FTNA takers in 2014 had their results withheld for lack of fee payments (these exam results were later made available). These students come from families that value education enough to have had at least one child make it halfway through secondary school. The failure to pay exam fees thus functions as a reasonable proxy indicator of which families faced binding financial constraints on secondary school access.

Because I am interested in outcomes for primary students, I identify primary students from these families by linking PSLE takers to 2014 FTNA takers who share a last name in the same ward. Wards are small: they are the third administrative unit of Tanzania, there are over 2200 of them, and the median ward has around 200 PSLE takers per year in the period I study. I first identify all 2014 FTNA takers whose last name is unique within their ward – 62% of the sample. I then identify PSLE students whose

\(^{10}\)Note that in this paper I refer to both “dropout” and “transition” – these quantities contain the same information; transition is simply 1 - dropout.

\(^{11}\)Section B details efforts to validate this proxy measure.

\(^{12}\)See also Blackmon (2017), who performs a similar same string matching procedure in order to measure school value-added.
last name is unique within their ward and year, which eliminates about half of the sample (there are more PSLE takers than FTNA takers, so duplicate names are more likely). I then merge these two unique-last-name subsamples, yielding about 27,000 PSLE takers in each year who have unique last names within their ward which are the same as the unique last name of someone who took the FTNA in 2014 in the same ward. I refer to these pairs of students within wards who share a last name as “siblings.” These students constitute the main analysis sample for my empirical strategy for identifying the causal effect of secondary access on primary outcomes (see Section 5).

3.3.3 Elites

I also use names to identify (much smaller) set of students likely at the other end of the income spectrum: the relatives of elites. I match the last names of PSLE takers to the last names of candidates standing for local council (diwani) elections in the same ward. Of the 2,987 wards with mappable electoral results, 1,865 of them contain at least one PSLE taker with the same last name as at least one ward council election candidate. I designate these PSLE takers as “elites.”

Table 1 displays summary statistics at the student level. Column 1 summarizes variables for the full universe of PSLE takers 2013-2019. Column 2 summarizes variables for the main analysis sample: PSLE takers in years 2013-2019, whose school can be located in a ward, whose last name is unique within their ward, and who can be matched to a 2014 FTNA taker in the same ward (see Section 5 for the sample selection criteria).
Table 1: Summary stats: PSLE takers

<table>
<thead>
<tr>
<th></th>
<th>All</th>
<th>Analysis Sample</th>
</tr>
</thead>
<tbody>
<tr>
<td>Female</td>
<td>0.528</td>
<td>0.527</td>
</tr>
<tr>
<td></td>
<td>(0.499)</td>
<td>(0.499)</td>
</tr>
<tr>
<td>Public school</td>
<td>0.967</td>
<td>0.961</td>
</tr>
<tr>
<td></td>
<td>(0.180)</td>
<td>(0.193)</td>
</tr>
<tr>
<td>km to nearest sec. school</td>
<td>3.491</td>
<td>2.656</td>
</tr>
<tr>
<td></td>
<td>(5.734)</td>
<td>(4.424)</td>
</tr>
<tr>
<td>School cohort size</td>
<td>83.557</td>
<td>87.177</td>
</tr>
<tr>
<td></td>
<td>(69.309)</td>
<td>(72.542)</td>
</tr>
<tr>
<td>Pass PSLE</td>
<td>0.677</td>
<td>0.760</td>
</tr>
<tr>
<td></td>
<td>(0.468)</td>
<td>(0.427)</td>
</tr>
<tr>
<td>A on PSLE</td>
<td>0.028</td>
<td>0.034</td>
</tr>
<tr>
<td></td>
<td>(0.166)</td>
<td>(0.181)</td>
</tr>
<tr>
<td>Took FTNA 2 yrs later</td>
<td>0.437</td>
<td>0.531</td>
</tr>
<tr>
<td></td>
<td>(0.496)</td>
<td>(0.499)</td>
</tr>
<tr>
<td>Elite</td>
<td>0.006</td>
<td>0.004</td>
</tr>
<tr>
<td></td>
<td>(0.077)</td>
<td>(0.061)</td>
</tr>
<tr>
<td>Full name unique in cohort</td>
<td>0.979</td>
<td>0.987</td>
</tr>
<tr>
<td></td>
<td>(0.144)</td>
<td>(0.114)</td>
</tr>
<tr>
<td>Last name unique in country × cohort</td>
<td>0.073</td>
<td>0.043</td>
</tr>
<tr>
<td></td>
<td>(0.259)</td>
<td>(0.203)</td>
</tr>
<tr>
<td>Last name unique in ward × cohort</td>
<td>0.480</td>
<td>1.000</td>
</tr>
<tr>
<td></td>
<td>(0.500)</td>
<td>(0.000)</td>
</tr>
<tr>
<td>Matched to 2014 FTNA sibling</td>
<td>0.030</td>
<td>1.000</td>
</tr>
<tr>
<td></td>
<td>(0.170)</td>
<td>(0.000)</td>
</tr>
<tr>
<td>Treatment (sibling failed to pay exam fee)</td>
<td>0.155</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.362)</td>
<td></td>
</tr>
<tr>
<td>N</td>
<td>6,068,930</td>
<td>156,814</td>
</tr>
</tbody>
</table>

An observation is an individual student. The full universe includes all PSLE takers from mainland Tanzania 2013-2019. The analysis sample is restricted to PSLE takers in years 2013-2019, whose school can be located in a ward, whose last name is unique within their ward, and who can be matched to a 2014 FTNA taker in the same ward.

Figure 1 maps the location of primary and secondary schools where students have taken the PSLE and FTNA in 2015, the last year before the policy change. Black lines represent the boundaries of wards, the third administrative division.
4 Descriptive: secondary access increased

The first piece of *prima facie* evidence that FSE increased secondary access is that the number of FTNA takers increased in the years after the policy. The policy also preceded dramatic jumps in the number of PSLE takers, again providing *prima facie* evidence consistent with the idea that FSE had effects on forward-looking primary students.\(^\text{13}\)

\(^{13}\)Cilliers, Mbiti, and Zeitlin (2020) provides evidence that “Big Results Now,” a previous government education initiative to share low-stakes performance information among district education officers beginning in 2013, caused schools to strategically exclude students from the last year of primary school. This could explain the falling trend in test takers for both PSLE and FTNA prior to the introduction of FSE.
4.1 PSLE became a better predictor of transition to secondary school

One plausible measure of the bindingness of financial constraints is how many students who qualify for secondary school fail to go. If it’s true that secondary school fees prevent poor clever kids from attending secondary school, one might expect the relationship between a school’s PSLE pass rate and its transition rate to strengthen following the reform. This is the pattern we see in the binscatter displayed in Figure 3, which plots PSLE score against the probability of transition, separated by year. As expected, all years see almost no transition for students scoring under 40, the PSLE passing threshold. All years also evince a large discontinuity at 40; passing the PSLE is associated with a sharp increase in the likelihood of transitioning to secondary school. However, the size of this discontinuity increases dramatically between 2015 (the last year before the reform) and 2016, the first year of the reform’s sudden implementation. It jumps even higher for the years 2017-2019, after schools and pupils had a chance to digest the policy.  

The modest increase in 2015 could also signify a response to FSE; the reform was announced after students had taken their primary exams, but before the beginning of the next secondary school year, so students who had passed may have had the chance to alter their enrollment decisions in response, and students who otherwise might have dropped out during Form 1 may have stayed in school. FSE was also a campaign promise mentioned by multiple candidates during the 2015 election cycle, so some people might
suggests that removing the financial constraint on secondary enrollment strengthened the relationship between passing the PSLE and enrolling in secondary school, allowing more poor clever kids to transition to secondary school.

Figure 3: Binscatter: school-level pass rate and primary to secondary transition rate

Binscatter of PSLE score vs. transition probability at the student level. The government announced the abolition of school fees for secondary public schools at the end of 2015 (after that year’s exams).

Regression tables also show how the relationship between passing the PSLE and transitioning to secondary school strengthened post-reform. Furthermore, because there is a discrete threshold for passing the PSLE, it is possible to use a regression discontinuity to measure how the causal effect of passing the PSLE changed post-reform. Here I define pupils near the cutoff as pupils whose total number of marks falls in the window between 170 to 230 marks, where 200 is the threshold to pass. In terms of the 5 subjects of the PSLE, this window corresponds runs roughly from “4 D’s and 1 C” to “4 C’s and 1 D.” NECTA’s coding of grades as corresponding to a certain window of possible marks in this data makes it impossible to know exactly how many marks a student received. There are students in the data, who both passed have anticipated it in 2015.
and did not pass, all through the 170-230 window. Students who passed are clearly delineated with a “C” overall, but my measure of the exact number of marks they got – how close they are to the threshold – is inexact. This lends itself nicely to the RD setup I use here.

Table 2: Passing PSLE became a better predictor of transition after the reform

<table>
<thead>
<tr>
<th></th>
<th>Full sample</th>
<th>Near passing threshold</th>
</tr>
</thead>
<tbody>
<tr>
<td>Pass PSLE</td>
<td>0.274***</td>
<td>0.253***</td>
</tr>
<tr>
<td></td>
<td>(0.002)</td>
<td>(0.002)</td>
</tr>
<tr>
<td>Pass PSLE × Post 2015</td>
<td>0.245***</td>
<td>0.263***</td>
</tr>
<tr>
<td></td>
<td>(0.002)</td>
<td>(0.003)</td>
</tr>
<tr>
<td>N</td>
<td>3246640</td>
<td>1234270</td>
</tr>
<tr>
<td>Adj. R²</td>
<td>0.300</td>
<td>0.290</td>
</tr>
<tr>
<td>Mean (pre-reform)</td>
<td>0.160</td>
<td>0.153</td>
</tr>
</tbody>
</table>

Standard errors clustered by school. All regressions include fixed effects for year and school. Column 2 limited to PSLE takers who scored near the passing threshold: between 170 and 230 inclusive (out of a possible 500 in total).

* p<0.10, ** p<0.05, *** p<0.01

Table 2 shows results from a regression of transition on passing the PSLE. Both columns include school and year fixed effects. Column 1 shows the coefficients from the full sample; Column 2 is limited to students whose PSLE score is near the threshold. In both estimations, passing the PSLE becomes much more strongly associated with transition to secondary school after the introduction of FSE.

4.2 The gap between elite and non-elite transition rates disappeared

Another index of school access is how regular folk fare in the system relative to elites. I match the last names of PSLE takers to the last names of candidates standing for local council elections in the same ward. Of the 2,987 wards with mappable electoral results, 1,865 of them contain at least one PSLE taker with the same last name as at least one ward council election candidate. I designate these PSLE takers as “elites.”

Prior to FSE, the elite PSLE takers were 2.8 percentage points likelier to transition to secondary school than non-elites in the same ward who earned the same PSLE score (the mean was 27%).

Figure 4 shows that after the introduction of FSE, this difference fell to zero. This is consistent with FSE reducing financial barriers to secondary school and increasing access.
Figure 4: Elite v. non-elite transition gap disappears under FSE

Point estimates and 95% confidence intervals represent linear combination of main effect + interaction effect from regressing transition on the interaction of “elite” dummy with year indicators, including year and PSLE-score-by-ward fixed effects. As 2015 is the omitted year for the interaction, point estimate for 2015 represents the main effect of “elite.” Coefficient on “elite” is 0.028. Standard errors clustered at the ward level.

5 Empirical strategy

The challenge in identifying the causal effect of Tanzania’s FSE policy is that it came into effect for the entire country simultaneously. There was no staggered rollout nor randomized trial. Who is the comparison group?

I argue that the policy can be thought of as mainly affecting those for whom financial constraints on secondary schooling were binding in the absence of the reform. As mentioned in Section 3, it is possible to identify primary school students from financially constrained families, using a list of FTNA exam fee payments (and non-payments). While these certainly weren’t the only students who faced any kind of financial constraints, they are those for whom we have evidence that financial constraints imposed by secondary school fees (in this case, exam fees) were binding in the absence of the reform.
My empirical strategy is a difference-in-differences design comparing the performance of primary students from constrained vs. less-constrained families, before vs. after the FSE reform.

\[ Y_{itw} = \alpha + \beta \text{NonPayment}_i + \sum_{t=2014}^{2019} \delta_t \text{NonPayment}_i \times \lambda_t + \eta_w + \epsilon_{itw} \] (1)

In Equation 1, \( Y_{itw} \) is the outcome for PSLE-taker \( i \) in year \( t \) in ward \( w \). \( \text{NonPayment}_{2014} \) is an indicator for whether the student’s sibling failed to pay the FTNA fee in 2014. This indicator is then interacted with indicators for the years 2014-2019 (I exclude 2013 in case 2013 PSLE performance could endogenously affect siblings taking the FTNA in 2014, the only year for which fee non-payment data is available). \( \lambda_t \) are year dummies, and \( \eta_w \) are ward dummies. The \( \delta_t \) coefficients on the interaction terms are the coefficients of interest.

Section 8 explores alternative methods of defining the affected population, and yields findings broadly consistent with those outlined here.

6 Results

Figure 5 depicts the main result, plotting the interactions between the sibling non-payment dummy and the year dummies from Equation 1. Prior to the reform, the difference in pass rates for treated and control students did not increase; after the reform, the gap narrowed and eventually reversed.
Figure 5: Difference in pass rates for siblings from families with and without non-payment of fees

Notes: This figure shows interaction coefficients from Equation 1. The specification shows how the difference in outcomes between the younger siblings of students who did vs. didn’t pay the FTNA test fee in 2014 changed over time. The figure plots the coefficients and confidence intervals on the interaction between the NonPayment dummy and each year dummy (where the omitted year is 2015, the last year before the policy began). The horizontal line at 0.027 indicates $-1 \times \beta$, the coefficient on NonPayment, – in other words, the outcome gap between siblings of fee-paying and non-fee-paying students in 2015.

Table 3 shows another view of the diff-in-diff results. These results show that the reform caused an increase in pass rates and transition rates for treated students. However, it also slightly reduced students’ likelihood of getting a very high score on the PSLE, such that the overall effect on PSLE score was zero, whether measured in percentage points (column 3) or standard deviations (column 4).

7 Analysis at the school market level

7.1 Design: ward-level variation

I complement the student-level design with a design which exploits geographic variation in treatment intensity. This analysis uses a larger sample of students (aggregated to the ward level), rather than being
Table 3: Diff-in-diff: family financial constraints measured by exam fee non-payment

<table>
<thead>
<tr>
<th></th>
<th>Pass PSLE A</th>
<th>PSLE score (%)</th>
<th>PSLE score (σ)</th>
<th>Transition</th>
</tr>
</thead>
<tbody>
<tr>
<td>Treat</td>
<td>-0.015**</td>
<td>0.000</td>
<td>-0.387*</td>
<td>-0.023*</td>
</tr>
<tr>
<td></td>
<td>(0.006)</td>
<td>(0.002)</td>
<td>(0.223)</td>
<td>(0.014)</td>
</tr>
<tr>
<td>Treat × Post</td>
<td>0.022***</td>
<td>-0.009***</td>
<td>-0.025</td>
<td>-0.000</td>
</tr>
<tr>
<td></td>
<td>(0.008)</td>
<td>(0.002)</td>
<td>(0.274)</td>
<td>(0.017)</td>
</tr>
<tr>
<td>N</td>
<td>181,246</td>
<td>181,246</td>
<td>181,290</td>
<td>181,290</td>
</tr>
<tr>
<td>Mean (untreated)</td>
<td>0.739</td>
<td>0.032</td>
<td>48.898</td>
<td>0.142</td>
</tr>
</tbody>
</table>

Year and ward FE. Standard errors clustered by ward.
* p<0.10, ** p<0.05, *** p<0.01

limited to those which match to uniquely-named siblings. Also, some outcomes can only be measured at a larger unit of geographic aggregation, such as school entry.

According to the descriptive Figure 6, the biggest school-level increases in PSLE takers were at schools that hadn’t existed in 2013 (the first year of PSLE micro data available).

Figure 6: New test takers move into new schools

This leads to a series of questions about how school markets reacted to the reform. Did new primary schools sprout up to service increased demand in more affected areas? Did students select into areas with certain types of schools?
7.1.1 Ward-level specification

I define treatment at the ward level as whether the ward had any fee non-payment in 2014, the year for which I have exam fee payment data.

\[ Y_{wt} = \alpha + \delta \text{treat}_w \times \text{post}_t + \lambda_t + \eta_w + \epsilon_{wt} \]

\( Y_{wt} \) designates the outcome for ward \( w \) in year \( t \). \( \text{treat}_w \) indicates that ward \( w \) had non-zero FTNA non-payment in 2014. \( \text{post}_t \): Dummy for implementation period (post-2015). \( \lambda_t, \eta_w \): year FE, ward FE.

Figure 7 maps the wards of Tanzania by whether they had 0 vs more than 0 FTNA exam fee non-payment in 2014. “Treated” ward are those that had some positive amount of exam fee non-payment.

7.2 Results: ward-level variation

Table 4 confirms the results from the student-level variation: treatment increased pass rates and transition rates. But it does not show that treated wards had a disproportionate increase in the number of students taking the test, nor in the number of schools operating in the ward.

<table>
<thead>
<tr>
<th>Pass rate</th>
<th>PSLE avg score</th>
<th>Transition rate</th>
<th>Num students</th>
<th>Num schools</th>
</tr>
</thead>
<tbody>
<tr>
<td>Treat x Post</td>
<td>0.022***</td>
<td>0.558**</td>
<td>0.016***</td>
<td>10,400</td>
</tr>
<tr>
<td>(0.006)</td>
<td>(0.221)</td>
<td>(0.005)</td>
<td>(6.927)</td>
<td>(0.097)</td>
</tr>
<tr>
<td>N</td>
<td>15,840</td>
<td>15,840</td>
<td>15,840</td>
<td>15,840</td>
</tr>
<tr>
<td>Mean (untreated)</td>
<td>0.679</td>
<td>46.462</td>
<td>0.438</td>
<td>299,873</td>
</tr>
</tbody>
</table>

Year and ward FE. Standard errors clustered by ward.
* \( p < 0.10 \), ** \( p < 0.05 \), *** \( p < 0.01 \)
8 Robustness of student results

8.1 Alternate identification 1: predicting dropout using older sibling’s dropout

A complementary method which allows me to use more of the sibling sample relies instead on the simple correlation between siblings’ outcomes. This is plausible: the correlation between siblings’ outcomes is fairly strong. The correlation coefficients for PSLE marks between 2013 takers and their younger siblings in 2014-2018 range between 0.32 and 0.24; those for secondary continuation in 2014-2016 range from 0.16 to...
0.15. Controlling for other observable covariates, I consider students whose older 2013 PSLE-taker sibling dropped out to be more exposed to the FSE treatment than those whose old siblings did not, as their families seem to be bound by unobservable financial or other constraints. In order to compare students with extremely similar older siblings, I include fixed effects for year and for school × older sibling’s PSLE marks. Thus I compare outcomes for the younger siblings of older siblings who took the same test in the same school on the same day and got the same score – but one of them continued to secondary school, and the other did not. δ is the coefficient of interest: the measure of how much the difference between the groups within this tiny cell changed after the introduction of FSE. See the specification in Equation 2:

\[ Y_{isgl} = \alpha + \beta \text{SiblingDropout}_i + \delta \text{SiblingDropout}_i \times \text{post}_t + \lambda_t + \eta_{sg} + \epsilon_{isgl} \]  

(2)

The unit of observation is a younger sibling taking the PSLE in the years 2014-2018, whose older sibling took the test in 2013.

8.1.1 Alternate identification 2: Regression discontinuity at the passing threshold

Finally, one concern could be that even after including school × older-sibling-PSLE-marks fixed effects, there is some omitted variable determining which older siblings do vs. don’t drop out, introducing bias and confounding our estimate of the causal effect of FSE on younger siblings’ outcomes. In order to circumvent this threat to identification, I identify a source of exogenous variation in older siblings’ dropout rates: their failure rates at the PSLE. Failing the PSLE makes it nearly impossible to continue onto secondary school. But the passing threshold is a discrete cutoff above 200 points (of a total possible 500), corresponding to a “C” overall grade, creating the conditions for a regression discontinuity. I know exactly which students passed because I have their final average grade, but I don’t know their exact number of marks, since grades for each subject and for the overall grade are expressed only in letter terms. So, as outlined in Section 3, I assign each letter grade to the midpoint of the window of marks it expresses – an A is worth 90, a C worth 50, and an E worth 10, for example – and sum up the subjects to get a rough approximation of each student’s total marks. Inside the window of 170 to 230 estimated marks there are large fractions of students who do and who do not pass. So I use this as my window around the cutoff.

An older sibling’s failing score is a significant predictor of her dropout, so I let this be a reduced-form source of exogenous variation in dropout and interact it with the Post variable to recover the causal effect
of FSE on younger students’ outcomes, as in Equation 3:

\[ Y_{isgt} = \alpha + \beta \text{SiblingFail}_i + \delta \text{SiblingFail}_i \times \text{post}_t + \lambda_t + \eta_{isg} + \varepsilon_{isgt} \]  

(3)

Table 5 shows summary statistics by year for these sibling pairs. Column 1 summarizes unique 2013 PSLE takers, or “older siblings,” who have at least one younger sibling who took the PSLE in the same school in 2014-2018; each of the other columns summarizes unique younger siblings who took the PSLE in each respective year and has matches last name to exactly one 2013 PSLE taker in the same school. Because the analyses described in this section depend on comparing how within-cohort differences change over time, it is important to note that the set of 2013 older siblings who match to younger siblings in 2014-2018 (and who provide those siblings their measures of treatment exposure, i.e., propensity to drop out) do not look meaningfully different by year. In contrast, note that PSLE scores, pass rates, and transition rates for younger siblings do rise dramatically with the years, highlighting the necessity of within-cohort variation for isolating the effects of FSE.
Table 5: Summary statistics: sibling pairs

<table>
<thead>
<tr>
<th></th>
<th>2013</th>
<th>2014</th>
<th>2015</th>
<th>2016</th>
<th>2017</th>
<th>2018</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Family variables:</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>$\hat{dropout}_{isg2014}$</td>
<td>0.670</td>
<td>0.675</td>
<td>0.670</td>
<td>0.667</td>
<td>0.667</td>
<td>0.671</td>
</tr>
<tr>
<td></td>
<td>(0.273)</td>
<td>(0.270)</td>
<td>(0.272)</td>
<td>(0.273)</td>
<td>(0.277)</td>
<td>(0.274)</td>
</tr>
<tr>
<td>Private primary school</td>
<td>0.0200</td>
<td>0.0156</td>
<td>0.0172</td>
<td>0.0198</td>
<td>0.0161</td>
<td>0.0163</td>
</tr>
<tr>
<td></td>
<td>(0.140)</td>
<td>(0.124)</td>
<td>(0.130)</td>
<td>(0.139)</td>
<td>(0.126)</td>
<td>(0.127)</td>
</tr>
<tr>
<td><strong>Older siblings (2013 PSLE):</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Female</td>
<td>0.525</td>
<td>0.514</td>
<td>0.517</td>
<td>0.531</td>
<td>0.535</td>
<td>0.542</td>
</tr>
<tr>
<td></td>
<td>(0.499)</td>
<td>(0.500)</td>
<td>(0.500)</td>
<td>(0.499)</td>
<td>(0.499)</td>
<td>(0.498)</td>
</tr>
<tr>
<td>PSLE score</td>
<td>42.60</td>
<td>41.84</td>
<td>42.29</td>
<td>42.70</td>
<td>42.12</td>
<td>41.86</td>
</tr>
<tr>
<td></td>
<td>(15.15)</td>
<td>(14.87)</td>
<td>(14.95)</td>
<td>(15.06)</td>
<td>(14.98)</td>
<td>(14.84)</td>
</tr>
<tr>
<td>Passed PSLE</td>
<td>0.517</td>
<td>0.493</td>
<td>0.510</td>
<td>0.523</td>
<td>0.506</td>
<td>0.498</td>
</tr>
<tr>
<td></td>
<td>(0.500)</td>
<td>(0.500)</td>
<td>(0.500)</td>
<td>(0.499)</td>
<td>(0.500)</td>
<td>(0.500)</td>
</tr>
<tr>
<td>PSLE total marks</td>
<td>202.8</td>
<td>198.5</td>
<td>201.7</td>
<td>203.2</td>
<td>200.6</td>
<td>199.0</td>
</tr>
<tr>
<td></td>
<td>(77.79)</td>
<td>(77.06)</td>
<td>(76.46)</td>
<td>(77.03)</td>
<td>(76.65)</td>
<td>(76.52)</td>
</tr>
<tr>
<td>PSLE marks $\in [170,230]$</td>
<td>0.410</td>
<td>0.415</td>
<td>0.417</td>
<td>0.412</td>
<td>0.411</td>
<td>0.408</td>
</tr>
<tr>
<td></td>
<td>(0.492)</td>
<td>(0.493)</td>
<td>(0.493)</td>
<td>(0.492)</td>
<td>(0.492)</td>
<td>(0.491)</td>
</tr>
<tr>
<td>Transitioned</td>
<td>0.313</td>
<td>0.296</td>
<td>0.314</td>
<td>0.323</td>
<td>0.311</td>
<td>0.306</td>
</tr>
<tr>
<td></td>
<td>(0.464)</td>
<td>(0.456)</td>
<td>(0.464)</td>
<td>(0.468)</td>
<td>(0.463)</td>
<td>(0.461)</td>
</tr>
<tr>
<td><strong>Younger siblings:</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Female</td>
<td>0.546</td>
<td>0.546</td>
<td>0.534</td>
<td>0.528</td>
<td>0.528</td>
<td>0.525</td>
</tr>
<tr>
<td></td>
<td>(0.498)</td>
<td>(0.498)</td>
<td>(0.499)</td>
<td>(0.499)</td>
<td>(0.499)</td>
<td>(0.499)</td>
</tr>
<tr>
<td>PSLE score</td>
<td>43.42</td>
<td>48.03</td>
<td>47.42</td>
<td>48.75</td>
<td>52.55</td>
<td>(15.18)</td>
</tr>
<tr>
<td></td>
<td>(17.11)</td>
<td>(15.84)</td>
<td>(16.42)</td>
<td>(17.81)</td>
<td>(17.81)</td>
<td>(17.81)</td>
</tr>
<tr>
<td>Passed PSLE</td>
<td>0.545</td>
<td>0.661</td>
<td>0.687</td>
<td>0.714</td>
<td>0.767</td>
<td>(0.498)</td>
</tr>
<tr>
<td></td>
<td>(0.473)</td>
<td>(0.464)</td>
<td>(0.452)</td>
<td>(0.422)</td>
<td>(0.422)</td>
<td>(0.422)</td>
</tr>
<tr>
<td>Transitioned</td>
<td>0.321</td>
<td>0.436</td>
<td>0.511</td>
<td>.</td>
<td>.</td>
<td>.</td>
</tr>
<tr>
<td></td>
<td>(0.467)</td>
<td>(0.496)</td>
<td>(0.500)</td>
<td>(.)</td>
<td>(.)</td>
<td>(.)</td>
</tr>
<tr>
<td>Observations</td>
<td>177612</td>
<td>70724</td>
<td>71416</td>
<td>63930</td>
<td>64157</td>
<td>62836</td>
</tr>
</tbody>
</table>

N. unique schools = 10969. This table includes pairs of 2013 PSLE takers whose last name is unique within their school grade, matched to 2014-2018 PSLE takers at the same school with the same last name, who I call siblings. Groups of last-name-matched students greater than 6 are excluded as likely non-siblings. Stats in the 2013 column are for unique 2013 PSLE takers with at least one younger sibling under these criteria. Older siblings stats in columns 2014-2018 are for 2013 takers who match to at least one PSLE taker in that year. Younger siblings stats in columns 2014-2018 are for 2013 takers who match to at least one PSLE taker in that year. Younger sibling stats in the columns 2014-2018 are for PSLE takers in those years which match to a 2013 taker, and are unique at the younger sibling x year level: multiple younger students are allowed to match to the same 2013 PSLE taker, even within the same year. Family-level predicted dropout is calculated only among sibling sets which include at least one 2015-2018 PSLE taker who matches to one of the 32384 takers in 2013 AND matches to at least one of the 40921 takers in 2014.
8.1.2 Using older sibling dropout as measure of student-level exposure intensity

I next check the robustness of these results using the alternate identification strategies outlined in the previous section. Table 6 shows results for the analyses which use older sibling dropout (odd columns) or older sibling PSLE failure conditional on being close to the cutoff (even columns) as a measure of student-level treatment exposure. The results are largely similar to Table E.2, though smaller, and in the case of the transition outcome, statistically insignificant.

Table 6: Student level: Diff in diff by older siblings’ continuation

<table>
<thead>
<tr>
<th></th>
<th>PSLE (%)</th>
<th>PSLE (σ)</th>
<th>Nationwide PSLE %ile</th>
<th>Transitioned</th>
</tr>
</thead>
<tbody>
<tr>
<td>Older sib dropout</td>
<td>-0.807***</td>
<td>-0.046***</td>
<td>-1.771***</td>
<td>-0.051***</td>
</tr>
<tr>
<td></td>
<td>(0.143)</td>
<td>(0.008)</td>
<td>(0.235)</td>
<td>(0.005)</td>
</tr>
<tr>
<td>Older sib dropout × Post</td>
<td>0.595***</td>
<td>0.044***</td>
<td>1.965***</td>
<td>0.009</td>
</tr>
<tr>
<td></td>
<td>(0.153)</td>
<td>(0.009)</td>
<td>(0.256)</td>
<td>(0.006)</td>
</tr>
<tr>
<td>Older sib failed PSLE</td>
<td>-0.990***</td>
<td>-0.048***</td>
<td>-1.512***</td>
<td>-0.016**</td>
</tr>
<tr>
<td></td>
<td>(0.218)</td>
<td>(0.012)</td>
<td>(0.358)</td>
<td>(0.008)</td>
</tr>
<tr>
<td>Older sib failed PSLE × Post</td>
<td>0.571***</td>
<td>0.020*</td>
<td>0.875***</td>
<td>0.014</td>
</tr>
<tr>
<td></td>
<td>(0.203)</td>
<td>(0.012)</td>
<td>(0.335)</td>
<td>(0.009)</td>
</tr>
</tbody>
</table>

N 308308 130191 308308 130191 308308 130191 176517 76155
Adj. R² 0.288 0.211 0.315 0.218 0.318 0.239 0.188 0.166
Mean (pre-reform) 44.867 44.467 0.005 -0.036 44.521 43.942 0.379 0.399

Standard errors clustered by school. All regressions include fixed effects for year and school. Sample limited to 2014-2018 PSLE takers who have the same last name as a 2013 PSLE taker whose last name is unique within her school (groups greater than 6 dropped to avoid false sibling matches). PSLE score outcome variables coded as 0 where data is missing, with a dummy included in the regression for missing observations. PSLE z-score is calculated as the average of students’ English, Swahili, and Math scores, normalized within year to a mean of 0 and a standard deviation of 1. Even columns limited to younger siblings whose older sibling scored near the passing threshold on the 2013 PSLE – between 170 and 230 inclusive (out of a possible 500 in total).

* p<0.10, ** p<0.05, *** p<0.01

One takeaway from these analyses is just how much student outcomes vary at the school level. To look at the example of columns 1 and 2: after including school and year fixed effects, the average PSLE score for the younger sibling of a dropout is only 0.8 percentage points lower than that of the younger sibling of a dropout, from a base of 45 percentage points. Depending on the measure of PSLE performance, the introduction of FSE fully or nearly erased the grade gap between the younger siblings of dropout vs. non-dropouts. However, for the outcome of transitioning to secondary school, the gap between the younger siblings of dropouts and non-dropouts is larger, and it does not appear to have been affected by FSE.
9 Conclusion

Free public education has become common across the developing world at the primary level, and is increasingly common at the secondary level as well. Because human capital acquisition is dynamic, understanding the full effects of Free Secondary Education (FSE) requires that we understand how it affects primary students’ responses. Understanding how free education affects students’ intertemporal investment choices is important for theoretical and policy reasons.

This paper examines primary student responses to a Free Secondary Education (FSE) program in Tanzania in 2016, using the universe of two high-stakes national standardized tests taken in mainland Tanzania between 2013 and 2018. Using variation in the intensity of exposure to the program at both the student and ward level, I identify the causal effect of this nationwide policy and to illuminate potential mechanisms. I show that students from financially constrained families increased their pass rates and transition rates after the reform. I also show that primary school markets do not respond to this shock to demand, at least in the short term.

My results show that access to secondary school affects the investments of students in primary school, and has various implications. First, even young children (or the people making decisions about their schooling) think intertemporally about educational investments. Second, many students who may appear to be “low-ability” in the absence of access to secondary school may in fact have the innate capacity to receive “high-ability”-level scores when they do have access to higher schooling. Third, FSE policies may have benefits beyond their target population, such that they may be less costly on net than previously understood. Fourth, policies which can alleviate constraints on school entry have the potential to increase the benefits of FSE policies yet further.

References


Kadzamira, E., & Rose, P. (2003, Sep). Can free primary education meet the needs of the poor?: evidence


A  Extra tables and figures

B  Data validation

This section seeks to validate the proxy measures I create for transition and PSLE score.
Figure A.1: Example questions from the PSLE

B.1 Validating the transition measure

Figure B.2 shows what fraction of PSLE takers match to the name of an FTNA taker from subsequent years.
Figure B.2: Match rate of 2013 PSLE takers to FTNA takers in subsequent years

Notes: This figure shows the fraction of unique-named 2013 PSLE takers whose name matches a unique-named FTNA taker from one to four years later (2014-2017).

A negligible fraction (less than 0.5%) of PSLE takers match to the name of an FTNA taker from one year later. These matches could be “false positives” – spurious name-sharers who don’t in fact represent the same student – or precocious students who skipped ahead one grade. Either way, there are not many of them.

Most PSLE takers who match to the name of an FTNA taker from the next four years match to the cohort from two years later – that which represents normal grade progression. This increases confidence that the PSLE-to-FTNA match is a credible measure of transition.

Smaller numbers of PSLE takers match to the FTNA cohorts from three and four years after they take the PSLE. These students may be students who failed the PSLE the first time around, or who were delayed for a year or two in their studies for other reasons. These students likely represent “false negatives” in my proxy measure of transition.
Figure B.3 demonstrates the high variability in transition rates across regions.

B.2 Validating the PSLE overall score measure

NECTA provides coarse measures of students’ overall PSLE score, while providing a sharp measure of whether they passed. My proxy measure of overall PSLE score is the sum of subject scores – where each subject score is interpolated to be the midpoint of the range of scores that correspond to the letter grade reported – normalized to a 100-point scale.

It is possible to partially validate this measure. For the 2016 PSLE cohort from Mwanza (the second-most populous region in Tanzania), the list of actual, precise overall PSLE scores is available, but only for students who cleared the passing threshold (i.e. scored over 40). Comparing this distribution with the proxy overall PSLE scores devised from the NECTA data for matchable students yields a correlation of .98. This suggests that the proxy measure of PSLE overall scores is a reasonably good one.
Notes: This histogram shows the distribution of proxy PSLE scores for 2016 Mwanza PSLE takers, on top of the distribution of true PSLE scores for the same cohort (of those who passed the exam).

C District-level variation

C.1 Results: district-level variation

In this section, I replicate the analysis using the coarser district-level variation which many previous studies of expansions of public schooling access rely on due to data limitations (Mbiti & Lucas, 2012; Brudevold-Newman, 2019).

D School-level variation

D.1 Design: school-level variation

If Section 2 demonstrated that FSE increased secondary access, I now turn to the question of whether this increased access to secondary affected primary students’ outcomes. It is challenging to identify the
causal effects of a policy implemented across the country at a single time. I begin by using a common strategy from the literature on school fee reduction programs: (primary) school dropout rate as a proxy for treatment exposure. This is an attractive and feasible method for the many school fee reduction programs have been implemented simultaneously nationwide, creating temporal variation but no geographical variation in their applicability. My data allows me to define treatment exposure at the school level; other users of this method have used less granular variation at the district or county level (Mbiti & Lucas, 2012; Brudevold-Newman, 2019).

This identification strategy rests on two key assumptions: 1) pre-reform variation in transition rates is due to unchanging characteristics of the school; and 2) outcome trends in the two sets of schools would have persisted in the absence of FSE. Under these assumptions, the difference-in-differences framework furnishes a consistent estimate of program impact by netting out school- and cohort-level differences.

Equation 4 expresses the specification:

$$Y_{dst} = \beta \text{DropoutRate}_{2013s} \times \text{post}_t + \lambda_t + \theta_s + \theta_d \times \text{trend}_t + \epsilon_{dst}$$  (4)

My school-level treatment variable, $\text{DropoutRate}_{2013s}$, is a school’s pre-reform primary-to-secondary dropout rate.\textsuperscript{15} The rationale for this is that a primary school which already sends most of its students on to secondary school in the absence of FSE likely experiences school fees as less of a constraint than one which does not. By this measure, in 2013, only 12% of the average primary school’s students continued on to secondary school. The coefficient of interest is $\beta$, the interaction of this measure of treatment intensity with a dummy for the post period (2016 and later). I include $\lambda_t$ year fixed effects and $\theta_s$ school fixed effects. I also include district-specific linear time trends, to control for potentially confounding pre-existing trends that may differ between geographies.\textsuperscript{16}

\textsuperscript{15}As defined using string matching on names within districts between PSLE and FTNA takers, see Section 3. This is likely an upper bound on the true dropout rate.

\textsuperscript{16}I include these district trends following Mbiti and Lucas (2012). But that paper takes the district as the unit of observation; controlling for district $\times$ time there would be equivalent to me controlling for school $\times$ time trends in my paper, since my variation is at the school level. This would be good. But due to the number of schools, it remains computationally infeasible for now.
D.2 Results: school-level variation

Table D.1: School-level variation: Diff in diff by 2013 dropout rate

<table>
<thead>
<tr>
<th>Dropout rate 2013 × Post</th>
<th>Student-level</th>
<th>School-level</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>PSLE (%)</td>
<td>PSLE (σ)</td>
</tr>
<tr>
<td></td>
<td>6.860***</td>
<td>0.428***</td>
</tr>
<tr>
<td></td>
<td>(0.460)</td>
<td>(0.028)</td>
</tr>
<tr>
<td>N</td>
<td>4424285</td>
<td>4424285</td>
</tr>
<tr>
<td>Mean (pre-reform)</td>
<td>43.813</td>
<td>0.014</td>
</tr>
<tr>
<td>Trends</td>
<td>District</td>
<td>District</td>
</tr>
</tbody>
</table>

Standard errors clustered by school. School and year fixed effects included in all regressions. District trends included in all regressions. 2013 dropout rate defined as the fraction of 2013 PSLE takers from that school whose names showed up exactly as takers of the FTNA at a secondary school in the same district in 2015.

* p < 0.10, ** p < 0.05, *** p < 0.01

Columns 1 and 2 in Table D.1 shows that PSLE scores for students in the highest pre-reform-dropout-rate schools rose differentially after the reform, by 7 percentage points on a 100-point scale, or by .43 σ. Recall that 30% of schools in the sample have a 100% dropout rate in 2013. Column 3 shows that this test score gain corresponded to 15 percentile positions in the nationwide score distribution, so it’s not merely an artifact of overall grade inflation (although overall grades are indeed rising sharply in this period). Column 4 shows that FSE caused an increase in 17 percentage points in the fraction of PSLE takers who show up in FTNA records two years later – a huge effect on a base of 16%.

Columns 5 and 6 measure outcomes at the school level rather than the student level. Column 5 shows that the number of people taking the PSLE in these schools actually declined after the reform, by 6.7 test takers from a base of 51.4. Combined with the overall post-reform increase in enrollments and test-taking, this reduction in test takers at low-transition-rate schools implies a large increase at high-transition-rate schools. Figure D.5 illustrates a similar effect at play, dividing up the schools by above-vs below-median-pass-rate. This suggests that students responded to FSE by selecting into better schools. Recent survey evidence supports the idea that parents in Tanzania may be open to this kind of strategic switching (Solomon & Zeitlin, 2019).

Column 6 looks at the number of students retaking the PSLE (they are designated in the data), as a possible measure of increased effort for older students to try again to qualify for secondary school, but there are very few retakers overall in the data and FSE seems not to have affected that.

17 Bold et al. (2015) documents a somewhat similar pattern, where affluent students shift to private schools in response to Free Primary Education in Kenya, where private schools likely play a bigger role than they do in Tanzania (Bold et al., 2015).
It is also worth noting that the increases in PSLE performance observed here are net of the loss of test takers in these schools. Because we don’t know exactly who these “missing” test takers are, that could mean a number of things. If the missing test takers are ambitious, high-achieving students who leave for better schools, then it is notable that the students left behind still register such a large increase in PSLE performance. If instead, the “missing” students are the worst students, being strategically kept by administrators from sitting the exam à la Cilliers et al. (2020), then the positive effects on PSLE performance would appear to be an artifact of changing student composition rather than changes in student performance. Distinguishing between these two stories is made easier by the results based on student-level variation in Section 5. Because those estimates leverage variation within schools, any effects on PSLE performance cannot be due to changing school-level student composition, since this is constant across students within a school.
E Alternate family-level variation

E.1 Alternate identification 3: predicted dropout using families with multiple siblings

I present a new methodology for measuring variation in treatment exposure at the student level. Differences in outcomes at the school level may be due to school-level changes in resources or student composition, but differences in outcomes at the student level are likelier to reflect differences in family or student effort. Because I can match students by name, I can create a reasonable proxy for family relationships by finding students with the same last name who both took the PSLE at the same school in neighboring years.\(^{18}\) I then use the first two years of pre-reform PSLE and transition data from these sibling groups to estimate a within-family measure of predicted drop-out in the absence of FSE, which I call \(\hat{\text{dropout}}_{\text{isg2014}}\). I consider students from these families to have a high treatment exposure. Then I set up a difference-in-differences framework similar to that which relied on school-level variation, comparing outcomes for the younger siblings from high- vs. low-\(\hat{\text{dropout}}_{\text{isg2014}}\) families before vs. after FSE. The data-intensiveness of this method reduces the sample only to families with many children in a short span of years. So I complement it with a version which takes as its measure of treatment exposure the dropout of older siblings (2013 PSLE takers), as a proxy for younger siblings’ own propensity to drop out. In both methods using student-level treatment exposure, I include fixed effects for year and school \(\times\) older siblings’ PSLE score, with the intuition that whatever difference between the families of two students who got the same score in the same school in the same year caused one of them to go to secondary school and the other to drop out, can be considered a constraint that might be alleviated by FSE and therefore expose that family more intensely to the treatment.

This measure of student-level variation confirms the previous set of results. Even within schools, students who were more exposed to the treatment (by being in families with high predicted dropout, or having an older sibling who dropped out) saw disproportionate gains in PSLE scores, for some measures completely erasing the gap between them and their non-sibling-dropout peers. These students may have also seen gains in transition rate, but these effects are smaller and less precisely estimated than the student-level effects on PSLE scores or the school-level effects on transition.

\(^{18}\)While Tanzania is a place where siblings generally share the same last name, I have no way of knowing whether students with the same last name who go to the same school are really from the same family. In this analysis I conservatively limit the sample to groups of at most 6 PSLE takers (one per year on average) from the same school whose shared last name was unique within their school’s 2013 PSLE cohort.
E.1.1 Predicting dropout using families with multiple siblings

In this subsection, I outline a strategy which seeks to identify which students within a school come from families for whom financial or other non-academic constraints on secondary schooling bind. I accomplish this by using students’ names to link them into sibling sets. I then develop two different student-level measures of treatment exposure by linking likely groups of siblings and creating measures of the family’s propensity to drop out.

Tanzania is a place where siblings usually share a last name, but also where there are enough unique last names to plausibly classify people as likely siblings.\(^{19}\) I start with the set of 2013 (pre-FSE) PSLE takers whose last name is unique within their school (“older siblings”).\(^{20}\) I further limit the sample to 2013 students whose last names match 5 or fewer other students at their school in the 2014-2018 period – one student per year, on average – to reduce the risk of matching unrelated students with common last names.\(^{21}\)

I create a dummy for whether these 2013 students took the FTNA two years later in 2015 (still pre-FSE). In each wave of PSLE takers from 2014-2018 (“younger siblings”), I match students to their own school’s 2013 PSLE takers who share their last name – this yields about 450,000 pupils, or 10% of the original sample.

E.1.2 Predicting dropout using families with multiple siblings

I begin by creating a family-level measure of propensity for younger siblings to drop out. I limit the sibling sample further to families with multiple siblings at the same school. By using an older sibling’s 2013 PSLE performance AND secondary transition to predict the secondary transition of her younger sibling who took the PSLE in (still pre-reform) 2014, I can recover a predictor \(\hat{\text{dropout}}_{\text{isg}2014}\) for the likelihood of a younger sibling’s secondary transition in the absence of the FSE reform using Equation 5:

\[
\hat{\text{dropout}}_{\text{isg}2014} = \alpha + \beta X_i + \eta_{sg}
\]  

\(^{19}\)See Cruz, Labonne, and Querubín (2017) for an example of similar family matching using name strings.

\(^{20}\)I chose to match students from the 2014-2018 cohorts back to their siblings in 2013, because 2013 was the last PSLE cohort whose decisions about FTNA would have been completely made before the introduction of FSE – in a sense they are the last cohort completely “uncontaminated” by FSE. An alternative version of this analysis instead defines each group of “younger” siblings’ families’ propensity to go to secondary school by matching them to the cohort immediately preceding them – i.e., matching 2014 to 2013, then 2015 to 2014, and checking for a significant break in the trend after FSE, for the 2016 cohort matched to 2015. It yields results very similar results to those I present here, and they are available upon request.

\(^{21}\)Tanzania’s Total Fertility Rate in 2000-2005, the years in which 13-year-old takers of the 2013-2018 PSLE would have been born, was between 5.7 and 5.6, so finding numerous sets of multiple siblings within this window should not be considered especially surprising. (The World Bank, 2021c).
where $X_i$ is parsimonious enough to include only the older sibling’s continuation, PSLE score in each subject, and sex.

I then assign the predicted values of $\hat{\text{dropout}}_{isg2014}$ to younger members of the family who took the PSLE in 2015-2018, letting it function as a measure of family-level treatment exposure. The intuition is that the families most likely to be unaffected by FSE are those whose pre-reform performance makes us predict with near-certainty that their children will transition to secondary school even without the reform. Families with less of a chance of secondary continuation in the absence of the reform are those most likely to be affected by FSE – most exposed to the treatment.

Then my specification, in Equation 6 lets $\hat{\text{dropout}}_{isg2014}$ be the treatment variable, where the $\delta$ coefficient on the interaction between $\hat{\text{dropout}}_{isg2014}$ and $\text{post}_t$ is the coefficient of interest:

$$Y_{isgt} = \alpha + \beta \hat{\text{dropout}}_{isg2014} + \delta \hat{\text{dropout}}_{isg2014} \times \text{post}_t + \lambda_t + \eta_{sg} + \epsilon_{isgt}$$

(E.2)

E.2 Results using family-level predicted dropout as measure of student-level exposure intensity

Table E.2 shows results which use pre-reform family data to predict which families’ students are likely to transition in the absence of the FSE reform ($\hat{\text{dropout}}_{isg2014}$). Students from high-$\hat{\text{dropout}}_{isg2014}$ families saw much higher increases in PSLE scores and percentiles. Effects on secondary transition rates were positive but smaller and less precisely estimated.
Table E.2: Differential post-reform outcomes by pre-reform predicted family dropout

<table>
<thead>
<tr>
<th></th>
<th>PSLE (%)</th>
<th>PSLE (σ)</th>
<th>Nationwide PSLE %ile</th>
<th>Transitioned</th>
</tr>
</thead>
<tbody>
<tr>
<td>$\hat{\text{dropout}_{isg2014}}$</td>
<td>-10.267</td>
<td>-0.111</td>
<td>-2.336</td>
<td>0.593</td>
</tr>
<tr>
<td></td>
<td>(11.508)</td>
<td>(0.645)</td>
<td>(18.402)</td>
<td>(0.563)</td>
</tr>
<tr>
<td>$\hat{\text{dropout}_{isg2014}} \times \text{Post}$</td>
<td>3.667***</td>
<td>0.140***</td>
<td>4.583***</td>
<td>0.060*</td>
</tr>
<tr>
<td></td>
<td>(0.848)</td>
<td>(0.047)</td>
<td>(1.344)</td>
<td>(0.035)</td>
</tr>
<tr>
<td>N</td>
<td>51094</td>
<td>51094</td>
<td>51094</td>
<td>20482</td>
</tr>
<tr>
<td>Adj. $R^2$</td>
<td>0.278</td>
<td>0.311</td>
<td>0.320</td>
<td>0.189</td>
</tr>
<tr>
<td>Mean (pre-reform)</td>
<td>46.217</td>
<td>-0.047</td>
<td>43.619</td>
<td>0.431</td>
</tr>
<tr>
<td>FE: year and ...</td>
<td>School</td>
<td>School</td>
<td>School</td>
<td>School</td>
</tr>
<tr>
<td>× marks</td>
<td>× marks</td>
<td>× marks</td>
<td>× marks</td>
<td></td>
</tr>
</tbody>
</table>

Standard errors clustered by school. All regressions include year and school × older-sibling-PSLE-marks FE. Sample limited to 2015-2018 PSLE takers who have the same last name as a 2013 PSLE taker whose last name is unique within her school AND to a 2014 PSLE taker (groups greater than 6 dropped to avoid false sibling matches). Family-level predicted dropout calculated by predicting 2014 siblings’ dropout using 2013 siblings’ data. PSLE score outcome variables coded as 0 where data is missing, with a dummy included in the regression for missing observations. PSLE z-score is calculated as the average of students’ English, Swahili, and Math scores, normalized over the distribution of all test takers within each year to a mean of 0 and a standard deviation of 1.

* $p<0.10$, ** $p<0.05$, *** $p<0.01$