Secondary school access raises primary school achievement

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Abstract

I study the effect of secondary school access on primary school achievement, using Tanzania's implementation of a Free Secondary Education (FSE) reform. Drawing on the universe of standardized test scores as well as household survey data, I confirm that the reform increased secondary school access: secondary enrollments rose, household secondary education spending fell, and the transition premium for students from local elite families disappeared. I estimate the causal effect of secondary access on primary student achievement in a difference-in-differences framework, considering the treatment group to be students from families who failed to pay secondary school fees ex-ante. The reform increased primary pass rates and on-time secondary transition rates by about 3 percentage points each (from bases of 62% and 21%, respectively). Effects along the distribution of test scores for each exam subject suggest that students responded by optimally reallocating effort to clear the passing threshold. Results are not driven by supply-side changes to inputs at the school level, nor by changes to the size of student cohorts.

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1 Introduction

Do individuals' education investments today depend on access to future education? Intertemporal spillovers matter for assessing the impact of free secondary education policies (FSE), especially in the developing world. Free secondary education is nearly universal in the developed world, but has only recently begun to be adopted among developing countries: as of 2023, 22 of the 47 countries in Sub-Saharan Africa offer fee-free education through lower-secondary school (Gruijters, Abango, & Casely-Hayford, 2023). However, some 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 spending on pre-primary education (Attanasio, Meghir, & Nix, 2020; Cunha & Heckman, 2007; Heckman, 2006). Still others emphasize that many students still aren't learning enough in primary school (Barro & Lee, 2013; Glewwe & Kremer, 2006; Pritchett & Beatty, 2015). But the existence of intertemporal spillovers would imply that the benefits of FSE are larger than previously understood. On the supply side, it has been shown that investments in higher education crowd in supply at lower levels (Jagnani & Khanna, 2020). On the demand side, universal primary education programs increased demand for secondary education (Opalo, 2022). Might universal secondary education in turn raise demand for primary education?

Existing empirical literature suggests that these intertemporal spillovers could be important. Secondary school students respond to increased college access, but this phenomenon has not been documented among primary school students at scale (Laajaj, Moya, & Sánchez, 2022). Evidence from randomized trials shows that secondary school scholarships provided by NGOs can raise individual achievement in primary schools, but whether such an effect in a fee abolition at national scale is uncertain (Kremer, Miguel, & Thornton, 2009; Bold, Kimenyi, Mwabu, Ng'ang'a, & Sandefur, 2018).[1]

This paper examines the effect of secondary school access on primary student learning in the context of Tanzania's 2016 implementation of free secondary education (FSE). 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 rise to the order 300,000 TZS (139 USD). GDP per capita at the time was about 929 USD.[2] This

[1] Decisions 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." 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).

[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.

helped contribute to the wide gap between net primary enrollment (83.5%) and net secondary enrollment (23.9%). One month before the January 2016 start of the school year, the president announced that all basic education through the fourth year of secondary school would be free, effective immediately. 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 (incompletely) replaced by capitation grants of USD 5 per student (Taylor, 2016).

I measure the causal effect of FSE on primary school attainment in a difference-in-differences framework, comparing individuals from more- vs. less-exposed families before and after the policy. My measure of treatment exposure comes from student-level microdata on secondary school fee payments prior to FSE's implementation. I identify groups of (likely) related students by linking each cohort of primary school students by last name within small geographic areas (wards); I refer to these groups as "families."[3] I consider students from non-fee-paying families as "treated" by the FSE policy: the stopping of educational progress due to inability to pay fees is precisely the problem which the policy was designed to solve. The comparison group consists of primary school students from families which succeeded in paying school fees prior to the reform. Before the reform, primary students from treated families were 2 percentage points less likely to transition from primary to secondary school than students from comparison families, suggesting it is a good measure of financial constraints. I compare how the difference in performance between treated and non-treated grade 7 students changed after the reform, including year and family 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 remained constant in the years prior to the reform.

I rely for outcomes on 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 ("grade 7"). The Form Two National Assessment (FTNA) is the first exam of secondary school, taken at the end of the second year ("grade 9"). Both exams are high-stakes for students: they must be passed to advance to the next grade. Grade 7 exams provide a measure of primary student achievement. To measure whether 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).[4]

I first show that the FSE policy, which was intended to increase access to secondary school, did so.

[3] In Tanzania, siblings tend to share a last name.
[4] This measure is likely an undercount, as I explain further in Section 2.

This is not trivial: Tanzania's government has made various commitments to universalizing education at both the primary and secondary level which ultimately failed (Opalo, 2022). 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 3). 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). Household surveys showed a sharp decrease in spending on school fees after the reform (Figure 2). 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 result is that greater secondary school access not only raised the transition rate; it also increased the pass rate for the primary school leaving examination in grade 7. 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 an alternate definition 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. These results broadly confirm the results from my main specification.

I examine FSE's effect on the distribution of grade 7 test scores to show that performance improvements appear to have been driven by reallocation of student effort. FSE increased primary students' likelihood of getting an overall score just over the passing threshold. Furthermore, it increased the likelihood of students receiving passing scores in more difficult subjects, and reduced their likelihood of getting high scores in easier subjects.

I do not find evidence that the policy had a large impact on the size of grade 7 test-taking cohorts, nor on the construction of new schools. 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. For this analysis, I define "treated"

wards as those with a non-zero number of 2014 exam fee non-payers (58% of the total). The ward-level diff-in-diff confirms that the policy raised grade 7 exam pass rates and transition rates, but shows no significant increase in either the number of students or the number of schools, further highlighting that the effects are driven by changes to investments from the demand side and not the supply side.

This paper's primary contribution is to show empirically that removing barriers to secondary education can raise individual students' investment in primary education at scale. 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, 2021a; Garlick, 2019; Blimpo, Gajigo, & Pugatch, 2019). Related papers show that increasing college access for underrepresented groups raised high school achievement in the United States and Colombia (Akhtari, Bau, & Laliberté, 2021; Laajaj et al., 2022). Another related literature shows construction of higher education institutions affects primary enrollment and attendance in India, at least partly by crowding in supply of primary schools (Mukhopadhyay & Sahoo, 2016; Jagnani & Khanna, 2020). And randomized trials show positive effects of scholarships, which can be considered expansions of access for certain individuals (Duflo, Dupas, & Kremer, 2017; Kremer et al., 2009). But these studies leave open the question of scalability. My paper uses high-stakes test score administrative data to show that primary demand responds to secondary access at scale.

This paper also contributes to the body of research evaluating free secondary education programs more generally. Expansions in access are often accompanied by (not wholly unjustified) worries about reductions in quality (Bold et al., 2015; Garlick, 2019). Tanzania's own institution of Free Primary Education in 2002 saw increased pupil-teacher ratios, reduced teacher quality, and possibly reduced test scores, at least in urban areas (Valente, 2015). Many of the Free Primary Education (FPE) programs of recent decades have failed to create measurable learning gains.[5] The literature on Free Secondary Education, by contrast, has found broad benefits, including learning gains for secondary students in some cases, and labor market gains in others – but these benefits come with the important caveat that these programs are very costly (Brudevold-Newman, 2021b; Blimpo et al., 2019). Benefits can be large, however, and can affect future generations (Agüero & Ramachandran, 2020). Efforts to evaluate the costs and benefits of these policies should also consider their intertemporal spillovers.

The rest of this article is structured as follows: Section 2 provides background on education in Tanzania

[5]See e.g. Kremer, Brannen, and Glennerster (2013); Bold et al. (2017); Pritchett (2013); Kadzamira and Rose (2003); Samarrai and Zaman (2007); Deininger (2003); Mbiti and Lucas (2012). This 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).

and presents the data. Section 3 describes the Free Secondary Education policy and presents evidence that it did in fact increase secondary school access. Section 4 lays out the empirical strategy; Section 5 presents the results. Section 6 examines student sorting and school entry at the level of the school market (ward). The conclusion is Section 7.

2 Background and Data

Primary schooling in Tanzania begins officially at age seven, and consists of seven years ("standards"). In the 2014-15 wave of the nationally representative National Panel Survey, 76% of students said they had started school at or before age 7 (see Figure 1a). At the end of Standard 7 — around age 13 for students who remain on-track — 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, but many do so, especially the most selective). In the 2014-15 NPS, 97% of respondents who finished standard 7 also sat the PSLE. Primary net enrollment in Tanzania was 81% as of 2018 (The World Bank, 2021a). Public primary schools have been fee-free since 2001 (Valente, 2015).

Secondary school consists of 4 years ("forms") of "ordinary level" followed by two years of "advanced level". (Tanzania's 2016 FSE policy covered schooling only from Standard 1 to Form 4.) The first national exam of secondary school comes at the end of Form 2 (grade 9), when 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).[6]

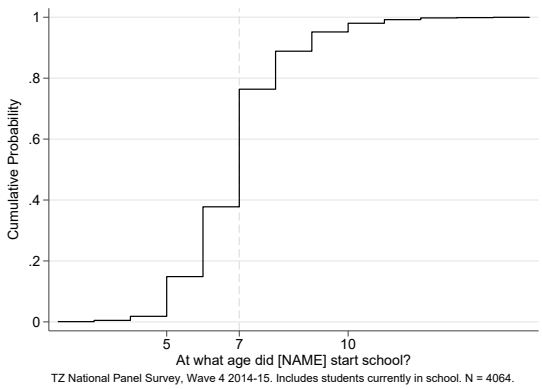
Primary schools and ordinary secondary schools begin the school year in January. The PSLE is administered in September; the FTNA is administered in November.

Descriptive data suggest there are 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 for those who had barely finished secondary school (11 years of schooling) were USD 27, compared to an average of USD 5 for those who had not finished secondary school.[7]

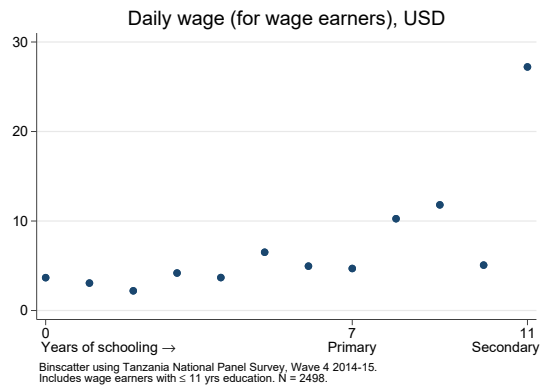
[6] At the end of Form 4, students sit the Certificate of Secondary Education Examination (CSEE) – roughly corresponding to the "O-level" in the British system. Students who continue through the sixth year of secondary school take the Advanced Certificate of Secondary Education Examination (ACSEE) – the equivalent of the British "A-level."

[7] From the nationally representative Tanzania National Panel Survey 2014-2015; $N = 2498$. I use the exchange rate of 1663.73 TZS = 1 USD, the rate as of 8 October 2014 according to https://www.exchangerates.org.uk/USD-TZS-spot-exchange-rates-history-2014.html

Figure 1: Schooling in Tanzania



(a) Age of school starting



(b) Mincer wage returns

2.1 Exam 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.

I focus on two of these exams: the Primary School Leaving Examination (PSLE), the last exam of primary school (grade 7); and the Form Two National Assessment (FTNA), the first exam of secondary school (grade 9).

2.2 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.[8] 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

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

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.[9] 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).

2.3 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 (grade 9, or "Form Two" in Tanzania's terminology). A pass is required to continue on to the third year of secondary school.

Both exams feature high variation in average pass rates across time, which likely has more to do with the difficulty of the exam than with large aggregate changes in students' preparedness (similar to other contexts; see Rossiter, Abreh, Ali, and Sandefur (2023)). For this reason my identification strategy will focus on differences in test outcomes within cohorts, and how these change over time.

2.4 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.

2.4.1 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. NECTA does not publicly report direct measures of secondary transition at the individual level, so I construct a proxy measure. I match the names of PSLE students in a given cohort to the names of FTNA takers from two years later (the same cohort assuming normal grade

[9] Section B details efforts to validate this proxy measure.

progression). This measure likely undercounts the true number of transitioners for at least four reasons: 1) To avoid false positives, I only use this measure for students whose full name is unique within their cohort across the entire country (98% of PSLE takers); 2) it misses pupils who transition to secondary school but drop out sometime in the first two years before taking the FTNA; 3) it misses pupils who skip ahead or fall behind the normal grade progression schedule; and 4) it misses pupils whose name is recorded differently between PSLE and FTNA, including misspellings. Section B provides more details on the construction of this measure.[10]

2.4.2 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. For the cohort of FTNA takers from 2014, available data show which students had their results withheld for lack of exam fee payments (these exam results were later made available). Because exam payments and school fee payments are typically made at the same time, a failure to pay exam fees implies a likely failure to pay school fees as well. These students, in other words, come from families which value education highly enough to have put at least one child at least halfway through basic secondary school. School fee non-payment thus functions as an indicator that school fees represent a binding financial constraints on secondary school access for these families.

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. In Tanzania, children carry their father's last name, so siblings tend to share a last name. Wards are small: they are the third administrative unit of Tanzania, there are over 3600 of them, and the median ward has around 200 PSLE takers per year in the period I study. I first identify the last names of all 2014 FTNA takers which can be clearly classified within a ward in terms of treatment – that is, all names for which all 2014 grade 9 exam takers in the ward either did or did not pay their exam fees (this describes 92% of the sample, 63% of whom are the only person in their ward$\times$cohort with that last name). I then match grade 7 exam takers to this group by last name within ward, yielding a sample of grade 7 test takers from 2014-2019 who share the last name of someone who took the grade 9 exam in 2014 in the same ward. I refer to these sets of students within wards who share a last name as "relatives." These students constitute the main analysis sample for my empirical strategy for identifying the causal effect of secondary access on primary

[10]See also Blackmon (2017), who performs a similar same string matching procedure in order to measure school value-added.

outcomes (see Section 4).

2.4.3 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 2014-2019, whose school can be located in a ward, and who can be matched to a 2014 FTNA taker in the same ward (see Section 4 for the sample selection criteria).

Table 1: Summary stats: PSLE takers

	All	Analysis Sample
Female	0.529	0.534
	(0.499)	(0.499)
Public school	0.966	0.970
	(0.182)	(0.171)
km to nearest sec. school	3.520	2.657
	(5.863)	(4.093)
School cohort size	83.708	84.036
	(69.952)	(70.631)
Pass PSLE	0.708	0.697
	(0.455)	(0.460)
A on PSLE	0.031	0.024
	(0.174)	(0.152)
Took FTNA 2 yrs later	0.483	0.468
	(0.500)	(0.499)
Elite	0.006	0.012
	(0.078)	(0.110)
Full name unique in cohort	0.979	0.958
	(0.144)	(0.201)
Last name unique in country $\times$ cohort	0.072	0.005
	(0.259)	(0.071)
Last name unique in ward $\times$ cohort	0.482	0.161
	(0.500)	(0.367)
Matched to 2014 FTNA sibling	0.208	1.000
	(0.406)	(0.000)
Treatment (sibling failed to pay exam fee)		0.106
		(0.307)
N	5,200,947	1,216,825

An observation is an individual student. The full universe includes all PSLE takers from mainland Tanzania 2014-2019. The analysis sample is restricted to PSLE takers in these years whose school can be located in a ward, and who can be clearly matched to a treatment status through a 2014 FTNA taker in the same ward. Distance to nearest secondary school measured from the primary school where PSLE was taken.

3 The policy: Free Secondary Education

3.1 The policy

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 ($\approx$ 375 million USD). (Twaweza East Africa, 2016b)

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

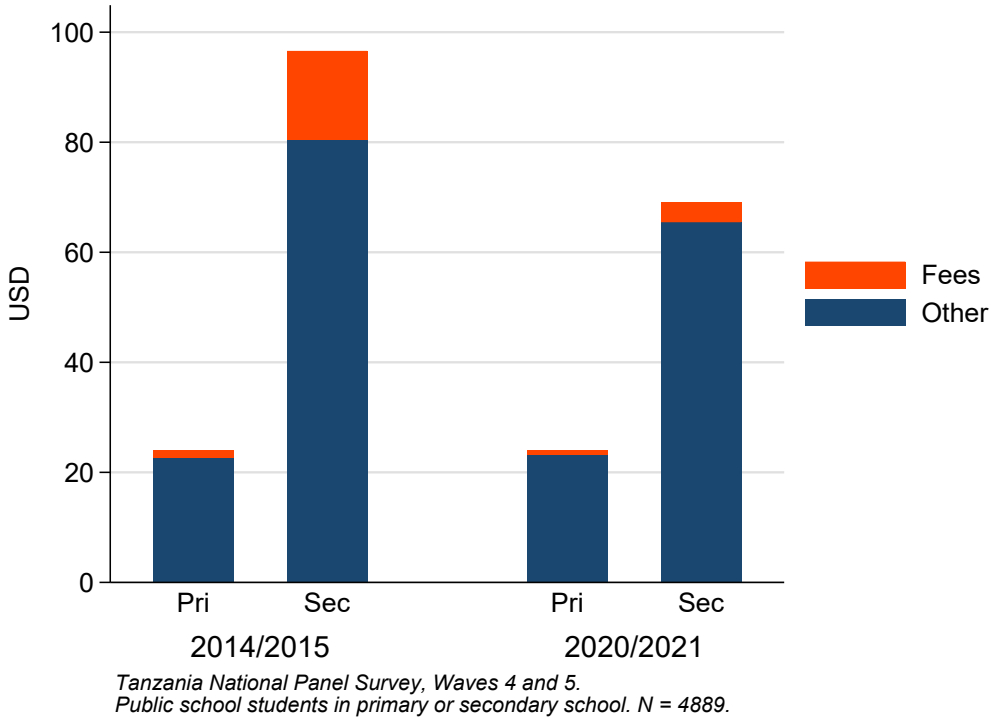
3.2 FSE increased secondary school access

To study the effect of secondary school access on primary school achievement, it is necessary that secondary access increase. Although this was FSE's stated goal, success was not a foregone conclusion. Prior to the reform, Tanzania had a history of education policies which didn't live up to expectations. It had failed to make sufficient capitation grant transfers for the Free Primary Education program, for example (Twaweza East Africa, 2013). In the decades leading up to 2016, Tanzania's government had made various commitments to free education, at both the primary and secondary level, not all of which were met (Opalo, 2022).

3.2.1 Education spending fell for students in public secondary schools

I draw on National Panel Survey data to show that in the wake of FSE's implementation, household spending on public secondary schools – especially on school fees – fell markedly. Figure 2 shows raw average annual education expenditures by category for students in public schools, at either the primary or secondary level, in the last wave prior to FSE (2014/15) and the first wave after FSE (2020/21). Primary students' expenditures on fees are close to zero ex ante – consistent with reasonably faithful implementation of Free Primary Education – and fall to almost zero by 2020/21. Secondary students' expenditure on school fees, meanwhile, falls precipitously, as does "other" spending that can include auxiliary fees.

Figure 2: Secondary school spending fell



Source: Tanzania National Panel Survey, 2014-15 and 2020-2021 waves. Y-axis measured in nominal USD. Sample consists of students in public schools.

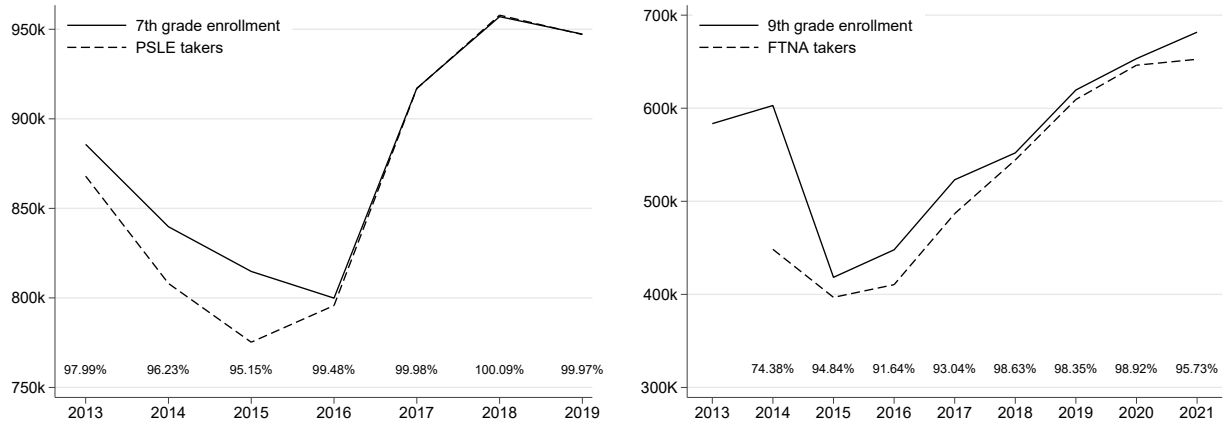
3.2.2 Enrollments and test-taking rose

Another piece of *prima facie* evidence that FSE increased secondary access is that secondary enrollments, as well as the number of students sitting the FTNA in grade 9, increased in the years after the policy. The policy also preceded dramatic jumps in primary enrollments, and in the fraction of grade 7 students sitting the PSLE, a requirement for entering public secondary school. This again proves *prima facie* evidence consistent with the idea that FSE had effects on forward-looking primary students.[11]

Upon the implementation of the FSE policy, the number of students enrolled in 7th grade jumped, as did the fraction of students who sat the PSLE exam.

[11] 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 and abandoned by 2015, caused schools to strategically exclude students from the last year of primary school. This may partly explain the falling trend in test takers for both PSLE and FTNA prior to the introduction of FSE.

Figure 3: Enrollment and test takers



(a) Grade 7 (primary)

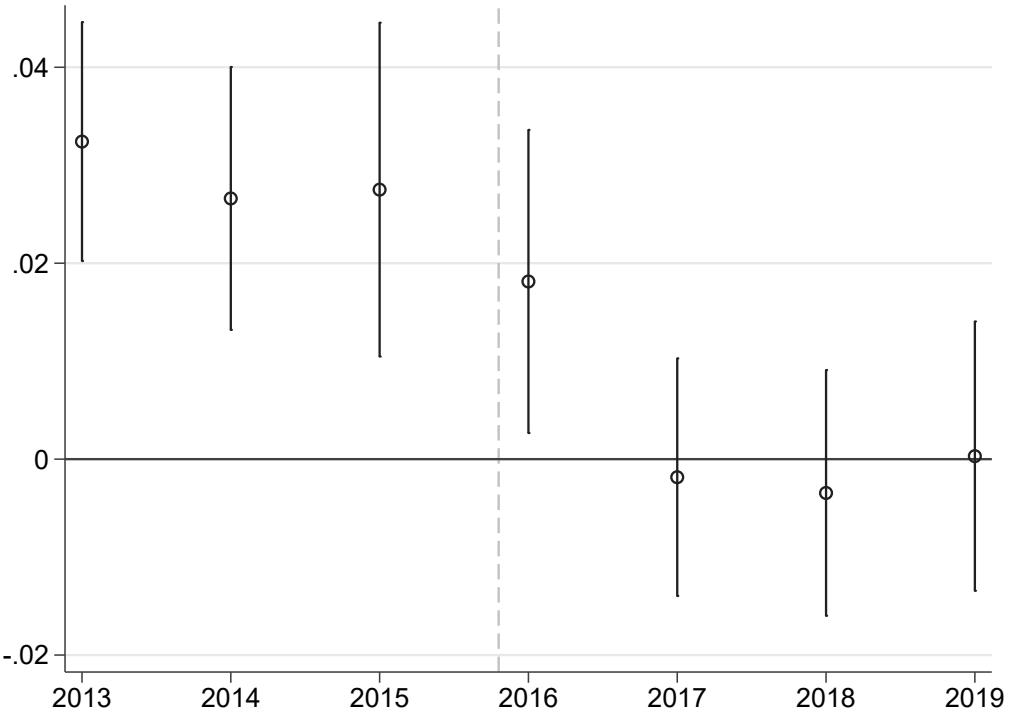
(b) Grade 9 (secondary)

3.2.3 The elite gap in transition rates disappeared

Another index of school access is how regular people fare in the system relative to elites. As outlines in Section 2.4.3, I identify "elite" students by matching them by last name to a local political candidate in their ward. 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.

4 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 2.1, 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 fees were binding in the absence of the reform (in this case, exam fees).

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.

Students from financially constrained families perform worse on the PSLE prior to the reform: on average their scores are 0.1 SD lower, and they are 4.5 percentage points less likely to pass. I include fixed effects for the older sibling's school $\times$ FTNA score.

$$Y_{itf} = \alpha + \eta_f + \lambda_t + \sum_{t=2014}^{2019} \beta_t \, NonPayment_f \times \lambda_t + varepsilon_{itf} \quad (1)$$

In Equation 2, Y_{itf} is the outcome for PSLE-taker i in year t, from family f, whose sibling took the FTNA in 2014. λ_t are year fixed effects; η_f are family (last name $\times$ ward) fixed effects. $NonPayment_f$ is an indicator for whether the student's 2014 sibling failed to pay the FTNA fee. This indicator is then interacted with indicators for the years 2014-2019.[12] The β_t coefficients on the interaction terms are the coefficients of interest.

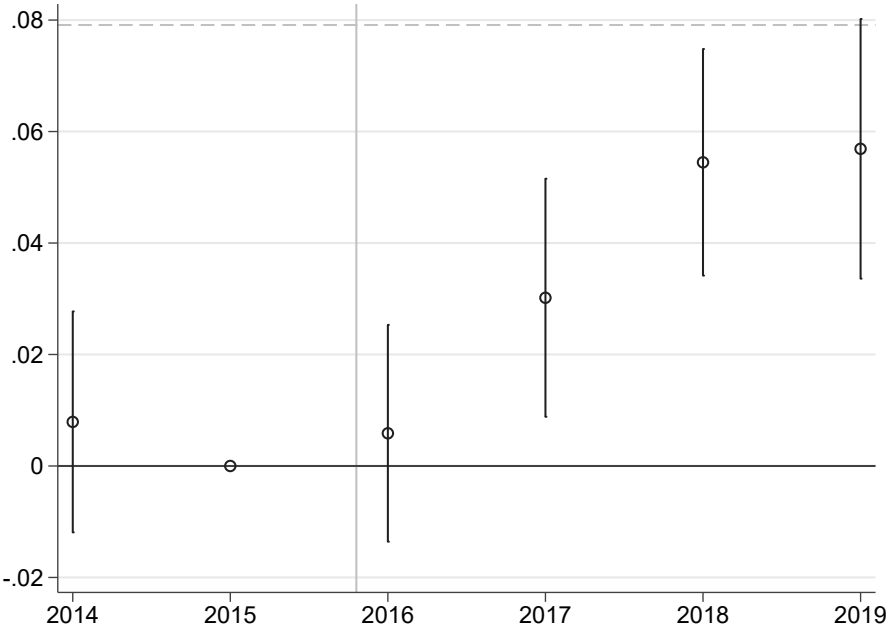
Section D explores alternative methods of defining the affected population, and yields findings broadly consistent with those outlined here.

5 Results

Free Secondary Education raised pass rates on the Primary School Leaving Examination. Figure 5 depicts the main result, plotting the interactions between the family financial constraints dummy and the year dummies from Equation 2. There was no significant change in the difference in pass rates between students from more vs. less financially constrained families prior to the reform. After secondary school fees were abolished for public schools, students from poorer families became relatively more likely to pass the exam.

[12] Although PSLE scores are available for 2013, I exclude this year to avoid potential endogeneity of 2014 FTNA payments to siblings' 2013 PSLE performance.

Figure 5: Difference in pass rates for students from families with and without non-payment of fees



Notes: This figure shows interaction coefficients from Equation 2. The specification shows how the difference in outcomes between grade 7 exam takers from more vs. less financially constrained families changed over time. The figure plots the coefficients and confidence intervals on the interaction between the $NonPayment_i$ dummy and each year dummy (where the omitted year is 2015, the last year before the policy began). For reference, the dotted line at 0.079 shows the average raw pre-reform difference in pass rates between pupils from more vs. less financially constrained families.

This change dramatically attenuated the raw gap in pass rates between students from more vs. less financially constrained families. While the estimation relies on family fixed effects, Figure 5 includes a horizontal line denoting the average pre-reform difference in pass rates of 7.9 percentage points. By 2019, this gap had narrowed considerably.

Table 2 shows another view of the diff-in-diff results. These results show that the reform increased pass rates by 2.8 percentage points. The policy also affected various other indices of individual investment in primary schooling. It caused an appreciable drop in the share of students who registered for the grade 7 exam but failed to show up on the day ("Absent"). The (pre-reform) pass rate of the primary schools from which students took the exam – a proxy for school quality – also increased. So did the rate of students making the transition from primary to secondary school.[13] However, despite raising pass rates, the reform

[13]The reform had no effect on the likelihood of students showing up in the PSLE data at all, as measured by comparing paying vs. non-paying 2014 FTNA takers' likelihood of matching to PSLE takers before vs. after the reform. See Table A.2.

had no impact on overall test scores. This shows that the reform caused students to reallocate effort, as explored in Section 5.1.

Table 2: Diff-in-diff: family financial constraints measured by exam fee non-payment

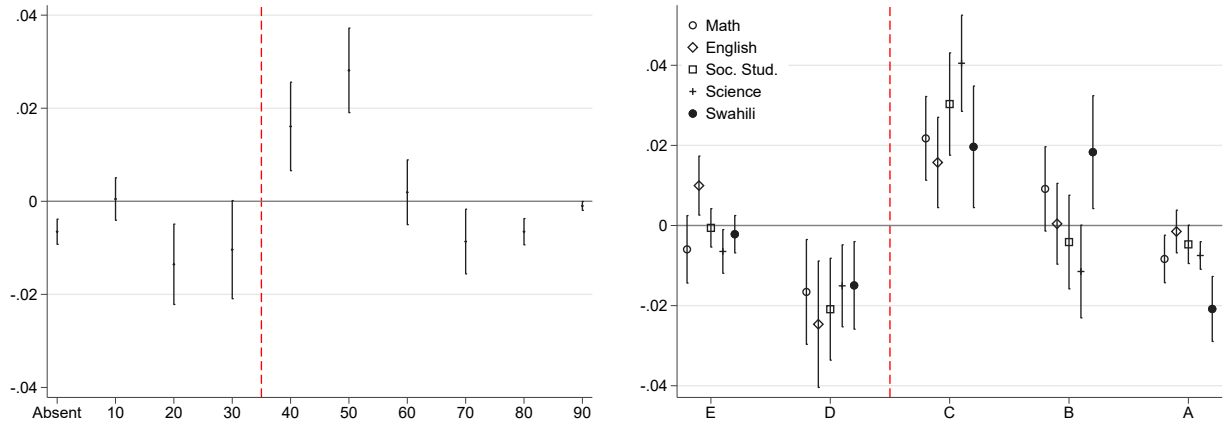
	Pri achievement		Pri investment			Sec achievement	
	Pass PSLE	PSLE score (σ)	Absent PSLE	Pri. school pass rate (pre-reform)	Pri. school cohort size	Transition	Pass yr9 exam | transition
Treat × Post	0.028***	0.011	-0.005***	0.023***	-1.261	0.027***	0.024***
	(0.008)	(0.017)	(0.001)	(0.005)	(1.297)	(0.009)	(0.007)
N	1,216,825	1,216,825	1,216,825	1,216,825	1,216,825	1,216,825	523,827
Mean (treated, pre)	0.529	-0.117	0.024	0.473	89.420	0.164	0.846

All regressions include family FE. Standard errors clustered by ward. Transition only measured for students whose full name is unique across the country within their cohort. * p<0.10, ** p<0.05, *** p<0.01

5.1 Distributional effects

Examining the effect of the policy on the distribution of test scores can shed important light on the mechanism behind the overall effect. For example, if we were to be observing an "Ashenfelter dip" phenomenon (Ashenfelter, 1978), we might expect that students from families who were labeled as "financially constrained" due to having received a temporary bad shock would see a subsequent shift in the entire distribution of their scores as their family returned to its steady state. If instead FSE had an effect on the effort of students from financially constrained families, we would expect to see the distribution change in ways that indicate an allocation of effort to maximize the likelihood of passing the PSLE to qualify for secondary school. This is what we see in Figure 6.

Figure 6: Effects on grade 7 test score distributions



(a) Overall score

(b) By subject

Panel 6a shows the diff-in-diff estimates on students' likelihood of receiving an overall score in each of ten bins. Panel 6b shows the diff-in-diff estimates on students' likelihood of receiving a score of each of five grades (A-E) in each of the PSLE's five subjects. Fraction of pupils receiving a grade of C (40%) or greater in each subject prior to 2016: Math (38%); English (41%); Social studies (57%); Science (58%); Swahili (72%)

Figure 6a shows that FSE did not move the entire distribution – instead, it caused students to be less likely to get a just-failing score and more likely to barely clear the threshold to pass the exam – an overall score of 40%. This is consistent with students changing their allocation of effort in order to pass the exam.

Figure 6b shows how FSE affected students' peformance on the five individual subjects in the exam: Math, English, Social studies, Science, and Swahili. Prior to FSE, Swahili was by far the subject with the highest scores – 72% of pupils scored a "C" or better, compared to just 38% who got at least a "C" in Math. FSE caused large shifts in the score distribution from "D" to "C" for Math, as well as for Social Studies and Science. But it caused large shifts from "A" to "B" in Swahili. This is consistent with students reallocating study effort away from easier subjects and toward more difficult subjects in pursuit of a passing grade overall.

6 Analysis at the school market level

6.1 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

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. Did new primary schools sprout up to service increased demand in more affected areas? Did students select into areas with certain types of schools?

6.1.1 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.

$$Y_{wt} = \alpha + \delta\, treat_w \times post_t + \lambda_t + \eta_w + \varepsilon_{wt}$$

Y_{wt} designates the outcome for ward w in year t. $treat_w$ indicates that ward w had non-zero FTNA non-payment in 2014.$post_t$: Dummy for implementation period (post-2015). λ_t, η_w: year FE, ward FE.

Figure 7 maps the wards of Tanzania by whether they had any FTNA exam fee non-payment in 2014. "Treated" wards are those that had some positive amount of exam fee non-payment.

6.2 Results: ward-level variation

Table 3 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 3: Ward-level results

	Pass rate	PSLE avg score	Transition rate	Num students	Num schools
Treat × Post	0.022***	0.558**	0.016***	10.400	0.073
	(0.006)	(0.221)	(0.005)	(6.927)	(0.097)
N	15,840	15,840	15,840	15,840	15,840
Mean (untreated)	0.679	46.462	0.438	299.873	5.966

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

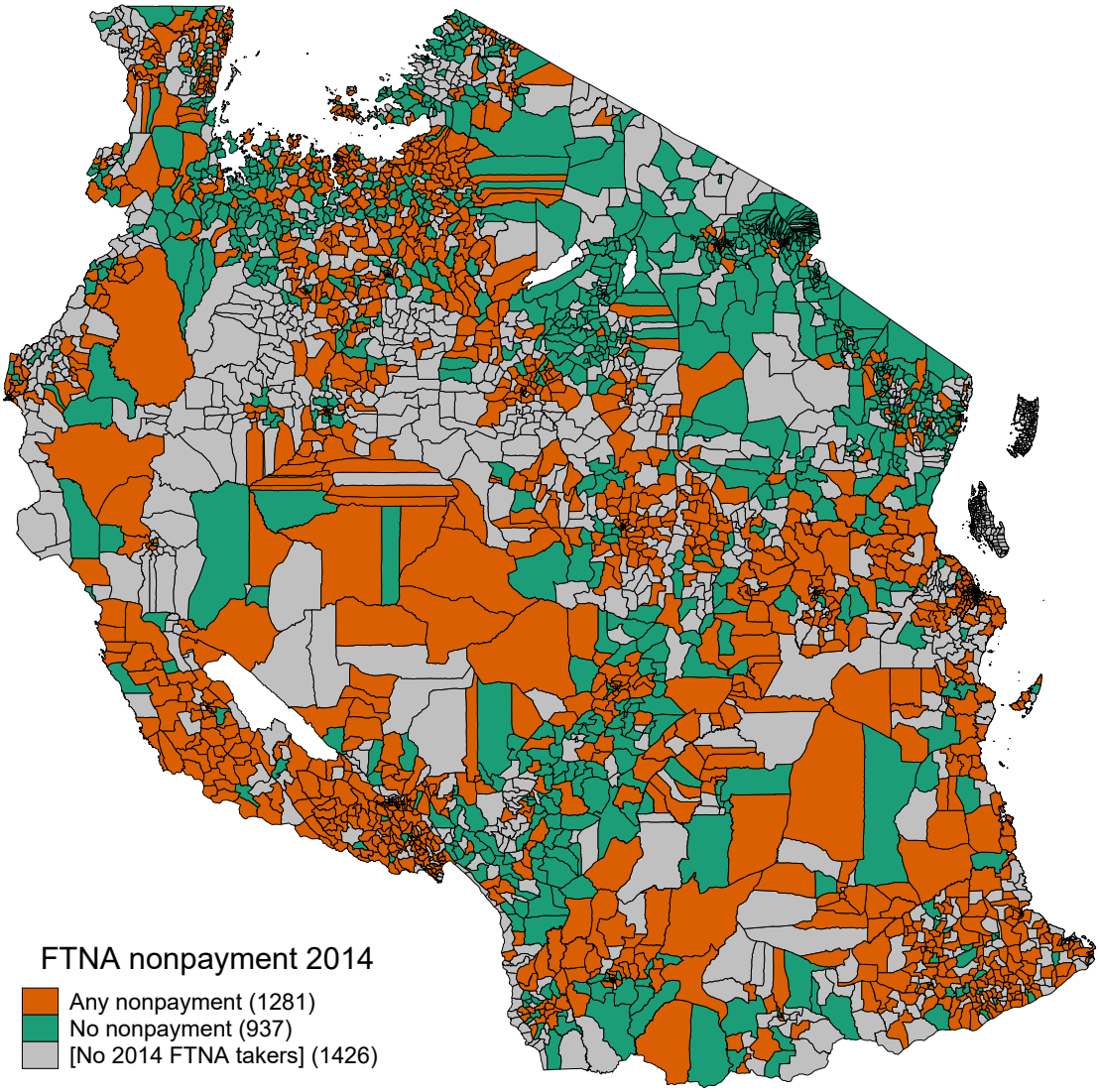


Figure 7: Ward-level 2014 FTNA fee non-payment

7 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.

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A Appendix: Extra tables and figures

A.1 Effect on secondary outcomes, conditional on transition

Table A.1: Diff-in-diff: effect on sec. outcomes, conditional on transition

	Pass FTNA	Absent FTNA	FTNA score (σ)	Sec. school pass rate (pre-reform)	Sec. school public	Moved wards
Treat × Post	0.024***	0.002	0.036*	-0.000	0.001	-0.018
	(0.007)	(0.003)	(0.019)	(0.000)	(0.005)	(0.012)
N	523,827	523,827	523,827	523,827	523,827	523,827
Mean (untreated)	0.887	0.020	-0.053	0.999	0.916	0.430

All regressions include family FE. Standard errors clustered by ward. Transition only measured for students whose full name is unique across the country within their cohort. * p<0.10, ** p<0.05, *** p<0.01

A.2 No effect on number of younger siblings taking PSLE

One potential margin could be students' likelihood of sitting the grade 7 exam at all. To test this, I match grade 7 exam takers by last name within ward to the list of 2014 grade 9 takers ("older siblings"), and collapse to the level of the older sibling × grade 7 year to measure how many younger siblings each older sibling had in each cohort of grade 7 test takers. I test whether the number of grade 7 takers matched to each older sibling changed after Free Secondary Education was introduced, clustering standard errors by year and older sibling.

Table A.2: Diff-in-diff: effect on number of younger siblings taking PSLE

	Any sibling took PSLE		Num. siblings took PSLE	
Treat × Post	0.002	0.000	0.002	-0.007
	(0.001)	(0.001)	(0.001)	(0.004)
Mean	0.089	0.147	0.089	0.293
N	1,977,675	1,977,675	1,977,675	1,977,675
Year FE	✓	✓	✓	✓
Family FE	✓	✓	✓	✓
Match criterion	Strict	Less strict	Strict	Less strict

An observation is a 2014 FTNA taker × year. Standard errors clustered by 2014 FTNA taker and year. Odd columns limit sibling definition to PSLE takers with unique last names within their ward × cohort to reduce the incidence of false positives. Even columns omit this restriction. * p<0.10, ** p<0.05, *** p<0.01

Table A.2 shows that grade 9 test takers from more vs. less financially constrained families are no more or less likely to have younger siblings sitting the grade 7 exam in the aftermath of the policy. This is consistent with Figure 3a, which shows that even the baseline rates of exam sitting for grade 7 students are very high.

A.3 Stricter sibling matching

My main analysis sample consists of all grade 7 test takers whose last name matches that of a 2014 grade 9 test-taker with a unique last name within the ward. That incorporates the largest possible sample, but it may be more likely to introduce false positives. In this section, I limit the analysis sample to grade 7 test takers *whose last name is unique within their ward* $\times$ cohort, and whose last name matches that of a 2014 grade 9 test-taker with a unique last name within the ward.

Table A.3: Diff-in-diff: family financial constraints measured by exam fee non-payment

	Pass PSLE	Absent PSLE	PSLE score (σ)	Pri. school pass rate (pre-reform)	Transition
Treat $\times$ Post	0.025***	-0.003	0.013	0.007**	0.028***
	(0.009)	(0.002)	(0.020)	(0.003)	(0.010)
N	134,322	134,322	134,322	134,322	134,322
Mean (untreated)	0.642	0.016	0.133	0.556	0.251

All regressions include family FE. Standard errors clustered by ward. Transition only measured for students whose full name is unique across the country within their cohort. * p<0.10, ** p<0.05, *** p<0.01

A.4 School $\times$ FTNA score fixed effects

An alternative specification omits family fixed effects, but includes fixed effects for the 2014 grade 9 sibling's school $\times$ FTNA score:

$$Y_{itsg} = \alpha + \beta\, NonPayment_i + \sum_{t=2014}^{2019} \delta_t\, NonPayment_i \times \lambda_t + \lambda_t + \eta_s \times \zeta_g + \varepsilon_{itsg} \quad (2)$$

In Equation 2, Y_{itsg} is the outcome for PSLE-taker i in year t, whose sibling took the FTNA in school s and received grade g. $NonPayment_i$ 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[14]. λ_t are year dummies, η_s are dummies for the secondary school in which the older sibling sat the FTNA, and ζ_g are dummies for the overall grade the older sibling received on the FTNA. The δ_t coefficients on the interaction terms are the coefficients of interest

Table A.4: Diff-in-diff: family financial constraints measured by exam fee non-payment

	Pass PSLE	Absent PSLE	PSLE score (σ)	Pri. school pass rate (pre-reform)	Transition
Treat	-0.019***	0.003**	-0.028**	0.001	-0.026***
	(0.006)	(0.001)	(0.014)	(0.003)	(0.007)
Treat × Post	0.027***	-0.004***	0.020	0.001	0.024***
	(0.007)	(0.001)	(0.016)	(0.002)	(0.008)
N	551,613	551,756	551,756	476,444	533,843
Mean (untreated)	0.609	0.017	0.064	0.540	0.240

All regressions include older sibling's school × FTNA score FE. Standard errors clustered by ward. Transition only measured for students whose full name is unique across the country within their cohort. * p<0.10, ** p<0.05, *** p<0.01

B Data details

This section provides more detail on construction of the proxy measures created using NECTA data.

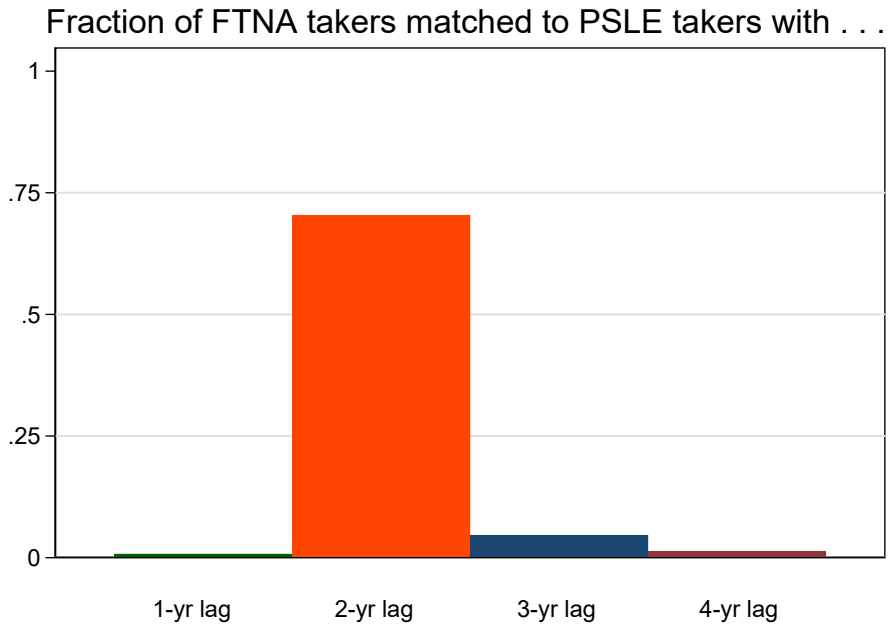
B.1 Secondary transition measure

While the quality of data fluctuates over time, data quality doesn't appear to drive the results. One measure of data quality is whether student names include a middle name or only a middle initial; students with only a middle initial are less likely to be uniquely identified, and middle initials are relatively more common earlier in the sample. Within ward×cohorts, names from treated families are no more or less likely than those from control families to have only a middle initial, in either the grade 7 PSLE exam or the grade 9 FTNA exam.

Figure B.1 shows what fraction of PSLE takers match to the name of an FTNA taker from subsequent years.

[14]Although PSLE scores are available for 2013, I exclude this year to avoid potential endogeneity of 2014 FTNA payments to siblings' 2013 PSLE performance.

Figure B.1: Match rate of PSLE takers to FTNA takers in subsequent cohorts



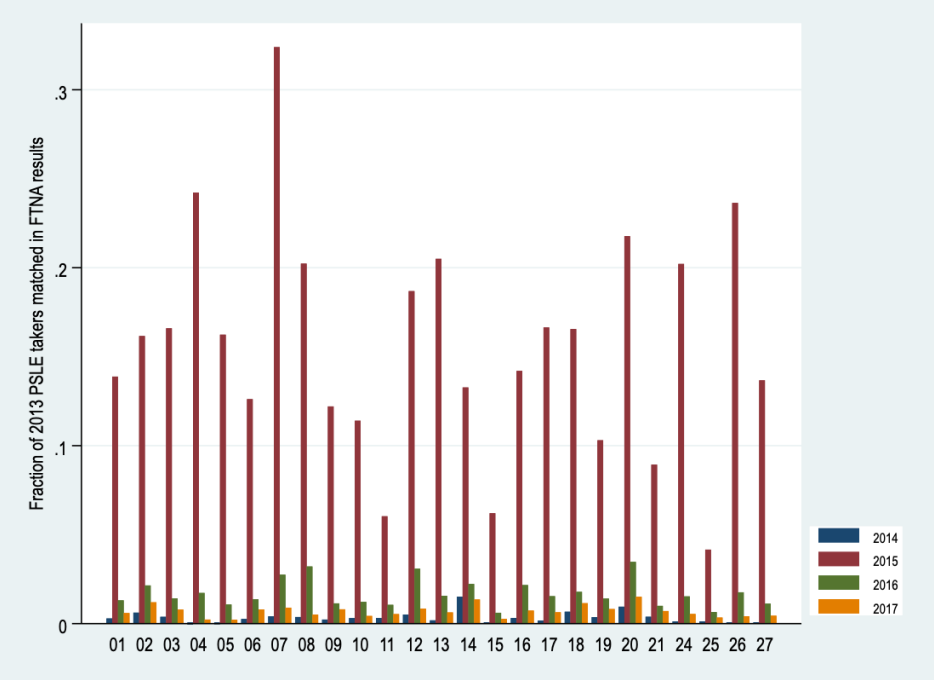
Notes: This figure shows the fraction of uniquely-named FTNA takers whose name matches a uniquely-named PSLE taker from one, two, three, or four years earlier. A two-year lag represents normal grade progression.

A negligible fraction (less than 1%) 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.

Smaller numbers of PSLE takers match to the FTNA cohorts from three and four years after they take the PSLE. These groups represent some combination of spurious name-sharers and students who failed the PSLE the first time around, or who were delayed for a year or two in their studies for other reasons, but ultimately made it to the end of the second year of secondary school.

Figure B.2: Match rate of 2013 PSLE takers to FTNA takers in subsequent years, by region



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), by region (labeled by region code).

Figure B.2 demonstrates the high variability in transition rates across regions.

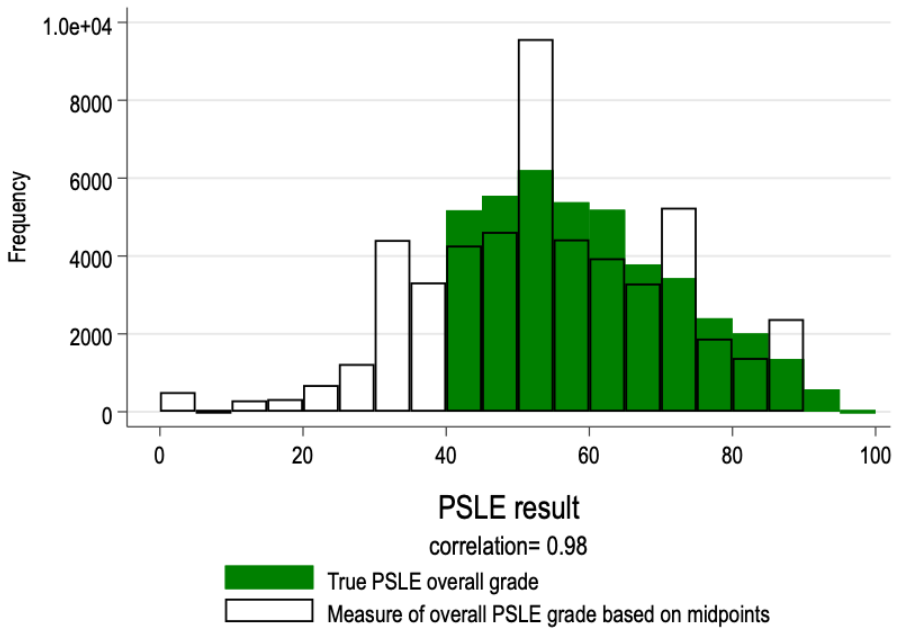
B.2 Validating the PSLE overall score measure

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.

This exercise also turns up no evidence of excess mass to the right of the passing threshold, suggesting a lack of manipulation of test scores.

Figure B.3: Mwanza 2016: comparison of proxy overall PSLE score with true overall PSLE score for PSLE passers



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 PSLE questions

D Robustness of student results

D.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 $\times$ older sibling's PSLE

Question 2: Juayote _____ reading a book now.

A. have
B. is
C. were
D. had
E. was

Response pattern

Option	A	B*	C	D	E	Omitted	Others
No. of candidates	148757	437924	104199	49250	100943	1361	2282
% of candidates	17.61	51.84	12.34	5.83	11.95	0.16	0.27

Swali la 1: $6.894 - 5.759 =$

A. 1.085
B. 1.045
C. 1.135
D. 1.145
E. 1.125

Majibu ya watahiniwa

Chaguo	A	B	C*	D	E	Wasiojibu	Mengine
Idadi ya watahiniwa	35,671	36,059	672,201	77,955	19,757	890	2,176
Asilimia ya watahiniwa	4.22	4.27	79.58	9.23	2.34	0.11	0.26

Question 15: This book is yours but the blue one is _____.

A. mine
B. your
C. my
D. me
E. their

Response pattern

Option	A*	B	C	D	E	Omitted	Others
No. of candidates	331523	186011	112602	114238	94206	2839	3297
% of candidates	39.25	22.02	13.33	13.52	11.15	0.34	0.39

Swali la 4: $472 \times 8,934 =$

A. 4,216,884
B. 4,126,848
C. 4,216,848
D. 4,126,748
E. 4,206,848.

Majibu ya watahiniwa

Chaguo	A	B	C*	D	E	Wasiojibu	Mengine
Idadi ya watahiniwa	87,192	132,281	427,855	91,799	93,863	8,697	3,022
Asilimia ya watahiniwa	10.32	15.66	50.65	10.87	11.11	1.03	0.36

Figure C.1: Example questions from the 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 3:

$$Y_{isgt} = \alpha + \beta\, SiblingDropout_i + \delta\, SiblingDropout_i \times post_t + \lambda_t + \eta_{sg} + \varepsilon_{isgt} \tag{3}$$

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

D.1.1 Alternate identification 2: Regression discontinuity at the passing threshold

Finally, one concern could be that even after including school $\times$ 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 2.1, 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 4:

$$Y_{isgt} = \alpha + \beta\, SiblingFail_i + \delta\, SiblingFail_i \times post_t + \lambda_t + \eta_{sg} + \varepsilon_{isgt} \tag{4}$$

Table D.1 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 D.1: Summary statistics: sibling pairs

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

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 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.

D.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 D.2 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 G.1, though smaller, and in the case of the transition outcome, statistically insignificant.

Table D.2: Student level: Diff in diff by older siblings' continuation

	Pass PSLE		PSLE (σ)		Transitioned	
Older sib dropout	-0.030***		-0.032***		-0.043***	
	(0.006)		(0.012)		(0.008)	
Older sib dropout × Post	0.040***		0.044***		0.008	
	(0.005)		(0.010)		(0.008)	
Older sib failed PSLE		-0.050***		-0.071***		-0.019
		(0.009)		(0.017)		(0.012)
Older sib failed PSLE × Post		0.067***		0.087***		0.021***
		(0.005)		(0.010)		(0.007)
N	271427	271427	271427	271427	143090	143090
Adj. R^2	0.153	0.154	0.280	0.280	0.066	0.066
Mean (pre-reform)	0.596	0.596	-0.006	-0.006	0.375	0.375

Standard errors clustered by school. All regressions include fixed effects for older sibling's year × school × PSLE score. 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. * 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.

E District-level variation

E.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).

F School-level variation

F.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 5 expresses the specification:

$$Y_{dst} = \beta\, DropoutRate2013_s \times post_t + \lambda_t + \theta_s + \theta_d \times trend_t + \varepsilon_{dst} \tag{5}$$

My school-level treatment variable, $DropoutRate2013_s$, is a school's pre-reform primary-to-secondary dropout rate.[15] The rationale for this is that a primary school which already sends most of its students on

[15] As defined using string matching on names within districts between PSLE and FTNA takers, see Section 2.1. This is likely an

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 β, the interaction of this measure of treatment intensity with a dummy for the post period (2016 and later). I include λ_t year fixed effects and θ_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.[16]

F.2 Results: school-level variation

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

	Student-level				School-level	
	PSLE (%)	PSLE (σ)	Nationwide PSLE %ile	Transitioned	Number of test takers	Number of retakers
Dropout rate 2013 × Post	6.860***	0.428***	15.059***	0.167***	-6.737***	-0.030
	(0.460)	(0.028)	(0.828)	(0.014)	(1.271)	(0.019)
N	4424285	4424285	4424285	2954216	81249	81249
Mean (pre-reform)	43.813	0.014	46.785	0.160	51.440	0.077
Trends	District	District	District	District	District	District

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 F.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%.

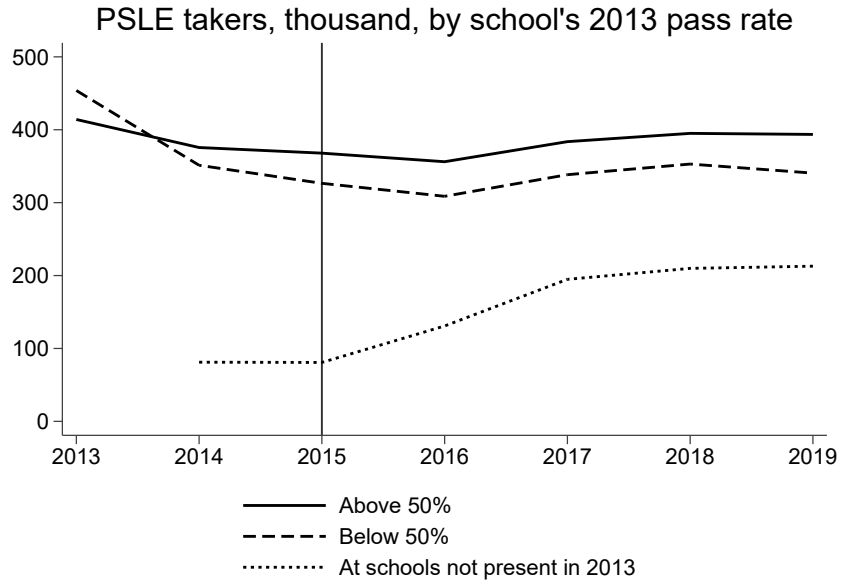
Columns 5 and 6 measure outcomes at the school level rather than the student level. Column 5 shows that the number of people taking the PSLE in these schools actually declined after the reform, by 6.7 test takers from a base of 51.4. Combined with the overall post-reform increase in enrollments and

upper bound on the true dropout rate.

[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 × time there would be equivalent to me controlling for school × 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.

test-taking, this reduction in test takers at low-transition-rate schools implies a large increase at high-transition-rate schools. Figure F.1 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).[17] 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.

Figure F.1: New test takers move into new schools



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

[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).

performance. Distinguishing between these two stories is made easier by the results based on student-level variation in Section 4. 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.

G Alternate family-level variation

G.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 $\widehat{dropout}_{isg2014}$. 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-$\widehat{dropout}_{isg2014}$ 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,

[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.

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.

G.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.

G.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

[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).

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 $\widehat{dropout}_{isg2014}$ for the likelihood of a younger sibling's secondary transition in the absence of the FSE reform using Equation 6:

$$\widehat{dropout}_{isg2014} = \alpha + \beta \mathbf{X_i} + \eta_{sg} \tag{6}$$

where $\mathbf{X_i}$ 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 $\widehat{dropout}_{isg2014}$ 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 7 lets $\widehat{dropout}_{isg2014}$ be the treatment variable, where the δ coefficient on the interaction between $\widehat{dropout}_{isg2014}$ and $post_t$ is the coefficient of interest:

$$Y_{isgt} = \alpha + \beta \widehat{dropout}_{isg2014} + \delta \widehat{dropout}_{isg2014} \times post_t + \lambda_t + \eta_{sg} + \varepsilon_{isgt} \tag{7}$$

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

Table G.1 shows results which use pre-reform family data to predict which families' students are likely to transition in the absence of the FSE reform ($\widehat{dropout}_{isg2014}$). Students from high-$\widehat{dropout}_{isg2014}$ families saw much higher increases in PSLE scores and percentiles. Effects on secondary transition rates were positive but smaller and less precisely estimated.

Table G.1: Differential post-reform outcomes by pre-reform predicted family dropout

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

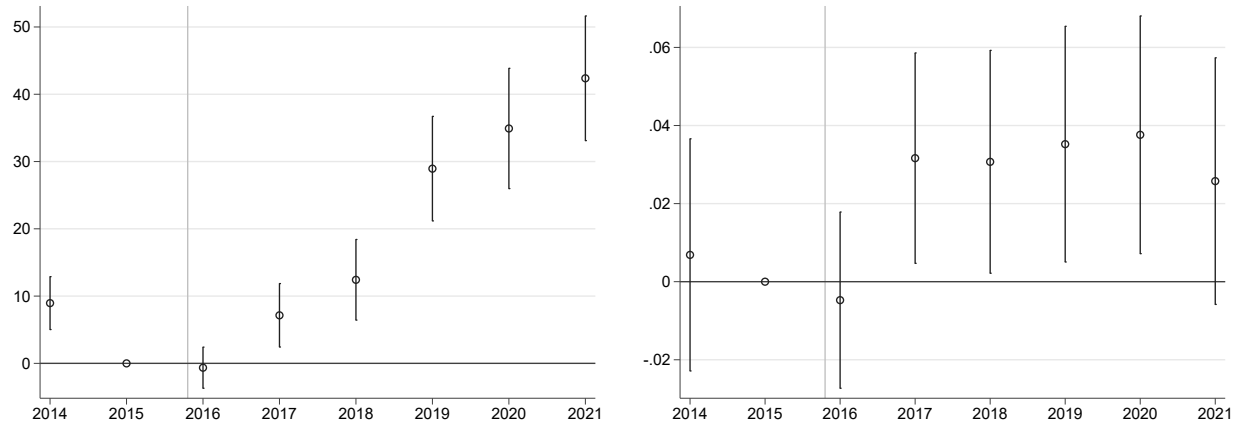
Standard errors clustered by school. All regressions include year and school × older-sibling-PSLE-marks FE. Sample limited to 2015-2018 PSLE takers who have the same last name as a 2013 PSLE taker whose last name is unique within her school AND to a 2014 PSLE taker (groups greater than 6 dropped to avoid 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

H Effects on secondary exam outcomes

H.1 Effect on FTNA takers (Grade 9)

Figure H.1: Test takers and performance



(a) FTNA takers per ward

(b) FTNA (z-score)

Table H.1: Ward-level diff-in-diff: effect on grade 9 exam outcomes

	FTNA Pass rate	FTNA avg score	Num FTNA students	Num Sec schools
Treat × Post	0.022***	0.023*	16.352***	0.020*
	(0.004)	(0.012)	(2.758)	(0.012)
N	20,234	20,229	20,234	20,234
Mean (pre, untreated)	0.820	0.010	133.746	1.598

Year and ward FE. Standard errors clustered by ward.

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

Table H.2: Ward-level diff-in-diff: effect on grade 9 outcomes, by school quality

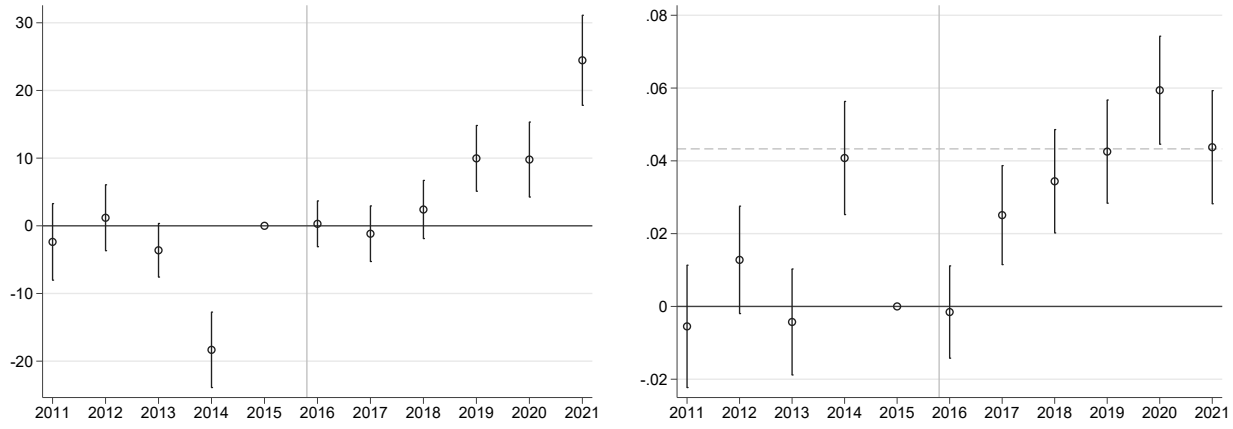
	Pass rate	FTNA avg score	Num students	Num schools
Treat × Post	0.001	-0.023	9.655***	0.008
	(0.006)	(0.017)	(3.388)	(0.017)
Hi Q × Post	-0.118***	-0.213***	-5.896	-0.019
	(0.006)	(0.019)	(3.654)	(0.018)
Treat × Hi Q × Post	0.012*	0.039	13.337**	0.023
	(0.007)	(0.024)	(5.497)	(0.024)
N	20,234	20,229	20,234	20,234
Mean (pre, untreated)	0.820	0.010	133.746	1.598

Year and ward FE. Standard errors clustered by ward.

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

H.2 Effect on CSEE takers (Grade 11)

Figure H.2: Test takers and performance



(a) CSEE takers per ward

(b) CSEE pass rate

Table H.3: Ward-level diff-in-diff: effect on grade 11 exam outcomes

	CSEE Pass rate	Num CSEE students	Num Sec schools
Treat × Post	0.022***	24.952***	0.192***
	(0.005)	(3.404)	(0.028)
N	36,357	36,357	36,357
Mean (pre, untreated)	0.630	114.266	1.439

Year and ward FE. Standard errors clustered by ward. Includes results from all schools that could be located in a ward.

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

Table H.4: Ward-level diff-in-diff: effect on grade 11 outcomes, by school quality

	Pass rate	Num students	Num schools
Treat × Post	0.013*	17.910***	0.115***
	(0.007)	(4.103)	(0.034)
Hi Q × Post	-0.057***	4.744	0.054
	(0.007)	(4.051)	(0.036)
Treat × Hi Q × Post	0.008	15.402**	0.171***
	(0.009)	(6.895)	(0.057)
N	36,132	36,132	36,132
Mean (pre, untreated)	0.630	114.528	1.441

Year and ward FE. Standard errors clustered by ward. Pre-reform school pass rate measured in 2015, the last year before the reform's implementation.

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