Secondary school access raises primary school achievement

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Abstract

I study how the introduction of Free Secondary Education (FSE) affected primary students in Tanzania. I first confirm FSE increased secondary access: secondary enrollments rose, household spending on secondary school fees plummeted, and elites' transition premium disappeared. Then, using variation in treatment exposure from fee payment microdata, I show that FSE increased primary exam pass rates by 6% and secondary transition rates by 23%. This was not due to changes to supply inputs: there was no effect on school entry, and class sizes rose. Instead it appears to be driven by demand-side investments: primary students selected into better schools, and household survey data confirms that they attended more and worked less.

Keywords: school access, human capital investments, high-stakes exam data, Tanzania
JEL Codes: I25, H52, O12

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1 Introduction

Can large-scale expansions of school access crowd in human capital investments by individuals? Complementarities between different supply-side educational inputs have been shown to be important (Mbiti et al., 2019; Kerwin & Thornton, 2021). But scholarly debates have largely overlooked how supply- vs. demand-side constraints on education in the developing world might interact (Glewwe & Muralidharan, 2016; Banerjee & Duflo, 2012).[1] Free school access is among the most fundamental supply-side inputs governments can provide, and understanding the demand-side response to it is a first-order question with important implications for the policy debate over free secondary education in poor countries (Crawfurd & Ali, 2022; Garlick, 2019).

Measuring the effect of supply increases on demand-side choices is challenging for a number of reasons. At-scale expansions in school access are rare, and by definition affect everyone, making it difficult to identify a counterfactual. RCT studies of the effect of scholarships provide evidence that access can raise individual student achievement (Kremer et al., 2009). However, general equilibrium and implementation concerns make the scalability of such effects an open question (Crawfurd & Ali, 2022; Bold et al., 2018). Furthermore, decomposing any aggregate effect into supply- and demand-side channels is complicated by the fact that student outcomes combine the effect of the intervention with demand-side adaptation (Das et al., 2013).

This paper overcomes these obstacles by studying how Tanzania's introduction of Free Secondary Education (FSE) affected the human capital investments and achievement of primary school students. By focusing on the anticipatory response among primary school students, I am able to isolate the demand-side investment responses to an expansion of (future) access — as distinct from the direct supply-side effect of FSE on secondary-age students.[2] Tanzania abolished fees for public lower secondary school in 2016 (public primary schooling has been free since 2002). Transition into secondary school, however, still requires that students pass the Primary School Leaving Examination (PSLE). Both of these policy decisions make Tanzania typical in the context of global expansions in school access: about half of sub-Saharan African countries have made lower secondary free, but two-thirds of African countries still have an exam requirement (Gruijters et al., 2024; Bashir et al., 2018).

The analysis uses the universe of administrative data on high-stakes exams from mainland Tanzania for primary and secondary school. The Primary School Leaving Examination (PSLE) is taken at the end of

[1] An important exception is Eble & Escueta (2023), who study complementarities between school quality and parents' aspirations.

[2] Decisions about individual investments in primary schooling may be taken by some combination of students and their parents or guardians. For the purposes of this paper, I abstract away from the question of who is responsible for demand-side investments.

Grade 7; passing it is a requirement for enrolling in a public secondary school (both before and after the reform). This is my main measure of primary student achievement. The Form Two National Assessment (FTNA), taken in Grade 9, is the first standardized test of secondary school; sitting this exam provides a proxy for secondary enrollment. It is also high-stakes: a pass is necessary to advance to grade 10. In the pre-reform period, FTNA exam results could be withheld (and grade progression halted) for failure to pay fees. I also measure effects on household educational investments using household data from the Tanzania Living Standards Measurement Survey.

I first show that the FSE policy did indeed increase access to secondary school. This is not trivial: Tanzania's government, like many in the region, has made various commitments to universalize primary and secondary education which ultimately failed (Opalo, 2022; Crawfurd & Ali, 2022). I present three pieces of descriptive evidence that the 2016 policy succeeded. Figure 2 shows that elites' advantage in secondary transition disappeared after school fees were abolished (where elites are those who share a last name with a local political candidate). Figure 1 shows that household spending on secondary students' school fees fell sharply after the reform. Figure 3 shows that secondary enrollments began to rise after the policy's implementation.

To measure the causal effect of FSE on primary school attainment, I compare outcomes before vs. after the reform for primary school students from families who were more vs. less constrained by school fees ex ante. This strategy shares an intuition with prior studies of universal schooling, which typically exploit broad geographic variation in treatment exposure as proxied by regional dropout rates (Lucas & Mbiti, 2012; Brudevold-Newman, 2021). My measure of treatment exposure is more granular and more directly indicative of fee constraints, coming from student-level microdata on secondary school fee payments prior to FSE's implementation. Families with a pre-FSE secondary student who stopped paying school fees are defined as fee-constrained — i.e., "treated" by the FSE policy. The comparison group consists of families with a pre-FSE secondary school student who did not stop paying fees. Before FSE, treated families' primary students were 6 percentage points less likely to transition to secondary school (conditional on test scores), a gap which did not vary significantly in the years prior to the reform. The trends in many outcomes for the two groups were on parallel trajectories prior to FSE. I compare how the difference in performance between treated and non-treated primary students changed after the reform.

My main result is that secondary school access raised the pass rate for the Grade 7 primary school leaving examination, as well as the transition rate to secondary school. After the reform, primary students from fee-constrained families began to catch up to comparison students in both outcome measures. By

the third year of the reform, the pass rate gap had almost completely disappeared (Figure 7). The reform raised pass rates for treated students by 6% (a 3.4 percentage point increase from a base of 55%). It also increased the transition rate by 23% (a 3.8 percentage point increase from a base of 17%). These findings are robust to changing the set of fixed effects, the procedure for identifying families, and the definition of treatment (using older sibling's dropout as an alternative measure), as well as to including controls interacted with time trends — see Section B for more details.

Turning to mechanisms, I show evidence that these effects are the result of increased demand-side investments rather than supply-side inputs. Administrative exam data show that the reform reduced absence rates and increased selection into better schools, two direct measures of demand-side investment. FSE also altered the distribution of Grade 7 test scores in a way that indicates a reallocation of student effort to maximize the probability of passing: effects were concentrated around the passing threshold, and the likelihood of getting high scores in easy subjects fell as the likelihood of getting passing scores in difficult subjects increased. Data from household surveys shows that the policy increased primary students' school attendance and reduced their participation in child labor.

I rule out the possibility that effects were driven by supply-side inputs or the manipulation of test scores. Because my estimates rely on within-school family-level variation in fee constraints, I can discount explanations involving inputs at the school level. Using variation in the prevalence of fee constraints across wards to study supply-side investments at a less granular level, I confirm the family-level results on test scores but find no evidence that the policy led to school entry or reduced class sizes. FSE also appears to have raised scores on the low-stakes Grade 4 exam, suggesting that learning effects were not the result merely of exam score manipulation.

This paper's main contribution is to the literature on the interaction between supply- and demand-side educational investments. Many studies find that households treat supply-side inputs as substitutes for their own investments (Houtenville & Conway, 2008; Pop-Eleches & Urquiola, 2013; Greaves et al., 2023; Fredriksson et al., 2016; Das et al., 2013). However, these studies consider only contexts where school access is taken as a given, mostly in the developed world: none examine an input as fundamental as access to school.[3] Andrabi et al. (2024) show that investments in public school quality spill over into the private sector due to competitive pressure, implying a supply-demand complementarity. The RCT literature on NGO-provided scholarships in the developing world is germane, as scholarships can considered individual-level fee abolition. Yi et al. (2015) finds no effect on test scores of unconditional junior-high

[3]In Tanzania, Burke & Beegle (2004) document a negative correlation between primary enrollment and distance to a secondary school, consistent with supply-demand complementarities.

scholarships in China. Duflo et al. (2021) show that secondary-school scholarships raised secondary educational attainment in Ghana, where secondary entry is subject to an exam requirement. Kremer et al. (2009) randomize the provision of merit scholarships to secondary school among 127 primary schools, showing that the scholarships raised test scores. This study is most relevant to mine, as it examines the primary-to-secondary transition in a context where access is conditional on performance. My study builds on this literature by showing evidence of these complementarities for a national-scale expansion of public school access.

I also contribute to the literature on anticipatory human capital investments. Dynamic considerations are central in human capital theory (Becker, 1964; Ben-Porath, 1967). One strand of the empirical literature on these dynamics measures anticipatory demand-side responses to environmental changes, such as reduced maternal mortality or increased labor market returns to education (Jayachandran & Lleras-Muney, 2009; Khanna, 2020; Jensen, 2012; Oster & Steinberg, 2013; Jensen, 2010; Moeeni & Tanaka, 2023). When considering changes to future school access in particular, there are many reasons investments might respond, including option value (Stange, 2012), continuation value (Heckman et al., 2018), dynamic complementarities (Foster & Gehrke, 2017), and cumulative returns (Fabregas & Navarro-Sola, 2024). Anticipatory effects of college access have been studied among secondary students, yielding positive estimates in the US and Colombia (Akhtari et al., 2024; Cortes & Zhang, 2011; Golightly, 2019; Laajaj et al., 2022) but null or negative effects in Brazil and Chile (Estevan et al., 2019; Tincani et al., 2023). My study shows that positive anticipatory effects can exist even for primary age children.[4]

Finally, this paper informs the active policy debate over the provision of free secondary schooling in developing countries. Free primary education is nearly universal, but 32% of African children live in countries which require fees for attending public secondary school (vs. 5% in the rest of the world).[5] FSE is expensive, and opponents sometimes argue that it constitutes a regressive transfer to the relatively wealthy who are more likely to send their children to secondary school (Doe-Glah, 2017; O'Malley, 2015).[6] Another worry is that overcrowding could reduce quality (Bold et al., 2015; Garlick, 2019). This concern is not unfounded in Tanzania: Valente (2019) found that the country's introduction of Free Primary Education (FPE) lowered teacher-pupil ratios, teacher quality, and urban test scores, and Hoogeveen

[4] A related literature shows that construction of higher education institutions affects primary enrollment and attendance in India, though part of the effect comes through crowding in supply of primary schools (Mukhopadhyay & Sahoo, 2016; Jagnani & Khanna, 2020).

[5] WORLD Policy Analysis Center (2024); World Bank (2022); figures based on population under 20. This number refers to *de jure* free secondary education; in practice, even many countries with nominally free secondary education still charge fees.

[6] Some commentators make the argument that FSE will reduce long-run GDP per capita through children's foregone wages (Fujimoto et al., 2023).

& Rossi (2013) found that it reduced grade achievement for poor and rural students.[7] In general, the FPE expansions of recent decades have not yielded large learning gains.[8] As such, some argue that raising primary quality (or pre-primary access) should precede secondary access (Glewwe & Kremer, 2006; Pritchett & Beatty, 2015; Attanasio et al., 2020; Heckman, 2006). However, existing studies of FSE expansions find evidence of increased enrollment (Borkum, 2012), test scores (Blimpo et al., 2019), labor market outcomes (Brudevold-Newman, 2021), and even educational attainment in the next generation (Agüero & Ramachandran, 2020). Supporters of FSE argue that expanding secondary access should be a priority, in part because it is politically feasible and has been shown to be technically implementable at scale (Crawfurd, 2024). My paper shows that cost-benefit analyses of these programs must also consider the crowding-in of demand-side investments which raise performance among primary students (complementing Ferreira & Sandholtz (2024)'s evidence that FSE can help equalize investments across children within households).

Section 2 of this paper provides context and background on FSE. Section 3 outlines the various data sources used. Section 4 presents descriptive evidence that FSE increased secondary school access. Section 5 outlines the diff-in-diff strategy for identifying the effect of FSE on primary student outcomes, with microdata on fee payments providing variation in treatment exposure. The results are in Section 6, with mechanisms explored in Section 7. Section 8 concludes.

2 Context

The context of Tanzania's Free Secondary Education (FSE) reform is one in which primary enrollment is high, but secondary enrollment lags behind. Public primary schools have been fee-free since 2001 (Valente, 2019), and primary net enrollment in Tanzania was 81% as of 2018 (World Bank, 2021). Public schools account for 97% of primary enrollment.[9] Primary schooling in Tanzania begins officially at age seven, and consists of seven years (Standards 1-7). Most students start primary school on time: in the (pre-reform) 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 A.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

[7] Brandt & Mkenda (2020), using an empirical strategy based on district-level dropout rates as a measure of FSE exposure, argue that FSE in Tanzania also reduced test scores.

[8] See e.g. Kremer et al. (2013); Bold et al. (2017); Pritchett (2013); Kadzamira & Rose (2003); Al-Samarrai & Zaman (2007); Deininger (2003); Lucas & Mbiti (2012).

[9] As proxied by the number of grade 7 students sitting the PSLE in public vs. private schools.

is a high-stakes national standardized test; passing it is a requirement for being admitted to a public secondary school.[10] Nearly all students who make it through primary school sit the PSLE.[11]

Participation in secondary school is much lower: as of 2018, secondary net enrollment stood at 26% (World Bank, 2021). Lower secondary school consists of 4 years (Forms 1-4), with upper secondary constituting an additional two years (Forms 5-6). Tanzania's 2016 FSE policy made public schooling fee free only through Form 4, the end of lower secondary. The private sector is still small, accounting for 13% of enrollment.[12] 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). Like the PSLE, it is high-stakes: it must be passed in order to progress to Form 3. The academic year begins in January, with the PSLE administered in September and the FTNA in November.

Prior to the FSE reform, official school fees for government secondary schools were about 20,000 Tanzanian shillings (TZS) per pupil per year (about $25 USD PPP).[13] But other compulsory contributions — for services such as building maintenance and security, and supplies such as desks and exams — could rise to the order 300,000 TZS ($372 USD PPP) – over 15% of GDP per capita in 2015 ($2294 USD PPP) (Taylor, 2016; World Bank, 2021).[14] For many households, this would have been a significant expenditure, especially in light of research on the difficulty of saving and the importance of household financial constraints on educational investments in Tanzania (Sandholtz, Carroll, et al., 2024; Burchardi et al., 2024). These fees likely played a role in the wide disparity between net enrollments rates in primary and secondary school. Government investment in lower secondary school, as of 2018, was TZS 615,000 per student ($827 USD PPP – Asim et al. (2019)).

However, returns to secondary education in Tanzania appear to be high. 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 $65 USD PPP, compared to an average of $11 USD PPP for those who had not finished secondary school (see Figure A.1b).[15] Montenegro & Patrinos (2014) estimate the Mincer returns to each year of secondary education in Tanzania at 15%, as of 2011. Perhaps unsurprisingly then, it has been claimed that universal primary

[10] Private secondary schools are not bound to require a passing grade on the PSLE, but many do so, especially the most selective.
[11] 97%, according to the National Panel Survey 2014-15 (**?**).
[12] As proxied by the number of students sitting the FTNA in public vs. private schools.
[13] Using World Bank's LCU per international $ PPP conversion factor of 806.45 for Tanzania in 2015 – World Bank (2022).
[14] See Habyarimana et al. (2020) on how Tanzania's government paid for public goods including education by requiring contributions from local resident users.
[15] Figures from the nationally representative Tanzania National Panel Survey 2014-2015; $N = 2498$.

education in Tanzania increased demand for secondary education (Opalo, 2022).

2.1 The policy: Free Secondary Education (FSE)

In December of 2015 – after that year's final exams, and one month before the start of the 2016 school year – newly-elected president John Magufuli announced that all basic education through the fourth year of secondary school would be free, effective immediately. Notably, the announcement stipulated 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. As officially articulated 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" (United Republic of Tanzania, 2015). In his speech announcing the policy, Magufuli emphasized the comprehensive nature of the fee abolition, underscoring his commitment with 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)

To help pay for the policy, the education budget was increased by 22%, from 3.9 trillion to 4.77 trillion TZS – an increase of 870 billion TZS (≈ $1.1 billion USD PPP) (Twaweza East Africa, 2016b). Some of these funds appear to have been used to increase the disbursement of capitation grants — officially set at 10,000 TZS for primary students and 25,000 TZS for secondary students — which had been required by law since 2004 but rarely fully delivered in practice (Shukia, 2020; James, 2023; Ministry of Education, Science and Technology, Tanzania, 2004).

Despite the rhetoric, it was not obvious that the program would increase school access: Crawfurd & Ali (2022) find that declarations of fee-free education, while common, have little impact unless accompanied by money to replace the foregone revenue. Even if the policy did raise access, a subsequent worry was that the program would lead to overcrowding in schools. Indeed, various studies find evidence that

the government's investments did not keep up with the large influx in students (Brandt & Mkenda, 2020; Lazaro & Matiku, 2022). At the same time FSE was increasing demand for secondary schools, the government decided to expel foreigners working without proper permits, including thousands of teachers from neighboring Kenya, further exacerbating worries of a supply crunch (The Economist, 2016).

However, existing evidence suggests 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; and 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 improved 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

This section outlines the data sources used in the analysis. The primary analyses draw on administrative exam data from the National Examinations Council of Tanzania (NECTA). I also use data from the President's Office, Regional Administration and Local Government Tanzania (PO-RALG); local election data from the national gazette; and the Tanzania Living Standards and Measurement Survey. I outline the role each dataset plays in my analyses.

3.1 PO-RALG data

The President's Office, Regional and Local Government (PO-RALG or TAMISEMI in Swahili) provides data on key public services in Tanzania, including education. It hosts the Ministry of Education's Basic Educational Statistics in Tanzania (BEST) series of reports, which provide nationwide aggregate educational data going back many years. These aggregate data can be used to show broad trends in enrollments and test takers before and after the introduction of FSE.

PO-RALG is also the source for geographic information on primary and secondary schools which makes it possible to geo-locate individual test-takers from the NECTA exam data within wards.[16]

[16] I use shapefiles for ward boundaries from GADM in the year XXXXXX.

3.2 NECTA exam data

My primary source of administrative microdata is the universe of high-stakes exam scores from mainland Tanzania. The agency responsible for administering these tests is the National Examinations Council of Tanzania (NECTA).[17] None of these exams are graded by students' own teachers. Instead, all exam papers nationwide are gathered to a central location to be graded, rendering remote the possibility of systematic manipulation in grading.

I focus primarily 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).

Primary School Leaving Examination (PSLE) — Grade 7.

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.[18] 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 (40%).

NECTA reports students' letter grade on each subject of the exam, as well as the letter grade corresponding to their overall marks. This provides a precise measure of which students passed the exam: those who achieved at least a "C" as their overall grade. However, it provides only a noisy measure of scores, both overall and for each subject. To convert these coarse categories into estimated scores, I let each grade correspond to the midpoint of its range. I.e., as students are assigned "As" for scores between 80% and 100%, 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 these proxy scores for each of their five subjects scores, then divide by five to express the score as a percentage. Section D provides evidence that this yields a good proxy for true overall scores: among a subset of students for whom precise overall scores are available, the correlation is 0.98. In the period from 2013-2019, between 867,000 and 958,000 pupils sat the PSLE each

[17] Zanzibar's examinations data are administered separately.
[18] Figure E.1 shows some example questions from the English and Math portions of the PSLE.

year.

Form Two National Assessment (FTNA) — Grade 9.

This is the first major test students take in secondary school. It is taken at the end of a student's second year of lower secondary school (grade 9). It is also high-stakes: a pass is required to continue on to the third year of secondary school. Students typically sit the exam for 9 subjects, though some sit more. As with the PSLE, NECTA data provides only letter grades for each subject, but the overall aggregate score in points is available, as well as a precise measure of passing. The number of students sitting the FTNA varied between 448,000 (2014) and 652,000 (2021).

Upper secondary school exams: O-level (Grade 11) and A-level (Grade 13).

At the end of Form 4 (grade 11) — the last year of lower secondary education — students sit the Certificate of Secondary Education Examination (CSEE), similar to the "O-level" in the traditional British system. This determines eligibility for upper secondary school. Students who continue through the sixth year of secondary school (i.e., the second year of upper secondary) take the Advanced Certificate of Secondary Education Examination (ACSEE) — similar to the "A-level" — which determines eligibility for college. Unlike the PSLE and FTNA exams, NECTA's CSEE and ACSEE data do not report student names, making it impossible to link them to earlier exams. I use the CSEE data as a proxy for secondary school enrollments in an analysis aggregated to the ward level.

3.2.1 Measures constructed using NECTA data

Transition to secondary school. 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 report direct measures of secondary transition at the individual level, so I use a student's participation in the FTNA exam as a proxy. In the period I study, NECTA did not assign unique student identifiers which allowed linking students across tests, so instead I link students using their names. Of the 6 million primary school pupils who sat the PSLE from 2013 to 2019, 98% have a name (typically "first middle last") which uniquely identifies them within their cohort nationwide. 98% of FTNA takers also have unique names. I match the names of PSLE students in a given cohort to the names of FTNA takers from two years later (the same cohort of students, assuming normal grade progression). This measure likely undercounts the true number of transitioners for at least three reasons: 1) it misses pupils who skip ahead or fall behind the normal

grade progression schedule; 2) it misses pupils who transition to secondary school but drop out sometime in the first two years before taking the FTNA; and 3) it misses pupils whose name is recorded differently between PSLE and FTNA, including misspellings. Section D provides more details on the construction of this measure, and suggests that dropouts and misspellings account for a greater source of measurement error than delayed grade progression. 70% of (uniquely-named) FTNA takers in this period have a name which matches precisely to the name of a PSLE taker from 2 years earlier.[19]

Geolocation of schools. I geolocate schools (and by extension students) by matching school identifiers in the NECTA data to coordinates of school locations provided by PO-RALG. I then locate these school coordinates within wards using GIS shapefiles available from the United Nations Office for the Coordination of Humanitarian Affairs (United Nations Office for the Coordination of Humanitarian Affairs, 2018). 98.8% of PSLE takers and 99.8% of FTNA takers are able to mapped to a ward in this way; the small fraction of other students, whose school identifiers did not match any school in the PO-RALG list, were dropped from the analysis.

Elites. Finally, I create a measure of which PSLE takers are more likely to be relatives of local political candidates. I label these "elites:" they are likelier than average citizens to wield local political power, as well as to be relatively wealthy given the costs required by running for office (Babeiya, 2011). I draw on ward-level election results for local council members (*diwani*) from the 2015 election, published in the national gazette (*Gazeti la Serikali la Tanzania*, 2016). I match the last names of PSLE takers to the last names of political candidates in the same ward; those students which share a last name with a candidate in their ward I designate as "elites." Of the 2,987 wards with mappable electoral results, 1,906 (64%) contain at least one PSLE taker with the same last name as at least one ward council election candidate.

3.3 Household survey data

I supplement the administrative data above with household survey data from the Tanzania National Panel Survey / Living Standards Measurement Survey (World Bank, 2014–2020). This survey is nationally representative, and includes (at the time of writing) five waves stretching from 2008 to 2021. The first three waves were a panel; waves 4 and 5 were a panel from a new "refreshed" nationally representative sample.

[19] See also Blackmon (2017) and Sandholtz, Gibson, & Crawfurd (2024), which use a similar same string matching procedure in order to measure school value-added, and Ferreira & Sandholtz (2024), who do so to measure intra-household allocations of human capital investments.

I use the survey as a repeated cross-section. To match as closely as possible the period for which I have administrative data, I focus on Waves 2, 3, 4, and 5, which were conducted in 2010-11, 2012-13, 2014-15, and 2020-21, respectively.

The NPS includes a number of questions relevant for assessing households' investments in primary education. These include measures of the child's enrollment, attendance, attainment, and labor market participation; test taking and score received; time spent studying; and money spent on education.

4 FSE increased secondary school access: descriptive evidence

To test how primary students' investments responded to secondary school access, it is first necessary to demonstrate that the FSE policy did indeed increase access to public secondary schooling. 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. Through the Free Primary Education program, the government had promised schools capitation grant transfers which faced perennial complaints for failing to materialize on time or in full (Twaweza East Africa, 2013; Manara & Mwombela, 2012). 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). Other countries in the region have struggled to make promises of free secondary education into reality, and *de jure* free secondary is much more common than *de facto* access (Crawfurd & Ali, 2022).

Preliminary studies left room for doubt as to whether implementation of the 2016 FSE policy would meaningfully improve school access. Case studies of its implementation in various parts of the country found the capitation grants provided to be unreliable and/or insufficient to meet the needs of the large influx of students (Lazaro & Matiku, 2022; Majumba, 2019; James, 2023; Munisi et al., 2018). That said, there is also evidence of optimism among ordinary Tanzanians about FSE after its announcement. As mentioned previously, a contemporaneous household survey showed widespread belief that the government would make good on its promise to abolish fees (Twaweza East Africa, 2016a). A qualitative report stated that, despite causing confusion among key implementers, "there is no doubt that the implementation of the fee-free education policy has significantly promoted access to basic education for children from various socio-economic backgrounds" (Shukia, 2020).

This section presents three pieces of descriptive evidence suggesting that that FSE succeeded in in-

creasing access to secondary school.[20] First I show using household survey data that public secondary school fee expenditures fell sharply after the policy's implementation. Next I show that the policy coincided with an abrupt end to the advantage in primary-to-secondary transition rates previously enjoyed by local elites. Finally I show using administrative data that the policy coincided with a sudden rise in secondary enrollments.

4.1 School fee expenditures fell for students in public secondary schools

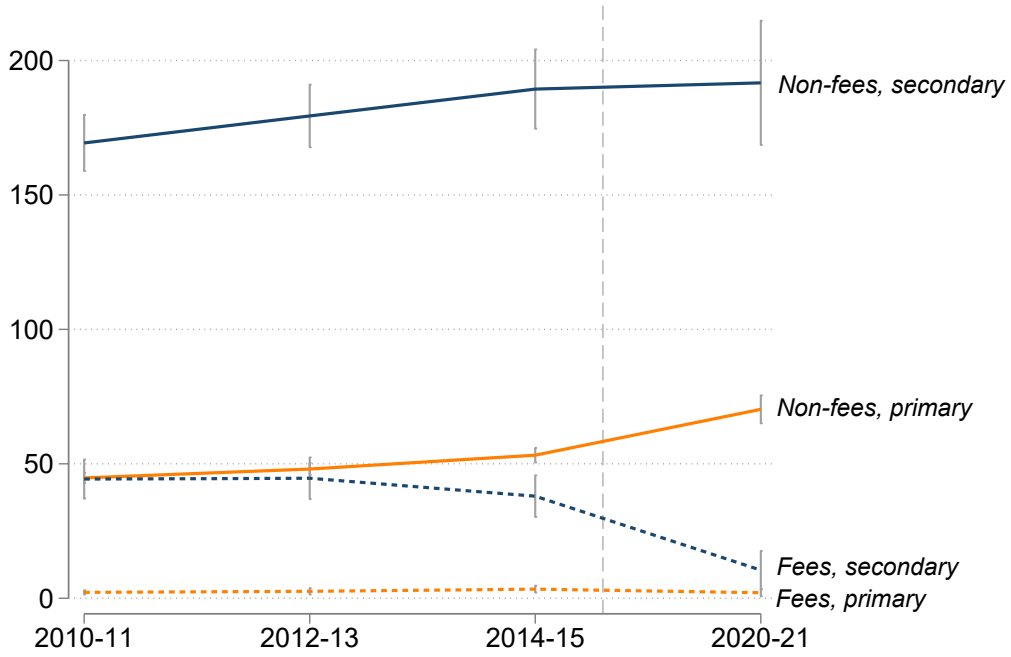
I draw on National Panel Survey data to show that in the wake of FSE's implementation, household spending on school fees fell markedly for students in public secondary schools. Figure 1 shows real average annual school expenditures over time, separately for students in public primary and secondary schools, and separately for fee expenditures and non-fee expenditures. Non-fee school expenditures include books & materials, uniforms, transportation, extra tuition, meals, and other contributions. The data come from three waves of the survey conducted before FSE and one conducted afterward.

These descriptive data suggest that FSE meaningfully reduced public secondary school fees in practice, not just on paper. Secondary students' expenditure on school fees was 44 USD PPP in the 2010-11 survey wave, and did not change significantly in the following two pre-FSE survey waves. In the first wave post-FSE, it plummeted by over 70% to 10 USD PPP. Meanwhile, non-fee secondary school expenditure rose over the first three survey waves, but then held constant over the period of FSE's implementation. This suggests that official school fees were not simply relabeled as other "voluntary" contributions in response to FSE.

These data also suggest increased primary student investments following the implementation of FSE. The reasonably faithful implementation of the existing Free Primary Education policy, first implemented in 2001, is evidenced in the fact that primary students' expenditures on fees remained constant and low throughout the period, hovering between 2 and 3.5 USD PPP, with no significant changes across waves. Over the same period, however, non-fee spending for primary school students rose, especially in the post-FSE wave — consistent with a crowd-in of primary student investment.

[20] Section 6.1 will identify causal evidence.

Figure 1: Descriptive: Annual household fee and non-fee school spending per student, real USD



Average household expenditure per student on schooling, among public school students in primary and secondary school. Data from Tanzania National Panel Survey, Waves 2-5. N = 14463. Vertical line represents implentation of FSE. Real values obtained by applying PPP conversion figures from WDI to original nominal values in TSH (World Bank, 2022).

4.2 The elite premium in secondary transition disappeared

Another index of school access is how regular people fare in the system relative to elites (Lucas & Mbiti, 2014). As outlined in Section 3, my measure of "elite" students is those who share a last name with a 2015 local council electoral candidate in their ward. Prior to FSE, 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 non-elite transition mean in 2015 was 27%).

To measure how the elite premium changed over time, I use Equation 1:

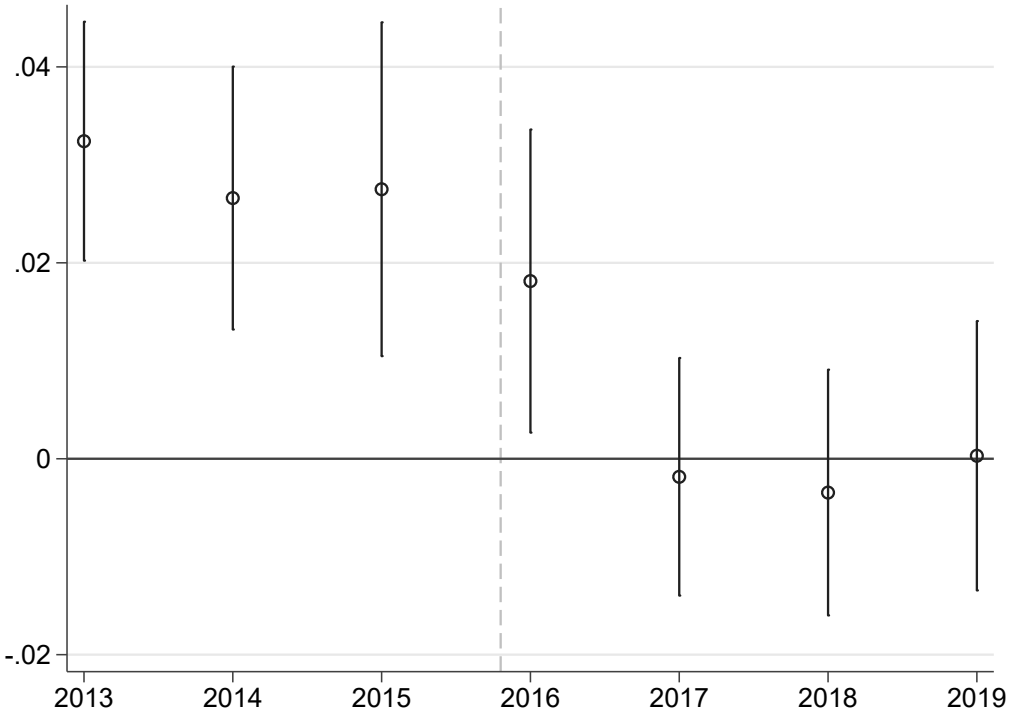
$$Y_{ifswt} = \alpha + \phi_{sw} + \lambda_t + \delta\, elite_f + \sum_{t=2013}^{2019} \beta_t\, elite_f \times \lambda_t + \varepsilon_{ifswt} \quad (1)$$

Y_{ifgwt} is a dummy for the secondary transition status of student i from family f who earned overall PSLE score s in ward w in year t. ϕ_{sw} are PSLE score $\times$ ward fixed effects. λ_t is a year fixed effect.

$elite_f$ indicates that the student comes from an elite family. The β_t coefficients measure the difference in secondary transition between elites and non-elites by year — *conditional on PSLE scores* — relative to the base year of 2015. To illustrate how the absolute difference between the two groups changed over time, I plot the linear combination of $\delta + \beta_t$ (for the omitted year 2015, I plot only δ). Standard errors are clustered at the ward level.

Figure 2 shows that after the introduction of FSE, the elite premium in secondary transition fell to zero. This is consistent with FSE reducing financial barriers to secondary school for ordinary people and widening access.

Figure 2: Descriptive: Elite transition premium over time

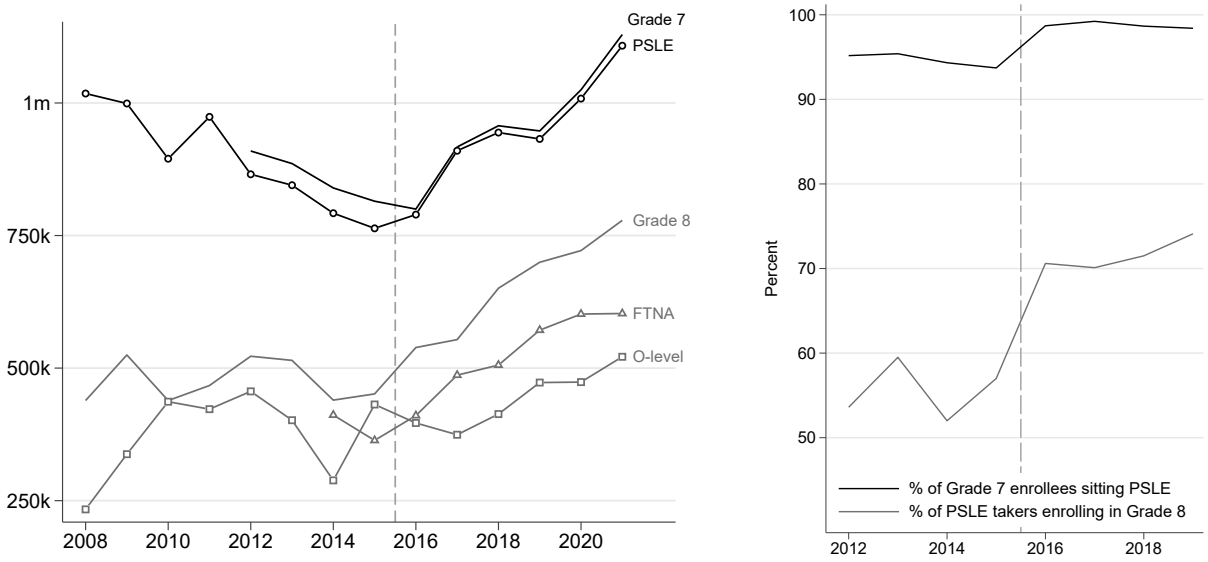


Point estimates and 95% confidence intervals represent linear combination of main effect + interaction effect from Equation 1's regression of secondary transition on the interaction of "elite" dummy with year indicators, including year and PSLE-score-by-ward fixed effects. As 2015 is the omitted year 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.

4.3 Secondary enrollments, primary enrollments, and PSLE taking increased

Increased enrollments following the adoption of FSE also suggest that secondary school access increased — and further suggest that primary school students began investing more. Figure 3 shows several aggregate raw time series for all of mainland Tanzania: students enrolled in Grade 7 (the last grade of primary school); students enrolled in Grade 8 (the first grade of secondary school); and the number of students taking the PSLE (Grade 7), FTNA (Grade 9), and O-level (Grade 11). FTNA and O-level data come from aggregating NECTA's individual-level data; all other data come from PO-RALG and are available only at the aggregate level.

Figure 3: Raw data on enrollments and test-taking



(a) Levels

(b) Shares

Panel (a): Aggregate nationwide trends in Grade 7 enrollment, PSLE test takers, Grade 8 enrollment, FTNA test takers, and O-level test takers. Panel (b): Share of Grade 7 enrollees who take the PSLE and share of PSLE takers who enroll in Grade 8. Vertical line represents implementation of FSE. Data come from PO-RALG.

The figure shows that secondary enrollments and test-taking increased after FSE. The gray lines at the bottom of Panel (a) in 3 shows that participation of students in secondary school began to rise sharply after FSE's implementation. The solid gray line shows that enrollments in Grade 8, the first year of secondary school, stayed relatively constant before FSE, then began to rise sharply immediately thereafter. The triangle series shows that the number of students taking the FTNA in Grade 9 began to rise one year later. The square series shows that the number of students taking O-level exams in Grade 11 likewise

began to rise two years after that. The gray line at the bottom of Panel (b) further shows that Grade 8 enrollees as a share of PSLE takers jumped sharply upon FSE's implementation as well. This suggests that various measures of secondary school access increased upon FSE's implementation (and also suggests that the transition from primary to secondary school was a more significant margin than continuation within secondary school).

The figure also shows that primary enrollments and test-taking increased after FSE. The gray lines at the top of Panel (a) in Figure 3 shows that the number of students enrolled in Grade 7 (the last grade of primary school) also began to rise after FSE, reversing a previous downward trend. (Grade-level enrollment data are available beginning only in 2012.) As discussed in Section 2, primary enrollment in Tanzania is high but not universal. Even in the absence of official school fees, there are costs associated with sending a child to school, not least the opportunity cost (as will be explored further in Section 7.2). The pattern of increased Grade 7 enrollments after FSE is consistent with an increase in the number of primary school students who choose to continue investing in education rather than drop out. The circle series shows that the number of students taking the PSLE saw a more-than-commensurate jump after FSE. Panel (b) makes this pattern more visible: while the fraction of Grade 7 students taking the PSLE was over 90% as far back as 2012, after FSE this share suddenly increased to nearly 100%. Sitting the PSLE represents another dimension of human capital investment. Like enrollment, it entails an opportunity cost, but it also has significant option value by unlocking future education. Passing the PSLE is a requirement for entering public secondary school. The increase in the fraction of students who sit the exam is consistent with the idea that FSE moved some households on the margin to make forward-looking investments in primary education.[21] This constitutes further *prima facie* evidence that FSE led primary students to increase educational investments.

5 Identification strategy

While the aggregate patterns shown above are consistent with FSE affecting primary student investments, they are not dispositive. Identifying the causal effect of Tanzania's FSE policy is challenging because it came into effect for the entire country simultaneously. There was no staggered rollout nor randomized

[21] Cilliers et al. (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 the years prior to the introduction of FSE. In Table B.4 I examine and reject the hypothesis that changes to the composition of who sat the PSLE are responsible for my main results.

trial. Who can be considered "treated" by the policy, and who should be considered as a valid comparison group?

To measure the policy's causal impact, I use administrative microdata on ex ante school fee payments as a measure of exposure to FSE. FSE abolished secondary school fees; students from families who had tried and failed to pay secondary school fees were those most directly treated, while students from families in similar circumstances who succeeded in paying fees were less directly affected, and therefore constitute a natural control group. I compare outcomes for primary students in treated vs. control families, before vs. after FSE.

This strategy requires that I can define treatment exposure using data on secondary school fee payments, and that I have a method for assigning a treatment status to primary students. Here I describe the data which allow me to measure treatment exposure, and present two complementary ways of using it to define primary students' treatment exposure.

Payment of secondary school fees. For the cohort of FTNA takers from 2014, available data show which students had their results withheld for failing to pay exam fees. 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 exam results were later made available for most students, implying that they paid their fees eventually. But the initial failure to pay functions as a measure of sensitivity to school fees which is useful in identifying the families most directly affected by FSE's fee abolition. This measure does not capture all the ways in which families could be financially constrained by school fees. Many families would have struggled to pay but nevertheless scraped together the funds on time; many other families would likely have never been able to pay to send a student to secondary school in the first place. However, this measure does identify families for whom secondary school fees are a *binding* constraint: they had overcome all the other obstacles to enrolling a child in secondary school, but their ability to keep up with the fees was marginal.

FTNA takers who struggle to pay school fees do not appear to be worse students than their on-time-paying peers. In fact, their pass rates for the 2014 FTNA were about 4pp higher than those who paid the fee on time (whether or not ward fixed effects are included). This suggests that fee non-payment is not driven by parents strategically cutting their investments in unpromising students, but rather by financial constraints *per se*.

5.1 Difference-in-differences: Ward-level variation

I first define treatment at the ward level, using an indicator variable for whether any 2014 FTNA taker failed to pay fees on time. This geographic measure of treatment exposure shares the intuition of Lucas & Mbiti (2012)'s strategy for measuring the effect of Free Primary Education in Kenya. Figure 4 presents a map of these wards. Orange-shaded wards contained at least one FTNA taker who failed to pay fees on time in 2014; in green-shaded wards all students paid their fees on time. Gray-shaded wards contained no secondary schools in 2014 on which to base this measure. Of the wards which had secondary schools in 2014, 1,473 (58%) were treated by this measure, while the other 1,063 saw no non-payment of fees.

I use a difference-in-differences framework to compare outcomes in treated vs. non-treated wards before vs. after the implementation of FSE.

$$Y_{wt} = \alpha + \delta\, treat_w \times post_t + \lambda_t + \eta_w + \varepsilon_{wt} \tag{2}$$

In Equation 2, Y_{wt} designates the outcome for ward w in year t. The variable $treat_w$ indicates that ward w had non-zero FTNA fee nonpayment in 2014. The indicator $post_t$ signifies the implementation period (post-2015). The variables λ_t and η_w indicate year and ward fixed effects, respectively.

In the next subsection I will outline an alternate treatment exposure definition at the level of the family. The benefit of the ward-level definition is that it provides an intent-to-treat measure at the community level, capturing spillovers that the family-level comparison nets out by design.

5.2 Difference-in-differences: Family-level variation

As an alternate treatment definition, I use fee payment data, student names, and geography to define treatment exposure at the family level. This permits a much more granular measure of treatment exposure, but also limits the sample. Here I describe my method for grouping families and assigning the primary school students in them to a treatment status.

Families. I group students into "families" – unique combinations of last name × ward.[22] In Tanzania, children typically take their father's last name, meaning students sharing a last name have an increased likelihood of being siblings. Wards (Swahili: *kata*) 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

[22] See Cruz et al. (2017) for another example of family grouping based on names.

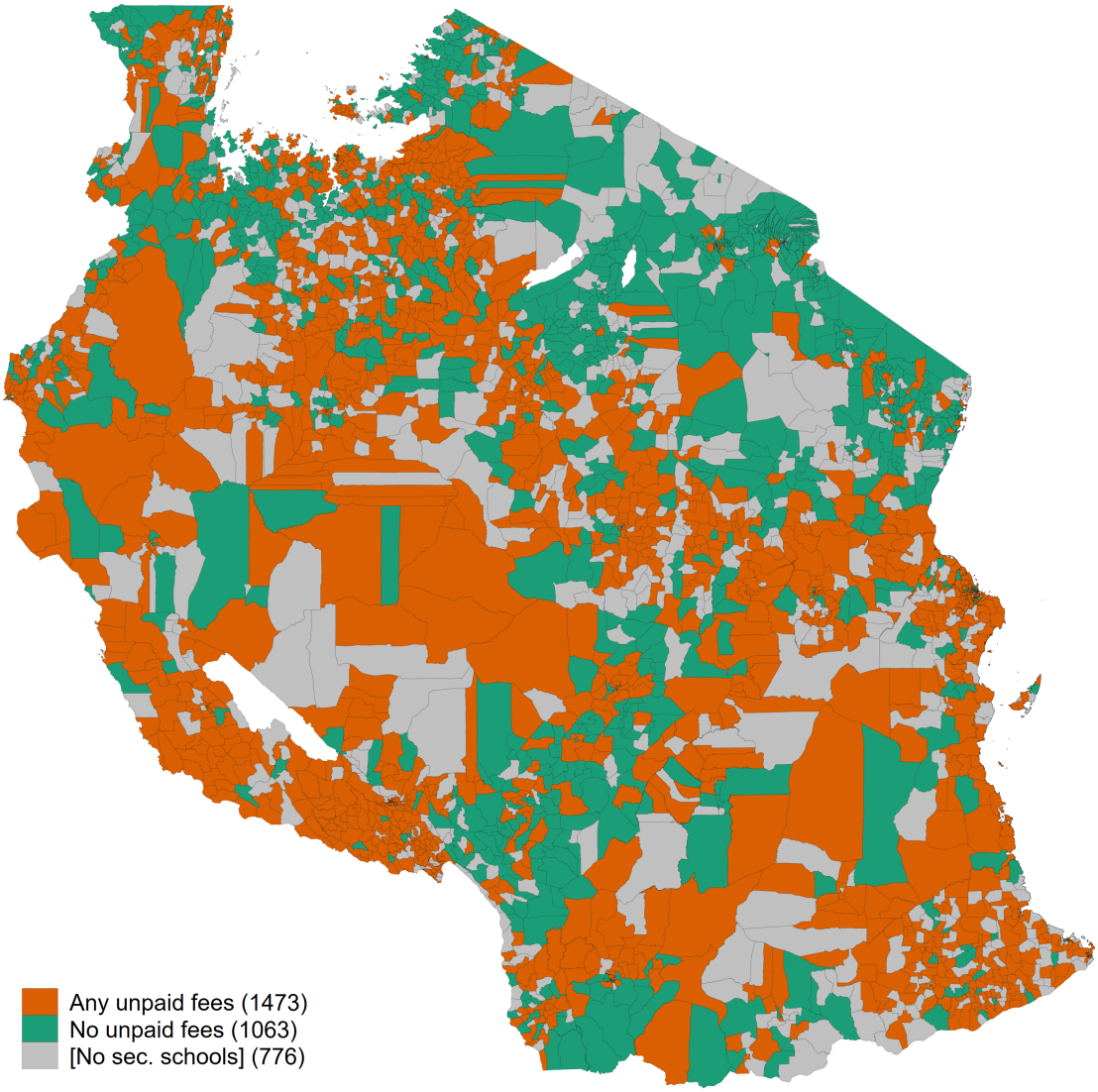


Figure 4: Ex ante payment of secondary school fees, by ward

Wards classified by binary measure of whether any 2014 Grade 9 (FTNA) exam taker had exam scores initially withheld for failure to pay fees. Map shows only wards in mainland Tanzania; the autonomous region of Zanzibar is excluded.

the period I study. The average family group thus created contains 2.72 PSLE takers. This is plausible: the average household in the contemporaneous and nationally representative Tanzania National Panel Survey contains 2.5 children. Tanzania's Total Fertility Rate in 2000-2006 — the years in which 13-year-old takers of the 2013-2019 PSLE would have been born — hovered around 5.6, so large families should not be considered especially surprising (World Bank, 2021). As a robustness analysis testing the sensitivity of the main analysis to the possible presence of false positive family matches, Table B.5 limits attention to

PSLE takers whose last name is unique within their ward × cohort, and yields similar results to the main analysis.

Defining treatment for primary school students. As my interest is in primary student outcomes, it is necessary to link them to this family-level measure of fee constraints. I define "families" among 2014 FTNA takers in the same way I did for grade 7 students: unique combinations of last name × ward. I limit attention to families with a clear treatment definition – that is, families in which *all* FTNA takers either did or did not pay their exam fees. This describes 92% of the students, 63% of whom have a unique last name in their ward×cohort. I then match up these FTNA takers with their PSLE-taking counterparts. Although 2013 is the earliest year for which PSLE data are available, I focus on the years from 2014 onward to avoid potential endogeneity between primary student outcomes in 2013 and the fee constraint measure defined using secondary student data from 2014. The result of this exercise is a set of 2014-2019 PSLE takers who share a last name with a 2014 FTNA taker in the same ward, and therefore can be defined as belonging to a fee-constrained family or not. This set of over a million PSLE takers makes up 21% of the total.

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. Primary students from families who failed to pay fees on time performed significantly worse *ex ante* than those from fee-paying families: on average their PSLE scores are 0.18 standard deviations lower, they are 8.4 percentage points less likely to pass, and 9.8 percentage points less likely to transition to secondary school.

Summary statistics

Table 1 displays summary statistics on NECTA data for primary students. Column 1 summarizes variables for the full universe of PSLE takers 2014-2019. Column 2 summarizes variables for the family-level analysis sample: PSLE takers in years 2014-2019, whose school can be located in a ward, and who share a last name with a 2014 FTNA taker in the same ward.

Although the family fee constraints treatment variable is not defined for *all* PSLE takers, those who are classifiable look very similar to the overall. PSLE pass rates are around 70% on average, with transition rates close to 50%. There is significant variation in exam performance by subject: students tend to score lower on English and Math, while scoring significantly higher on Science and especially Swahili.

To measure the policy's causal effect on primary students, I use a difference-in-differences design which measures how the gap between students from fee-constrained vs non-constrained families changed after FSE.

Equation 3 articulates a dynamic difference-in-differences framework which measures these differences year-by-year. Equation 4 shows a standard difference-in-differences estimation comparing the before- and after-FSE periods:

$$Y_{itf} = \alpha + \eta_f + \lambda_t + \sum_t \beta_t \, Treat_f \times \lambda_t + \varepsilon_{itf} \tag{3}$$

$$Y_{itf} = \alpha + \eta_f + \lambda_t + \beta \, Treat_f \times Post_t + \varepsilon_{itf} \tag{4}$$

Y_{itf} is the outcome for PSLE-taker i in year t, from family f, whose sibling took the FTNA in 2014.[23] λ_t are year fixed effects. η_f are family (last name $\times$ ward) fixed effects, which absorb all variation at the family level or higher, such that the estimate is identified off of within-family differences over time. $Treat_f$ 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 in Equation 3, and with a dummy for whether the year is 2016 or later in the Equation 4. The β coefficients on the interaction terms are the coefficients of interest.

6 Results: FSE raised primary students' investments and achievement

Section 4 provided descriptive evidence that FSE increased access to secondary school; here I use the ward-level identification strategy to show that FSE caused this increase. Section 6 then presents causal evidence for FSE's effect on primary student investments and achievement.

6.1 Secondary enrollments grew most where school fees were binding ex-ante

Figure 3a showed that secondary enrollments and test-taking began to rise after FSE; here I test whether these enrollment increases were concentrated in more fee-constrained places. I use Equation 2 to compare the number of FTNA and O-level takers in treated vs. non-treated wards before vs. after the implementation of FSE.

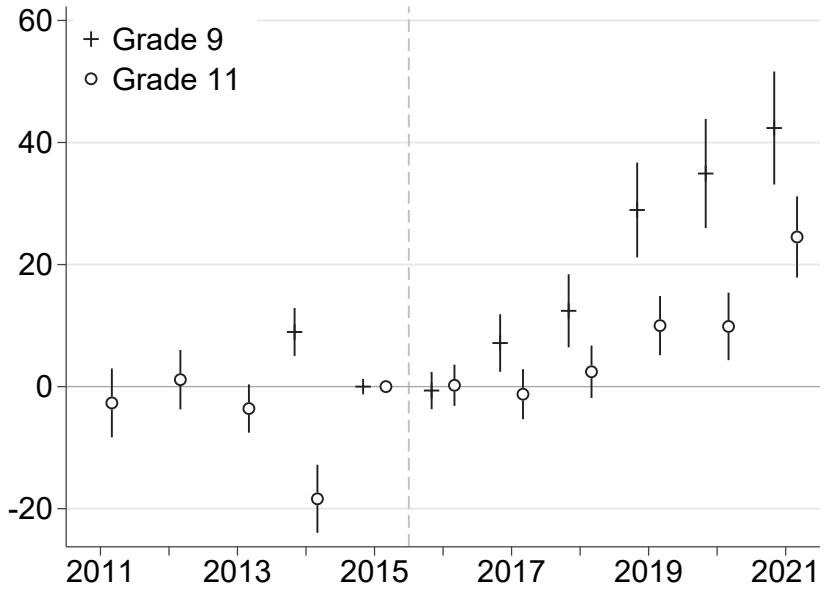
[23] I replace missing values with zero and include a dummy for missingness in the regression.

Table 1: Summary stats: Grade 7 exam takers

	All	Analysis Sample
School cohort size	83.708	84.068
	(69.952)	(71.008)
Female	0.529	0.534
	(0.499)	(0.499)
Public school	0.966	0.969
	(0.182)	(0.174)
Elite (shares last name with local political candidate)	0.006	0.013
	(0.078)	(0.114)
Pass exam	0.708	0.723
	(0.455)	(0.447)
Sec. transition (sat Grade 9 exam 2 yrs later)	0.483	0.510
	(0.500)	(0.500)
Overall exam score (%)	47.835	48.021
	(17.118)	(16.433)
English subject score (%)	41.070	41.226
	(20.648)	(20.060)
Math subject score (%)	44.105	43.934
	(22.249)	(21.609)
Social studies subject score (%)	47.601	47.805
	(18.393)	(17.835)
Science subject score (%)	49.654	50.009
	(18.864)	(18.240)
Swahili subject score (%)	56.744	57.133
	(20.982)	(20.358)
Full name unique in nationwide cohort	0.979	0.957
	(0.144)	(0.203)
Family fee payment status available	0.201	1.000
	(0.401)	(0.000)
Family failed to pay sec. school fees ('treated')		0.127
		(0.333)
N	5,200,947	1,012,446

An observation is an individual primary school 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. 'Sec. transition' defined only for students whose full name is unique within their nationwide cohort.

Figure 5: Diff-in-diff: Secondary enrollments rose most where secondary school fees were binding



Coefficients and 95% confidence intervals for interaction terms in Equation 2 with number of O-level and A-level takers as outcomes.

Figure 5 shows the results. Prior to FSE, the gap in enrollments between treated and untreated wards was more or less constant (although as with the raw enrollments data, the year 2014 is anomalous). Thereafter, treated wards see significantly greater relative growth in enrollments. As in Figure 3a, the patterns match up with what one would expect given the grade progression of affected cohorts. The positive effect on FTNA takers starts in 2017, the year in which the first fee-free cohort reached Grade 9. The positive effect on O-level takers starts in 2019, the year in which the first fee-free cohort reached Grade 11. These patterns are highly consistent with FSE expanding access to secondary school by alleviating school fees as a binding constraint.

This dramatic increase in enrollments is especially striking given that Tanzania's secondary fee abolition was not accompanied by a removal of test score requirements to enter public secondary education. This suggests that prior to FSE, a large number of students met the academic requirements necessary for secondary entry and were only held back by their inability to pay school fees.

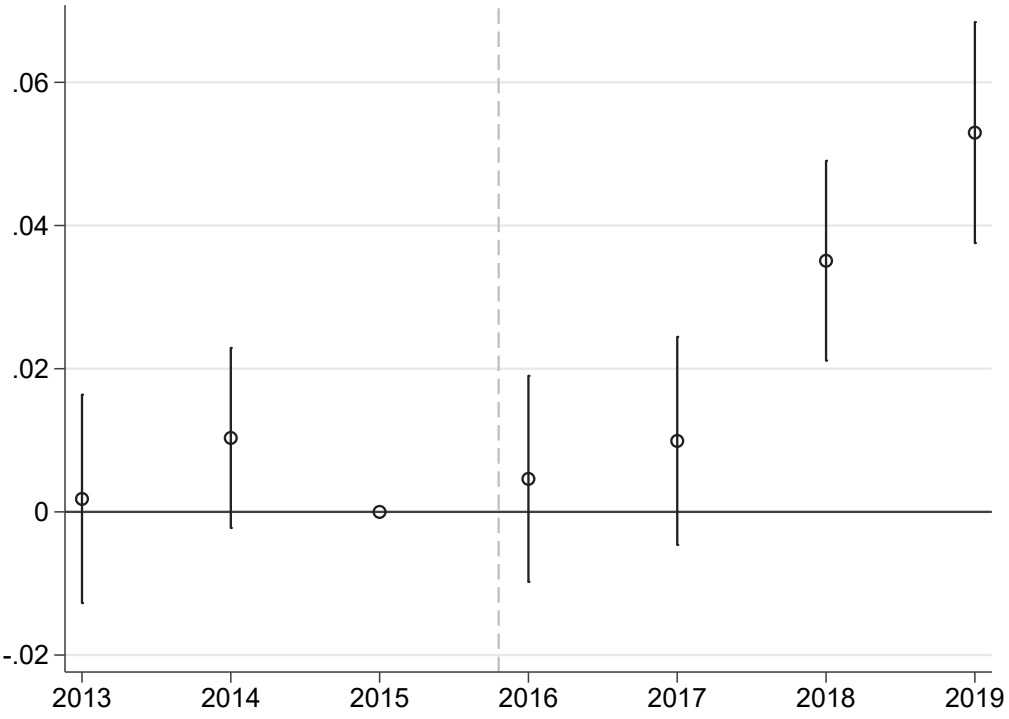
6.2 Ward-level analysis

I use ward-level variation in fee constraints to provide further evidence that the main results are not driven by supply-side investments. Equation 4's use of family-level (within-ward) variation in fee constraints rules out simple ward-level supply-side channels as potential explanations of the effects. However, it does not preclude the possibility that fee-constrained families responded differentially to potential ward-level supply-side investments — such as school construction and class sizes. Aggregate trends in supply-side investments make this look unlikely: as mentioned in Section 6.3, FSE was followed by a slowdown in primary school construction and an increase in PTR. But if these putative supply-side investments were targeted at wards with more affected families, it is possible they could help explain the increases in primary student achievement.

To test this possibility, I return to the specification outlined in Equation 2 which relies on ward-level variation in fee constraints. This permits the measurement of outcomes at a larger level of aggregation – including some outcomes which can only be measured in a more aggregated way, such as school entry and overall enrollments. It also provides outcomes which, although aggregated to the ward level, are based on a wider sample of students (because it is not limited to students for whom the family-level fee constraint measure can be defined).

Figure 6 shows that PSLE pass rates were on the same trend in treated and control wards in the three years leading up to FSE.

Figure 6: Effect of FSE on Grade 7 exam pass rates – ward level



This figure shows interaction coefficients from Equation 2. The specification shows how the difference in average outcomes between more vs. less fee-constrained wards changed over time. The figure plots the coefficients and 95% confidence intervals on the interaction between the $Treat_f$ dummy and each year dummy (where the omitted year is 2015, the last year before the policy began). For reference, the dotted line at 0.000 shows the average raw pre-reform difference in pass rates between pupils from fee-constrained vs. non-constrained families.

Table 2: Ward-level results

	Pri achievement		Pri (demand) investment		Pri (supply) investment	Sec achievement
	Pass rate PSLE	PSLE avg score (σ)	Absent rate	Num students	Num schools	Transition rate
Post × Treat	0.023***	0.033**	-0.002***	10.400	0.073	0.016***
	(0.005)	(0.013)	(0.001)	(6.927)	(0.097)	(0.005)
N	19,720	15,840	15,840	15,840	15,840	15,840
Mean (treated, pre)	0.506	-0.033	0.020	324.179	5.844	0.200

Year and ward FE. Sample consists of ward-level aggregates of student-level PSLE data from 2013-2019. Standard errors clustered by ward. * p<0.10, ** p<0.05, *** p<0.01

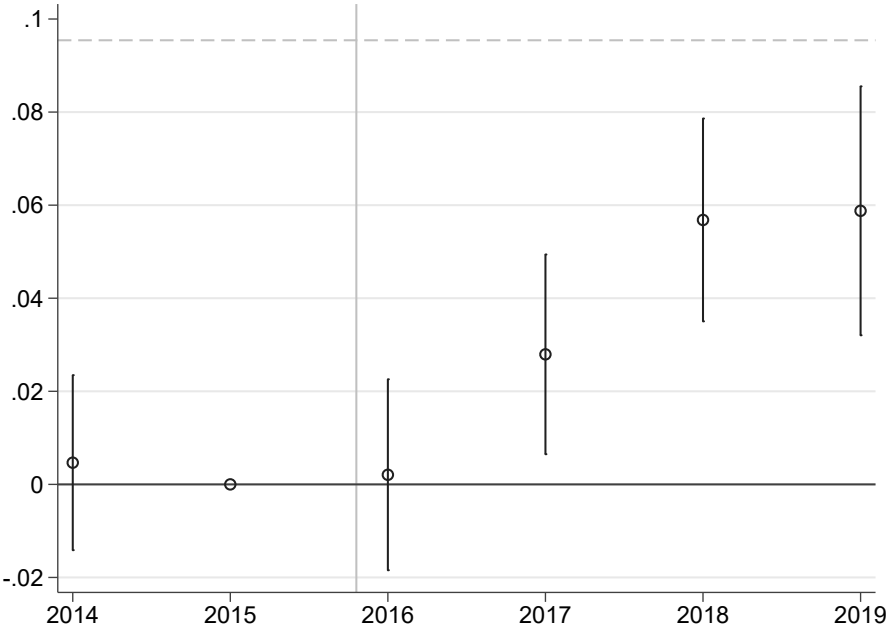
Table 2 shows evidence that supply-side investments are unlikely to explain the student achievement

effects. The results are consistent with those from the family-level analysis: FSE increased pass rates in treatment areas by 2.3 percentage points relative to control areas, and raised overall PSLE score levels by about 0.03 standard deviations. It also reduced absent rates by 0.2 percentage points (from a base of 1.4%) and increased enrollments by about 10 students (though this effect is not statistically significant). However, this increased enrollment was not accompanied by a greater number of schools per ward, perhaps implying more crowded schools. Despite this, the effect on transition rates was a positive and significant 1.6 percentage points.

6.3 Family-level analysis

Free Secondary Education raised pass rates on the Primary School Leaving Examination. Figure 7 depicts this result, plotting the interactions between the family-level fee constraints dummy and the year dummies from Equation 3. 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.

Figure 7: Effect of FSE on PSLE pass rates



This figure shows interaction coefficients from Equation 3. The specification shows how the difference in outcomes between PSLE takers from fee-constrained vs. non-constrained families changed over time. The figure plots the coefficients and 95% confidence intervals on the interaction between the $Treat_f$ dummy and each year dummy (where the omitted year is 2015, the last year before the policy began). For reference, the dotted line shows the average raw pre-reform difference in pass rates between pupils from fee-constrained vs. non-constrained families.

This change dramatically attenuated the raw gap in pass rates between students from more vs. less financially constrained families. Figure 7 includes a horizontal line for reference denoting the average pre-reform difference in pass rates of 6.0 percentage points. By 2019, this gap had narrowed considerably, to 2 percentage points.

Table 3 shows the diff-in-diff results from Equation 4. The reform increased pass rates by 3.4 percentage points — a 6% increase from a base of 55% among fee-constrained families. There was no statistically significant effect on overall test scores however (this fact will be explored in further Section 7.1). The policy did affect 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 of the test ("Absent"). Few students are absent — 2 percent of fee-constrained students in the pre-reform period — but FSE reduced absenteeism significantly with an effect of -29% (-0.3 percentage points).

Table 3: Effects of FSE on primary student investment and achievement

	Primary: achievement		Primary: investment			Secondary: 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	0.034***	0.029	-0.006***	0.027***	-1.422	0.038***	0.020**
	(0.008)	(0.020)	(0.001)	(0.006)	(1.281)	(0.012)	(0.008)
N	1,012,446	1,012,446	1,012,446	1,012,446	1,012,446	1,012,446	476,323
Mean (treated, pre)	0.554	-0.155	0.022	0.470	86.580	0.167	0.869

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

FSE also caused students to select into better schools. I proxy for school quality with the PSLE pass rate of the school at which the student sat the exam (with pass rate measured in 2013, prior to any students in the analysis taking the exam, to avoid endogeneity). The pre-reform pass rate of the primary school at which treated students sat the exam increased by 6% (2.7 percentage points from a base of 47%). Because I constrain the definition of "families" to consist of students with the same last name at schools within the same ward, this may be an underestimate of the true effect on school selection: it does not capture selection into primary schools in different wards. This finding is consistent with evidence from other contexts showing that households respond to public investments in schooling by selecting into better schools (Andrabi et al., 2024).

Perhaps the most direct effect FSE had on primary school students was to raise their likelihood of making the transition to secondary school. The transition rate — 17% for the treated group prior to the reform — increased by 23% (3.8 percentage points). There is also suggestive evidence that the policy may have affected these students' investments beyond the transition to secondary school: conditional on making the transition, FSE made students slightly more likely to pass the high-stakes FTNA exam (though these results should be interpreted cautiously given the policy's effects on transition).

How large are these effects? Evans & Yuan (2022) compile a distribution of reported effect sizes of educational interventions studied with RCTs in the developing world, measured in standard deviations. My estimate on secondary transition corresponds to an effect size of 0.11σ, placing it at the 70th percentile of the distribution of effects on enrollment. The effect I estimate on overall test scores was not statistically significant in my preferred specification (though it was in the alternate specification shown in Table B.6). However, the point estimate of 0.029σ would place it at the 30th percentile of effect sizes on reading and

math assessments.

My within-family design makes it unlikely that these results could be driven by changes to supply-side inputs such as school construction or teacher hiring which might affect class sizes. Indeed, Figure A.2 shows that FSE was followed by large *increases* in aggregate pupil:teacher ratios and a slowdown in the pace of public primary school construction. Consistent with these aggregate trends, my estimates show no significant effect on the size of the cohort in which students sat the PSLE.

These effects also do not appear to be driven by changes to the composition of students sitting the PSLE. I test this by comparing paying vs. non-paying 2014 FTNA takers' likelihood of matching by last name $\times$ ward to any PSLE taker before vs. after the reform. The reform had no effect on the likelihood of students appearing in the PSLE data (see Table B.4 in Section B).

In Section B, I test the robustness of these findings to alternative treatment definitions and estimation strategies. I consider a stricter matching method for creating family groups; an estimation which eschews family fixed effects for older sibling's school $\times$ FTNA score fixed effects; and defining treatment based on older siblings' dropout rather than fee non-payment. All results support those presented in this section.

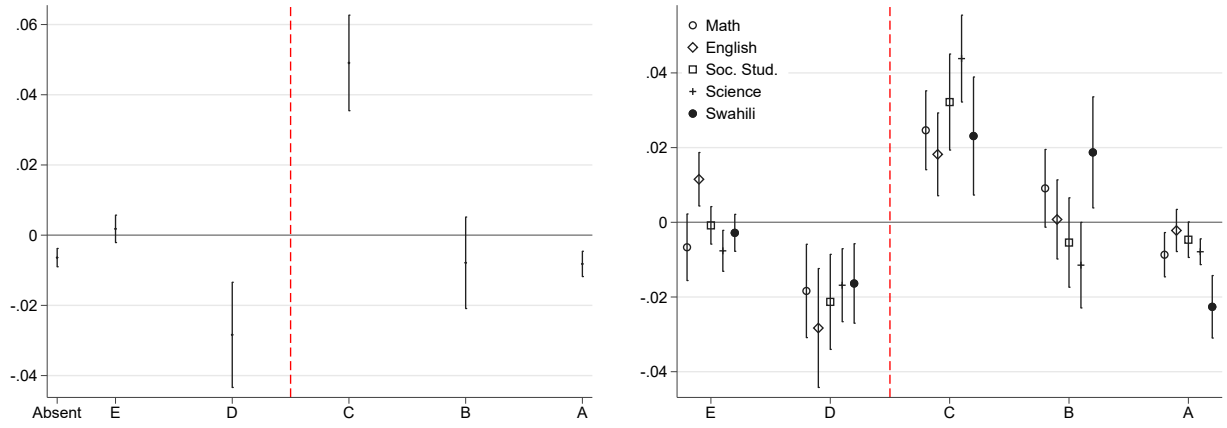
7 Mechanisms

In this Section I show two main pieces of evidence to support the hypothesis that the main effects shown in Section 6.3 are driven by demand-side investments. I first investigate patterns in effects on the distribution of test scores which are consistent with reallocated effort to maximize the probability of passing the PSLE. Then I analyze nationally representative household survey data in a difference-in-differences framework to show evidence that FSE increased households' investments in education — especially by reducing school absences and child labor. In the Appendix, I also show suggestive evidence that the effects on improved exam performance are present for low-stakes Grade 4 exams, which increases confidence that results are not driven by manipulation of exam results.

7.1 Effects on the distribution of test scores

Table 3 showed that the reform raised exam pass rates even though it had no impact on overall test scores. In Figure 8, I show that this is consistent with the reform causing students to reallocate effort, by examining the effect of the policy on the distribution of test scores.

Figure 8: Effects on grade 7 test score distributions



(a) Overall score

(b) By subject

Panel 8a shows the diff-in-diff estimates on students' likelihood of receiving an overall score in each of five bins (or of being absent for the exam). Panel 8b 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 8a 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 suggests that results are not driven by mean reversion (Ashenfelter, 1978). If the effects on pass rates merely reflected students recovering from a negative shock which temporarily reduced exam performance and ability to pay school fees, this would be expected to appear as a rightward shift in the distribution of scores and a positive effect on average overall PSLE scores. Neither is observed in the data. Instead I observe large effects around the passing threshold, indicative of an allocation of effort to maximize the likelihood of passing the PSLE to qualify for secondary school. I also observe a slight reduction in the likelihood of getting very high grades, which is also consistent with students shifting effort away from strong subjects toward weak ones.

Figure 8b shows how FSE affected students' performance 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. Science was the next "easiest" subject, with 58% of students scoring a "C" or better. By comparison, only 38% of students got at least a "C" in Math, the most-failed subject. FSE caused large reductions in "D's' and increases in "C"s for all subjects. But it also reduced scores for the subjects with the highest scores ex ante. It caused a large reduction in "A" grades

in Swahili as well as "A"s and "B"s in science. This is consistent with students reallocating study effort away from easier subjects and toward more difficult subjects in pursuit of a passing grade overall.

7.2 Household survey data: FSE's effect on primary student investments

The results from administrative data presented in Table 3 — higher pass rates, lower absence rates, selection into better schools — are consistent with the narrative that expanding secondary access increased educational investments for primary students. The distributional results in Figure 8 further suggest that the reform changed students' allocation of effort. But administrative data are limited in their capacity to speak directly to students' investments in education.

High-quality measures of household educational investments can be found in the nationally representative household survey data of the Tanzanian National Panel Survey (NPS). I employed this data in Section 4 to supply evidence that FSE reduced public secondary school fees. The survey also contains good measures of household investment in each individual child's education. I employ these seven measures of educational investment: the child's enrollment in primary school; whether she worked in the last seven days; whether she was absent from school in the last two weeks; how many hours she studied last week; whether she is at grade level; whether she has sat the PSLE; and education expenditures in the last year.[24]

To test whether FSE affected household measures of student investment, I employ a diff-in-diff framework which tests whether educational investments changed differentially for families likely to have been more affected by FSE. As in the Figure 1, I use Waves 2-5 of the LSMS, which were conducted in 2010-11, 2012-13, 2014-15, and 2020-21. To match as closely as possible the sample from the main analysis on administrative data, I focus on primary schoolchildren in households who have at least one child in secondary school at the time of the interview. When considering the outcome of whether a child is enrolled in primary school, the sample includes all primary-age students. When considering the outcomes of whether the child sat the PSLE, the sample includes only children in primary school who are at least 14 years old. For all other outcomes, the sample includes all children in primary school.

As before, it is necessary to identify which households were more "treated" by the FSE policy which eliminated the requirement of school fee payment for entry into public secondary schools. The household survey data, unlike the administrative data, does not include information on which students paid school

[24]Education expenditures include expenditures on school fees, transportation, meals, uniforms, books, private tutoring, and "other" expenses.

fees. However, it does include direct measures of families' financial circumstances. I assume that poorer families would have been more treated by the abolition of school fees, and proxy for a family's financial constraints using an indicator for whether the family lives in a house with a thatched roof. This is a common and simple means test for poverty (Egger et al., 2022).

$$Y_{it} = \alpha + \lambda_t + \beta_1 \, Treat_i + \beta_2 \, Treat_i \times Post_t + \varepsilon_{it} \tag{5}$$

Equation 5 mimics the structure of Equation 4, substituting in the financial constraints measure from the household survey data. Y_{it} is the outcome for primary school student i in survey wave t, in a household with at least one student in secondary school. λ_t are survey wave fixed effects. $Treat_i$ is an indicator for whether the student lives in a thatched-roof house. Unlike Equation 4, this regression does not include family fixed effects because it is based on a repeated cross section, so the family-level $Treat_i$ variable is included individually. This indicator is then interacted with a dummy for whether the year is 2016 or later, with the β_2 from this interaction as the coefficient of interest.

Figure B.3 shows parallel pre-FSE trends for each of these measures of school investments across more- vs. less-financially-constrained households.

Table 4: Diff-in-diff: family financial constraints measured by thatched roof homes, comparable sample (primary school students from households with a secondary school student)

	Invest-ment index	Worked last 7 days	Absent last 2 weeks	Hours studied last wk	Total ed. spending (USD, yr)	At grade level	Enrolled primary school	Sat PSLE
Treat	-0.480***	0.125***	0.053*	-0.963***	-99.324***	-0.129***	0.018*	-0.012
	(0.047)	(0.028)	(0.029)	(0.152)	(10.668)	(0.024)	(0.011)	(0.020)
Treat × Post	0.282***	-0.128**	-0.125**	-0.087	23.956	0.031	0.019	0.105
	(0.104)	(0.053)	(0.058)	(0.356)	(24.202)	(0.050)	(0.022)	(0.085)
N	6,978	6,978	6,978	6,978	6,978	6,978	6,636	967
DV Mean	0.070	0.179	0.314	2.334	130.056	0.772	0.918	0.040

Data: Waves 2-5 of Tanzania National Panel Survey. All regressions limited to households with at least one child in secondary school. 'Treat' is an indicator that the student lives in a house with a thatched roof. 'Post' is an indicator for the post-FSE survey wave, wave 5. Survey wave fixed effects for Waves 2 (2010-11), 3 (2012-13), 4 (2014-15), and 5(2020-21) included. 'DV Mean' displays means for non-treated students in Wave 4. 'Investment index' is the first principal component of a PCA index consisting of the outcomes in Columns 2-5, reverse coded where appropriate, and defined for the set of students in primary school. 'Enrolled Primary School' is defined as a dummy among all children age 7-13. 'Sat PSLE' defined as a dummy among all primary students age 14 and over. All other outcomes are defined over all children in primary school. Missing outcome values replaces with zero; dummies for missing included in all regressions. Standard errors clustered at the level of the first-stage sampling unit (*clusterid*). * p<0.10, ** p<0.05, *** p<0.01

The results can be seen in Table 4. The most dramatic effects are on child labor and school absence: FSE

reduced the incidence of both by 13 percentage points. This is consistent with the negative effects of FSE on exam absenteeism found in the administrative data (Table 3). It is also consistent with research showing that resource shocks affect households' allocation of child time into schooling vs. labor (Edmonds, 2006; Tang et al., 2020; Akresh et al., 2012), and with prior work suggesting that household income shocks affect child labor in Tanzania specifically (Beegle et al., 2006; Human Rights Watch, 2013). Coefficients are positive but not significant for primary enrollment, being enrolled at grade level, taking the PSLE, and household education spending on the child. Overall, these results imply that that students' attendance at school (and absence from work) was an important dimension on which FSE affected investments in primary students' human capital.

8 Conclusion

Free Secondary Education (FSE) has expanded rapidly in the developing world in recent decades, and is likely to continue to do so. One reason is that FSE programs appear to bring large benefits to the target demographic of secondary school students (Crawfurd, 2024; Crawfurd & Ali, 2022; Duflo et al., 2017; Brudevold-Newman, 2021; Blimpo et al., 2019; Agüero & Ramachandran, 2020). This helps make these reforms politically popular, and politically popular education reforms are something of a rarity (Sandholtz, 2023, 2024; Biasi & Sandholtz, 2024; Dias & Ferraz, 2019; Cox et al., 2024; Bursztyn, 2016).

Because human capital acquisition is dynamic, understanding the full effects of FSE requires that we also understand how it affects *primary* students' responses. This paper examines primary student responses to Tanzania's FSE reform, using the universe of high-stakes national standardized exams. Comparing students from *ex-ante* fee-constrained families with their less-constrained counterparts, I show that the reform increased primary school exam pass rates and transition rates to secondary school. This appears to have operated through demand-side channels: the policy changed patterns of effort on the exams, reduced absences and child labor, and increased positive school selection.

I find little evidence of a supply-side response, at least in the short term. However, the continued success of the policy likely hinges on that changing (Garlick, 2019; Bold et al., 2015). Other countries, faced with supply crunches and substandard quality, have turned to the private sector, with mixed results (Romero et al., 2020; Barrera-Osorio et al., 2020, 2022). While many promising education reforms are technically or politically complicated to scale up, expanding access is expensive but relatively straightforward (Bold et al., 2018). This makes it a potentially attractive target for aid from other countries or multilateral

institutions (Crawfurd & Ali, 2022). Knowing that the benefits of FSE extend to the (much larger) group of children still in primary school implies a higher cost-effectiveness than previously understood.

Like much research on education in developing countries, this paper offers a glimpse of the world's vast reservoir of untapped ability. Households' and students' decisions about educational investments are dependent on the menu of investments available to them, both present and future. This implies that measures of student ability depend on the context in which they are taken. Many students rationally disinvest in education when faced with future obstacles, yet have the ability to succeed when opportunities are available.

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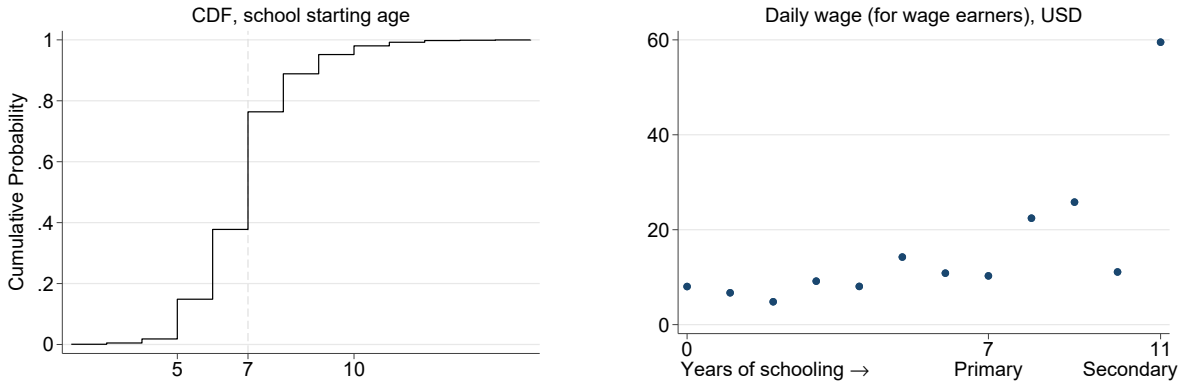
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A Descriptive data

A.1 Descriptive: Primary school starting age and mincer returns

Figure A.1: Schooling in Tanzania



(a) Age of school starting

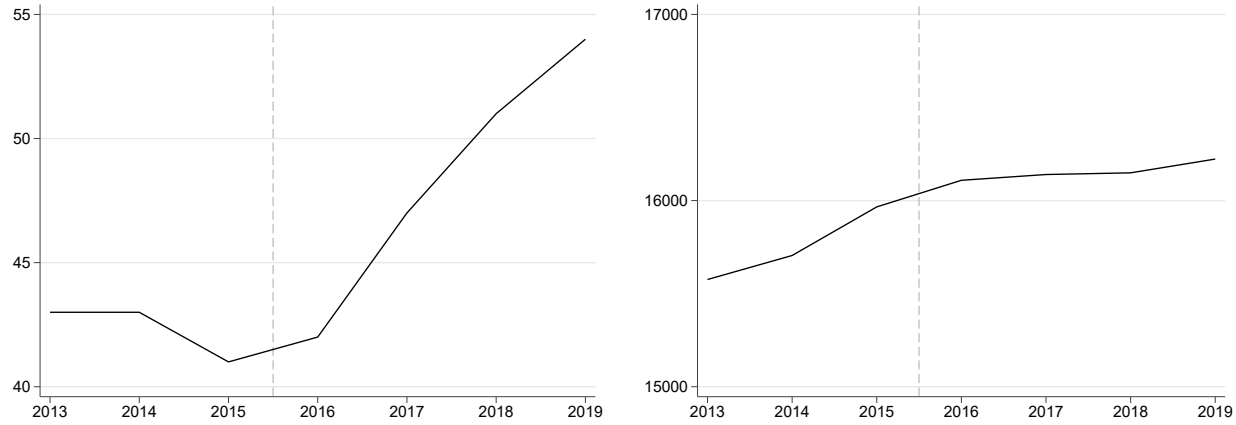
(b) Mincer wage returns

Panel (a): TZ National Panel Survey, Wave 4 2014-15. Includes students currently in school. N = 4064. Panel (b): Binscatter using Tanzania National Panel Survey, Wave 4 2014-15. Includes wage earners with $\leq$ 11 yrs education. N = 2498. Real USD values obtained by applying PPP conversion figures from WDI to original nominal values in TSH (World Bank, 2022).

Figure A.1a shows that the vast majority of Tanzanian students start primary school by the mandated age of seven. Figure A.1b shows that the descriptive Mincer returns to finishing secondary school are large.

A.2 Descriptive: Primary PTR and school

Figure A.2: Aggregate primary school resources



(a) Pupil:teacher ratio in primary schools

(b) Number of government primary schools

Pupil:teacher ratios at the primary level increased sharply following the policy, while government school construction flattened out.

A.3 Descriptive: Raw trends in PSLE pass rates for treated and control wards

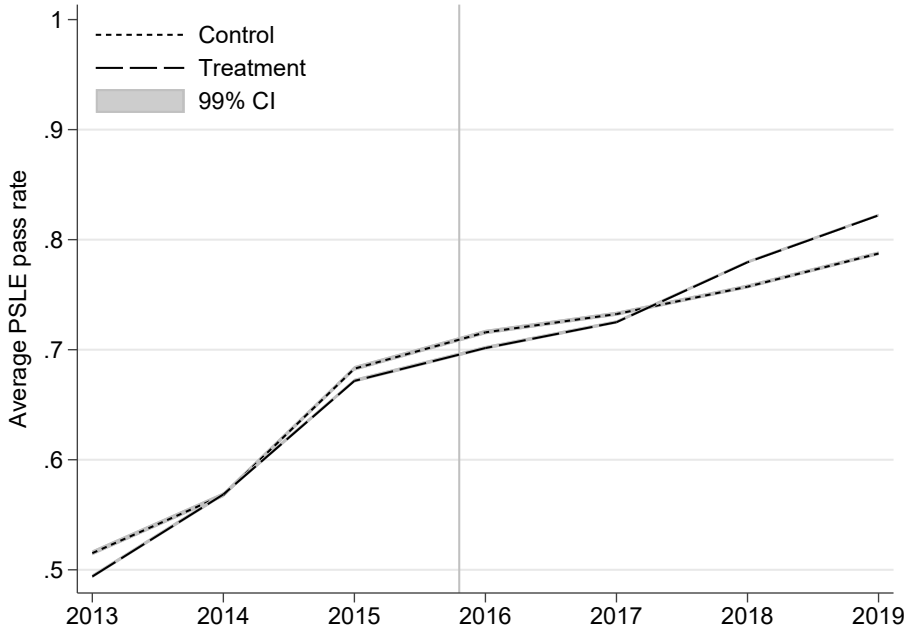


Figure A.3: PSLE pass rates in treated and control wards

Average likelihood a given PSLE takers passes the exam, by whether they sit the exam at a school located in a ward where 2014 FTNA fee non-payment is zero (control) or positive (treatment), by year of exam.

B Robustness

In this section I test the robustness of the main findings to various alternative treatment definitions, estimation strategies, and channels. I examine and reject the potential for effects to be driven by changes in the composition of Grade 7 exam takers or by false positives in the family matching procedure. I then show that the results are robust to replacing family fixed effects with older sibling's school $\times$ by Grade 7 exam score fixed effects, and to using older siblings' dropout as a measure of "treatedness" by FSE rather than fee constraints.

B.1 Parallel trends

B.1.1 Parallel trends: O-level and A-level pass rates

One concern might be that the members of "treated" and "control" families are on different trajectories in a way that is difficult to observe with the small number of pre-treatment periods for which individual PSLE data are available.

To address this concern, I follow a similar estimation strategy for different educational outcomes for which individual data are available going back many years: secondary school exam scores. Specifically, I examine pass rates for the Certificate of Secondary Education Examination (CSEE) — taken at the end of the four years of lower secondary school and commonly referred to as the ordinary or "O-level" — and the Advanced Certificate of Secondary Education Examination (ACSEE), taken at the end of the two years of upper secondary school and known as the Advanced or "A-level" exam. Similar pass rates on these exams for students from fee-paying and non-fee-paying families prior to FSE would be consistent with the parallel trends assumption.

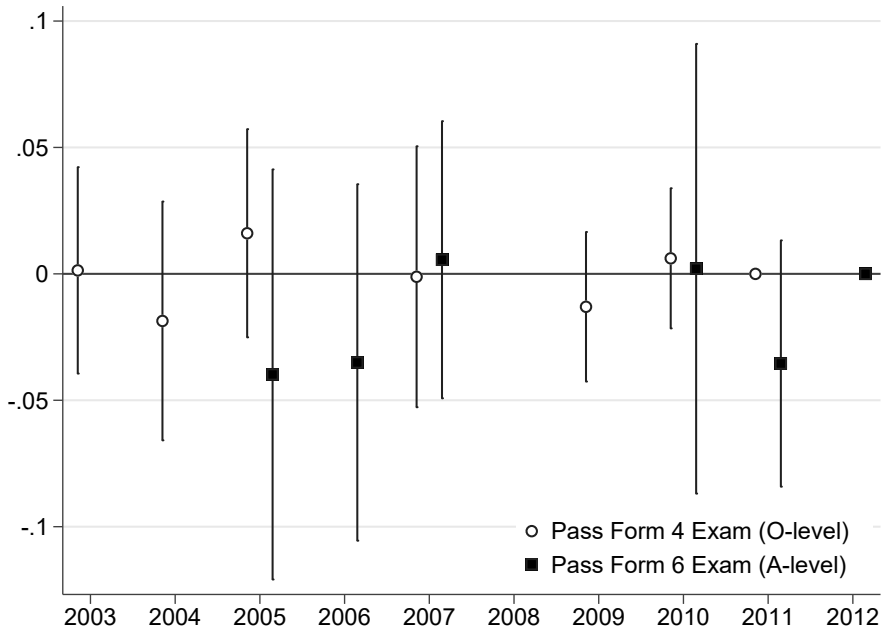
Individual O-level and A-level exam scores are available as far back as 2003 and 2005, respectively. These records have names associated with them, but only for years before FSE — making them well-suited for studying pre-trends, but not for measuring the causal effect of FSE on these outcomes. Specifically, O-level results include names for the years 2003-2005, 2007, and 2009-2011. A-level results include names for 2005-2007 and 2010-2012.

For this analysis, I use the same strategy and specification laid out in Equation (3), substituting the available years. I perform the regression separately for the O-level and A-level outcomes. For each set of test takers, I include in the regression only those who share a last name within their ward with a 2014 FTNA taker. The treatment variable takes a value of 1 where this test taker had their results withheld for

failure to pay examination fees. In each regression, I take the latest available year as the base year (2011 for O-level; 2012 for A-level).

Figure B.1 shows the results. In both cases, there is no evidence that treated and control families were on different trajectories more than a decade before treatment.

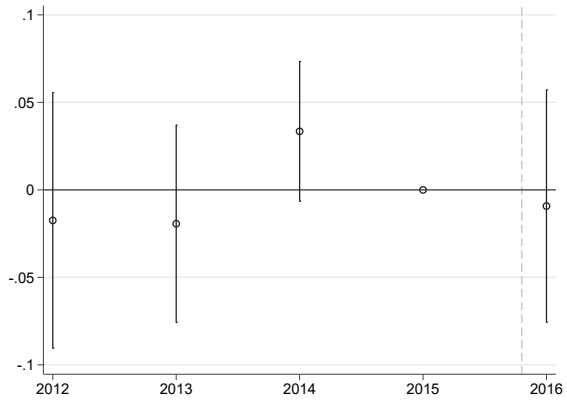
Figure B.1: Pre-trends: Difference in O-level and A-level pass rates in treated vs. control families



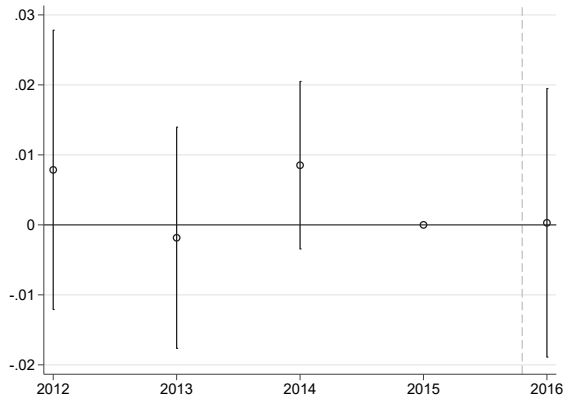
O-level results include names for the years 2003-2005, 2007, and 2009-2011. A-level results include names for 2005-2007 and 2010-2012.

B.1.2 Parallel trends: PSLE measures at the school level

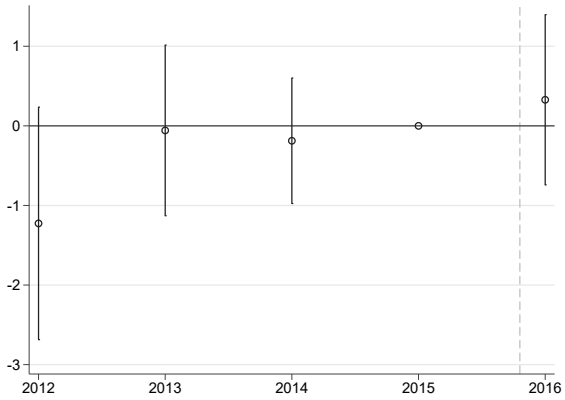
Figure B.2: Pre-trends: Difference in school-level PSLE measures in treated vs. control wards



(a) Avg. PSLE score (SD)



(b) Pass rate



(c) Number of candidates who finished the exam

Point estimates and 95% confidence intervals from a regression of school-level PSLE outcomes on the interaction of ward treatment status with year indicators, relative to 2015. School and year fixed effects included. Standard errors clustered at the ward level.

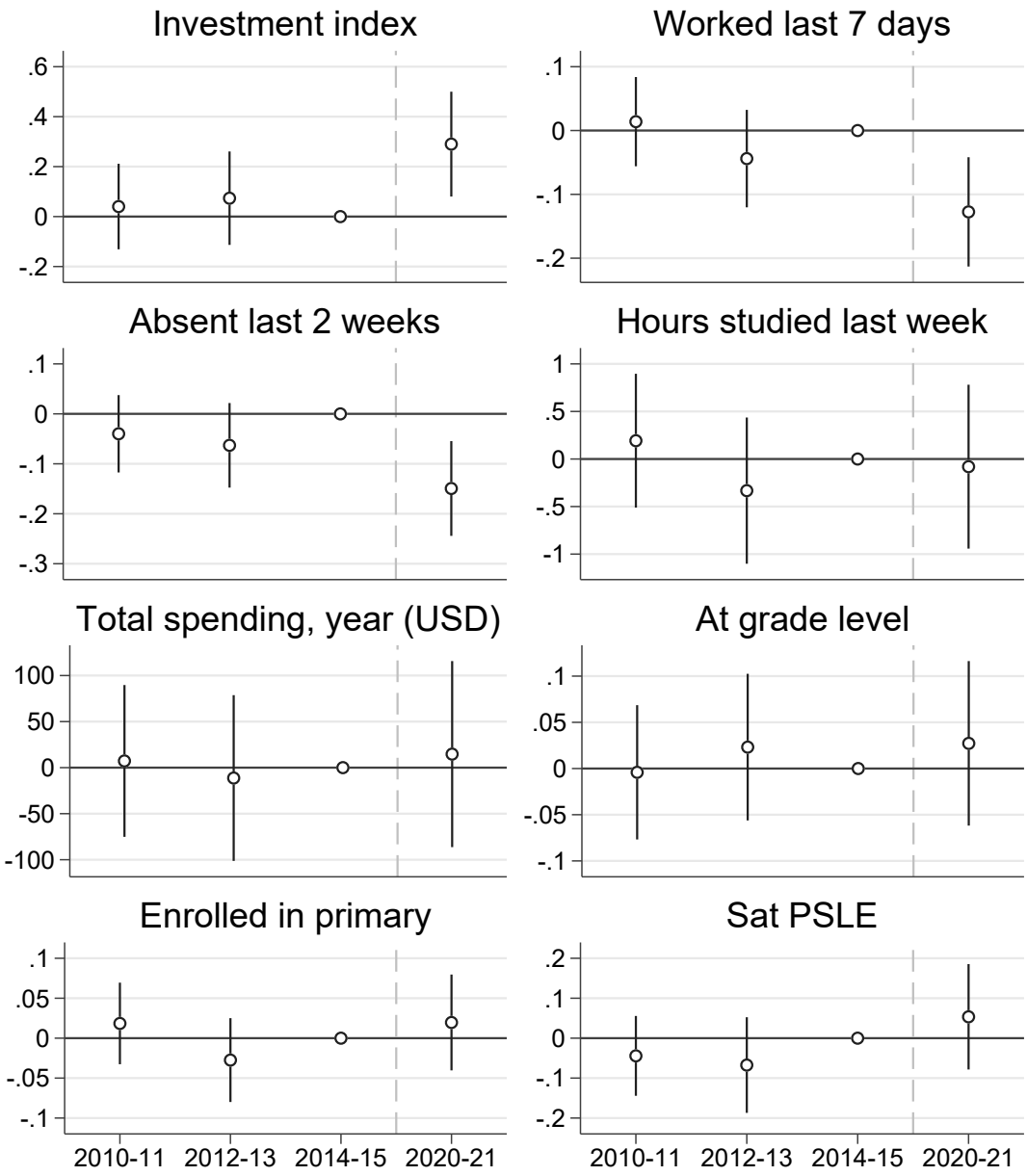
Table B.1: Diff-in-diff pre-trends: PSLE measures at the school level

	Pass rate PSLE	Num students	PSLE avg score (σ)
Treat × 2012	0.008	-1.226	-0.017
	(0.010)	(0.745)	(0.037)
Treat × 2013	-0.002	-0.057	-0.019
	(0.008)	(0.547)	(0.029)
Treat × 2014	0.009	-0.188	0.033
	(0.006)	(0.402)	(0.020)
Treat × 2016 (post)	0.000	0.328	-0.009
	(0.010)	(0.545)	(0.034)
N	62,235	62,235	62,235
School FE	✓	✓	✓
Year FE	✓	✓	✓

Omitted category is 2015. School and year FE. Sample consists of school-level BRN PSLE ranking data from 2012-2016. Standard errors clustered by ward. * p<0.10, ** p<0.05, *** p<0.01

B.2 Parallel trends: Household survey data

Figure B.3: Parallel trends on measures of student investment from household survey data



Data from Tanzania National Panel Survey, Waves 2-5. Each subfigure uses the same x-axis, reproduced only on the bottom set of graphs. Estimates shown in white circles include survey wave fixed effects. Estimates shown in black triangles include survey wave fixed effects interacted with district fixed effects.

B.3 Interacting controls with time trends

Because it is only possible to test for parallel pre-trends over a relatively short window in the family-level analysis, one concern might be that results could be driven by differential tends in observables. To test for this, I include a battery of family- and individual-level controls, as well as controlling for these variables' interaction with year fixed effects. The administrative test score data encodes a significant amount of information about family structure, demographics, and student performance. I limit these controls to pre-treatment characteristics of families and their 2014 FTNA takers to avoid including "bad controls" which may themselves be outcomes affected by the reform.

Family level controls include: the share of pre-treatment PSLE takers in the family which are female; the number of pre-treatment PSLE takers in the family; the commonness of the family's last name among 2014 FTNA takers in the ward; and a dummy for whether the family's last name matches a list of common Muslim names in Tanzania. Individual-level controls relating to the family's 2014 FTNA taker include: a dummy for whether the student was female; a dummy for whether they were repeating the FTNA in 2014; a dummy for whether they passed the FTNA; the overall FTNA score; the number of subjects for which the student chose to sit the FTNA; and a measure of the commonness of the student's full name in their cohort.

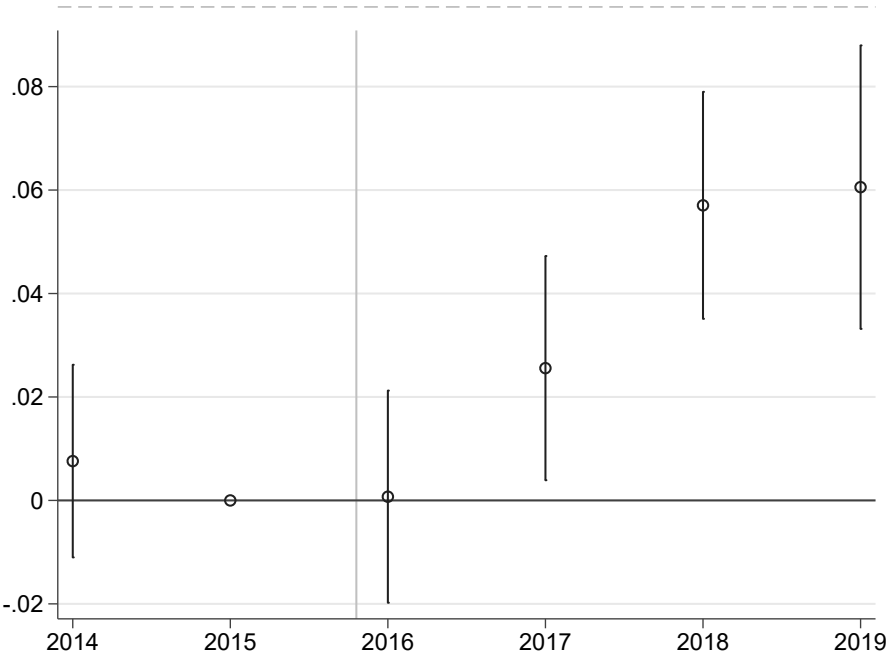
Table **??** shows the effects of FSE on the main outcomes of interest using this approach. All coefficients are comparable to those from the main specification presented in Table 3.

Table B.2: Diff-in-diff: effect on number of younger siblings taking PSLE – controlling for covariate trends

	Primary: achievement		Primary: investment			Secondary: achievement	
	Pass PSLE	PSLE score (σ)	Absent PSLE	Pri. school pass rate (pre-reform)	Pri. school cohort size	Transition	Pass FTNA | transition
Treat × Post	0.032***	0.031	-0.006***	0.026***	-1.861	0.048***	0.019**
	(0.008)	(0.019)	(0.001)	(0.006)	(1.258)	(0.011)	(0.008)
N	964,959	964,959	964,959	964,959	964,959	964,959	449,947
R^2	0.198	0.226	0.092	0.783	0.702	0.299	0.167
Mean (treated, pre)	0.554	-0.155	0.022	0.470	86.459	0.167	0.869
β_{oster}	0.031	0.032	-0.006	-0.270	-1.974	0.095	0.019

All regressions include family and year FE, as well as controls and controls interacted with year FE from the following list. Family level controls include: the share of pre-treatment PSLE takers in the family which are female; the number of pre-treatment PSLE takers in the family; the commonness of the family's last name among 2014 FTNA takers in the ward; and a dummy for whether the family's last name matches a list of common Muslim names in Tanzania. Individual-level controls relating to the family's 2014 FTNA taker include: a dummy for whether the student was female; a dummy for whether they were repeating the FTNA in 2014; a dummy for whether they passed the FTNA; the overall FTNA score; the number of subjects for which the student chose to sit the FTNA; and a measure of the commonness of the student's full name in their cohort. Standard errors clustered by ward. β_{oster} is a bound constructed according to Oster (2019) using suggested values of $\delta = 1$ and $R_{max} = \min\{1,\ 1.3 \times R^2\}$ from the controlled regression. 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

Figure B.4: Dynamic diff-in-diff, controlling for covariate trends



This figure shows interaction coefficients from Equation 3, including also controls and controls interacted with year fixed effects from the following list. Family level controls include: the share of pre-treatment PSLE takers in the family which are female; the number of pre-treatment PSLE takers in the family; the commonness of the family's last name among 2014 FTNA takers in the ward; and a dummy for whether the family's last name matches a list of common Muslim names in Tanzania. Individual-level controls relating to the family's 2014 FTNA taker include: a dummy for whether the student was female; a dummy for whether they were repeating the FTNA in 2014; a dummy for whether they passed the FTNA; the overall FTNA score; the number of subjects for which the student chose to sit the FTNA; and a measure of the commonness of the student's full name in their cohort. The specification shows how the difference in outcomes between PSLE takers from fee-constrained vs. non-constrained families changed over time. The figure plots the coefficients and 95% confidence intervals on the interaction between the Treat$_f$ dummy and each

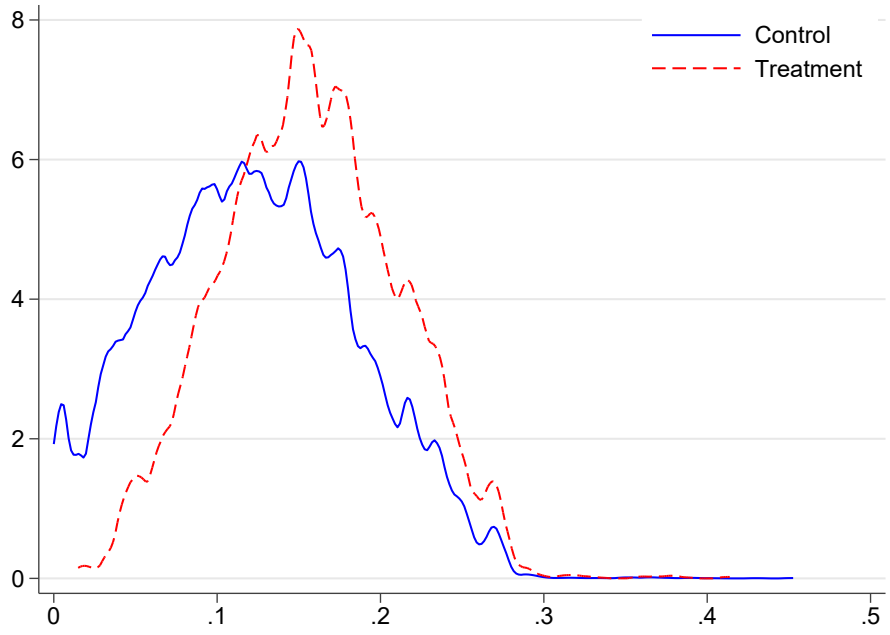
Figure B.4 shows estimates from Equation 3 for the outcome of passing the PSLE, using the same controls. Again the result is comparable to that of the main specification presented in Figure 7.

B.4 Propensity score matching

Another way to test robustness is to predict treatment using covariates, create propensity scores, and then perform a regression using overlap weights following (Li et al., 2018). I use the same controls included in the analysis of Section B.3 to create these propensity scores.

Figure B.5 shows kernel density plots for these propensity scores. The distribution for the treatment group is higher than that of the control group, suggesting that the predictors do indeed carry information useful for predicting families' fee payments. However, there is also significant overlap between the two distributions — necessary for the overlap weights to produce common support.

Figure B.5: Overlap in distribution of propensity scores, treatment and control



Kernel density plots for the propensity scores of the treatment and control groups, based on covariates presented in Section B.3.

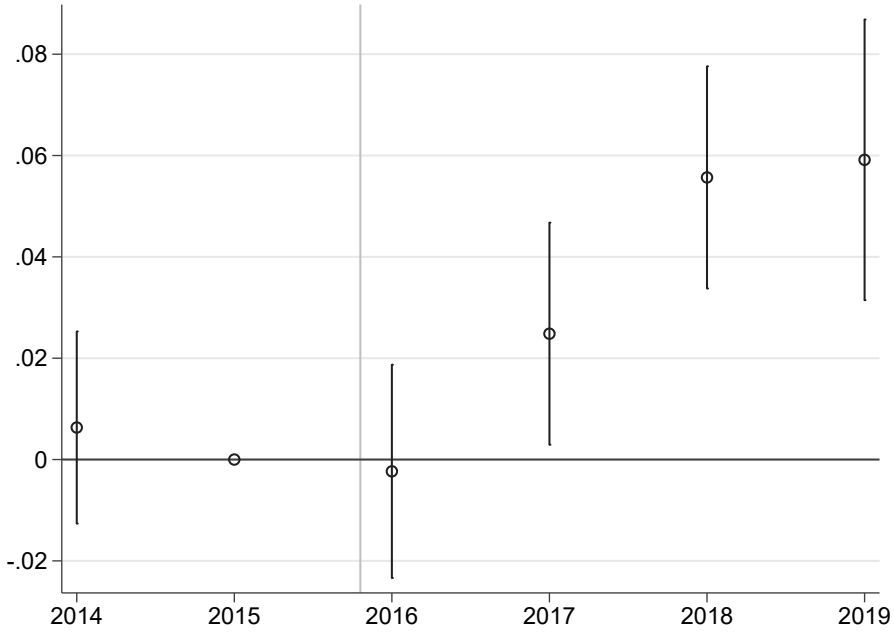
The results, seen in Table B.3, are very similar to the main results.

Table B.3: Diff-in-diff: effect on number of younger siblings taking PSLE

	Primary: achievement		Primary: investment			Secondary: achievement	
	Pass PSLE	PSLE score (σ)	Absent PSLE	Pri. school pass rate (pre-reform)	Pri. school cohort size	Transition	Pass FTNA | transition
Treat × Post	0.032***	0.030	-0.006***	0.019***	-2.045	0.045***	0.023***
	(0.008)	(0.020)	(0.001)	(0.004)	(1.284)	(0.012)	(0.008)
N	964,959	964,959	964,959	964,959	964,959	964,959	449,947
Mean (treated, pre)	0.554	-0.155	0.022	0.470	86.459	0.167	0.869

All regressions include family and year FE. Overlap weights included according to propensity of treatment. 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

Figure B.6: Dynamic diff-in-diff, PSM overlap weights



Notes.

B.5 No effect on number of younger siblings taking PSLE

One potential margin could be students' likelihood of sitting the grade 7 exam at all. Previous work from Tanzania has found evidence of school administrators manipulating which children are allowed to sit the exam in order to engineer higher pass rates (Cilliers et al., 2020). 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 B.4: 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 B.4 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.

B.6 Stricter family 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 × cohort*, and whose last name matches that of a 2014 grade 9 test-taker with a unique last name within the ward.

The results, shown in Table B.5, are very similar to those in Table 3, showing evidence that FSE raised Grade 7 exam pass rates, slightly increased selection into higher-pass-rate schools, and increased transition rates to secondary school. This specification finds slightly noisier but qualitatively similar results on

Table B.5: Diff-in-diff: family financial constraints measured by exam fee non-payment
Unique last names within ward × cohort

	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	0.031***	0.031	-0.003	0.008**	0.330	0.043***	0.017
	(0.010)	(0.023)	(0.002)	(0.004)	(1.234)	(0.012)	(0.015)
N	108,740	108,740	108,740	108,740	108,740	108,740	38,487
Mean (treated, pre)	0.627	0.016	0.016	0.525	91.284	0.218	0.903

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

reduced absences.

B.7 School × FTNA score fixed effects

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

$$Y_{itsg} = \alpha + \delta\, Treat_i + \sum_{t=2014}^{2019} \beta_t\, Treat_i \times \lambda_t + \lambda_t + \eta_s \times \zeta_g + \varepsilon_{itsg} \quad (6)$$

In Equation 3, Y_{itsg} is the outcome for PSLE-taker i in year t, whose sibling took the FTNA in school s and received grade g. $Treat_i$ 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[25]. 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 β_t coefficients on the interaction terms are the coefficients of interest.

The results, in Table B.6, are very similar to those in Table 3. FSE raised Grade 7 exam pass rates (and scores), significantly reduced absences from an already low base, increased selection into better schools, and increased transition rates to secondary school.

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

Table B.6: Diff-in-diff: family financial constraints measured by exam fee non-payment
School × score fixed effects

	Pass PSLE	PSLE score (σ)	Absent PSLE	Pri. school pass rate (pre-reform)	Pri. school cohort size	Transition	Pass yr9 exam | transition
Treat	-0.026***	-0.036**	0.003**	-0.010***	0.992	-0.038***	-0.010
	(0.007)	(0.017)	(0.002)	(0.004)	(0.951)	(0.009)	(0.009)
Treat × Post	0.034***	0.035**	-0.005***	0.017***	-1.366	0.037***	0.017*
	(0.008)	(0.018)	(0.001)	(0.004)	(0.971)	(0.011)	(0.009)
N	477,302	477,302	477,302	477,302	477,302	477,302	227,237
Mean (treated, pre)	0.577	-0.096	0.019	0.492	86.851	0.187	0.879

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

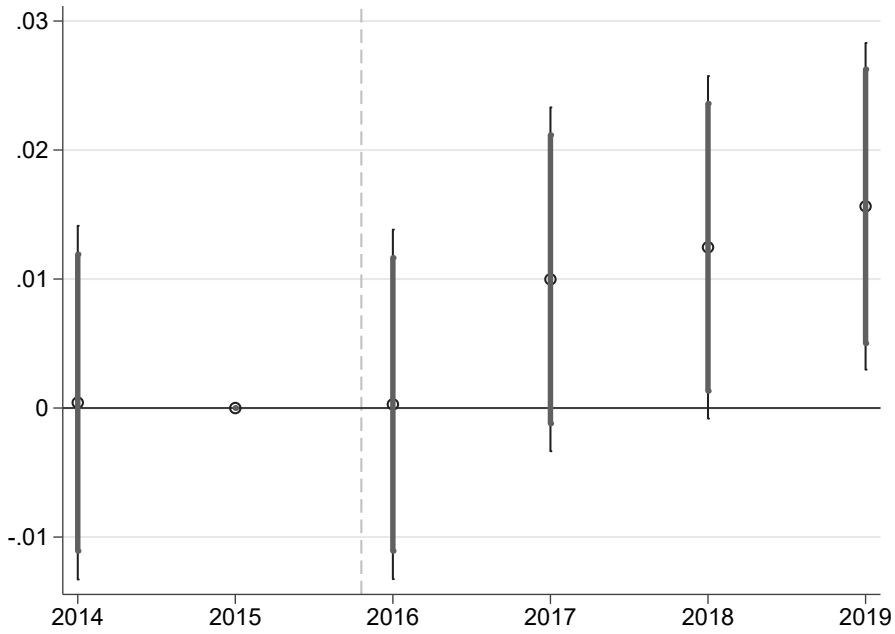
B.8 Older sibling dropout as treatment measure

Another measure of which families might be "treated" by the abolition of secondary school fees is whether children in the pre-reform period who passed the PSLE but nevertheless dropped out before sitting the FTNA — perhaps partly due to an inability to pay secondary school fees. This population may be considered less "marginal" than the main sample in terms of its ability to pay secondary school fees: these families may or may not have sent any pupils to secondary school in the pre-reform period, whereas all families in the main analysis sample sent at least one. (Among families for whom both measures of treatment can be defined – 42,801 of the 2,221,612 total families – the correlation between $Dropout_f$ and $NonPayment_f$ at the family level is just .0253.)

$$Y_{itf} = \alpha + \eta_f + \lambda_t + \sum_{t=2014}^{2019} \beta_t \, Dropout_f \times \lambda_t + \varepsilon_{itf} \tag{7}$$

In Equation 7, Y_{itf} is the outcome for PSLE-taker i in year t, from family f, whose sibling took the PSLE in 2013. λ_t are year fixed effects; η_f are family (last name × ward) fixed effects. The sample is limited to 2014-2019 PSLE takers who shared a last name with exactly one 2013 PSLE taker in their ward who obtained a passing score ("older family member"). $Dropout_f$ is an indicator for whether the student's older family member failed to make the transition to secondary school (as proxied by sitting the FTNA exam 2 years later in 2015). This indicator is then interacted with indicators for the years 2014-2019. The β_t coefficients on the interaction terms are the coefficients of interest.

Figure B.7: Difference in pass rates for younger siblings of PSLE-passing dropouts vs. non-dropouts



This figure shows interaction coefficients from Equation 7. The specification shows how the difference in outcomes between grade 7 exam takers whose older siblings passed the PSLE in 2013 but did vs. did not drop out changed over time. The figure plots the coefficients and 90% and 95% confidence intervals on the interaction between the $Dropout_f$ dummy and each year dummy (where the omitted year is 2015, the last year before the policy began).The average raw pre-reform difference in pass rates between these two groups was 13.9 percentage points.

Figure B.7 shows that, while effects are a bit noisier and smaller, the general pattern evident from the main analysis is present here as well. After the introduction of FSE, families more likely to have been constrained by school fees (here proxied by previous dropout within the family) saw disproportionate increases in primary school test scores.

This analysis can be repeated for various outcomes in a difference-in-differences framework:

$$Y_{itf} = \alpha + \eta_f + \lambda_t + \beta\, Dropout_f \times post_t + \varepsilon_{itf} \tag{8}$$

Table B.7 shows coefficients from Equation 8, with results broadly similar with those in Table 3. PSLE takers from treated families (defined here as those where an older family member failed to transition to secondary school despite passing the exam) became more likely after the reform to pass the PSLE and

Table B.7: Diff-in-diff: family financial constraints measured by prior dropout

	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	0.010**	0.001	-0.000	0.001	-0.727	0.054***	0.001
	(0.005)	(0.011)	(0.001)	(0.002)	(0.522)	(0.006)	(0.006)
N	310,271	310,271	310,271	310,271	310,271	310,271	144,553
Mean (treated, pre)	0.677	0.146	0.014	0.600	84.193	0.235	0.905

All regressions include family and year FE. Sample limited to students whose older sibling passed the PSLE. 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

to transition to secondary school (though this analysis shows no evidence of reduced absence rates or selection into better schools).

C Grade 4 low-stakes exam outcomes

The PSLE is a high-stakes exam determining entry to secondary school, which implies that after secondary fees were removed, passing the exam would have been the binding constraint on secondary entry for many more students. Could the average effects I measure be driven by the manipulation or corruption of grading practices to help students get over this barrier? PSLE tests are graded centrally, not marked by students' own teachers or administrators, which makes this possibility remote. In the one region and cohort for which precise PSLE scores are available — Mwanza in 2016 (after the introduction of FSE) — there is no evidence for excess mass to the right of the passing threshold (see Figure D.2).

To further interrogate this hypothesis, I look at Grade 4 low-stakes exam results. In Grade 4, students sit the first national standardized test of primary school, the Standard Four National Assessment. Like the PSLE, it entails five subjects on which all students are tested: English, Math, Science, Social Studies, and Swahili, with NECTA providing data on letter grades for each subject as well as on the overall score (A, B, C, D, and E). For the SFNA, a D is considered a passing grade. However, failing the SFNA does not prevent students from continuing on to Grade 5; in this sense the test is low-stakes. Manipulation around the passing threshold may therefore be considered less likely than on the PSLE given that failing has no consequences for students' ability to continue their schooling.

Table C.8 displays summary statistics for Grade 4 (low-stakes) exam takers, for the overall sample and the analysis sample of students for whom the fee constraints variable can be defined.

Table C.8: Summary stats: Grade 4 exam takers

	All	Analysis Sample
School cohort size	142.852	137.724
	(130.083)	(131.680)
Female	0.504	0.508
	(0.500)	(0.500)
Public school	0.966	0.966
	(0.181)	(0.181)
Elite (shares name with local political candidate)	0.006	0.013
	(0.075)	(0.111)
Pass Grade 4 exam	0.860	0.879
	(0.347)	(0.326)
Overall exam score (%)	47.447	48.441
	(19.389)	(18.923)
English subject score (%)	37.126	38.040
	(22.485)	(22.318)
Math subject score (%)	40.224	40.866
	(24.303)	(23.957)
Social studies subject score (%)	51.434	52.178
	(22.098)	(21.717)
Science subject score (%)	49.756	50.757
	(22.270)	(21.940)
Swahili subject score (%)	58.694	60.363
	(25.360)	(24.793)
Full name unique in nationwide cohort	0.971	0.938
	(0.167)	(0.240)
Family fee payment status available	0.183	1.000
	(0.386)	(0.000)
Family failed to pay sec. school fees ('treated')		0.098
		(0.297)
N	9,944,667	1,783,048

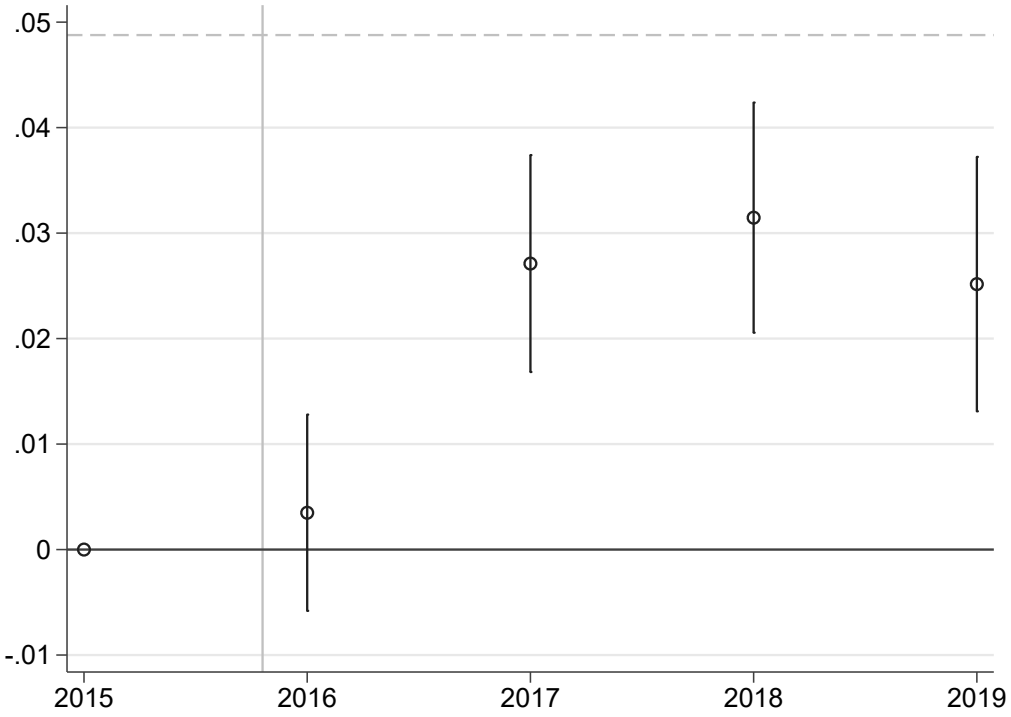
An observation is an individual student. The full universe includes all SFNA takers from mainland Tanzania 2015-2019. The analysis sample is restricted to SFNA takers in these years whose school can be located in a ward, and who can be clearly matched to a treatment status through a 2014 FTNA taker in the same ward.

To study how FSE might have affected Grade 4 students' performance on the low-stakes SFNA, I follow

the same strategy I did with PSLE outcomes. I assign treatment status by identifying students who share a last name with at least one 2014 FTNA taker in the same ward ("family members"); treated students are those whose 2014 FTNA family members failed to pay school fees on time, and control students are those whose 2014 FTNA family members succeeded in paying school fees on time. I then estimate Equations 3 and 4 using the SFNA data.

Unfortunately, Grade 4 exam results are only available in digitized format starting in 2015, one year before the FSE reform was implemented. This means that while it is possible to measure the difference in outcomes between these two groups in the year prior to treatment, testing for parallel pre-treatment trends is impossible (although the fact that parallel trends were established on Grade 7 outcomes using the same family-level treatment variation may diminish concerns somewhat). I interpret these estimates with caution.

Figure C.8: Differences in Grade 4 exam pass rates



This figure shows interaction coefficients from estimating Equation 3 on the SFNA data. The specification shows how the difference in outcomes between Grade 4 exam takers from fee-constrained vs. non-constrained families changed over time. The figure plots the coefficients and 95% confidence intervals on the interaction between the $Treat_f$ *dummy and each year dummy (where the omitted year is 2015, the last year before the policy began). For reference, the dotted line at 0.049 shows the average raw pre-reform difference in pass rates between pupils from fee-constrained vs. non-constrained families.*

Figure C.8 plots the β coefficients from estimating Equation 3 on the SFNA data. The broad patterns of differences for Grade 4 students look very similar to those for Grade 7 students. Prior to the reform, failure rates were 66% higher in the treatment group (13.4% compared to 8.9%). This difference narrowed dramatically after the reform took place. That performance on low-stakes Grade 4 exams among students from fee-constrained families converged toward that of students from non-constrained families after FSE's implementation is consistent with FSE causing greater primary student investment. Summary statistics for this sample and a regression table for this analysis can be found in Section A.

Table C.9 shows regression results for estimating Equation **??** on various outcomes: passing the SFNA Grade 4 exam; score on the Grade 4 exam; being absent for the Grade 4 exam; and the size of the cohort in which the student sat the Grade 4 exam.

Table C.9: Grade 4 exam takers

	Pri achievement		Pri investment	
	Pass Grade 4 exam	Grade 4 exam score (σ)	Absent Grade 4 exam	Pri. school cohort size
Treat × Post	0.029***	0.086***	-0.003	-6.657**
	(0.005)	(0.018)	(0.003)	(2.821)
N	1,783,048	1,783,048	1,783,048	1,783,048
Mean (treated, pre)	0.864	-0.190	0.055	107.591

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

D Data details

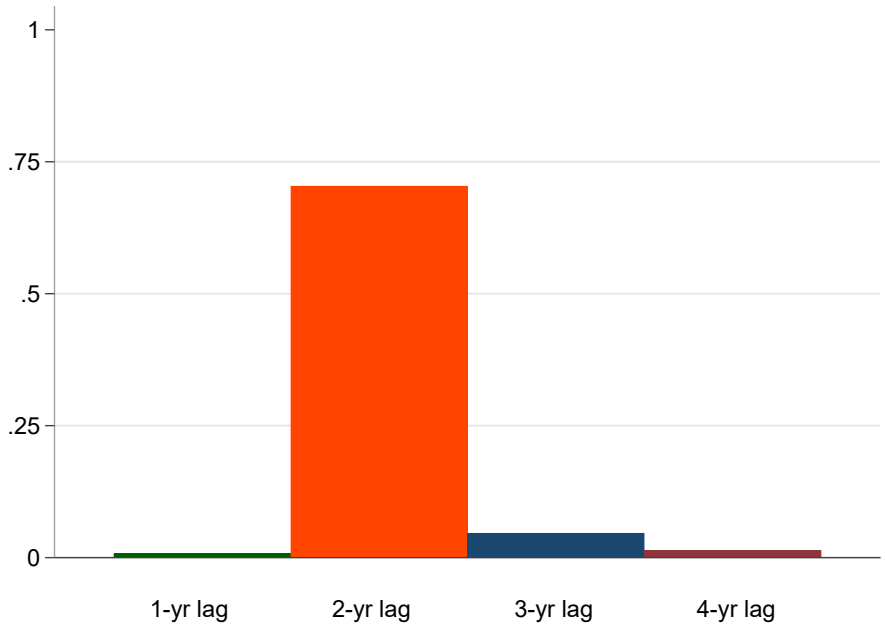
This section provides more detail on variable construction using NECTA data.

D.1 Transition to secondary school

Because the measure of secondary transition relies on matching student names across exams, assessing the fidelity of this match is important. One measure of data quality is whether student names include a middle name or only a middle initial. This would affect the probability of matching on multiple dimensions: students with a middle name in one test but a middle initial in the other will not be matched, and students with only a middle initial are less likely to be uniquely identified in general. Within ward$\times$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.

Testing how many FTNA (grade 9) test takers match to a PSLE (grade 7) test taker from two years prior is another way to assess the quality of this measure of transition. Figure D.1 shows what fraction of Grade 9 FTNA takers match to the name of a PSLE taker from prior years.

Figure D.1: Match rate of FTNA takers to PSLE takers in prior cohorts



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.

Most FTNA takers with unique names — 70% — match to the unique name of someone who took the PSLE two years earlier, representing normal grade progression. Less than 1% match to the name of a PSLE taker from one year earlier. 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. About 5% and 1% match to the PSLE cohorts from three and four years prior, respectively. Many of these matches likely represent 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 (though some are likely also spurious name-sharers).

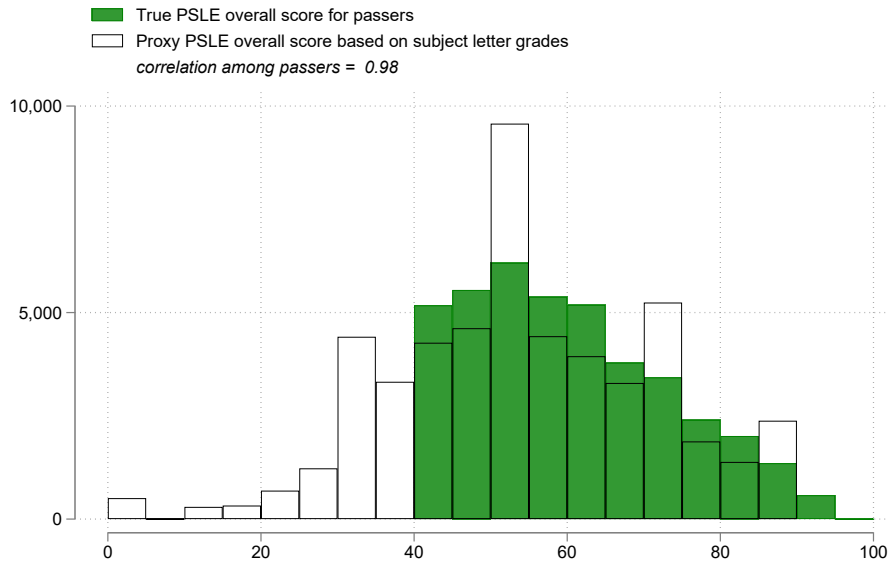
D.2 Proxy PSLE overall score

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, at least for one cohort in one region. For the 2016 PSLE cohort from Mwanza (the second-most populous region in Tanzania), the list of 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 shows no evidence of excess mass to the right of the passing threshold, increasing confidence that test scores are not being manipulated.

Figure D.2: Histogram: comparison of proxy overall PSLE score with true overall PSLE score



This histogram shows the distribution of proxy PSLE overall scores — calculated by assigning each of the five subject letter grades the midpoint of its score range and summing each student's five subject scores — on top of the distribution of true overall PSLE scores for those who passed the exam. Both distributions show test takers from Mwanza region in 2016, the only group for which precise overall PSLE scores were available.

E PSLE questions

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 E.1: Example questions from the PSLE