Sibling Spillovers and Free Schooling

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Abstract

We use administrative data to measure sibling spillovers on academic performance before and after Tanzania's introduction of Free Secondary Education (FSE). Prior to FSE, students whose older siblings narrowly passed the secondary school entrance exam were less likely to go to secondary school themselves; with FSE, the effect became positive. A triple differences analysis, using geographic variation in FSE exposure, shows that FSE caused the reversal. Negative pre-FSE spillovers were concentrated in poorer regions. Positive post-FSE spillovers were largest for lower-performing younger siblings. Our results demonstrate that FSE alleviated financial constraints, allowing families to distribute educational investments more equitably rather than concentrating resources on high-performing children.

Keywords: Sibling spillovers, free secondary education, intra-household allocation, resource constraints, high-stakes exams, Tanzania
JEL Codes: I25, O15, D13, I24, J13

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1 Introduction

Can free public education affect households' allocations of human capital investment across siblings? Theoretically, parents face a tension between "compensating" investments in lower-ability children vs. "reinforcing" investments in higher-ability children (Becker and Tomes, 1976). Many studies from the developing world document the existence of parental reinforcement (sometimes denoted "sibling rivalry"): parents invest more in the human capital of higher-ability children, generating negative sibling spillovers (Vogl, 2013; Morduch, 2000; Akresh et al., 2012; Jensen and Miller, 2017; Rosenzweig and Schultz, 1982). In the developed world, however, most studies find evidence of positive human capital spillovers from older to younger siblings (Qureshi, 2018b; Nicoletti and Rabe, 2019). This naturally raises the question of whether the negative spillovers observed in poorer settings are driven by resource constraints. Parents in both high- and low-income countries express preferences for equalizing investments across siblings (Berry et al., 2024; Adhvaryu and Nyshadham, 2016; Behrman et al., 1982). Even within developing countries, reinforcement of existing sibling differences is associated with lower household resources (Leight and Liu, 2020; Giannola, 2023). By relaxing resource constraints, free public education policies may enable households to allocate human capital investments more equitably among children rather than "picking a winner" (Banerjee and Duflo, 2012).

Little existing research examines how sibling spillovers respond to changes in school access. Large literatures on sibling spillovers exist in both the developed world (where access is near-universal) and the developing world (where access is more limited), but no studies have yet managed to measure spillovers in the context of a shock to school access. Exogenous variation permitting the identification of sibling spillovers is rare, and coincident policy changes in access to schooling are even rarer. Some RCT evidence from the developing world has shown that increased resources (in the form of cash transfers or scholarships) can have null or even negative effects on educational outcomes for recipients' siblings (Barrera-Osorio, Felipe and Bertrand, Marianne and Linden, Leigh L and Perez-Calle, Francisco, 2011; Duflo et al., 2017). However, it is not obvious that effects would be similar for large changes in public school access at scale (Bold et al., 2018).

In this paper, we use administrative data to identify sibling spillovers in education before and after the introduction of a nationwide Free Secondary Education policy (FSE) in Tanzania. Tanzania offers an ideal context for examining how household investments allocations respond to increased education access, for two reasons. The first is that the binding exam score threshold for secondary school entrance provides exogenous variation in secondary school entry which can be used to measure sibling spillovers. The second is the 2016 FSE policy which abolished lower secondary school fees (primary school fees had been abolished over a decade earlier). This policy is not unique to Tanzania: secondary school is the frontier of schooling access in much of the developing world, and about half of the countries in sub-Saharan Africa offered fee-free public secondary school as of March 2023 (Garlick, 2019; Gruijters et al., 2023). School fees were a binding constraint on many Tanzanian households' educational investments prior to the policy, and existing evidence shows that household educational expenditure in this context is responsive to a positive resource shock (Sandholtz, 2024b; Burchardi et al., 2024).

To measure sibling spillovers, we exploit a discontinuity at the exam score required for admission to public secondary school. We draw upon the universe of administrative data on the results of the Primary School Leaving Exam (PSLE). This exam is high-stakes: a pass is required to continue in the public school system. In the words of a local newspaper, failing is associated with a higher likelihood of moving from primary school into "mining, grazing, and family activities" instead of secondary instruction (Kippenberg, 2014). Our data include multiple cohorts of exam takers before and after the introduction of FSE. We link students by last name within wards to identify pairs of likely siblings. Our sample consists of sibling pairs in which the older sibling scored at or near the passing threshold on the exam. We compare the educational outcomes of younger siblings in these pairs whose older sibling narrowly did vs. did not qualify for entry into public secondary school. We measure the "intent-to-treat" effect of having a sibling pass the qualifying exam, rather than using exam passing as an instrument for secondary school entry.[1]

We find that sibling spillovers are significant and negative prior to the introduction of the FSE reform — but significant and positive afterward. Prior to the reform, an older sibling who marginally passed the exam *reduced* their younger siblings' likelihood of transitioning to secondary school by about 9% (2.1 percentage points from a base of 24%) — consistent with models of parental reinforcement. After secondary school fees were abolished, older siblings' exam passage *increased* their younger siblings' transition rates by about 2% (0.9 pp from a base of 61%). For comparison, these effect sizes represent a meaningful fraction of the descriptive difference in transition rates between places with above- vs. below-median poverty (4.8 pp in the pre-reform period). For a comparison in the literature, a Tanzanian conditional cash transfer program of 3600 Tanzanian shillings per child per month (USD $4 in 2024) raised (focal) children's likelihood of ever having attended school by about 4 pp (Evans et al., 2023).

We show evidence that this reversal in the sign of sibling spillovers was caused by the FSE policy,

[1]If passing the exam is interpreted by parents as a signal of student ability, this could affect investments in subsequent children independently of whether the older sibling ends up attending secondary school or not, which would violate the exclusion restriction.

comparing the difference in spillovers between areas more vs. less affected by FSE, as in Lucas and Mbiti (2012). This difference remained constant prior to FSE, then widened significantly after FSE's implementation. We complement this evidence with a family-fixed-effects analysis, which likewise shows that the sign of spillovers within families remained constant prior to FSE then increased significantly for siblings finishing primary school after the reform's implementation.

We present evidence that FSE changed the sign of sibling spillovers by (partially) alleviating financial constraints. Negative pre-reform spillovers were strongest — and positive post-reform spillovers weakest — in districts with higher poverty rates. Examining heterogeneity by gender, we find that negative spillovers in the pre-period appear to be driven by competition for resources among male siblings, while girls lag far behind in achievement. Post-reform, the gender achievement gap narrowed considerably, and the gender gap in spillover effects disappeared.[2]

We also find evidence consistent with parental preferences for equalization of opportunity across siblings, at least in the post-FSE period. For cohorts of younger students who sat the qualifying exam after the introduction of FSE, we observe a measure of *ex-ante* student ability: 4th grade exam scores (available data do not extend back far enough to observe this measure for pre-reform cohorts). We find that positive sibling spillovers on pass rates and transition rates are concentrated among lower-ability students. We interpret this as consistent with parents responding to positive older-sibling information shocks by reallocating resources to compensate struggling younger siblings.

Our main contribution is to show that sibling spillovers in human capital respond to changes in public school access. This finding helps to reconcile divergent estimates from the developed and developing world. In poorer countries, where resource constraints are more likely to bind, much of the existing literature documents 'reinforcing' investments in children with an existing advantage (real or perceived). Using Tanzanian household survey data, Morduch (2000) shows a positive correlation between a child's educational attainment and the share of his siblings who are girls, consistent with "rivalry for scarce resources in which parents favor sons." Our causal results (from the pre-reform period) support this interpretation. Akresh et al. (2012) show similarly that parents invest more in high-ability children in Burkina Faso. These negative spillovers are often gendered: Vogl (2013) shows that it is common in sub-Saharan Africa for early marriage and dropout to create competition among sisters, while Shrestha and Palaniswamy (2017) show in Nepal that men's educational opportunities crowd out human capital investment in their

[2] It is also possible that sibling spillovers are driven by direct interactions between siblings that may affect performance through self-esteem, envy, or other psychological channels considered in the peer effects literature (e.g., the Invidious Comparison model (Hoxby and Weingarth, 2005; Antecol et al., 2016)). We are unable to test these channels directly, but we are not aware of formulations of these models which can explain the reversal of the sign on sibling spillovers we observe at the time of the FSE reform.

sisters. Meanwhile, nearly all studies on developed countries find evidence of positive older-to-younger sibling spillovers, through various channels including mentoring and role model effects, returns to scale, and school selection (Qureshi, 2018b; Nicoletti and Rabe, 2019; Karbownik and Özek, 2021; Aguirre and Matta, 2021; Figlio et al., 2023; Zang et al., 2023).[3] Some existing studies have examined how intra-household allocations respond to resource shocks: Behrman (1988) finds that parental reinforcement in India is stronger in the lean season, Leight and Liu (2020) show a negative gradient between reinforcement and maternal education in rural China, and Giannola (2023) demonstrates in a survey experiment in India that reinforcement falls with household resources. Ours is the first study to measure at scale how sibling spillovers change over time in a given country, and in response to expanded public school access. We show that after instituting FSE, Tanzania's pattern of sibling spillovers began to resemble that of richer countries. While there are many differences between rich and poor countries that might explain differences in sibling spillovers, our results suggest that access to fee-free public education may be an important one.

Our paper also underscores that sibling spillovers can alter the cost-benefit calculus of public policies in general, and schooling expansions in particular. Existing work emphasizes the importance of sibling spillovers in the evaluation of girls' education in Pakistan and targeted school policies in the US (Qureshi, 2018a; Figlio et al., 2023). Our paper shows that sibling spillovers matter for evaluating the effectiveness of school access policies at scale (Brudevold-Newman, 2021; Lucas and Mbiti, 2012; Crawfurd, 2024; Garlick, 2019).

Finally, we contribute more broadly to the literature on sibling spillovers in education by introducing a novel source of variation in older sibling school attendance: discontinuities in qualifying exam scores. Existing work on sibling spillovers has exploited variation in siblings' college-going (Altmejd et al., 2021), disabilities (Black et al., 2021), school starting age rules (Karbownik and Özek, 2021; Zang et al., 2023), grade retention policies (Figlio et al., 2023), teen pregnancy (Heissel, 2021), peer quality (Nicoletti and Rabe, 2019), and teacher quality (Qureshi, 2018b). Much of this literature comes from developed countries where secondary schooling is not subject to an entrance exam. However, secondary school qualifying exams with binding thresholds are still common in many parts of the developing world, providing an important potential source of variation in siblings' school attendance that can be used for measuring

[3]While this pattern in the sign of spillovers in the literature is strong, it is not exact: Qureshi (2018a) and Lindskog (2013) find evidence of positive spillovers in Pakistan and Ethiopia, respectively, and Leight (2017) shows evidence of parental compensating investments in rural China. Ozier (2018) documents large positive spillovers on siblings' cognition from deworming in Kenya, though these appear to have operated through the direct epidemiological channel. Meanwhile de Gendre (2022) identifies small negative spillovers of higher class rank on younger siblings' test scores in the Netherlands, though she also finds that higher-ranked older siblings cause greater parental investment in younger siblings.

sibling spillovers in other developing contexts.

2 Context and Data

Prior to the implementation of FSE in 2016, the vast majority of Tanzanian students finished primary school but failed to finish secondary school. Primary school is compulsory from the age of 7 and lasts for seven years (Standards 1-7). These are followed by four years of non-mandatory lower secondary instruction (Forms 1-4) and 2 years of upper secondary (Forms 5-6). Public schools are dominant, comprising 95% of all primary schools and 79% of all secondary schools as of 2016. Net enrollment rates in 2016 stood at 84% for primary but only 24% for secondary, in which Tanzania trails the average completion rate in Sub-Saharan Africa by approximately 10 percentage points (World Bank, 2022). However, returns to secondary school in Tanzania have been estimated at 15% per year, suggesting that many more students could benefit from staying in school longer (Montenegro and Patrinos, 2014). For many students, the outside option of school was work. Household survey data show that prior to FSE, 45% of secondary-age children had worked in the past seven days (including wage labor, apprenticeships, household businesses, and farming).[4]

2.1 Introduction of Free Secondary Education (FSE)

In November 2015, the Tanzanian government approved a Free Secondary Education policy (FSE). Under the new policy, public schooling — free at the primary level since 2001 — would become fee-free through the first four years of secondary school. This entailed the abolition of school fees and examination fees, and was intended to include even other auxiliary fees. Prior to FSE, yearly secondary school fees were about TZS 20,000 per pupil (equivalent to about USD $25 in 2024 at PPP) (Oxford Business Group, 2019; World Bank, 2022). (For comparison, average household consumption expenditure in Tanzania around this time was just over 400,000 TZS (National Bureau of Statistics, 2019).) Then-president John Magufuli said in a speech, "We have planned to transfer these funds directly to all the relevant schools . . . money for capitation grants, money for chalk, money for examinations, money for everything, we are sending it" (Taylor, 2016). This was an ambitious aim, but household survey evidence shows clearly that the policy succeeded: the amount of money households reported spending on school fees for their children in secondary public schools fell dramatically after the policy came into effect, without any accompanying

[4]Source: Tanzania National Panel Survey, Wave 4 (2014-15) (World Bank, 2014–2020).

increase in non-school-fee spending (Sandholtz, 2024b; World Bank, 2014–2020). This shows that school fees were not simply relabeled or made unofficial. Descriptive evidence suggests that these fees had been binding constraints on many students' transition to secondary school; transition rates rose sharply after the policy's implementation, even though students were still required to pass the qualifying exam in order to enroll in secondary school.

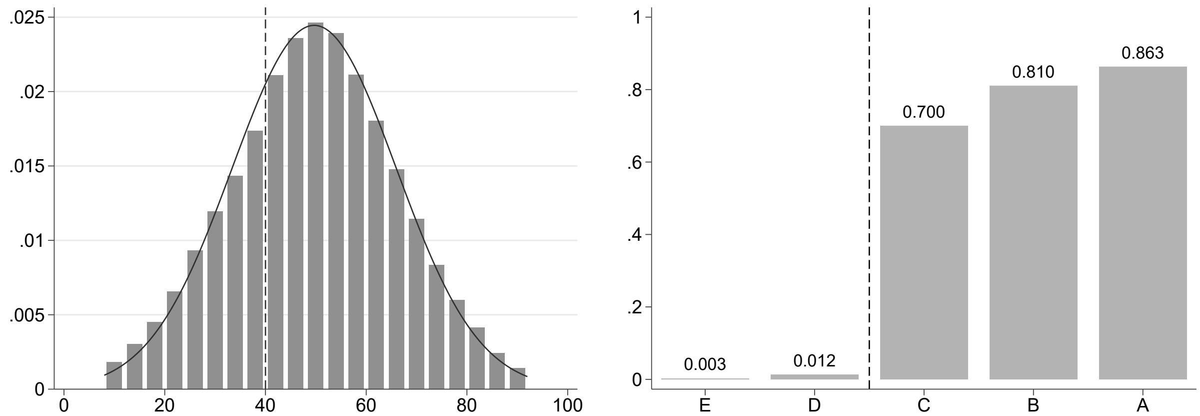
2.2 Administrative data on national standardized exams

The National Examinations Council of Tanzania (NECTA) administers a series of national examinations that determine whether and how students may continue their studies. These tests are graded centrally — not by each school's own teachers — thereby alleviating concerns of systematic manipulation of scores. We use the full universe of administrative data from two of these exams: the Primary School Leaving Examination (PSLE) and the Form Two National Assessment (FTNA).

Primary School Leaving Examination (PSLE): This exam is administered at the end of the seventh and final year of primary school, in September. It is not required, but in practice well over 95% of pupils enrolled in Standard 7 sit the exam. It is a high-stakes test, functioning as a qualifying exam for continuing in the public education system: a passing grade is required to enroll in secondary (i.e., non-vocational) instruction at a government-run school. (Private schools are not required to consider PSLE scores for admission but in practice many do.) Therefore in this paper we use interchangeably the terms "PSLE" and "secondary school entrance exam" or "qualifying exam." Our analysis covers 7 PSLE cohorts (2013-2019), during which 6,068,930 total pupils sat the exam.

Five subjects are covered in the PSLE: English, Mathematics, Swahili, Social Studies, and Science, with each subject accounting for one-fifth of the possible exam marks (i.e., 50 out of 250 total points). NECTA does not make students' precise marks available, but does provide the associated letter grades for each subject and the overall test. The marks for each subject map onto letter grades according to the following scale: 0-9 marks correspond to an E ($<$ 20%), 10-19 to a D (20-39%), 20-29 to a C (40-59%), 30-39 to a B (60-79%), and 40-50 to an A ($>$ 80%). Regarding the overall score, 200 marks is the threshold for an A, whereas B's, C's, and D's correspond to scores of at least 150, 100, and 50 respectively. The passing threshold for the exam is an overall score of 100 marks (40%) — the bottom end of scores corresponding to a C average. Our data therefore provide a sharp measure of which students passed the PSLE, and a noisy measure of their exam marks (overall and by subject).

Figure 1: Descriptive: testing for manipulation of exam scores and secondary transition



(a) Histogram of PSLE scores

(b) Secondary enrollment, by own PSLE score

N = 1,084,960, consisting of all individual "younger siblings" from the analysis sample. Panel 1a shows the histogram of imputed overall PSLE scores, created by averaging the midpoints of the percentage windows corresponding to each of the student's five subject letter grades, with an overlaid normal distribution. The dotted line at 40% indicates the passing threshold. Panel 1b shows the share of PSLE takers who enrolled in secondary school (as proxied by appearing in the FTNA grade 9 exam data two years later), by the letter grade of their overall PSLE score. A "C" is required to pass.

Because students' PSLE papers are gathered and sent from all over the country to a central location to be graded, manipulation of the grading process is unlikely. Figure 1 illuminates this fact, using data from the roughly one million students whose outcomes we consider in our main analysis sample (see Section 3). Panel 1a shows the histogram of imputed overall PSLE scores (scaled to a total possible score of 100).[5] The histogram shows no excess mass around the passing threshold, and follows closely the normal distribution plotted on top of it. This suggests that manipulation of scores at an appreciable scale is unlikely. Panel 1b, meanwhile, plots the share of PSLE takers who went on to enroll in secondary school on time (according to our proxy measure), by the letter grade of their overall PSLE score. Very few of those who failed the PSLE enrolled in secondary school, demonstrating the binding nature of the passing threshold and the high stakes of the exam.

Form Two National Assessment (FTNA): Two years after enrolling in secondary education, pupils sit this exam in order to determine whether they can proceed to Form 3. Because administrative data on transition to secondary school is not available at the individual level, we use students' participation in this exam as a proxy for transition to secondary school. Because NECTA had not yet adopted unique student

[5] These imputed scores were created by averaging the midpoints of the percentage windows corresponding to each of the student's five subject letter grades; i.e. a student who received a D on all five subjects would receive an imputed score of 30%, while a student scoring two A's and 3 B's would receive a 68%.

identifiers in the exam data during the time period we study, we match students across exams using their names. About 98% of PSLE takers' names are unique within their cohort.

We proxy for whether a PSLE taker in year t transitioned to secondary school using a binary variable for whether their full name appeared in the list of FTNA takers in year $t+2$. (We drop the 2% of students with non-unique names, following Sandholtz (2024b) and Sandholtz et al. (2024).) We consider this to be a conservative measure of secondary transition for a number of reasons. First, students who enrolled in secondary school but dropped out before the end of the second year will be missed. Second, students who sat both exams but used differently-spelled names (e.g. middle name vs. middle initial) will be missed. Third, students who skipped or repeated a year will be missed.[6] We confirm that passing the PSLE is a strongly binding constraint on secondary transition: only about 1% of those who fail appear as FTNA takers two years later (Figure 1b). (See cohort-wise summary statistics for these exams in Table A.1.)

3 Design

Causally identifying sibling spillovers is challenging. Siblings are a type of peer, whose behavior may affect each other mutually. Identifying the effect of one sibling's educational outcomes on those of another requires overcoming the reflection problem using a credible source of exogenous variation (Manski, 1993).

Our strategy for overcoming this problem entails comparing the younger siblings of older students who narrowly passed or failed the PSLE to qualify for secondary school. This allows us to bypass the problem of simultaneity in peer effects by measuring non-contemporaneous outcomes: we are interested in how older siblings' exam passage affects the performance of younger siblings at least one year later. It also affords us a source of quasi-random variation in the older sibling's educational attainment. Because we are interested in spillovers on enrollment in secondary school, we study the effect of older siblings' outcomes on younger siblings' outcomes (and not the effect of younger siblings on older siblings, as in Karbownik and Özek (2021)). To carry out this strategy, we must first identify pairs of siblings, and then identify which older siblings are near the score threshold for passing the qualifying exam.

[6] Less than 1% of PSLE takers in year t match to an FTNA taker's name from year $t+1$, and less than 5% match to an FTNA name from $t+3$, suggesting that normal grade progression is common for those who pass the exam on their first try, and that retaking the exam is uncommon.

3.1 Sibling Matching

Because NECTA administrative data does not contain family identifiers, we create a proxy for them by combining information from students' names with information about their locations.[7] We link students into family groups using shared last names within wards (Swahili: *kata*), the third administrative subdivision of Tanzania. Naming conventions in Tanzania dictate that children typically take their father's last name, meaning that siblings (children of the same father) share a last name. To reduce false positives, we consider a set of potential 'older siblings' as PSLE takers whose last name is unique within their PSLE cohort × ward (constituting 48% of PSLE takers).[8] (We define a student's ward as the ward of the school in which she sat the PSLE.) We then match these older siblings to the set of individuals in more recent PSLE cohorts who share that last name in the same ward: the 'younger siblings'. We discard family groups consisting of more than six matches (one per year) as likely false positives due to common names. This procedure yields around 1.54 million older-younger sibling pairs. These exclusions mean our analysis is not representative of the entire country – students with more common last names are underrepresented – but permits greater confidence in the precision of sibling matching.

'Siblings' matched in this way exhibit similar educational outcome on average, as would be expected of students within families (Hanushek et al., 2021). Positive and significant correlations between PSLE pass status in sibling pairs exist both before and after the introduction of FSE (0.20 and 0.15, respectively). Correlations between secondary transition outcomes are similar: 0.13 prior to FSE, and 0.12 afterward. Figure A.1a makes visible these positive correlations by plotting younger siblings' PSLE pass rates and secondary transition rates by older siblings' overall PSLE score, before and after FSE's introduction.

We explore alternative sibling matching procedures in Section 4.1 and Appendix Section B.1. We show that our results are qualitatively similar when we match siblings at the school (rather than ward) level, and when we match siblings on middle and last names (rather than just on last names). Our preferred specifications look at outcomes on younger siblings but take as the unit of observation the sibling pair, considering all matched pairs which meet the above criteria. This means that some individuals may appear more than once, as the younger sibling to multiple older siblings. Our results are robust to enforcing that each younger sibling appear only once by excluding all but the closest-spaced older sibling of each younger sibling.

[7] See Cruz et al. (2017) for a similar example of inferring family connections through naming patterns.

[8] Due to its high linguistic and ethnic diversity, Tanzania shows a high degree of surname variability. Out of approximately 6 million PSLE takers, we find over 300,000 distinct last names, of which over 170,000 appear more than once.

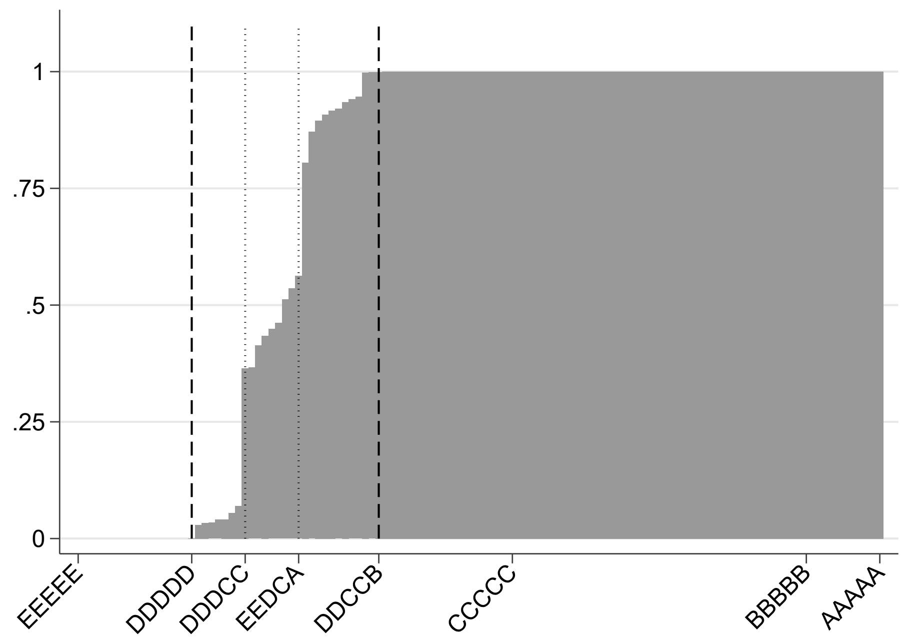
3.2 Probability of Passing the PSLE

Our identification strategy rests on the assumption that older siblings who barely pass the PSLE are fundamentally similar *ex-ante* to those who barely fail it, meaning that the determination of which of these students have the opportunity to attend secondary school is as good as randomly assigned. Our strategy therefore shares the intuition of a regression discontinuity design, with the important difference that in the absence of precise numerical PSLE scores, there is no clear running variable. If precise numerical PSLE scores were available, the setting would be well-suited to a standard regression discontinuity design. If we had noisy PSLE score measures but lacked precise information on treatment assignment, the setting would be well-suited to a "doughnut design" excluding observations near the cutoff (Dong and Kolesár, 2023). Instead, our setting features information on subject grade permutations, which noisily measure the overall PSLE score but precisely measure the treatment assignment. As such, we limit our attention to exactly those observations for which the subject grade permutation bins do not perfectly predict treatment assignment — the inside of the "doughnut hole."

To identify older siblings near the passing threshold, we use information on their performance on each of the five subject exams whose scores are aggregated to determine the overall score. We compute the observed probability of passing the PSLE conditional on each of the 2000 distinct observed permutations of the five subject grades. Many of these are unambiguous: students whose five subject grades are all equal to a C or higher all pass the overall exam (i.e., they have a passing probability of 1); pupils receiving only E's have a passing probability of 0. Many other grade permutations, however, reflect scores near enough to the passing threshold that they include both passers and failers. For example, the most common combination of subject grades is 3 C's and 2 D's: 87% of these students pass the exam. The next-most common combination is 3 D's and 2 C's: 36% of these students pass. We consider older siblings near the passing threshold to be those whose grade permutations are not determinate of passing status; i.e., those with a passing probability in the open interval of]0, 1[. Our estimation strategy entails comparing the outcomes of younger students whose older siblings received the same permutation of subject grades, but differed in whether they passed the exam or not, employing the idea of local randomization (Cattaneo et al., 2017).

Figure 2 plots the fraction of students who passed the exam in each of the observed combinations of letter grades. (See Figure A.2 for a histogram of passers and failers at each grade combination.) For the sake of legibility, the figure considers grade combinations along the x-axis (i.e., it collapses all grade sets featuring the same number of each grade regardless of which subject those grades were in). This understates the granularity of our main analysis, which includes fixed effects for grade permutations (i.e., same grades in same subjects). The lowest grade combination observed in which at least one student passed overall was DDDDD. The highest grade combination observed in which at least one student failed was DDCCB. This set of grade combinations is indicated on the graph by the dashed lines. We show that our results are robust to smaller windows of passing probability: the dotted lines on Figure 2 demarcate the open interval of passing probabilities from]0.3, 0.7[.

Figure 2: PSLE letter grade combination and probability of passing



Bar graph showing the fraction of PSLE takers who received a passing grade overall for each observed combination of letter grades on the five subject exams: English, Math, Social studies, Science, and Swahili. For ease of exposition, this graph plots letter grade combinations without regard for which subject the grade was received in; it includes 121 observed combinations, from a theoretically possible 126. Our identification strategy in fact includes grade permutation fixed effects, comparing the younger siblings of older students who both got the same set of letter grades in the same subjects, but differed in whether they passed overall.

Limiting our attention to the younger siblings of students with scores near the PSLE passing threshold does not yield a group of students who are appreciably different from the average student in mainland Tanzania. Table 1 reports summary statistics on key variables for the full universe of test takers, as well as for students in our analysis sample by virtue of being matched to older siblings with a PSLE score near the passing threshold (as measured by a passing probability in the open intervals of]0,1[and]0.3,0.7[, respectively).

Table 1: Summary statistics

	All	Analysis sample]0,1[	Analysis sample]0.3,0.7[
Female	0.529	0.532	0.533
	(0.499)	(0.499)	(0.499)
Muslim name (inferred)	0.230	0.198	0.201
	(0.421)	(0.398)	(0.401)
Government school	0.966	0.982	0.984
	(0.182)	(0.133)	(0.124)
Full name unique in nationwide cohort	0.979	0.992	0.992
	(0.144)	(0.092)	(0.092)
Pass PSLE	0.708	0.733	0.733
	(0.455)	(0.443)	(0.442)
Grade A or B on PSLE	0.256	0.238	0.233
	(0.436)	(0.426)	(0.422)
Secondary transition	0.483	0.547	0.552
	(0.500)	(0.498)	(0.497)
District: fraction of homes with grass/leaf roofs	0.251	0.266	0.265
	(0.197)	(0.194)	(0.193)
Ward: primary $\rightarrow$ secondary dropout rate (2013)	0.835	0.846	0.847
	(0.110)	(0.107)	(0.106)
N	5,200,947	522,712	136,373

An observation is an individual primary school student. The full universe includes all PSLE takers from mainland Tanzania 2014-2019. The main analysis sample]0,1[is restricted to PSLE takers in these years whose school can be located in a ward, and who share a last name with a PSLE taker from an earlier cohort in the same ward whose PSLE score had a passing probability in the open interval]0,1[. The narrower analysis sample restricts this window further to students whose older sibling's PSLE score had a passing probability in the open interval]0.3,0.7[. 'Secondary transition' is defined only for students whose full name is unique within their nationwide cohort.

3.3 Passing the PSLE increases students' own likelihood of secondary transition

Table 2 shows that among these students near the passing threshold, passing the PSLE has a large and statistically significant effect on a student's own likelihood of making the transition to secondary school: about 25 percentage points in the pre-reform period, and 43 percentage points after the reform's implementation. The effect of passing the PSLE on one's own transition probability is robust to narrowing the interval of passing probability, with the coefficient remaining strikingly stable as the window narrows, both before and after the introduction of FSE. The average transition rates for those who fail the PSLE are extremely low ($\leq$ 2%) both before and after the reform, showing that the score cutoff is binding and not subject to meaningful manipulation.

Table 2: Effect of passing PSLE on student's own secondary transition probability

	Pre-FSE			Post-FSE		
	]0,1[	]0.15,0.85[	]0.3,0.7[	]0,1[	]0.15,0.85[	]0.3,0.7[
Passed PSLE	0.248***	0.257***	0.258***	0.434***	0.428***	0.427***
	(0.006)	(0.009)	(0.016)	(0.011)	(0.013)	(0.017)
N	26,132	6,299	3,568	199,073	66,505	44,990
Mean, PSLE fail	0.009	0.015	0.016	0.014	0.020	0.020

Notes: Standard errors clustered by grade permutation in parentheses. Outcome measure – student's own transition to secondary school – is only measured for students whose full name is unique across the country within their cohort. All regressions include fixed effects for cohort, school, and grade permutation. The unit of observation is a unique older sibling. The row labeled 'Mean, PSLE fail' displays the mean secondary transition for students in the regression who failed the PSLE. Windows of passing probabilities for each regression are indicated in the column titles. Significance levels: * <0.1, ** <0.05, *** <0.01

3.4 Econometric specification

Our estimates of sibling spillover effects in educational achievement are obtained by estimating equations of the form:

$$y_{iogst} = \beta PSLEpass_o + \gamma_g + \rho_s + \tau_t + \varepsilon_{iogst} \tag{1}$$

where y_{iogst} is one of our binary outcomes of interest for younger sibling i: transition to secondary school, passing the PSLE, and achieving a high score on that exam (A or B). $PSLEpass_o$ is a dummy variable that takes value one if student i's older sibling o passed the PSLE, and zero otherwise; β is the parameter of interest. In order to compare the younger siblings of older siblings who are as similar as possible, we employ a set of extremely granular fixed effects: γ_g captures the fixed effect of the older sibling's subject-specific PSLE grade permutation, while ρ_s and τ_t capture fixed effects for younger sibling i's school and cohort, respectively. Our main analyses estimate this equation separately for pre-FSE and post-FSE cohorts. In robustness analyses, we extend equation (1) by including controls for the sex of each sibling and the age gap between them. Our main specifications report standard errors clustered by (older sibling's) score permutation, according to Lee and Card (2008). Given concerns about the coverage properties of confidence intervals constructed from these standard errors, we also report Eicker-Huber-White heteroskedasticity-robust standard errors for our principal regressions (Kolesár and Rothe, 2018).

To formally test whether sibling spillovers are different in the pre- and post-FSE periods, in equation

(2) we interact $PSLEpass_o$ with a $Post_t$ dummy for whether the younger sibling's cohort is 2016 or later. Year fixed effects absorb the effect of the $Post_t$ dummy on its own.

$$y_{iogst} = \beta_1 PSLEpass_o + \beta_2 PSLEpass_o \times Post_t + \gamma_g + \rho_s + \tau_t + \varepsilon_{iogst} \tag{2}$$

As mentioned above, we restrict our primary analysis sample to sibling pairs in which the older sibling's PSLE grade permutation could have corresponded to either a passing or failing numerical score (i.e., an implied "probability of passing" strictly between zero and one).

Our identification assumption is that conditional on the older sibling's PSLE subject letter grades, her passing the PSLE is as good as random. We test this assumption by comparing whether the characteristics of older and younger siblings differ significantly across the threshold, before and after FSE, for sibling pairs in which the older sibling's PSLE score corresponded to a passing probability in the open interval]0,1[or]0.3,0.7[.

Table 3 compares time-invariant characteristics of students from pairs in which the older sibling was on one side or the other of the PSLE passing threshold, conditional on the fixed effects described in equation (1). Our administrative data do not provide much detail about individual characteristics, but they do provide students' sex, as well as names, which we can compare with a list of common Muslim names to infer religion. This conservative inference method identifies 20% of students as having Muslim names in both the pre- and the post- period. (About 35 % of Tanzania's population is Muslim (Pew Research Center, 2012).) We can also test for differences in the number of characters in a student's name. This is important because our main outcome measure, Transition to secondary school, relies on matching students by name. As expected, we find few significant differences across the older sibling's PSLE passing threshold on these characteristics. Of twenty-four tests, one shows a difference significant at the 5% level, and one shows a difference significant at the 10% level, consistent with what would be expected from random chance. These differences are of small magnitudes, and appear for the sex of the older sibling in the pre-FSE period, and the name length of the younger sibling in the pre-FSE period (both differences disappear in the]0.3, 0.7[window). Including controls for these variables in the main analysis has no appreciable impact on the estimates. This suggests that variation in exam passing near the threshold can be considered quasi-random (and also that an older sibling passing the PSLE does not alter the composition of younger siblings who sit the exam).

Table 3: Balance

	]0, 1[				]0.3, 0.7[			
	Fail		Pass		Fail		Pass	
	Mean	(SE)	Diff.	N	Mean	(SE)	Diff.	N
Younger siblings — Pre-FSE:								
Female	0.546	(0.002)	-0.009	84,730	0.551	(0.005)	-0.004	16,848
Name length	20.941	(0.012)	-0.068*	84,730	20.904	(0.025)	0.044	16,848
Muslim name (inferred)	0.195	(0.002)	0.001	84,730	0.195	(0.004)	0.002	16,848
Younger siblings — Post-FSE:								
Female	0.534	(0.001)	-0.002	521,738	0.532	(0.002)	-0.003	121,259
Name length	20.908	(0.005)	0.005	521,738	20.911	(0.010)	0.003	121,259
Muslim name (inferred)	0.197	(0.001)	0.000	521,738	0.203	(0.001)	-0.001	121,259
Older siblings — Pre-FSE:								
Female	0.575	(0.002)	-0.023**	84,730	0.561	(0.005)	-0.016	16,848
Name length	20.729	(0.013)	0.007	84,730	20.712	(0.028)	0.071	16,848
Muslim name (inferred)	0.193	(0.002)	0.003	84,730	0.185	(0.004)	0.007	16,848
Older siblings — Post-FSE:								
Female	0.594	(0.001)	-0.009	521,738	0.586	(0.002)	-0.006	121,259
Name length	20.828	(0.005)	0.007	521,738	20.843	(0.010)	0.020	121,259
Muslim name (inferred)	0.195	(0.001)	-0.001	521,738	0.195	(0.001)	0.003	121,259

Notes: Standard errors clustered by older sibling's grade permutation. Columns 2 and 3 display the mean and standard error of the variable for a given group among those whose older sibling failed the PSLE. Column 4 displays the regression coefficient from regressing this variable on a dummy for whether the older sibling passed the PSLE, with fixed effects for older sibling's school and grade permutation and younger sibling's cohort, and considering only those pairs in which the older sibling's grade permutations corresponds to a passing probability in the open interval]0, 1[. Column 5 presents the number of observations in this group. Columns 6-9 present the same quantities for students from pairs in which the older sibling's grad permutation corresponds to a passing probability in the open interval]0.3, 0.7[. The unit of observation is a (younger or older) sibling in a sibling pair, as in the main analysis; this means that individual younger or older siblings from sibships of greater than two are counted more than once. 'Muslim name (inferred)' indicates that the student had at least one name from a list of common names among Muslims. 'Not missing Grade 4 exam' is defined only for the 2018 and 2019 PSLE cohorts of younger siblings, and indicates that the student was able to be matched to their Grade 4 exam result (Standard Four National Assessment). 'Low score, Grade 4 exam' indicates (for matched students) a Grade 4 exam score of C or lower. Significance levels: * <0.1, ** <0.05, *** <0.01

4 Results

Table 4 presents our main results on sibling spillovers, showing coefficients from estimating equation (1) separately in the periods before and after the introduction of FSE. As outcomes of interest, we consider three complementary margins of educational achievement: enrolling in secondary school (*Transition*), passing the PSLE, and receiving a high score (i.e., an A or B average) on that exam. We find that before FSE, a student whose older sibling passed the exam was 0.9 percentage points less likely to enroll in secondary school than a pupil with a comparable older sibling who did not pass the exam. Moreover, she faced a 1.2 p.p. lower probability of passing the PSLE herself. We find no effect on the likelihood of obtaining a high score.

After the introduction of FSE, students whose older sibling passed the exam were 0.7 percentage points *more* likely to enroll in secondary schooling, 0.4 percentage points more likely to pass the PSLE, and 0.8 percentage points more likely to obtain a high score compared to their peers whose older sibling failed the exam. We report standard errors clustered by grade permutation in parentheses and Eicker-Huber-White heteroskedasticity-robust standard errors in brackets; in all regressions they are nearly identical.

Table 4: Spillover effects from older siblings' qualification for secondary school

	Pre-FSE			Post-FSE		
	Transition	PSLE Pass	High score	Transition	PSLE Pass	High score
Older sibling passed the PSLE	-0.009**	-0.012***	0.004	0.007***	0.004**	0.008***
	(0.005)	(0.004)	(0.004)	(0.002)	(0.002)	(0.002)
	[0.005]	[0.006]	[0.004]	[0.002]	[0.002]	[0.002]
N	84,035	84,730	84,730	517,117	521,738	521,738
Mean (PSLE fail)	0.223	0.587	0.143	0.572	0.723	0.226

Notes: Standard errors clustered by grade permutation in parentheses. Heteroskedasticity-robust Eicker-Huber-White standard errors in brackets. 'Transition' is only measured for students whose full name is unique across the country within their cohort. 'High score' is a binary variable equal to one if the younger sibling got an A or B average on the PSLE, and zero otherwise. All regressions include fixed effects for younger sibling's school and cohort and older sibling's grade permutation. The sample includes younger siblings from all sibling pairs in which the older sibling's grade permutation was associated with a probability of passing the PSLE between — and not including — 0 and 1. The row labeled 'Mean, PSLE fail' displays the mean for students whose older sibling failed the PSLE. Significance levels: * <0.1, ** <0.05, *** <0.01

Table 5 estimates these effects in a more structured way according to equation (2), interacting the treatment dummy (older sibling passed PSLE) with a dummy for the post-FSE period. We focus on our main outcome, secondary transition for younger students. The result in column 1 confirms that the effect of older siblings passing the exam on younger siblings' secondary transition is statistically different across the two periods.[9] The remaining columns show that these estimates are robust to narrowing the

[9] Because the estimation uses grade-combination fixed effects, the coefficients vary slightly compared to the results from equation

width of the passing probability window, as well as to including fixed effects for younger sibling's school, older sibling's school, or both (all regressions include fixed effects for younger sibling's cohort and older sibling's grade permutation). As before, we report standard errors clustered by grade permutation in parentheses and Eicker-Huber-White heteroskedasticity-robust standard errors in brackets; here, robust standard errors are somewhat smaller than those obtained by clustering.

Table 5: Effect of older sibling's qualification on younger sibling's transition, interacted with post-FSE indicator, varying passing probability intervals and fixed effects

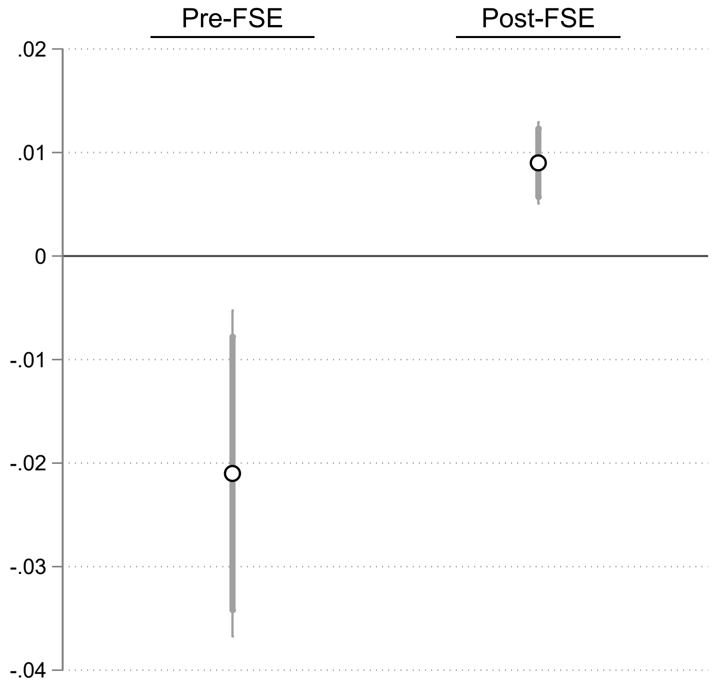
	]0, 1[	]0.3, 0.7[	]0, 1[	]0.3, 0.7[	]0, 1[	]0.3, 0.7[
Older sibling passed	-0.021***	-0.016*	-0.019**	-0.018**	-0.018**	-0.018*
	(0.008)	(0.009)	(0.008)	(0.008)	(0.008)	(0.010)
	[0.003]	[0.007]	[0.003]	[0.007]	[0.003]	[0.007]
Older sibling passed × Post-FSE	0.030***	0.020**	0.031***	0.023***	0.029***	0.022**
	(0.008)	(0.009)	(0.009)	(0.008)	(0.009)	(0.010)
	[0.003]	[0.007]	[0.003]	[0.007]	[0.003]	[0.008]
N	603,143	140,483	603,072	140,794	602,646	139,038
Mean (pre-FSE)	0.240	0.241	0.240	0.242	0.240	0.242
Younger school FE	✓	✓			✓	✓
Older school FE			✓	✓	✓	✓

Notes: Standard errors clustered by grade permutation in parentheses. Heteroskedasticity-robust Eicker-Huber-White standard errors in brackets. 'Post-FSE' is a dummy for whether the year is 2016 or later. Outcome measure – younger sibling's transition to secondary school – is only measured for students whose full name is unique across the country within their cohort. All regressions include fixed effects for younger sibling's cohort and older sibling's grade permutation. Windows of passing probabilities for each regression are indicated in the column titles. Significance levels: * <0.1, ** <0.05, *** <0.01

Figure 3 illustrates this effect visually using estimates from equation (2), with younger siblings' secondary transition as the outcome of interest. On the left, the figure displays the sibling spillover effect prior to FSE: the coefficient on β_1 from equation (2) (with confidence intervals at 90 and 95%). On the right, the figure displays the full spillover effect under FSE: the linear combination of $\beta_1 + \beta_2$ (with 90 and 95% confidence intervals). Cohort-specific estimates can be found in Table A.2 and Figure A.3.

(1), although results remain qualitatively similar.

Figure 3: Effect of older sibling's qualification on younger sibling's secondary school attendance



Notes: Coefficients and 95% confidence intervals from regressing younger sibling's secondary transition on a dummy for whether their older sibling passed the secondary school qualification exam, (conditional on being near the threshold), interacted with a dummy for being in the post-FSE period, as in equation (2). The estimate on the left shows the estimate of β_1, while the estimate on the right shows the linear combination of $\beta_1 + \beta_2$.

The magnitude of the sibling spillovers we identify are reasonably large, especially in the pre-FSE period. Using the estimates from column 1 in Table 5 (and plotted in Figure 3), we show that an older sibling's qualification for secondary school causes a 2.1 percentage point reduction in secondary transition for younger siblings — an effect of -9% (from a base of 24% average transition in the pre-FSE period). This translates to -0.05 standard deviations (σ). After FSE, we estimate that an older sibling's qualification increases younger siblings' transition by .09 percentage points, corresponding to an effect of 1.5% (from a base of 61% in the post-FSE period). This translates to 0.02 σ. Comparing these effects with the literature is difficult, as few well-identified studies exist which measure sibling spillovers on school enrollment, but we can compare our effect sizes with measures of sibling spillovers on years of schooling and test scores. Qureshi (2018a) finds in Pakistan that an additional year of schooling for older sisters raises younger brothers' schooling by 0.2 years, or 7% relative to the mean. Using historical data from Nepal, Shrestha and Palaniswamy (2017) find that a brother's eligibility for an educational program reduced female siblings' education by 0.12 years, an 8% decline. Studies from the developed world tend to measure spillover effects on test scores, as enrollment is nearly universal. Karbownik and Özek (2021) find that among impoverished families in the Florida district they study, having an older sibling be born after the school-entry cutoff (and hence be old rather than young in their cohort) caused a 0.15 σ increase in younger siblings' standardized test scores (they find no effect in richer families). Figlio et al. (2023) find that a grade retention policy which helped focal children also raised their younger siblings' test scores by 0.05-0.06 σ. Qureshi (2018b) shows that the reading experience of older siblings' teachers increases younger siblings' reading test scores by 0.010-0.013 σ. The effects we measure are of a similar magnitude to these effects from the literature.

We can also compare the effect sizes we measure with the distribution of published effect sizes in international education studies reviewed by Evans and Yuan (2022). The -0.05 σ effect we identify in the pre-FSE period ranks below the 10th percentile of published effect sizes on enrollment. In the post-FSE period, the 0.02 σ effect we estimate ranks at the 25th percentile. Given that the effect sizes included in this review are direct effects on focal children, not sibling spillovers, we consider that the sibling spillover effects we identify are of an economically meaningful magnitude.

4.1 Robustness

Table 6 shows that these estimates are robust to the inclusion of controls and to alternative choices over matching and estimation. Column 1's regression uses the same estimation and sample as Column 1 in Table 5, adding controls for both siblings' gender and the age gap between siblings. Estimates on the main coefficients of interest are virtually unchanged. Columns 2 and 3 show that restricting the sample to individual younger siblings (rather than sibling pairs) by considering only the most recent PSLE-taker among each student's older siblings has similarly little effect on the estimates. Columns 4-7 show that estimates using alternative sibling matching methods — matching siblings within school rather than within ward, and matching on middle and last name — produce similar estimates as well. Appendix Section B provides more detail on these robustness checks.

Table 6: Robustness: Effect of older sibling passing on younger sibling's transition, interacted with post-FSE indicator, for alternative samples

	Controls	Most recent sibling		Matching at school level		Matching with middle name	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Old sib. pass	-0.021***	-0.022***	-0.023***	-0.020**	-0.020**	-0.012**	-0.012**
	(0.008)	(0.007)	(0.007)	(0.009)	(0.009)	(0.006)	(0.006)
Pass × Post-FSE	0.031***	0.031***	0.031***	0.029***	0.029***	0.022***	0.022***
	(0.009)	(0.008)	(0.008)	(0.011)	(0.011)	(0.006)	(0.006)
N	603,143	410,849	410,849	668,300	668,300	329,487	329,487
Mean (pre-FSE)	0.240	0.237	0.237	0.239	0.239	0.282	0.282
Controls	✓		✓		✓		✓

Notes: Standard errors clustered by grade permutation in parentheses. Unit of observation is a younger-older sibling pair. 'Transition' is only measured for students whose full name is unique across the country within their cohort. All regressions include fixed effects for younger sibling's school and PSLE cohort, and older sibling's grade permutation. The sample includes all older siblings whose grade permutation was associated with a probability of passing the PSLE between — and not including — 0 and 1. Controls include indicators for female and Muslim (inferred from name) for older and younger siblings, and the sibling pair's age gap as measured as the years between cohorts. Significance levels: * <0.1, ** <0.05, *** <0.01

5 Mechanisms

While comparing younger siblings of students near the exam passing threshold permits the causal identification of sibling spillovers, thus far we have made only descriptive comparisons between effects in the pre- vs. post-FSE periods. A limitation of our paper is that our data do not allow us to measure parental investments (or sibling interactions) directly. However, in this section we present five pieces of evidence that FSE had a causal effect on sibling spillovers, alleviating resource constraints so parents could compensate rather than reinforce ability differences among children. First, and most importantly, we show evidence that FSE caused the temporal difference in sibling spillovers using a triple-difference strategy which leverages ward-level variation in exposure to FSE (as proxied by dropout rates). Next, we present results from a family-fixed-effects estimation to show that spillover effects became significantly more positive for post-FSE siblings even within families. We then present heterogeneous treatment effects by poverty, gender, and students' *ex-ante* ability.

5.1 Triple difference by FSE treatment exposure

To test whether FSE played a causal role in changing sibling spillovers, we compare the evolution of these spillovers in places which stood to be more vs. less affected by FSE. We consider areas with high *ex-ante* primary-to-secondary dropout rates to have been more intensely treated by FSE. This is because areas with low dropout rates — equivalent to high transition rates — were those in which more students were able to make the transition to secondary school even in the absence of FSE. The approach shares the intuition of Lucas and Mbiti (2012), who use a similar strategy to study the effect of Kenya's free primary education program. We measure ward-level dropout rates prior to FSE in the first year of our analysis (2014), and designate wards above the median dropout rate as "high-dropout" — and hence more exposed to the FSE treatment. Sandholtz (2024b) shows that primary school achievement rose disproportionately in these areas after the introduction of FSE, reaffirming the value of dropout rates as a measure of FSE treatment exposure.

Equation (3) articulates our estimation strategy:

$$\begin{aligned} y_{iogstw} =& \beta_1 PSLEpass_o + \beta_2 PSLEpass_o \times HighDropout_w + \beta_3 PSLEpass_o \times Post_t \\ &+ \beta_4 HighDropout_w \times Post_t + \beta_5 PSLEpass_o \times HighDropout_w \times Post_t + \gamma_g + \rho_s + \tau_t + \varepsilon_{iogstw} \end{aligned} \quad (3)$$

As before, y_{iogstw} stands for the outcome of interest (secondary transition) for younger sibling i of older sibling o with grade permutation g in school s and cohort t. We also include a ward subscript w as dropout rates are measured at the ward level, with $HighDropout_w$ indicating that the younger sibling's school is in a ward with an above-median dropout rate in 2014. School-level fixed effects ρ_s absorb the ward fixed effect, and cohort-level fixed effects τ_t absorb the $Post_t$ dummy. Our coefficient of interest is β_5, which measures the interaction effect of $PSLEpass_o \times HighDropout_w \times Post_t$.

Table 7 displays the coefficients from Equation (3). Prior to FSE, sibling spillovers were significantly more negative in high-dropout wards (though still negative even in low-dropout wards). After FSE's introduction, this relationship flipped: sibling spillovers became *more* positive in high-dropout wards than in low-dropout wards. Appendix Figure A.4 visualizes these estimates, plotting the overall effect of having an older sibling pass the PSLE before and after FSE's introduction, for students in high- and low-dropout wards.

Table 7: Effect of older sibling passing on younger sibling's transition, interacted with post-FSE indicator and ex-ante ward dropout rate

	(1)
Older sibling passed PSLE	-0.020**
	(0.008)
Older sibling pass × High-dropout ward	-0.012**
	(0.005)
Older sibling pass × Post-FSE	0.028***
	(0.008)
High-dropout ward × Post-FSE	0.091***
	(0.004)
Pass × High-dropout × Post-FSE	0.018***
	(0.006)
Pass + Pass × High	-0.032***
	(0.009)
Pass + Pass × Post	0.007***
	(0.002)
Pass + Pass × High + Pass × Post + Pass × High × Post	0.013***
	(0.003)
N	600,235
Mean (pre-FSE)	0.240

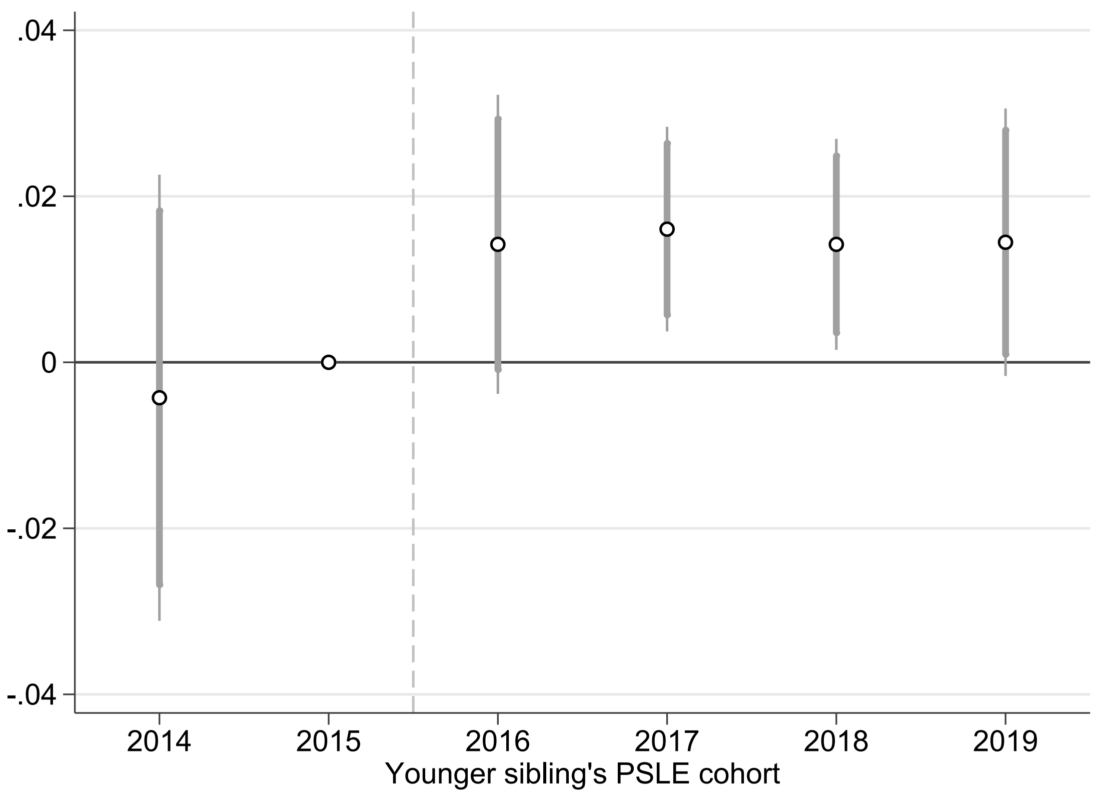
Notes: Standard errors clustered by grade permutation in parentheses. 'Transition' is only measured for students whose full name is unique across the country within their cohort. 'High-dropout ward' indicates the student took the PSLE at a school located in a ward with a below-median transition rate in 2014. All regressions include fixed effects for (younger sibling's) school and cohort and (older sibling's) grade permutation. The sample includes all older siblings whose grade permutation was associated with a probability of passing the PSLE between — and not including — 0 and 1. Significance levels: * <0.1, ** <0.05, *** <0.01

The fact that the reversal in sibling spillovers was most dramatic in places more affected by FSE suggests that FSE played a causal role in this reversal. However, the identifying assumption in ascribing a causal interpretation to this relationship is that in the absence of FSE, the difference in differences between the younger siblings of PSLE passers and failers near the threshold in high- vs. low-dropout wards would have continued on its existing trajectory. This can be tested by examining whether these trends are parallel in the pre-FSE period. Equation (4) is a dynamic version of equation (3), interacting $PSLEpass_o$ and $HighDropout_w$ with year dummies τ_t rather than a simple $Post_t$ dummy. This enables the testing of how these interactions change over time.

$$
\begin{aligned}
y_{iogstw} =& \beta_1 PSLEpass_o + \beta_2 PSLEpass_o \times HighDropout_w \\
&+ \sum_{t=2014}^{2019} \beta_{3t}\, PSLEpass_o \times \tau_t + \sum_{t=2014}^{2019} \beta_{4t}\, HighDropout_w \times \tau_t \\
&+ \sum_{t=2014}^{2019} \beta_{5t}\, PSLEpass_o \times HighDropout_w \times \tau_t + \gamma_g + \rho_s + \tau_t + \varepsilon_{iogstw} \qquad (4)
\end{aligned}
$$

Figure 4 plots the β_{5t} coefficients from equation (4), measuring how the achievement gap between younger siblings of PSLE passers and failers near the threshold changed in high- vs. low-dropout wards by cohort:

Figure 4: Sibling spillovers in high- vs. low-dropout wards, by cohort



Notes: Coefficients from regressing younger siblings' secondary school transition on a triple interaction of year dummies with an above-median 2014 ward dropout rate dummy and older sibling's PSLE pass result (conditional on having a passing probability in the open interval]0,1[). Triple interaction coefficients shown, with 2015 as the omitted year. Confidence intervals at 95% and 90% shown.

In the years prior to the introduction of FSE, the gap in differences between high- and low-dropout wards remained constant, becoming significantly positive (and relatively constant) after FSE was implemented. This strengthens our confidence that FSE caused the reversal in sibling spillovers we measure.

5.2 Family fixed effects

Another way to shed light on whether FSE caused the reversal in sibling spillover effects is to look at effects within families. By including family fixed effects, we can hold constant everything about a family's time-invariant characteristics, isolating only how the effect of an older sibling passing the PSLE changed for younger siblings within the same family who took the PSLE before vs. after the introduction of FSE. This is valuable because FSE may have changed which older students (and families) were on the margin of secondary school qualification. If the post-FSE increase in sibling spillover effects survives the inclusion of family fixed effects, this implies that the change happened even within families and was not driven merely by students at different points in the distribution responding differently to their older sibling's qualification.

For this exercise, we consider families in which one older sibling sat the PSLE in 2013 or 2014, at least one younger sibling sat the exam in 2014 or 2015 (before FSE), and at least one younger sibling sat the exam in 2016 or later (after FSE). We take the unit of analysis to be an individual student, classifying each student exclusively as either an older sibling (if they are the first sibling in their family to sit PSLE and do so in 2013 or 2014) or a younger sibling. As before, we limit our measurement of outcomes to those of younger siblings, using older siblings only to define treatment (passed PSLE or not); unlike before, here no individual appears more than once in the data. As before, we limit attention to groups of siblings of 6 or less to reduce the incidence of false positives.

Equation (5) describes the estimation framework. Family fixed effects ϕ_f absorb both the school fixed effects and the older sibling grade permutation effects used in previous estimations, as well as the main effect $PSLEpass_o$, such that the coefficient of interest is simply β_1 on the interaction $PSLEpass_o \times Post_t$, which tells us how the effect of having an older sibling changed — within families — after the introduction of FSE.

$$y_{ioft} = \beta_1 PSLEpass_o \times Post_t + \phi_f + \tau_t + \varepsilon_{ioft} \tag{5}$$

Table 8 displays the coefficients on β_1 from equation (5), for samples in which the older sibling's PSLE score corresponded to a passing probability in the open intervals]0,1[,]0.15,0.85[, and]0.3,0.7[.

Table 8: Family fixed effects: sibling spillovers on secondary transition, after vs. before FSE

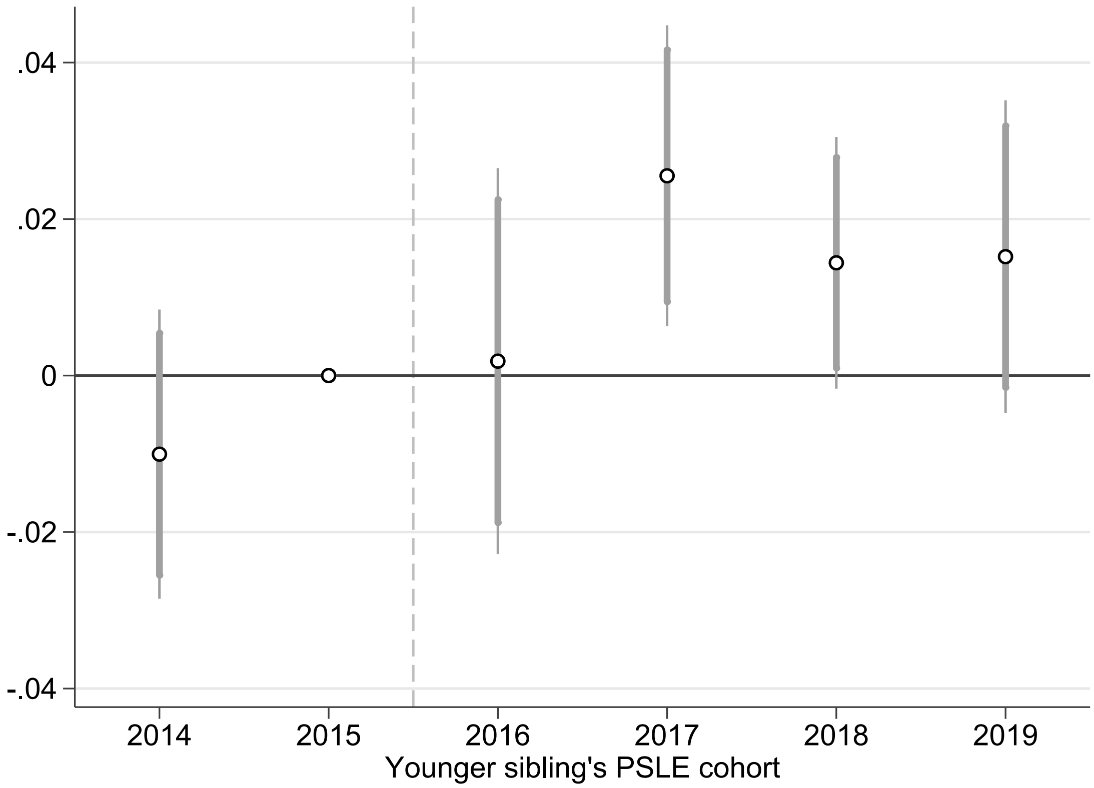
	]0, 1[	]0.15, 0.85[	]0.3, 0.7[
Older sibling passed PSLE × Post-FSE	0.018***	0.028***	0.028**
	(0.006)	(0.009)	(0.011)
N	91,289	37,413	24,599
Mean (pre-FSE)	0.241	0.242	0.241
Older sibling's score FE	Yes	Yes	Yes
School FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes

Notes: Standard errors clustered by grade permutation in parentheses. 'Post-FSE' is a dummy for whether the year is 2016 or later. Outcome is whether younger student transitioned to secondary school as measured by appearing in Grade 9 exam data; defined only for students whose full name is unique across the country within their cohort. All regressions include family (Last name × ward) fixed effects. 'Year FE' denotes binary indicators for the PSLE cohort of each younger sibling. Column titles denote the (open) interval of older sibling grade permutation passing probabilities included in the regression. Significance levels: * <0.1, ** <0.05, *** <0.01

The interaction term is positive and significant for all windows, indicating that within families in which some younger siblings sat the PSLE before and some after the introduction of FSE, the effect of having an older sibling pass on younger siblings' transition was larger after FSE's introduction. As before, interpreting these estimates causally requires the assumption that the difference in outcomes for younger siblings of PSLE passers and failers would have remained constant in the absence of FSE. By interacting $PSLEpass_o$ with younger-sibling-cohort-dummies, as in equation (6), we can test this assumption.

$$y_{ioft} = \sum_{t=2014}^{2019} \beta_t \, PSLEpass_o \times \tau_t + \phi_f + \tau_t + \varepsilon_{ioft} \tag{6}$$

Figure 5: Sibling spillover effects on secondary transition over time, with family fixed effects



Notes: Coefficients from regressing younger siblings' secondary school transition on an interaction of year dummies with older sibling's PSLE pass result (conditional on being near the threshold), including family fixed effects. Interaction coefficients shown. Confidence intervals at 95% and 90% are shown.

Figure 5 shows that the coefficient on the interaction of interest did not change significantly prior to FSE, growing significantly larger only in the post-FSE period. (Figure A.5 in the appendix shows that the analysis looks similar when using 3 progressively narrow windows of passing probabilities for the older sibling's PSLE score.) This provides more evidence that FSE was responsible for the reversal of the sign in sibling spillovers. It also suggests the sibling spillover sign reversal we identify is not driven by changes in the composition of families at the margin of secondary school qualification.

5.3 Heterogeneity by poverty

To provide further evidence on whether FSE reversed the sign of sibling spillovers by alleviating resource constraints, we examine heterogeneity by socioeconomic status, using a district-level measure of poverty. Recent empirical evidence from Tanzania shows that many households struggle to save, and financial constraints are binding for household educational investments (Carroll et al., 2023; Burchardi et al., 2024). Although FSE reduced the cost of secondary education, transportation and other associated costs remained — as did the opportunity cost. Even if the abolition of school fees were perfectly enforced, school fees represented only a part of the cost of school attendance. However, FSE did succeed in its direct aim, with its effect showing up in household survey data as a ≈70% reduction in reported expenditure on school fees for public secondary school students (Sandholtz, 2024b). For families with a marginal ability to send students to secondary school, this may have been sufficient to shift the optimal strategy from "quality" to "quantity" — in other words, to invest in all siblings' education rather than concentrating resources on one promising potential "winner." For families who were still far from being able to send many kids to secondary school even after the abolition of school fees, however, FSE would have no expected effect on the allocation of educational resources among children. If negative sibling spillovers are concentrated among poorer families (and/or positive spillovers are concentrated among richer ones), that would be consistent with FSE alleviating resource constraints for families on the margin.

While our administrative data do not provide family-level measures of socioeconomic status, we can proxy for families' income levels using census data at the district level. We use data on the proportion of homes with grass- or leaf-thatched roofs, reported for each district in the 2012 Population and Housing Census, as a proxy for local poverty. This is a common poverty measure, used for example by means-tested programs elsewhere in the region (Egger et al., 2022). Specifically, we code districts with above-median prevalence of grass- or leaf-thatched roofs as 'poorer districts.' We then estimate equation (1) with an additional interaction term between $PSLEpass_o$ and this poverty indicator (the latter variable is not separately included in the estimated model since it is time-invariant for each district and thus collinear with school fixed effects). Results can be found in Table 9.

We find that before the introduction of FSE, negative sibling spillovers on secondary transition and exam pass rates were significant only for pupils in poorer districts, and positive spillovers on high scores appear only in richer districts. After the reform, spillover effects on secondary transition are significantly smaller in richer districts. While district-level measures of poverty can capture household-level socioeconomic well-being only coarsely, the pattern we observe suggests that FSE's alleviation of resource constraints is an important mechanism for the reversal of sibling spillovers we observe.

Table 9: Heterogeneity by proxy for district-level poverty

	Pre-FSE			Post-FSE		
	(1) Transition	(2) PSLE Pass	(3) High score	(4) Transition	(5) PSLE Pass	(6) High score
Older sib. pass	-0.006	-0.008	0.011***	0.011***	0.005**	0.010***
	(0.006)	(0.005)	(0.004)	(0.002)	(0.002)	(0.002)
Pass × Poorer district	-0.007	-0.009	-0.015***	-0.007***	-0.002	-0.003
	(0.006)	(0.006)	(0.004)	(0.002)	(0.002)	(0.002)
Pass + Pass × Poorer	-0.013**	-0.017***	-0.003	0.004*	0.003*	0.007***
	(0.005)	(0.005)	(0.005)	(0.002)	(0.002)	(0.002)
N	84,035	84,730	84,730	517,117	521,738	521,738
Mean dep. var. (Poorer)	0.217	0.584	0.152	0.562	0.728	0.234
Older sibling's score FE	Yes	Yes	Yes	Yes	Yes	Yes
School FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Standard errors clustered by grade permutation in parentheses. 'Transition' is only measured for students whose full name is unique across the country within their cohort. 'High score' is a binary variable equal to one if the younger sibling got an A or B average on the PSLE, and zero otherwise. 'Poorer district' is proxied by living in an above-median district in terms of roofs made of grass/leaves, measured as of the 2012 Population and Housing Census. All regressions include (younger sibling's) school and (older sibling's) grade permutation fixed effects. 'Year FE' denotes binary indicators for the PSLE cohort of each younger sibling. The sample includes all older siblings whose grade permutation was associated with a probability of passing the PSLE between — and not including — 0 and 1. Significance levels: * <0.1, ** <0.05, *** <0.01

A related question is how these effects vary by family size (which in our context, we can only measure as the number of siblings sitting the PSLE during the window we observe). Resource constraints are likely to be more binding when there are more siblings among whom resources may potentially be divided. We show in Table A.3 that the positive sibling spillovers we estimate in the post-reform period are limited to smaller families.[10]

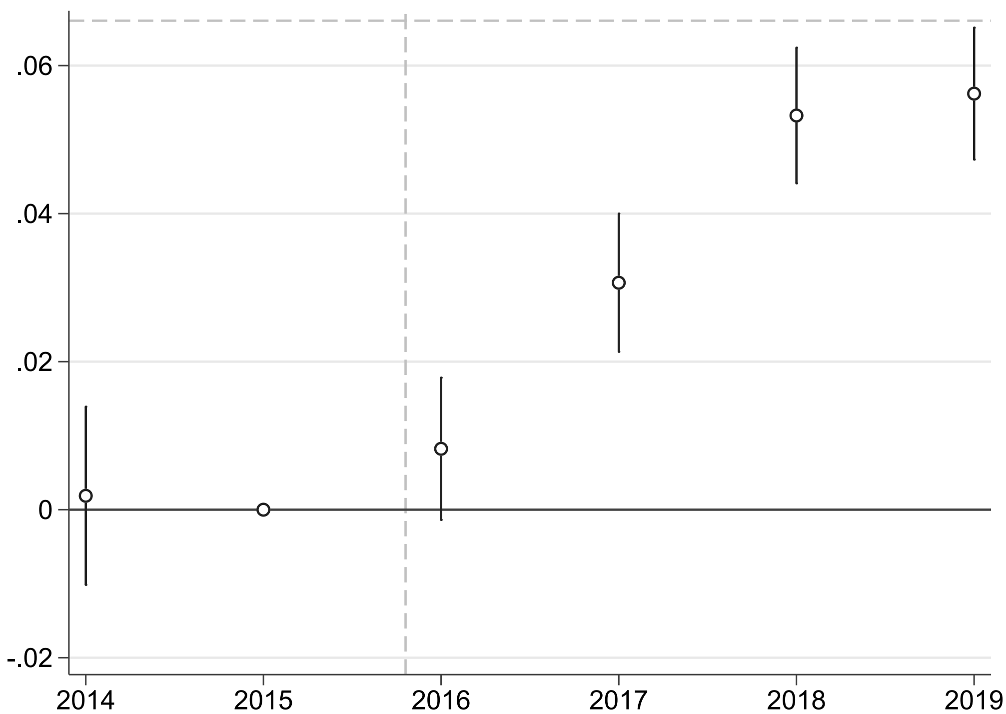
5.4 Heterogeneity by gender

We next examine how sibling spillovers vary by gender. In descriptive work on sibling rivalry using household survey data from Tanzania, Morduch (2000) shows that children with a greater share of female siblings have higher educational attainment, which he interprets as consistent with a parental preference to invest in sons when resources are scarce. The administrative data in our sample are consistent with this interpretation – girls are 6.6 percentage points less likely to pass the PSLE than boys prior to FSE, even controlling for family fixed effects. Figure 6 plots the gender gap by cohort, considering individuals (not pairs) from our sample of students with at least one older sibling. The gender gap is characterized by

[10]The existence of differential effects in urban and rural areas could be of related interest. To test this possibility, we code each ward as 'urban', 'mixed', or 'rural' according to its Census classification. We do not find evidence of differently-sized spillover effects in urban wards as compared to non-urban wards (see Table A.4).

regressing a dummy for whether the student passed the PSLE on a female indicator, interacted with year dummies, including family and year fixed effects (with 2015 as the omitted year):

Figure 6: Gender gap in PSLE pass rates, over time



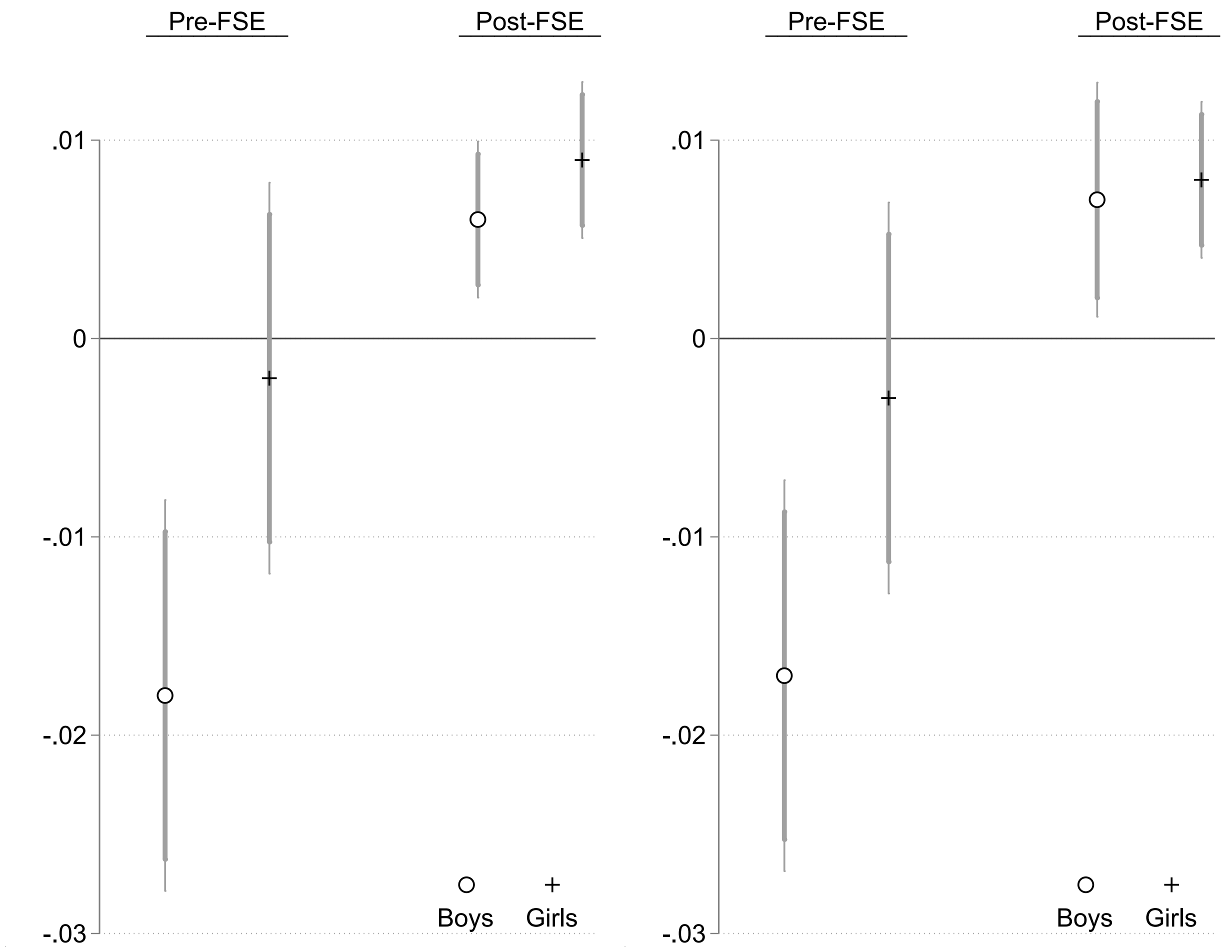
Notes: This plot takes individual PSLE takers (not sibling pairs) as the unit of observation. The plot shows interaction coefficients and 95% confidence intervals from regressing PSLE passing on an indicator for whether the student is female, year dummies, and the gender $\times$ year interaction, with 2015 as the omitted year, family fixed effects, and standard errors clustered at the ward level. The dotted line on the y-axis at .066 indicates the absolute value of the gender gap in PSLE pass rates in 2015 (in favor of boys).

The dotted line at .066 indicates that girls were 6.6 percentage points less likely than boys to pass the PSLE in 2015 (prior to FSE), even considering family fixed effects, and that gap was constant in the pre-FSE years in our sample. After the introduction of FSE, this gap narrowed sharply, nearly disappearing by 2019. This pattern suggests that girls received less investment within families than boys did, especially prior to FSE.

To examine how sibling spillovers vary by gender, we expand equation (2) to include an indicator for whether the younger sibling is female, as well as an interaction between that indicator and our main independent variable ('Older sibling passed the PSLE'). Figure 7 shows the results, with *Transition* as the dependent variable. (In the appendix, Table A.5 shows results from a complementary analysis which interacts younger sibling gender and older sibling gender, separately before and after the introduction of

FSE.)

Figure 7: Heterogeneity in sibling spillovers by gender of younger and older sibling



(a) Younger sibling's gender

(b) Older sibling's gender

Notes: Coefficients from regressing younger siblings' secondary school transition on an interaction between a dummy for the older sibling passing the PSLE (conditional on being near the threshold) and a dummy for (panel a) the younger sibling being female or (panel b) the older sibling being female. Regressions carried out separately for the pre-FSE and post-FSE period, in each case, as in equation (1). Confidence intervals at 95% and 90% are shown.

We find that in the pre-FSE period, negative spillovers are concentrated among boys. This is true whether we consider the younger sibling's gender (panel a) or the older sibling's gender (panel b), although the effect sizes are much smaller than the gender gap favoring boys. After FSE's introduction, sibling spillovers are positive for children of all genders. Together with Figure 6, this paints a picture of gendered investment dynamics. Prior to FSE, resource-constrained families appear to have focused investments on boys, with brothers competing for scarce resources and girls more likely to be neglected altogether.[11] After the introduction of FSE, the gender gap in PSLE achievement narrowed considerably, and the gender gap in spillovers on secondary transition disappeared completely, consistent with an equalization of resources across children of different genders. This may indicate that FSE allowed families to shift away from a strategy of "picking winners" (usually boys), which freed up household resources to be invested in girls. This provides further suggestive evidence that FSE affected sibling spillovers by alleviating families' resource constraints.

5.5 Heterogeneity by prior academic achievement

Finally, we test whether the students who benefit most from their older siblings' academic achievement are those younger siblings with high or low *ex-ante* measures of ability. This sheds light on whether parents practice a strategy of "compensating" or "reinforcing."

In order to have a measure of younger siblings' prior academic achievement, we link PSLE data to results from the earliest national standardized test students take in primary school: the Standard Four National Assessment (SFNA), taken at the end of students' fourth year of primary school, three years before the PSLE. Unlike the PSLE, it does not determine pupils' opportunities for further schooling. But it does provide an *ex-ante* measure of student ability. Unfortunately, SFNA information is only available from 2015 onward, so we are only able to consider younger siblings in cohorts who sat the PSLE in the post-FSE period – 2018 and 2019 (i.e., those who were part of the 2015 and 2016 SFNA cohorts, respectively). We use the same matching method used to link PSLE to FTNA, limiting attention to nationally-unique full-name matches between individuals who sat the PSLE in year t and pupils who took the SFNA in year $t-3$. We are able to match just short of 50% of our sample of 2018 and 2019 PSLE takers to a score on the SFNA.

We estimate an expanded version of equation (1) that includes a binary indicator for whether younger sibling i achieved a low grade (C or worse) on the SFNA, and interacts that indicator with $PSLEpass_o$, our measure of older siblings' academic achievement.

[11] Sandholtz (2024b) presents suggestive evidence that the most important margin of investment may have been simply sending children to school rather than work.

Table 10: Heterogeneity by younger siblings' prior academic achievement

	(1) Transition	(2) PSLE Pass	(3) High score
Older sibling passed PSLE	-0.018***	-0.011**	0.021***
	(0.006)	(0.005)	(0.006)
Low ability younger sibling	-0.222***	-0.183***	-0.356***
	(0.005)	(0.004)	(0.007)
Older pass × Younger low ability	0.028***	0.027***	-0.019***
	(0.006)	(0.005)	(0.007)
Pass + Pass × Low ability	0.010**	0.016***	0.001
	(0.004)	(0.003)	(0.003)
N	122,566	123,560	123,560
Mean dep. var.	0.639	0.786	0.298
Older sibling's score FE	Yes	Yes	Yes
School FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes

Notes: Standard errors are clustered by grade permutation. 'Transition' is only measured for students whose full name is unique across the country within their cohort. 'High score' is a binary variable equal to one if the younger sibling got an A or B average on the PSLE, and zero otherwise. 'Low ability' refers to an average grade below B (A-E scale) on the Standard Four National Assessment, an exam taken three years before the PSLE. All regressions include (younger sibling's) school and (older sibling's) grade permutation fixed effects. We only have information for the 2015 and 2016 SFNA, which we link to the 2018 and 2019 PSLE cohorts. 'Year FE' denotes binary indicators for the PSLE cohort of each younger sibling. The sample includes all older siblings whose grade permutation was associated with a probability of passing the PSLE between — and not including — 0 and 1. Significance levels: * <0.1, ** <0.05, *** <0.01

Table 10 shows that in the post-FSE period, the benefits more from exposure to an older sibling's achievement accrued to lower-ability younger siblings. Positive spillover effects on the probabilities of primary-to-secondary school transition and of passing the PSLE are driven by students who achieved a grade of C or lower on the SFNA. Meanwhile, spillovers are negative for higher-ability younger siblings. A marginal older sibling's qualification for secondary school (still costly even without school fees) leads to worse performance for higher-ability younger students and better performance for lower-ability younger students, such that spillover effects mitigate preexisting differences. These results are consistent with compensatory parental investments aimed at equalizing educational outcomes across children (Berry et al., 2024).[12] We are unable to distinguish between this and other mechanisms such as sibling role model effects, though the divergent effects by younger sibling ability are more suggestive of parental compensating investments than role model effects.

[12]This also implies at some level of parental knowledge about children's relative abilities, despite evidence from other contexts that parents' beliefs about their children's abilities are often inaccurate (Dizon-Ross, 2019; Cunha et al., 2020).

5.6 Other alternative mechanisms

We consider two additional potential mechanisms: the extensive margin of whether students sat the exam at all, and school selection. There is evidence that Tanzanian schools in this period sometimes strategically excluded low-performing students from being allowed even to sit the PSLE (Cilliers et al., 2021). To test whether our results might be partly explained by changes to the composition of which younger siblings sit the exam, we measure the effect of an older sibling marginally passing the PSLE on the probability of having any younger sibling sit the exam. We find no evidence in favor of this hypothesis (see Table A.6). (We are not able to test for possible effects of the FSE reform on fertility itself; we observe students when they sit the PSLE, around age 13, and our window of data availability is only 6 years, so no birth cohorts from after FSE's implementation are in our sample. For the same reason, we are not able to test the effects of secondary school qualification on fertility.)

Other work on sibling spillovers has found an important role for school selection (Figlio et al., 2023; Dustan, 2018). In Table A.7 we test for effects on whether younger siblings sit the exam at a different school than the older sibling, and whether they do so at a higher-quality school than the older sibling (as measured by ex-ante pass rates). We find no evidence of effects on school quality, and only a small negative effect on taking the exam at a different school from one's siblings in the post period, making it unlikely that school selection could drive our results.

6 Conclusion

In this paper, we study sibling spillover effects and how they responded to a major policy reform: the nationwide abolition of public secondary school tuition fees in Tanzania (Free Secondary Education, or FSE). We draw on the universe of administrative data from national standardized test results. We compare the outcomes of the younger siblings of students who narrowly passed vs. failed the secondary school qualifying exam. Before FSE was implemented, we identify negative spillovers of an older sibling's educational attainment on younger siblings' Primary School Leaving Examination pass rates and secondary enrollment rates. After the introduction of FSE, the sign on both these effects was reversed. These results are robust to a wide variety of alternative estimation choices, and a triple-differences design leveraging variation in FSE exposure shows that FSE caused the reversal. Heterogeneity analyses suggest that FSE's alleviation of financial constraints played an important role. We also show evidence that in the post-reform period, it was the weakest students who benefited most from having a sibling qualify for secondary school.

This suggests that in the presence of FSE, parents preferred to compensate lower-ability children rather than reinforce the advantages of higher-ability children.

These findings are relevant for assessing the ongoing expansion of access to secondary education in the developing world. The politics of improving education access and quality are not always straightforward. There is some evidence that voters reward politicians commensurately with school quality (Cox et al., 2024; Biasi and Sandholtz, 2024; Dias and Ferraz, 2019). Other work, meanwhile, finds negative electoral effects of policies to improve schools (Boas et al., 2021; Sandholtz, 2023, 2024a). One benefit of schooling expansions is that they tend to be politically popular (Crawfurd, 2024). However, some worry that they will exacerbate inequality by functioning as a regressive transfer (Lewin, 2009; Davoodi et al., 2003; Malala Fund, 2016; Wokadala and Barungi, 2015). Our paper shows that in fact these policies might reduce inequality — at least within households. Benefits to younger siblings, which begin to accrue long before they will be eligible for secondary school, should be considered along with the benefits to focal children when weighing the cost effectiveness of free schooling expansions.

Our results show that expansions in public schooling access — which lower the cost of educating children — can enable families on the margin to alter patterns of intra-household resource allocation. Existing literature on sibling spillovers tends to find positive effects in developed countries and negative effects in many (though not all) developing countries. Our paper helps to reconcile these divergent results, especially in light of existing empirical evidence on the context-specific nature of findings in the peer effects literature more broadly (Sacerdote, 2014). Inequality aversion within households may be a luxury good.

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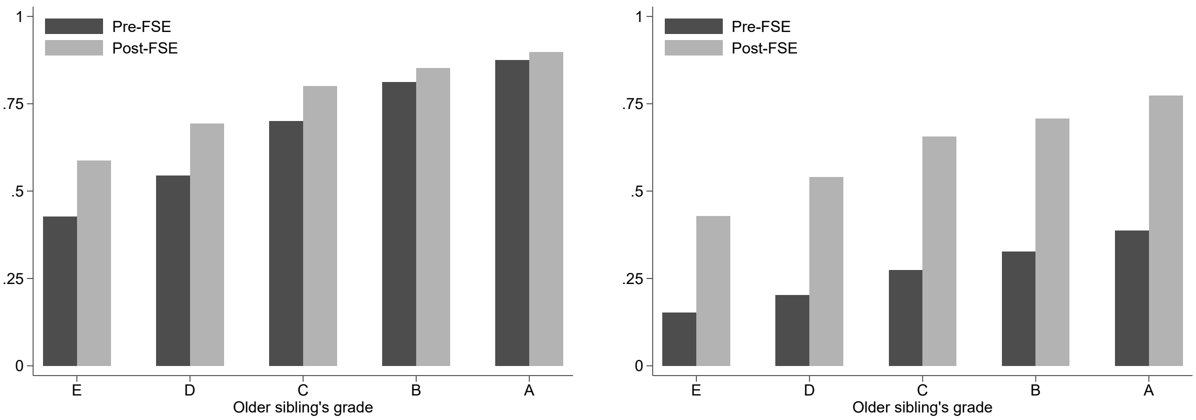
A Additional tables and figures

A.1 Descriptives

Figure A.1: Descriptive: younger siblings' outcomes, by older siblings' grades

(a) Pass PSLE (b) Transition to secondary



Averages computed using analysis sample. Correlation between older and younger siblings' PSLE pass status is .2 before FSE and .15 after FSE. Correlation between older siblings' PSLE pass status and younger siblings' transition status is .1 before FSE and .14 after FSE. Correlation between older and younger siblings' transition status is .13 before FSE and .12 after FSE.

Figure A.2: PSLE letter grade combination and probability of passing

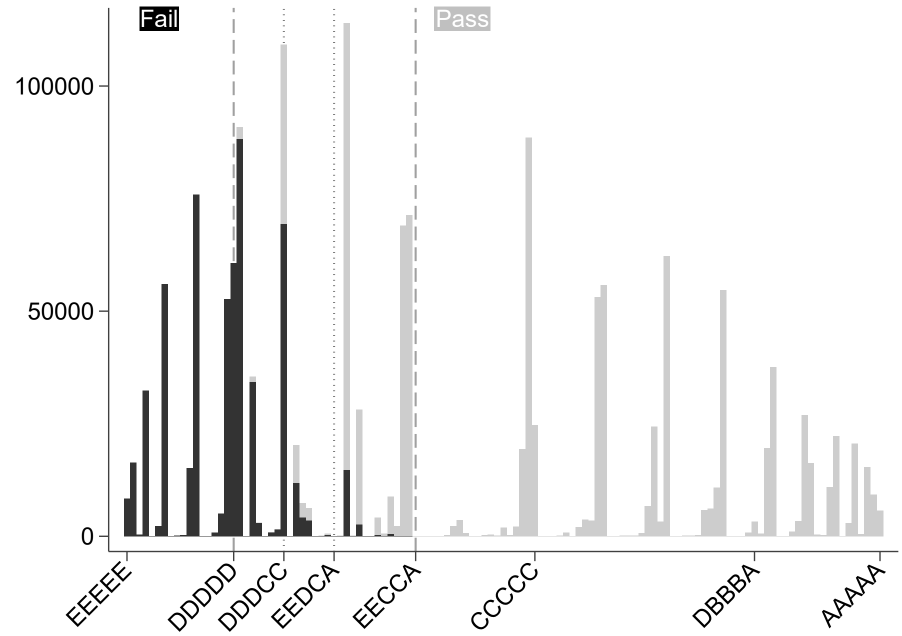


Table A.1: Summary statistics

	2013	2014	2015	2016	2017	2018	2019
Primary schools							
Number of schools	15,656	15,867	16,096	16,350	16,575	16,826	17,047
Public schools (proportion)	0.962	0.964	0.961	0.955	0.952	0.945	0.940
PSLE							
Number of exam takers	867,983	808,085	775,273	795,740	916,885	957,893	947,071
Female (proportion)	0.525	0.532	0.534	0.531	0.528	0.525	0.524
Pass rate (proportion)	0.493	0.559	0.668	0.698	0.722	0.766	0.803
FTNA							
Transition rate (proportion)	0.161	0.177	0.270	0.453	0.620	0.640	0.654
Preferred sample							
Number of older siblings	74,830	76,419	53,468	51,779	36,544	16,310	—
Number of younger siblings	—	27,810	55,893	73,715	104,672	125,631	135,415
Urban (proportion)	—	0.187	0.186	0.196	0.182	0.181	0.185

Notes: the proxy for primary-to-secondary-school transition (obtained using FTNA data) is explained in Subsection 2.2. Our preferred sample is created through ward-level sibling matching, as described in Section 3. 'Number of older [younger] siblings' is a count of individuals, not sibships.

A.2 Cohort-wise spillover effects

$$y_{iogst} = \sum_{t=2014}^{2019} \beta_t \, PSLEpass_o \times \tau_t + \gamma_g + \rho_s + \tau_t + \varepsilon_{iogst} \quad (7)$$

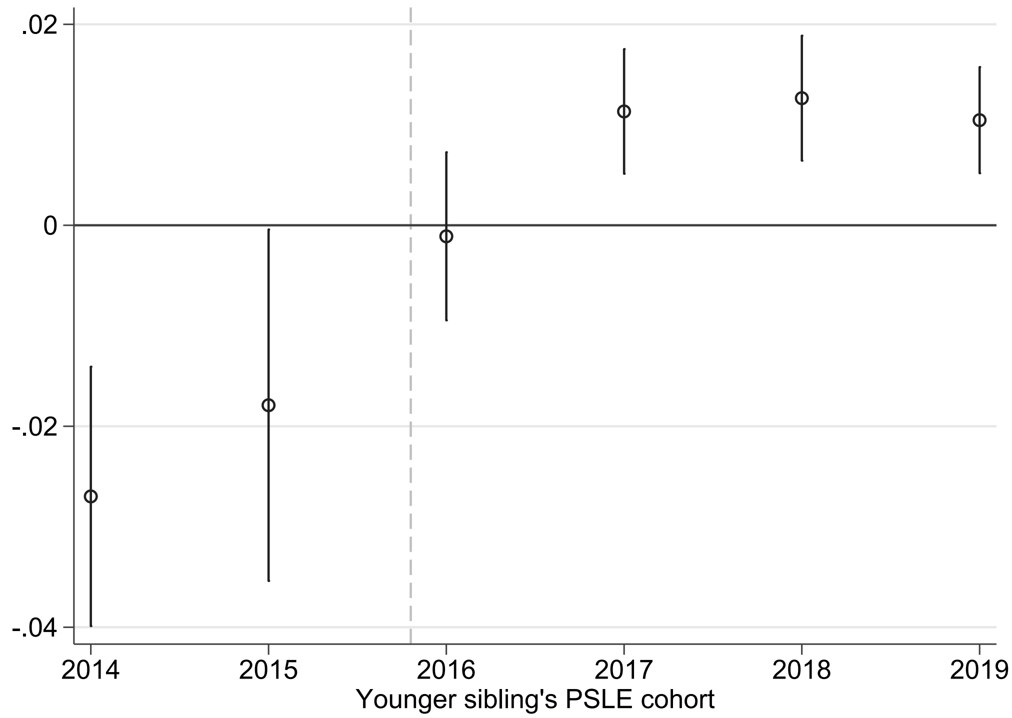
Table A.2: Spillover effects from older siblings' achievement (year-by-year)

Matching:	(1) Ward level	(2) School level	(3) Middle name
Younger sibling's PSLE cohort = 2015	0.093***	0.095***	0.098***
	(0.004)	(0.005)	(0.008)
Younger sibling's PSLE cohort = 2016	0.261***	0.259***	0.264***
	(0.006)	(0.005)	(0.008)
Younger sibling's PSLE cohort = 2017	0.423***	0.409***	0.392***
	(0.008)	(0.009)	(0.008)
Younger sibling's PSLE cohort = 2018	0.450***	0.438***	0.416***
	(0.007)	(0.009)	(0.007)
Younger sibling's PSLE cohort = 2019	0.465***	0.457***	0.436***
	(0.007)	(0.009)	(0.008)
Older sibling passed the PSLE x (Younger's cohort = 2014)	-0.027***	-0.021**	-0.027***
	(0.007)	(0.009)	(0.009)
Older sibling passed the PSLE x (Younger's cohort = 2015)	-0.018**	-0.020*	-0.007
	(0.009)	(0.010)	(0.006)
Older sibling passed the PSLE x (Younger's cohort = 2016)	-0.001	0.002	-0.000
	(0.004)	(0.004)	(0.006)
Older sibling passed the PSLE x (Younger's cohort = 2017)	0.011***	0.014***	0.014***
	(0.003)	(0.003)	(0.005)
Older sibling passed the PSLE x (Younger's cohort = 2018)	0.013***	0.011***	0.012***
	(0.003)	(0.003)	(0.005)
Older sibling passed the PSLE x (Younger's cohort = 2019)	0.010***	0.009**	0.008
	(0.003)	(0.003)	(0.005)
N	603143	668300	329487
Mean dep. var.	0.555	0.539	0.568
R^2	0.215	0.201	0.200
Older sibling's score FE	Yes	Yes	Yes
School FE	Yes	Yes	Yes

Notes: Standard errors clustered by grade permutation in parentheses. 'Transition', the dependent variable, is only measured for students whose full name is unique across the country within their cohort. All regressions include (younger sibling's) school and (older sibling's) grade permutation fixed effects. The sample includes all older siblings whose grade permutation was associated with a probability of passing the PSLE between — and not including — 0 and 1. Significance levels: * <0.1, ** <0.05, *** <0.01

To illustrate the temporal change in the sign of the sibling spillover effects, we plot coefficients from the treatment $\times$ year dummy interactions in Equation (7), with secondary transition as the outcome variable, in Figure A.3. This shows the estimated spillover effect for each cohort of younger siblings.

Figure A.3: Effect of older sibling's qualification for secondary school
on younger sibling's secondary school attendance, by cohort



Coefficients and 95% confidence intervals from estimates of equation (7) regressing younger sibling's secondary transition on a dummy for whether their older sibling passed the secondary school qualification exam, conditional on being near the threshold.

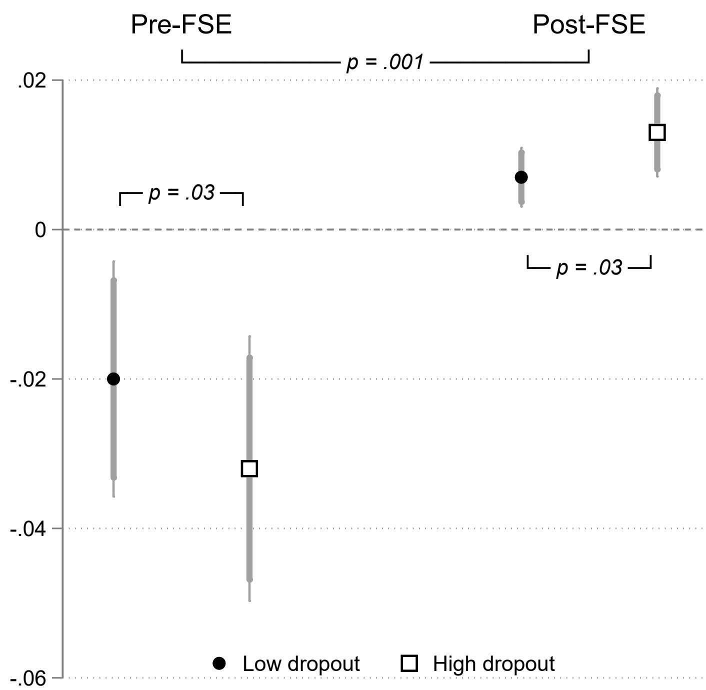
In the two years prior to FSE, the effect of having an older sibling qualify for secondary school on one's own likelihood of attending is negative and significant, with no statistically significant difference between the coefficients.[13] The effect is null in 2016 — the first cohort who sat the PSLE under the FSE policy — then becomes significantly positive (just over 1 percentage point) in the 2017, 2018, and 2019 PSLE cohorts. The null effect in 2016 may reflect the fact that the policy was announced only in late 2015 (after that year's cohort had taken the PSLE), leaving little time for adaptation on either the supply or the demand side prior to the policy's intended start in January of 2016. See the first column of Table A.2 for detailed estimates.

[13]While these two coefficients are not statistically different from one another, the point estimate is higher in 2015 than in 2014. Insofar as this reflects a real increase in the point estimate, it could be due to the fact that although students from the 2015 cohort sat the PSLE before the announcement of the FSE policy, they would have enrolled in the first year of secondary school after the announcement of the policy. Families in a position to capitalize immediately on the fee abolition may then have enrolled students who they had not counted on sending to secondary school but who succeeded in passing the PSLE, attenuating negative sibling spillovers.

A.3 Additional evidence on the effect of FSE

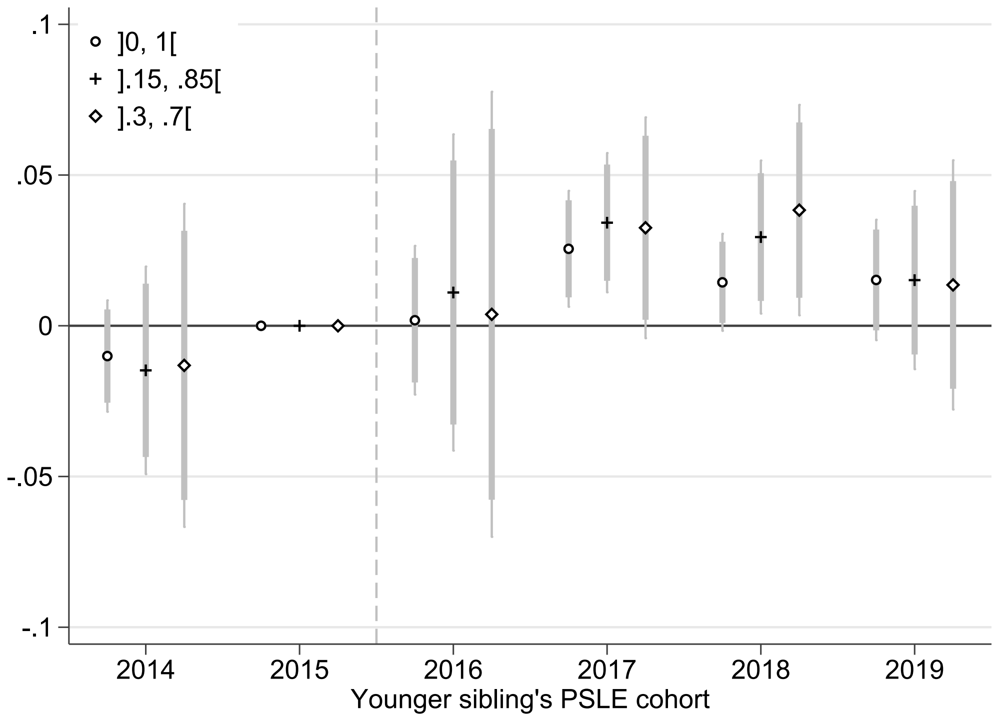
Figure A.4 shows that not only were pre-FSE spillovers more negative in high-dropout wards, but post-FSE spillovers were more positive in these wards as well. As a result, the pre-post difference in measured sibling spillovers is significantly larger in *ex-ante* high-dropout wards, where FSE had a greater direct effect on transition rates.

Figure A.4: Effect of older sibling passing on younger sibling's transition
Before and after FSE, in high- vs. low-dropout wards



Coefficients and 95% confidence intervals from regressing younger sibling's secondary transition on the full interaction of dummies for high-dropout ward, post-2016 cohort, and older sibling PSLE pass (conditional on being near the threshold). 'Low dropout' pre-FSE reports the main effect on older sibling PSLE pass; 'High dropout' pre-FSE reports the linear combination of the main effect on older sibling PSLE pass plus the interaction of older sibling PSLE pass with high-dropout ward; 'Low dropout' post-FSE reports the linear combination of the main effect on older sibling PSLE pass plus the interaction of older sibling PSLE pass with post-2016; and 'High dropout' post-FSE reports the linear combination of the main effect plus the interaction of older sibling PSLE pass with high-dropout ward plus the interaction of older sibling PSLE pass with post-2016 plus the triple interaction of older sibling PSLE pass with post-2016 and high-dropout ward. P-values from t-tests of the difference between high- and low-dropout ward effect sizes (and the difference between these pre- and post-FSE differences) are displayed.

Figure A.5: Effect of older sibling passing on younger sibling's transition relative to 2015 (pre-FSE), with family FE



Coefficients and 95% confidence intervals from regressing younger sibling's secondary transition on a dummy for whether their older sibling passed the secondary school qualification exam (conditional on being near the threshold), interacted with year dummies, including year and family fixed effects, for different windows of closeness to the threshold. Unit of observation is an individual student with an older sibling who took the PSLE in 2013 or 2014.

A.4 Additional heterogeneity

Table A.3: Spillover effects from older siblings' achievement (Heterogeneity by sibship size)

	Pre-FSE			Post-FSE		
	(1) Transition	(2) PSLE Pass	(3) High score	(4) Transition	(5) PSLE Pass	(6) High score
Older sibling passed the PSLE	-0.003	0.001	0.016*	0.018***	0.015***	0.016***
	(0.007)	(0.009)	(0.008)	(0.004)	(0.004)	(0.003)
Sibship size	0.002***	-0.001	0.002	0.001	-0.000	0.001***
	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)	(0.000)
Older sibling passed x Sibship size	-0.002	-0.004*	-0.004	-0.003***	-0.003***	-0.002***
	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)
N	84,035	84,730	84,730	517,117	521,738	521,738
Mean dep. var.	0.239	0.622	0.161	0.608	0.754	0.250
Mean sibship size	3.242	3.248	3.248	3.296	3.302	3.302
R^2	0.292	0.333	0.385	0.176	0.171	0.230
Older sibling's score FE	Yes	Yes	Yes	Yes	Yes	Yes
School FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Standard errors clustered by grade permutation in parentheses. 'Transition' is only measured for students whose full name is unique across the country within their cohort. 'High score' is a binary variable equal to one if the younger sibling got an A or B average on the PSLE, and zero otherwise. All regressions include (younger sibling's) school and (older sibling's) grade permutation fixed effects. 'Year FE' denotes binary indicators for the PSLE cohort of each younger sibling. The sample includes all older siblings whose grade permutation was associated with a probability of passing the PSLE between — and not including — 0 and 1. Significance levels: * <0.1, ** <0.05, *** <0.01

Table A.4: Spillover effects from older siblings' achievement (urban/rural heterogeneity)

	Pre-FSE			Post-FSE		
	(1) Transition	(2) PSLE Pass	(3) High score	(4) Transition	(5) PSLE Pass	(6) High score
Older sibling passed the PSLE	-0.009*	-0.010**	0.001	0.008***	0.004**	0.007***
	(0.005)	(0.005)	(0.005)	(0.002)	(0.002)	(0.002)
Older sibling passed x Urban	-0.001	0.001	0.011	-0.002	-0.004*	0.003
	(0.006)	(0.005)	(0.008)	(0.003)	(0.002)	(0.003)
Passed + Passed x Urban	-0.010	-0.010**	0.012	0.006*	0.001	0.010***
	(0.006)	(0.005)	(0.007)	(0.003)	(0.002)	(0.004)
N	80,240	80,892	80,892	494,341	498,685	498,685
Mean dep. var.	0.239	0.622	0.162	0.607	0.755	0.252
Mean dep. var. (Urban)	0.259	0.738	0.221	0.750	0.848	0.330
R^2	0.291	0.333	0.388	0.175	0.170	0.230
Older sibling's score FE	Yes	Yes	Yes	Yes	Yes	Yes
School FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Standard errors clustered by grade permutation in parentheses. 'Transition' is only measured for students whose full name is unique across the country within their cohort. 'High score' is a binary variable equal to one if the younger sibling got an A or B average on the PSLE, and zero otherwise. All regressions include (younger sibling's) school and (older sibling's) grade permutation fixed effects. Since school fixed effects are always included in our models and given that we classify wards according to the 2012 Census definition (i.e., ward status is time-invariant), we do not include a dummy for ward status ('Urban') in the estimated equations. 'Year FE' denotes binary indicators for the PSLE cohort of each younger sibling. The sample includes all older siblings whose grade permutation was associated with a probability of passing the PSLE between — and not including — 0 and 1. Significance levels: * <0.1, ** <0.05, *** <0.01

Table A.5: Spillover effects from older siblings' achievement (Heterogeneity by older sibling's gender)

	Pre-FSE			Post-FSE		
	(1) Transition	(2) PSLE Pass	(3) High score	(4) Transition	(5) PSLE Pass	(6) High score
Old sib passed PSLE	-0.020**	-0.017**	0.007	0.006	0.004	0.009**
	(0.008)	(0.007)	(0.005)	(0.004)	(0.003)	(0.004)
Female	-0.018***	-0.082***	-0.059***	0.002	-0.037***	-0.077***
	(0.006)	(0.007)	(0.006)	(0.004)	(0.004)	(0.004)
Old sib passed PSLE × Female	0.005	0.003	-0.007	0.002	0.002	-0.008*
	(0.009)	(0.011)	(0.007)	(0.005)	(0.006)	(0.005)
Old sib female	-0.006	0.003	0.010**	-0.008**	-0.003	0.002
	(0.007)	(0.006)	(0.004)	(0.004)	(0.004)	(0.003)
Old sib passed PSLE × Old sib female	0.003	-0.014	0.005	0.000	-0.004	0.004
	(0.010)	(0.010)	(0.006)	(0.005)	(0.005)	(0.005)
Female × Old sib female	-0.001	-0.010	-0.008	0.011	0.009	-0.002
	(0.008)	(0.009)	(0.005)	(0.007)	(0.006)	(0.005)
Old sib passed PSLE × Female × Old sib female	0.020	0.033**	-0.006	0.001	0.005	0.004
	(0.014)	(0.015)	(0.008)	(0.009)	(0.007)	(0.006)
Boys with older brothers	-0.020**	-0.017**	0.007	0.006	0.004	0.009**
	(0.008)	(0.007)	(0.005)	(0.004)	(0.003)	(0.004)
Boys with older sisters	-0.015**	-0.014*	-0.000	0.008**	0.006	0.001
	(0.006)	(0.008)	(0.007)	(0.004)	(0.004)	(0.003)
Girls with older brothers	-0.017***	-0.031***	0.012**	0.006*	-0.000	0.013***
	(0.006)	(0.006)	(0.006)	(0.003)	(0.003)	(0.002)
Girls with older sisters	0.008	0.005	-0.002	0.010***	0.007***	0.009***
	(0.006)	(0.006)	(0.005)	(0.002)	(0.002)	(0.002)
N	84,035	84,730	84,730	517,117	521,738	521,738
Mean dep. var.	0.239	0.622	0.161	0.608	0.754	0.250
R^2	0.292	0.339	0.393	0.176	0.172	0.238
Older sibling's score FE	Yes	Yes	Yes	Yes	Yes	Yes
School FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Standard errors clustered by grade permutation in parentheses. 'Transition' is only measured for students whose full name is unique across the country within their cohort. 'High score' is a binary variable equal to one if the younger sibling got an A or B average on the PSLE, and zero otherwise. All regressions include (younger sibling's) school and (older sibling's) grade permutation fixed effects. 'Year FE' denotes binary indicators for the PSLE cohort of each younger sibling. The sample includes all older siblings whose grade permutation was associated with a probability of passing the PSLE between — and not including — 0 and 1. Significance levels: * <0.1, ** <0.05, *** <0.01

A.5 Alternative mechanisms

A.5.1 Extensive margin

Table A.6 tests whether passing the PSLE affects the likelihood that younger siblings sit the PSLE. This could be a concern given evidence of schools strategically excluding some poorly performing students from sitting the exam (Cilliers et al., 2021). To analyze this possibility, we examine the sample of PSLE takers from years 2013-2018 whose last name is unique within ward×cohort. We match each cohort of these students by last name within ward to all younger cohorts of PSLE takers whose last name is unique within ward×cohort (2014-2019). (This is to limit/minimize false positives.) We then create outcome variables from these merges: a dummy for whether anyone with the same last name sat the PSLE in the same ward in the following year; a dummy for whether anyone with the same last name sat the PSLE in the same ward in any subsequent year; and the number of people with the same last name who sat the

PSLE in the same ward in all subsequent years. As before, we regress these outcomes on a dummy for exam passage, considering only students with grade permutations indicating ambiguous pass status, and including fixed effects for year, school, and grade permutation.

Table A.6: Extensive margin: effect of passing PSLE on whether younger siblings sit PSLE

	Pre-FSE			Post-FSE		
	Sibling sat PSLE in following yr	Any younger sib sat PSLE	Num. younger sibs sat PSLE	Sibling sat PSLE in following yr	Any younger sib sat PSLE	Num. younger sibs sat PSLE
Pass PSLE	-0.002	-0.001	-0.002	-0.000	-0.000	0.002
	(0.002)	(0.003)	(0.004)	(0.002)	(0.003)	(0.003)
N	510,012	510,012	510,012	405,802	405,802	405,802
Mean dep. var.	0.138	0.458	0.724	0.150	0.274	0.324

Notes: Standard errors clustered by grade permutation in parentheses. Unit of observation is an individual PSLE taker. 'Sibling sat PSLE in following year' is a dummy for whether the student had a sibling sit the PSLE one year later. 'Any younger sib sat PSLE' is a dummy for whether the student had any siblings subsequently sit the PSLE during the period our data covers (2014-2019). 'Num. younger sibs sat PSLE' measures how many younger siblings subsequently sat the PSLE during the period our data covers. All regressions include fixed effects for the student's cohort, school, and grade permutation. The sample includes all students from year 2013-2018 whose last name is unique within their ward × cohort, whose grade permutation was associated with a probability of passing the PSLE between — and not including — 0 and 1. Significance levels: * <0.1, ** <0.05, *** <0.01

We find no evidence that passing the PSLE on the margin changed the likelihood of younger siblings taking the PSLE.

A.5.2 School selection

School selection represents another potential mechanisms for the effects we measure. Table A.7 tests whether an older sibling passing the PSLE affects the quality of schools at which younger students sit the PSLE. The estimation strategy mirrors that of our main results, with the caveat that we include fixed effects for the older sibling's school rather than the younger sibling's school in order to measure effects on younger siblings' school choice. Existing literature suggests this could be a meaningful mechanism in some contexts; school quality appears to have been an important channel for the positive sibling spillovers measured by Figlio et al. (2023).

We create three measures of households' selection into higher-quality schools: 1) A dummy for whether the younger sibling sat the PSLE at the same school as the older siblings; 2) the *ex-ante* PSLE pass rate of the younger sibling's school (measured in 2013, prior to any of our sample of younger siblings sitting the exam to avoid endogeneity)[14]; and 3) a dummy for whether the younger sibling sat the PSLE at a

[14]School-level correlations between pass rate percentile (within the national distribution) from one year to the next are above .5 for all years in our data.

higher-*ex-ante*-pass-rate school than her older sibling.

Table A.7: School selection: effect of passing PSLE on quality of schools at which younger siblings sit PSLE

	Pre-FSE			Post-FSE		
	Different school	Ex-ante PSLE pass rate	School pass rate higher than older sib's	Different school	Ex-ante PSLE pass rate	School pass rate higher than older sib's
Older sibling passed the PSLE	0.010	0.001	0.002	-0.004**	-0.001	-0.001
	(0.007)	(0.001)	(0.006)	(0.002)	(0.001)	(0.002)
N	84,999	82,003	81,939	521,583	436,585	433,651
Mean dep. var.	0.450	0.488	0.237	0.482	0.487	0.232
R^2	0.303	0.719	0.336	0.231	0.675	0.250
Older sibling's score FE	Yes	Yes	Yes	Yes	Yes	Yes
Older sibling's school FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Standard errors clustered by grade permutation in parentheses. All regressions include older sibling's school and grade permutation fixed effects. 'Year FE' denotes binary indicators for the PSLE cohort of each younger sibling. The sample includes all older siblings whose grade permutation was associated with a probability of passing the PSLE between — and not including — 0 and 1. Significance levels: * <0.1, ** <0.05, *** <0.01

We find little evidence that an older sibling passing the PSLE changed school selection for younger siblings, either before or after the reform. In the post period, parents appear slightly more likely to send younger siblings to the same school as an older sibling in response to the older sibling (marginally) passing the PSLE at that school, which may be interpreted as a kind of positive school selection. The effect is small, however: less than one percent (0.4 percentage points from a base of 48%).

B Robustness

Table B.1: Spillover effects from older siblings' achievement (with controls)

	Pre-FSE			Post-FSE		
	(1) Transition	(2) PSLE Pass	(3) High score	(4) Transition	(5) PSLE Pass	(6) High score
Older sibling passed the PSLE	-0.010**	-0.013***	0.004	0.008***	0.004***	0.008***
	(0.005)	(0.004)	(0.004)	(0.002)	(0.002)	(0.002)
Female older sibling	-0.001	-0.001	0.006**	-0.002	0.001	0.004***
	(0.003)	(0.002)	(0.003)	(0.002)	(0.001)	(0.001)
Female	-0.012***	-0.078***	-0.069***	0.011***	-0.029***	-0.081***
	(0.003)	(0.003)	(0.003)	(0.001)	(0.002)	(0.001)
Age gap	-0.002	0.000	0.002	0.004***	0.003***	0.003***
	(0.004)	(0.005)	(0.003)	(0.001)	(0.000)	(0.000)
N	84,035	84,730	84,730	517,117	521,738	521,738
Mean dep. var.	0.239	0.622	0.161	0.608	0.754	0.250
R^2	0.292	0.338	0.393	0.176	0.172	0.238
Older sibling's score FE	Yes	Yes	Yes	Yes	Yes	Yes
School FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Standard errors clustered by grade permutation in parentheses. 'Transition' is only measured for students whose full name is unique across the country within their cohort. 'High score' is a binary variable equal to one if the younger sibling got an A or B average on the PSLE, and zero otherwise. 'Female' and 'Female older sibling' are dummies for the gender of each sibling. 'Age gap' is proxied by the difference in years between siblings' PSLE cohorts. All regressions include (younger sibling's) school and (older sibling's) grade permutation fixed effects. 'Year FE' denotes binary indicators for the PSLE cohort of each younger sibling. The sample includes all older siblings whose grade permutation was associated with a probability of passing the PSLE between — and not including — 0 and 1. Significance levels: * <0.1, ** <0.05, *** <0.01

Table B.2: Spillover effects from older siblings' achievement (Most recent older sibling)

	Pre-FSE			Post-FSE		
	(1) Transition	(2) PSLE Pass	(3) High score	(4) Transition	(5) PSLE Pass	(6) High score
Older sibling passed the PSLE	-0.011**	-0.013***	0.005	0.005**	0.002	0.005**
	(0.005)	(0.004)	(0.004)	(0.002)	(0.002)	(0.002)
N	77,965	78,595	78,595	330,736	333,476	333,476
Mean dep. var.	0.236	0.619	0.158	0.597	0.745	0.239
R^2	0.290	0.334	0.383	0.184	0.177	0.234
Older sibling's score FE	Yes	Yes	Yes	Yes	Yes	Yes
School FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Standard errors clustered by grade permutation in parentheses. 'Transition' is only measured for students whose full name is unique across the country within their cohort. 'High score' is a binary variable equal to one if the younger sibling got an A or B average on the PSLE, and zero otherwise. All regressions include (younger sibling's) school and (older sibling's) grade permutation fixed effects. 'Year FE' denotes binary indicators for the PSLE cohort of each younger sibling. The sample includes the most recent older siblings whose grade permutation was associated with a probability of passing the PSLE between — and not including — 0 and 1. Significance levels: * <0.1, ** <0.05, *** <0.01

B.1 Alternative Sibling Matching

Besides our preferred (ward-level) sibling matching procedure, we test two alternative methods. First, we repeat the matching procedure but search for within-school matches (instead of within-ward). Additionally, we perform the initial matching algorithm but look for last- *and* middle-name matches in the same ward, exploiting a Tanzanian naming convention by which children of the same father have the same middle and last names. While this method has the potential to minimize mismatches, it is only possible for sibling pairs in which both siblings' full name (first, middle, last) is present in the data; many students' records feature only a middle initial. This matching method therefore reduces the sample size dramatically.

Figure B.1 shows the estimated (spillover) effect of older sibling's qualification for secondary school on younger sibling's secondary school attendance (i.e., *Transition*), by cohort, under the school-level matching procedures. The evolution of the coefficients over time is remarkably similar to that displayed in Figure A.3 (for a more detailed analysis, compare the estimates in the second and third columns of Table A.2 with those in the first column).

Meanwhile, Tables B.3 and B.4 present the aggregated (i.e., pre- and post-FSE) estimates under the two alternative matching methods – these results compare with those in Table 4 under our preferred procedure. Once again, our estimates are qualitatively similar under any of these alternative matching methodologies.

B.1.1 School-level Matching

Figure B.1: Effect of older sibling's qualification for secondary school on younger sibling's secondary school attendance, by cohort (School-level Matching)

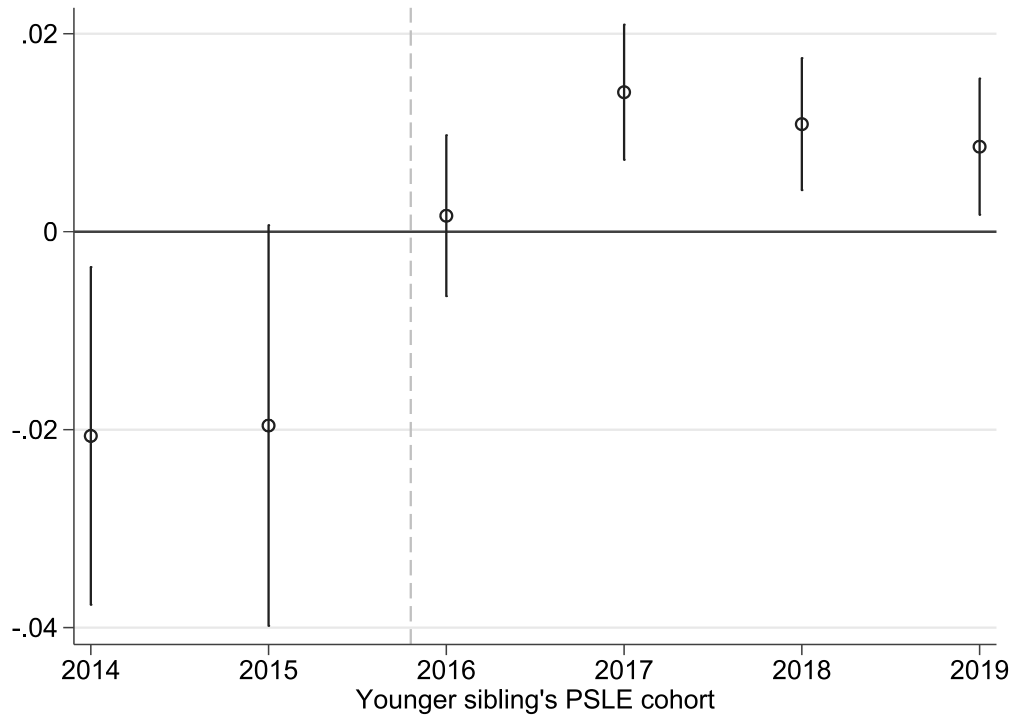


Table B.3: Spillover effects from older siblings' achievement (School-level Matching)

	Pre-FSE			Post-FSE		
	(1) Transition	(2) PSLE Pass	(3) High score	(4) Transition	(5) PSLE Pass	(6) High score
Older sibling passed the PSLE	0.002	-0.002	0.007***	0.006***	0.006***	0.007***
	(0.005)	(0.006)	(0.003)	(0.002)	(0.002)	(0.002)
N	99,494	101,567	101,567	567,211	580,166	580,166
Mean dep. var.	0.238	0.609	0.142	0.592	0.739	0.224
R^2	0.273	0.309	0.333	0.161	0.157	0.183
Older sibling's score FE	Yes	Yes	Yes	Yes	Yes	Yes
School FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Standard errors clustered by grade permutation in parentheses. 'Transition' is only measured for students whose full name is unique across the country within their cohort. 'High score' is a binary variable equal to one if the younger sibling got an A or B average on the PSLE, and zero otherwise. All regressions include school and (older sibling's) grade permutation fixed effects. 'Year FE' denotes binary indicators for the PSLE cohort of each younger sibling. The sample includes all older siblings whose grade permutation was associated with a probability of passing the PSLE between — and not including — 0 and 1. Significance levels: * <0.1, ** <0.05, *** <0.01

B.1.2 Middle and Last Name Matching

Figure B.2: Effect of older sibling's qualification for secondary school on younger sibling's secondary school attendance, by cohort (Middle and Last Name Matching)

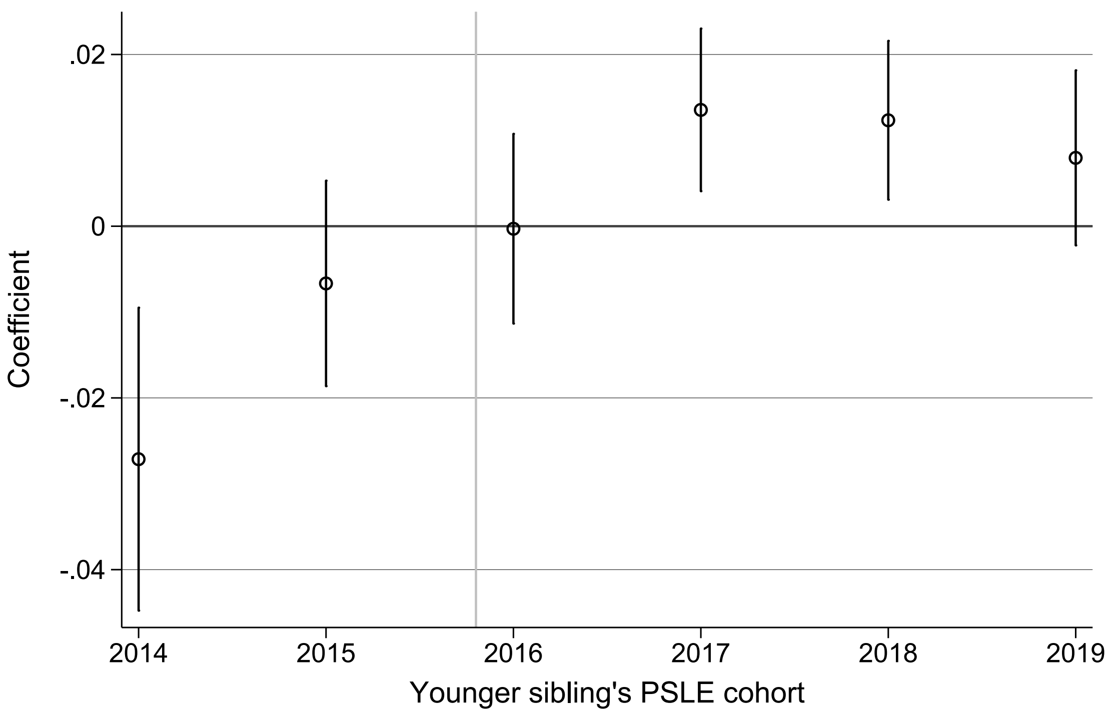


Table B.4: Spillover effects from older siblings' achievement (Middle and Last Name Matching)

	Pre-FSE			Post-FSE		
	(1) Transition	(2) PSLE Pass	(3) High score	(4) Transition	(5) PSLE Pass	(6) High score
Older sibling passed the PSLE	-0.009 (0.008)	-0.006 (0.007)	-0.001 (0.004)	0.009** (0.004)	0.006** (0.003)	0.007*** (0.002)
N	36,798	38,737	38,737	289,442	303,243	303,243
Mean dep. var.	0.283	0.624	0.145	0.608	0.746	0.228
R^2	0.358	0.385	0.420	0.176	0.170	0.219
Older sibling's score FE	Yes	Yes	Yes	Yes	Yes	Yes
School FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Standard errors clustered by grade permutation in parentheses. 'Transition' is only measured for students whose full name is unique across the country within their cohort. 'High score' is a binary variable equal to one if the younger sibling got an A or B average on the PSLE, and zero otherwise. All regressions include (younger sibling's) school and (older sibling's) grade permutation fixed effects. 'Year FE' denotes binary indicators for the PSLE cohort of each younger sibling. The sample includes all older siblings whose grade permutation was associated with a probability of passing the PSLE between — and not including — 0 and 1. Significance levels: * <0.1, ** <0.05, *** <0.01