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# Sibling Spillovers and Free Schooling

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**Abstract**

We use administrative data to measure sibling spillovers on academic performance before and after the introduction of Free Secondary Education (FSE) in Tanzania. 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. Mechanism analyses suggest that changes in parental investments were a more likely channel for this reversal than direct sibling interactions. By alleviating financial constraints, FSE allowed households 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

How can public service provision affect the distribution of human capital investments within families? Theoretically, resource-constrained parents face a tension between investing in lower-ability children ("compensating") vs. higher-ability children ("reinforcing") (Becker & Tomes, 1976). Reinforcing investments generate negative sibling spillovers: one child's positive shock diverts resources toward him at the expense of other children in the family. There is ample evidence that parental investments reinforce children's educational endowments, mostly (though not exclusively) from the developing world (Vogl, 2013; Akresh et al., 2012; Jensen & Miller, 2017; Karbownik & Özek, 2021). But parents in both high- and low-income countries express preferences for equalizing educational opportunities across siblings, a preference more in line with compensating than reinforcing (Berry et al., 2025). In richer countries where resource constraints are less binding, more empirical evidence finds that parents make compensating educational investments, leading to positive sibling spillovers (Figlio et al., 2023; Black et al., 2021; Behrman et al., 1982). Even within developing countries, reinforcement is more prevalent in lower-resource households (Giannola, 2023; Leight & Liu, 2020; Fan & Porter, 2020; Restrepo, 2016; Parish & Willis, 1993). This suggests that within-household inequality aversion is a normal good.

Access to free public education may play a role in parents' investment decisions. If parents choose reinforcement due to a resource constraint, then the easing of this constraint via the removal of school fees could permit more equitable or compensating investments — and a reversal of negative sibling spillovers. But pinpointing the effect of free schooling on sibling spillovers is difficult: the exogenous variation necessary to identify sibling spillovers is rare, and coincident policy changes in access to schooling are even rarer. Consequently, little existing research examines how sibling spillovers respond to changes in school access. The RCT literature on scholarships and cash transfers is germane, showing that relaxing families' resource constraints using these targeted tools can have null or even negative effects on educational outcomes for recipients' siblings (Duflo et al., 2017; Barrera-Osorio et al., 2011). Whether similar spillovers materialize at scale is therefore an important policy question (Bold et al., 2018; Muralidharan & Niehaus, 2017).

In this paper, we use administrative data to measure the effect of Tanzania's Free Secondary Education policy (FSE) on sibling spillovers in education. Tanzania offers an ideal context for examining how sibling spillovers 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 spillover effects.[1] The second is the 2016 FSE policy which abolished lower secondary school fees (primary school fees had been abolished over a decade earlier). Secondary school is the frontier of schooling access not only in Tanzania but in much of the developing world; 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). Prior to FSE, school fees were a binding constraint on many Tanzanian households' educational investments, 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 high-stakes Primary School Leaving Exam (PSLE). Both before and after FSE, passing this exam is a requirement for entering public secondary school. Students who fail are more likely to move into "mining, grazing, and family activities" than 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, focusing on pairs in which the older sibling scored at or near the passing threshold. We then compare the educational outcomes of younger siblings whose older sibling narrowly did vs. did not qualify for entry into public secondary school. Our main specifications 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.[2]

We find that sibling spillovers are negative before FSE and positive afterward. Prior to the reform, an older sibling who marginally passed the exam *reduced* their younger sibling's likelihood of transitioning to secondary school by about 3.5% (0.8 percentage points from a base of 22%) — consistent with models of parental reinforcement. After secondary school fees were eliminated, older siblings' exam passage *increased* younger siblings' transition rates by about 1.4% (0.8 pp from a base of 57%). The magnitude of these effects is non-trivial. The difference in transition rates between students in districts with above- vs. below-median poverty in our sample is -3.1 pp in the pre-reform period. As another point of comparison, a recent study in Tanzania found that conditional cash transfers of USD $4 per month 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. We

[1] Such primary leaving exam score requirements are used for rationing access to secondary school in more than 30 African countries (Gilligan et al., 2022).

[2] If passing the exam provides a signal of student ability, this could affect student behavior and family investments through channels other than secondary school enrollment, which would violate the exclusion restriction. Nevertheless, we present IV estimates of the spillover effect on our main outcome of interest in our discussion of potential mechanisms.

compare the difference in spillovers between areas more vs. less affected by FSE changed after the policy was introduced, following the empirical design of Lucas & Mbiti (2012). This difference remained constant in the years prior to FSE, then widened significantly after FSE's implementation.

We then turn to the mechanisms behind these sibling spillovers and their reversal. Previous research on FSE has shown that the policy alleviated household resource constraints and increased households' investments in primary school students, raising school attendance and reducing child labor (Sandholtz, 2024b). This makes parental investments an intuitive possible channel for the effects we identify here. But an extensive literature shows that sibling spillovers in education can also result from direct sibling interactions — including learning, mentoring, role modeling, and returns to scale — with most empirical work finding non-negative estimates of these spillover effects (Qureshi, 2018b; Nicoletti & Rabe, 2019).[3] These channels may be particularly important in our context, where FSE's dramatic effect on secondary enrollments might plausibly have produced many more school-based sibling interaction.

On balance, the evidence is more consistent with a mechanism of parental investments than direct sibling interactions. We find positive spillovers among post-FSE younger siblings whose older sibling finished primary school before FSE. This means that the change in spillovers was not a result of changes to the number of older siblings enrolling in secondary school. The positive post-FSE change also shows up in a family fixed effects model, implying that changes to the composition of families or older siblings also cannot explain the effects. Heterogeneity by poverty, gender, family size, age gap, and prior academic achievement all offer evidence consistent with FSE's alleviation of financial constraints on parental investments as an important channel for the spillover patterns we observe.

Our main contribution is to provide evidence that expansions of public school access change households' patterns of human capital investment allocations across siblings. Parental reinforcement of children's human capital is a well-documented phenomenon in many parts of the world.[4] The literature on parental investments in education specifically includes many studies finding evidence of reinforcement: Akresh et al. (2012) show that the siblings of high-ability children in Burkina Faso are significantly less likely to be enrolled in school; Ayalew (2005) and Yi et al. (2015) find that educational investments reinforce differences in health endowments in Ethiopia and China respectively. Morduch (2000) finds evidence in Tanzanian household survey data of "rivalry for scarce resources in which parents favor sons," some-

[3] It is possible that some channels for negative peer effects proposed in the literature could apply to sibling spillovers as well, e.g. the Invidious Comparison model (Hoxby & Weingarth, 2005; Antecol et al., 2016).

[4] Almond & Mazumder (2013) provide an excellent review of the related literature which uses health endowments at birth to examine these patterns, concluding that reinforcement is more common than compensation. However, many studies in this area find evidence of compensation (Griliches, 1979; Behrman et al., 1982; Adhvaryu & Nyshadham, 2016; Leight, 2017; Bharadwaj et al., 2018).

thing our causal results from the pre-reform period corroborate. Dizon-Ross (2019) uses an RCT to show that Malawian parents respond to information about child ability with reinforcing investments, increasing or decreasing their children's enrollment according to ability. Barrera-Osorio et al. (2011) provide RCT evidence from Colombia that cash transfers for one child in a household reduce educational investments in the child's siblings. Many studies which identify reinforcing educational investments among siblings examine developing-country settings, although Karbownik & Özek (2021) find evidence of reinforcement in the U.S. state of Florida. Studies of richer countries more commonly find compensating investments (Figlio et al., 2023; de Gendre, 2022; Black et al., 2021; Behrman et al., 1982). Of particular interest is a set of studies finding heterogeneity in parental investments by household income or education. Many studies show a negative gradient between reinforcement and maternal education: Fan & Porter (2020) in Ethiopia, Leight & Liu (2020) and Hsin (2012) in China, and Restrepo (2016) in the U.S. Similarly, Behrman (1988) and Parish & Willis (1993) find more evidence of reinforcement during times of economic hardship, in India and Taiwan respectively. Giannola (2023) demonstrates in a survey experiment in India that reinforcement is more common when resource constraints bind more tightly. Perhaps the most closely related study to ours is that of Collins et al. (2025), which uses DHS survey data to show that Free Primary Education in sub-Saharan Africa lowered fertility and increased household educational investments in children (although it does not examine allocation across siblings). This evidence suggests that as families and societies get richer, they tend to move from reinforcing to compensating investments. Our study is the first to present causal estimates at nationwide scale of how sibling spillovers change over time within a country. It is also the first to our knowledge to measure sibling education spillovers in a developing country using administrative data. A key contribution of the paper is to show that public policy can play a role in alleviating constraints, allowing families to allocate resources differently within the household.[5]

Our paper also contributes to the literature on expanding access to schooling. The frontier of schooling access varies across time and across regions of the world. Eliminating fees for public primary schools has been shown to have increased enrollment in Ethiopia (Chicoine, 2019), Kenya (Lucas & Mbiti, 2012), Malawi (Samarrai & Zaman, 2007), Tanzania (Valente, 2019), and Uganda (Deininger, 2003). Eliminating

[5]While our paper focuses on the channel of parental investments, siblings can affect one another through many possible channels, including role model effects (Qureshi, 2018b; Zang et al., 2023), sibling tutoring (Qureshi, 2018a; Nicoletti & Rabe, 2019), returns to scale (Aguirre & Matta, 2021), and sibling income easing credit constraints (Lindskog, 2013; Baland et al., 2016; Emerson & Souza, 2008). Health interventions can affect intra-family educational outcomes: Ozier (2018) documents large positive spillovers on siblings' cognition from deworming in Kenya, and Alsan (2017) shows that older sisters of Turkish children eligible for vaccines have better educational outcomes. Another important channel is information spillovers, which can affect siblings' choices of where and what to study (Figlio et al., 2023; Altmejd et al., 2021; Dustan, 2018; Joensen & Nielsen, 2018; Dahl et al., 2024). Although our study focuses on sibling spillovers in education at the primary and secondary level, positive sibling spillovers have also been documented at the university level (Barrios-Fernández, 2022).

fees for secondary school has been shown to increase enrollment in The Gambia (Blimpo et al., 2019), Kenya (Brudevold-Newman, 2021), and Tanzania (Brandt & Mkenda, 2021).[6] Similar results are found in the higher education literature for more targeted policies for increasing access: Dynarski (2003) shows that student aid improves college completion in the U.S., and Solis (2017) finds that access to student loans raises college enrollment in Chile. Existing work on sibling spillovers emphasizes their importance in assessing the costs and benefits of education policies (Qureshi, 2018a; Figlio et al., 2023). Our paper highlights sibling spillovers as an additional source of benefits of free secondary schooling, a policy currently being considered in many parts of the developing world (Crawfurd & Ali, 2022; Crawfurd, 2024; Garlick, 2019).

# 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 & 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).[7]

## 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

[6] See Langsten (2017) for a review of fee abolition policies across sub-Saharan Africa.
[7] Source: Tanzania National Panel Survey, Wave 4 (2014-15) (World Bank, 2014–2020).

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 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).[8]

**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

[8] These administrative data have also been used in studying other aspects of Tanzania's FSE policy (Brandt & Mkenda, 2021; Sandholtz, 2024b), Catholic schools (Sandholtz, Gibson, & Crawfurd, 2024), and heat and learning (Melo et al., 2025).

each subject weighted equally in the overall score. Each subject is graded on a scale of 0-50, and the overall score is the sum of the marks from each subject. NECTA does not make students' precise marks available, but does provide the associated letter grades for each subject and for the overall exam. The marks for each subject map onto letter grades according to the following scale: 0-10 marks correspond to an E ($\leq$ 20%), 11-20 to a D (21-40%), 21-30 to a C (41-60%), 31-40 to a B (61-80%), and 41-50 to an A ($>$ 80%). An 'A' grade on the overall exam means the student received at least 200 marks, while 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%) — i.e., all students receiving an overall grade of C or higher pass the exam.[9] 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 about here.]

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 we classify as "younger siblings" (see Section 3). Panel 1a shows the histogram of imputed overall PSLE scores (scaled to a total possible score of 100).[10] 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 described below), 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 identifiers in the exam data during the time period we study, we match students across exams using their

[9] Because a subject score of 20 marks (40%) corresponds to the very top of the 'D' range, it is technically possible for students receiving a 'D' in each subject to obtain an overall score of 100 marks and thereby pass the exam. We indeed observe that a tiny fraction of these students pass.

[10] These imputed scores were created by averaging the midpoints of the score windows corresponding to each of the student's five subject letter grades; e.g. 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%. We validate this imputed measure of PSLE scores on a subset of the sample for which true PSLE scores are available; see Figure A.1.

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).) 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.[11] 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 is to compare the younger siblings of older students who narrowly did vs. did not 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.[12] It also affords us a source of quasi-random variation in the older sibling's educational attainment. 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.

[11] 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. In Table A.8 we test for any effect of older siblings' passing on younger siblings' likelihood of taking the FTNA a year earlier than expected, or one or two years later. In all cases, both before and after FSE, the result is a very precisely-measured zero effect.

[12] A related literature demonstrates the importance of spillovers from younger to older siblings (Karbownik & Özek, 2021; Martinez et al., 2017; Attanasio et al., 2022; Alsan, 2017). Given the temporal ordering in the outcomes we measure, our focus is on spillovers from older to younger siblings. We discuss the (un)feasibility (in our setting) of *younger-to-older* sibling spillover estimation in the appendix.

### 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.[13] We link students into family groups using shared last names within wards (Swahili: *kata*), the third administrative subdivision of Tanzania, and define a student's ward as the ward of the school in which she sat the PSLE. 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 $\times$ ward (constituting 48% of PSLE takers).[14] 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. We then match each cohort of potential older siblings to the set of individuals in each subsequent PSLE cohort by last name and ward: the 'younger siblings.' The matches created in this way are the sibling pairs which constitute the unit of observation of our analysis. We discard family groups consisting of more than seven matches (one per year on average) as likely false positives due to common names. This procedure yields around 1.55 million older-younger sibling pairs. This implies that some individuals appear more than once — as the younger sibling to multiple older siblings, or as both a younger and an older sibling — as our interest is in measuring how a given older sibling's performance affects a given younger sibling's outcomes. 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.

Sibling pairs matched in this way exhibit similar educational outcomes on average, as would be expected of students within families (Hanushek et al., 2021). There is a positive correlation in imputed PSLE marks of 0.26 (rising to 0.32 when we also match on middle name).[15] Figure A.3a illustrates these significant positive correlations, 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 Appendix Section A.7.2. We show that our

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

[14]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.

[15]These correlations are lower than the 0.48 correlation in raw scores reported by Nicoletti & Rabe (2019) for their English sample, likely reflecting some combination of our data's coarser score measures and greater uncertainty regarding family composition, but potentially also reflecting genuinely weaker within-family correlations in academic outcomes. To test whether our matching procedure is indeed recovering information about familial relationships, and not just randomly linking individuals within the same ward, we matched PSLE takers at the ward level using a randomly-generated family identifier. Then, we computed the correlation in imputed PSLE scores among those 'random siblings' who were two or three years apart; this value is about 0.15. (Restricting our samples of ward-level and middle-name matched "true" siblings to those with a two- or three-year age gap leads to slightly increased correlations of 0.27 and 0.33, respectively.) This difference suggests that our sibling matching procedures are indeed recovering meaningful information about family composition.

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 main results use matching at the ward level to allow for the possibility of school choice as a mechanism (see Table A.20), and last-name matching to avoid dropping students whose records do not include middle names.

## 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 & Kolesár, 2023). Instead, our setting features information on letter grades for each subject and the overall grade, 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. For each of the 2000 distinct observed permutations of the five subject grades, we compute a "passing probability:" the share of individuals in our sample who pass the PSLE conditional on receiving that grade permutation. Many of these are unambiguous: students who earn a C or better in each of the five subject grades 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).[16]

Figure 2 plots the fraction of students who passed the exam in each of the observed combinations of letter grades. (See Figure A.4 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 about here.]

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 about here.]

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

Table 2 shows that among students with an overall score near the passing threshold, passing the PSLE has a large and statistically significant effect on the student's own likelihood of making the transition to secondary school. These regressions include fixed effects for the student's school, cohort, and grade permutation. The effect is about 28 percentage points in the pre-reform period, and 64 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

[16] We obtain granular test scores for a subset of students, and using this information we show that the vast majority of students obtaining the same grade permutation receive an overall score in a relatively narrow cluster around the midpoint of the window of possible scores within that permutation. See Figure A.2.

who fail the PSLE are extremely low ($< 3\%$) both before and after the reform, showing that the score cutoff is binding and not subject to meaningful manipulation.

[Table 2 about here.]

### 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; $\beta$ 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: $\gamma_g$ captures the fixed effect of the older sibling's subject-specific PSLE grade permutation, while $\rho_s$ and $\tau_t$ 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 & 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 & 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.

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

A related test interacts $PSLEpass_o$ with year indicators to show how the sibling spillover evolves year-by-year:

$$y_{iogst} = \sum_{t=2014}^{2019} \beta_t \, PSLEpass_o \times \tau_t + \gamma_g + \rho_s + \tau_t + \varepsilon_{iogst} \tag{3}$$

Robustness checks will add controls and other fixed effects to the above specifications. As mentioned above, our main analyses 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). In robustness checks, we narrow this window to grade permutations with implied passing probabilities between 0.3 and 0.7.

### 3.5 Balance

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 siblings, younger siblings, and the sibling pairs differ significantly across the threshold, for 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[.

We compare time-invariant characteristics of students from pairs in which the older sibling's grade was on one side or the other of the PSLE passing threshold, conditional on the fixed effects described in Equation (1), including older sibling's grade permutation. Our empirical approach includes school fixed effects, absorbing any variation in characteristics which vary at the level of the school (or higher geographic levels). Our balance test therefore consists in checking for differences around the threshold at the individual level, within school. Our administrative data provide limited detail on individual-level characteristics: in addition to students' test scores, we have their name, sex, and ownership of the school where they sat the exam. However, these data provide non-trivial information on socioeconomic background and other correlates of performance. We can make an informed inference about religion by comparing students' names against a list of common Muslim names. This conservative method identifies 20% of students as having Muslim names in both the pre- and post-FSE periods. (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 — important because our main outcome measure, Transition to secondary school, relies on matching students by name. Building on literature showing that name distinctiveness correlates with educational performance, we calculate the fraction of students in a cohort who share that first name (Figlio, 2005, 2007).[17] Table A.3 shows that these measures are all predictive of performance and socioeconomic status. Transitioning to secondary school is significantly more likely for both older

[17] Existing literature also shows that the distinctiveness of names correlates with crime (Kalist & Lee, 2009) and serves as a marker of ethnic identity or cultural assimilation (Fryer & Levitt, 2004; Fouka, 2020; Abramitzky et al., 2020).

and younger siblings who are male, not Muslim, and have names which are longer and more common. Names in high-poverty wards are significantly shorter, more common, and more likely to appear Muslim. Private schools are much more common in low-poverty wards, and students who sit the PSLE at a private school are much more likely to transition to secondary school. For a subset of the sample who sat the PSLE in 2018 or later, we can link individual younger siblings to their own performance on the Grade 4 exam (only available starting in 2015). Finally, we can check balance on characteristics of sibling pairs: age gap, gender match, and whether they share a last name with a local politician (this has been found to predict secondary transition prior to FSE; see Sandholtz (2024b)).

[Table 3 about here.]

Table 3 shows that few of these measures are significantly different across the older sibling's PSLE passing threshold.[18] In our main sample, examining sibling pairs across the full period of analysis, older siblings who pass do not have names that are differentially long, common, or likely to be Muslim; they are no likelier to attend private school. They are very slightly less likely to be female ($p < .1$). An F-test of joint significance fails to reject the hypothesis that these variables do not jointly predict older sibling passing. Younger siblings also exhibit no significant differences in any of these variables at the threshold.[19] Furthermore, younger siblings from the later cohorts are no likelier to be matched to Grade 4 exam results, and among those who do match, they are no more likely to obtain low scores. An F-test of joint significance also fails to reject the null hypothesis.[20] There is some evidence of imbalance in the pair-specific variables: pairs in which the older sibling passed have slightly smaller age gaps and are slightly less likely to have the same gender. These differences are small: 2.3% and 1.7% of the respective means. Sibling pairs with passing older siblings are no likelier to share a last name with a local politician. However, the F-test of joint significance does reject the null hypothesis for these pair-level variables. We show in Table A.7 that our main results are robust to controlling for the individual and pair-level characteristics available. Overall, our balance tests suggest that variation in exam passing near the threshold can be considered quasi-random.

[18] Tables A.4 and A.5 perform the same exercise separately for the pre-FSE and post-FSE periods.

[19] We do not check balance on younger siblings' private school attendance, as this is an outcome that could be affected by an older sibling passing the PSLE.

[20] To avoid dropping observations from years in which Grade 4 information is not available, this test omits the Grade 4 variables. They are included in the F-tests of Table A.5.

# 4 Results

## 4.1 Sibling spillovers before and after FSE

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 0.8 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 0.8 percentage points *more* likely to enroll in secondary schooling, 0.4 percentage points more likely to pass the PSLE, and 0.9 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 about here.]

Table 5 estimates these effects in a more structured way according to Equation (2), including the full-period sample and interacting the treatment dummy (*Older sibling passed [the 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, using the same set of fixed effects as the analysis from Table 4 — older sibling's grade permutation, and younger sibling's cohort and school.[21]

The remaining columns in Table 5 show that these estimates are robust to including fixed effects for older sibling's cohort, older sibling's school, both, or their interactions with 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.

[21] Using the same set of fixed effects implies constraining these effects to be constant across the full period, which explains why the coefficients vary in magnitude from those in Table 4. While the analysis of Table 5 permits the formal statistical test of difference of the spillover effects across periods, the analysis of Table 4 is more flexible.

[Table 5 about here.]

Figure 3 provides a visual illustration of how sibling spillovers went from negative to positive after the introduction of FSE. It plots coefficients and 90% and 95% confidence intervals from Equation (3), interacting $PSLEpass_o$ with year dummies, and using younger siblings' secondary transition as the outcome of interest. It also includes the most restrictive set of fixed effects from the rightmost column in Table 5: for each cohort of younger siblings, it compares the secondary enrollment outcomes of students whose older siblings got the same grade permutation in the same school in the same year, but differed in whether they passed or not.

[Figure 3 about here.]

#### 4.1.1 Robustness

The results in the first column of Table 5 are robust to restricting the sample to successively narrower windows of passing probability (see Table A.6). Table A.7 shows that these estimates are robust to the inclusion of controls and to alternative choices over matching and estimation. We perform a similar analysis to that outlined above on unique younger siblings, considering only each student's youngest older sibling. We also change the sibling matching algorithm, first by allowing sibling matches only within school (not within ward), and then by only allowing siblings who match on middle name as well as last name. In all cases, results are qualitatively similar.

In addition, we show there is a precisely estimated null effect on younger siblings' "early" or "late" sitting of the FTNA exam, our proxy for secondary transition, both before and after FSE (see Table A.8). This is expected since there is no evidence of systematic delaying or anticipation of FTNA taking to the extent where its usefulness as a proxy could be cast into doubt; but also reassuring as it suggests that families with children at the margin of secondary transition are not strategically delaying or accelerating the enrollment of their younger siblings.

#### 4.1.2 Interpreting magnitudes

Comparing these effects with the literature is challenging, 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 & 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 & Özek (2021) find that among impoverished families in the Florida district they study, having an older sibling born after the school-entry cutoff (and hence be old rather than young in their cohort) caused a 0.15 $\sigma$ 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 $\sigma$. Qureshi (2018b) shows that the effect of an older sibling's teacher having more than one year of experience on younger siblings' reading test scores is an increase of 0.010-0.013 $\sigma$. The effects we measure in the pre-FSE period correspond to -0.02 $\sigma$ on both transition rates and pass rates, and in the post-FSE period we find effects of 0.02 $\sigma$ on transition rates and 0.01 $\sigma$ on pass rates — 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 & Yuan (2022). The -0.02 $\sigma$ effect we identify in the pre-FSE period ranks between the 10th and 20th percentile of published effect sizes on enrollment. In the post-FSE period, the 0.02 $\sigma$ 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.

As a measure of the policy relevance of these effects, we can estimate how many PSLE takers prior to FSE were adversely affected by their older sibling qualifying for secondary school. We outline this exercise in Appendix Section A.3.4. We conclude that negative sibling spillovers in the pre-FSE period since 2000 were responsible for preventing roughly one region $\times$ cohort's worth of PSLE takers from making the transition to secondary school.

### 4.2 The causal effect of FSE on sibling spillovers

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. Here 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).

#### 4.2.1 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 & 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 (4) articulates our estimation strategy:

$$\begin{aligned} y_{iogstw} =& \beta_1 PSLEpass_o + \beta_2 PSLEpass_o \times HighDropout_w + \beta_3 PSLEpass_o \times Post_t \\ &+ \beta_4 HighDropout_w \times Post_t + \beta_5 PSLEpass_o \times HighDropout_w \times Post_t + \gamma_g + \rho_s + \tau_t + \varepsilon_{iogstw} \end{aligned} \tag{4}$$

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 $\rho_s$ absorb the ward fixed effect, and cohort-level fixed effects $\tau_t$ absorb the $Post_t$ dummy. Our coefficient of interest is $\beta_5$, which measures the interaction effect of $PSLEpass_o \times HighDropout_w \times Post_t$.

Table 6 displays the coefficients from Equation (4).[22] 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.5 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.

[22] Table A.11 performs the same analysis with a continuous measure of ward dropout rates. The results reinforce those presented here.

[Table 6 about here.]

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 (5) is a dynamic version of Equation (4), interacting $PSLEpass_o$ and $HighDropout_w$ with year dummies $\tau_t$ rather than a simple $Post_t$ dummy. This enables the testing of how these interactions change over time.

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

Figure 4 plots the $\beta_{5t}$ coefficients from Equation (5), 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. In the years prior to the introduction of FSE, the gap in differences between high- and low-dropout wards remained constant. Then it became significantly smaller after FSE was implemented, again remaining relatively constant after the shock. This strengthens our confidence that FSE caused the reversal in sibling spillovers we measure.

[Figure 4 about here.]

# 5 Mechanisms

Patterns of parental investment present a plausible mechanism for the sibling spillovers we identify, through reinforcing and compensating investments of the kind proposed by the work of Becker & Tomes (1976), Griliches (1979), and Behrman et al. (1982), among others. Indeed, other research on Tanzania's FSE policy has already found evidence that it affected household investments in primary students' human capital, via increased school attendance, reduced child labor, and selection into better schools (Sandholtz, 2024b). But there are also other possible drivers of sibling spillovers attested in the literature, including a variety of direct sibling effects. Siblings in many contexts have been shown to affect one another independently of parental investment, including through direct human capital transmission (Qureshi, 2018a; Nicoletti & Rabe, 2019), role modeling (Altmejd et al., 2021), information (Dustan, 2018), and returns to scale (Aguirre & Matta, 2021). This broad group of studies is closely related to the peer effects literature, considering siblings as a special kind of peer (Sacerdote, 2011; Epple & Romano, 2011).[23] The vast majority of the empirical literature on spillovers via direct sibling interactions find that these effects are positive or at least non-negative. However, some channels for negative peer effects proposed and empirically attested in the literature could apply to sibling spillovers as well, such as that of the Invidious Comparisons model (Hoxby & Weingarth, 2005; Antecol et al., 2016). de Gendre (2022) finds that higher class ranks for older siblings in the Netherlands have negative spillovers on their younger siblings, and that sibling interactions could be a channel for these effects.

In this section we consider alternative mechanisms and present evidence that the sibling spillover patterns we measure are more consistent with a mechanism of parental investments than direct sibling effects. A limitation of our paper is that our data do not permit the direct measurement of parental investments (or sibling interactions). However, we can construct various tests which help distinguish between the different hypotheses. To demonstrate that the change in sign of spillovers is not the result of post-FSE changes in transition rates or selection among older siblings, we show that spillovers are positive for post-FSE younger siblings of pre-FSE older siblings. To further reinforce that results are not driven by changes in the composition of families in the data, we use a family fixed effects analysis which shows that spillover effects became significantly more positive for post-FSE siblings even within families. We then present heterogeneous treatment effects by poverty, family size, sibling age gap, gender, and students' *ex-ante* ability. The results from these analyses are more consistent with sibling spillovers driven by parental investments than by direct sibling interactions. While we do not discount the possibility that direct sibling effects could play a role in the spillovers we measure, most of the evidence speaks more directly to the role of parental investments and FSE's alleviation of household resource constraints.

## 5.1 Positive post-FSE spillovers are due to pre-FSE older siblings

It is important to note that the "treatment" — qualifying for secondary school by passing the PSLE — had different implications before and after the reform. The fraction of students transitioning to secondary

[23]Other kinds of peers who have been shown to affect students' educational choices and outcomes include neighbors (Barrios-Fernández, 2022), roommates (Sacerdote, 2001; Ahimbisibwe, 2024), and friends (Batista et al., 2025).

school increased dramatically after the reform, a partly mechanical effect of lowering the cost of public secondary school. This implies that many more older siblings who sat the PSLE in the post-FSE period did in fact transition to secondary school than the pre-FSE period. PSLE pass rates also increased after the reform. This was partly driven by households' increased investment in primary students (Sandholtz, 2024b). But we cannot exclude the possibility that the PSLE became easier; if so, this would imply a shift in the ability distribution of older siblings who qualified for secondary school.

Spillovers hypothetically driven by sibling interactions could plausibly be affected by shifts in how and which older siblings responded to qualifying for secondary school. One channel could be that any benefit of older siblings' secondary attendance on their younger siblings could be magnified when the rate of older sibling attendance increased. Another could be that the set of older siblings choosing to attend secondary school under the new rules might turn out to be siblings who benefit younger siblings more. These potential channels could drive changes to sibling spillover effects even in the absence of any change in parental investment strategies.

An initial piece of evidence against this hypothesis comes from the robustness of our main results to the inclusion of different kinds of fixed effects as demonstrated in Table 5. If it is the case that the exam got easier over time, the grade permutation fixed effects we include in all specifications could imply different levels of older sibling ability for different cohorts. Interacting grade permutation fixed effects with cohort fixed effects alleviates this concern by making comparisons only among older siblings who received the same grade permutation in the same year. Column 5 performs precisely this analysis and shows that the coefficients both before and after FSE are unchanged.

To further test this hypothesis, we measure spillover effects on post-FSE younger siblings separately among those whose older sibling sat the PSLE before vs. after the introduction of FSE. Spillovers driven by direct sibling interactions would be likely to appear only for younger siblings whose older siblings finished primary school after FSE's implementation. Spillovers driven by the allocation of family resources, meanwhile, could show up for younger siblings of pre-FSE older siblings who cease to require school fees — even if FSE led to no change in how much the siblings interacted.

[Table 7 about here.]

Table 7 shows that the positive post-FSE spillover effects are driven by older siblings who took the PSLE *before* FSE was implemented. This is consistent with a mechanism of parental investments: parents already paying secondary school fees for an older child would end up with a windfall after these fees were canceled, which could then be invested in younger siblings. In contrast, any explanation of this reversal which relies on direct sibling interactions must reconcile the negative pre-FSE spillovers with the fact that these effects become positive after school fees are abolished but before the entry into secondary school of new cohorts of older siblings under FSE. We are unaware of models which can rationalize these stylized facts using direct sibling interactions alone.

An instrumental variable approach, using an older sibling passing the PSLE as an instrument for their enrollment in secondary school, offers another potential test of the hypothesis that the sibling spillover pattern we measure is the result of changes in older siblings' enrollment response to passing the PSLE. If spillovers from sibling interactions are positive and our main effects are due merely to a larger "first stage" of secondary enrollment among older siblings, we would expect the IV results to be positive in both the pre-FSE and post-FSE periods. We interpret such a test with caution; the exclusion restriction is violated if older siblings passing the PSLE can affect younger siblings' outcomes through a channel besides secondary enrollment. Other literature suggests that the information signal alone provided by passing the exam could be enough to affect parental investment patterns (Dizon-Ross, 2019). With that significant caveat, Table A.9 presents IV results for regressing younger siblings' secondary enrollment on older siblings' secondary enrollment. The naive OLS correlations are positive before and after the reform, consistent with children within families having similar outcomes. But the IV regressions, which make use of the exogenous variation from the passing threshold, show similar patterns as our main results in Table 4. This suggests that the spillovers we measure are not driven by changes in the share or composition of older siblings who attend secondary school.

These tests provide evidence more in line with a mechanism of parental investments than of sibling interactions. We find that the positive spillovers in the post-FSE period are driven by the post-FSE younger siblings of pre-FSE older siblings, the opposite of what would be expected if positive spillovers were driven by increased secondary enrollment among post-FSE older siblings.

### 5.2 Family fixed effects

Studying how sibling spillover effects changed within families over time sheds further light on potential mechanisms. By including family fixed effects, we can hold constant the family's time-invariant characteristics, and remove variation in the composition of families appearing in the data as a potential driver of the results. This allows us to isolate only how an older sibling passing differentially affected her younger siblings 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 is visible among siblings within families, this helps to rule out changes in the set of marginally qualifying older siblings as a mechanism.

For this exercise, we limit our attention to families with at least three siblings spanning the introduction of FSE. Specifically, we restrict the sample to families in which one "oldest" 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 younger sibling: no individual appears more than once in this analysis. The treatment variable is whether the family's "oldest" sibling passed the PSLE or not; our interest is in how the effect of this treatment changed after FSE. As before, we discard groups of more than seven siblings to reduce the incidence of false positives.

Equation (6) describes the estimation framework. We omit the school fixed effects and the older sibling grade permutation effects used in previous estimations, as these are absorbed by the family fixed effects $\phi_f$. Family fixed effects likewise absorb the main effect $PSLEpass_o$, which is defined based only on the oldest sibling of each family. The coefficient of interest is $\beta_1$, on the interaction $PSLEpass_o \times Post_t$, which tells us how the effect of having an older sibling pass the PSLE changed — within families — after the introduction of FSE.

$$y_{ioft} = \beta_1 PSLEpass_o \times Post_t + \phi_f + \tau_t + \varepsilon_{ioft} \tag{6}$$

Table A.12 displays the coefficients on $\beta_1$ from Equation (6), 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[. The interaction term is positive and significant for all windows, and of a similar magnitude to the interaction term in our main sample in Table 5. This indicates that the effect of having an older sibling pass on younger siblings' transition was significantly more positive after FSE's introduction, even when looking only within families. To interpret these estimates as the causal effect of FSE requires a parallel trends 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 (7), we can test this assumption.

$$y_{ioft} = \sum_{t=2014}^{2019} \beta_t \, PSLEpass_o \times \tau_t + \phi_f + \tau_t + \varepsilon_{ioft} \tag{7}$$

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. (Appendix Figure A.6 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 reinforces that the sibling spillover sign reversal we identify is not driven simply by changes in the composition of families at the margin of secondary school qualification.

[Figure 5 about here.]

### 5.3 Heterogeneity by poverty

Testing for differences in sibling spillovers by socioeconomic status can also help illuminate mechanisms. Recent empirical evidence from Tanzania shows that many households struggle to save, and financial constraints are binding for household educational investments (Sandholtz, Carroll, et al., 2024; Burchardi et al., 2024). FSE succeeded in its direct aim of reducing the cost of secondary education: its effect showed up in household survey data as a reduction of about 70% in reported expenditure on school fees for public secondary school students (Sandholtz, 2024b). But other costs remained, including transportation costs and the opportunity cost. For families with a marginal ability to send students to secondary school, the fee abolition 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 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. Observing negative sibling spillovers concentrated in places with more poor families (and/or positive spillovers concentrated in richer places) would therefore be consistent with FSE alleviating resource constraints for families on the margin and thus affecting patterns of parental investment. Conversely, spillovers driven by direct sibling interactions would not be expected to vary by socioeconomic status in the absence of further assumptions about how these might interact with family resource levels.

While our administrative data lack individual- or family-level measures of socioeconomic status, we can proxy for poverty using census data on Tanzania's 161 districts. 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 metric, used 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 first note that although the effect of passing the PSLE on older siblings' own secondary school enrollment is a statistically-significant 2.8 percentage points (10%) lower in poor districts than richer districts prior to FSE, this deficit neither grows nor shrinks after FSE. As such, any differences in sibling spillovers by district poverty rate are not the result of a differential enrollment response to FSE.

We 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 8.

[Table 8 about here.]

We find patterns consistent with family resource constraints playing a meaningful role in sibling spillovers. Before the introduction of FSE, negative sibling spillovers on secondary transition and exam pass rates were statistically significant only for pupils in poorer districts (point estimates were much lower in poor districts, although not distinguishably so at traditional thresholds of statistical significance). After the reform, positive spillovers on secondary transition were significantly smaller in poorer districts — less than half the size of those in richer districts. We also note that the interaction effect of an older sibling passing with the dummy for poorer districts is of the same magnitude before and after FSE, further reinforcing the finding that there was no differential enrollment response to FSE in poor districts that might have explained the change in the sign of the spillover effect. Taken together, these results are consistent with the hypothesis that FSE alleviated resource constraints enough to allow marginal households to make compensating investments — even as many poorer households continued to be constrained to make reinforcing investments.

### 5.4 Heterogeneity by gender

Understanding how sibling spillovers vary by gender can also help illuminate likely mechanisms. Existing research documents many cases of negative spillovers with a gendered component. Collins (2025) shows that across 27 sub-Saharan African countries, the effect of having a younger brother vs. sister on older children's education depends on whether traditional inheritance customs; when legal reforms mandate that all children can inherit, sibling gender effects converge. Vogl (2013) shows that in sub-Saharan Africa as well as in South Asia, marriage market competition reduces girls' educational attainment; Shrestha & Palaniswamy (2017) show in Nepal that men's educational opportunities crowd out human capital investment in their sisters; and Rosenzweig & Schultz (1982) find that intrahousehold resource allocation in India in the mid-20th century was responsive to sex differences in expected earnings, disadvantaging girls.

We begin by presenting descriptive patterns of gendered investment under the status quo in Tanzania. In our context, girls appear to have received significantly less investment than boys prior to FSE. To illustrate this, Figure 6a plots the gender gap by cohort, considering individuals (not pairs) from our sample of students with at least one older sibling. We regress a dummy for whether the student passed the PSLE on a female indicator, interacted with year dummies, including family and year fixed effects. Girls are 6.6 percentage points less likely to pass the PSLE than boys prior to FSE, even controlling for family fixed effects. This strongly suggests that girls tend to be left out of the competition for educational resources within families. Other research in the region has similarly found evidence of households neglecting girls' educational investment. Using household survey data from Tanzania, Morduch (2000) shows that children with a greater share of female siblings had higher educational attainment, which he interprets as consistent with a parental preference to invest in sons when resources are scarce. In a study from Kenya, patterns in girls' schooling likewise suggested that parents perceived higher returns to investing in boys' human capital (Jakiela et al., 2020). Figure 6a also shows that after the introduction of FSE, the gender gap in PSLE pass rates narrowed sharply, nearly disappearing by 2019. The pattern is consistent with FSE alleviating resource constraints in a way that allowed families to invest in children more equally, although other factors may have also played a role.

We next examine how sibling spillovers varied by gender, expanding Equation (2) to include indicators for whether the older and younger siblings are female, and interact these indicators with each other and with our main independent variable (*Older sibling passed the PSLE*) and the Post-FSE indicator. *Transition* is the dependent variable. (Table A.13 in the appendix shows all the coefficients from this regression, as well as from regressions which examine the gender of younger and older siblings separately.)

[Figure 6 about here.]

The patterns of sibling spillovers in Figure 6b are consistent with a mechanism of gendered parental investments under resource constraints which are alleviated by FSE. In the pre-FSE period, negative spillovers were concentrated among boys: younger students of all genders experienced negative spillovers from an older brother passing the PSLE, and younger brothers experienced negative spillovers from an older sibling of any gender passing the PSLE. Only the younger sisters of girls experienced no negative spillovers prior to FSE. In light of the low status-quo investment in girls apparent in Figure 6a, this suggests that resource-constrained families focused investments on boys, with brothers competing for scarce resources and girls more likely to be neglected altogether.

After FSE's introduction, sibling spillovers became positive for children of all genders. This may indicate that FSE allowed families to shift away from a strategy of "picking winners" (usually boys) and freed up household resources to be invested in girls. It also cuts somewhat against the hypothesis that these spillovers result from direct sibling interactions: role-model-based sibling spillovers have been found in many contexts to be dependent on gender combination (Avdeev et al. (2024) and Nicoletti & Rabe (2019) find larger effects for pairs of siblings of the same gender, while Dahl et al. (2024) find that same-sex siblings copy one another but younger brothers are less likely to copy older sisters). This is also consistent with the findings of Parish & Willis (1993), who study sibling spillovers in Taiwan across decades in which that country experienced rapid economic growth. They show that older siblings (especially girls) with many younger siblings fared poorly, but that as incomes increased (both in the cross section and the time series), the pattern of negative sibling spillovers weakened. Taken together, the gendered nature of the spillover effects we find prior to FSE and the uniform effects after the reform are more consistent with a mechanism of parental investments than direct sibling interactions.

### 5.5 Heterogeneity by family size, sibling age gap, and prior academic achievement

**Family size:** Testing how spillover effects vary with family size can help illuminate mechanisms. We have access only to a proxy measure of family size: the number of siblings sitting the PSLE during the window we observe in our data. Resource constraints are likely to be more binding *ceteris paribus* when there are more siblings among whom resources may potentially be divided — which would make spillovers more likely to be negative if they are driven by parental investments. Conversely, if direct sibling interactions drive the spillovers we observe, we would expect them to be more positive for individuals with more siblings with whom to interact. We show in Table A.14 that sibling spillovers are more negative for larger families. These effects are imprecisely measured in the pre-period but very precisely measured in the post period, implying a reduction of 0.3 percentage points in the positive effect of an older sibling passing for every additional sibling.

**Sibling age gaps:** Comparing how effects differ by the age gap between siblings is another helpful way to distinguish among potential mechanisms. If positive sibling spillovers are driven by sibling interactions, they would be expected to be more positive for closely-spaced siblings who attend school together for many years and have more chances to interact. Conversely, shorter age gaps between siblings would amplify the negative effect of resource constraints by giving parents less time to save up for the younger sibling's schooling expenses. In Table A.15, we show that spillover effects grow with the age gap between siblings (although estimates are imprecise for the Pre-FSE period, where age gaps can only be of 1 or 2 years given the window we observe). We can provide an even sharper test of this hypothesis in the post period, which is longer and hence includes more variation in age gaps. We create a binary variable for whether a pair's age gap is at least four years — long enough that the two siblings would never have attended secondary school together (provided that the older sibling was never retained). Table A.16 shows that positive spillovers are twice as large for these "Large age gap" pairs. This helps to rule out many kinds of direct sibling interaction mechanisms.

**Prior academic achievement:** We can also distinguish between potential mechanisms by testing whether the younger siblings who benefit most from their older siblings' academic achievement are those with high or low *ex-ante* measures of ability. If sibling spillovers primarily benefit high-ability younger siblings, this would be consistent with parental reinforcement. If spillovers benefit lower-ability younger siblings, this could indicate parental compensation, but it could also be consistent with sibling interactions — e.g. if low-ability siblings benefit more from the tutoring of older siblings. However, benefits from sibling interactions would be expected to be concentrated among closely-spaced siblings who attend school together for more years, while parental compensation behavior would be more likely to produce benefits independently of sibling age gaps.

Our data provide a pre-PSLE measure of younger siblings' ability, but unfortunately only for cohorts in the post-FSE period. We link students in our analysis sample to results from the earliest national standardized test students take in primary school: the Standard Four National Assessment (SFNA). Students sit this exam at the end of Grade 4 in primary school, three years before the PSLE. Unlike the PSLE, this test does not determine pupils' opportunities for further schooling, but it does provide an *ex-ante* measure of student ability. Grade 4 exam scores are 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 Grade 4 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 Grade 4 exam in year $t-3$. This method yields Grade 4 information for 39% of the sibling pairs in our analysis sample for these years. Students whose Grade 4 information is available are balanced across the treatment threshold in our main specification (see Table 3). We interact the treatment variable from Equation (1) with a binary indicator for whether younger sibling $i$ achieved a low grade (C or worse) in Grade 4. We also interact these variables with an indicator for whether the age gap between the siblings was large ($>=$ 4 years) to help determine whether differential effects by younger sibling ability are the result of sibling interactions or parental investments.

Table A.17 shows that in the post-FSE period, the benefits from exposure to an older sibling's achievement accrued to lower-ability younger siblings. (We note that the coefficient on "Low ability younger sibling" is highly and significantly negative for all outcomes, indicating that the Grade 4 exam does indeed contain information about younger siblings' ability.) Positive spillover effects on the probabilities of primary-to-secondary school transition and of passing the PSLE are driven by younger siblings with lower Grade 4 exam scores. Spillovers are negative for higher-ability younger siblings. This shows that spillovers mitigate preexisting differences.[24] These results are consistent with compensatory parental investments aimed at equalizing educational outcomes across children (Berry et al., 2025).[25] The even columns show that this relationship is unaffected by sibling age gap, suggesting that the effects we observe are due to parental compensation rather than sibling interactions.

These analyses offer further evidence that the spillovers we observe are more likely to be driven by parental investments than by direct sibling interactions. They are consistent with the hypothesis that resource constraints, partly alleviated by FSE, played an important role in these spillovers.

### 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 schools in Tanzania and beyond have sometimes strategically excluded low-performing students from being allowed even to sit the PSLE (Cilliers et al., 2021; Gilligan et al., 2022). To test whether our results might be partly explained by changes to the composition of younger siblings sitting the exam, we look at the full set of PSLE takers and measure the effect of a given student marginally passing the PSLE on the likelihood that other students with the same last name in the same ward show up in the PSLE data in subsequent years. Table A.19 shows the results. Passing the PSLE has no effect on the extensive margin of whether any younger siblings sit the PSLE. It has a negative effect on the total number of younger siblings sitting the exam, but the magnitude of this

[24] These patterns are reversed for the outcome of whether younger siblings achieve a very high grade, however.

[25] This also implies 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).

effect is very small, and similar in the pre- and post-FSE periods (-0.01 to -0.02 siblings). We conclude that the composition of siblings sitting the PSLE is unlikely to explain our results.

Other work on sibling spillovers has found an important role for school selection. Figlio et al. (2023) show that the younger siblings of grade-retained third-graders in Florida end up in better schools than the younger siblings of non-retained students. Dustan (2018) shows that older siblings' admission to a given high school makes younger siblings more likely to go to the same or similar schools. In Table A.20 we test for effects on whether younger siblings sit the exam at a different school than the older sibling, or 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.

In the appendix, we also test for whether spillovers are different in urban vs. non-urban areas. We do not find evidence of differential spillover effects in urban wards as compared to non-urban wards (see Table A.18).

# 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 reversed. These results are robust to a wide variety of alternative estimation choices. A triple-differences design leveraging variation in FSE exposure shows that FSE caused the reversal, suggesting that the alleviation of financial constraints played an important role. A family fixed effects model indicates parental investments as a more likely mechanism than sibling interactions, and analyses of heterogeneity by poverty, gender, sibship characteristics, and prior academic achievement are on balance more consistent with this mechanism than with alternatives.

Our findings may indicate that parents who can afford it prefer to compensate lower-ability children rather than reinforce the advantages of higher-ability children; free public education may allow families to do so who otherwise would not. This is consistent with the findings of research on parental preferences over intrahousehold allocations in both poor and rich countries (Berry et al., 2025). The absolute number of children FSE caused to enroll in secondary school specifically through sibling spillovers is small relative to the policy's large direct effect on access. But the reversal of spillovers illuminates another way FSE improved lives which would otherwise be difficult to measure. No parent wants to have to choose which of their children will enjoy opportunities denied to the others. For many families, the introduction of FSE appears to have spared them from having to do so.

This can inform debates about expansions of public services in the developing world. The politics of public service provision is not always straightforward. There is some empirical evidence that electorates reward governments which improve public service quality (Cox et al., 2024; Biasi & Sandholtz, 2024; Dias et al., 2025; Sandholtz & Vicente, 2024; Leff Yaffe et al., 2025; Armand et al., 2025). In other cases, however, voters (or other important political actors) prefer to see resources allocated differently (Bursztyn, 2016; Boas et al., 2021; Sandholtz, 2023, 2024a).[26] In the case of free public secondary schooling, detractors argue that the policy functions as a regressive transfer to the relatively wealthy families who pay fees under the status quo (Lewin, 2009; Davoodi et al., 2003; Malala Fund, 2016; Wokadala & Barungi, 2015). Our paper underscores that universal school access benefits society more broadly than has previously been appreciated — not only by providing additional gains for younger siblings, but also by affording families more flexibility in how to allocate scarce educational resources. This perspective helps explain why, despite the concerns, abolishing school fees has broadly been politically popular (Harding & Stasavage, 2014; Crawfurd, 2024).

[26] See Hartmann & Sandholtz (2024) for a review of this literature.

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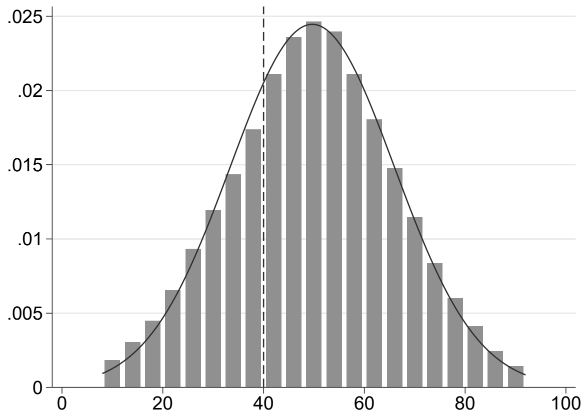
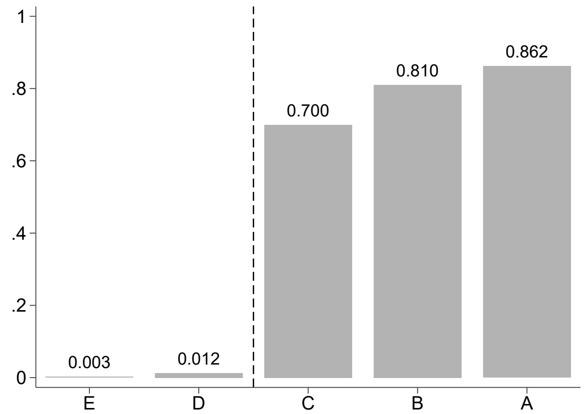
# Figures

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



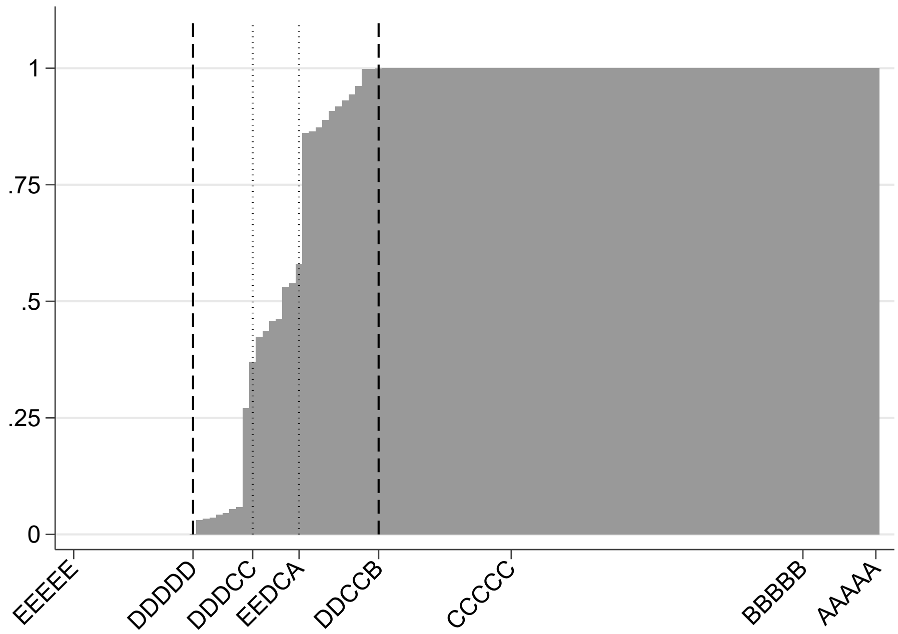

(a) Histogram of PSLE scores




(b) Secondary enrollment, by own PSLE score

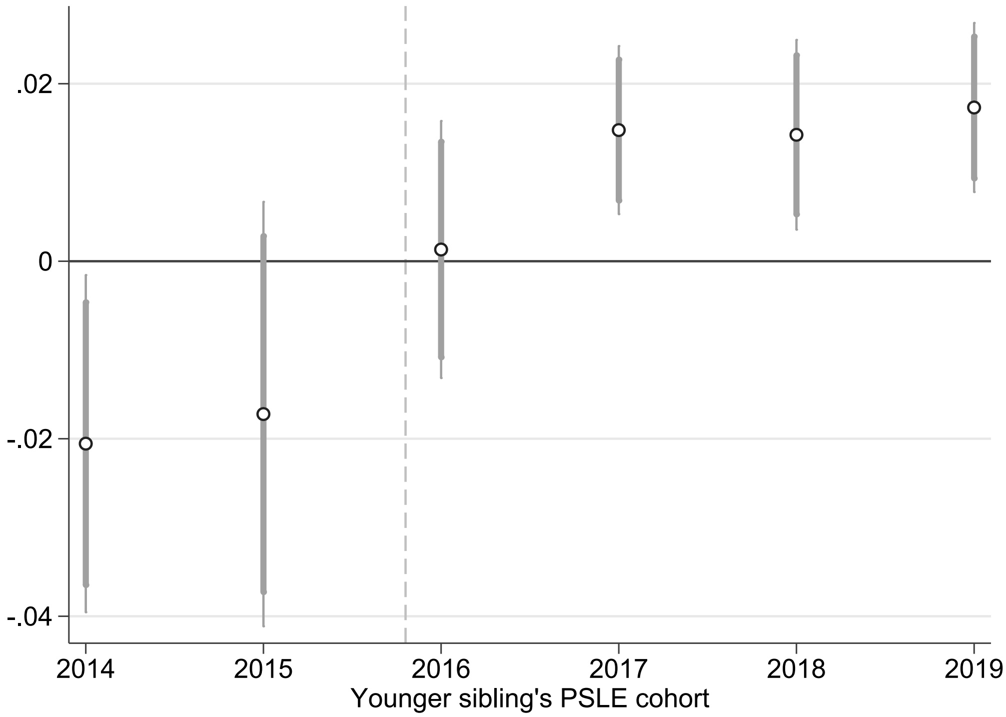
*Sample includes all individual "younger siblings" identified under our preferred sibling matching procedure. Panel 1a shows the histogram of imputed overall PSLE scores, created by averaging the midpoints of the percentage windows corresponding to each of the student's five subject letter grades, with 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.*

Figure 2: PSLE letter grade combination and probability of passing



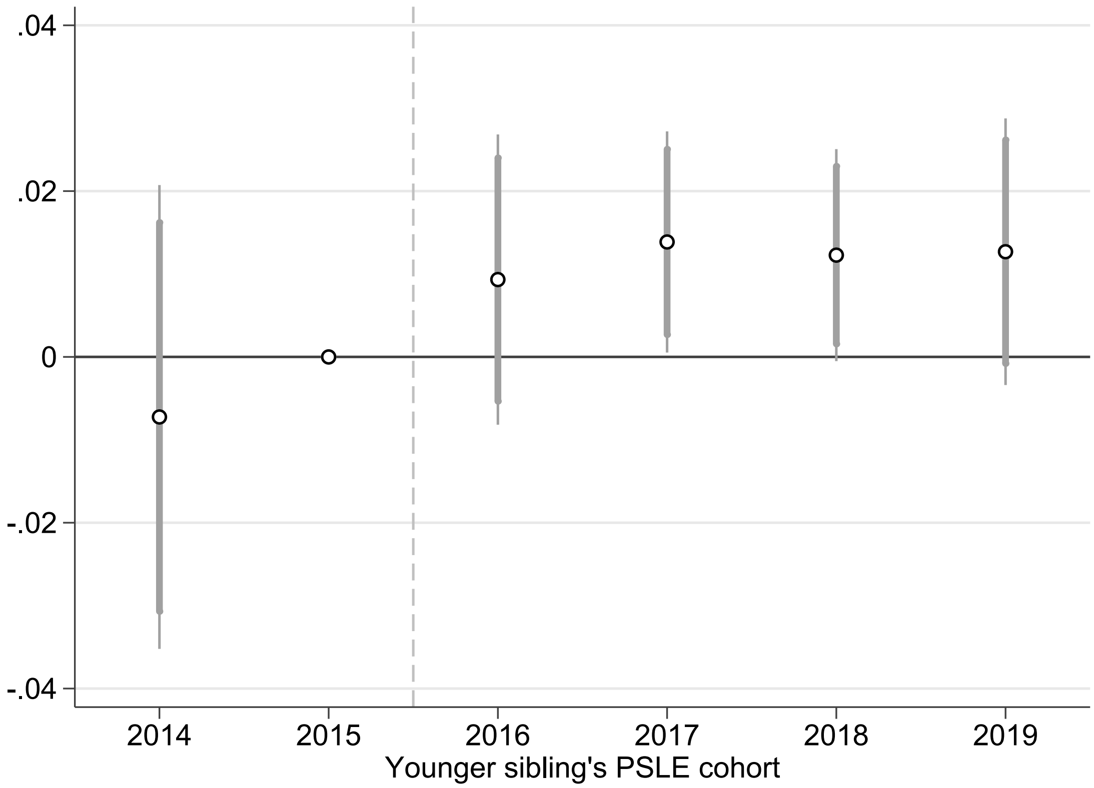

*Bar graph showing the fraction of PSLE takers who received a passing grade overall for each observed combination of letter grades on the five subject exams: English, Math, Social studies, Science, and Swahili. For ease of exposition, this graph plots letter grade combinations without regard for which subject the grade was received in; it includes 121 observed combinations, from a theoretically possible 126. Thick dashed lines demarcate the set of grade combinations corresponding to a passing probability in the open interval of ]0,1[. Thin dotted lines demarcate grade combinations with a passing probability in the open interval of ]0.3,0.7[. Our identification strategy uses the more restrictive grade permutation fixed effects, comparing the younger siblings of older students who not only received the same grade combination, but the same set of letter grades in the each subject — differing only in whether their overall score was above or below the passing threshold.*

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



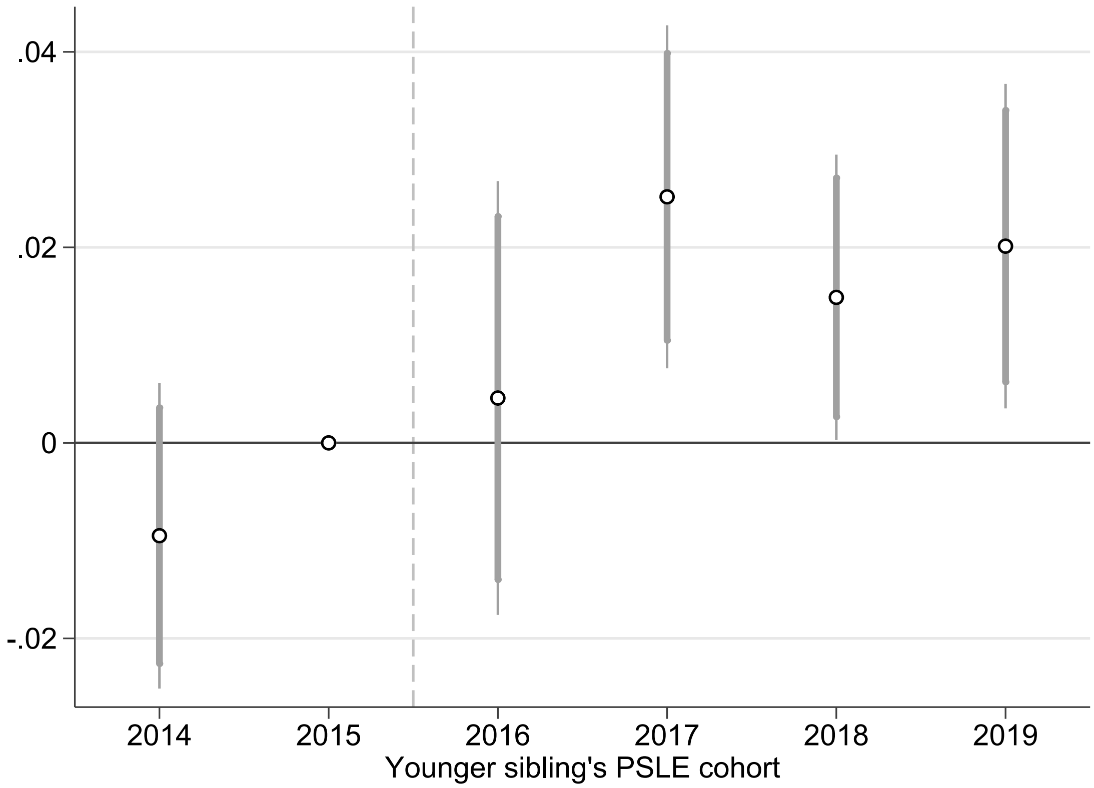

*$\beta_t$ coefficients and 90% and 95% confidence intervals from regressing younger sibling's secondary transition on a dummy for whether their older sibling passed the secondary school qualification exam interacted with year dummies, as in Equation (3) — with the sample limited to the younger siblings of older siblings whose grade permutation corresponded to a passing probability in the open interval ]0,1[. The estimation includes fixed effects for younger sibling's cohort and school, as well as older sibling's grade permutation $\times$ cohort $\times$ school.*

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



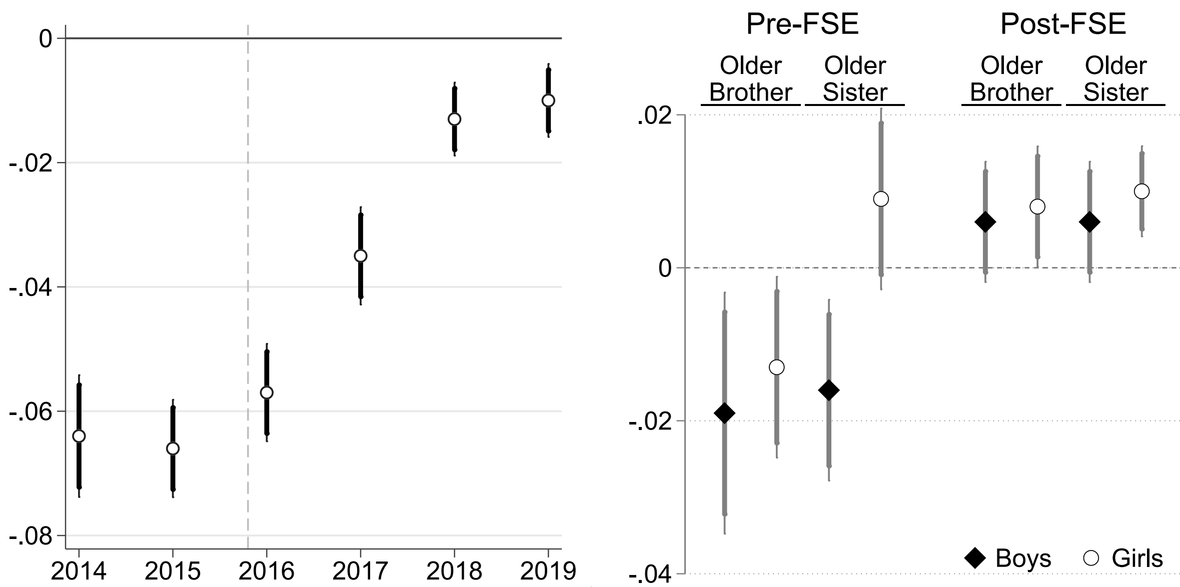

*Coefficients from regressing younger siblings' secondary school transition on a triple interaction of year dummies with an above-median 2014 ward dropout rate dummy and older sibling's PSLE pass result (conditional on having a passing probability in the open interval ]0,1[). Triple interaction coefficients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

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




*Coefficients from regressing younger siblings' secondary school transition on an interaction of year dummies with their 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 6: Gender




(a) Descriptive: within-family gender gap in PSLE pass rates

(b) Sibling spillovers on secondary transition by gender combination

*Panel (a) takes individual PSLE takers (not sibling pairs) as the unit of observation, regressing a PSLE pass indicator on a dummy for female interacted with year dummies, including family and year fixed effects. For the omitted year 2015, the main effect of 'female' is displayed. For other years, the plot displays the linear combination of the coefficient on 'female' plus the coefficient on the gender $\times$ year interaction term. Confidence intervals are displayed at 90% and 95%, with standard errors clustered at the ward level. Panel (b) shows coefficients from regressing younger siblings' secondary school transition on a triple interaction between the 'Older sibling pass' dummy (conditional on being near the threshold) and dummies for whether the older and younger siblings are female. Regressions performed separately for the pre-FSE and post-FSE period, as in equation (1). The estimates and 90% and 95% confidence intervals displayed are those of the linear combinations reflecting the full effect on each group, i.e. adding together the relevant set of coefficients.*

# Tables

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 | 0.199 | 0.202 |
| | (0.421) | (0.399) | (0.402) |
| Government school | 0.966 | 0.982 | 0.984 |
| | (0.182) | (0.132) | (0.124) |
| Full name unique in nationwide cohort | 0.979 | 0.991 | 0.991 |
| | (0.144) | (0.093) | (0.093) |
| Pass PSLE | 0.708 | 0.733 | 0.733 |
| | (0.455) | (0.443) | (0.442) |
| Grade A or B on PSLE | 0.256 | 0.238 | 0.232 |
| | (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 (2014) | 0.818 | 0.830 | 0.832 |
| | (0.122) | (0.120) | (0.119) |
| N | 5,200,947 | 526,433 | 137,131 |

**Notes:** 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 (i.e., an 'older sibling') in the same ward whose PSLE score had a passing probability in the open interval ]0,1[. (Only individuals with unique last names in their ward-cohort are considered as possible 'older siblings' under our sibling matching procedure.) 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.

Table 2: Effect of passing the 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 the PSLE | 0.280*** | 0.278*** | 0.278*** | 0.644*** | 0.648*** | 0.649*** |
| | (0.006) | (0.009) | (0.009) | (0.010) | (0.014) | (0.014) |
| N | 204,260 | 45,691 | 45,536 | 102,511 | 20,192 | 20,096 |
| Mean, PSLE fail | 0.009 | 0.013 | 0.013 | 0.022 | 0.029 | 0.029 |

**Notes:** Standard errors clustered by grade permutation in parentheses. Unit of observation is a unique older sibling from the analysis sample of sibling pairs. 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 row labeled 'Mean, PSLE fail' displays the secondary transition rate 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

Table 3: Balance: Full period

| | ]0, 1[ | | | ]0.3, 0.7[ | | |
|---|---|---|---|---|---|---|
| | Mean (fail) (SD) | Difference (SE) | N | Mean (fail) (SD) | Difference (SE) | N |
| **Older siblings:** | | | | | | |
| Female | 0.591 (0.492) | -0.010* (0.006) | 612,938 | 0.581 (0.493) | -0.007 (0.007) | 142,500 |
| Name length | 20.807 (2.717) | 0.011 (0.023) | 612,938 | 20.819 (2.722) | 0.031 (0.035) | 142,500 |
| Muslim name (inferred) | 0.196 (0.397) | -0.000 (0.003) | 612,938 | 0.195 (0.396) | 0.004 (0.003) | 142,500 |
| First name commonness | 0.138 (0.188) | 0.000 (0.001) | 612,938 | 0.139 (0.186) | 0.001 (0.001) | 142,500 |
| Private school | 0.002 (0.048) | -0.000 (0.000) | 612,828 | 0.003 (0.052) | 0.000 (0.000) | 142,481 |
| *p-value, joint significance F-test:* | | 0.639 | | | 0.798 | |
| **Younger siblings:** | | | | | | |
| Female | 0.536 (0.499) | -0.003 (0.002) | 612,938 | 0.534 (0.499) | -0.004* (0.002) | 142,500 |
| Name length | 20.908 (2.615) | -0.005 (0.013) | 612,938 | 20.904 (2.616) | 0.009 (0.015) | 142,500 |
| Muslim name (inferred) | 0.197 (0.398) | 0.000 (0.002) | 612,938 | 0.203 (0.402) | -0.002 (0.003) | 142,500 |
| First name commonness | 0.131 (0.178) | -0.000 (0.001) | 612,938 | 0.132 (0.178) | 0.000 (0.001) | 142,500 |
| Grade 4 exam score available | 0.392 (0.488) | 0.004 (0.003) | 321,025 | 0.388 (0.487) | 0.010*** (0.003) | 73,169 |
| Low Grade 4 exam score | 0.787 (0.410) | -0.004 (0.003) | 124,637 | 0.792 (0.406) | -0.002 (0.004) | 26,038 |
| *p-value, joint significance F-test:* | | 0.556 | | | 0.406 | |
| **Sibling pairs:** | | | | | | |
| Age gap | 2.830 (1.507) | -0.066*** (0.014) | 612,938 | 2.772 (1.489) | -0.064*** (0.015) | 142,500 |
| Last name of local politician | 0.003 (0.053) | -0.000 (0.000) | 612,938 | 0.003 (0.052) | -0.000 (0.000) | 142,500 |
| Siblings same gender | 0.521 (0.500) | -0.009*** (0.002) | 612,938 | 0.518 (0.500) | -0.006*** (0.002) | 142,500 |
| *p-value, joint significance F-test:* | | 0.009 | | | 0.000 | |

**Notes:** Standard errors clustered by older sibling's grade permutation. Unit of observation is a sibling pair. Means and SDs shown among those whose older sibling failed the PSLE. Differences shown are calculated among those with older siblings whose PSLE score was in the corresponding passing probability window, and include fixed effects for older sibling's grade permutation and younger sibling's school and cohort. Name length measured in number of characters. 'Muslim name (inferred)' indicates that the student had at least one name from a list of common names among Muslims. 'First name commonness' denotes the percentage of students in the cohort with the student's first name (measured on a scale of 0-100). 'Low Grade 4 exam score' denotes a grade of C or lower. 'Last name of local politician' indicates the siblings share a last name with a candidate in the 2015 local council elections in the same ward. Significance levels: * <0.1, ** <0.05, *** <0.01

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 | -0.008* | -0.012*** | 0.005 | 0.008*** | 0.004** | 0.009*** |
| | (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 | 84,750 | 85,478 | 85,478 | 520,678 | 525,485 | 525,485 |
| Mean, PSLE fail | 0.223 | 0.587 | 0.142 | 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: Effect of older sibling's qualification on younger sibling's transition, interacted with post-FSE indicator, varying fixed effects

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Older sibling passed | -0.019** | -0.020** | -0.017** | -0.017** | -0.018** | -0.018 |
| | (0.008) | (0.008) | (0.008) | (0.008) | (0.008) | (0.011) |
| | [0.003] | [0.003] | [0.003] | [0.003] | [0.004] | [0.006] |
| Older sibling passed × Post-FSE | 0.029*** | 0.030*** | 0.028*** | 0.029*** | 0.029*** | 0.030*** |
| | (0.009) | (0.009) | (0.009) | (0.009) | (0.009) | (0.011) |
| | [0.003] | [0.003] | [0.003] | [0.003] | [0.003] | [0.005] |
| Passed + Passed x Post-FSE | 0.010*** | 0.010*** | 0.011*** | 0.011*** | 0.011*** | 0.013*** |
| | (0.002) | (0.002) | (0.002) | (0.002) | (0.002) | (0.004) |
| N | 607,411 | 607,411 | 606,917 | 606,917 | 606,795 | 502,411 |
| Mean (pre-FSE) | 0.240 | 0.240 | 0.240 | 0.240 | 0.240 | 0.239 |
| Younger year FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Younger school FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Older grade FE | ✓ | ✓ | ✓ | ✓ | | |
| Older year FE | | ✓ | | ✓ | | |
| Older school FE | | | ✓ | ✓ | ✓ | |
| Older grade × year FE | | | | | ✓ | |
| Older grade × year × 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. 'Older grade' refers to grade permutation of older siblings; the last two columns interact grade permutation fixed effects with older sibling year and older sibling year × older sibling school. Significance levels: * <0.1, ** <0.05, *** <0.01

Table 6: 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 | -0.019** |
| | (0.008) |
| Older sibling pass $\times$ High-dropout ward | -0.011** |
| | (0.006) |
| Older sibling pass $\times$ Post-FSE | 0.027*** |
| | (0.008) |
| High-dropout ward $\times$ Post-FSE | 0.094*** |
| | (0.005) |
| Older sibling pass $\times$ High-dropout $\times$ Post-FSE | 0.017*** |
| | (0.006) |
| Pass + Pass $\times$ High | -0.030*** |
| | (0.010) |
| Pass + Pass $\times$ Post | 0.008*** |
| | (0.002) |
| Pass + Pass $\times$ High + Pass $\times$ Post + Pass $\times$ High $\times$ Post | 0.013*** |
| | (0.003) |
| N | 604,567 |
| 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

Table 7: Spillover effects among post-FSE younger siblings,
by whether older sibling sat PSLE before or after FSE

| | Older pre-FSE, Younger post-FSE | | | Both took PSLE post-FSE | | |
|---|---|---|---|---|---|---|
| | (1) Transition | (2) PSLE Pass | (3) High score | (4) Transition | (5) PSLE Pass | (6) High score |
| Older sibling passed PSLE | 0.014*** | 0.009*** | 0.009*** | 0.002 | 0.003 | 0.010*** |
| | (0.002) | (0.002) | (0.002) | (0.004) | (0.003) | (0.003) |
| N | 356,059 | 359,327 | 359,327 | 163,119 | 164,673 | 164,673 |
| Mean dep. var. | 0.597 | 0.750 | 0.246 | 0.630 | 0.764 | 0.257 |
| $R^2$ | 0.189 | 0.185 | 0.242 | 0.226 | 0.219 | 0.274 |
| 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. 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 8: 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 sibling passed | -0.005 | -0.007 | 0.012*** | 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 | -0.014*** | -0.007*** | -0.001 | -0.003 |
| | (0.006) | (0.006) | (0.004) | (0.003) | (0.002) | (0.002) |
| Pass + Pass × Poorer | -0.012** | -0.017*** | -0.002 | 0.004* | 0.004* | 0.007*** |
| | (0.005) | (0.005) | (0.005) | (0.002) | (0.002) | (0.002) |
| N | 84,750 | 85,478 | 85,478 | 520,678 | 525,485 | 525,485 |
| Mean dep. var. (Poorer) | 0.217 | 0.584 | 0.151 | 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

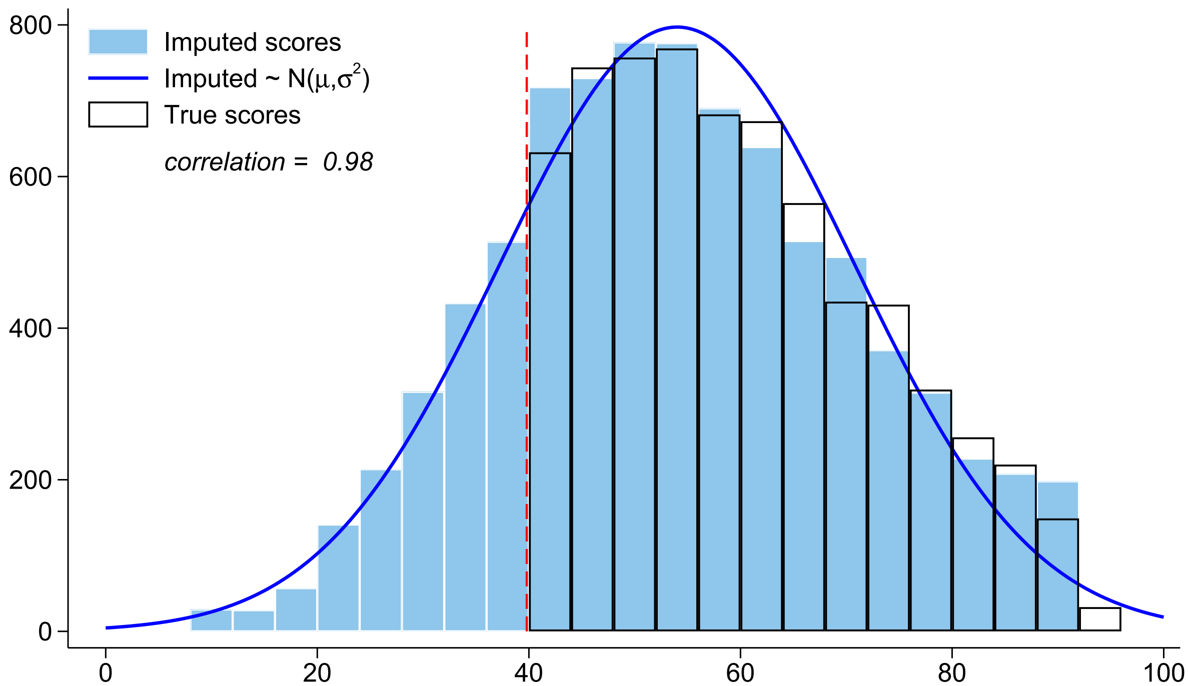
# A Appendix

## A.1 Imputed PSLE scores

Because we do not observe precise raw PSLE scores, most of our analyses rely on grade permutations rather than continuous test scores. However, we do use imputed measures of continuous PSLE scores to demonstrate the smoothness of the distribution (see Figure 1a). We impute continuous overall PSLE scores by adding up the midpoints of the score windows corresponding to the student's letter grade in each of the five PSLE subject exams. If our imputation method were to obscure manipulation around the score threshold, this could cast doubt on our findings.

We are able to compare our imputed PSLE scores with true PSLE scores for a subset of the sample: we have granular data on raw PSLE test scores for students who *passed* the PSLE in 2015 in the province of Mwanza (Tanzania's second-most populous). We can perform a high-quality merge of this data with our sample of (unique) younger siblings using student ID, yielding a set of 6,664 PSLE takers for whom we can compare our imputed measure of PSLE scores with the true measure. Among this sample, the correlation between the two measures is 0.98.

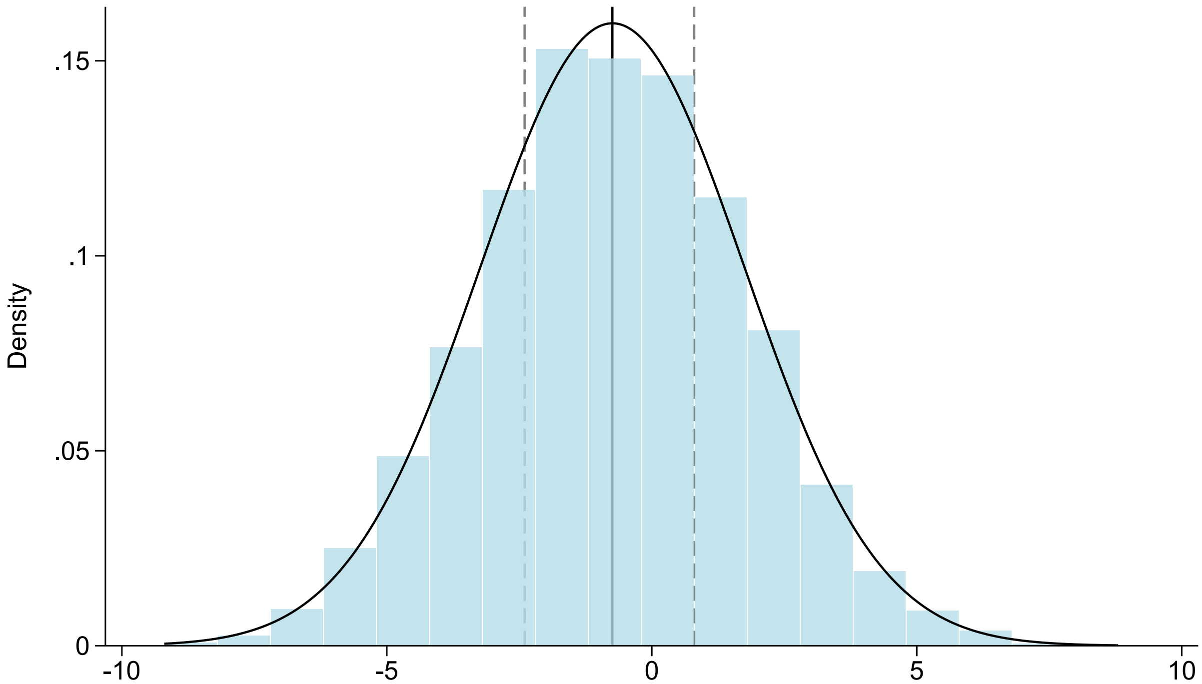
Figure A.1: Distribution of imputed PSLE scores and scores among passers (Mwanza 2015)




*Histograms of 2015 PSLE takers in Mwanza who pertain to our sample of younger siblings. PSLE scores scaled to a range of 0–100; the red dotted line at 40% indicates the passing threshold. The blue histogram shows the frequency of imputed PSLE scores of all students in the sample (N = 8,391). The blue line overlays a normal distribution with a mean of 54.0 and a standard deviation of 16.8 as in the data. The black histogram shows the frequency of true PSLE scores for all students in the sample who passed the PSLE (N = 6,664). Among passers, the correlation between the two measures is 0.98.*

Figure A.1 plots histograms of these two measures. In light blue is our imputed measure of overall PSLE score for younger siblings in our sample who took the PSLE in Mwanza in 2015 ($N$ = *8,391*). The blue line overlays a normal distribution. The black histogram shows true overall PSLE scores for the subset of these students who passed the exam ($N$ = *6,664*). The two histograms match each other closely over the region of common support, and neither shows evidence of heaping around the passing threshold (indicated by the red dashed line at 40%).

Figure A.2: Distribution of difference between true score and grade permutation midpoint (Mwanza 2015 passers)




*Histogram showing the difference between the grade permutation midpoint and the true PSLE score. Sample consists of individual younger siblings who took the PSLE in Mwanza in 2015 and passed with an unambiguous grade permutation — i.e. the lowest possible raw score was a passing grade. $N = 5,079$. Both measures are expressed in percentage points out of 100. The solid line shows the mean at -0.74; the dotted lines show the inter-quartile range of [-2.40, 0.80].*

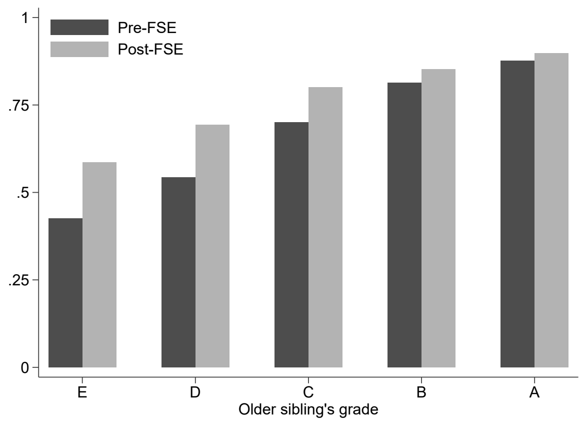
The true scores from Mwanza 2015 also allow us to test for the amount of variation in scores within grade permutations. Our identification strategy relies on the assumption that students with the same grade permutation are similar, such that variation in PSLE pass status within a grade permutation is as good as random. Our imputed PSLE scores denote the midpoint of possible raw PSLE scores: they are constructed by summing the midpoints of the score windows for each subject grade. So the distribution of differences between these imputed scores and the true scores functions as a measure of the dispersion of scores within grade permutations. (We limit the exercise to students whose grade permutation was unambiguously a passing grade to avoid artificially truncating the distribution, and we exclude the small number of students whose "true" scores are inconsistent with the grade permutation provided by NECTA.) This leaves us with 5,079 students.

Figure A.2 displays a histogram of these differences. The distribution is normal, consistent with Figure A.1. Half of scores fall within a window of [-2.4, 0.8] percentage points around the grade permutation's midpoint. 90% fall within [-4.8, 3.2] percentage points. Assuming the distributions look similar for the lower-scoring students who provide our identifying variation, this demonstrates that while large score differences within grade permutation are theoretically possible, in practice the normal distribution of scores implies that most students' scores are clustered near the midpoint.

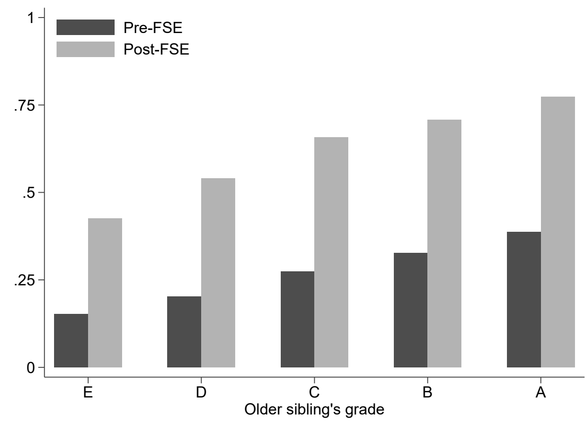
## A.2 Other descriptive analyses

Figure A.3: Descriptive: younger siblings' outcomes, by older siblings' grades

(a) Pass PSLE




(b) Transition to secondary




*Averages computed using full sample of sibling pairs under the preferred matching procedure. 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.4: PSLE letter grade combination and probability of passing

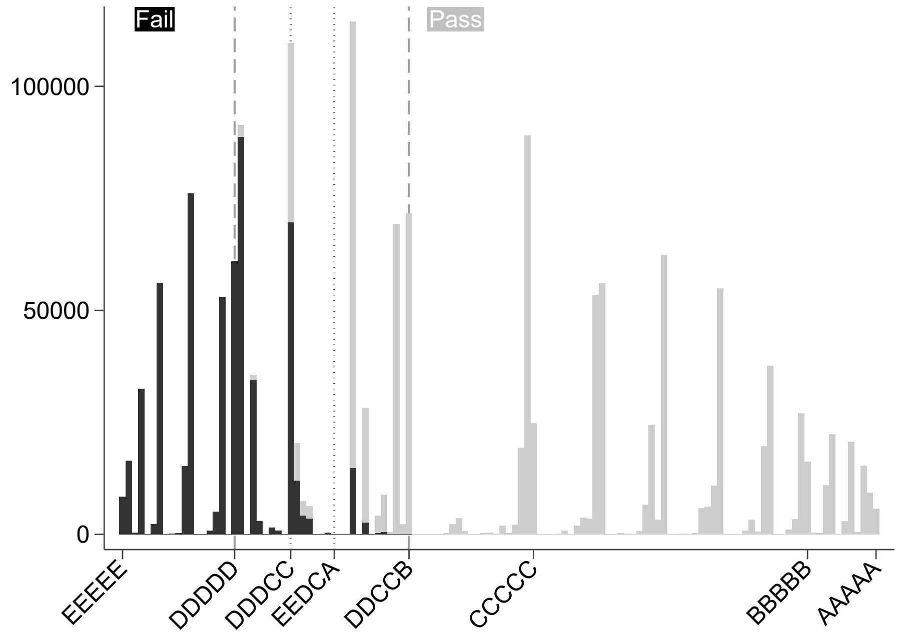



Table A.1 summarizes the number of schools, test takers, and pass rates for the full PSLE sample from the years 2013-2019. It also displays our proxy for the transition rate, i.e., what fraction of a given year's PSLE takers' names show up exactly in the Form Two National Assessment (FTNA) of secondary school two years later.

Table A.2 summarizes PSLE pass rates and transition rates specifically for individual older and younger siblings in our sample, before and after FSE.

Although the data we have on any given student is limited, Table A.3 shows that the measures we have are predictive of performance and socioeconomic status. This table regresses secondary transition and ward-level poverty (a dummy for whether the ward has an above-median incidence of grass/leaf thatched roofs) on the covariates in Table 3. In each column, the unit of observation is an individual student (not a sibling pair); whether the regression considers older or younger siblings from our sibling sample is indicated in the column titles. In each case, we find significant correlations for both younger and older siblings. Girls are significantly less likely to transition to secondary school. Longer names are associated with a reduced likelihood of secondary enrollment (though they are more common in less-poor districts). Students with traditionally Muslim names are less likely to enroll in secondary school and more likely to live in poor districts.[27] Students with more common first names are more likely to transition to secondary school (though commoner names are also more prevalent in poorer wards). Finally, students in private primary schools are overwhelmingly more likely to enroll in secondary school, and less likely to live in poor districts.

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 | 75,506 | 77,084 | 53,927 | 52,193 | 36,717 | 16,424 | — |
| Number of younger siblings | — | 28,080 | 56,331 | 74,248 | 105,442 | 126,429 | 136,328 |
| Urban (proportion) | — | 0.197 | 0.200 | 0.205 | 0.194 | 0.192 | 0.200 |

The first three panels use data from the full set of PSLE takers. The proxy for primary-to-secondary-school transition (obtained using FTNA data) is explained in Subsection 2.2. The fourth panel ("Preferred sample") includes only unique individuals in our preferred sample (i.e., older siblings are PSLE takers with a unique last name in their ward who share a last name with at least one PSLE taker in their ward in a subsequent year; younger siblings are PSLE takers who share a last name with at least one PSLE taker in their ward in a previous year); more detail in Section 3. 'Number of older [younger] siblings' is a count of individuals, not sibships.

Table A.2: Secondary school transition and PSLE pass rates by group (0–1 scale)

| | Transition rates | | PSLE pass rates | |
|---|---|---|---|---|
| | Older | Younger | Older | Younger |
| Pre-FSE | 0.158 | 0.239 | 0.494 | 0.622 |
| N | 205,150 | 83,727 | 206,517 | 84,411 |
| Post-FSE | 0.425 | 0.606 | 0.605 | 0.754 |
| N | 104,494 | 438,507 | 105,334 | 442,447 |

Means measured on a scale of 0–1. Sample consists of individual older or younger siblings in our analysis sample. Transition rates measured only among PSLE takers whose full name is unique in their cohort nationwide.

[27] This is consistent with evidence from across sub-Saharan Africa on the education gap between Christians and Muslims (Platas, 2018).

Table A.3: Covariates' predictiveness of performance and socioeconomic status

| | Transition | | | | Poor District | |
|---|---|---|---|---|---|---|
| | Older | Older | Younger | Older | Older | Younger |
| Female | -0.017*** | -0.017*** | -0.014*** | -0.013*** | -0.004 | -0.002 |
| | (0.003) | (0.003) | (0.002) | (0.002) | (0.003) | (0.002) |
| Name length | -0.004*** | -0.003*** | -0.004*** | -0.004*** | -0.004*** | -0.004*** |
| | (0.001) | (0.000) | (0.000) | (0.000) | (0.001) | (0.000) |
| Muslim name (inferred) | -0.034*** | -0.021*** | -0.026*** | -0.025*** | 0.083*** | 0.093*** |
| | (0.004) | (0.003) | (0.003) | (0.003) | (0.004) | (0.003) |
| First name commonness | 0.061*** | 0.074*** | 0.072*** | 0.077*** | 0.035*** | 0.023*** |
| | (0.008) | (0.008) | (0.006) | (0.006) | (0.010) | (0.007) |
| Private school | | 0.158*** | | 0.225*** | -0.336*** | -0.334*** |
| | | (0.009) | | (0.007) | (0.008) | (0.006) |
| N | 84513 | 86388 | 175369 | 175353 | 86978 | 176832 |
| Mean | 0.177 | 0.177 | 0.245 | 0.245 | 0.494 | 0.492 |
| Year FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| School FE | ✓ | | ✓ | | | |

**Notes:** Dependent variables listed in column group titles. Unit of observation in each column is a unique individual student from the main sample (older or younger sibling depending on the column) in the pre-FSE period. All regressions (regressands, regressors, fixed effects) correspond to students' own characteristics and outcomes, not those of siblings. No restriction on passing probability is imposed on the sample; all individuals from our full sample of siblings are included. 'Transition' is only measured for students whose full name is unique across the country within their cohort. Robust standard errors in parentheses. Significance levels: * <0.1, ** <0.05, *** <0.01

## A.3 Sibling spillovers: additional analyses and robustness

### A.3.1 Balance, measured separately in the pre-FSE and post-FSE periods

Table A.4: Balance: Pre-FSE

| | ]0, 1[ | | | ]0.3, 0.7[ | | |
|---|---|---|---|---|---|---|
| | Mean (fail) (SD) | Difference (SE) | N | Mean (fail) (SD) | Difference (SE) | N |
| **Older siblings:** | | | | | | |
| Female | 0.576 (0.494) | -0.023*** (0.009) | 85,478 | 0.561 (0.496) | -0.017 (0.014) | 16,998 |
| Name length | 20.725 (2.829) | 0.013 (0.031) | 85,478 | 20.707 (2.875) | 0.091 (0.067) | 16,998 |
| Muslim name (inferred) | 0.193 (0.395) | 0.004 (0.006) | 85,478 | 0.187 (0.390) | 0.008 (0.008) | 16,998 |
| First name commonness | 0.137 (0.186) | -0.002 (0.003) | 85,478 | 0.140 (0.185) | -0.001 (0.004) | 16,998 |
| Private school | 0.003 (0.056) | -0.001 (0.001) | 85,459 | 0.003 (0.057) | -0.000 (0.002) | 16,996 |
| *p-value, joint significance F-test:* | | 0.217 | | | 0.645 | |
| **Younger siblings:** | | | | | | |
| Female | 0.546 (0.498) | -0.008 (0.007) | 85,478 | 0.551 (0.497) | -0.006 (0.010) | 16,998 |
| Name length | 20.937 (2.638) | -0.059 (0.040) | 85,478 | 20.895 (2.630) | 0.067* (0.035) | 16,998 |
| Muslim name (inferred) | 0.195 (0.396) | 0.001 (0.006) | 85,478 | 0.197 (0.398) | 0.002 (0.011) | 16,998 |
| First name commonness | 0.135 (0.183) | 0.005*** (0.002) | 85,478 | 0.136 (0.182) | 0.008** (0.003) | 16,998 |
| *p-value, joint significance F-test:* | | 0.006 | | | 0.000 | |
| **Sibling pairs:** | | | | | | |
| Age gap | 1.335 (0.472) | -0.018** (0.007) | 85,478 | 1.349 (0.477) | -0.026*** (0.009) | 16,998 |
| Last name of local politician | 0.004 (0.059) | -0.002* (0.001) | 85,478 | 0.003 (0.056) | -0.001 (0.001) | 16,998 |
| Siblings same gender | 0.527 (0.499) | -0.020*** (0.006) | 85,478 | 0.531 (0.499) | -0.016* (0.009) | 16,998 |
| *p-value, joint significance F-test:* | | 0.000 | | | 0.002 | |

**Notes:** Standard errors clustered by older sibling's grade permutation. Unit of observation is a sibling pair in which the younger sibling sat the PSLE before the introduction of FSE. Means and SDs shown among those whose older sibling failed the PSLE. Differences shown are calculated among those with older siblings whose PSLE score was in the corresponding passing probability window, and include fixed effects for older sibling's grade permutation and younger sibling's school and cohort. Name length measured in number of characters. 'Muslim name (inferred)' indicates that the student had at least one name from a list of common names among Muslims. 'First name commonness' denotes the percentage of students in the cohort with the student's first name (measured on a scale of 0-100). 'Last name of local politician' indicates the siblings share a last name with a candidate in the 2015 local council elections in the same ward. Significance levels: * <0.1, ** <0.05, *** <0.01

Table A.5: Balance: Post-FSE

| | ]0, 1[ | | | ]0.3, 0.7[ | | |
|---|---|---|---|---|---|---|
| | Mean (fail) (SD) | Difference (SE) | N | Mean (fail) (SD) | Difference (SE) | N |
| **Older siblings:** | | | | | | |
| Female | 0.594 (0.491) | -0.008 (0.006) | 525,485 | 0.585 (0.493) | -0.006 (0.007) | 121,877 |
| Name length | 20.824 (2.696) | 0.011 (0.024) | 525,485 | 20.838 (2.699) | 0.027 (0.034) | 121,877 |
| Muslim name (inferred) | 0.196 (0.397) | -0.001 (0.003) | 525,485 | 0.196 (0.397) | 0.004 (0.003) | 121,877 |
| First name commonness | 0.138 (0.188) | 0.000 (0.001) | 525,485 | 0.139 (0.186) | 0.001 (0.001) | 121,877 |
| Private school | 0.002 (0.046) | -0.000 (0.000) | 525,393 | 0.003 (0.051) | 0.000 (0.000) | 121,861 |
| *p-value, joint significance F-test:* | | 0.797 | | | 0.603 | |
| **Younger siblings:** | | | | | | |
| Female | 0.534 (0.499) | -0.002 (0.002) | 525,485 | 0.532 (0.499) | -0.003 (0.002) | 121,877 |
| Name length | 20.904 (2.611) | 0.005 (0.014) | 525,485 | 20.908 (2.615) | 0.005 (0.018) | 121,877 |
| Muslim name (inferred) | 0.198 (0.398) | 0.000 (0.002) | 525,485 | 0.203 (0.403) | -0.001 (0.002) | 121,877 |
| First name commonness | 0.131 (0.177) | -0.001 (0.001) | 525,485 | 0.132 (0.177) | -0.001 (0.001) | 121,877 |
| Grade 4 exam score available | 0.392 (0.488) | 0.004 (0.003) | 321,025 | 0.388 (0.487) | 0.010*** (0.003) | 73,169 |
| Low Grade 4 exam score | 0.787 (0.410) | -0.004 (0.003) | 124,637 | 0.792 (0.406) | -0.002 (0.004) | 26,038 |
| *p-value, joint significance F-test:* | | 0.363 | | | 0.011 | |
| **Sibling pairs:** | | | | | | |
| Age gap | 3.112 (1.468) | -0.072*** (0.016) | 525,485 | 3.019 (1.466) | -0.067*** (0.015) | 121,877 |
| Last name of local politician | 0.003 (0.052) | -0.000 (0.000) | 525,485 | 0.003 (0.051) | 0.000 (0.000) | 121,877 |
| Siblings same gender | 0.519 (0.500) | -0.006** (0.003) | 525,485 | 0.516 (0.500) | -0.002 (0.003) | 121,877 |
| *p-value, joint significance F-test:* | | 0.022 | | | 0.000 | |

**Notes:** Standard errors clustered by older sibling's grade permutation. Unit of observation is a sibling pair in which the younger sibling sat the PSLE after the introduction of FSE. Means and SDs shown among those whose older sibling failed the PSLE. Differences shown are calculated among those with older siblings whose PSLE score was in the corresponding passing probability window, and include fixed effects for older sibling's grade permutation and younger sibling's school and cohort. Name length measured in number of characters. 'Muslim name (inferred)' indicates that the student had at least one name from a list of common names among Muslims. 'First name commonness' denotes the percentage of students in the cohort with the student's first name (measured on a scale of 0-100). 'Low Grade 4 exam score' denotes a grade of C or lower. 'Last name of local politician' indicates the siblings share a last name with a candidate in the 2015 local council elections in the same ward. Significance levels: * <0.1, ** <0.05, *** <0.01

### A.3.2 Narrowing the passing probability window

Table A.6: Spillover effects, narrowing "passing probability" window

| | ]0, 1[ | ]0.1, 0.9[ | ]0.2, 0.8[ | ]0.3, 0.7[ |
|---|---|---|---|---|
| Older sibling passed | -0.019** | -0.013* | -0.014* | -0.014* |
| | (0.008) | (0.007) | (0.008) | (0.008) |
| Older sibling passed × Post-FSE | 0.029*** | 0.020** | 0.019** | 0.019** |
| | (0.009) | (0.008) | (0.008) | (0.008) |
| N | 607,411 | 234,184 | 141,536 | 141,191 |
| Mean (pre-FSE) | 0.240 | 0.244 | 0.242 | 0.241 |
| Older sibling's score FE | ✓ | ✓ | ✓ | ✓ |
| School FE | ✓ | ✓ | ✓ | ✓ |
| Year FE | ✓ | ✓ | ✓ | ✓ |

**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 (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. 'School FE' denotes binary indicators for the school in which each younger sibling sat the PSLE. 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

### A.3.3 Controls and alternate sibling matching procedures

Table A.7 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 qualitatively similar estimates as well. Appendix Section A.7 provides more detail on these robustness checks.

Table A.7: 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 | -0.019** | -0.020*** | -0.021*** | -0.020** | -0.021** | -0.013** | -0.013** |
| | (0.008) | (0.007) | (0.007) | (0.009) | (0.009) | (0.006) | (0.006) |
| Pass × Post-FSE | 0.030*** | 0.029*** | 0.030*** | 0.029*** | 0.030*** | 0.022*** | 0.023*** |
| | (0.009) | (0.008) | (0.008) | (0.011) | (0.011) | (0.006) | (0.006) |
| Pass + Pass × Post | 0.010*** | 0.009*** | 0.009*** | 0.009*** | 0.010*** | 0.009** | 0.010** |
| | (0.002) | (0.003) | (0.003) | (0.002) | (0.002) | (0.004) | (0.004) |
| N | 607,296 | 413,998 | 413,917 | 668,892 | 668,892 | 328,646 | 328,571 |
| 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 length and commonness of name and indicators for female, private school, and Muslim (inferred from name), for older and younger siblings; and the sibling pair's age gap and gender match. Significance levels: * <0.1, ** <0.05, *** <0.01

### A.3.4 Policy relevance of spillover effects

To measure the policy magnitude of the spillovers we observe, we calculate how many PSLE takers prior to FSE were adversely affected by the negative spillovers of their older siblings' passing. We first estimate how many younger sibling PSLE takers were likely to have had older siblings who passed the PSLE. We consider unique older siblings in the year 2013, the first year in our microdata, for whom we have the greatest number of years in which to observe younger siblings. We use the strictest sibling matching algorithm from Table A.7, employing middle names (and discarding matches leading to "families" of more than one child per cohort as likely false positives). We find that the average unique older siblings in this dataset who passed the PSLE had 1.47 younger siblings in the following 6 years of PSLE takers, with an average age gap of 3.45 years. This is likely a lower bound on the true number of younger siblings for a number of reasons.[28] Our dataset includes 69,959 unique PSLE passers from 2013 who we can link to at least one younger sibling; there were 427,606 total PSLE passers that year. We therefore conservatively estimate that at least 16.4% of PSLE passers are older siblings, and that conditional on being older siblings they have at least 1.47 younger siblings. The potential pool of younger siblings who could be negatively

[28] To wit: we drop any potential older siblings whose middle and last name are not unique within their ward; we consider only sibling matches within ward, meaning we miss siblings who sit the PSLE in a different ward; we fail to match any siblings with misspelled names; we fail to match any siblings with an age gap of more than 6 years.

impacted by their older sibling's passing of the PSLE is therefore the number of PSLE passers in a cohort times .164 times 1.47. We can measure how large this group is starting in the early 21st century. From 2000 through 2012, there were 4,320,558 pupils who passed the PSLE; under our assumptions, this corresponds to 1,041,600 younger siblings of PSLE passers. If we apply the estimate of -0.8 percentage points on the likelihood of younger siblings transitioning to secondary school (see Table 4) to this group, we estimate that negative sibling spillovers prevented at least 8,333 younger siblings from transitioning to secondary school who otherwise would have. This is more than made the transition in a given cohort in most regions prior to FSE. Similarly, if we apply the estimate of -1.2 percentage points on the likelihood of younger siblings passing the PSLE to this group, we estimate that negative sibling spillovers prevented 11,730 younger siblings from qualifying for secondary school who otherwise would have.

For the 6,922,148 PSLE passers from 2012-2022 whose younger siblings finished primary school after FSE, our assumptions from above yield an estimated 1,668,791 younger siblings. Projecting our pre-FSE estimates onto this group (in reverse) implies that 13,350 of them transitioned to secondary school and 20,025 of them passed the PSLE who would not have done so in the absence of FSE. (If we were to further consider that each of these younger siblings experienced the post-FSE positive spillover effects presented in Table 4, that would imply that 13,350 younger siblings transitioned to secondary school and 6,675 passed the PSLE in this period because of sibling spillovers. However, given the analysis of Section 5.1 showing that the positive spillovers in the post-FSE period were mostly confined to the younger siblings of pre-FSE older siblings, we do not find it plausible to apply the positive spillovers to later cohorts in considering the policy impact of the reform, choosing instead to focus on the elimination of the negative spillovers.)

### A.3.5 No effects on early or late secondary transition

Table A.8: Spillover effects from older siblings' qualification for secondary school

| | (1) Transition (1 year early) | (2) Transition (1 year late) | (3) Transition (2 years late) |
|---|---|---|---|
| Older sibling passed PSLE | -0.000 | 0.000 | 0.000 |
| | (0.000) | (0.001) | (0.001) |
| Older sibling pass × Post-FSE | 0.000 | 0.000 | -0.000 |
| | (0.000) | (0.001) | (0.001) |
| N | 603,143 | 603,143 | 603,143 |
| Mean (pre-FSE) | 0.002 | 0.025 | 0.008 |

**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. Significance levels: * <0.1, ** <0.05, *** <0.01

### A.3.6 Instrumental Variables regression

Table A.9: IV: Spillover effects from older siblings' transition to secondary school on younger siblings' enrollment

| | Pre-FSE | | Post-FSE | |
|---|---|---|---|---|
| | Naive OLS | IV | Naive OLS | IV |
| Older sibling transitioned | 0.031*** | -0.030 | 0.019*** | 0.019*** |
| | (0.006) | (0.019) | (0.002) | (0.005) |
| N | 84,096 | 84,096 | 516,646 | 516,646 |
| Mean (control) | 0.223 | 0.223 | 0.587 | 0.587 |
| F (first stage) | | 2,629 | | 1,777 |
| Older score FE | Older | Older | Older | Older |
| School FE | Younger | Younger | Younger | Younger |
| Year FE | Younger | Younger | Younger | Younger |

**Notes:** Standard errors clustered by older sibling's grade permutation in parentheses. The unit of observation is a unique older-younger sibling pair, and the dependent variable is whether the younger sibling transitioned to secondary school. Secondary school transition is only measured for students whose full name is unique across the country within their cohort. All regressions include fixed effects for older sibling's grade permutation and for younger sibling's school and year. 'Older sibling transitioned' is instrumented by 'Older sibling passed PSLE' in Columns 2 and 4. 'Mean (control)' reports the mean of the dependent variable for the group for whom the regressor equals 0. 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. First stage F-statistics computed according to Kleibergen & Paap (2006). Significance levels: * <0.1, ** <0.05, *** <0.01

Table A.9 presents IV results for regressing older siblings' secondary enrollment on younger siblings' secondary enrollment, using an older sibling passing the PSLE as an instrument for their enrollment in secondary school. We present these results for expositional purposes despite the fact that the exclusion restriction is likely violated: the information signal alone provided by passing the exam could be enough to affect parental investment patterns even if it had no effect on older siblings' enrollment (Dizon-Ross, 2019). However, insofar as the analysis functions as a test of the hypothesis that the sibling spillovers we measure result from changes in older siblings' enrollment response to passing the PSLE, the evidence suggests this is not the case. If spillovers from sibling interactions are positive and our main effects are due merely to a larger "first stage" of secondary enrollment among older siblings, we would expect the IV results to be positive in both the pre-FSE and post-FSE periods. In fact, the IV regressions show similar patterns as our main results in Table 4: negative spillovers pre-FSE ($p = 0.11$), positive spillovers afterward ($p < 0.01$). This suggests that the spillovers we measure are not driven by changes in the share or composition of older siblings who attend secondary school.

### A.3.7 Cohort-wise spillover effects

Table A.10: 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 | 0.092*** | 0.095*** | 0.098*** |
| | (0.004) | (0.005) | (0.008) |
| Younger sibling's PSLE cohort = 2016 | 0.261*** | 0.259*** | 0.263*** |
| | (0.005) | (0.005) | (0.008) |
| Younger sibling's PSLE cohort = 2017 | 0.423*** | 0.409*** | 0.391*** |
| | (0.007) | (0.009) | (0.008) |
| Younger sibling's PSLE cohort = 2018 | 0.450*** | 0.438*** | 0.415*** |
| | (0.007) | (0.009) | (0.007) |
| Younger sibling's PSLE cohort = 2019 | 0.465*** | 0.457*** | 0.435*** |
| | (0.007) | (0.009) | (0.009) |
| Older sibling passed the PSLE = 1 x (Younger's cohort = 2014) | -0.027*** | -0.021** | -0.027*** |
| | (0.006) | (0.009) | (0.009) |
| Older sibling passed the PSLE = 1 x (Younger's cohort = 2015) | -0.016* | -0.020* | -0.007 |
| | (0.009) | (0.010) | (0.006) |
| Older sibling passed the PSLE = 1 x (Younger's cohort = 2016) | -0.000 | 0.002 | -0.001 |
| | (0.004) | (0.004) | (0.005) |
| Older sibling passed the PSLE = 1 x (Younger's cohort = 2017) | 0.012*** | 0.014*** | 0.013*** |
| | (0.003) | (0.003) | (0.005) |
| Older sibling passed the PSLE = 1 x (Younger's cohort = 2018) | 0.013*** | 0.011*** | 0.012*** |
| | (0.003) | (0.003) | (0.005) |
| Older sibling passed the PSLE = 1 x (Younger's cohort = 2019) | 0.010*** | 0.009** | 0.008 |
| | (0.003) | (0.003) | (0.005) |
| N | 607,411 | 668,892 | 328,646 |
| 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

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.[29] 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

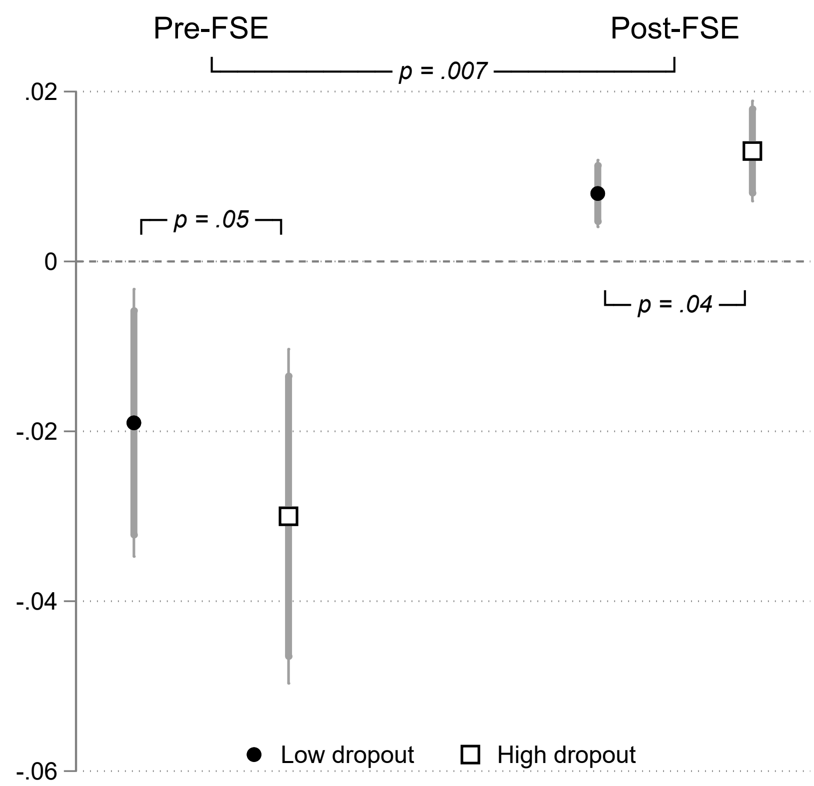
[29] While these two coefficients are not statistically different from one another, the point estimate is higher in 2015 than in 2014. Insofar as this reflects a real increase in the point estimate, it could be due to the fact that although students from the 2015 cohort sat the PSLE before the announcement 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.

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.10 for detailed estimates.

## A.4 Additional evidence on the effect of FSE

Figure A.5 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.5: 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.*

Table A.11 replicates the analysis of Table 6 with a continuous measure of ward dropout rates rather than a dummy for above/below median. The coefficients point in the same direction, although they are necessarily measured with less precision. If we consider having an older sibling pass the PSLE to be the treatment variable, then the coefficient which provides a formal test of whether treatment's heterogeneous effect across wards changed after the introduction of FSE is the coefficient on "Older sibling pass × Post-FSE × Ward dropout rate." This coefficient is positive and statistically significant at the 5% significance level.

Table A.11: Effect of older sibling passing on younger sibling's transition, interacted with post-FSE indicator and ex-ante ward dropout rate (continuous)

| | (1) |
|---|---|
| Older sibling passed PSLE | -0.012 |
| | (0.018) |
| Older sibling pass $\times$ Ward dropout rate | -0.019 |
| | (0.019) |
| Older sibling pass $\times$ Post-FSE | 0.002 |
| | (0.018) |
| Post-FSE $\times$ Ward dropout rate | 0.449*** |
| | (0.017) |
| Older sibling pass $\times$ Post-FSE $\times$ Ward dropout rate | 0.044** |
| | (0.021) |
| Pass + Pass $\times$ Dropout rate | -0.031*** |
| | (0.009) |
| Pass + Pass $\times$ Post | -0.010 |
| | (0.008) |
| Pass + Pass $\times$ Dropout rate + Pass $\times$ Post + Pass $\times$ Dropout rate $\times$ Post | 0.016*** |
| | (0.003) |
| N | 604,567 |
| Mean (pre-FSE) | 0.240 |

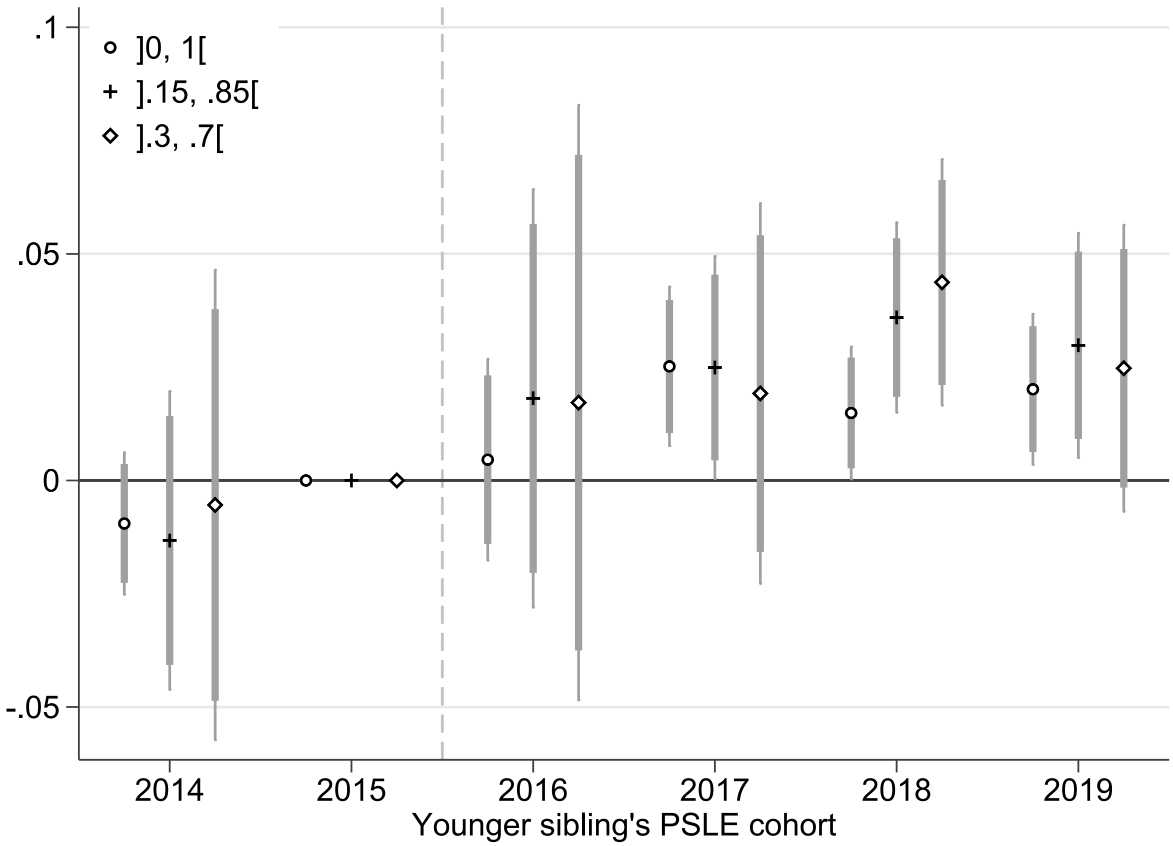
**Notes:** Standard errors clustered by grade permutation in parentheses. 'Transition' is only measured for students whose full name is unique across the country within their cohort. 'Ward dropout rate' is a continuous variable measuring the share of 2014 PSLE takers who failed to transition to secondary school in the ward where the student sat the PSLE. 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

Table A.12: 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 $\times$ Post-FSE | 0.020*** | 0.032*** | 0.029*** |
| | (0.006) | (0.008) | (0.009) |
| N | 106,822 | 43,567 | 28,896 |
| Mean (pre-FSE) | 0.240 | 0.241 | 0.240 |
| Family FE | ✓ | ✓ | ✓ |
| Year FE | ✓ | ✓ | ✓ |

**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 $\times$ 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

Figure A.6: 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.5 Additional heterogeneity

Table A.13: Spillover effects on secondary transition (Heterogeneity by gender)

| | Pre-FSE | | | Post-FSE | | | All |
|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| Older sibling passed PSLE (Pass) | -0.017*** | -0.016*** | -0.019** | 0.006** | 0.007*** | 0.006 | -0.025*** |
| | (0.005) | (0.005) | (0.008) | (0.003) | (0.003) | (0.004) | (0.009) |
| Female younger sibling ($F_y$) | -0.019*** | | -0.017*** | 0.009*** | | 0.002 | -0.016*** |
| | (0.003) | | (0.006) | (0.002) | | (0.005) | (0.005) |
| Female older sibling ($F_o$) | | -0.007 | -0.005 | | -0.003 | -0.008** | 0.005 |
| | | (0.004) | (0.007) | | (0.002) | (0.004) | (0.007) |
| Pass × $F_y$ | 0.016*** | | 0.006 | 0.003 | | 0.002 | 0.007 |
| | (0.004) | | (0.009) | (0.002) | | (0.006) | (0.009) |
| Pass × $F_o$ | | 0.014*** | 0.003 | | 0.001 | 0.000 | -0.009 |
| | | (0.005) | (0.010) | | (0.003) | (0.006) | (0.011) |
| Pass × $F_o$ × $F_y$ | | | 0.019 | | | 0.002 | 0.024* |
| | | | (0.014) | | | (0.009) | (0.014) |
| Pass × Post-FSE | | | | | | | 0.033*** |
| | | | | | | | (0.010) |
| Pass × $F_y$ × Post-FSE | | | | | | | -0.005 |
| | | | | | | | (0.010) |
| Pass × $F_o$ × Post-FSE | | | | | | | 0.010 |
| | | | | | | | (0.010) |
| Pass × $F_o$ × $F_y$ × Post-FSE | | | | | | | -0.022 |
| | | | | | | | (0.014) |
| $F_o$ × $F_y$ | | | -0.003 | | | 0.011 | -0.006 |
| | | | (0.009) | | | (0.008) | (0.008) |
| Post-FSE | | | | | | | 0.462*** |
| | | | | | | | (0.009) |
| $F_y$ × Post-FSE | | | | | | | 0.018*** |
| | | | | | | | (0.007) |
| $F_o$ × Post-FSE | | | | | | | -0.014** |
| | | | | | | | (0.007) |
| $F_o$ × $F_y$ × Post-FSE | | | | | | | 0.016 |
| | | | | | | | (0.010) |
| N | 84,750 | 84,750 | 84,750 | 520,678 | 520,678 | 520,678 | 607,411 |
| Mean dep. var. | 0.239 | 0.239 | 0.239 | 0.608 | 0.608 | 0.608 | 0.555 |

**Notes:** Standard errors clustered by grade permutation in parentheses. Outcome, 'Transition,' is only measured for students whose full name is unique across the country within their cohort. All regressions include (younger sibling's) school and cohort 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

Table A.14: Spillover effects from older siblings' achievement (Heterogeneity by family 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 | 0.006 | 0.021* | 0.022*** | 0.018*** | 0.017*** |
| | (0.008) | (0.011) | (0.011) | (0.005) | (0.005) | (0.003) |
| Number of siblings | 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 No. of siblings | -0.001 | -0.004 | -0.004 | -0.003*** | -0.003*** | -0.002*** |
| | (0.002) | (0.003) | (0.002) | (0.001) | (0.001) | (0.001) |
| N | 84,750 | 85,478 | 85,478 | 520,678 | 525,485 | 525,485 |
| Mean dep. var. | 0.239 | 0.622 | 0.160 | 0.608 | 0.754 | 0.250 |
| Mean no. siblings | 3.239 | 3.246 | 3.246 | 3.293 | 3.298 | 3.298 |
| $R^2$ | 0.291 | 0.333 | 0.385 | 0.176 | 0.170 | 0.229 |
| 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.15: Spillover effects from older siblings' achievement (Heterogeneity by age gap)

| | 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 | -0.021* | -0.024*** | -0.006 | -0.004 | 0.001 | -0.000 |
| | (0.013) | (0.008) | (0.011) | (0.003) | (0.004) | (0.003) |
| Age gap | -0.005 | -0.003 | 0.001 | 0.002* | 0.002*** | 0.001*** |
| | (0.006) | (0.004) | (0.005) | (0.001) | (0.001) | (0.001) |
| Sibling passed x Age gap | 0.010 | 0.009 | 0.008 | 0.004*** | 0.001 | 0.003*** |
| | (0.008) | (0.006) | (0.007) | (0.001) | (0.001) | (0.001) |
| N | 84,750 | 85,478 | 85,478 | 520,678 | 525,485 | 525,485 |
| Mean dep. var. | 0.239 | 0.622 | 0.160 | 0.608 | 0.754 | 0.250 |
| Mean age gap | 1.332 | 1.332 | 1.332 | 2.988 | 2.989 | 2.989 |
| $R^2$ | 0.292 | 0.333 | 0.385 | 0.176 | 0.170 | 0.229 |
| 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. 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.16: Spillover effects from older siblings' achievement
(Heterogeneity by 'large age gap' of 4+ years)

| | 4+ year gap (only 2017/9) | | |
|---|---|---|---|
| | (1) Transition | (2) PSLE Pass | (3) High score |
| Older sibling passed the PSLE | 0.007*** | 0.004** | 0.006*** |
| | (0.002) | (0.002) | (0.002) |
| Large age gap | 0.005** | 0.005** | 0.004*** |
| | (0.002) | (0.002) | (0.002) |
| Sibling passed x Large age gap | 0.006** | 0.004* | 0.006*** |
| | (0.003) | (0.002) | (0.002) |
| N | 438,968 | 443,145 | 443,145 |
| Mean dep. var. | 0.638 | 0.767 | 0.267 |
| Share large age gap | 0.420 | 0.420 | 0.420 |
| $R^2$ | 0.179 | 0.179 | 0.239 |
| 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. 'Transition' 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. '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, and whose younger siblings sat the PSLE between 2017 and 2019, inclusive. Significance levels: * <0.1, ** <0.05, *** <0.01

Table A.17: Heterogeneity by younger siblings' prior academic achievement

| | (1) Transition | (2) Transition | (3) PSLE Pass | (4) PSLE Pass | (5) High score | (6) High score |
|---|---|---|---|---|---|---|
| Older sibling passed PSLE | -0.018*** | -0.028*** | -0.011** | -0.016** | 0.020*** | 0.017* |
| | (0.006) | (0.009) | (0.005) | (0.006) | (0.006) | (0.010) |
| Older pass × Younger low ability | 0.028*** | 0.040*** | 0.027*** | 0.035*** | -0.017** | -0.010 |
| | (0.006) | (0.010) | (0.006) | (0.008) | (0.007) | (0.012) |
| Older pass × Large age gap | | 0.016 | | 0.005 | | 0.004 |
| | | (0.010) | | (0.007) | | (0.011) |
| Old pass × Young low × Large gap | | -0.018 | | -0.011 | | -0.013 |
| | | (0.012) | | (0.008) | | (0.012) |
| Low ability younger sibling | -0.221*** | -0.238*** | -0.184*** | -0.200*** | -0.361*** | -0.371*** |
| | (0.005) | (0.009) | (0.004) | (0.007) | (0.007) | (0.010) |
| Large age gap | | -0.011* | | -0.009* | | -0.003 |
| | | (0.006) | | (0.005) | | (0.007) |
| Younger low ability × Large age gap | | 0.030*** | | 0.028*** | | 0.018** |
| | | (0.008) | | (0.007) | | (0.008) |
| N | 124,078 | 124,078 | 124,119 | 124,119 | 124,119 | 124,119 |
| Mean dep. var. | 0.639 | 0.639 | 0.786 | 0.786 | 0.298 | 0.298 |
| Older sibling's score FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| School FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Year FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |

**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 A.18: 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 | -0.008* | -0.010** | 0.002 | 0.008*** | 0.005** | 0.007*** |
| | (0.005) | (0.005) | (0.005) | (0.002) | (0.002) | (0.002) |
| Older sibling passed x Urban | -0.002 | -0.000 | 0.010 | -0.002 | -0.005** | 0.004 |
| | (0.006) | (0.005) | (0.008) | (0.003) | (0.002) | (0.003) |
| Passed + Passed x Urban | -0.011* | -0.010** | 0.012* | 0.006* | -0.000 | 0.011*** |
| | (0.006) | (0.005) | (0.007) | (0.003) | (0.002) | (0.003) |
| N | 80,945 | 81,629 | 81,629 | 497,714 | 502,234 | 502,234 |
| Mean dep. var. | 0.239 | 0.622 | 0.161 | 0.607 | 0.755 | 0.252 |
| Mean dep. var. (Urban) | 0.260 | 0.738 | 0.220 | 0.750 | 0.847 | 0.328 |
| $R^2$ | 0.290 | 0.334 | 0.387 | 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

## A.6 Alternative mechanisms

### A.6.1 Extensive margin

Table A.19 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; Gilligan et al., 2022). To analyze this possibility, we examine the sample of PSLE takers from years 2013-2018 whose last name is unique within ward×cohort and who received a grade permutation with a passing probability between (but not including) zero and one. This sample could be considered "potential older siblings;" we construct it using the same restrictions on last name uniqueness and grade permutations as we use in our main sample, but we include all qualifying PSLE takers whether or not they have any younger siblings. We then identify siblings for this sample by matching each cohort by last name within ward to all younger cohorts of PSLE takers (2014-2019). As in our main sample, we do not impose restrictions of last name uniqueness on younger sibling cohorts, and we exclude groups of more than seven PSLE takers (one per year) as likely 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 with a passing probability between (but not including) zero and one, and including fixed effects for year, school, and grade permutation.

Table A.19: Extensive margin: effect of passing PSLE on whether younger siblings sit PSLE

| | Pre-FSE | | | Post-FSE | | |
|---|---|---|---|---|---|---|
| | Sibling sat PSLE in following yr | Any younger sibling sat PSLE | Num. younger siblings sat PSLE | Sibling sat PSLE in following yr | Any younger sibling sat PSLE | Num. younger siblings sat PSLE |
| Pass PSLE | -0.004* | -0.003 | -0.017** | -0.001 | -0.002 | -0.011** |
| | (0.002) | (0.003) | (0.008) | (0.002) | (0.003) | (0.005) |
| N | 450,562 | 450,562 | 450,562 | 361,543 | 361,543 | 361,543 |
| Mean dep. var. | 0.151 | 0.470 | 1.012 | 0.176 | 0.305 | 0.477 |

**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 sibling 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 siblingss 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

Passing the PSLE has no effect on whether any younger sibling sat the PSLE, either before or after FSE. A small negative effect on whether a sibling sat the PSLE in the following year is evident in the pre-FSE period (0.4 percentage points, $p < 0.10$). Passing the PSLE also has a very small negative effect on the number of younger siblings sitting the PSLE in the pre-FSE period (less than 2 percentage points, $p < 0.05$), with this effect becoming even smaller in the post-FSE period.

### A.6.2 School selection

School selection represents another potential mechanism for the effects we measure. Table A.20 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);[30] and 3) a dummy for whether the younger sibling sat the PSLE at a higher-*ex-ante*-pass-rate school than her older sibling.

Table A.20: 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 | 0.003 | -0.004* | -0.001 | -0.001 |
| | (0.007) | (0.002) | (0.006) | (0.002) | (0.001) | (0.002) |
| N | 85,760 | 82,773 | 82,709 | 525,336 | 440,076 | 437,282 |
| Mean dep. var. | 0.446 | 0.488 | 0.234 | 0.477 | 0.487 | 0.229 |
| $R^2$ | 0.304 | 0.727 | 0.338 | 0.233 | 0.683 | 0.253 |
| 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%).

[30] 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.

## A.7 Robustness

Table A.21: 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 | -0.009* | -0.012*** | 0.005 | 0.008*** | 0.004*** | 0.009*** |
| | (0.005) | (0.004) | (0.004) | (0.002) | (0.002) | (0.002) |
| Female older sibling | -0.000 | -0.001 | 0.006** | -0.002 | 0.001 | 0.004*** |
| | (0.003) | (0.002) | (0.002) | (0.002) | (0.001) | (0.001) |
| Female | -0.011*** | -0.078*** | -0.068*** | 0.010*** | -0.029*** | -0.081*** |
| | (0.003) | (0.003) | (0.003) | (0.001) | (0.002) | (0.001) |
| Age gap | -0.001 | 0.001 | 0.004 | 0.004*** | 0.003*** | 0.003*** |
| | (0.004) | (0.004) | (0.004) | (0.001) | (0.000) | (0.000) |
| N | 84,750 | 85,478 | 85,478 | 520,678 | 525,485 | 525,485 |
| Mean dep. var. | 0.239 | 0.622 | 0.160 | 0.608 | 0.754 | 0.250 |
| $R^2$ | 0.292 | 0.338 | 0.392 | 0.176 | 0.171 | 0.237 |
| 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 A.22: 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 | -0.009* | -0.013*** | 0.006 | 0.005** | 0.002 | 0.006*** |
| | (0.005) | (0.004) | (0.004) | (0.003) | (0.002) | (0.002) |
| N | 78,638 | 79,298 | 79,298 | 333,219 | 336,082 | 336,082 |
| Mean dep. var. | 0.236 | 0.619 | 0.158 | 0.597 | 0.745 | 0.239 |
| $R^2$ | 0.290 | 0.334 | 0.382 | 0.183 | 0.177 | 0.233 |
| 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

### A.7.1 Spillover effects of younger siblings

Since our analysis exploits variation in PSLE performance, we would expect the direction of any spillover effect to be mostly from older to younger siblings. We formally test for *younger-to-older* sibling effects by estimating a modified version of Equation (1) in which older and younger sibling subscripts are swapped. (Moreover, in the estimation considering pre-FSE older siblings, we add a 'Post-FSE younger sibling' indicator variable and an interaction term with the main independent variable.) In essence, we ask whether having a younger sibling pass the PSLE can predict older siblings' transition to secondary school. In line with our main analysis, we restrict the sample to (now, younger) siblings whose PSLE subject letter grades were consistent with either passing or failing that exam. To be fully consistent with our main analysis we also redo our sibling matching procedure, now imposing that each younger sibling must have a unique last name within their ward and cohort.

However, a number of factors prevent us from interpreting this analysis as a true falsification test. The sequential nature of siblings' exam-taking implies that a younger sibling's PSLE performance may itself have been affected by the 'outcome' (i.e. their older sibling's PSLE performance or enrollment in secondary school). Indeed, the effects of older siblings passing on younger siblings' *PSLE Pass* and *High score*, shown elsewhere in the paper, suggest precisely this. Furthermore, because our proxy for secondary enrollment can only be measured when a student reaches the end of 9th grade (the second year of secondary school), there is ample scope for sufficiently closely-spaced younger siblings' PSLE performance to affect older siblings' participation in the Grade 9 exam, and hence our proxy measure of secondary school enrollment.

Table A.23 displays the results of our analysis. As expected, we find a null younger-to-older sibling effect when we focus on post-FSE cohorts; we also find null effects for the subsample in which both siblings took the PSLE before the introduction of FSE. However, we obtain positive effects of younger siblings' achievement on their older counterparts when considering pre-FSE older/post-FSE younger sibling pairs. Given the potential for reverse causality and the legitimate possibility of younger-to-older sibling spillovers, we interpret these results with caution.

Table A.23: “Effect” of younger sibling passing on older sibling’s transition

| | Pre-FSE older siblings | | | Post-FSE older siblings | | |
|---|---|---|---|---|---|---|
| | (1) Transition | (2) PSLE Pass | (3) High score | (4) Transition | (5) PSLE Pass | (6) High score |
| Younger sibling passed PSLE | -0.000 | -0.003 | 0.003 | 0.009 | 0.004 | 0.005 |
| | (0.002) | (0.005) | (0.003) | (0.006) | (0.005) | (0.003) |
| Post-FSE younger sibling | -0.011*** | -0.030*** | -0.013*** | | | |
| | (0.002) | (0.003) | (0.002) | | | |
| Passed x Post-FSE younger sibling | 0.008*** | 0.014*** | 0.002 | | | |
| | (0.003) | (0.005) | (0.003) | | | |
| Passed + Passed x Post-FSE younger sibling | 0.008*** | 0.011*** | 0.005** | | | |
| | (0.002) | (0.003) | (0.002) | | | |
| N | 300,028 | 302,707 | 302,707 | 122,777 | 123,922 | 123,922 |
| Mean dep. var. | 0.182 | 0.532 | 0.109 | 0.503 | 0.673 | 0.173 |
| $R^2$ | 0.176 | 0.237 | 0.262 | 0.253 | 0.258 | 0.312 |
| Sibling’s score FE | Younger | Younger | Younger | Younger | Younger | Younger |
| School FE | Older | Older | Older | Older | Older | Older |
| Year FE | Older | Older | Older | Older | Older | Older |

**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 *older* sibling got an A or B average on the PSLE, and zero otherwise. All regressions include (*older* sibling’s) school and (*younger* sibling’s) grade permutation fixed effects. ’Year FE’ denotes binary indicators for the PSLE cohort of each *older* sibling. The sample includes all *younger* 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.7.2 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 from the year 2013 do not include middle names but only middle initials; this poor data quality is more prevalent among students in poor districts and among students with lower PSLE scores. This matching method therefore reduces the sample size dramatically and shifts its composition, potentially excluding some families where sibling spillovers are large.

Figures A.7 and A.8 show the estimated (spillover) effect of older siblings’ qualification for secondary school on younger siblings’ secondary school attendance (i.e., *Transition*), by cohort, under the school-level and middle-name matching procedures, respectively. The evolution of the coefficients over time is remarkably similar to that observed under our preferred matching algorithm (for a more detailed analysis, compare the estimates in the second and third columns of Table A.10 with those in the first column).

Meanwhile, Tables A.24 and A.25 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.

Figure A.7: Effect of older sibling's qualification for secondary school on younger sibling's secondary school attendance, by cohort (School-level Matching)

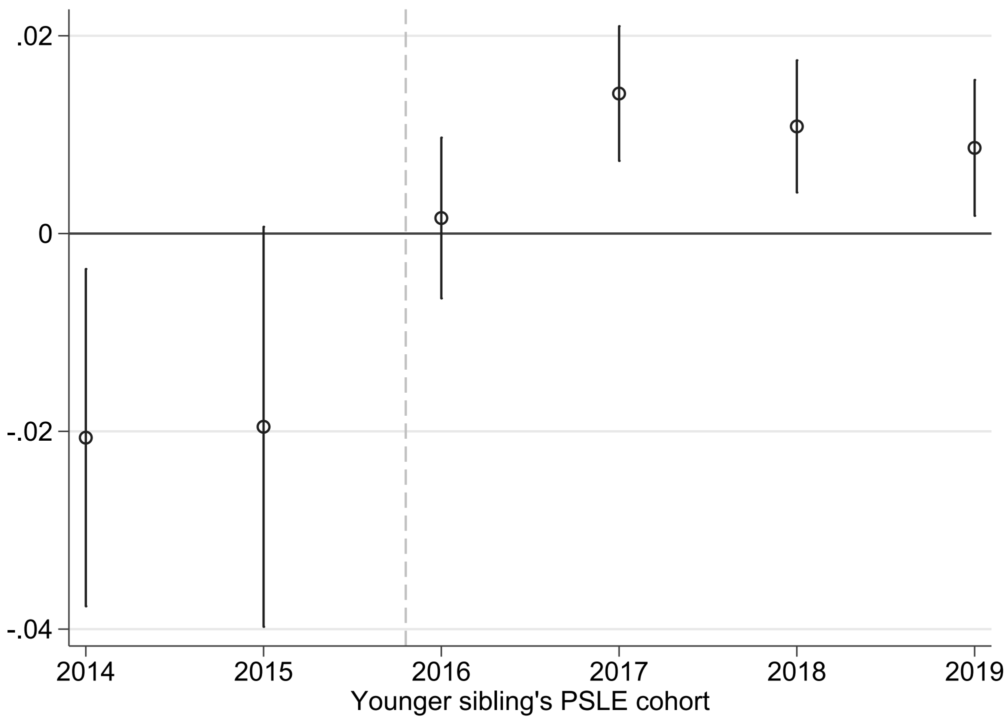


Table A.24: Spillover effects from older siblings' achievement (School-level Matching)

| | 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 | 99,549 | 101,624 | 101,624 | 567,751 | 580,725 | 580,725 |
| 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

Figure A.8: Effect of older sibling's qualification for secondary school
on younger sibling's secondary school attendance, by cohort (Middle and Last Name Matching)

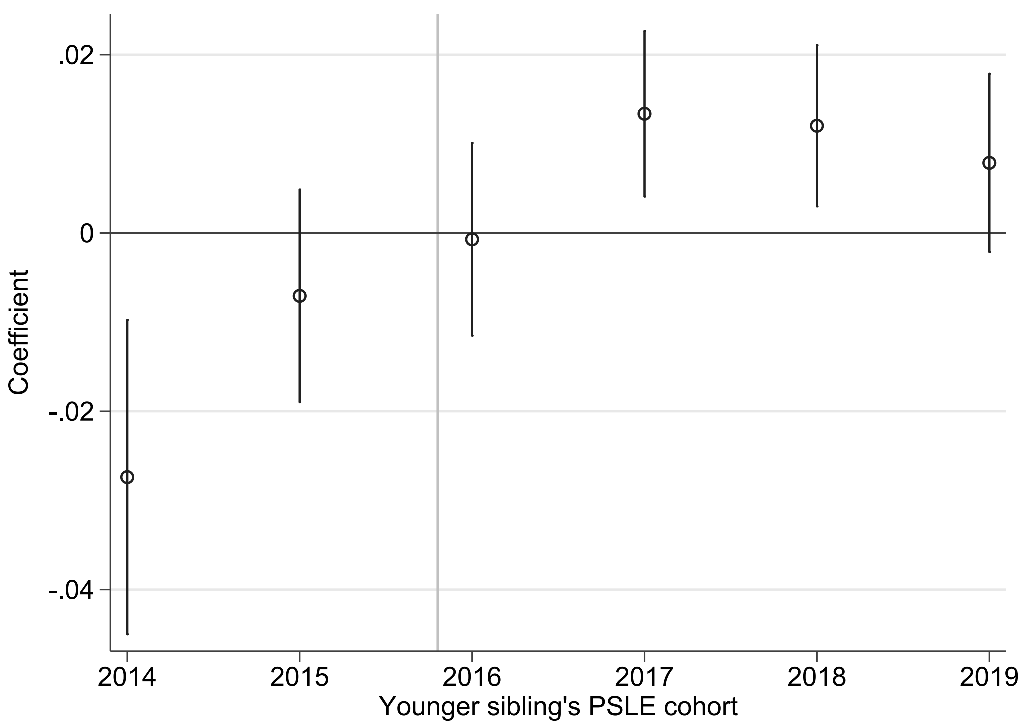


Table A.25: Spillover effects from older siblings' achievement (Middle and Last Name Matching)

| | 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 | -0.010 | -0.008 | -0.001 | 0.009** | 0.006** | 0.007*** |
| | (0.008) | (0.007) | (0.004) | (0.004) | (0.003) | (0.002) |
| N | 36,638 | 38,508 | 38,508 | 288,760 | 302,350 | 302,350 |
| Mean dep. var. | 0.283 | 0.624 | 0.145 | 0.608 | 0.745 | 0.228 |
| $R^2$ | 0.359 | 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