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Merit, Inequality, and Opportunity

THE IMPACT OF MALAWI'S SELECTIVE SECONDARY SCHOOLS

Esme Kadzamira, Symon Winiko, Tsirizani M. Kaombe, Jack Rossiter

Abstract

This paper examines the effectiveness of Malawi's selective secondary schools in influencing student learning outcomes. Using data from Malawi's National Examination Board, we employ value-added and regression discontinuity methods to gauge the impact of school types on high-stakes exam results. Findings reveal that National schools enhance student learning progress by an average of 0.57 standard deviations more than day schools, within two years. Regression discontinuity results corroborate National schools' positive impact, with National school attendance yielding a 0.40 standard deviation increase in student exam outcomes. Importantly, students from districts with relatively low-performing primary schools benefit substantially from attending National schools, especially those with low-quality secondary education alternatives. Compared to global evidence, our study highlights the importance of evaluating the broader educational context when analysing school tracking effects on student outcomes. Our findings are relevant to policy discussions around secondary school expansion, performance reporting, and student selection in Malawi.

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Merit, Inequality, and Opportunity: The Impact of Malawi's Selective Secondary Schools

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1. Introduction

Malawi's education sector faces chronic resource shortages, especially at the secondary level where public schools can only accommodate 40 percent of eligible students. The allocation of limited seats relies on a standard merit-based criterion, with students selected based on their past academic achievements. Notably, only a small fraction, approximately three to four percent, of the highest achievers secure coveted spots in the most well-resourced National schools. These institutions demonstrate consistently strong results in national examinations, capturing the attention of parents while fueling debates around regional disparities in educational opportunity (Chiuta, 2019).

However, despite the substantial resources and their perceived excellence, we currently lack empirical evidence regarding the effectiveness of Malawi's most selective secondary schools. Public secondary school admission relies on a nationally standardised selection process, where students are categorised into four-tiers of schools based solely on their primary school achievement score. This introduces a selection bias, as school performance differences may mirror student abilities and family backgrounds rather than school effectiveness. Fortunately, the rigid selection process also offers an opportunity to overcome this selection bias.

This paper offers estimates of the impact of different school types on high-stakes secondary school exam results, which are important for future university entry and labour market opportunities. Using linked administrative records of primary and secondary school exam scores from Malawi's National Examination Board (MANEB), we employ both value-added and regression discontinuity methods to generate impact estimates for schools and school types. The assignment policy for National schools—the highest school tier—also explicitly reserves seats for students from all districts, generating a wide range of test score cutoffs for entrance to the same school type. As a result, rather than being centred on a single level of ability, our regression discontinuity estimates are based on the progress of students with different primary achievement scores, many of whom will not have the lowest intake score in the school.

Our analysis reveals three key findings. First, our value-added analysis shows that National schools substantially boost student learning progress compared to day schools, even after adjusting for variations in student abilities. On average, this translates to a 0.57 standard deviation advantage in exam scores after two years of schooling, when compared to students attending day schools. Notably, National schools consistently outperform, with none falling below the national average, and 14 out of 23 significantly surpassing their counterparts. Conversely, second-tier District Boarding schools exhibit a more modest advantage of 0.13 standard deviations over day schools, with only 10 percent demonstrating superior effectiveness. We detect no differences in average value-added between third-tier District Day and fourth-tier Community Day secondary schools.

Second, our regression discontinuity results also show that access to Malawi's National schools leads to substantial learning benefits for the marginal student—that is, the student who just gains access to National schools. Attending a National school results in a 0.40 standard deviation increase in the secondary school test score of students who are just above the admissions threshold. Our regression discontinuity findings corroborate results from the value-added model, despite relying on different identifying assumptions.

Third, we use the variation in entry cutoffs across districts to show that access to a selective National school is particularly valuable for students from lower-performing primary school districts. This effect can be substantial, with a student from one of the lowest-performing primary school districts anticipating a 0.24 standard deviation greater benefit from attending a National school than a peer securing admission in an average-performance district. Access to a high-achieving peer group seems to be more beneficial to high-performing students from low-performing districts who might otherwise attend schools with comparatively disadvantaged peers. Indeed, further tests suggest that this effect may be most pronounced among female students or those from districts with limited high-quality alternatives for secondary education.

Comparing our findings to international evidence highlights the importance of accounting for variations in the quality of alternative educational options and the potential differences in the "dosage" of school quality. In numerous countries, the impact of being placed in elite schools through tracking systems has produced mixed results. Several studies report small effects (Pop-Eleches and Urquiola 2013, Deming et al. 2014) or negligible impacts on test scores and future outcomes (Abdulkadiroglu, Angrist, and Pathak 2014, Ajayi 2014, Angrist, Pathak, and Walters 2013, Clark and Royer 2013, Dobbie and Fryer 2014, de Hoop 2010, Lucas & Mbiti 2014, and Rubinstein and Sekhri 2011). In contrast, Jackson (2010) in Trinidad and Tobago, and Park et al. (2015) in China, find large positive effects of being selected into the best secondary schools on examination performance. These positive outcomes coincide with disparities in the school environments experienced by students with varying initial levels of achievement. Deming et al. (2014) offer a possible explanation for these divergent findings. They suggest that students with limited access to high-quality alternative schooling options may benefit more from attending more selective schools, as the relative improvement in the quality of education is more pronounced in such cases. This underscores the need to consider the broader educational landscape when assessing the effects of school tracking on student outcomes.

Our research also aligns with work showing that attending better-resourced schools has the greatest positive impact on students from low-income backgrounds (Smith et al. 2020) or on those with relatively low academic skills (Cohodes and Goodman 2014, Goodman et al. 2017, Zimmerman 2014). The marginal student in our study—that is to say, the student who just gains admission to a National school—experiences improved secondary school exam performance when granted the opportunity to attend a school where their academic skills are notably lower than their peers (Canaan and Mouganie 2018). This contrasts with the literature on "mismatch" (Arcidiacono and Lovenheim 2016) whereby access to higher-quality schools can harm the outcomes for students whose academic preparation is significantly below that of their classmates (Barrow et al. 2020). Arcidiacono et al. (2011) discuss how the negative effects of mismatch can depend on students' ability to identify their ranking relative to their peers, but this is not at risk in Malawi where students receive only their pass/fail status and school assignment. Our findings suggest that the overall school environment and resources play a significant role in shaping student outcomes in Malawi, regardless of each student's initial skill level.

We conduct several robustness checks, confirming the comparability of observations near the admission threshold. We observe different rates of attrition on either side of the cutoff and our continuity tests of covariates indicate that sample composition may change with treatment status. We account for sample selection differences in several bounding exercises and estimate a conservative lower bound National school effect of 0.28 standard deviations.

These findings have implications for policies regarding secondary school expansion and regarding the reporting of school performance. Malawi is one of only 26 low-income countries and can accommodate only a small fraction of primary school completers in secondary schools. The government is in the midst of a school construction programme, with support from several external donors. In this context, information on the relative performance of different school types can be helpful. Relatedly, the release of information on school exam performance has become more prominent in the past three years, with rankings of districts put into the public domains. Our results underscore the importance of distinguishing student-level factors from school-level factors when assessing school quality and performance.

Additionally, our findings are relevant to policies on how students are selected for secondary school. An emerging line of enquiry indicates that school examinations may underestimate the ability of students from resource poor areas (Sethi and Somanathan 2023). In this work, resource deficits mean that measures of past achievement are noisy signals of future potential, so that, conditional on a level of performance, disadvantaged groups may have higher expected ability. In a situation of large resource differences at primary level, information on group membership can be informative as a predictor of future success—such as via place-based affirmative action. These approaches can also help to close inequalities in educational achievement between districts.

Malawi's secondary school selection rules are, however, moving in the opposite direction. Recent changes to National Boarding school selection rules have shifted toward a "national merit list" approach and away from the use of district-based quotas which combined representation-based and merit-based goals. If seats are awarded based solely on student achievement, the most selective schools will primarily serve students from higher-performing—and more-affluent—urban centres and undo the geographical diversity of these schools that was previously achieved.

The rest of this paper proceeds as follows: section 2 provides the background to offer context for the empirical results, section 3 describes the data, section 4 details the empirical framework, section 5 presents the main value-added and regression discontinuity results, section 6 provides robustness checks and bounds main estimates, section 7 concludes.

2. Background

A chronic shortage of secondary schools

Malawi faces a persistent shortage of schools (Mtika and Gates, 2011), particularly at higher levels where, in 2023, students from 6,954 primary schools are eligible for selection into just 943 public secondary schools (Table 1; Ministry of Education, 2023).[1] The scale of the capacity gap becomes clear when considering that out of 267,330 candidates who sat for the PSLCE in 2023, 234,644 passed and were eligible for secondary school, but only 41 percent of those (96,101 students)[2] secured spots in public secondary schools. Private schools accepted only another 9 percent (23,667 students; Ministry of Education 2023).

There is a complicated nomenclature of secondary school types in Malawi. Seven categories of schools exist, namely: Community Day Secondary Schools (CDSS), District Day, District Boarding, National Boarding, Grant Aided, Open Day, and Private schools. For many purposes, and the approach we use in this paper, National Boarding and Grant Aided schools are rolled together into a single National Boarding school type. Open Day and Private schools are not the subject of this paper because regular secondary school selection processes do not admit students to either type, with Open Day tending to be used for night schools for mature students and Private schools following an entirely separate admissions process.

Notably, about half (45 percent) of the 1,774 secondary schools are CDSS, enrolling 46 percent of the student population in 2023. District Day, District Boarding, and National Boarding schools, collectively referred to as "Conventional" schools, accounted for a further 16 percent of the student population in 2023. Private schools and Open Day schools account for the remainder of the enrolment, but these do not receive candidates through the centralised selection process.

The allocation of resources across different secondary school types in Malawi has raised concerns about inequity. Instead of evenly distributing limited resources, the government allocates a significant share to a select few schools, especially those with boarding facilities (Zubairi & Rose, 2019; de Hoop, 2010). In contrast, CDSS often lack essential teaching and learning resources, leading to criticism in public and policy discourse (Kafumbu, 2020).

1 A further 419 Open Day Secondary Schools, formerly night schools, are not included in the main secondary selection process. Open schools use the infrastructure of regular public schools and enrol students of any age that pass the Primary School Leaving Certificate of Education (PSLCE).

2 https://bnn.network/breaking-news/education/education-experts-express-concerns-over-exam-results-in-malawi/

TABLE 1. Distribution of secondary schools and enrolment by type of school in 2023[3]

	Schools		Students	
Secondary School Type	**Number**	**Proportion (%)**	**Number (1000s)**	**Proportion (%)**
Public regular schools				
Community Day	805	45	223	46
District Day	61	3	34	7
District Boarding	45	3	24	5
National Boarding	32	2	19	4
Other schools				
Open Day	419	24	92	19
Private	412	23	102	21
Total	1,774	100	486	100

Source: MoE, Education Statistics Report 2023. Notes: Open Day Secondary Schools (formerly Night Schools) use infrastructure of other schools. We choose to present data on schools from Figure 54 (p.54), which differs slightly from other school counts reported in the same document. Enrolment counts are based on values in Figure 61 (p.64). Rounding may mean that sum does not correspond to total reported.

Government embracing school competition and league tables

Malawi's education authorities are increasingly publicising primary and secondary school performance tables for national examinations. In 2023, a typical press release for primary and secondary examinations includes full district performance rankings, lists the top and bottom ten performing schools and is communicated as a press release and via social media channels (Ministry of Education, 2023).[4] National secondary schools consistently achieve strong results in these examinations, and rankings, and are favoured by parents and students.

However, these rankings fail to account for initial student achievement, making it challenging to separate the contribution of schools to student learning from pre-existing differences in learning across schools at the beginning of the schooling phase (Leckie & Prior, 2022). Only 5 percent of the highest-achieving candidates in Malawi's Primary School Leaving Certificate of Education (PSLCE) gain access to these school types, determined based on performance in a nationally standardised selection process. As a result, little is known about the actual effectiveness of various secondary school types in enhancing learner performance, a matter which we can overcome in this paper.

A new selection rule for national schools

The significance of National schools and their perceived benefits has prompted public debate and reforms in how students are assigned to these institutions in Malawi. In the early 2000s, a quota system was used to allocate students to National secondary schools (de Hoop, 2010).

3 Our analysis is based on the cohort of students selected for public secondary schools in 2012 and expected to complete secondary school in 2016. The equivalent number of each school type in our data is: Community Day: 552, District Day: 45, District Boarding: 34, National Boarding: 23.

4 For example, the Malawi National Examinations Board Facebook page: https://www.facebook.com/photo.php?fbid=406697444971641&id=100068941655280&set=a.228561596118561

This quota ensured representation of male and female candidates from each district where candidates took the PSLCE. While this system aimed to ensure fairness, it led to supposedly less academically meritorious candidates gaining admission to prestigious National schools, as achievement levels varied significantly across districts.

By 2012, a new quota rule based on a student's 'district of origin' was implemented to maintain representation of all ethnic groups yet recognize internal educational migration. However, this change unintentionally overrepresented students from Malawi's Central region in National schools and underrepresented those from the Northern districts.[5] In 2018, these Northern districts, accounting for 19 percent of grade eight enrolments, received only 12 percent of national school placements. In this paper, we are concerned with cohorts of pupils who entered secondary school under this rule.

Public dissatisfaction culminated in 2019 protests, prompting the Ministry of Education to revise the selection process once again.[6] The current method allocates National school seats based on a national merit list, ranking all PSLCE candidates by sex and filling available seats accordingly. While this system has altered the regional makeup of selected students, it has raised concerns about increasing inequality between urban and rural students. Therefore, understanding how different school types affect student achievement is essential to assess the impact of these policy changes.

This paper utilises linked examination records to estimate the effect of attending different secondary schools and school types. These efforts to estimate school quality confront the challenge of selection bias, where school-to-school comparisons may reflect differences in student ability and family background more than differences in school effectiveness. Our analysis draws on education administrative data encompassing both primary and secondary school learners facing national examinations at the end of Grade 8 and two years later, at the end of Grade 10.

3. Data

Malawi's systems of national examinations and secondary school selection provide a natural source of data for assessing school effectiveness. We combine three administrative data sources: (1) 2012 Primary School Leaving Certificate of Education (PSLCE) results, (2) 2012 secondary school selection lists, and (3) 2014 Junior Certificate of Education (JCE) results. Access to this data was secured through a close partnership between the Centre for Educational Research and Training (CERT) at the University of Malawi and the Malawi National Examinations Board (MANEB).

In our analysis, we utilise aggregate scores from the PSLCE and JCE, which are standardised. The PSLCE is written by learners who are completing their final grade at primary school. Six papers

5 By 2018, 50 (47) percent of male (female) candidates were selected from the Central region yet that region accounted for only 38 percent of grade eight enrolment (for both male and female students).

6 The revised system was used in the second selection in 2019, but not in the main round of selection until 2020.

are administered and these are: English, Chichewa, Mathematics, Social and Religious Studies, Primary Sciences, Arts and Life Skills. Each student receives a score for each subject. An aggregate score, constructed as the sum of the top five subject scores, is used to assign students to secondary schools.[7] We use this same aggregate to create the intake scores for our value-added analysis and the running variable for our regression discontinuity approach. The JCE is written by students who are completing lower-secondary school. At this level, there is a wider choice of subjects and not all are universally available. We aggregate the JCE subject scores in Mathematics and English only to create a comparable measure of student achievement across schools.

There may be reason to be sceptical of the use of high-stakes examination data, especially when these data are used to certify student proficiency, or in monitoring system performance (Rossiter et al., 2023). For our application, however, we do not use the exam results to measure student or system performance in absolute terms. What we need is a primary school assessment that returns a fine-grained measure of relevant skills, so that students can be placed in a suitable rank order for selection (and as an input to our analysis); and we need the two examinations to measure similar cognitive skills, which we would expect to detect through primary school results having some predictive power for secondary school results. While we cannot test the performance of individual items in any subject test, Figure 1 and Table 2 summarise scores for the PSLCE and JCE overall, showing neither floor nor ceiling effects, and in Table A3 we report bivariate correlations between subject scores in each examination. We show correlation between PSLCE subject scores of 0.51 to 0.83. The lowest correlations are between Chichewa language and all other subjects which is unsurprising given Chichewa is not a first language for all candidates. For JCE subjects we show equivalent bivariate correlation coefficients of 0.69 to 0.84. The correlation between PSLCE and JCE scores for matched individuals is 0.72, suggesting that there is substantial information in the PSLCE result that predicts later achievement.

Since there is no unique identifier across these datasets, we establish links by first connecting PSLCE candidates to their assigned secondary schools. Subsequently, we employ approximate string matching techniques to compare each PSLCE candidate's name with every name within their selected school.[8] For example, for a candidate with the name "NAMWALI MEMORY WYSON," assigned to Lilongwe Secondary school, we calculate a distance metric between this name and every name at Lilongwe Secondary.[9] In our application, a higher distance value indicates less similarity between names.

7 This was true for the cohort in our study but has since changed to the sum of the top five scores *including English*.

8 Only selected MANEB staff are allowed to handle data containing student names. This exercise required CERT and MANEB to work together to establish matches, before the removal of personal identifying information. Datasets used for analysis are anonymised and contain no personal information.

9 We use package "stringdist", in R, to complete the approximate string matching, https://cran.r-project.org/web/packages/stringdist/stringdist.pdf

We select the most likely match (nearest distance) for each name pair while excluding matches above a specific threshold. This threshold, determined through collaboration between researchers at the Malawi National Examinations Board and the University of Malawi, minimises false positive matches.[10] We only permit matches on names within assigned secondary schools. Although this approach may result in missing some genuine matches (false negatives), where candidates enrol in schools different from their assignments, it significantly reduces false positives.[11]

Out of 216,912 PSLCE candidates, 49,113 were selected into public secondary schools. Among these selected candidates, we successfully match 26,496 records (see Table 2) who complied with their assignments. Matching rates vary by school type, with higher rates in national (75 percent), district boarding (74 percent), and district day (65 percent) schools compared to CDSS (49 percent) (refer to Table A2 for a breakdown by sex).

The main reason that students are absent from our dataset is the incredibly low internal efficiency of Malawi's school system. By this we mean that students may not have enrolled at their assigned school, perhaps on the basis that they could not pay the fees; or that they have dropped out or repeated a grade during the two-year period between exams. Using data from Malawi's Integrated Household Survey (IHS), we estimate that only 6 in 10 students admitted to secondary Form 1 would have progressed on time for the following two years and should be expected to appear in the JCE records. Three quarters of the students who didn't make smooth progress were still in school, just having repeated a grade or more, while about a quarter had dropped out altogether.
If we take this on face value, then it explains around 90 percent of the attrition between our selected and matched samples. In Section 6 we discuss how these rates of attrition vary by school type and provide more details on how they have evolved over time. Other unmatched students, who account for the remaining 10 percent of the attrition that we see, may be absent from our dataset for various reasons, including substantial name changes or transfers to unassigned secondary schools, making matching unfeasible.

10 We could relax or tighten the cutoff for name matches and test how that affected our results, if we wanted.

11 Unconstrained (by assigned school) matches between PSLCE and JCE records would lead to a huge number of false positives, due to the number of records and similarity in names and name spellings.

TABLE 2. Summary statistics of examination and selection records

	Obs.	Mean	Standard Deviation	Min	Max
Panel A: All PSLCE candidates					
PSLCE raw score	216,912	179.6	58.4	0	427
PSCLE standardised score	216,912	0.0	1.0	−3.1	4.2
Female	216,912	0.46	0.50	0	1
Age	216,351	15.8	1.8	7.5	45.6
Primary school size	4,011	54.1	45.9	1	667
Primary school quality	4,011	−0.1	0.6	−1.6	2.8
Panel B: PSLCE candidates selected to public secondary school					
PSLCE raw score	49,113	241.1	50.5	113	427
PSCLE standardised score	49,113	1.1	0.9	−1.1	4.2
Female	49,113	0.48	0.50	0	1
Age	48,980	15.2	1.7	7.7	41.5
Primary school size	3,133	58.2	49.5	3	667
Primary school quality	3,133	−0.0	0.6	−1.6	2.8
Panel C: PSLCE candidates matched across PSLCE and JCE exams					
PSLCE raw score	26,496	244.9	52.2	113	427
PSCLE standardised score	26,496	1.1	0.9	−1.1	4.2
Female	26,496	0.47	0.50	0	1
Age	26,431	15.2	1.7	9.6	26.3
Primary school size	2,887	60.3	50.6	3	667
Primary school quality	2,887	−0.0	0.6	−1.5	2.8
Panel D: JCE candidates in public schools					
JCE raw score	53,617	61.2	25.1	6	169
JCE standardised score	53,617	0.0	1.0	−2.2	4.3
Female	53,617	0.47	0.5	0	1
Secondary school size	652	82.2	47.9	15	363
Secondary school quality	652	−0.1	0.6	−1.2	2.5

Notes: School quality is the average exam score for each school. We include 'internal' candidates sitting the JCE in public schools and omit 'external' and 'open distance learning' candidates. We retain only the secondary schools into which students were selected in 2012, therefore omitting prison centres, army centres, and any new public school, established in the two-year period between exams. This also omits non-national schools located in the Central East Education Division which were omitted from our 2012 selection dataset.

4. Empirical strategy

Our analysis employs two distinct approaches: first, we establish value-added measures for each school. This approach allows us to estimate impacts for the average student admitted to each school, and allows us to investigate variation in school effects within the same school type.

Second, we leverage the fact that secondary school placements depend on observed PSLCE test scores, creating a discontinuity in access to each school type, and compare students who gained access to a particular school type with those who narrowly missed out. The regression discontinuity method is perhaps more restrictive, in that it estimates the difference in the National school impacts for so-called marginal students who performed exactly at the entry cutoff and only just gain admission to National schools. However, the method provides stronger claims to causality and where modest school expansion is being pursued, as is the case in Malawi, estimates of the benefit for the *average* student and for the *marginal* student are both policy relevant measures. By using two methods we can provide a more complete picture of differences in effectiveness across Malawi's secondary schools and school types.

Value-added method

Across the globe, school systems increasingly employ school value-added models (VAMs) and standardised student tests to monitor and hold schools accountable for their performance (Koedel et al. 2015; OECD, 2008). These models aim to gauge the impact of schools on student learning, i.e., the value they contribute.

VAMs are panel-data models estimated using student-level data with repeated test performance measures (Castellano & Ho, 2013). They separate schools' unique contributions to student learning from other sources of achievement. With a handful of exceptions, VAMs are estimated using observational data. In sub-Saharan Africa, however, data constraints have limited empirical studies on school and teacher effectiveness (Oketch, Rolleston, and Rossiter, 2021).

Research in recent years has established that well-designed VAMs provide unbiased estimates on average. While the use of VAMs requires careful consideration of their underlying assumptions, a consensus has emerged that they offer reliable insights into school and teacher effectiveness in the USA (Bacher-Hicks & Koedel, 2022). Value-added models have also provided useful insights into school performance in sub-Saharan Africa and South Asia (Oketch et al., 2021; Singh, 2019), but it is rare, on account of access restrictions, to see this applied to examination data. Crawfurd and Elks (2018) use examination data from Uganda to test several value-added models using these data. They find that the model is robust to a variety of different specifications and control variables and that it has the potential to provide a clearer signal to parents, teachers, schools, and policymakers about how much learning is happening in different schools.

To recover causal estimates of school effects, VAMs require independence between school value added and student outcomes, either unconditionally or conditionally (Meyer and Dokumaci, 2010). Unconditional independence is plausible in cases where student-school assignments are random, while conditional independence applies to typical scenarios where VAMs are estimated using observational data.

The most common approach in VAMs is to fit a linear regression model of student current achievement on student prior achievement, where school effects are represented by school means of the predicted residuals (Goldstein, 1997). Some VAMs incorporate low-order polynomials or bands for prior achievement to capture nonlinear relationships between current and prior achievement (Leckie & Prior, 2022). While prior attainment is a key predictor of current achievement, demographic and socioeconomic characteristics also play a role (Leckie & Goldstein, 2019, Steiner et al., 2010).

For simplicity and effective communication with policymakers, we employ a conventional linear regression model, accounting for student intake differences to the extent possible given our data (Burgess & Thomson, 2023). Our approach does not estimate the effects of specific school and teacher characteristics but highlights the relative benefit of a student belonging to a particular group, such as a school, in comparison to others (Glas et al. 2006; Oketch et al. 2021).

Our basic VAM model can be represented as follows:

$$Y_{ijt} = \alpha + \beta Y_{ijt-2} + \gamma X_{ijt} + \varepsilon_{ijt} \tag{1}$$

where, Y_{ijt} is the test score for student i in school j at JCE exam time t. Y_{ijt-2} denotes the PSLCE test score from two years earlier, for the same individual, whose effect on current score is β. The prior (lagged) test score Y_{ijt-2} may also be understood to absorb the effects of prior educational inputs including home inputs and the quality of previous education. X_{ijt} is a vector of student characteristics including sex and age, with corresponding vector-valued effects γ on X_{ijt}. Finally, ε_{ijt} represents the within-school random error term associated with the measurement of the i-th student's score in the j-th school.

Following estimation, each student's value-added score is calculated as the difference between their realised JCE score and the JCE score predicted by model (1). To prevent distortion of a school's overall performance, we cap the value-added scores of students whose scores are more than two standard deviations below the mean (Leckie & Prior, 2022). Approximately 1.5 percent of student scores are adjusted in this manner. Subsequently, school value-added scores are computed as school averages of student scores.

Regression discontinuity method

To complement our value-added estimates, we leverage the fact that students do not have a choice in selecting their secondary schools; instead, these placements are determined through a centralised allocation process. This process assigns students to different types of schools based on a single measure of their academic achievement, which means that students with very similar scores may end up in schools of different types.

The Regression Discontinuity (RD) design, a well-established non-experimental research approach, comprises three key components: a score, a cutoff, and a discontinuous treatment assignment rule. In our study, (i) all units receive a score, which, in our case, is their aggregate PSLCE score; (ii) the 'treatment' (in our case, assignment to a National school) is only assigned to units whose scores exceed a specified cutoff; and (iii) this abrupt shift in the likelihood of receiving treatment at the cutoff allows us to examine the causal effects of the treatment on subsequent exam performance.

We make an important assumption that student characteristics, apart from their test scores, do not undergo abrupt changes at the cutoff, a hypothesis we rigorously test in Section 6. The RD method allows us to use candidates with scores just below the cutoff, attending non-National schools, as a comparison group for those with scores just above it, attending National schools (Calonico et al. 2014).

The allocation rule for secondary school places in Malawi

Students do not choose which public secondary school they attend. Instead, a centralised school allocation process assigns them to one of 652 schools, categorised into four school types, across 34 educational districts.[12] This allocation process involves the Ministry of Education, Education Divisions, and the Malawi National Examinations Board (MANEB) and unfolds in a specific sequence, starting with National schools and progressing to Community Day Secondary Schools.

The allocation rule hinges on a single measure of student achievement at the end of primary school. Each student's aggregate score is calculated as the sum of their best five subject scores in the primary school leaving exam.[13] Students are ordered according to this score, which becomes the running variable for our analysis. Students are selected first for National schools, then District Boarding schools, before being selected to District Day schools and Community Day Schools.

National Boarding Schools: When our cohort was selected to secondary school, each district received a quota of spaces for male (N_m) and female (N_f) students based on district population.[14] The number of spaces allocated ranged from 5 in the island-based Likoma district (population 14,527 in 2018 census) to 43 in Mangochi district (1,148,611 in 2018 census). In each district the top performing N_m and N_f students are selected to fill these spaces. In the event of ties, students are selected in ascending order of pupil number.

District Boarding Schools: Each district has a fixed number of boarding school spaces for male (DB_m) and female (DB_f) students who attend primary schools within the same district. Students already assigned to National schools are removed from the list and the top performing DB_m and DB_f students are selected to fill these spaces.

District Day Schools and Community Day Secondary Schools: These schools admit students, who commute daily, from designated feeder primary schools within the same district. Students not

12 The number of public secondary schools involved in the centralised assignment process has risen to 943 in 2023.

13 It has since changed to the sum of the top five scores *including English*.

14 The quota-based allocation system was removed in 2019, since when students have been admitted to National schools based on their rank-order in a national merit list, without regard to their district of origin or the district in which they sat the PSLCE.

already selected, from relevant feeder schools, are ranked and assigned to day schools until all spaces are filled. Then the same process continues for community day schools.

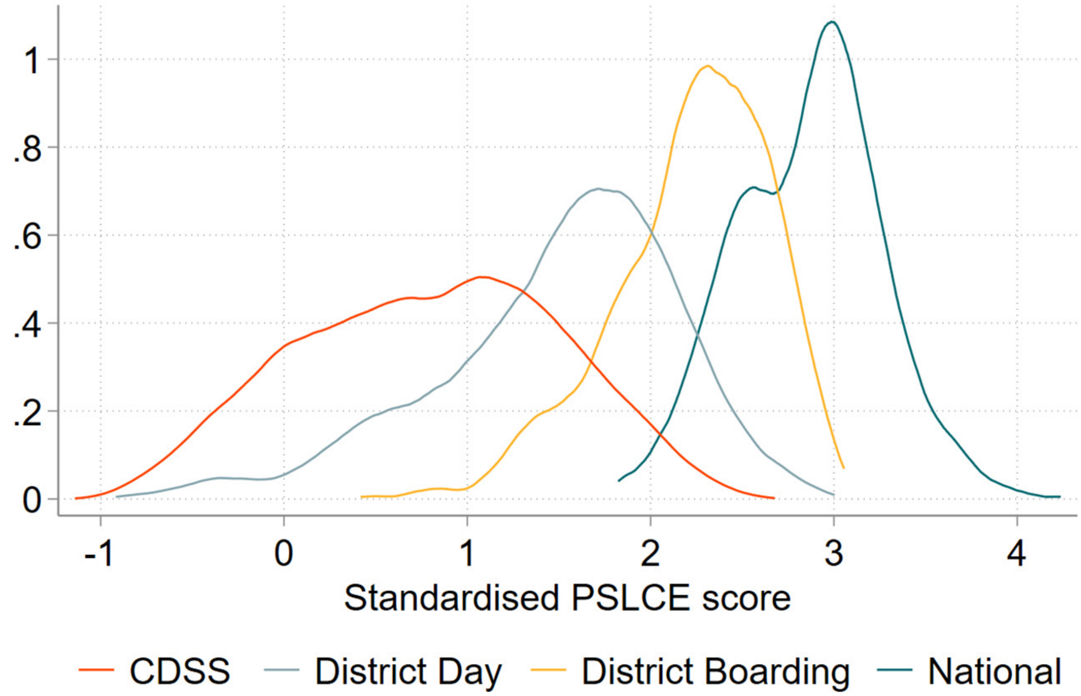
Consequently, there is no universal cutoff score (in PSLCE points) for admission to National schools or other school types. For each district, the number of available places in each school type, combined with student performance on the PSLCE, implicitly determines cutoff points for entry into each school type. And this can vary by sex. This results in a wide distribution of abilities among children just admitted to different school types (Appendix Figure A1).

For instance, consider two equally sized districts, A and B, each with 20 spaces for National schools and 100 spaces each for district boarding and district day schools. Assume that District A performs well overall. District B performs less well. As allocations occur within each district, the level of primary school achievement required for selection is higher in District A than District B, for every school type. Some pupils assigned to District Boarding schools in District A may have higher PSLCE performance than pupils assigned to National schools from District B. These variations in achievement among assigned students are further influenced by factors like proximity constraints for attending day schools, sex-specific assignment to boarding schools, and the absence of boarding schools in some districts. When aggregated over 34 districts, these factors contribute to the broad range of intake performance shown in Figure 1.

We normalise our running variable by subtracting the relevant district cutoff from each candidate score and then pool across all district cutoffs. However, as detailed, Malawi employs a fixed-allocation assignment process, which fills seats until they are exhausted. Consequently, each cutoff is defined by the value of the running variable for the marginal subject exposed to treatment in that district. This leads to one observation precisely at each threshold (Fort et al. 2022; Lucas & Mbiti, 2014), potentially causing a discontinuity in the running variable at the cutoff. Fort et al. (2022) demonstrate that under this assignment rule, the estimand of the standard normalised and pooled estimator may not align with any meaningful causal parameter, even if the identification assumptions of the sharp regression discontinuity design hold. One way to mitigate this is to introduce an asymmetry between the last subject who is exposed to treatment (the marginally treated) and the first subject who is not (the marginally non-treated). In each site, the relevant cutoff for the treated (non-treated) then corresponds to the score of the marginally non-treated (treated), eliminating the presence of a unit at any cutoff point, and candidates scores are normalised accordingly.[15]

15 We choose this approach, as opposed to introducing Fort et al.'s (2022) preferred method of adding site fixed effects, to allow our later heterogeneity analysis across districts.

FIGURE 1. Intake ability varies, and overlaps, substantially across school types



Notes: kernel density plots for standardised PSLCE scores. Sample: all individuals selected into secondary school.

Regression discontinuity strategy

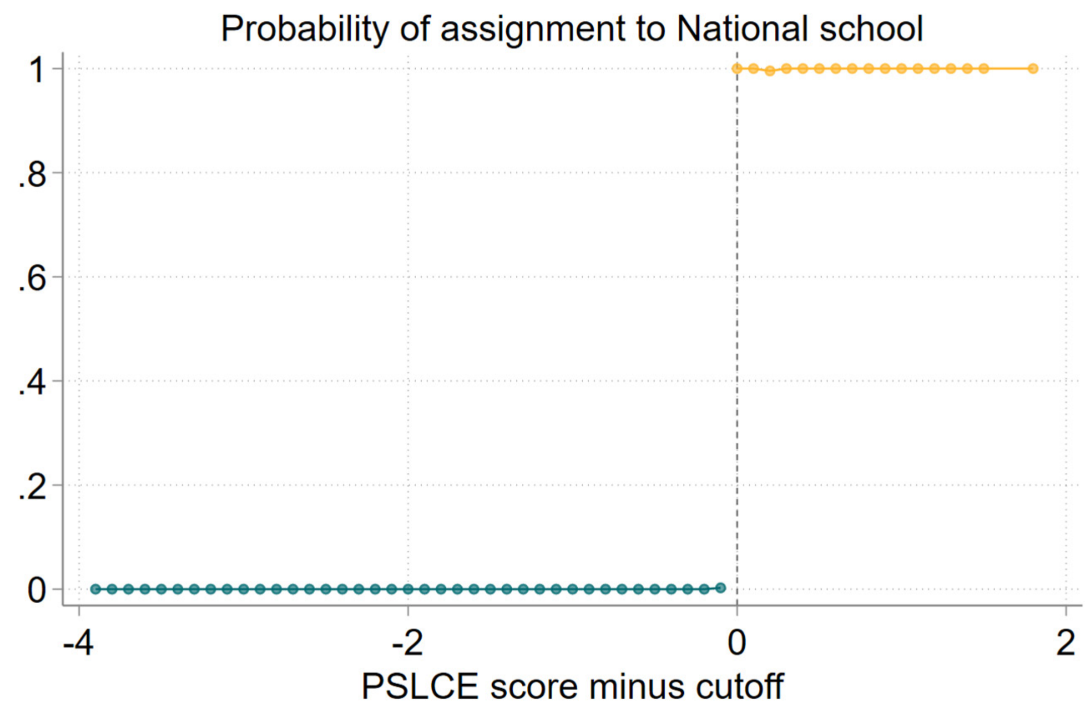
Our objective is to evaluate the impact of National schools on student secondary school exam performance. However, students in various school types differ in their PSLCE scores, reflecting both observable and unobservable differences. Our value-added approach aimed to address these differences by considering intake ability. The RD design, on the other hand, compares treated students just above the National school cutoff to control students just below it. The rationale behind the RD comparison is that students in a narrow range around the cutoff are comparable, with similar observed and unobserved characteristics, except for treatment status.

While students from all districts can attend National boarding schools, not all school types are available in all districts. In our data, 26 out of 34 districts have district boarding schools, and 25 have district day schools. For instance, Lilongwe City has no district boarding school, and Likoma district has no district day school.

The RD analysis uses the matched sample and by construction that will be composed of students who were assigned to a secondary school and are attending these schools (the compliers).[16] We estimate a sharp RD model and we investigate the impact of our matching process on results in Section 6.

Figure 2 shows the association between PSLCE score and treatment assignment and demonstrates that the probability of attending boarding school rises sharply at a given cutoff. In our case, there is a sharp discontinuity at 0, i.e. the normalised cutoff point for boarding school assignment. This discontinuity motivates our interest in whether the opportunity to attend boarding school impacts later learning outcomes.

FIGURE 2. Probability of treatment assignment jumps sharply for National schools



Notes: student scores are centred around the cutoff for admission to National school. Data are shown in equal width (0.1 point) bins. There are very slight deviations from perfect probability of treatment assignment (0 or 1).

In our setup, students ($i = 1, 2, \ldots, n$) have PSLCE test scores (X_i) and receive treatment (D_i) when their score exceeds the cutoff (c) and do not receive treatment otherwise (i.e. $D_i = 1$ if $X_i \geq c$ and $D_i = 0$ if $X_i < c$). Each student has two potential outcomes in their JCE exam: $Y_i(1)$ under treatment and $Y_i(0)$

16 We only permit matches on names within assigned secondary schools and so our matched sample is a set of compilers, by construction. We argue in Section 3 that the majority of unmatched individuals are likely to be repeaters or dropouts. Nevertheless, by constraining matches to assigned schools, we will miss some genuine matches of students who attend a school other than the one to which they were assigned. However, the benefit is that we significantly reduce false positives—an unconstrained match on names between PSLCE and JCE records would lead to a huge number of false positives.

without treatment. Only one potential outcome is observed, represented as $Yi = DiYi(1) + (1 - Di)Yi(0)$ for the i-th student (Cattaneo and Titiunik, 2022).

Hahn et al. (2001) introduced the continuity framework for RD designs, considering potential outcomes as random variables, assuming continuous distribution of the score Xi. For average treatment effects, the key identifying assumptions are that (i) the regression functions $E[Yi(0)|Xi = x]$ and $E[Yi(1)|Xi = x]$ are continuous in x at c, and (ii) the score density near the cutoff is positive. Then any difference between the average outcomes of treated and control units at the cutoff can be attributed to the treatment and interpreted as the causal average effect of the treatment at the cutoff, that is, for units with score variable $Xi = c$ (Cattaneo and Titiunik, 2022). In this framework, the RD treatment effect is defined as:

$$\beta_{RD} = E[Yi(1) - Yi(0)|Xi = c] = lim\, x \downarrow c\, E[Yi|Xi = x] - lim\, x \uparrow c\, E[Yi|Xi = x] \quad (2)$$

To estimate the jump size of the discontinuity in Yi at c, we fit the following regression model to the data (Huntington-Klein, 2021):

$$Y_i = \alpha + \beta_{RD} D_i + \delta(X_i - c) + \gamma(X_i - c) \times D_i + \varepsilon_i \quad (3)$$

Here, we are interested in estimating the treatment effect of attending boarding school, β_{RD}. The variables are defined as previously, and εi represents the unknown random error due to individual factors affecting JCE performance, with a mean of zero.

Equation (3) can be estimated using linear, local polynomial, or penalised regression methods around the cutoff. This yields the Local Average Treatment Effect (LATE) of National school attendance on JCE performance for students near the cutoff. Importantly, this parameter is policy-relevant for a marginal increase in available National school seats (Kline and Walters, 2016). However, RD designs have low external validity, making it challenging to infer treatment effects away from the cutoff.

In our context, cutoffs are defined within district and sex, allowing us to make more general statements about the effects of different school types across a wider range of student intake abilities. We pool data and estimate the LATE as a pooled average across district-specific cutoffs, which vary widely in Xi scores. This lends support to more general statements about the effects of different school types for a wider range of student intake abilities.

5. Results

Value-added results

Our value-added results are straightforward and are shown in Table 3. First, the basic conditional model indicates the importance of prior score for later achievement, supporting the argument that prior achievement should be accounted for if we are to understand school effectiveness as something different from school selectivity. It also shows that being a female is associated with slightly less progress, on average, and that's also true for older students.[17]

TABLE 3. Results from conditional linear value-added model

	JCE Score
PSLCE score	0.768***
	(0.021)
Female	−0.0705***
	(0.020)
Age	−0.0895***
	(0.005)
Constant	0.807***
	(0.088)
Observations	25,831
Adjusted R-squared	0.543

Notes: Standard errors in parentheses, clustered at school-level. * p < 0.10, ** p < 0.05, *** p < 0.01. To enter the VAM a student's PSLCE and JCE score is required. Appendix Figure A5 plots the average PSLCE intake score for each secondary school against the average intake score for the VAM sample in each school—i.e. the matched sample. There are no systematic differences by prior achievement or by school type.

When evaluated at the school level, National schools substantially outperform other school types in terms of how much value they add to student learning in the two years between the PSLCE and the JCE. Results in Table 4 show that compared with Community Day Secondary School (CDSS) and adjusting for the other factors in the model, a candidate placed at National Secondary School can expect to see 0.57 SD more progress. There is a more muted but still positive effect of being placed at a District Boarding school, worth around 0.13 SD over the same two-year period. There is little evidence that district day schools and community day secondary schools differ in terms of the average value that they add to student learning.

School-level value-added varies substantially across and within school categories. We can look at this by grouping schools into performance bands, from "well above average" to "well below average". By doing so, we can see that the variation in National school value-addition is fairly narrow, with all schools average or better and most well above average. District boarding schools, on the other hand, are more hit and miss with almost as many below average as above average.

Despite the low (relative) value that they add overall, there are still high performing district day and community day secondary schools. In fact, there are as many CDSS that perform well above average

17 We also test a two-subject (Mathematics & English) aggregate as our prior achievement measure, and our results do not change. If anything, National schools do very slightly better on this measure. The correlation between student two-subject and five-subject aggregate scores is 0.92. We must use the five-subject aggregate in our regression discontinuity analysis, as it is the basis of our running variable, so for consistency we use that in our VAM too.

(21 of them) as there are schools from all other categories combined (20)—in part because there are just so many schools in this category.

This perspective on school performance can be invaluable. By accounting for differences in intake, policymakers could begin to think about how to target support to low value-addition schools or areas, or seek to understand how practices differ between high and low value-add schools within the same category or area.

TABLE 4. The effect of different school types on secondary school exam performance, value-added estimates

Value added	National	District Boarding	District Day	CDSS	Overall
Average	**0.54**	0.10	–0.02	–0.03	0.00
Standard deviation	**0.23**	0.35	0.30	0.34	0.35
Minimum	**0.02**	–0.60	–0.99	–1.04	–1.04
Maximum	**0.90**	1.17	0.74	2.26	2.26
School bandings					**Share of Schools**
Well above average	14	3	3	21	6%
Above average	7	8	8	84	16%
Average	2	16	20	308	53%
Below average	0	5	13	97	18%
Well below average	0	2	1	42	7%
Total schools	23	34	45	552	100%

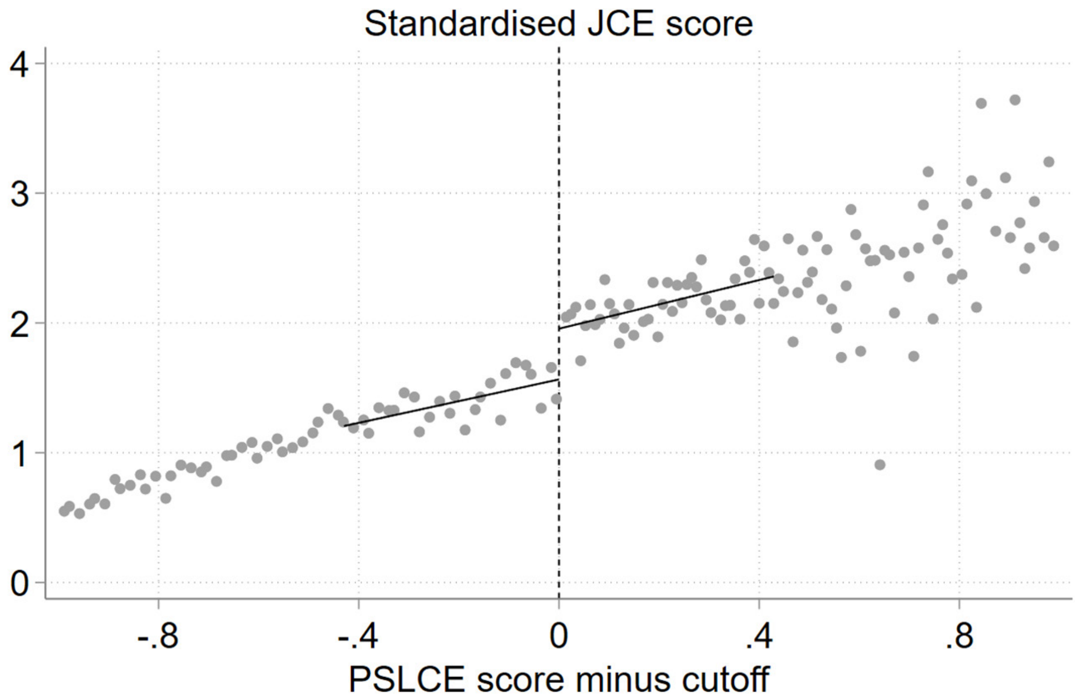
Notes: regression outputs underpinning each school value-added estimate are shown in the Appendix. Average (mean) value-added is weighted by the number of pupils studying in each school. Results do not change if weights are removed. School bandings are defined as follows: Well above average requires VAM >= 0.5 & 95% lower confidence interval > 0. Above average requires > 0.0 VAM < 0.5 & 95% lower confidence interval > 0. Average is any school that is not statistically significantly different from zero. Below average requires < 0.0 VAM > –0.5 & 95% upper confidence interval < 0. Well below average requires VAM <= –0.5 & 95% lower confidence interval > 0.

Regression discontinuity results

RD model estimates

The plots on binned means in Figure 3 confirm that there appears to be a discontinuity in later JCE performance at the cutoff for admission to a National school, for both females and males.

FIGURE 3. Discontinuity in JCE score at the cutoff for admission to national schools



Notes: We centre each candidate's PSLCE score around the cutoff that they face for entry to National school and plot student scores in mimicking variance evenly-spaced bins, using spacings estimators (following Calonico et al., 2014). The marginal student admitted to a National school takes a centred score of 0. Solid lines are fitted values of regressions of the dependent variable on a linear trend in the transition score, estimated separately on each side of the cutoff.

Given the sharp discontinuity, we first fit a linear OLS model to the data, to estimate the JCE score jump at the cutoff for boarding schools (Gelman and Imbens, 2019). This parametric approach uses all observations and suggests that boarding school attendance increases JCE performance by 0.60 standard deviations of the JCE test score (Table 5). When we restrict our estimates to observations within a fixed bandwidth, equal to half a standard deviation of the running variable, either side of the cutoff. After retaining only observations within this window, our local-linear estimate of the impact of boarding school falls to 0.39 standard deviations. We then add a bit more flexibility by including a second-order polynomial, which has little effect on our treatment effect estimate. Finally we use a data-driven approach to select a mean squared error (MSE)-optimal bandwidth following Calonico et al. 2014, and return a National school effect of 0.40 standard deviations.[18]

18 Including a second-order polynomial reduces this only slightly, to 0.30 (and increases the bandwidth).

TABLE 5. The effect of boarding school and national school attendance on secondary school exam performance, regression discontinuity estimates

	(1) Linear	(2) Linear with Bandwidth	(3) Quadratic with Bandwidth	(4) Linear w/ CCT Optimal Bandwidth
Centred PSLCE	0.747***	0.839***	1.208*	
	(0.029)	(0.179)	(0.599)	
Attend National (Treatment)	**0.605*** **	**0.385*** **	**0.399*** **	**0.403*** **
	(0.049)	**(0.086)**	**(0.103)**	**(0.113)**
Attend National X Centred PSLCE	0.059	0.125	−0.906	
	(0.100)	(0.223)	(1.075)	
Centred PSLCE squared			0.799	
			(1.195)	
Attend National X Centred PSLCE squared			0.811	
			(1.641)	
Constant	1.371***	1.566***	1.596***	
	(0.051)	(0.067)	(0.073)	
Observations	25,895	2,541	2,541	2,541
Adjusted R-squared	0.493	0.160	0.159	
Window (equal either side)	–	0.43	0.43	0.43

Notes: Column 1 includes all observations and fits a linear function to the running variable. Column 2 limits observations to a window equal to half a standard deviation of the running variable and fits a linear model within this. Column 3 extends this by including a second order polynomial of the running variable. Column 4 uses a mean squared error (MSE)-optimal bandwidth and retains observations within that window, which enter with triangular kernel weights (following Calonico et al., 2014 (CCT) using the rdrobust package). It is a coincidence that the MSE-optimal bandwidth in Column 4 matches the author-selected bandwidth in Columns 2 and 3. Columns 1, 2 and 3: cluster-robust standard errors in parentheses. Column (5): cluster-robust and bias-corrected standard errors in parentheses. * p < 0.10, ** p < 0.05, *** p < 0.01.

Heterogeneous effects on outcomes

We now explore whether impacts on secondary school achievement scores are different across candidates. Recall that our context is one in which the cutoff for entry to National schools varies across districts, and can also vary by student sex. For example, the average cutoff for entry to National schools is a standardised PSLCE score of 2.5, but can be as low as 1.8 and as high as 3.0 (Appendix Figure A2). As a result, marginal students from different districts—i.e. the individuals who just gain admission to National schools—enter secondary schools with a wide range of primary school achievement scores.

Consequently, students join their new classes with different levels of preparation, they will rank at different positions relative to their new peers, and they will experience a set of peers that is more (or less) like the individuals that they have completed primary school with. All of these factors can affect students' learning experiences and progress. In addition, for any district there is an observable difference between the quality of the average National school and the quality of the alternative school option in that district. The size of this difference varies across districts, and may influence the impacts on secondary school achievement scores for students from different districts too.

In our main analysis we pool all cutoffs across districts; here, we carry over the original linear specification with restricted bandwidth (Table 5, Column 2) and also include three interactions which may influence impact estimates: (i) the PSLCE score cutoff that each student faces, (ii) student sex, and (iii) the outside schooling option that each student faces. In Table 6 we report the results for each.

Column 1 of Table 6 shows a negative and statistically significant relationship between National school attendance and National school cutoff. The lower the cutoff the higher is the National school effect. Rather than looking at the most extreme cases, we can split districts into three groups based on their National school cutoff (call them low-cutoff, medium-cutoff, high-cutoff). These can be thought of as proxies for student primary achievement in each district. Candidates from the low-cutoff group have on average a 0.70 SD lower cutoff score than those from the medium-cutoff group. This translates into a 0.24 SD larger impact of attending a National school for candidates from the low-cutoff group.[19]

19 i.e. the lower cutoff score (–0.70) multiplied by our coefficient on cut faced (–0.341) = 0.24.

TABLE 6. Heterogeneous effects by student prior achievement and sex

	(1) Standardised JCE Score	(2) Standardised JCE Score	(3) Standardised JCE Score
Centred PSLCE	0.848***	0.817***	0.854***
	(0.162)	(0.165)	(0.171)
Attend National (Treatment)	1.306***	0.337***	0.387***
	(0.325)	(0.083)	(0.097)
National X Centred PSLCE	0.217	0.213	0.0858
	(0.217)	(0.219)	(0.240)
National X Cut Faced	**−0.341*** **		
	(0.118)		
Cut Faced	0.995***		
	(0.087)		
National X Female		**0.185*** **	
		(0.058)	
Female		−0.580***	
		(0.048)	
National X Outside option			**−0.514**
			(0.462)
Outside option			1.421***
			(0.403)
Constant	−1.043***	1.789***	1.668***
	(0.220)	(0.067)	(0.070)
Observations	2,541	2,541	2,423
Adjusted R-squared	0.251	0.235	0.185
Window (equal either side)	0.43	0.43	0.43

Notes: for consistency, we carry over the specification from Table 5, Column 2, retaining the same window and number of observations around the cutoff. In Column 1 we add an interaction between National school attendance and the PSLCE score cutoff for entry to National school in each district. In Column 2 we add an interaction between National school attendance and an indicator variable which takes the value of 1 when the student is a female. In Column 3 we add an interaction between National school attendance and a measure of the outside option in each district, which we construct by bringing over our school value-added estimates and taking the average value-added of secondary schools, excluding National schools, in each district. Cluster-robust and bias-corrected standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Some of this additional impact of National schools among low-cutoff districts may be explained by sex. Female candidates face lower cutoffs overall (Appendix Figure A2), and seem to gain more from attending National schools too. In Column 2 we add a sex interaction to our model and show that effect sizes are indeed 0.185 SDs higher for female students.

Finally, we look at heterogeneity by alternative school option. Our hypothesis is that, for each student, the impact on their secondary achievement score will depend on the “dosage” of the treatment, which we can think of as the difference in quality between the National school and the outside schooling option in their district. This “dosage” depends on the quality of non-National secondary schools and is not necessarily reflected by the average quality of primary schools in the district. We bring over school value-added estimates from our earlier analysis and construct a measure of the outside option for each student, as the average value-added of secondary schools, excluding National schools, in their district. We find a large and negative effect of the outside option interaction, which we interpret as a National school effect that's higher where the quality of the outside option is lower. Although consistent with our "dosage" hypothesis, the point estimate is insignificant. The bottom line seems to be that being able to attend a selective National school is more valuable to a student from a district of low-performing primary schools—and particularly to female students—than it is to a student from a district with high-performing primary schools.

6. Robustness checks

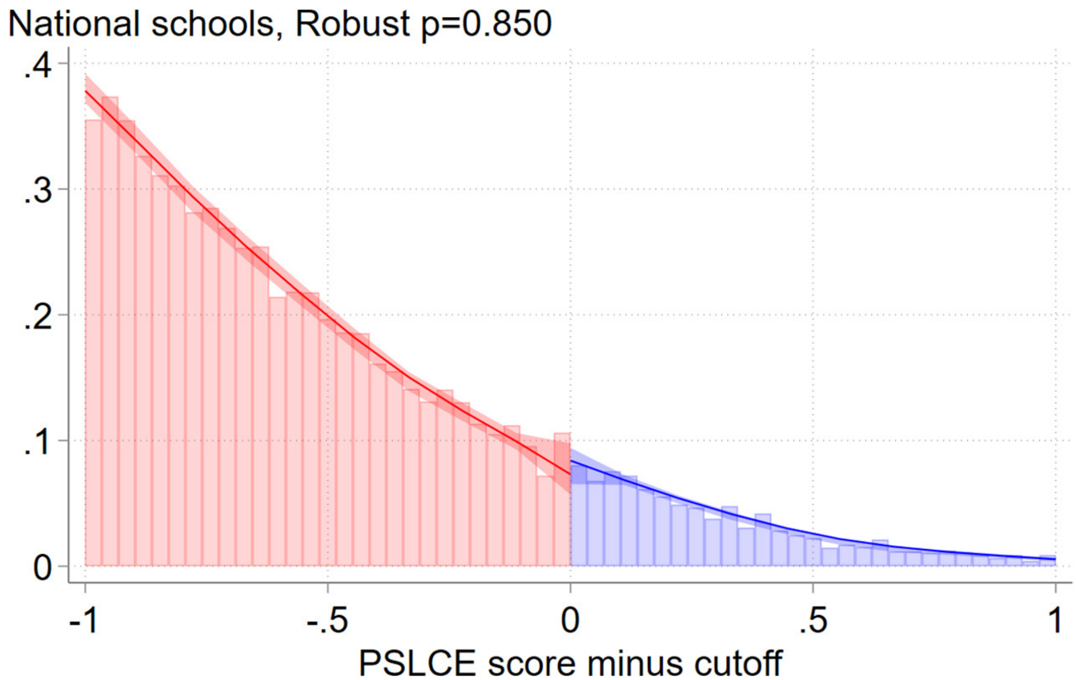
Manipulation of the assignment variable

The most important threat to any regression discontinuity design is the possibility that units are able to strategically and precisely change their score to be assigned to their preferred treatment condition (Lee, 2008; McCrary, 2008). These behaviours might induce a discontinuous change in observable and/or unobservable characteristics at or near the cutoff and confound causal conclusions.

Following McCrary (2008), we check for smoothness in the density of observations, violation of which could suggest that students can at least partially control which side of the threshold they fall on. The density of observations in our sample is nearly identical just below and just at the threshold. This suggests students can not manipulate their PSLCE scores (Figure 4).

This is unsurprising for several reasons. First, the assignment variable is an aggregate score across five, non-trivial, subject tests. Second, the aggregate score cutoffs required for entering each school type are not known ex-ante and change by district, sex and year. If entry thresholds were persistent across years, it would be feasible for parents, schools or students to find out the final thresholds. But in our setting, these groups cannot predict the scores required for entry to any school type, and so cannot use that information to manipulate their PSLCE score to secure an admission offer. For an accurate guess of boarding school or national school thresholds, families would need a formidable amount of additional information. Third, students never receive information about their raw PSLCE scores, they are only informed of their Pass/Fail status. This reduces the possibility that, for instance, more motivated marginal students appeal their marks in a bid to just cross the cutoff for National school entry.

FIGURE 4. McCrary density test



Covariate continuity

Next we test for balance in observed covariates across the threshold for the entire sample as well as for our matched sample. As a consequence of the assumption of local random assignment around the cutoff, it is standard practice to test whether individuals on either side of the cutoff are well-matched in their observed baseline covariates (Lee & Lemieux, 2008). In the usual implementation, failure to reject the null hypothesis of covariate balance is interpreted as evidence of comparability of control and treatment groups near the cutoff. In this section we test two predetermined covariates.

Given the nature of our administrative data, we have limited demographic information on students to test for additional discontinuities at the threshold. However, we do have information on their age at the time of sitting the PSLCE, as well as the average performance of peers in their primary school, which could be thought of as an approximation of socioeconomic status since richer students are more likely to attend higher-performing primary schools.[20]

20 We also have data on student sex, but this is not a predetermined covariate. The proportion of spaces available to female students in National schools (49 percent) is higher than those available to females in lower-tier schools (35 percent in District Boarding schools, 49 percent across day schools). As such, treatment will directly influence the proportion of female students on either side of the cutoff.

In Table 7 we reject the presence of a statistically significant discontinuity in age or primary school quality. Our empirical test retains the main treatment specification, including the windows either side of the cutoff used in estimating that effect, and replacing the test score outcome with each pre-treatment covariate in turn. For each, we first present results for the sample of students selected into secondary schools and then for the sample that we match between exam records. We plot continuities for visual inspection in Appendix Figure A3.

TABLE 7. Continuity tests for age and primary school quality, for students selected into secondary schools and for the matched sample, by cutoff

	Age		Primary School Quality	
	(3)	(4)	(5)	(6)
	Selected	Matched	Selected	Matched
Estimated discontinuity	**0.054**	**−0.194**	**0.027**	**0.077**
	(0.169)	(0.171)	(0.077)	(0.075)
Observations	3,785	2,537	3,795	2,541
Window (equal either side)	0.43	0.43	0.43	0.43

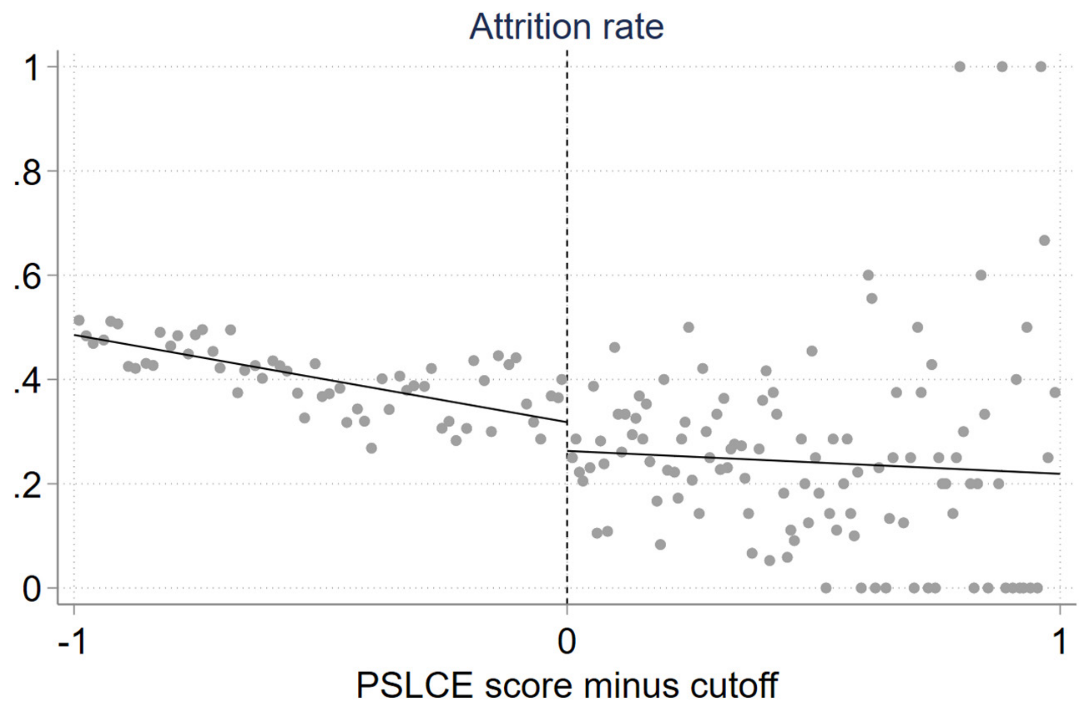
Notes: All columns use the mean squared error (MSE)-optimal bandwidth carried over from the main treatment estimates in Table 5. Per the main specification, observations enter with triangular kernel weights (following Calonico et al., 2014, using the rdrobust package). Cluster-robust bias-corrected standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Attrition and sample selection

In this section, we address attrition and sample selection in our study, focusing on the factors that may influence the probability of attrition among students in different school types. Attrition is relatively high in our study, with linked records for 54 percent of all individuals selected to secondary schools. Attrition rates also vary by school type, with just under 50 percent of candidates matched in CDSS, but 74 and 75 percent matched in National schools and District Boarding schools, respectively (Table A2). Persistently low rates of internal efficiency are the main reason for high attrition in our sample.

Attrition in our study is not evenly distributed across treatment and control groups (Figure 5). Notably, we observe a lower rate of attrition among National school students compared to students in other schools, and this is consistent with higher rates of repetition and dropout in lower-quality schools. To understand this phenomenon, we explore potential explanations for attrition overall and why that might differ between school types.

FIGURE 5. Discontinuity plot for attrition rate (i.e. unmatched students from selection list)



Notes: We use the same setup as in the main specification, substituting our dependent variable for an indicator of whether or not the selected candidate left our sample. The running variable is calculated as each student's standardised PSLCE score minus the cutoff for the last student not-admitted to National schools from their district. Solid lines are fitted values of regressions of the dependent variable on a linear trend in the transition score, estimated separately on each side of the cutoff.

We use data from Malawi's Integrated Household Survey (IHS) to estimate dropout and repetition rates in secondary school grades for students who would have been in school around the time of our 2012 primary school completing cohort. From round 3 data, which was fielded between 2010 and 2011, just before our cohort entered secondary school, we estimate that only 62 percent of students admitted to secondary Form 1 would have progressed on time for the following two years. Of those that didn't make it, 27 percent of students were still in school but had repeated a grade one or more times and 11 percent had dropped out. Six years later, in IHS-4 and just after our cohort would have passed through lower-secondary school, rates of on-time progression were slightly lower at 57 percent. Both repetition and dropout had risen slightly, to 30 percent and 14 percent, respectively, with the highest rates of repetition in Form 2, likely reflecting student choices to repeat and improve their JCE examination scores. With such low rates of on-time progression, our 54 percent record match rate represents something like a 91 percent match among all those selected to secondary schools who should also exist in JCE records two years later.[21] Unfortunately we are not able to look

21 E.g. we take an average of IHS records, i.e. (62 + 57) / 2 = 59.5 percent, as the largest share that we expect to link between PSLCE and JCE records. We link 54 percent of records, which represents a 91 percent match rate.

at internal efficiency by school type in these household survey data but we discuss potential reasons for differential attrition next.

Students attending higher quality and resource-rich schools might have a higher likelihood of progressing to the end of secondary school on time. This would result in a greater number of them being included in our records, thereby influencing differential attrition rates. Students selected for National schools may also have a higher propensity to attend their assigned schools in the first place. This could be attributed to factors such as the prestige associated with the school, increased opportunities, and the convenience of not having to commute daily.

Conversely, students without a National school assignment may be more likely to opt out of public schooling altogether. This could be due to the absence of appealing alternatives, reducing the number of candidates in the control group just below the cutoff. Another possibility is that students who just miss out on National school choose to resit the PSLCE in the following year. However, students are never told their score, only their fail/pass status. So although students may perceive that they were close to the line, it is not plausible that they know that they only just missed out and made their choice based on that.

Although we have demonstrated continuity of covariates earlier in this section, high attrition raises important questions regarding the internal validity of our comparisons between treatment and control groups. In the next section we attempt to account for sample selection in putting bounds on our treatment effect estimates.

Bounding treatment effect estimates

Identification in the standard RD design relies on comparability of observations right above and right below the RD threshold (Hahn, Todd, and van der Klaauw, 2001). Differential sample selection or missing outcomes near the threshold may undermine such comparability (Dong, 2017). We observe different rates of attrition on either side of the cutoff and our continuity tests of covariates indicate that sample composition may change with treatment status.

In this section we test the robustness of our main results by putting a lower bound on our treatment effect estimate. There is no standard approach, that we are aware of, to estimating treatment effects in RD designs with differential sample selection. We provide four estimates, based on methods in Lee (2009), but more frequently used in randomised experiments, and Dong (2017) specifically for RD designs.

Our first approach follows the Lee (2009) for non-random sample selection. Lee's bounds estimator rests on very few assumptions (Tauchmann, 2014). The main requirements are that treatment is randomly assigned and that it affects attrition in only one direction. That is, either being assigned to the treatment makes you less likely to attrit or more likely to attrit but not with different impacts on different individuals. This is plausible in our scenario, in that treatment (assignment to a National school) induces individuals to participate rather than to quit (conversely, assignment to a non-national school induces individuals to quit).

Under these assumptions, it is possible to obtain an estimate of the average treatment effect for "never attriters", i.e. the stable group of always participants. We do this by trimming observations from the group that is more frequently observed so that the share of students with an observed outcome is equal for both groups. In our case, this means excluding the largest outcome values in the treatment group from the analysis.

We carry over the bandwidth from the main specification and given participation rates of 64 percent in the window below the cutoff and 75 percent above it, we trim the highest scoring 14.7 percent of outcomes from the treatment group.[22] After doing so we re-run the RDD, with highest scorers excluded and return a lower bound treatment effect estimate of 0.273 standard deviations of JCE score (Table 8, Column 1), compared with 0.407 from our main specification.

Selecting the highest JCE performers within the window around the cutoff can bias upward the estimated outcome as we approach the cutoff from the right-hand-side. As we show in Table 5, there is a positive relationship between distance to cutoff and JCE outcome and so selecting the highest JCE performers within the window will tend to select individuals further from the cutoff. In a regression discontinuity setup, this can change the slope of the line in the right-hand-side window as it approaches the cutoff and, if anything, will tend to attenuate the difference between the main estimate and its lower bound. Therefore, in a more-conservative variation of our approach we first break the right-hand-side window into ten equally sized groups before taking an equal share of top performers from each. This adjustment is likely to limit any flattening effect we would previously create and Column 2 of Table 8 shows that the lower bound falls slightly, to 0.231 using this approach.

In a third approach based on Lee (2009), we first remove the association between the running variable and the outcome, before estimating bounds as if this were a regular randomised experiment. We regress the outcome (JCE score) on prior attainment before predicting residuals. We then use these residuals as the outcome and follow the same approach as above to estimate bounds on our treatment effect. By this method we return a lower bound of 0.282 standard deviations of JCE score.

In a final approach we follow Dong (2017) in estimating treatment effects in regression discontinuity designs with sample selection. Dong (2017) extends the standard regression discontinuity design to allow for differential sample selection or missing outcomes above or below the cutoff (see Annex for description of how this is applied). The approach provides nonparametric identification of the extensive margin—the treatment effect on sample selection probability—and the intensive

22 i.e. (75-64)/75 = 14.7 percent.

margin—the treatment effect on the observed outcome, conditional on being selected into the sample, at the cutoff. Neither exclusion restriction nor bounded support of the outcome is required for these bounds. We can use this approach to construct sharp bounds on the treatment effect among the group of always participating compliers, a measure of the causal effect of the treatment that is not due to changes in participation (as in Lee 2009). We estimate a lower bound of 0.280 standard deviations of the JCE score.

TABLE 8. Estimates of treatment effect bounds

	Lee (2009) as RDD (1)	Lee (2009) as RDD, Binned (2)	Lee (2009) as RCT (3)	Dong (2017) as RDD (4)
Main treatment effect		0.407***		
(Table 5, Column 4)		(0.111)		
Lower bound	0.273***	0.231**	0.282***	0.280*
	(0.100)	(0.099)	(0.055)	(0.149)
Upper bound	0.699***	0.606***	0.664***	0.485***
	(0.089)	(0.092)	(0.061)	(0.090)

Notes: Columns 1 and 2 exclude observations from the treatment group so that sample selection rates are equalised on either side of the cutoff (Appendix Figure A4 shows the RD plot for each Column). Lower and upper bound estimates carry over all features of the main specification in Column 4 of Table 5. In Column 3 the outcome is the residuals of a regression of the dependent variable on prior achievement, before estimating bounds as if this were a regular randomised trial. Column 4 uses methods in Dong (2017) to estimate intensive and extensive margins and then bounds on the main effect. All estimates are for the group of "always-takers" at the cutoff. Columns 1 and 2: cluster-robust bias-corrected standard errors in parentheses. Column 3: cluster-robust standard errors in parentheses Column 4: bootstrapped standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

7. Conclusion

Despite the varied interventions and their perceived excellence to improve education access, we face a paucity of evidence regarding the effectiveness of selective secondary schools across low- and middle-income countries and the roles they may play in reducing or reinforcing educational inequality. In this study, we show that access to Malawi's National schools leads to substantial learning benefits, of around 0.40 standard deviations of the secondary school examination score, for students who get the chance to attend. Our value-added findings show that National schools are substantially more effective than schools in any other category, whether they are day or boarding options. Our heterogeneity analysis indicates that the largest benefits accrue to students from districts with low-performing primary schools.

The overall school environment and superior resources available at National schools does appear to play a significant role in shaping student outcomes in Malawi. In contrast with findings from other contexts, including Kenya where Lucas and Mbiti (2014) find little evidence of positive impacts on learning outcomes for students who attended the most selective schools, the consistently high performance of Malawi's National schools seems to be about more than just selection of the most able students. One way of understanding the larger benefits for students overall, and particularly for those from lower-performing primary school districts, is to think of it in terms of a National school treatment "dosage". The outside option in Malawi is to attend schools with significantly less-able peers and in very low resource environments. In contrast, it is possible that the difference between Kenya's National schools, which charge 500 USD per year and the second-tier Extra County schools, which charge 400–500 USD per year, is less severe (Bonds, 2023).[23]

Our findings face a few limitations. In particular, we do not link every student between examination datasets. This is a direct consequence of the administrative data at our disposal which, by design, do not include individuals who dropout of schools. It is also a product of low rates of internal efficiency leading to large shares of students repeating one or more times; and some limits to our ability to match individuals who have entered schools to which they weren't assigned. This can bias our estimates (upwards) if the higher rates of attrition in non-National schools reduce the quality of the comparison group that we observe. Despite this causing non-trivial attrition rates in the sample around the cutoff, we use novel techniques to bound our regression discontinuity estimates, returning still large and statistically significant National school treatment effects of close to 0.30 standard deviations. Malawi's new system of assigning students a unique identification number and having that follow them across exam rounds, should allow future analysis of this type to proceed with complete confidence in student-to-school linkages (even if it will not overcome the issue of dropouts not appearing in administrative datasets).

Other important limitations include that our main outcome is narrowly focused on school examination performance. We can say nothing of the impacts on other outcomes in the shorter- or longer-terms, including employment, earnings, health outcomes and so on. A researcher interested in extending this work could—through collaboration with the examination board—seek to make these connections to adult outcomes, as the individuals in our study sample will now be around 26 years of age.

Implications for policymakers include the importance of considering students' intake ability when evaluating school effectiveness and reporting to the public on relative successes and failures. Adapting our value-added approach could be a place to start in supplementing the existing judgments made based on raw levels of achievement. Our findings are also relevant to policies on how students are selected for existing secondary school places, and whether this should incorporate place-based considerations. More generally, better information on quality variations by school type, and on how this varies by location, can be a helpful input to planning the rollout of secondary school expansion projects.

23 Note also, in Kenya students indicate a list of secondary schools they would prefer to attend but there is no process of school choice in Malawi. We do not have evidence of how students make their selections in Kenya, but Ghana adopts the same approach as Kenya and Ajayi et al. (forthcoming) show that students from low-performing primary schools are more likely to apply to weaker secondary schools than equally qualified students from high-performing primary schools. This sub-optimal school choice outcome cannot occur in Malawi, where students are assigned to schools based on PSLCE score alone, and which may provide greater opportunities for highly talented children from poorer districts to excel in the most selective National schools.

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Appendix 1

TABLE A1. Number of secondary schools and spaces in 2012 selection

School Type	Female			Male			Total	
	Schools	Spaces	Spaces (%)	Schools	Spaces	Spaces (%)	Schools	Spaces
CDSS	551	18,179	49	551	19,289	51	552	37,468
District Day	45	3,355	50	43	3,347	50	45	6,702
District Boarding	32	1,188	35	32	2,175	65	34	3,363
National Boarding	13	780	49	13	800	51	23	1,580
Total	641	23,502	48	639	25,611	52	654	49,113

TABLE A2. The matched sample in our analysis

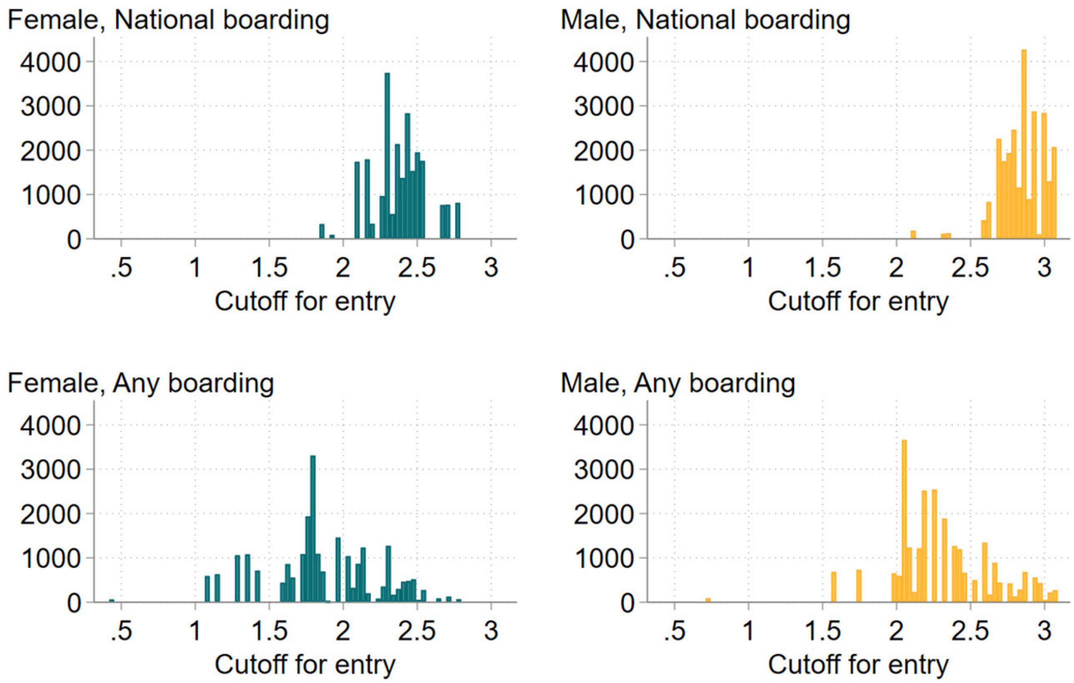
School Type	Female			Male			Total		
	Match	Attrit	Match (%)	Match	Attrit	Match (%)	Match	Attrit	Match (%)
CDSS	8,933	9,246	49.1	9,577	9,712	49.7	18,510	18,958	49.4
District Day	2,061	1,294	61.4	2,266	1,081	67.7	4,327	2,375	64.6
District Boarding	870	318	73.2	1,602	573	73.7	2,472	891	73.5
National Boarding	591	189	75.8	596	204	74.5	1,187	393	75.1
Total	12,455	11,047	53.0	14,041	11,570	54.8	26,496	22,617	54.0

TABLE A3. Bivariate correlation coefficients for PSLCE and JCE tests

A: PSLCE Subjects	Eng	Mat	Lif	Sci	Soc	Chi
English	1.00					
Mathematics	0.65	1.00				
Life Skills	0.77	0.67	1.00			
Sciences	0.72	0.69	0.82	1.00		
Social Sciences	0.74	0.67	0.83	0.80	1.00	
Chichewa	0.55	0.51	0.61	0.56	0.54	1.00
B: JCE subjects	**Eng**	**Mat**	**Bio**	**Phy**	**Agr**	
English	1.00					
Mathematics	0.69	1.00				
Biology	0.78	0.74	1.00			
Physics	0.74	0.79	0.84	1.00		
Agriculture	0.76	0.69	0.82	0.78	1.00	
C: PSLCE to JCE	**PSLCE score**		**JCE score**			
PSLCE score	1.00					
JCE score	0.72		1.00			

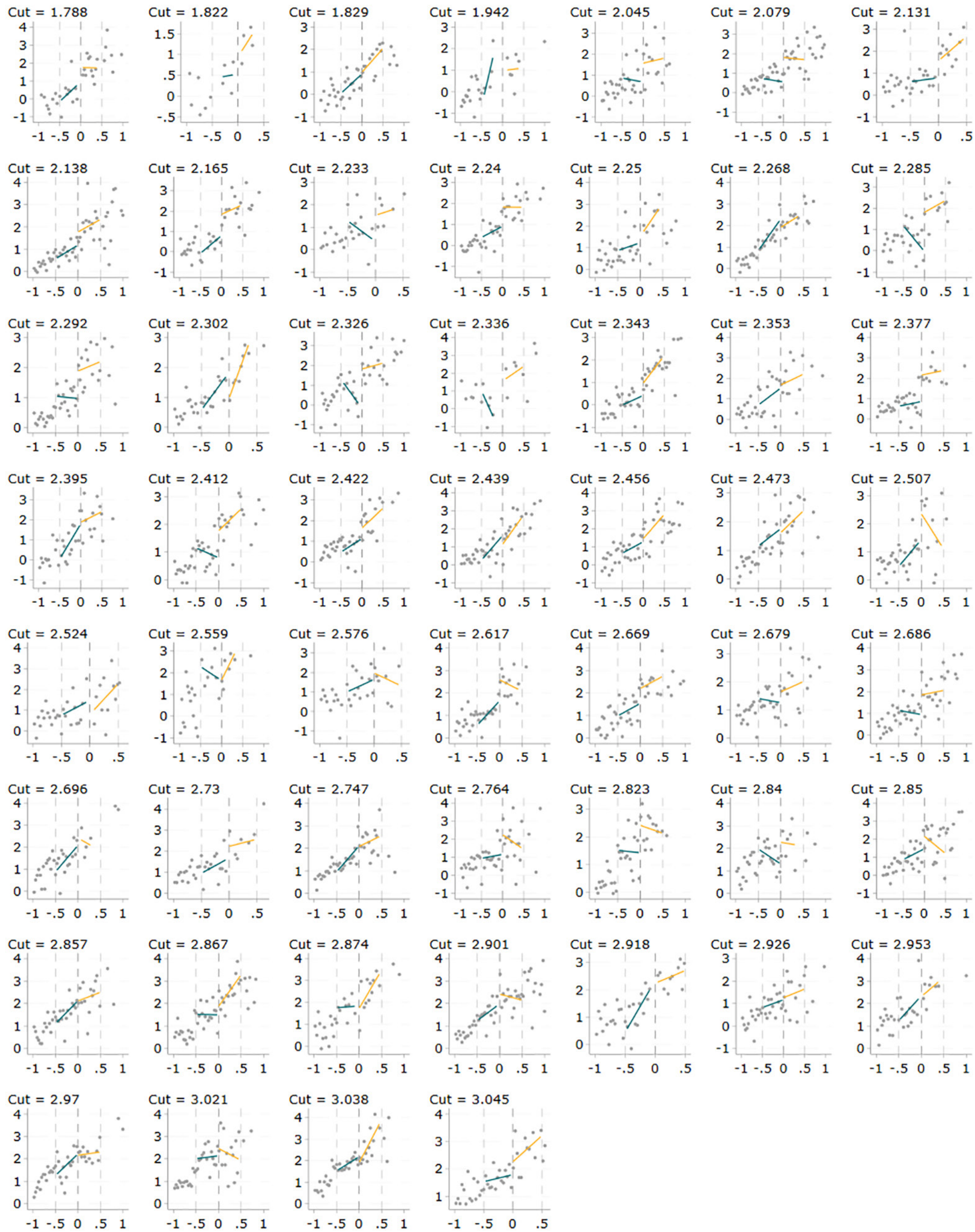
Notes: this table reports correlation coefficients between student exam scores for individual subjects, for all candidates at PSLCE (Panel A) and for all candidates at JCE (Panel B). It also reports the correlation coefficient between the standardised PSLCE and JCE scores we use in our analysis, for matched individuals (Panel C).

FIGURE A1. Distribution of cutoffs that students face for entry to National boarding schools, by sex



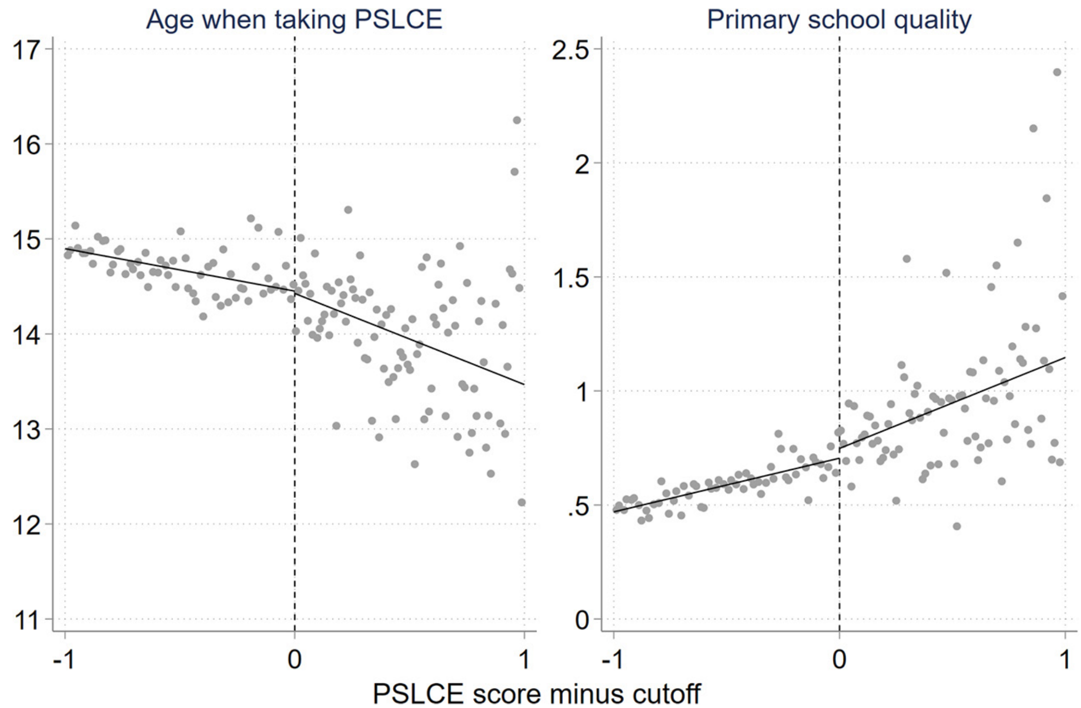
Note: Figure shows the number of students facing each cutoff and the value of that cutoff in standardised PSLCE score points.

FIGURE A2. Site-specific regression discontinuity plots for National schools



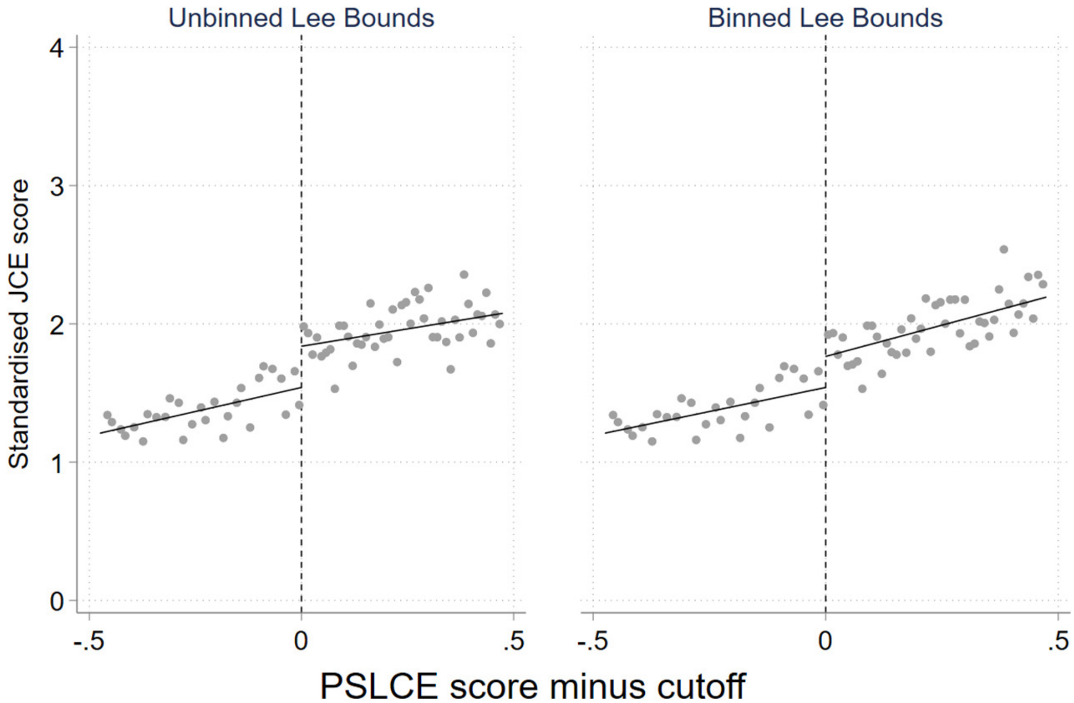
Notes: 34 districts produce 68 district-by-sex cutoffs. These collapse to 54 unique PSLCE score cutoffs. One cutoff has insufficient observations to estimate a cut-specific plot.

FIGURE A3. Covariate continuity (age and primary school quality) across the cutoff



Notes: The running variable is calculated as each student's standardised PSLCE score minus the cutoff for the last student not-admitted to National schools from their district. Age is reported for 30 June 2012, as students were sitting their PSLCE, and is calculated based on student reports of their date of birth. Primary school quality is the leave-self-out mean PSLCE score of pupils in each child's primary school. Solid lines are fitted values of regressions of the dependent variable on a linear trend in the transition score, estimated separately on each side of the cutoff.

FIGURE A4. RD plots for lower bound estimates



Notes: Regression discontinuity plots of Lee's lower-bound estimate, i.e. after excluding observations with the highest outcome values from the treatment group, such that both treatment and control have matching attrition rates. In the left hand panel the highest value observations from within the bandwidth are excluded. In the right hand panel, we first split observations into ten equally sized bins, before taking the highest value observations from each, until we reach equal attrition in treatment and control.

FIGURE A5. Average intake ability among those selected into each secondary school versus average intake ability of those we match across tests and base our VAM on

Appendix 2

Dong (2017) presents a method for estimating the treatment effects in a regression discontinuity (RD) design, using, while accounting for sample selection. A simplified summary of the process follows and returns a range for the treatment effect that considers the uncertainty introduced by missing data due to attrition.

1. Identify and estimate initial effects: using standard RD techniques (Calonico et al. 2014), estimate the initial effects of the treatment on the probability of being observed (extensive margin) and on the outcome itself (intensive margin) at the cutoff point.
2. Determine the proportion of quitters: calculate the proportion of individuals who drop out due to (not receiving) the treatment, which represents attrition, and is estimated at the cutoff point.
3. Estimate quantile treatment effects (QTEs): Calculate the treatment effects at various points in the outcome distribution for the observed sample, not yet accounting for attrition (following Frandsen, Frölich, and Melly 2012).
4. Adjust QTEs for attrition: incorporate the estimated proportion of quitters into the QTE calculations to correct for the potential bias caused by missing outcomes. For a lower bound estimate, assume that quitters have the worst possible outcomes (i.e. they sit at the bottom

end of the outcome distribution and there is no need to impute their outcomes). For an upper bound estimate, assume that quitters would have had outcomes similar to those who are observed.

5. Construct bounds for treatment effects: Using the adjusted QTEs, determine the lower and upper bounds of the treatment effects for the subgroup of 'always-participants' (individuals whose outcomes are observed regardless of treatment status)

TABLE A4. Dong (2017) RDD with sample selection, full estimation of bounds

Panel A: Components of RD with Sample Selection			
A	Pr(S1=1)	0.749***	*Probability of being in sample if treated, at cutoff*
		(0.025)	
B	Pr(S0=1)	0.619***	*Probability of being in sample if untreated, at cutoff*
		(0.022)	
(A)–(B)	Extensive margin	0.129***	*Attrition difference, estimated at the cutoff*
		(0.033)	
C	E(Y1|S1==1)	1.990***	*Outcome among those treated & observed, at cutoff*
		(0.056)	
D	E(Y0|S0==1)	1.589***	*Outcome among those untreated & observed, at cutoff*
		(0.054)	
(C)–(D)	Intensive margin	0.401***	Outcome difference, at cutoff
		(0.077)	
Panel B: Treatment effect bounds under monotonic selection (for group of always participating compliers)			
	Lower bound	0.280*	
		(0.149)	
	Upper bound	0.485***	
		(0.090)	

Notes: All estimates are conditional on compliers, with normalised PSLCE score equal to zero (i.e. at the cutoff for National school entry). Estimation of the extensive and intensive margins, and the bounds follows methods in Dong (2017). Calonico, Cattaneo and Titiunik (2014) bias-corrected robust inference is used. Panel A: Cluster-robust bias-corrected standard errors in parentheses. Panel B: bootstrapped standard errors in parentheses. * p < 0.10, ** p < 0.05, *** p < 0.01.