"DO STUDENTS BENEFIT FROM ATTENDING BETTER SCHOOLS?: EVIDENCE FROM RULE-BASED STUDENT ASSIGNMENTS IN TRINIDAD AND TOBAGO"

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In Trinidad and Tobago students are assigned to secondary schools after fifth grade based on achievement tests, leading to large differences in the school environments to which students of differing initial levels of achievement are exposed. This paper uses instrumental variables based on the discontinuities created by the assignment mechanism, and exploits rich data which include the students' test scores at entry and secondary school preferences to address self-selection bias. I find that attending a better school has large positive effects on examination performance at the end of secondary school. The effects are about twice as large for girls than for boys.

I Introduction and Background

In Trinidad and Tobago, students take an exam at the end of fifth grade that is used to assign them to secondary school. Students list their secondary school choices, and the likelihood of being assigned to their first-choice school increases with their score. Since students usually rank higher-achieving schools higher on their lists, high-achieving students typically attend high-performing secondary schools while low-achieving students typically attend the poorest-performing schools. This assignment mechanism has a profound effect on the schooling environments to which students are exposed. First, it lowers average peer quality for low-achievement students and increases average peer quality of high-achievement students. This is important because several studies have found that students tend to have better outcomes when they are exposed to higher-achieving peers.[1] Further, the quality of school inputs may be endogenous to the quality of peers because schools with bright, motivated students may attract better teachers, and may have more affluent alumni networks leading to better facilities and

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[1] Several studies find that students tend to have better outcomes on average when their peers are brighter on average [Hoxby (2000), Hoxby and Weingarth (2005), Sacerdote (2001), Zimmerman (2003)] while others provide mixed evidence [Katz, Kling Liebman (2007), Angrist and Lang (2004), Burke and Sass (2006)].

better funding.[2] As such, ability-grouping (assigning students to schools based on their demonstrated ability ─ not to be confused with streaming, tracking, or ability-grouping *within* schools) may engender large differences in the quality of schools students of different initial achievement levels attend. As such, ability grouping across schools provides a unique opportunity to investigate the relationship between school quality and academic outcomes.

Researchers have linked differences in school quality to differences in labor market outcomes (Card and Krueger 1992a;1992b, Betts 1995, and Grogger 1996) and higher test scores to higher subsequent earnings (Murnane, Tyler and Willet 2000), suggesting that attending a "better" school may have important and long-lasting positive effects on students. The empirical difficulties in uncovering the causal effect of attending a "better" school lie in the fact that students may self-select into schools. Students with the same incoming test scores who attend different schools may have different preferences or levels of motivation. Since preferences and motivation are typically not observed, researchers have dealt with this issue by relying on plausibly exogenous variation in school attendance. Using lottery assignment to schools, Cullen, Jacob and Levitt (2005) find that Chicago students who transfer to high-achieving schools show no improvement in test scores while Hastings and Weinstein (2007) find that students in Charlotte-Mecklenburg who transfer to substantially higher-achieving schools experience sizable improvements in test scores. Other studies have used Regression Discontinuity (RD) designs that compare the outcomes of students with test scores just above and just below some exogenously set cut-off above which admission to a high-achievement school is very likely and below which admission to such a school is unlikely. Clark (2008) finds that gaining admission to selective secondary schools in the United Kingdom does not improve test scores, while Pop-Eleches and Urquiola (2008) find that students in Romania who gain admission to more selective schools have better test score performance. It is apparent that there is no consensus on whether students benefit from attending "better" schools.

Contributing to this literature, I use data from Trinidad and Tobago to investigate the following empirical questions: (1) Do students, on average, benefit from attending "better" schools (i.e. schools that attract higher-achieving peers) on a range of academic outcomes? (2) Do the marginal effects vary by gender, and (3) Are the marginal effects non-linear (i.e. does

[2] Supporting this notion, Jackson (2009) finds that a quasi-exogenous repatriation of low-income black students into schools at the end of school desegregation was associated with decreases in teacher quality.

attending a school with marginally higher-achieving peers have larger effects at low or high peer achievement levels)? Trinidad and Tobago data are well suited to identifying school effects because: (a) the student assignment mechanism creates exogenous variation in school attendance, (b) there is a national curriculum so that school effects are not confounded with large curricular differences,[3] and (c) all schools have homogenous student populations so that school effects are not confounded with a "homogeneous student body" effect.[4] As such, differences in school value-added in Trinidad and Tobago primarily reflect differences in peer quality and differences in teacher and input quality endogenous to peer quality.

To address the self-selection bias that often makes it difficult to obtain credible causal effects when comparing observationally similar students who attend different schools, I use rule-based instrumental variables in the spirit of Campbell (1969) and Angrist and Lavy (1999) based on the student school assignment rules used by the Ministry of Education. The assignment rules (described in Section II) are largely deterministic, non-linear functions of student preferences and incoming test scores that lead to test score cut-offs for each school below which admission is unlikely. As suggested in Fisher (1976), I use the deterministic portion of the assignment rules to obtain rule-based assignments, which are complicated non-linear functions of test scores and preferences, as exogenous instruments while directly controlling for smooth functions of these same underlying covariates. The rule based assignments are, in essence, an interaction between students' preferences and student test scores, resulting in two distinct sources of plausibly exogenous variation: (a) the variation in schools attended among students with the same preferences and similar scores because some scored just above the rule-based cut-off while others scored just below (conditional on their test scores); and (b) the variation in schools attended among students with the same test scores because they had slightly different preferences for schools (conditional on the actual preferences). A unique feature of these data is that I can

[3] Ability-grouping is often coupled with a dual education system where certain schools have an academic focus while others have a vocational focus. Malamud and Pop-Eleches (2007) find that students in Romania were less likely to work in manual or craft-related occupations when they received a general education. While selective schools in Trinidad and Tobago may teach at a faster pace than non-selective schools, the core material covered will largely be the same so that curricular differences, if any, are small.

[4] The main theoretical justification for ability-grouping (both at the school and classroom level) is that a homogeneous student body may lead to improved student outcomes by allowing for more student cohesion, greater teacher focus, and a curriculum and pace more closely tailored to the particular ability level of the students. Researchers have studied the distributional and efficiency effects of *classroom* ability-grouping, and the results are mixed [studies include Betts and Shkolnik (1999); Rees, Brewer and Argys (1999), Figlio and Page (1998, 2002); Hoffer (1992)]. Using experimental data, Duflo, Dupas and Kremer (2008) find that the classroom homogeneity created by ability-grouping may benefit both high and low-achieving students.

observe, and control for, a student's desired schools so that I can credibly compare the outcomes of students who attend different schools even if they did not score near a test score cut-off. I implement both discontinuity-based and difference-in-difference based identification strategies that isolate two distinct sources of plausibly exogenous variation, and I show that the results are similar across the two. Furthermore, to show that my identification strategies are valid, I show that, conditional on test scores and preferences, the instruments are not correlated with incoming student characteristics such as religion, gender, and primary school district.

This paper is related to the school ability-grouping (often referred to as tracking or streaming) literature as I estimate the effects of attending a school with marginally higher-achieving peers on students in a an ability-grouped schooling system. Researchers generally find that school ability-grouping is associated with increased educational and socio-economic inequality.[5] However, much of this evidence relies on comparisons between observationally similar students in ability-grouped and non-ability-grouped school systems. As documented by Dustmann (2004) and argued by Manning and Pischke (2006), much of the evidence may not reflect causal relationships since students may select into schools based on *unobserved* characteristics that also affect outcomes. As such, the effect of ability-grouping on students remains unclear. Even though I do not identify the effect of moving from an ability grouped system to an ungrouped system, because the full effect of ability-grouping will reflect, in part, the effect of ability-grouping on the margin, credible evidence on how students in an ability-grouped education system are affected by ability-grouping contributes to this literature.[6]

While school effects likely reflect a combination of peer, teacher, and school input quality effects, it is helpful to categorize schools by the achievement level of the students. The results indicate that there is student self-selection such that OLS estimates overstate the benefits to attending schools with higher-achieving peers. However, instrumental variables and RD-type estimates show that students who attend schools with higher-achieving peers are more likely to

[5] Atkinson, Gregg and McConnell (2006); Ariga, Brunello, Iwahashi and Rocco (2005); Brunello and Checci (2006); Hanushek and Woessmann (2007); Maurin and McNally (2007).

[6] Using an RD design, Duflo, Dupas and Kremer (2008) find that marginal students who gain admission to high ability classrooms within tracked schools have similar outcomes to those students who do not ─ evidence that effects for the marginal student may differ from the full effects of moving from one system to another. However, it is important to note that Duflo et. al. look at tracking within schools and are based experimental variation that does not allow for the natural long run differences in input quality that may develop across high and low achievement schools. Since these cross school differences in inputs across schools may be very important, the applicability of Duflo et. al. (2008) to across school tracking may be limited.

have high test scores, pass more exams, and earn the prerequisites for admission to tertiary education. I find little effect on staying in school to take the secondary leaving exams. These findings suggest that ability-grouping may increase educational inequality, *on the margin*, by reinforcing pre-existing achievement differences. The estimated effects are about twice as large for girls than for boys, indicating that girls benefit more from attending schools with higher-achieving peers than boys. The results suggest students benefits from attending better schools at all points in the school quality distribution. However, the effect of attending a school with marginally higher-achieving peers is low among schools with low-achieving students.

The remainder of the paper is as follows: Section II describes the Trinidad and Tobago education system and the data used. Section III describes the empirical strategy. Section IV presents the results, and Section V concludes.

II The Trinidad and Tobago Education System and the Data.

The Trinidad and Tobago education system evolved from the English education system. Secondary school begins in first form (the equivalent of grade 6, hereinafter referred to as 6^{th} grade) and ends at fifth form (the equivalent of grade 10, hereinafter referred to as 10^{th} grade) when students take the Caribbean Secondary Education Certification (CSEC) examinations. These are the Caribbean equivalent of the British Ordinary levels (O-levels) examinations.[7] The CSEC exams are externally graded by examiners appointed by the Caribbean Examinations Council. Students seeking to continue their education typically take five or more subjects, and the vast majority of testers take the English language and mathematics exams.[8]

In Trinidad and Tobago, there are eight educational school districts. Unlike in many countries where private schools are often of higher perceived quality, private schools in Trinidad and Tobago account for a small share of student enrollment and tend to serve those who "fall

[7] There are 31 CSEC subjects covering a range of purely academic subjects such as Physics, Chemistry and Geography, and more work and vocationally related subjects such as Technical Drawing and Principles of Business and Office Procedures.

[8] The CSEC examinations are accepted as an entry qualification for higher education in Canada, the United Kingdom and the United States. After taking the CSEC, students may continue to take the Caribbean Advanced Proficiency Examinations (CAPE), at the end of sixth form (the equivalent of grade 12), which is considered tertiary level education but is a prerequisite for admission to the University of the West Indies (the largest University in the Caribbean and is the primary institution of higher learning for those seeking to continue academic studies). The CAPE is the Caribbean equivalent of the English Advanced Levels (A-Levels) examinations.

through the cracks" in the public system.[9] There are three types of public secondary schools: Government schools, Government assisted schools (referred to as assisted schools) and Comprehensive schools. Government schools are secondary schools that provide instruction from 6th through 10th grade and often continue to 12th grade (called upper-sixth form). These schools teach the national curriculum and are fully funded and operated by the Government. Government assisted schools, often the more elite schools, are like Government schools but differ along a few key dimensions. They are run by private bodies (usually a religious board) and, while capital expenses are publicly funded, their teacher costs are not paid for by the Government. Along all other dimensions, Government and Government assisted schools are virtually identical. The third type of schools, Comprehensive schools, are Government schools that were *historically* vocational in focus. In the past, students with low test scores after 5th grade were assigned to such schools and after 3 years took an exam to gain admission to a senior secondary school (or possibly a regular Government school) which would prepare them for the CSEC examinations. During the relevant sample period Comprehensive schools differed from Government schools only in name. All schools taught the same academic curriculum, and only a few Comprehensive schools did not provide instruction through to the CSEC exams.[10]

II.1. Data and Summary Statistics:

The data used come from two sources: the official SEA test score data (from 5th grade) for the 2000 cohort and the official 2004 and 2005 CSEC test score data. The SEA data contain each of the nation's 31,593 student's SEA test scores, their list of preferred secondary schools, their gender, age, religion code,[11] primary school district, and the secondary school to which they were assigned by the Ministry of Education. The SEA exam is comprised of five subjects that all students take: math, English, science, social studies, and an essay. The total SEA score is the sum of the scores on the individual sections and ranges from 0 to 650. To track these 5th grade

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[9] This is evidenced by the fact that students who attend private secondary schools have test scores that are a third of a standard deviation lower than the average SEA taking student, and half a standard deviation lower than the average among those students who take the CSEC exams.

[10] In those few junior Comprehensive schools that do not provide instruction through to the CSEC exams the vast majority of students would attend the senior secondary school associated with their junior secondary school. For example, a typical student who is assigned to Arima junior secondary school will take the CSEC examinations at Arima senior secondary school, provided the student does not drop out of the system.

[11] To preserve confidentiality I was not given access to the actual religion, but a code that identified students' religions.

students through to secondary school in 10th grade, I link the SEA data with the 2005 and 2004 CSEC examination data. Of the 31,593 SEA test takers in 2000, 22,876 students were linked to CSEC exam data five years later (or four years for early takers).[12] The CSEC data contain each student's grades on each CSEC exam and the secondary school they attended. In the data, there are 123 public secondary schools and several small test taking centers and private schools. Among those students linked to CSEC data, 1,364 (just under six percent) attended a private institution, were home schooled, or were unaffiliated with any public education institution. With the CSEC data, I determine whether a student took the CSEC exams, compute the number of examinations taken and passed, and determine if they obtained the pre-requirements for tertiary education (passing five CSEC exams including English and mathematics). I also report students' grades on the English and mathematics CSEC exams. In its raw form, lower scores on the CSEC examinations denote better performance. For ease of interpretation, the CSEC scores have been recoded so that higher scores reflect better performance.

Table 1 summarizes the final dataset, broken up by the secondary schools' rankings in incoming SEA scores (i.e. the school with the highest average incoming total SEA scores is ranked first and the school with the lowest average total SEA scores is ranked last).[13] The SEA scores have been normalized to have a mean of zero and a standard deviation of one. As one can see in Table 1, there is substantial variation in school and peer quality in Trinidad and Tobago. The average total SEA scores at the top 40 schools are 1.14 standard deviations higher than at the middle 40 schools and 1.78 standard deviations higher than at the bottom 43 schools (similar patters exist more math and English SEA scores). The difference between students in the top and bottom ranked schools is a full 4.93 standard deviations. To provide a deeper sense of the variation in peer quality across schools in Trinidad and Tobago, Appendix Figure A1 shows the distribution of total SEA scores for schools with different ranks in mean peer quality.

As is becoming increasingly common in many countries, females make up slightly more than half of students in each school group. As one might expect, those schools that have the

[12] Students were matched based on name, gender and date of birth. The match rate was just over 70 percent, which is consistent with the national high school dropout rate of one third.

[13] To get a sense of the distribution of mean peer quality across schools, Appendix Figure A2 shows the distribution of mean incoming SEA scores for the schools to which students were assigned. This measure is not identical to the peer quality students are actually exposed to since not all students remain in their assigned school. While there are a few schools with mean peer test scores lower than one standard deviation below the mean, the remaining schools are relatively evenly distributed between 1 standard deviation below the mean and 2 standard deviations above the mean.

brightest peers also have the best outcomes. In 2000, 90 percent of students at schools ranked better than 40 took the CSEC exams compared to 75 percent for schools ranked 41 to 80, and 65 percent for schools ranked below 80. Also, the average student at a top 40 school takes 6.38 exams and passes 5.45 of them, compared to taking 4.43 exams and passing 2.2 exams in schools ranked between 41 and 80 and taking 2.93 exams and passing only 1.03 at schools ranked below 80. Some of these differences reflect the fact that students who do not take the CSEC exams have no passes or exams attempted.[14] There are also large differences in math and English grades earned by these students on the CSEC exams. The CSEC grades go from 1 through 7, with 1 being the lowest score and 7 being the highest. A one point difference represents the difference between an A and a B. I assign students who have not taken the CSEC exams the lowest grade of 1 (I discuss the implications of this imputation in Section IV). Students at top 40 schools score on average 2.05 and 2.2 grade points better in math and English CSEC exams, respectively, than students in schools ranked 41 through 80. They also score 3.08 and 3 grade points better in math and English, respectively, than students at schools ranked below 80. In other words, if the average student at a top 40 school earns a B, the average student at schools ranked between 41 and 80 earns a D and a student in a school ranked below 80 earns an F. The last outcome is obtaining a certificate. This variable denotes passing five CSEC subjects including math and English. This is a common prerequisite for continuing education. There are clear differences in this outcome across schools such that 70 percent of students at the top 40 schools earn a certificate, compared to only 18 percent at schools ranked between 41 and 80, and 5 percent at schools ranked below 80. Surprisingly, virtually no student who attends a school ranked below 80 satisfies the requirement to continue to 11th and 12th grades.

Table 1 documents that schools with the highest achieving students are on average smaller and disproportionately Government assisted schools, while the schools with the weakest performing students are disproportionately Comprehensive schools. Roughly two thirds of the top 40 schools are assisted while none are Comprehensive, and about one third of schools outside of the top 40 are Comprehensive schools. In Trinidad and Tobago, as in many nations, the schools that attract the brightest students typically have the best school resources. The one input for which there is aggregate data across school types is teachers. In 1999, 86 percent of teachers

[14] In section IV.1, I decompose the full effect of attending a better school into the effect associated with an increased likelihood of taking the CSEC exams and the effect due to improving CSEC performance among students who would have taken the CSEC irrespective of the school they attended.

at Government assisted schools had a bachelor's degree compared to 70 percent for Government schools and only 64 percent for Comprehensive schools. Similarly, 31 percent of Government assisted school teachers had an education degree compared to 28 percent for Government schools and 12 percent for Comprehensive schools (National Institute of Higher Education and Science and Technology 1999).

II.2. Student Assignment Rules (Algorithm): Students in Trinidad and Tobago compete for a limited number of places at premium schools. After grade five, students take the SEA examinations. Each student lists four ordered secondary school choices. These choices and their SEA score are used by the Ministry of Education to assign them to schools using the following algorithm. Each secondary school has a predetermined number of open slots each year and these slots are filled sequentially such that the most highly subscribed/ranked school fills its spots first, then the next highly ranked school fills is slots and so on and so forth until all school slots are filled. This is done as follows: (1) Each student is placed in the applicant pool for their first choice school. The school that is oversubscribed with the highest "cut off" score fills its slots first. For example, suppose both school A and school B have 100 slots, and 150 students list each of them as their top choice. If the 100th student at school A has a score of 93% (its "cut-off" score) while the 100th student at school B has a score of 89%, school A is ranked first and fills all its spots first. (2) Those filled school slots and the students who are assigned to the highest ranked school are removed from the applicant pool and the process is repeated, where a student's second choice now becomes their first choice if their first choice school has been filled. This is repeated until all slots are filled.

This process is used to assign over 95 percent of all students. However, there is a group of students for whom this mechanism may not be used. Government assisted schools (which account for about 16 percent of school slots) are allowed to admit 20 percent of their incoming class at the principal's discretion. As such, the rule is used to assign 80 percent of the students at these schools, while the remaining 20 percent are hand-picked by the school principal before the next highest ranked school fills any of its slots. For example, suppose the highest ranked school has 100 slots and is a Government assisted school. The top 80 applicants to that school will be assigned to that school while the principal will be able to hand pick 20 other students at their discretion. The remaining 20 students would be chosen based on family alumni connections, being relatives of teachers or religious affiliation (Since Government assisted schools are often run by religious bodies). These hand-picked students may list the school as their top choice, but this need not be the case. Students receive one assignment and are never made aware of other schools they would have been assigned to had they not been hand-picked. Only after all the spots (the assigned 80 percent and the hand-picked 20 percent) at the highest ranked school have been filled will the process be repeated for the remaining schools. As such, the school assignments are based partly on a deterministic function of student test scores and student preferences (which is beyond students' control after taking the SEA exams), and partly on the hand-picking of students by school principals (which can potentially be manipulated by students).

Since student preferences are an important part of the assignment process, it is important to better understand them. Students' school choices are based largely on their own perceived ability, geography, and religion. Specifically, higher ability students tend to have higher achievement schools in their list, students often request schools with the same religious affiliation as their own, and students typically list schools that are geographically close to their homes. Figure 1 shows the cumulative distribution of the mean peer incoming SEA scores of students' school choices. As one would expect, the distribution of mean SEA scores of first choice schools is to the right of the second choice schools which is to the right of the third choice schools which, in turn, is to the right of the fourth choice schools. In other words, students tend to put schools with higher-achieving peers higher up on their preference ranking. In fact, on average the difference between the mean incoming SEA scores at a student's top choice school and second choice school is 0.277 standard deviations, between the top choice school and the third choice school is 0.531 standard deviations, and between the top choice school and the fourth choice school is 0.82 standard deviations. Roughly 15 percent of students make their top choice school, and for those students who do not, the difference in mean total SEA scores between their actual school and their top choice school is 0.87 standard deviations.

III Identification Strategy

I aim to estimate the effect of attending a higher-achievement school on students' academic performance. In sub-section III.1, I describe a baseline empirical model and point out its limitations. I then describe the rule-based instruments, and show that they are a good approximation of the real assignment algorithm in sub-section III.2. In sub-section III.3, I discuss the two distinct sources of exogenous variation in students' school assignments that are generated by the rule-based instrument, I detail the different identification strategies used to isolate each of them, and I then detail a rule-based instrumental variables model that exploits all the exogenous variation. In sub-section III.4, I present specification and falsification tests to show the validity of the identification strategies.

III.1 Baseline model: To estimate the effect of attending a school with higher-achieving peers, the basic empirical strategy is to compare the outcomes of students with similar incoming test scores at different schools using cross-sectional variation. For the baseline specification, I model the outcome of student *i* at a school *s* with the following equation.

[1] $$Y_{i,s} = SEA_i \cdot \beta + \overline{SEA}_s \pi + X_i \delta + \varepsilon_{i,s}$$

In [1], $\overline{SEA}_s$ is the mean total SEA scores for incoming students at school *s*, SEA_i is a matrix of incoming test scores (a quartic in the student's total SEA score, and a quadratic in the math and English SEA score), X_i is a vector of control variables that includes student gender, religion, primary school district, and their school preferences, and $\varepsilon_{i,s}$ is the idiosyncratic error term. One would expect individual SEA scores to remove a large amount of self-selection bias. Despite this, OLS without preferences may still be biased because students may know more about their ability and aspirations beyond their SEA scores, which may be noisy. Adding preferences should remove that bias. However, OLS estimates of π from [1] may still be biased since (1) students who are unhappy with their initial school assignment may be able to transfer across schools, and (2) Government assisted schools can admit 20 percent of their incoming class at the discretion of the school principal. Because there is opportunity for students to self-select into schools and schools to hand-pick students, I propose a rule-based instrumental variables strategy to deal with this endogeneity concern.

III.2 Rule-Based Instrument: To remove self-selection bias from the actual school attended, one needs the school assignment that would prevail if Government assisted schools could not select students. Such an assignment can be constructed by "tweaking" the school assignment mechanism to impose the deterministic portion of the assignment mechanism on *all* students. Since the deterministic portion of the assignment mechanism is used to assign most students to schools, the school assignments based on the "tweaked" assignment mechanism should be correlated with the schools students actually attend. However, since the deterministic portion of the assignment mechanism cannot be manipulated by students or school principals, the "tweaked" assignments should be uncorrelated with *unobserved* student characteristics such as motivation and ability, conditional on student test scores and school choices. As such, I propose two instrumental variables strategies based on these "tweaked" assignments.

The rule-based instrumental variables strategies are in the spirit of Campbell (1969), Angrist and Lavy (1999) and Andrabi, Das and Khwaja (2007). I exploit the fact that the school attended, and therefore the mean SEA scores of students at the school attended, is partly based on a deterministic function of the student's total SEA score and the student's school preferences. Since this deterministic function is non-linear and non-smooth, it can be used as an instrument while directly controlling for smooth functions of the underlying covariates themselves (Fisher 1976). For each school student pair, I define the variable $Rule_{is}$ that is equal to 1 if student i would have been assigned to school s had there been no student self-selection or school selection of students and 0 otherwise. $Rule_{is}$ is the deterministic portion of the student assignment algorithm and is not only determined by student test score or student preferences, *but by the interaction between the two*. This fact plays a central role in my identification strategy.

The rule-based instrument, $Rule_{is}$, is constructed sequentially as follows: (1) All secondary school sizes are given,[15] (2) all students are put in the applicant pool for their top choice school, (3) the school for which the first rejected student has the highest test score fills all its slots (with the highest scoring students who listed that school as their first choice), (4) the students who were rejected from the top choice school are placed back into the applicant pool and their second choice school becomes their first choice school, (5) Steps 2 through 5 are repeated, after removing previously assigned students and school slots until the lowest ranked school is filled. The *only* difference between how students are actually assigned and the "tweaked" rule-based assignment is that at step (3) the "tweaked" rule does not allow any students to be hand-picked while, in fact, some students are hand-picked by principals only at Government assisted schools. The resulting $Rule_{is}$ variables correctly identify the school assignment for 16,705 students. Since students who list schools above their score range will not

[15] School sizes are not endogenous to the application process and are based on strict capacity rules. School sizes are determined before students are assigned to schools and based on their predetermined school sizes the algorithm is applied. As such, the number of students assigned to a particular school (even if they do not attend) is the actual number of predetermined slots at the school.

be assigned based on their preferences, there are 6,177 students with no simulated assignment. Among students assigned to schools within their choice set, the rule is correct about two thirds of the time.

Since I aim to identify the effect of attending a better school using only credibly exogenous variation, the final estimation sample is limited to students who (a) were assigned to a school that provides instruction through to the CSEC exams and (b) had a simulated school assignment. This sample restriction excludes 6,177 students without a simulated school assignment, and 2,119 students who were assigned to the three junior secondary schools that have no associated senior secondary school and do not provide instruction through to 10th grade.[16] Of the 123 public secondary schools in Trinidad and Tobago, 98 of them have students who are simulated to be assigned to them.[17] As such, the final data set used comprises of 23,322 students at 98 schools.

If the simulation works well so that the simulated cut-offs are close to the actual cut-offs, among those students who apply to any given school, the likelihood of being assigned to (and thus attending) that school should increase relatively sharply for those right above the simulated cut-off relative to those who score just below the simulated cut-off. To provide evidence of this, I follow an approach used in Pop-Eleches and Urquoila (2008) for combining several discontinuities into one. Specifically, for each school I find all students who list that school as the top choice, re-center all those students' test scores around the cut-off for that school, and then create a sample of applicants for each school. To mimic the sequential nature of the assignment mechanism (i.e. the top ranked school fills its slots before the applicant pool for the second rank school is determined), I then remove students who were assigned to their top choice schools, replace students' first choice with their second choice, and repeat this process with the second choice, third choice, and fourth choice. The applicant samples for all schools are then stacked so that every student has one observation for each school for which they were an actual applicant. For example, a student who attends their top choice school will only be in the data once for their top choice school, while a student who gets into their second choice school will be in the data

[16] To ensure that the results were not being driven by the exclusion of these schools from the sample, I ran models that used the modal secondary school attended by student from these junior secondary schools and included them. The results were not appreciably different.

[17] The remaining schools are schools that nobody lists in their preferences, either because they are new schools, or because they are undesirable. Since students with low scores will be assigned to the local high school that has available space if they "fail" out of their choice schools, students have no incentive to list these schools if they believe they have a chance of gaining entry to a higher ranked school.

twice (once for their top choice school and once for their second choice school). With this stacked dataset, one can see if mean incoming peer test scores increase suddenly for those applicants with scores above the simulated cut-off relative to applicants with scores below simulated the cut-off.

Based on the approach described above, the left panel of Figure 2 shows the likelihood of being assigned to a preferred school as a function of one's incoming SEA score relative to the simulated cut-off for that school (a score of zero is the cut-off for the preferred school). On the left panel, each point is a cell that represents a unique SEA test score relative to the cut off. To give some sense of the sizes of each cell, data points based on more than 200 observations are solid black circles, those based on between 50 and 200 observations are solid grey circles, and points based on fewer than 50 observations as grey hollow circles.[18] One can see that the likelihood of being assigned to a preferred school increases sharply just above a cut-off. It is also apparent that the vast majority of the data lie within 100 points of a cut off. A regression predicting the likelihood of being assigned to one's preferred school as a function of scoring above the threshold for the preferred school and a linear, quadratic, cubic and quartic in the relative score yields a coefficient of 0.72 (se=0.012). The standard error is adjusted for clustering at the assigned school level. In words, on average, an applicant with a test score just above the cut-off for their preferred school is 72 percentage points more likely to be assigned to their preferred school than an applicant with a test score just below the cut-off.

To show that this jump in the likelihood of attending a preferred school is associated with an increase in peer test scores, the right panel of figure 2 shows mean peer SEA quality (re-centered for each cut-off) as a function of one's SEA score relative to the cut-off for a preferred school. These data on the right panel are put into bins 4 SEA points wide. Since students who miss the cut-off for a preferred school are assigned to less preferred schools, and those with

[18] Given the unusual decline in the likelihood of assignment above the cut-off, it is important to highlight three important points about this figure: (1) each data point represents a distinct relative score rather than a given number of students so that while it appears that many students score above the cut-off and are not assigned, in fact this occurrence is uncommon and represents less than 4 percent of the sample. (2) The figure aggregates across all schools so that while the decline in the likelihood of assignment far above the cut-off appears to be a general phenomena, it is actually driven by a few students at a few low-achieving schools. (3) Those students who are above the cut-off to a school but are not assigned, are high-scoring students who were assigned to high achievement government assisted schools that were not in the students' list of schools ─ showing that this non-compliance is driven entirely by the latitude granted to government assisted schools (exactly the kind of endogenous behaviors the rule is constructed to remove).

higher SEA scores will be assigned to "better" schools on average, there is a natural positive relationship between one's relative score and mean peer quality below any given cut-off. One can see this positive relationship in the right panel of Figure 2. Consistent with the sharp increase in the likelihood of attending a preferred school on the left panel, mean peer quality increases suddenly for those applicants with scores above the simulated cut-off relative to applicants with scores below the simulated the cut-off. Figure 2 provides compelling visual evidence that there were cut-off rules used, the simulated cut-offs are approximately in the same areas as the real cut-offs, and scoring above a simulated cut-off for a preferred school results in a discontinuous increase in peer quality. A regression predicting mean peer SEA scores as a function of scoring above the threshold for the preferred school and a linear, quadratic, cubic and quartic in the relative score yields a coefficient of 0.2 with a standard error of 0.018.[19] This indicates that, on average, an applicant with a test score just above the simulated cut-off for their preferred school attends a school where mean peer test scores are one fifth of a standard deviation higher than an applicant with a test score just below the cut-off.[20]

The second important aspect of the rule is that students be assigned to schools that are in their choice set, and are not assigned to schools that are not in their choice set unless they fail out of all their listed schools or they are hand-picked by assisted school principals. As such, it is important to establish that in general the stylized assumption driving the "tweaked" rule is supported by the data. Some statistics will show that this is the case. Of the 31,593 students who took the SEA exams, 21,466 were assigned to schools in their choice set. Second, as shown in Figure 2, students were more likely to be assigned to their preferred school the higher their score. Third, among those students who were assigned to schools not in their choice set, average mean peer SEA scores were 0.638 standard deviations lower in the actual school assigned than in the student's fourth ranked school. In sum, the evidence strongly suggests that the assignment mechanism operates as described, that the simulated rule is a good approximation of the actual

[19] Results obtained by estimating separate quadratic function on either side of the simulated cut-offs yield a coefficient of 0.211with a standard error of .0075.

[20] The R-squared in a model predicting the likelihood of being assigned to one's preferred school goes from 0.41 to 0.55 and the estimated slope through the cut off (based on the global quartic fit of the relationship between SEA scores and the outcomes) goes from -0.024 to -0.007 without and with the cut-off, respectively. The R-squared in a model predicting mean peer SEA scores goes from 0.66 to 0.68 and the estimated slope through the cut off goes from -0.017 to -0.01 without and with the cut-off, respectively. The differences in estimated slopes are large for both outcomes ─ indicating that the cut-offs have much explanatory power. The small differences in R-squared for mean peer SEA scores is not surprising given that there is a lot of variation other than that due to the cut off (e.g. the strong positive relationship between mean peer test scores and own SEA scores below the cut-off).

mechanism, and the assignment rule results in the expected treatment differential.

III.3 Sources of Exogenous Variation and the Econometric Models:

Conditional on incoming test scores and preferences, $Rule_{is}$ captures two plausibly exogenous sources of variation. In this section I discuss these two distinct sources of exogenous variation, and I describe instrumental variables estimation strategies based on the two sources of variation described above. I then present my preferred strategy that exploits both sources of credibly exogenous variation.

Discontinuity Variation: The first source comes from comparing the outcomes of students at different schools who score just above and just below a school's cut-off. The logic behind this source of variation is similar to the familiar regression discontinuity logic. Specifically, the likelihood of being assigned to one's preferred school and therefore attending a school with higher achieving peers increases in a sudden and discontinuous manner as one's score goes from below the cut-off to above the cut-off for that school (as Figure 2 demonstrates). If the location of the cut-off is exogenous to student characteristics, one can reasonably attribute any discontinuous jumps in the outcomes as one's score goes from below to above the cut-offs to the increased likelihood of attending one's preferred school.

If there is a causal relationship between attending a better school and CSEC performance, then scoring above the cut-off should be associated with improved outcomes. Using the stacked dataset as described previously, I use scoring above the cut-off as an instrument for attending a school with higher-achieving peers. Specifically, I estimate [2] and [3] with 2SLS.

[2] $$\overline{SEA}_s = f(SEA_{i,t-1}) + Above_{is} \cdot \varphi_1 + \nu_{s1} + \varepsilon_{i,s,t,1}$$

[3] $$Y_{i,s,t} = f(SEA_{i,t-1}) + \overline{SEA}_s \pi_{s,2} + \nu_{s2} + \varepsilon_{i,s,t,2}$$

All variables are defined as in [1], $\overline{SEA}_s$ is the mean total SEA scores for incoming students at school *s*, $Above_{is}$ is an indicator variable that is equal to *1* if student *i* has a SEA score above the simulated cut-off for school *s* and *0* otherwise, and ν_s is a fixed effect for each cut-off (preferred school). Since we know *ex ante* that Government assisted schools do not comply with the cut-offs, I present results that exclude estimates based on cut-offs for Government assisted schools. The excluded instrument $Above_{is}$ yields a first stage F-statistic greater than 100. It is worth noting that while the setup looks a lot like a fuzzy-regression discontinuity approach, it is not. Since the location of the discontinuities are not known, they are simulated. This introduces additional noise. As such, this strategy is best described as an instrumental variables strategy that lives of the discontinuities inherent in the assignment process. I will refer to it as a discontinuity design. For the results reported in the main text of the paper, to rely on variation due to the cut off and to be less reliant on functional form assumptions, I am careful to focus the analysis to students within 100 points of the cut-off. I show that the results are robust to controlling for different flexible functional forms to account for smooth functions of the total score in Appendix Table A2. Following Clark (2008), since the cut-offs are simulated and therefore not exact, to avoid misclassification around the cut-off I remove observations within 3 points of the cut-off.[21] While this has little qualitative effect on the estimated coefficients, it has a discernable effect on the strength of the first stage and the precision in the second stage. Appendix Note A1 presents a visual representation of discontinuity-based model.

Difference In Difference Variation: The second source of variation comes from comparing the outcomes of students with the same test scores at different schools because they have different school preference orderings. Since preferences are directly observed, and the cut-offs generate exogenous variation in school assignments among students with the same preferences, one can directly control for a student's preferences (a unique feature of the Trinidad and Tobago data). To make this clearer, consider two students (A and B) with the same test score X at different schools. Suppose both A and B list the same first choice school, but list different second choice schools. If they both just missed the cut-off for their top choice school, then they will both end up attending their second choice schools. A comparison of the outcomes of A and B across their different schools will reflect both differences in preferences and differences in schools. Consider now, two other students (A' and B') such that A' has the same preferences as A, and B' has the same preferences as B, but A' and B' have the same score X' that is higher than X. If X' is above the cut-off for the top choice school, then A' and B' will both attend the same top choice school

[21] Since the cut-offs are approximate (by construction) the clean discontinuity occurs on the outer limits of some approximation error around the simulated cut-off. Specifically, for any given school, the simulation will define the cut-off one or two points above (or below) the actual cut-off generating classification error in the instrument within a narrow band of the cut-off. By removing observations within two or three points of the cut-off, I remove this classification error and improve the statistical inference. While removing these observations does not change the estimates in any meaningful way, it does increase power in the first stage and it does reduce standard errors in the second stage.

even though they listed different second choice schools. Any difference in outcomes between A' and B' must reflect their preferences, since they have the same test scores and attend the same school. Under the assumption that differences in outcomes due to preferences are the same across all levels of achievement, one can subtract the difference between A' and B' from the difference between A and B to isolate the differences in outcomes associated with different schools.

This variation comes from the fact that the simulated assignment is a deterministic function of *the interaction between preferences and incoming test scores*, so that conditional on test scores and preferences, there is useful exogenous variation in simulated school assignments. To exploit this variation for identification, I use a DID-2SLS strategy that estimates the effect of schools after controlling for a full set of preference indicator variables and a full set of test score indicator variables (i.e. an indicator variable for each distinct total SEA score - there are 301 such values). Since there is slippage between the assigned school and the attended school, I instrument for the mean peer scores at the school attended with the mean *simulated* peer scores at the *simulated* school assigned (i.e. the mean total SEA scores of all other students assigned to the same school under the simulation). Specifically, I estimate the following system of equations by 2SLS.

[4] $$\overline{SEA}_s = \sum_{k=1}^{300} I_{SEA_i=k} \cdot \theta_k + \pi_1(\overline{\overline{SEA}} \mid Rule_{is}) + X_i\delta_1 + \sum_{p=1} I_{i,p} \cdot \theta_{p1} + \varepsilon_{i,s,t,1}$$

[5] $$Y_{i,s,t} = \sum_{k=1}^{300} I_{SEA_i=k} \cdot \theta_{k,2} + \overline{SEA}_s \pi_{s,2} + X_i\delta_2 + \sum_{p=1} I_{i,p} \cdot \theta_{p2} + \varepsilon_{i,s,t,2}$$

All variables are defined as in [1], $\overline{SEA}_s$ is the mean total SEA scores for incoming students at school *s*, $I_{i,p}$ is an indicator variable equal to 1 if a student's rank ordering is preference group *p* and equal to zero otherwise[22], $I_{SEA_i=k}$ is an indicator variable equal to one if the student's SEA score is equal to *k*, and $(\overline{\overline{SEA}} \mid Rule_{is})$ is the mean total SEA scores of all other students who were assigned to the same school *s* as student *i* based on $Rules_{is}$. Simulated peer quality $(\overline{\overline{SEA}} \mid Rule_{is})$

[22] Each preference group is defined by a distinct preference ordering of schools. All students who list schools A,B,C,D in that order form a group, while students who list schools B,A,C,D are in a different group because even though they have the same list of schools, the ordering is different. There are 4561 preference groups with more than one student.

is the exogenous instrument excluded in the second stage equation.[23] Standard errors are clustered at the assigned school level.

Full Rule-Based Instrument Using all Exogenous Variation: While showing robustness across specifications is important, the most efficient estimates should use all the available clean sources of variation. In my preferred model, I exploit both the discontinuity variation and the difference in difference variation by estimating the DID-2SLS model (equations [4] and [5]) while replacing the SEA test score indicator variables with smooth functions of the total SEA score ─ allowing for additional variation due to the discontinuities. Specifically, I estimate the following system of equations by 2SLS.

[6] $$\overline{SEA}_s = f(SEA_{i,t-1}) + \pi_1(\overline{\overline{SEA}} \mid Rule_{is}) + X_i\delta_1 + \sum\nolimits_{p=1} I_{i,p} \cdot \theta_{p1} + \varepsilon_{i,s,t,1}$$

[7] $$Y_{i,s,t} = f(SEA_{i,t-1}) + \overline{SEA}_s\pi_{s,2} + X_i\delta_2 + \sum\nolimits_{p=1} I_{i,p} \cdot \theta_{p2} + \varepsilon_{i,s,t,2}$$

III.4 Specification Tests and Falsification Tests:

To show that my identification strategies are valid, I first present evidence that the discontinuity-based model is likely to yield consistent and unbiased estimates of the effect of attending a school with higher-achieving peers. The first test of the exogeneity of the cut-off is to see if there is less density than would be expected by random chance right below a cut-off and more density right above the cut-off than would be expected by random chance. Such a pattern would be consistent with gaming of the cut-offs. Figure A3 shows the density of incoming test scores and the vertical line is the cut-off. There is little evidence of such a pattern visually. Following McCrary (2009), I test for discontinuity in the density of the total score at the simulated cut-off while controlling for the relative score, and the quadratic, cubic and quartic of the relative score. Where the dependent variable is the empirical density, the coefficient on an indicator variable denoting "above cut-off" is a statistically and economically insignificant -0.003. Since gaming would imply a positive and statistically significant coefficient, this test suggests no gaming. To further ensure that the discontinuity-based results are not driven by gaming or sorting around the cut-offs, and because the cut-offs are approximate, I remove all points within 3 points of the simulated cut-off in all regression models. This does not affect the

[23] Results using the actual school assignment, $Rule_{is}$ are extremely similar, but yield a weaker first stage.

results in any meaningfull way.

Another test of the validity of the discontinuity design is to see if scoring above the simulated cut-off is associated with a shift in preferences. If the discontinuity design is working correctly, then preferences should be roughly balanced above and below the cut-off. If there is sorting around the cut-offs however, since having preferences for higher-achievement schools is associated with better outcomes (even conditional on test scores and school effects), one would expect that being above the cut-off is associated with having preferences for higher-achieving schools. Unlike most contexts where a discontinuity-based strategy is employed, I do not have to assume that preferences are balanced around a cut-off, and I can test for it directly (and even control for it). However, to test for differences in preferences, I include as the dependent variable the mean peer quality of the student's top choice school. Such a model yields a coefficient on scoring above the threshold of -0.035 with a standard error of 0.026. The same exercise with the second, third, and fourth choice schools yield coefficients of -0.012 (se= 0.027), -0.004 (se= 0.032), and -0.095 (se= 0.051). Only the coefficient for the fourth choice school is even marginally statistically significant. Also, all the point estimates have negative coefficients which, if interpreted causally, would imply *negative* selection. As such, the results suggest that there is little or no selection, and if there were selection, the discontinuity-based results are likely to be biased downward.[24]

As evidence of the validity of the full rule-based instrumental variables method, I test if the instruments, $Rule_{is}$, are correlated with other observable student characteristics before entering secondary school conditional on test score indicator variables and preference indicator variables. I carried out these tests by estimating equations [6] and [7] while using student religion, gender and primary school district as outcomes. Mean peer total SEA scores, as predicted by the mean peer quality from the rule-based instruments, are not associated with any pre-treatment student characteristics. The p-values associated with the null hypothesis that peer achievement (as predicted by the rule-based instrument) is correlated with the pre-treatment characteristics are all above 0.9. Because student religion is explicitly used by principals when hand-picking students at religious schools, the fact that student religion is not correlated with the instruments

[24] Scoring above the cut-off does not predict student gender. Of the ten religion indicator variables, nine had p-value associated with scoring above the threshold greater than 0.3 and one had a p-value of 0.07 (with an economically insignificant point estimate of 0.004). Of the eight school district indicator variables, one yeilded a statstically significant and small effect, while the remaining seven had p-values above 0.2.

lends credibility to the identification strategy.

IV Main Results:

I present the effects of attending a school with higher-achieving peers using different specifications in Table 2. Columns 1 through 4 are based on OLS, columns 5 and 6 show the discontinuity results (2SLS-D) that use scoring above the desired school's threshold as an instrument for attending a school with higher-achieving peers while controlling for cut-off fixed effects and the quartic of the incoming total SEA score. Columns 7 and 8 present the difference-in-difference instrumental variables results (2SLS-DID) that use the simulated peer quality as an interment for actual peer quality while including fixed effects for each individual test score and preference group, and columns 9 and 10 present the full rule-based instrumental variables results (2SLS-Full) based on all the exogenous variation. I present the results for each outcome in a separate row. Since clustering at the student level leads to smaller standard errors than clustering at the school level, I present the more conservative standard errors adjusted for clustering at the school level. The difference in peer achievement between a student's top choice school and their third choice school is roughly half a standard deviation. I use this difference as my measure of the typical difference in peer achievement that a student may face. While categorizing schools by the achievement level of the peers is helpful, the estimated effects will reflect a variety of differences across schools such as teacher quality, input quality and peer quality.

To summarize the main findings, there are large benefits to attending a school with higher achieving peers in the baseline OLS model that are reduced by about 40 percent after controlling for incoming SEA scores. Adding additional controls for preferences and restricting the sample to the students assigned to non-assisted schools has little additional effect on the OLS coefficients. The 2SLS-D results using the full sample are similar to OLS with controls for preferences and SEA scores, however, the 2SLS-D results using the subsample of non-assisted schools that comply with the cut-offs show benefits on the number of exams passed and earning a certificate that are about two-thirds as large as the 2SLS-D using the entire sample.[25] The 2SLS-DID and full rule based 2SLS models show positive effects of attending a higher achievement school on the number of exams passed and earning a certificate that are very similar

[25] The reduced form coefficients on scoring above the cut-off in models with and without assisted schools respectively are 0.029 (se= 0.0165) and 0.01 (se= 0.022) for CSEC taking, are 0.323 (se=0.123) and 0.187 (se=0.123) for the number of exams passed, and are 0.026 (se=0.006) and 0.039 (se=0.007) for earning a certificate.

to the clean 2SLS-D based on the non-assisted schools ─ suggesting that models that deal with the possible self-selection yield slightly smaller point estimates than the OLS models. For the 2SLS-DID and 2SLS-Full models, excluding students assigned to government assisted schools has little effect on the estimates.[26]

The top row of Table 2 shows the coefficient on mean peer SEA scores on an indicator variable equal to 1 if a student took the CSEC exams and equal to zero otherwise. The OLS estimates that include controls for incoming test scores and preferences (top row, columns 2 through 4) suggest that attending a school where peer test scores are half a standard deviation higher is associated with approximately a 3 percentage point increase in CSEC exam taking (none of the estimates is statistically significant at the 10 percent level). However, the 2SLS-D results yield negative coefficients, and the 2SLS-DID and 2SLS-Full models yield small and statistically insignificant coefficients ─ suggesting little or no effect on CSEC taking.

The second row shows the effect on the number of exams passed. The OLS estimates that include controls for incoming test scores and preferences (second row, columns 2 through 4) suggest that attending a school where peer test scores are half a standard deviation higher is associated with passing between 0.67 and 0.75 more CSEC exams. However, the clean 2SLS-D based on the non-assisted schools, the 2SLS-DID and the 2SLS-Full models suggest that attending a school where peer test scores are half a standard deviation higher is associated with passing between 0.28 and 0.37 more CSEC exams. All these estimates are statistically significant at the 5 percent level.

The third row shows the effect on an indicator variable that is equal to 1 if the student obtained a certificate (passed 5 exams including math and English ─ the prerequisite to pursuing tertiary education) and zero otherwise. The OLS estimates that include controls for incoming test scores and preferences (third row, columns 2 through 4) suggest that attending a school where peer test scores are half a standard deviation higher is associated with being between 8 and 9 percentage points more likely to obtain a certificate. The clean 2SLS-D based on the non-assisted schools, the 2SLS-DID and the 2SLS-Full models suggest that attending a school where peer test scores are half a standard deviation higher is associated with being between 5 and 8 percentage points more likely to obtain a certificate. All these effects are positive and statistically significant

[26] The sample sizes differ between the OLS models and the 2SLS-DID and 2SLS-Full models because the instrumental variables models exclude observations with singleton preferences. The OLS results are very similar when one excludes observations with singleton preferences.

at the one percent level. These estimates imply that a student who misses their top choice school would be between 9 and 15 percentage points less likely to obtain the prerequisites to pursue tertiary education.

IV.1 Effects on the Intensive Margin:

Since students who do not take the CSEC exams necessarily pass zero exams and do not earn a certificate, these outcomes are equal to zero for all students who did not take the CSEC exams so that the outcomes can be written as below.

[8] $$Y = I_{take=1} \times (Y \mid I_{take=1} = 1) + 0\,.$$

Where $I_{take=1}$ is equal to one for CSEC takers and zero otherwise. Equation [8] makes explicit that changes in the number of passing grades or the likelihood of obtaining a certificate, shown in Table 2, reflect the effects on both the intensive margin (improvements in CSEC performance for students who would have taken the CSEC exams irrespective of the school they attend) and the extensive margin (the effect of taking the CSEC exams and potentially having some CSEC passes). One may wonder how much of the effect on the number of exams passed or obtaining a certificate are due to students being more likely to take the CSEC exams, as opposed to students who would have taken the CSEC exams regardless performing better at higher-achievement schools. Using the product rule, the expected change in outcomes due to attending a "good" school as opposed to a "bad" school can be written as

[9] $$\Delta E[Y] \equiv \Delta[P(I_{take=1} = 1)] \times (Y_0 \mid I_{take=1} = 1) + P_0(I_{take=1} = 1) \times \Delta(Y \mid I_{take=1} = 1)\,.$$

Where Y_0 is the outcome of CSEC taking students at the "bad" school, and P_0 is the likelihood of taking the CSEC in the "bad" school. Equation [9] shows that changes in outcomes will reflect an effect from increasing the likelihood of taking the CSEC exams $\Delta[P(I_{take=1} = 1)] \times (Y_0 \mid I_{take=1} = 1)$, and an effect from improvements in the outcomes among those students who would have taken the CSEC exams regardless of their assigned school $P_0(I_{take=1} = 1) \times \Delta(Y \mid I_{take=1} = 1)$.[27] The preferred models suggest that there are no differences in the likelihood of taking the CSEC exams across school types. As such, there is likely no effect on CSEC taking so that one can uncover the effect on the intensive margin (the change in outcomes for those students who take the CSEC exams) by dividing the estimated coefficient by the

[27] This approach is similar to that use in Jackson (2009) and Lavy (2009).

likelihood of taking the CSEC exams in "bad" schools. Since all schools except for the highest achieving school are "bad" schools in comparison to higher-achieving schools, I use the mean likelihood of taking the CSEC exams. Since attending a school with 1 standard deviation higher-achieving peers increases the number of CSEC exams passed by between 0.48 and 0.75 and increases the likelihood of earning a certificate by between 0.11 and 0.16, and the likelihood of taking the CSEC exams is 0.73, the implied intensive margin coefficients are between 0.66 and 1.02 for the number of exams passed and between 0.15 and 0.23 for obtaining a certificate. Even if one were to take the OLS estimate of the effect on CSEC taking (a 5 percentage point increase in CSEC taking associated with a one standard deviation increase in peer test scores), this would imply a very small effect on the contribution of the intensive margin.[28]

Another approach to uncovering the effect of attending a better school, conditional on taking the CSEC exams, is to use only the sample of CSEC takers while conditioning on the likelihood of taking the CSEC exams (Angrist 1995) to control for sample selection bias. The results of this method are very similar to those of the decomposition above and are, as such, not presented here. Since the effect on the participation margin is negligible, the fact that most of the effect can be attributed to the intensive margin is not surprising.

IV.2 Effects by gender

There is a growing literature documenting that females often benefit from interventions while males are unaffected and in some cases perform worse.[29] To investigate the effects of attending a school with higher-achieving peers by student gender, I estimate the preferred full rule-based instrumental variables models for the samples of females and males separately.[30] Table 3 presents the result of the model that uses the full sample and the result of the model that

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[28] To get a lower bound of the effect on the intensive margin, I multiply the *maximum* estimated increase in CSEC taking (from the OLS model) by the average outcomes of all students who take the CSEC exams. Since marginal students are likely to have worse outcomes than the average CSEC taker, this calculation will overstate the contribution of the extensive margin yielding a lower bound of the effect of attending a better school conditional on taking the CSEC exams. The average CSEC taker passes 3 exams and obtains a certificate with probability 0.278. Given that attending a school with 1 standard deviation higher-achieving peers increases CSEC taking by at most 5 percentage points, the extensive margin could *at most* be responsible for a $0.05\times3=0.15$ increase in the number of exams passed and a $0.05\times0.278=0.014$ increase in the likelihood of earning a certificate. Subtracting the contribution of the extensive margin from the full effect and then dividing by the likelihood of taking the CSEC exams (0.73) yield lower bound intensive margin coefficients between 0.6 and 0.98 for the number of exams passed and between 0.14 and 0.21 for obtaining a certificate.

[29] For example Kling, Ludwig, and Katz (2005); Anderson (2007); Angrist, Lang, and Oreopoulos (2007); Angrist and Lavy (2007); Hastings, Kane and Staiger (2006a; 2006b).

[30] The 2SLS-D results are much less precise but qualitatively similar and are presented in Appendix Table A1.

omits those students who were assigned to assisted schools. The results for males are presented in the top row, and those for females are in the second row. The results indicate that attending a higher-achievement school has about twice as large an effect on the number of exams passed and on obtaining a certificate for girls than for boys (these differences by gender are statistically significant at the five percent level). There is no effect on the probability of taking the CSEC exams for either sex.

Columns 1 and 2 in the top row show that for neither females nor males are the effects on CSEC taking statistically significant. Turning to the number of exams passed (columns 3 and 4), large gender differences emerge. Specifically, for males, attending a school where peers have incoming test scores half a standard deviation higher results in passing between 0.15 and 0.18 additional CSEC exams (neither estimate is statistically significant). In contrast, for females, attending a school where peers have incoming test scores half a standard deviation higher results in passing between 0.43 and 0.5 additional CSEC exams (both estimates are statistically significant at the 5 percent level). The gender differences in obtaining a certificate are similar to those for the number of exams passed. For males (top row, columns 5 and 6), attending a school where peers have incoming test scores half a standard deviation higher increases the likelihood of obtaining a certificate by between 4.2 and 5.5 percentage points (both estimates are statistically significant). In contrast, for females (bottom row, columns 5 and 6), attending a school where peers have incoming test scores half a standard deviation higher increases the likelihood of obtaining a certificate by about 10 percentage points (significant at the 1 percent level). As with the number of exams passed, the marginal effects are about twice as large for females than for males, and the point estimates are sufficiently different and precisely enough estimated that these differences are both economically and statistically meaningful.

IV.3 Effect on Grades Earned

Much of the literature on the effect of attending a "better" school has found benefits on non-cognitive outcomes such as the number of subjects taken, being suspended, and other behavioral outcomes. However, the findings on the effects on test scores or grades have been mixed. Most studies that look at school effects on student test scores, do so in contexts where all students take the tests. To present a comparable set of effects, I need to estimate the effect of attending a school with higher-achieving peers on performance on a particular exam, conditional on taking the exam. Because virtually all students who take the CSEC exams take both math and English, there is almost no selection to taking these exams conditional on taking the CSEC exams. However, since there may be some *slight* selection into taking the CSEC exams, one needs to take this into account when determining the effect of attending a higher achievement school on students' math and English exam performance for those students who would have taken the CSEC exams irrespective of their school attended. I do this in two ways. First, I estimate the model on all students, assigning the lowest possible grade to students who do not take the subject exam, and find a lower bound of the intensive margin effect using the decomposition discussed in Section IV.1. For the second approach, I condition on the likelihood of taking the CSEC exams and estimate the model only on those individuals who took the CSEC exams [Angrist 1995].[31] Since the effect of attending a better school on the CSEC participation margin, if any, is small, both strategies to account for selection yield similar results.

Table 4 presents the rule-based instrumental variables estimates of attending a school with higher-achieving peers on math and English grades. The top row presents the results for English and the second row presents the results for math. Columns 1 and 2 presents results using all students irrespective of whether they took the CSEC exams, including and excluding the assisted schools, respectively. Columns 3 and 4 present results using only those students who took the CSEC exams while controlling for the likelihood of CSEC taking, including and excluding the assisted schools, respectively. While the point estimates differ across models somewhat, they are all positive and most are marginally statistically significant. Applying the decomposition described above, if one were to divide the estimates in columns 1 and 2 by the likelihood of taking the CSEC (0.73), the estimates suggest that the coefficients on those who take the CSEC would be between 0.28 and 0.45. These figures are very similar to the estimated effects conditional on CSEC taking in columns 3 and 4 (including and excluding assisted schools, respectively) of 0.38 and 0.46. Both these intensive marginal effects are statistically significant at the 10 percent level. The point estimate of 0.46 for the English grade suggests that a student who attends a school where peers have half a standard deviation higher test scores will score 0.23 grade points higher in the English exam. This represents about a quarter of the distance between

[31] To obtain the likelihood of taking the CSEC, I estimate a probit model that predicts the likelihood of taking the CSEC exams as a function of the quartic in incoming total test scores, the quadratic in reading and math test scores, gender, religion indicator variables, and primary school district indicator variables. The non-linear probit model will not converge with 4561 the preference indicator variables so these variables are not included in the propensity score estimation.

an A and a B.[32] The results for math in the second row are much less consistent than those for English. For math, estimates that include all schools yield positive and statistically insignificant estimates between 0.16 and 0.11, while those that exclude assisted school yield negative coefficients.

In an attempt to improve statistical precision, I estimate the same models using the 95 rule-based school assignments as instruments instead of the simulated peer quality associated with those assignments (columns 5 through 8). These instruments yield a reasonable first stage F-statistic of 12.5. While the point estimates are largely the same, the positive effects on English grades are statistically significant at the 1 percent level across all models, while there are no consistent statistically significant effects for math. In sum, while the results indicate positive effects on English examination performance on average, there is little evidence that attending a school with higher-achieving peers improves student's math exam performance on average.

Effects on Grades Earned by Gender: To test for gender differences in exam grades, I estimate the preferred full rule-based 2SLS specification for the test score outcomes (using the sample of CSEC takers and controlling for the likelihood of taking the CSEC) separately for males and females. The results are presented in Table 5. The top row presents models using simulated peer quality based on the simulated school assignment and the models presented in the second row use the actual simulated school assignments as instruments. The first stage F-statistics using the actual simulated school assignments are somewhat smaller than the rule of thumb (8.6 for females and 9.2 for males) so the results in the second row should be interpreted with some caution. The differences in the effects on math and English grades by gender exhibit similar patterns to the other outcomes. The point estimates using the simulated peer quality as an instrument on English grades for females (columns 5 and 6) range from 0.4 to 0.58, while those for males (columns 1 and 2) range from 0.24 to 0.38. Estimates using the school assignments for instruments are very similar, more precise, and are statistically significant at the 1 percent level for females, and marginally statistically significant for males. The estimates suggest a female

[32] Even assuming an extensive margin coefficient 0.05 (the OLS estimate), according to the decomposition, a student who would have taken the CSEC exams regardless of the school assignment would have scored 0.2 grade points (twenty percent of a grade point) higher in the English CSEC exam at a school where peer test scores were half a standard deviation higher. The average CSEC taking students earns a grade of 4.44 on the English exam, while students who do not take the CSEC have a grade of 1. As such a 5 percent increase in CSEC taking could explain at most 0.05*(4.44-1)=0.1725 of the marginal effect. Removing this effect and dividing by the likelihood of taking the CSEC exams, yields an intensive margin coefficient of 0.41.

who attends a school with half a standard deviation higher peer test scores will score between 0.2 and 0.3 grade points higher on her English CSEC exams, while a male student would score between 0.12 and 0.19 grade points higher.

The results for math (columns 3,4,7, and 8) show starker differences by gender. The results indicate that while a female who attends a school with peers with half a standard deviation higher test scores will score between 0.14 and 0.26 grade points higher on her Math CSEC exams (only those estimates using the full sample are statistically significant), males do not appear to benefit at all. In fact, the point estimates in all models for males are negative, suggesting that males could actually have worse math performance when attending schools with higher-achieving peers. In sum, while both males and females may have higher English grades when attending a school with higher-achieving peers (with females benefitting more), females benefit in math performance while males do not.

IV.4 Elite Schools or Bad Schools?

Proponents of school ability-grouping support ability-grouping based on the belief that it creates excellent schools at the top of the achievement distribution, while opponents of school ability-grouping are concerned that it creates an underclass of schools with high concentrations of low-achieving students that produce very low value-added. Much research on school quality has focused on the effect of attending high-achieving or "elite" schools. Since the rule-based instruments provide exogenous variation in school attendance for *all* schools, I can test whether the benefits to attending a school with higher-achieving peers, on average, are driven by large benefits to elite schools at the top of the school achievement distribution, large ill-effects to attending low-achievement schools at the bottom of the school achievement distribution, or if the effect is roughly linear.

To test for such non-linearly, I put schools into groups based on their rank in the school assignment algorithm (top third, middle third, and bottom third). I estimate models for subsamples of students assigned to different schools within these groups. Note that these rankings are among the subsample of the 98 schools to which students have simulated assignment. To allow for a more flexible test of nonlinearity, I estimate the full rule-based 2SLS model using the actual rule-based school assignments as instruments as opposed to the single linear simulated assigned peer quality instrument. The single simulated peer quality instrument performs very poorly in these models.[33] Unfortunately, the rule-based school assignments also yield relatively weak first stages on the subsamples of schools. As such, to show that the estimated patterns are robust, I present *both* the rule-based instrumental variables estimates (top row of Table 6) and the discontinuity based estimates (bottom row of Table 6). Patterns are consistent across both models.

The 2SLS-Full results suggest that attending a better school may increase the likelihood of taking the CSEC exams among low-achievement schools. However this is not supported by the discontinuity results. For the number of exams passed and obtaining a certificate, the results suggest that the marginal effects of attending a higher-achievement school are largest within the top two thirds of schools. For both the discontinuity and the rule-based 2SLS models, within the top two-thirds of schools the marginal effect on the number of exam passes and obtaining a certificate are positive and mostly statistically significant at the 10 percent level. In contrast, the effects on these two outcomes among the lowest group of schools are much smaller and are statistically insignificant. This may reflect the fact that the top two-thirds of schools are better at improving student outcomes than schools in the bottom third, or it may reflect that fact that there are more marginal certificate earners at these schools.

In sum, the results in Table 6 do not provide any strong evidence that the marginal effects of attending higher-achievement schools are larger among the top third of schools than in the middle third. However, insofar as there is any non-linearity, it would appear that for the main outcomes of interest, the marginal effects are low at low levels of school peer achievement, and are higher at medium and high levels of school peer achievement.

V Conclusions

The empirical evidence on whether students benefit, on average, from attending "better" schools is mixed. Ability-grouping, by grouping students by ability, has a profound effect on the peers to which students may be exposed. Since peer quality may be a determinant of other school inputs such as funding levels and teacher quality, ability-grouping may engender large differences in the quality of schools to which students of differing initial levels of achievement are exposed. The large differences in schooling environments created by school ability grouping

[33] Using the single linear instrument performs very poorly on certain sub-samples of schools such that the first stage F-statistics are far below 10 and the point estimates "blow up" in the second stage.

provide a unique opportunity to investigate the effect of attending a "better" school.

To better understand if students benefit from attending "better" (i.e. more selective, more elite, or higher achievement) schools, and to deepen our understanding of ability-grouping, I use Trinidad and Tobago data, where there are no curricular differences across schools, to identify an ability-grouping effect on the margin. Specifically, I test whether students benefit from attending those schools that attract higher-achieving peers. Since students with higher initial achievement attend schools with higher-achieving peers under ability-grouping, this is also a test for whether ability-grouping increases educational inequality, on the margin, by assigning high-achieving students to schools that produce the most value-added while consigning students with low initial achievement to schools that provide the least value-added.

I exploit the rules used by the Ministry of Education to assign students to secondary schools to implement a discontinuity-based, a difference-in-difference based, and a rule-based instrumentation strategy to remove self-selection bias that could affect my findings. All methods yield similar results, and I present falsification tests indicating that the identification strategies are likely valid. After taking self-selection bias into account, I show that students benefit on several outcomes from attending schools with higher-achieving peers — implying that those schools with the highest-achieving peers produce more value-added than schools with lower-achieving peers. The findings present compelling evidence that students do benefit from attending higher-achievement schools, and suggest that, *on the margin*, ability-grouping may lead to increased educational inequality on a broad range of academic outcomes such as test scores, the number of examinations passed, and years of educational attainment.

The results indicate that the marginal effect of attending a school with higher-achieving peers is non-linear so that the benefits to attending schools with marginally brighter peers are low at the lower end of the peer achievement distribution. However, I do not find evidence that attending schools with marginally brighter peers is higher at high-achievement levels than in the middle of the peer achievement distribution. Adding to a growing literature documenting stronger benefits to interventions for females than for males, I find that females benefit more from attending schools with high-achieving peers than do boys on all outcomes. In fact, the marginal effects are about twice as large for females than those for males. One implication of this result is that policies that increase the variability of school quality, such as ability-grouping, could increase the male-female achievement gap. Given the growing concern that boys may be falling behind, particularly in the Caribbean, further research is needed to better understand these gender differences.

From a policy perspective, the finding that attending a more selective or higher achievement school improves one's outcomes implies that practices that group students by ability will tend to reinforce and exacerbate pre-existing differences in academic achievement. More broadly, the findings suggest that policies that create greater variation in school quality, such as the creation of private schools or charter schools, or policies that break-up school districts along socio-economic lines, will tend to increase inequality in educational outcomes. However, one important positive implication of these findings is that policies that improve the schooling environments of children may be effective at improving their educational outcomes and their subsequent economic well-being.

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Table 1
Summary Statistics: By School Rank in Average Incoming Total SEA Scores

Rank Range (by incoming peer scores)	1-40	41-80	81+
Normalized SEA Score Total (incoming)	1.26	0.12	-0.52
	(0.67)	(0.76)	(0.80)
Normalized SEA Score Math (incoming)	1.11	0.01	-0.59
	(0.68)	(0.83)	(0.77)
Normalized SEA Score English (incoming)	1.16	0.06	-0.51
	(0.67)	(0.81)	(0.81)
Female	0.53	0.52	0.55
	(0.50)	(0.50)	(0.50)
Take CSEC	0.90	0.75	0.65
	(0.30)	(0.43)	(0.48)
Exams Taken	6.38	4.43	2.96
	(2.37)	(2.82)	(2.69)
Exams Passed	5.45	2.26	1.03
	(2.61)	(2.43)	(1.73)
English Grade (1=lowest , 7=highest)	5.73	3.68	2.65
	(1.94)	(2.08)	(1.88)
Math Grade (1=lowest , 7=highest)	5.36	3.13	2.36
	(1.98)	(1.88)	(1.59)
Certificate [a]	0.70	0.18	0.05
	(0.46)	(0.38)	(0.22)
Admitted Cohort Size	179.24	389.18	544.75
	(150.87)	(232.58)	(203.32)
Government Assisted School	0.65	0.00	0.00
	(0.48)	(0.00)	(0.00)
Government School	0.35	0.65	0.64
	(0.00)	(0.47)	(0.47)
Observations	5337	10016	16240

Standard deviations are reported below the sample means.
a Certificate denotes passing five CSEC exams including English and math. This is a prerequisite to most tertiary education institutions.

Table 2
Effect on CSEC Taking, the Number of Exams Passed and Earning a Certificate

	1	2	3	4	5	6	7	8	9	10
	OLS	OLS	OLS	OLS	2SLS-D	2SLS-D	2SLS-DID	2SLS-DID	2SLS-Full	2SLS-Full
	Taking the CSEC Exams									
Mean SEA Scores	0.137	0.058	0.05	0.067	-0.11	-0.056	0.008	-0.01	0.016	-0.008
	[0.038]**	[0.040]	[0.043]	[0.047]	[0.051]*	[0.053]	[0.034]	[0.039]	[0.030]	[0.037]
	Number of Exams Passed									
Mean SEA Scores	2.407	1.337	1.504	1.338	1.29	0.748	0.609	0.502	0.673	0.479
	[0.060]**	[0.144]**	[0.152]**	[0.133]**	[0.394]**	[0.352]*	[0.219]**	[0.224]*	[0.219]**	[0.231]*
	Earning a Certificate									
Mean SEA Scores	0.351	0.178	0.207	0.168	0.224	0.159	0.116	0.11	0.135	0.107
	[0.012]**	[0.028]**	[0.025]**	[0.019]**	[0.072]**	[0.066]*	[0.035]**	[0.035]**	[0.035]**	[0.036]**
Controls [a]	Yes	Yes	Yes	Yes	No	No	Yes	Yes	Yes	Yes
SEA Score Controls	None	Quartic	Quartic	Quartic	Quartic	Quartic	Dummies	Dummies	Quartic	Quartic
Preference dummies	No	No	Yes	Yes	No	No	Yes	Yes	Yes	Yes
Cutoff Fixed Effects	-	-	-	-	Yes	Yes	-	-	-	-
Assisted Included	Yes	Yes	Yes	No	Yes	No	Yes	No	Yes	No
Excluded Instrument	-	-	-	-	$above_{is}$	$above_{is}$	$(\overline{\overline{SEA}} \mid Rule_{is})$	$(\overline{\overline{SEA}} \mid Rule_{is})$	$(\overline{\overline{SEA}} \mid Rule_{is})$	$(\overline{\overline{SEA}} \mid Rule_{is})$
Observations	23322	23322	23322	23322	50928	28953	15796	13136	15796	13136

Robust standard errors in brackets. Standard errors adjust for clustering at the attended school level in the OLS models and at the assigned school level in the 2SLS-RD models, the 2SLS-DID and 2SLS-Full models. The discontinuity model uses a bandwidth of 100 points and excludes observations within 3 points of the simulated cut-off. All discontinuity models control for school cut-off fixed effects.

Note: Sample sizes differ across the OLS, 2SLS-DID, and 2SLS-Full models because students with unique preference orderings cannot be included in models that include the full set of preference dummies. The OLS results are very similar when one excludes observations with singleton preferences groups.

+ significant at 10%; * significant at 5%; ** significant at 1%

a. The control variables are student gender, religion, and primary school district.

Table 3
Full Rule-Based 2SLS Results by Gender

	1	2	3	4	5	6
	Males					
	Take	Take	Passes	Passes	Cert.	Cert.
Mean Peer Scores	0.037	0.057	0.378	0.314	0.111	0.084
	[0.059]	[0.066]	[0.244]	[0.229]	[0.037]**	[0.035]*
	Females					
Mean Peer Scores	0.033	-0.028	1.071	0.864	0.201	0.212
	[0.054]	[0.076]	[0.329]**	[0.404]*	[0.057]**	[0.073]**
Assisted included	Yes	No	Yes	No	Yes	No
Observations	8484	6952	8484	6952	8484	6952
Pr(Male=Female) [a]	0.512	0.265	0.002	0.011	0.012	0.015

+ significant at 10%; * significant at 5%; ** significant at 1%.
Robust standard errors in brackets are adjusted for clustering at the assigned school level.
For males, regressions using the full sample have 6165 observation , while those excluding assisted schools have 5053 observations. The corresponding samples sized for females are 8484 and 6952, respectively.
The excluded instrument in these models is $(\overline{\overline{SEA}} \mid Rule_{is})$. All regressions include the quartic of the total SEA score, the quadratic of the math and English SEA scores, student gender, religion, and primary school district.
a. This is the test that an interaction between a female indicator variable and peer test scores is equal to zero in a 2SLS model using both genders where incoming SEA scores are all interacted with gender.

Table 4
Full Rule-Based 2SLS Estimates on Math and English Exam Performance

	1	2	3	4	5	6	7	8
	English Grade							
Mean of Total SEA	0.329	0.211	0.382	0.46	0.467	0.467	0.486	0.603
	[0.182]+	[0.205]	[0.219]+	[0.273]+	[0.144]**	[0.149]**	[0.136]**	[0.166]**
	Math Grade							
Mean of Total SEA	0.161	-0.049	0.115	-0.064	0.287	0.073	0.239	0.122
	[0.133]	[0.137]	[0.184]	[0.209]	[0.137]*	[0.153]	[0.201]	[0.165]
CSEC Takers only?	No	No	Yes	Yes	No	No	Yes	Yes
Polynomial order	4	4	4	4	4	4	4	4
Assisted School included	Yes	No	Yes	No	Yes	No	Yes	No
Propensity Score included?	No	No	Yes	Yes	No	No	Yes	Yes
Observations	15796	13136	11638	9312	15796	13136	11638	9312
Excluded Instrument	$(\overline{\overline{SEA}} \mid Rule_{is})$	$(\overline{\overline{SEA}} \mid Rule_{is})$	$(\overline{\overline{SEA}} \mid Rule_{is})$	$(\overline{\overline{SEA}} \mid Rule_{is})$	$Rule_{is}$	$Rule_{is}$	$Rule_{is}$	$Rule_{is}$

+ significant at 10%; * significant at 5%; ** significant at 1%. Robust standard errors in brackets are adjusted for clustering at the assigned school level. All regressions include the quartic of the total SEA score, the quadratic of the math and English SEA scores, student gender, religion, and primary school district.

Table 5

Effect on English and Math Grades by Gender (Full Rule-Based 2SLS estimates)

	1	2	3	4	5	6	7	8
	Male CSEC takers only				Female CSEC takers only			
	English Grade	English Grade	Math Grade	Math Grade	English Grade	English Grade	Math Grade	Math Grade
Mean of Total SEA	0.245	0.384	-0.207	-0.366	0.416	0.406	0.486	0.521
	[0.228]	[0.227]+	[0.294]	[0.306]	[0.305]	[0.416]	[0.215]*	[0.290]
Mean of Total SEA	0.241	0.363	-0.092	-0.217	0.575	0.585	0.373	0.286
	[0.181]	[0.205]+	[0.194]	[0.216]	[0.168]*	[0.203]*	[0.164]*	[0.199]
Observations	3994	3039	3994	3039	6813	5458	6813	5458
Number of groups	1088	869	1088	869	1667	1411	1667	1411
Propensity score	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Assisted School	Yes	No	Yes	No	Yes	No	Yes	No

+ significant at 10%; * significant at 5%; ** significant at 1%.

Robust standard errors in brackets are adjusted for clustering at the assigned school level. The excluded instrument for the estimates in the top row is the simulated peer achievement based on the simulated school assignment $(\overline{\overline{SEA}} \mid Rule_{is})$, and the excluded instruments for the estimates in the second row are the individual rule-based school assignments $Rule_{is}$. All regressions include the quartic of the total SEA score, the quadratic of the math and English SEA scores, student gender, religion, and primary school district.

Table 6

Effect Within Schools of Different Ranks

	1	2	3	4	5	6	7	8	9
	Take CSEC			Exams Passed			Certificate		
	Top third	Middle third	Bottom third	Top third	Middle third	Bottom third	Top third	Middle third	Bottom third
Full Rule Based 2SLS variables results (assisted schools included)									
Mean of Total SEA	-0.086	0.052	0.204	1.635	1.424	-0.232	0.386	0.249	-0.06
	[0.089]	[0.069]	[0.118]+	[0.715]*	[0.437]**	[0.427]	[0.121]**	[0.076]**	[0.053]
First Stage F-Statistic	3.36	3.61	3.15	3.36	3.61	3.15	3.36	3.61	3.15
2SLS Discontinuity (assisted schools not included)									
Mean of Total SEA	0.039	-0.075	0.087	0.747	0.834	0.28	0.091	0.188	-0.033
	[0.0676]	[0.063]	[0.185]	[0.954]	[0. 516]+	[0.922]	[0.192]	[0.106]+	[0.093]
First Stage F-Statistic	112.5	278.5	50.3	112.5	278.5	50.3	112.5	278.5	50.3

+ significant at 10%; * significant at 5%; ** significant at 1%.

Robust standard errors in brackets are adjusted for clustering at the assigned school level. The excluded instruments in the full rule based 2SLS models are the $Rule_{is}$ variables, and the excluded instrument in the discontinuity model is $above_{is}$.. The full rule based 2SLS regressions include the quartic of the total SEA score, the quadratic of the math and English SEA scores, student gender, religion, and primary school district. The discontinuity models include the quartic of the total SEA score and cut-off fixed effects only. The discontinuity model includes observations within 100 points of the cut-off and excludes observations within three points of the simulated cut-off.

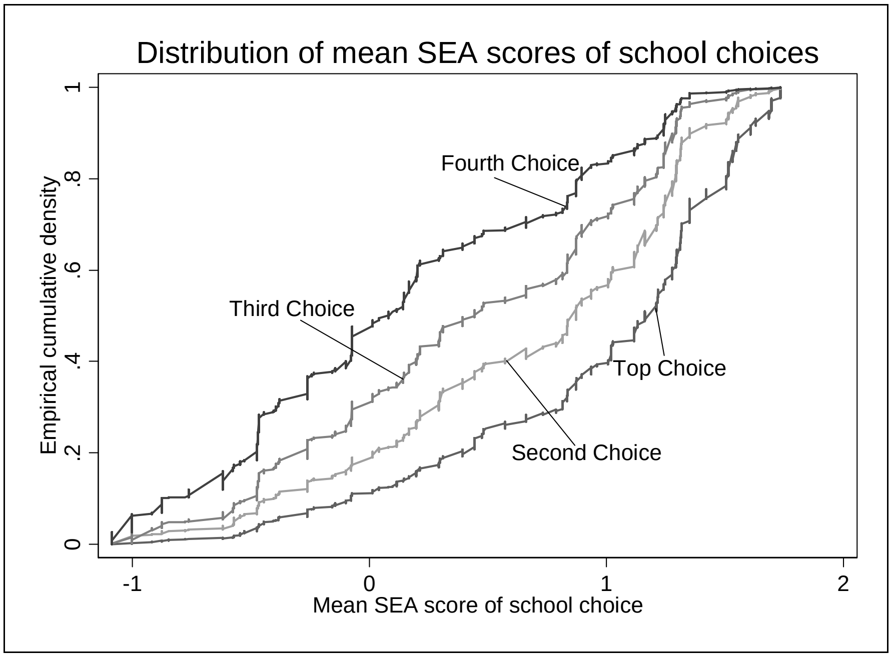


Figure 1

Distribution of Incoming Peer Achievement by School Choice Rank

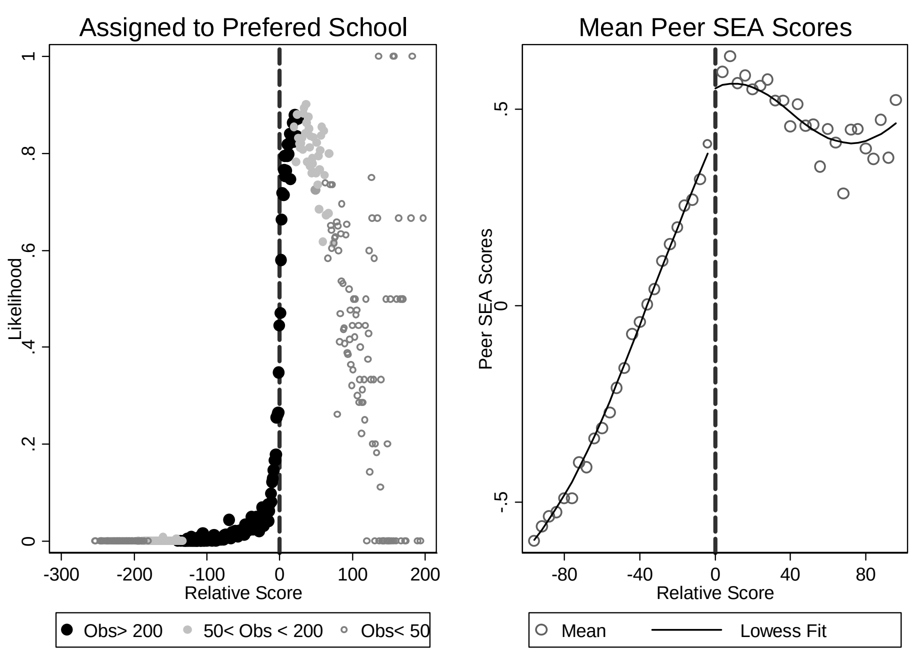


Figure 2
Likelihood of Being Assigned to a Preferred School and Having Higher Achieving Peers

Appendix:

Table A1

Discontinuity based 2SLS results by gender

	1	2	3	4	5	6	
	Take	Take	Passes	Passes	Cert.	Cert.	Obs.
	Male						
Mean Peer Scores	0.025	-0.096	0.72	0.097	0.144	0.0649	12731
	[0.071]	[0.109]	[0.396]+	[0.482]	[0.0789]+	[0.0923]	
	Female						
Mean Peer Scores	0.054	-0.02	1.121	1.133	0.241	0.204	16222
	[0.053]	[0.069]	[0.324]**	[0.444]*	[0. .068]**	[0.0807]*	
Polynomial order of total SEA	2	4	2	4	2	4	

+ significant at 10%; * significant at 5%; ** significant at 1%

Robust standard errors in brackets are adjusted for clustering at the school level. The discontinuity model uses a bandwidth of 100 points and excludes observations within 3 points of the simulated cut-off. All discontinuity models include preferred school cut-off fixed effects and the quartic of the total SEA score.

Table A2

Robustness of Discontinuity Estimates to Bandwidth and Smooth Functions of the Total Score

The Coefficient on Mean Peer SEA scores in Different Discontinuity Models are reported

		Quartic of total SEA			Quadratic of total SEA		
Outcome	Bandwidth	Coef.	se_1	se_2	Coef.	se_1	se_2
Take CSEC	20	-0.582	[0.678]	[0.619]	-0.025	[0.112]	[0.140]
Take CSEC	30	0.033	[0.137]	[0.198]	-0.06	[0.070]	[0.081]
Take CSEC	50	0.051	[0.081]	[0.091]	-0.048	[0.046]	[0.050]
Take CSEC	100	-0.056	[0.053]	[0.049]	0.04	[0.047]	[0.032]
Take CSEC	200	0.059	[0.052]	[0.038]	0.078	[0.054]	[0.028]**
Take CSEC	all	0.076	[0.057]	[0.038]*	0.082	[0.057]	[0.028]**
Exams Passed	20	5.067	[5.224]	[4.530]	0.408	[0.724]	[0.839]
Exams Passed	30	0.65	[1.110]	[1.193]	0.42	[0.496]	[0.473]
Exams Passed	50	0.708	[0.545]	[0.538]	0.718	[0.333]*	[0.286]*
Exams Passed	100	0.749	[0.351]*	[0.274]**	1.05	[0.288]**	[0.178]**
Exams Passed	200	0.821	[0.329]*	[0.211]**	1.018	[0.316]**	[0.158]**
Exams Passed	all	0.788	[0.339]*	[0.212]**	0.996	[0.327]**	[0.159]**
Certificate	20	0.774	[0.826]	[0.721]	0.154	[0.145]	[0.145]
Certificate	30	0.28	[0.231]	[0.211]	0.149	[0.084]+	[0.081]+
Certificate	50	0.176	[0.094]+	[0.091]+	0.162	[0.061]**	[0.048]**
Certificate	100	0.159	[0.066]*	[0.045]**	0.21	[0.062]**	[0.030]**
Certificate	200	0.167	[0.075]*	[0.036]**	0.212	[0.070]**	[0.027]**
Certificate	all	0.156	[0.079]+	[0.036]**	0.211	[0.070]**	[0.027]**

+ significant at 10%; * significant at 5%; ** significant at 1%

Robust standard errors in brackets are adjusted for clustering at the school level and student level for se1 and se2 respectively. All models exclude observations within 3 points of the simulated cut-off. All discontinuity models control for the preferred school cut-off fixed effects.

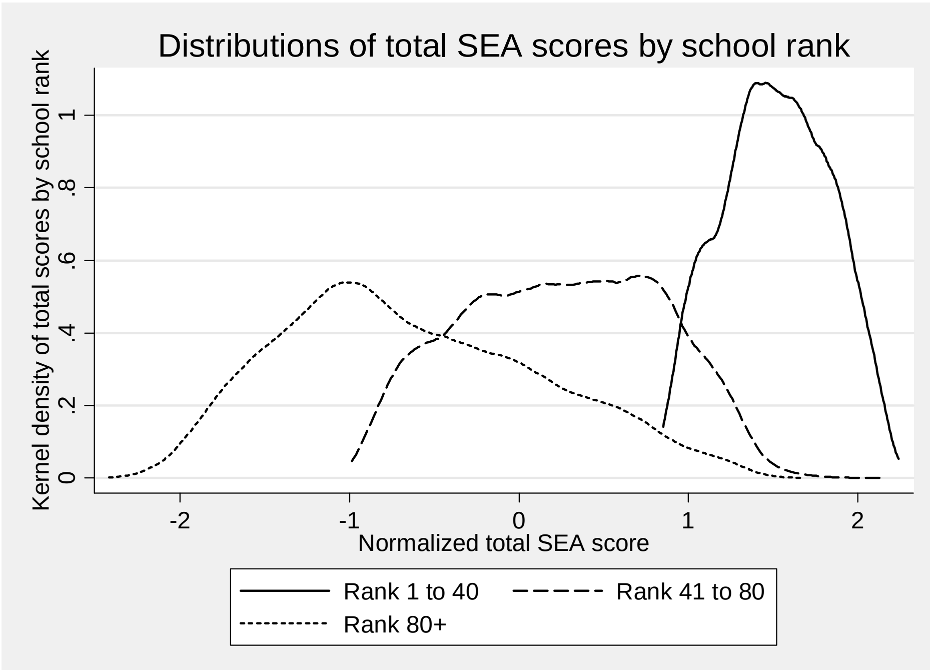


Figure A1
Distribution of Total SEA Scores by School Rank

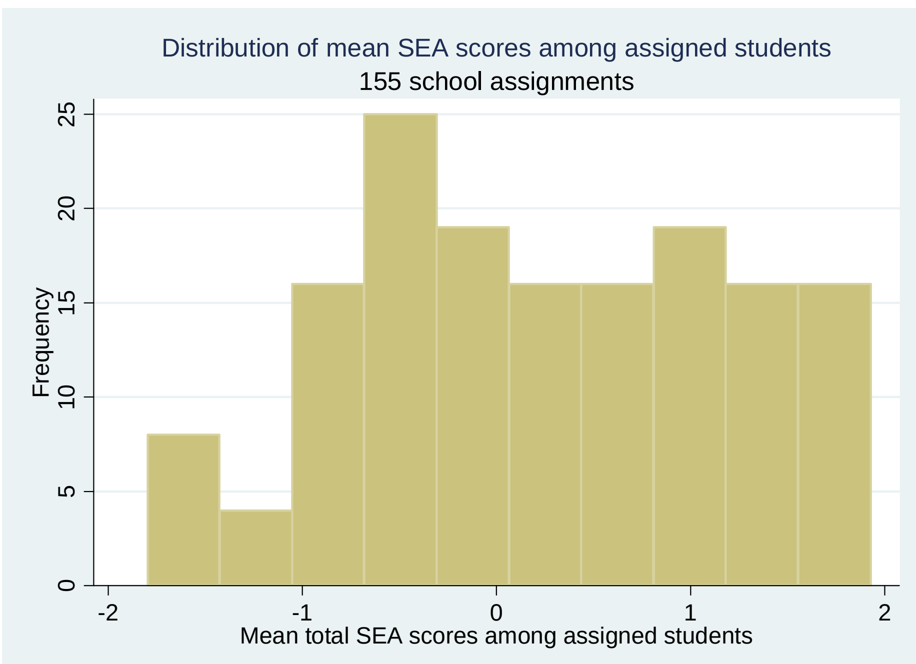


Figure A2
Distribution of Mean SEA Scores Across Actual School Assigents

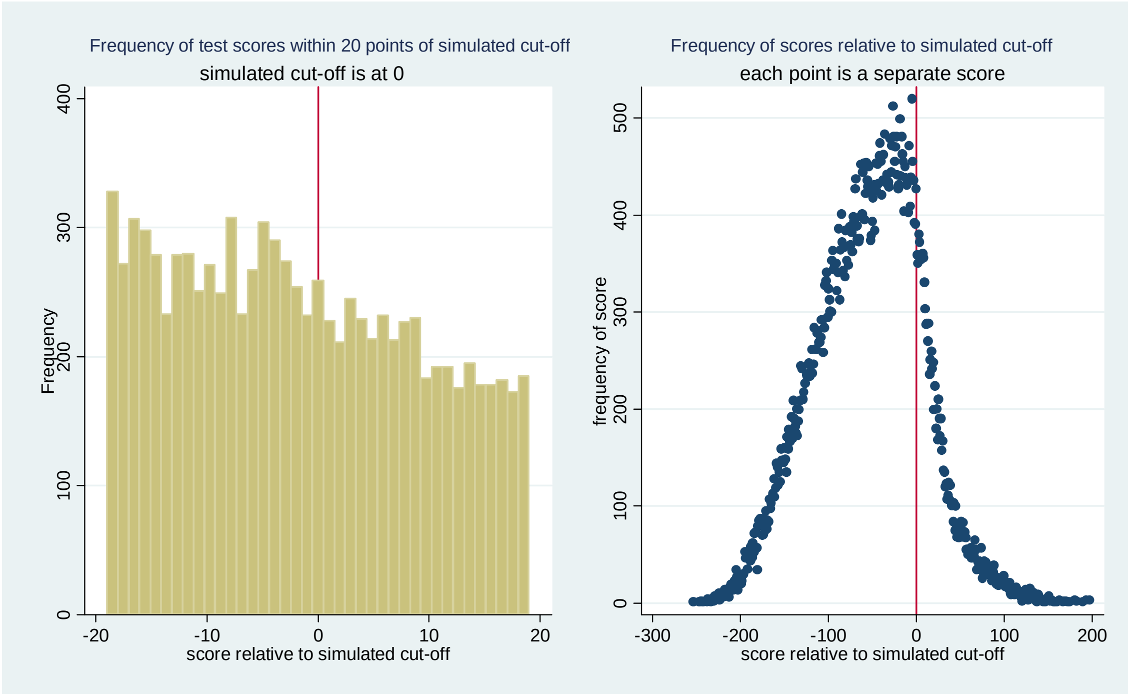


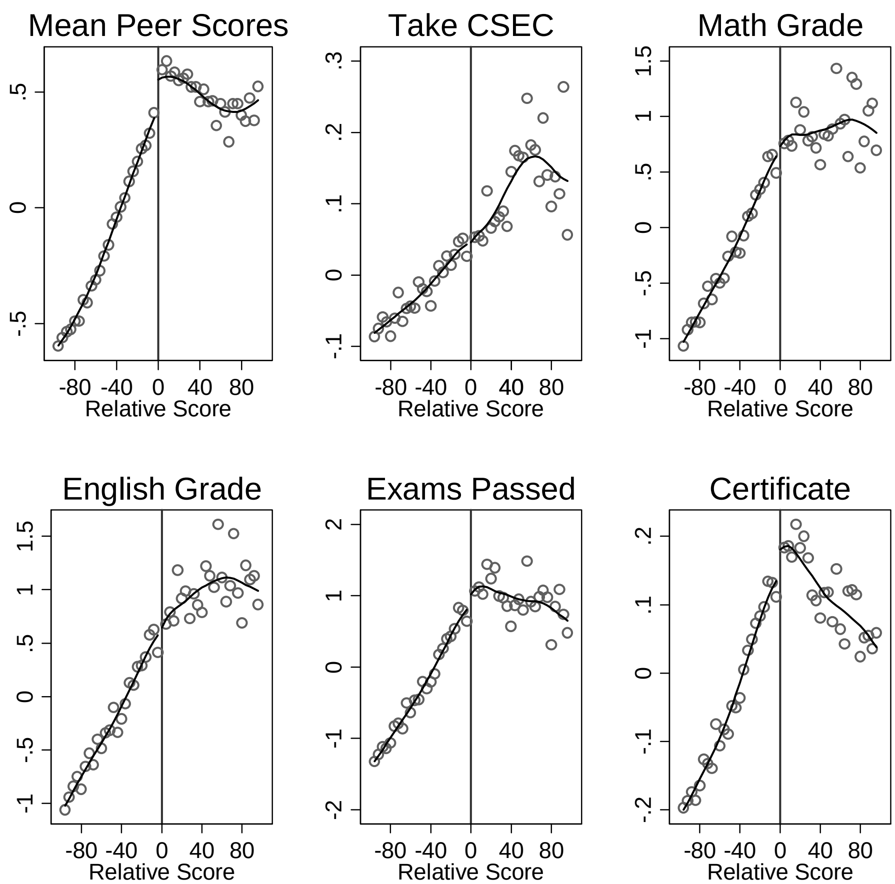
Figure A3
Test for Smoothness Through the Simulated Cut Offs

Appendix Note A1: *Visual Evidence of a Discontinuity in Outcomes.*

To provide visual evidence of a discontinuous shift in outcomes right around the simulated cut-off, I pool all the cut-offs and collapse the data into one cut-off where each student's score is presented relative to the cut-off for the preferred school. In the regression analysis all models include cut-off fixed effects so that comparisons are made between students just above and just below *the same cut-off*. However, *for purely illustrative purposes,* if one were to plot outcomes for each relative score based on the pooled data, one may not compare students just above and just below the same cut-off. For example, if the top score is 650 and the top ranked school has a cut-off score of 630, then there will be no students with a relative score of 50 at the top ranked school. In contrast, a school with a cut-off score of 500 may have several applicants with relative scores of 50 (absolute score of 550). This example illustrates that as one has a higher relative score, one actually is likely to have a lower absolute score if one does not limit one's analysis to students relative to the same cut-off. As such, in a plot of outcomes on the relative score with pooled data, outcomes should decline above the cut-off at higher relative scores (since they represent lower absolute scores). I reiterate that all regression analysis is based on within cut-off comparisons so this only refers to the visual evidence from the pooled sample.

Another important feature to note is what happens below the cut off. Students who just miss the cut-off for a preferred school are likely to be assigned to a less preferred school with similar mean peer quality. As such, since students who miss the cut-off for a preferred school are assigned to less preferred schools, and those with higher SEA scores will be assigned to "better" schools on average, there is a natural positive relationship between one's relative score and mean peer quality below any given cut-off. As such, while the likelihood of attending a preferred school may increase dramatically below and above any given cut-off, the actual difference in peer quality below and above a cut-off, while sharp, will not be as dramatic. In sum, due to (1) a negative correlation between the relative score and the absolute score above a cut-off in the pooled data, and (2) the fact that even below a cut-off there is a positive relationship between peer quality and the relative score, one would expect to see a positive relationship between outcomes and the relative score to the left of a cut-off, and a negative relationship between outcomes and the relative score to the right of a cut-off (in the pooled data).

To provide some visual evidence of a discontinuity, Figure A4 shows the residuals after taking out a fixed effect for each simulated cut-off. Discontinuities are evident in all outcomes except CSEC taking. This is consistent with the regression analysis using both the discontinuity design and the difference-indifference instrumental variables design.



Shows the outcome for each test relative SEA score bin. Bins are delineated in four-point intervals. This figure shows residuals of the outcomes after taking out an intercept for each cut-off. **Note:** The regression models estimated include cut-off fixed effect and are not subject to composition bias. This figure is for illustrative purposes only.

Figure A4
Visual Evidence of the Discontintuity at the Simulated cut-off