Privately Managed Public Secondary Schools and Academic Achievement in Trinidad and Tobago: Evidence from rule-based student assignments

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Abstract[*]

Many nations allow private entities to manage publicly-funded schools and grant them greater flexibility than traditional public schools. However, isolating the causal effect of attending these privately-managed public schools relative to attending traditional public schools is difficult because students who attend privately-managed schools may differ in unobservable ways from those who do not. This paper estimates the causal effect of attending privately-managed public secondary schools in Trinidad and Tobago (assisted schools) relative to traditional public secondary schools on academic outcomes. In Trinidad and Tobago, students are assigned to secondary schools based on an algorithm that created exogenous variation in school attendance -- allowing us to remove self-selection bias. Despite large differences in teacher quality and peer quality across these school types, we find little evidence of any relative benefit in attending an assisted school between the ages of 10 and 15 in terms of dropout rates or examination performance at age 15.

JEL classifications: H4; I2

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1. Introduction

It is common practice worldwide for governments to allow private entities to manage publicly-funded schools. In the United States and Canada these are called charter schools, in Sweden and Norway these are called *friskoler*, in the U.K these are called free schools, and in many nations such as Germany and Trinidad and Tobago most privately managed schools are publicly funded. These privately managed schools operate within the context of the public schooling system but are typically awarded greater flexibility than traditional public schools in personnel decisions, day to day operations, and choosing the curriculum. Despite this widespread practice worldwide, there is a paucity of evidence on the relative effectiveness of these privately managed public schools outside of the U.S. context. We use administrative data from Trinidad and Tobago to investigate the effect of attending a privately managed public secondary school versus a traditional public secondary school on tenth-grade exam performance and on the likelihood of dropping out of high school. We also explore heterogeneous treatment effects by gender and stated preferences for schools.

In principle, privately managed public schools should outperform traditional public schools for two reasons. First, as noted by Chubb and Moe (1990), traditional public schools are often managed by institutions that are heavily influenced by interest groups (such as teachers unions) whose primary goal may not be to improve student outcomes. Second, the flexibility awarded to privately managed public schools allows them to experiment in order to find the optimal mix of pedagogical methods, hiring practices, incentivizing, and teacher training (Finn, et al., 2000). However, the extent to which these privately-managed schools public tend to actually outperform traditional public schools remains an open question. There are clear demonstrations of successful charter schools in the United States (Hoxby and Rockoff, 2005; Hoxby and Murarka, 2009; Dobbie and Fryer, 2011; Abdulkadiroglu et al., 2009; and Angrist et al. 2012). [1] However, the evidence for all charter schools in the U.S. is somewhat mixed (Clark et al. 2011) and there is virtually no credible evidence on the effectiveness of privately managed

[1] Evidence produced exploiting lottery-based admissions to selected charter schools located in different parts of the US like Hoxby and Rockoff (2005) for Chicago; Hoxby and Murarka (2009) and Dobbie and Fryer (2011) for New York City; Abdulkadiroglu et al. (2009) and Angrist et al. (2012) for Boston show positive effects of attending a charter versus a traditional public school on math and reading achievement. However, Clark et al. (2011) studied lottery-based admissions to charter schools within a more generalizable setting, including 36 middle schools in 15 states, and found that charter schools are no more effective than traditional public schools at increasing math and reading test scores.

public schools from outside the U.S.[2] There are important educational interventions that have been found to have very different effects in the U.S. than in other contexts.[3] As such, given that the use of privately managed public school is widespread worldwide, it is important to determine whether the U.S. experience generalizes to other national contexts. This paper fills this important gap by assessing the benefits associated with attending privately managed public secondary schools in Trinidad and Tobago.

There are two types of public secondary schools in Trinidad and Tobago: government schools and government assisted (assisted) schools. All secondary schools provide instruction from 6th through 10th grade, and teach the same national curriculum. However, government schools are fully funded and operated by the government, while assisted schools are run by private bodies (usually a religious board) and all operating expenses except teacher costs are publicly funded. A simple comparison of student outcomes between who attended assisted schools and those who did not is unlikely to isolate the causal effect of attending an assisted school on student outcomes due to self-selection bias. That is, because students who *chose to* attend assisted schools may differ from those who *chose to* attend government schools in important unobserved ways, such comparisons may be subject to biases of unknown magnitude and direction.

To address self-selection bias, we follow Jackson (2010, 2012, and 2013) and take advantage of the fact that attendance at assisted schools is partially beyond students' control: The Trinidad and Tobago Ministry of Education assigns students to secondary schools based on the secondary school entrance exam scores and a list of student choices for preferred schools. We apply the assignment algorithm to form rule-based instrumental variables that predict assisted school attendance, but are not subject to self-selection bias. Under the assignment rules, the likelihood of assignment to an assisted school is a deterministic, nonlinear, non-monotonic, non-smooth function of student choices and incoming test scores. Specifically, (a) conditional on two students having the same test score, differences in school assignments are due to their different choices, and (b) conditional on two students having the same choices, differences in school assignments are due to small differences in their test scores. This allows for both a regression

[2] One notable exception is an unpublished working paper. Bonilla (2011) uses data from Bogota, Colombia, and finds large positive academic effects of attending a privately managed versus a traditional public school, equivalent to 0.6 and 0.2 standard deviations higher in math and verbal tests, respectively.

[3] For example, performance pay has been found to be very effective outside of the United States, but relatively ineffective in the United States (Jackson et. al. 2015).

discontinuity strategy and a difference-in-differences (DID) instrumental variables (IV) strategy that identifies the causal relationship off the interaction between student choices and test scores. We show that each strategy independently yields similar results. As such, our preferred IV strategy exploits both sources of exogenous variation. Our key outcome is performance on a high-stakes examination accepted as an entry qualification for higher education across the Caribbean, Canada, the United Kingdom, and the United States. We also examine effects on high-school dropout. While naive ordinary least squares (OLS) yield large treatment effects, all three IV strategies to account for selection show overall null effects of gaining admission to an assisted secondary school over a traditional government secondary school. There were also no differential effects between females and males. We also use data on the number of assisted schools that students list in their school choices to test if this overall null results masks some important heterogeneous treatment effects. We use this to determine if the treatment effect varies with preferences for assisted schools, and to determine if the treatment effect for those who typically apply to assisted schools differs from that of the average student. Consistent with a real null result, we find no systematic treatment differences between students with weak or strong preferences for assisted schools. We can rule out modest effect sizes so that our analysis provides little evidence that privately managed secondary schools create better educational outcomes than traditional public schools in Trinidad and Tobago. This is despite large improvements in peer quality and teacher quality from attending assisted schools.

Our lack of an effect stands in contrast to some of the large positive effects of attending charter schools documented in the United States. This suggests that the positive effect privately-managed public secondary schools (charter schools) sometimes documented in the United States may not generalize to other settings. We speculate on why this may be the case and note that a key difference between charter schools and privately managed public schools in other nations is that in the U.S. charter schools are subject to heightened accountability (Angrist et al., 2011). This is important because it suggests that further work should be done to better understand whether granting publicly funded schools greater flexibility in the areas of personnel and curriculum tends to lead to better student outcomes in most national contexts.

The remainder of the paper is organized as follows: Section 2 describes the Trinidad and Tobago education system, the assignment mechanism, and the data. Section 3 describes the empirical strategy. Section 4 presents the results and Section 5 concludes.

2. Trinidad and Tobago's Education System and the Data

At the end of primary school (after grade 5), students take the Secondary Entrance Assessment (SEA) and are assigned to secondary schools. The assignments are made by the Ministry of Education (MOE) based on students' SEA scores and students' top four school preferences.[4] SEA scores will serve as our measure of students' incoming preparedness, and student choices will serve as our proxy of student preferences for secondary schools at the end of primary school. We exploit the exogenous variation in school attendance caused by this MOE assignment algorithm to uncover the causal effects of attending an assisted versus a traditional government secondary school. The assignment mechanism is a student-proposing deferred acceptance algorithm similar to that studied in Pathak (2011). We detail the assignment mechanism further in Section 3.

Secondary school begins in first form (grade 6) and ends at fifth form (grade 10). All secondary schools teach the same national curriculum and at the end of fifth form, students take the Caribbean Secondary Education Certificate (CSEC) examinations. The CSEC exams are given in 31 subjects and are externally graded by the Caribbean Examinations Council. Students who pass five or more subjects including English language and mathematics meet the requirements for secondary school graduation, earning a CSEC certificate.[5] Student performance on the CSEC exams will serve as our main achievement outcome. Because students can legally drop out of school at age 14 (typically during grade 8 or 9), and all students who attend school by grade 10 take the CSEC exams, we use not taking the CSEC exams as our measure of high-school dropout.

There are eight public school districts and private schools serve a very small share of students (roughly 2 percent).[6] Our analyses focus on students who attended public secondary schools. There are two types of public secondary schools: government schools and government assisted (assisted) schools. Government schools are fully funded and operated by the

[4] Cohorts who took the SEA between 2002 and 2006 were allowed to list up to six school choices. However, before 2002 and from 2007 onwards, students were only allowed to list up to four school choices.

[5] CSEC examinations are accepted as an entry qualification for higher education in the Caribbean, Canada, the United Kingdom, and the United States. Students may continue to take the Caribbean Advanced Proficiency Examinations (CAPE) at the end of grade 12, which is a prerequisite for more selective colleges and universities in most nations.

[6] Private schools tend to serve those who fall through the cracks in the public system. Indeed, for the cohorts who took the SEA between 2002 and 2009, only 3.42 percent were enrolled in private secondary schools, and their SEA scores were 0.5 standard deviations below the average public student's score.

government, while assisted schools are run by private bodies (usually a religious board) and all operating expenses except teacher costs are publicly funded.

2.1 Assisted Schools in Trinidad and Tobago

There were 137 public secondary schools between 2005 and 2014. Among these, 44 assisted schools were spread across Trinidad and Tobago's eight school districts.[7] Trinidad is sufficiently small (about 37 by 50 miles) that an assisted school is located within about 20 miles of any location.

Table 1 shows several characteristics and educational inputs by school type for the 2005–2006 academic year. Given that assisted schools (much like charter schools) are granted greater flexibility in personnel decision one might expect these schools to have teacher workforces that differ from traditional schools. Indeed, this is the case. Assisted schools tend to have teacher with higher degrees than traditional public schools but also tend to have teacher with fewer years of experience. While 74 percent of teachers at assisted schools possess a Bachelor of Arts degree, only 43 percent do at government schools. Similarly, while six percent of teachers at assisted schools possess a Master of Arts degree, only two percent do at government schools. However, government school teachers have more years of teaching experience, with an average of 14.07 years compared to 10.98 years for assisted schools. In other datasets, years of experience is associated with improved student outcomes while higher degrees are not (Rowan et al., 1997; Rowan et al., 2002). As such, the simple comparisons suggest that teachers at assisted schools have less productive observable characteristics than those at traditional public school (despite being much more highly educated).

In terms of school size as measured by the number of teachers, number of academic teachers, school enrollment, and grade 6 enrollment, government schools are larger than assisted schools. The student-to-faculty ratio is higher at assisted schools (17.32 versus 13.82 at government schools). This is because government schools hire more guidance officers, assistant teachers, and vocational teachers. However, when focusing on classroom teachers for academic courses, the student-to-teacher ratio is similar at assisted and government schools (25.17 at assisted versus 26.74 at government schools, with the difference being statistically indistinguishable from zero). Assisted schools are much more likely to be single-sex school (77

[7] These include seven of 17 public secondary schools in Caroni, one of 11 in the North Eastern district, nine of 23 in Port of Spain, two of 15 in the South Eastern district, eight of 28 in St. George East, six of 15 in St. Patrick, three of 9 in Tobago, and eight of 19 in Victoria.

percent of assisted schools are single-sex, while only three percent of government schools is single-sex).

Incoming peer quality significantly differs across school types. The average student attending an assisted school has incoming test scores that are 1.08 standard deviations higher than those of students at government schools. To show the distribution of incoming peer achievement across school types, figure 1 displays the peer achievement across all schools between years 2002 and 2009 into ten equally spaced bins. The figure plots the number of assisted and government schools that fall into each bin. The unit of observation is a school year. While there is overlap in the distribution of incoming peer achievement between assisted and government schools, the highest achieving peers disproportionally attend assisted schools. This fact provides *de-facto* evidence that students and parents perceive assisted schools as being better than traditional schools. However, as we show in Section 4, this widely held perception may be false.

2.2 Data and Summary Statistics

Our analytic sample is the population of SEA takers between 2002 and 2009. We employ the official SEA testing data (grade 5) for these cohorts. The SEA data contain the SEA test scores of each of the nation's students, their list of preferred secondary schools, their gender, age, religion, primary school district, and the secondary school to which they were assigned by the Ministry of Education.[8] These SEA data are linked to the official 2007 through 2014 CSEC examination data (grade 10).[9] The CSEC data contain each student's exam grades and secondary school attended. For those who did not take the exam (i.e. dropouts), we use the official school assignment from the Ministry of Education. We determine whether a student took the CSEC exams, and compute the number of examinations taken and passed. Taking a subject is defined as taking a CSEC exam in the subject. We exclude students who attended private secondary schools. The resulting dataset contains 142,376 students across eight cohorts and 137 schools.

Table 2 summarizes the data. Students assigned to assisted schools have incoming SEA scores that are 1.19 standard deviations higher than those assigned to government schools. Given that incoming test scores after grade 5 is a very strong predictor for outcomes in grade 10, as one

[8] The SEA exam is composed of math, English, science, social studies, and essay elements.

[9] We link the SEA data with the corresponding CSEC data from four, five, six, seven, and eight years later. We were able to link roughly two-thirds of SEA takers to CSEC exam data. Students were matched based on name, gender, and date of birth. The match rate was 72.8 percent, consistent with the national dropout rate. Students with missing CSEC data are included in all regressions and coded as having zero passes.

would expect, average outcomes are also better at assisted schools. About 83 percent of students assigned to assisted schools remain in secondary school to take the CSEC exams five years later compared to 66 percent at government schools. Students assigned to assisted schools pass, on average, 5.15 CSEC exams, compared to only 2.09 at government schools. An important academic outcome is earning a certificate (passing five exams including math and English) because it is the key prerequisite to tertiary education. About 61 percent of students assigned to assisted schools earn a certificate compared to only 0.17 for students assigned to government schools. The better student outcomes, coupled with these schools having much more highly educated teachers, helps reinforce the common perception that assisted schools are better than traditional schools.

A key conditioning variable in our analysis is the students' school choices. As we detail in Section 3, this variable is used in the assignment algorithm that we exploit for identification. However, it also serves as a powerful proxy for student and parent preferences that is often difficult to observe. These choices are based largely on students' perceived ability as well as their geography and religion. Higher achieving students tend to have better achieving schools on their list; students often request schools matching their religious affiliation and that are close to home. Also, students tend to place schools with higher achieving peers higher up on their preference ranking (Jackson, 2010). On average, the difference between the mean incoming scores at a student's top choice school and their second, third, and fourth choice school is 0.224, 0.415, and 0.634 standard deviations, respectively.[10] About 86 percent of students have an assisted school as one of their secondary school choices, and those schools tend to be higher up on their lists. Specifically, assisted schools are the top choice of 68 percent of students, the second choice of 49 percent, the third choice of 35 percent, and the fourth choice of 25 percent.[11]

3. Econometric Framework

3.1 Identification Strategy

[10] For the 2002–2006 cohorts who were allowed to list up to six choices, the difference between the mean incoming SEA scores at a student's top choice school and their second-, third-, fourth-, fifth-, and sixth-choice school is 0.194, 0.347, 0.511, 0.681, and 0.955 standard deviations, respectively. For the 2007–2009 cohorts who were allowed to list up to four choices, and the difference between the mean incoming SEA scores at a student's top choice school and their second-, third-, and fourth-choice school is 0.285, 0.548, and 0.873 standard deviations, respectively.

[11] For the 2002–2006 cohorts who were allowed to list up to six choices, 20 percent list an assisted school as their fifth choice, and 13 percent listed an assisted school as their sixth choice.

In this section we describe how we aim to identify the effect of attending an assisted school. To do this, we compare the outcomes of similar students who attend different schools. For the baseline specification, we model the outcome of student *i* at school *j* with the following equation.

$$Y_{ij} = f(SEA_i) + assisted_{ij} \cdot \beta + X_i\gamma + \sum_{c=1} I_{ic} \cdot \theta_c + \varepsilon_{ij} \quad (1)$$

In (1), $assisted_{ij}$ is an indicator variable equal to 1 if the student attends an assisted school and equal to 0 otherwise, SEA_i is a matrix of incoming test scores, X_i includes demographic controls such as student gender and a set of primary school district fixed effects, I_{ic} is an indicator variable denoting the school choice list of student *i* (an indicator variable identifying each unique list of school choices),[12] and ε_{ij} is the idiosyncratic error term. The parameter β captures the expected difference in outcomes between students who attended assisted schools and students who attended traditional government schools. While including individual SEA scores and school choices should remove a large amount of self-selection bias, OLS estimates of β may suffer from bias if students can select schools based on *unobserved* dimensions. In the following sections we detail how students are assigned to schools, explain why there may be selection to assisted schools, and detail how we use the assignment rules to remove selection bias and identify the causal effect of attending an assisted school relative to a traditional government school.

3.2 Student Assignment Rules

Students in Trinidad and Tobago compete for a limited number of places at preferred secondary schools. The Ministry of Education assigns students to schools using an algorithm that incorporates SEA scores and students' school choices. More than 94 percent of students are placed through a student-proposed deferred acceptance algorithm (Gale and Shapley, 1962). Under this mechanism, truth-telling for students in school preferences constitutes a dominant strategy (Dubins and Freedman, 1981; Roth, 1982). Given this desirable property, this kind of assignment algorithm is used to assign students to schools in many cities in the U.S., as well as many nations in Europe, Asia, Africa, Latin America, and the West Indies (Abdulkadiroglu et. al. 2015).

[12] Each choice group is defined by a distinct ordering of schools. Students who list schools A, B, C, D, E, F in that order form a group, while students who list schools B, A, C, D, F, E form a different group because even though they have the same schools, the ordering is different. Also, since we are pooling different cohorts, groups are differentiated by cohort. For example for cohort 2002, students who list schools A, B, C, D, E, F form a different group than students from the 2003 cohort who list the same schools in the same order.

This process in Trinidad and Tobago involves six steps. First, the number of school slots at each school n_j is predetermined based on capacity constraints. Second, students are tentatively placed in the applicant pools for their first-choice schools and are ranked in descending order by SEA score within each application pool. Third, the school at which the n_j^{th} ranked applicant has the highest SEA score is determined to be the most highly subscribed/ranked school, this score becomes the cutoff score for this school, and the top n_{j1} students in the applicant pool for top-ranked school j_1 are admitted to school j_1. Fourth, the top-ranked school's slots and the admitted students are removed from the process, and the second choice becomes the new first choice for students who had the top-ranked school as their first choice but did not gain admission. Fifth, the process repeats in another round to assign students to the second highest ranked school j_2 and determine the cutoff score for the second-ranked school. Six, the process repeats in subsequent rounds until all slots are filled.[13] Abdulkadiroglu et. al. (2014) point out that this mechanism creates a test score cut-off for each school such that applicants to that school with scores just above the cut-off are admitted while those with scores just below are not admitted.

There is an important exception to this rule that we are careful to account for in our identification strategy. Specifically, assisted schools *can* admit up to 20% of their incoming class at the principal's discretion. As such, the rule is used to assign at least 80% of the students at these schools, while the remaining student *can be* hand-picked by the principal before the next-highest ranked school fills any of its slots. For example, suppose the highest ranked school has 100 slots and is an assisted school. The top 80 applicants to that school will be assigned to that school while the principal can 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. 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% and the hand-picked 20%) 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 the described deterministic function of student test scores and student choices and partly on the endogenous selection of students by school principals. Below, we describe how we address

[13] In some cases, students may not have scores high enough to be assigned to a school in their list of choices. In this case, students receive an administrative assignment. This administrative assignment will be the closest secondary school to the students primary school with an open slot.

any potential endogeneity in the secondary school assignment that is introduced by this "handpick" exception.

3.3 Simulating the Student Assignments Using the Rules

Fortunately, because the assignment algorithm is known and we have the same data used by the Ministry of Education to assign students, we can simulate where the cutoffs *would have been* (and therefore the school's students would have been assigned to) if assisted schools could not select any of their own students.[14] This simulated assignment removes the part of the actual assignment that may be driven by endogenous selection and leaves only the variation in the assignments that are known deterministic functions of student test scores and student choices.

To show the validity of the simulation, we estimate the likelihood of assignment to a preferred school as a function of one's score relative to the simulated cutoff for that school. To combine all the various cutoffs into one, we stack the applicant data for all the cutoffs and re-center the SEA scores for applicants each school around the simulated cutoff for that school. [15] Scoring above zero means scoring above the cutoff for a preferred school. Figure 2 shows the relationship between being *actually assigned* to one's preferred school as a function of one's incoming test score relative to the *simulated* cutoff for that school. Consistent with our simulated assignments capturing real exogenous variation in actual assignments, there is a sudden increase in the likelihood of being assigned to a preferred school as one's score goes from below to above the simulated cutoff. This shows that there are meaningful differences in schooling environments associated with scoring above versus below a simulated cutoff that are not due to selection or handpicking. The fact that the assignment rules create exogenous cutoffs that are well approximated by the simulated cutoffs plays a central role in our identification strategy.

3.3.1 Exogenous Variation Due to Simulated Test Score Cutoffs

Following Jackson (2010), we exploit only the *exogenous* variation in school attendance driven by the assignment algorithm (and exclude the variation driven by student selection or handpicking by principals). To achieve this, we use the simulated cutoffs and the resulting

[14] The only difference between how students are actually assigned and the simulated rule-based assignment is that at the third step of the student assignment process, the tweaked rule does not allow any students to be handpicked.

[15] Specifically, for each school we find all students who list that school as their top choice, re-center those students' scores around the cutoff for that school, and create a sample of applicants for each school. To mimic the sequential nature of the algorithm, we remove students assigned to their top choice schools, replace students' first choice with their second choice, and repeat this process with their second, third, fourth, fifth, and sixth choices. The applicant samples for all schools are then stacked so that every student has one observation for each school for which she/he was an applicant. We use four or six choices, as relevant per cohort limit.

simulated school assignments that would have prevailed if assisted schools could not select students. For each school student pair, we define $Rule_{ij}$ that is equal to 1 if student i would have been assigned to school j had there been no student selection or handpicking and 0 otherwise.

Under the simulated assignments, the only reason two students with the same set of school choices are assigned to different schools would be because of differences in their tests scores—students above the cutoff are assigned to one school and those below it to another. Accordingly, one source of exogenous variation comes from comparing the outcomes of students assigned to different schools (one of which is an assisted school) who score just above and just below a cutoff. This is amenable to a fuzzy regression discontinuity design. Among students who chose an assisted school, the likelihood of being assigned to (and attending) an assisted school increases in a sudden and discontinuous manner as one's score goes from below to above the cutoff. If the location of the cutoffs are orthogonal to student characteristics, and the effect of incoming test scores on outcomes are smooth through the cutoffs, any sudden jumps in the outcomes as one's score goes from below to above the cutoffs can be attributed to the sudden increased likelihood of attending one's preferred assisted school.

To isolate the discontinuity based variation, we implement something similar to a fuzzy regression discontinuity design. Using the stacked dataset described in section 3.3, we create a subsample of cutoffs for preferred assisted schools. Using this subsample, as shown in figure 3, there is a rapid increase in the likelihood of attending a preferred assisted school through the simulated cutoff. The figure also shows that the increase in likelihood is somewhat smooth—suggesting that results using variation through the cutoffs alone may be sensitive to how one controls for smoothness through the cutoffs. Figure 3 also shows suggestive visual evidence of null discontinuities through two of the outcomes of interest (number of exams passed and earning a certificate). Due to the noisiness of this procedure, this is not the preferred source of variation. However, it is worthwhile to see what a discontinuity-type design might yield and see if the results are similar to those obtained using other sources of variation.

Using the stacked dataset, we use scoring above the simulated cutoff for a preferred assisted school as an instrument for attending a preferred assisted school. Specifically, we estimate model (2) by two-stage least squares (2SLS).

$$assisted_{ij} = f_1(SEA_i) + Above_{ij} \cdot \beta_1 + v_{j1} + \varepsilon_{ij1} \tag{2}$$

$$Y_{ij} = f_2(SEA_i) + \widehat{assisted}_{ij} \cdot \beta_2 + v_{j2} + \varepsilon_{ij2}$$

All variables are defined as before, $Above_{ij}$ is an indicator variable equal to 1 if student i has a SEA score above the simulated cutoff for assisted school j and 0 otherwise, and v_j is a fixed effect for each cutoff (preferred school by cohort) to account for students in the admission pool for the top assisted school potentially having different characteristics from those in the applicant pool for a less-selective assisted school. We present results using a second-, third-, fourth-, and fifth-order polynomial in the SEA score. The first-stage F-statistics are all above 60, and standard errors are clustered at the cutoff level.

3.3.2 Exogenous Variation Due to Interaction Between School Choices and Test Scores

As pointed out in Abdulkadiroglu et. al. (2015), the discontinuity variation described above (while easy to exploit) uses only a fraction of the exogenous variation created by these assignment mechanisms. A second source of exogenous variation is due to the fact that different assisted schools have different cutoffs. This variation is best illustrated with a simple example, as in figure 4. Consider a world with two assisted schools, 1 and 2, and one government school, 3. There are two choice groups; both groups list the same government school 3 as the second choice, but choice group 1 lists assisted school 1 as the top choice and choice group 2 lists assisted school 2 as the top choice. The test score cut-off for assisted school 1 is 82 and that for assisted school 2 is 92. We can put all students into one of three test score groups: group A, with scores of 82 and below; group B, with scores between 83 and 92; and group C, with scores of 93 and above.

Students in group A are never admitted to an assisted school, regardless of their choice group. Similarly, students in test score group C are all admitted to an assisted school. However, those in test score group B who are in choice group 1 are admitted to an assisted school while those in choice group 2 are not admitted to an assisted school. Therefore, if the choice-group effect is additively separable from that of test scores, we can use a DID approach to identify the effect of attending an assisted school. Specifically, because the difference in choices does not lead to a difference in assisted school attendance within test score groups A and C, the difference in outcomes between choice groups 1 and 2 within test score groups A and C cannot be due to differences in test scores or differences in assisted school attendance and must therefore be due to differences in choices. However, because the difference in choices leads to differences in assisted school attendance within test score range B, the difference in outcomes between choice groups 1 and 2 within test score group B reflects both differences in assisted school attendance

and differences in choices. As long as the effect of choices is the same across all test score levels, the difference in outcomes between choice groups 1 and 2 within test score group B (assisted effect + choice group effect), minus the difference in outcomes between choice groups 1 and 2 within test score groups A or C (choice group effect), reflects the effect of attending an assisted school. This identification strategy is analogous to that employed in Tyler et al. (2000) to estimate the labor market effects of a General Educational Development (GED) certificate.[16]

To capture only the DID variation obtained by looking at the difference in outcomes for students with the same test scores who attend different schools because of differences in their chosen schools' cutoff points, we use a 2SLS-DID. To isolate the DID variation we control for a full set of choice-group indicator variables and a full set of incoming test score indicator variables (i.e., one indicator variable for each distinct total SEA score for each test year). [17] Note that this model does not rely on the smoothness of outcomes through the test score cutoffs.

$$assisted_i = \left(\widetilde{assisted}|Rule_{ij}\right) \cdot \beta_1 + \sum_{t=1} I_{SEA_i=t} \cdot \delta_{1t} + \sum_{c=1} I_{ic} \cdot \theta_{1c} + X_i\gamma_1 + \varepsilon_{ij1} \quad (3)$$

$$Y_{ij} = \widehat{assisted}_i \cdot \beta_2 + \sum_{t=1} I_{SEA_i=t} \cdot \delta_{2t} + \sum_{c=1} I_{ic} \cdot \theta_{2c} + X_i\gamma_2 + \varepsilon_{ij2}$$

In the first stage of (3), $assisted_i$ denotes whether student i attended an assisted school, X_i includes demographic controls such as student sex and a set of primary school district fixed effects, I_{ic} is an indicator variable equal to 1 if a student's rank ordering is choice group c and equal to zero otherwise, $I_{SEAi=t}$ is an indicator variable equal to 1 if the student's SEA score is equal to t. We remove the potential endogeneity of the actual school assignment by instrumenting for the actual school attended with $\left(\widetilde{assisted}|Rule_{ij}\right)$, an indicator that denotes whether student *i's* simulated school assignment is assisted. The first stage F-statistic on the excluded instrument is above 60. Standard errors are clustered at the simulated school level.

3.3.3 Rule-Based Instrument Using All Exogenous Variation

To simultaneously exploit both sources of plausibly exogenous variation, we use a 2SLS strategy that estimates the effect of attending an assisted school after controlling for a full set of choice indicator variables but using smooth functions of incoming SEA tests scores (i.e., controlling for the underlying test scores that generate variation in school assignments in a smooth manner). We

[16] They exploit variation across states in the test score needed to pass the GED exam. They compare the differences between students with slightly different test scores from the same state (such that some scores just above and others scores just below the GED test score cutoff) to differences between students with the same test scores in other states who all passed the exam (because they both score above the lower test score cutoff in this other state).

[17] As in equation 1, each choice group is defined by a distinct ordering of schools.

instrument for assisted school attendance with an indicator variable denoting whether the simulated school is assisted. Specifically, we estimate the following system of equations by 2SLS where all variables are defined as in (3), but instead of indicator variables for each test score in each year, we control for a fifth-order polynomial in the student's total SEA score, $f(SEA_i)$.[18] The first stage F-statistics are all above 60, and standard errors are clustered at the simulated school level.

$$assisted_i = \left(\widetilde{assisted}|Rule_{ij}\right) \cdot \beta_1 + f_1(SEA_i) + \sum_{c=1} I_{ic} \cdot \theta_{1c} + X_i\gamma_1 + \varepsilon_{ij1} \qquad (4)$$

$$Y_{ij} = \widehat{assisted}_i \cdot \beta_2 + f_2(SEA_i) + \sum_{c=1} I_{ic} \cdot \theta_{2c} + X_i\gamma_2 + \varepsilon_{ij2}$$

3.4 Isolating the Assisted School Effect

The assignment mechanism is such that students with higher school entrance exam scores are more likely to be assigned to their more-preferred schools. Because assisted schools are often preferred, attending an assisted school is associated with attending a preferred school. The estimated coefficients on $\widehat{assisted}_{ij}$ from (2), (3), and (4) provide a selection-free estimate of the effect of attending a preferred assisted school for students applying to assisted schools. However, these coefficients may not isolate an assisted school effect for three main reasons.

First, the majority of assisted schools are also single-sex schools, so this comparison of assisted versus government schools could be confounded with the potential effect of attending a single-sex school.[19] Second, because of the nature of the assignment mechanism, students are more likely to attend an assisted school when they gain admission to a preferred school. Given that students' motivation and effort may be affected if they are not able to attend their top-choice school, an independent effect may result, which could lead to changes in parental inputs such as extra tutoring (Pop-Eleches and Urquiola, 2013). Therefore, part of the effect of attending a preferred assisted school may be driven by the psychological or behavioral effects associated with attending a preferred school. Third, attending an assisted school is associated with greater school selectivity (higher average incoming SEA scores at the school). Because Jackson (2010, 2013) documents that attending a school with higher achieving peers improves academic outcomes, our comparison of assisted versus government schools could be confounded with a school-selectivity effect.

[18] All results are robust to using a second-, third-, fourth-, or fifth-order polynomial.

[19] For an evaluation on the academic effects of attending a single-sex school versus a coed school, see Jackson (2012). His findings suggest no average academic benefits of attending a single-sex school in Trinidad and Tobago.

Fortunately, because the assignment mechanism generates hundreds of cutoffs (a cutoff for each of secondary school in each year), we can exploit the rich variation across cutoffs to remove these confounding factors. Specifically, because some cutoffs create exogenous variation to assisted schools but not single-sex schools, while others create exogenous variation to single-sex schools but not assisted schools, the causal effect of attending an assisted school can be isolated from that of attending a single-sex school. Similarly, we can leverage the fact that some cutoffs do not entail being admitted to an assisted school, to isolate the effect of being admitted to an assisted school from that of scoring above a cutoff for any preferred school. Finally, because some cutoffs are associated large increases in peer quality while others are not, we can isolate the effect of being admitted to an assisted school from that of being admitted to a school with higher achieving peers. By exploiting variation across cutoffs in school type and peer quality, we can remove the effects of attending an assisted school from those of attending a single-sex school, the effects of attending a preferred school (irrespective of type), and the effects of attending a school with higher achieving peers (irrespective of type).

We account for all these potentially confounding factors by augmenting equation (4) to include the following; (a) an indicator for whether a student attends a single-sex school, (b) indicators for whether a student attends their first-, second-, third-, fourth-, fifth-, or sixth-choice school, and (c) average incoming test scores of peers at the school. To account for selection, we instrument for attending a single-sex school with an indicator denoting whether the student was assigned to a single-sex school based on the simulation. We instrument for whether a student attends their first-, second-, third-, fourth-, fifth-, or sixth-choice school with whether a student was assigned to their first-, second-, third-, fourth-, fifth-, or sixth-choice school based on the simulation. Finally, we instrument for average incoming test scores of peers at the actual school with average incoming test scores of peers at the simulated assigned school.

The interpretation of the coefficient on $\widehat{assisted}_{ij}$ with these additional covariates would be the effect of attending a preferred assisted school beyond the effect of attending a single-sex school, a preferred school (of any type), or a school with higher achieving peers. Arguably, this is the policy parameter of interest as this is what policy-makers would like to know when considering allowing private entities to manage publicly-funded schools and granting them greater flexibility.

4. Results and Discussion

4.1 Naive Estimates of the Effect of Attending a Preferred Assisted School

To illustrate the importance of addressing student selection in observed and unobserved dimensions, we present naive estimates of the effects of attending an assisted school before showing how the results change as we account for selection. Table 3 presents the coefficient on attending an assisted school on the main academic outcomes analyzed. We include incoming mean peer quality (average incoming SEA scores at the school) as an outcome to give a sense of how much more selective assisted schools are relative to government schools.

The naive OLS results indicate that incoming peer achievement is 1.13 standard deviations higher, on average, for students who attend assisted schools than those who do not, and outcomes are much better (panel A). Conditional on SEA scores, demographic controls, and choice fixed effects (panel C) using model (1), the magnitude of the estimated coefficients are lower, but significant differences in school selectivity and outcomes remain. After accounting for selection on observables (i.e. conditional on several key observable characteristics), students at assisted schools are exposed to peers with 0.405 standard deviations higher incoming test scores, are 5.6 percentage points more likely to take the CSEC exams and take and pass about one more exam, 8.5 percentage points more likely to pass their CSEC English exam, 9.5 percentage points more likely to pass their CSEC math exam, and 12.6 percentage points more likely to earn a certificate.

4.2 Direct Evidence of Positive Selection into Assisted Schools

To assess the degree of selection into assisted schools, we compare the incoming achievement of students who express preferences for assisted schools to those of students who do not. Students who list an assisted school as their top choice have incoming test scores 0.93 standard deviations higher than those who do not. This could be because assisted schools are more selective, and better prepared students put more selective schools on their list. To test for this, we model incoming SEA scores as a function of the selectivity of each of the school choices and whether each of the choices is an assisted school. For SEA cohorts 2002–2006, those who chose an assisted top, second, third, fourth, fifth, and sixth choice have test scores 0.15, 0.10, 0.10, 0.10, 0.06, and 0.04 standard deviations higher than those who do not. Similarly, for SEA cohorts 2007–2009, those who chose an assisted top, second, third, and fourth choice have test scores 0.17, 0.12, 0.12, and 0.11 standard deviations higher than those who do not. These significantly

higher scores are direct evidence of positive selection into assisted schools that is not merely due to assisted schools being more selective. This highlights the need for exogenous variation in school attendance.

4.3 Selection Free Effects of Attending a Preferred Assisted School

While our preferred strategy simultaneously uses discontinuity and DID variation, we present results for each strategy independently. If both strategies yield similar results, it would suggest that each strategy and the combination of the two yields the true relationship.

4.3.1 Discontinuity Variation Only

Table 4 presents the range of results obtained from the discontinuity variation under different choices of bandwidth and different polynomial orders of the SEA scores. The estimates do vary depending on the modeling assumptions made, but some general patterns emerge. First, attending an assisted school as a result of scoring above a cutoff for a preferred assisted school is associated with peer achievement that is between 0.166 and 0.79 standard deviations higher than comparable achievement at a government school (all statistically significant at the 1 percent level).

Second, there appear to be no effects on any of the outcomes measured. None of the specifications yield significant estimated effects on the number of exams taken or the likelihood of earning a certificate. For the rest of outcomes, only six estimated coefficients out of 48 were significant at the 10 percent level or lower (five were negative). The lower panel of figure 3 shows the discontinuity evidence for the number of CSEC exams passed and the likelihood of obtaining a CSEC certificate; as with the regression estimates, no effects are visible.

One interpretation of the difference between the OLS and the RD variation is that the OLS estimates are overstated due to positive selection on *unobservables* to assisted schools. Given the strong evidence of selection to assisted schools on observables this interpretation is reasonable. However, an alternative interpretation is that the marginal effect of gaining admission to an assisted school may be lower than that of the average effect of attending an assisted school (Angrist and Rokkanen, 2015). One common critique of identification via regression discontinuity is that identification is based only on individuals near the cutoff. In our context, this means that we are comparing the effect of being the least well-prepared student at an assisted school versus being average or above average at a regular government school. Indeed, Pop-Eleches and Urquiola (2013) present evidence that being among the relatively weaker

students can have deleterious effects on students. The results that use all exogenous variation suggest that both explanations may hold in this context. We now turn to the DID strategy, which allows for identification away from the cutoff and should yield more precise estimates.

4.3.2 Difference in Difference Variation Only

Panel D of table 3 presents the results from the 2SLS-DID model that instrument attending an assisted school with whether a student was assigned to an assisted school in the simulation. This model is conditional on indicator variables for each unique test score and combination of school-choice orderings (both differentiated for each SEA cohort). Similar to the discontinuity results, attending an assisted school is associated with 0.126 standard deviations higher peer achievement than at a government school. Also, consistent with the OLS results being biased due to selection on unobervables, there are no significant effects for any of the academic outcomes, and all the point estimates are orders of magnitude smaller than the OLS. Given the general similarity of the results across the distinct sources of variation, it is reasonable to turn to the model that exploits both sources of variation simultaneously. The general idea of using *all* the exogenous variation in school assignments embedded in the deferred acceptance algorithm is also suggested in Abdulkadiroglu et. al. (2015).

4.3.3 All Exogenous Variation

The rule-based IV strategy exploiting all exogenous variation yields fairly similar results to those from the previous models, except for the number of CSEC exams passed and the likelihood of earning a CSEC certificate (panel E of table 3). After accounting for student selection, students who attend assisted schools are exposed to peers whose incoming SEA scores are 0.17 standard deviations higher than those attending regular government schools. Also, consistent with the previous models, we find no effects on whether students took the CSEC exams, the number of exams each student took, the likelihood of passing the English exam, or the likelihood of passing the math exam. However, we do find significant impacts equivalent to 0.46 more exams passed and being 10 percentage points more likely to earn a certificate (the prerequisite to tertiary education). These later effects, while positive, are smaller than the OLS estimates, underscoring the importance of exploiting exogenous variation when analyzing the effects of assisted schools.

In sum, the results show that attending an assisted school is significantly associated with greater school selectivity. Both the discontinuity variation (based on those right around the cutoff) and the difference in difference variation (based on all students) suggest no effect on

taking the CSEC (our proxy for not dropping out), and no achievement effects in math or English. However, while the discontinuity variation also yields no effect on the number of CSEC exams passed and the likelihood of obtaining a CSEC certificate, the preferred specification indicates positive effects associated with attending an assisted school on the number of CSEC exams passed and the likelihood of obtaining a CSEC certificate that cannot be attributed to student selection. The similar results suggest that (a) there are no achievement effects on test scores associated with attending assisted schools, but (b) the difference in results for the two identification strategies for exams passed and earning a certificate likely reflects the fact that the most able students who attend assisted schools are induced to take and pass more exams. Given that teachers at assisted schools are much more likely to have attended college and these schools are perceived as high status, it is plausible that higher achieving students at these schools are encouraged to pursue tertiary education even though these schools provide no test score achievement effects.

In Section 4.4 below, we investigate whether these positive effects reflect assisted schooling impacts or if they are due to other factors associated with attending assisted schools.

4.3.4 Specification Checks and Falsification Tests

To show that our identification strategy is valid, we begin by presenting evidence that the simulated cutoffs are exogenous. The first test of this is to see if there is less density right below a cutoff and more density right above the cutoff than would be expected by random chance. Such a pattern would be consistent with gaming the cutoffs, and we test for this using the stacked dataset (described in section 3.3). Appendix figure A1 shows the density of incoming scores, and there is little evidence of such a pattern. Following McCrary (2008), we test for discontinuity in the density of the total score at the simulated cutoff while controlling for a fifth-order polynomial in the relative score. Where the dependent variable is the empirical density, the coefficient on an indicator variable denoting "above cutoff" is a statistically and economically insignificant 0.000035 (p-value = 0.97), which suggests no gaming.

If the cutoffs are exogenous, preferences should be balanced above and below the cutoff, and there should be no difference in the selectivity of school choices for those assigned to assisted schools conditional on school choices and test scores. As a second check on the discontinuity variation, we regress mean peer test scores at the first-choice school on a fifth-order polynomial in the relative score and the "above cutoff" indicator for the different

bandwidths used in the analyses (appendix table A1). This yields insignificant estimated coefficients on "above cutoff" of <0.001 for all bandwidths. The same exercise with the second-, third-, and fourth-choice schools yields similarly small and statistically insignificant coefficients. There is also no evidence of shifts in other observables (eleven religion indicators) associated with the cutoffs.

While it is not possible to estimate models with the full interaction between all test scores and all choices in the 2SLS-DID model (because this is the level of the variation), as a check on the DID variation, models can be estimated with interactions between coarse measures of test scores and coarse measures of preferences. Because an average student who lists an assisted school may differ from an average student who does not, we include group indicator variables defined by the unique combination of five indicators for the student's SEA score quintile, and four indicators for whether the first-, second-, third-, and fourth-choice school is assisted.[20] These group indicators control for coarse interactions between test scores and school choices so that comparisons are made among students who have similar test scores and similar school preferences, within which the assumption of additivity is likely to hold. Results from this specification (shown in table A2) are almost identical to those from the models without these indicator variables, suggesting that the DID identifying assumption of additive separability is valid.

Finally, we test the 2SLS model by seeing if a simulated assisted school assignment is correlated with observable preassignment characteristics conditional on smooth functions of test scores and choice indicator variables (appendix table A1). All 11 religion indicators tested yield small point estimates that are statistically indistinguishable from zero. All these tests suggest that the empirical strategies employed are likely valid.

4.4 Isolating the Assisted School Effect

The effect of attending a preferred assisted school may not isolate an assisted schooling effect because it may be confounded with the effects of attending a single-sex school, attending a preferred school (of any type), or attending a relatively more selective schools. We present results that account for these potentially confounding sources below.

4.4.1 Are the Results Driven by a Single-Sex School Effect?

[20] For SEA cohorts 2002–2006, we also included interactions with indicators for whether the fifth or sixth school choices were assisted. Results for this subsample yielded similar estimates.

Given that assisted schools are significantly associated with single-sex schooling regimes, the estimates presented so far would be confounded if there were academic benefits of single-sex schooling. To explore this possibility, we remove the effect of attending a preferred single-sex school from that of attending a preferred assisted school by instrumenting for and including attendance at a single-sex school as a covariate (panel F of table 3). After conditioning on single-sex schooling, the estimated coefficient for peer quality remains fairly constant, suggesting that assisted schools are associated with higher peer incoming quality, equivalent to 0.181 standard deviations in SEA scores. Outcomes that were previously found insignificant remain so, and the two outcomes that were significant without accounting for single-sex schooling become insignificant. However, the point estimates remain positive.

4.4.2 Are the Results Driven by Benefits to Gaining Admission to a Preferred School of Any Type?

To isolate the effect of scoring above the cutoff for a preferred school from that of attending an assisted school, we instrument for and include attending one's first-, second-, third-, fourth-, fifth-, or sixth-choice school as covariates (panel G of table 3). Conditional on single-sex schooling and the choice attained, students who attend a preferred assisted school end up with peers with 0.2 standard deviations higher achievement than those who attend a government school. Despite attending more selective schools, such students experience no statistically significant benefits to attending an assisted school. However, as in the previous specification, the point estimates for exams passed and earning a certificate remain positive and economically important.

We also estimate discontinuity models similar to (2) using cutoffs where the preferred school is a government school, where attending a preferred school is the endogenous treatment, and where scoring above the cutoff for a preferred school is the excluded instrument. The results are shown in table 5 for various bandwidths and polynomial orders of the running SEA relative score. Although imprecisely estimated, the effects on number of exams taken, the number of exams passed, and the likelihood of passing the English exam are mainly positive, and all the significant coefficients are positive. For passing the math exam and obtaining a CSEC certificate, there is a 50-50 mix between positive and negative estimated coefficients across bandwidths and specifications. However, all significant coefficients are positive, which suggests that attending a preferred government school leads to some improvements not found when analyzing the effects

of attending a preferred assisted school. Therefore, it appears that the benefits associated with scoring above a cutoff for a preferred school (of any type) can explain significant portions of the benefits to attending a preferred assisted school.

4.4.3 Do Students Perform Better at Assisted Schools Than at Equally Selective Government Schools?

Our results thus far indicate that despite being more selective, conditional on single-sex schooling and being admitted to a preferred school, there is no significant benefit to attending an assisted school. Even though school selectivity may be endogenous to whether the school is assisted, it is relevant to see if assisted schools have better outcomes compared to equally selective government schools. Panel H of table 3 presents 2SLS results conditioned on school selectivity. Because peer quality (average incoming SEA scores at the school) is a characteristic of the school, and students select to schools, we instrument for peer quality at the school attended with the peer quality at the simulated assigned school. These conditional effects yield insignificant results for taking the CSEC exams, the number of exams taken, the likelihood of passing the English exam, and the likelihood of passing the math exam. We do find, however, a marginally significant effect equivalent to 0.368 more exams passed and a significant 9.5 percentage points increase in the likelihood of obtaining a CSEC certificate.

The results suggest that attending an assisted school does not provide across-the-board benefits over attending an equally selective government school on average. Assisted schools do not outperform equally selective government schools in the likelihood of taking the CSEC exams, the number of exams taken, or the likelihood of passing the English and math exams. However, we do observe benefits in terms of the number of exams passed and success in obtaining a CSEC certificate, suggesting that assisted schools provide value added in these outcomes after accounting for both self-selection into schools and school selectivity (in terms of incoming peer SEA achievement). This patters on effects is similar to the finding that winning a lottery to a preferred school in Charlotte-Mecklenburg increase the number of courses taken and post-secondary education, but had no real effects on standardized tests (Deming et al., 2014). The lack of any test score effects and modest effects on exam taking and completing the prerequisites for tertiary education echo those on Clark (2010) who looks at the effect of attending grammar schools in the U.K.

4.5 Differential effects by gender

To assess whether assisted schools differentially benefit girls and boys, we estimate all models for males and females separately. Table 6 presents estimated effects for females and Table 6 presents estimated effects for males. While the standard errors are much larger when the samples are broken up by gender, the point estimates are very similar to those using the full sample.

4.6 Response Heterogeneity by Preferences for Assisted Schools

The treatment effect for students with strong preferences for assisted schools may be much larger than that for students with weak preferences for assisted schools if there is response heterogeneity and if individuals rationally select schools based on their personal benefits of attending a specific school; observing student choices allows us to investigate this. We infer the intensity of a student's preferences for assisted schools based on the number of assisted schools put on their preference list. In our population, 13.72 percent of students list zero assisted schools in their choices, 25.07 percent list one, 28.26 percent list two, 20.46 percent list three, and 12.49 percent list four or more assisted schools. As expected, those who actually attend assisted schools have stronger preferences for assisted schools than the average student. Among students who were assigned to and who attended assisted schools, 7.12 percent list one, 25.27 percent list two, 35.95 percent list three, and 31.66 percent list four or more assisted schools. Because only 32.95 percent of all students have relatively strong preferences for assisted schools (listing three or more assisted schools), while almost 68 percent of those who attend assisted schools have relatively strong preferences for them, the treatment effect for the marginal student may be very different from that of the average treated student, which may in turn be very different from that of the average student in the population.

To test for response heterogeneity by preference for assisted schools, we estimate both the 2SLS model (without conditioning on single-sex school, choice attended, or selectivity) and the 2SLS model, conditional on peer selectivity for students who list one, two, three, four, or more assisted schools in their choices separately. We cannot estimate causal effects for those who do not list any assisted school choices because they will not be assigned to an assisted school based on the simulated assignment algorithm. Figure 5 presents the conditional effects—which are all statistically insignificant—along with their 95% confidence intervals. Moreover, we observe flat patterns of estimates for different intensities regarding assisted school preferences. This suggests that the average effects found previously do not vary significantly in terms of stated preferences for assisted schools.

Finally, we also assess whether there are differential effects by intensity of preferences for assisted schools by gender by estimating the same models but splitting the populations between females and males. Figure 6 shows that all estimated effects are statistically indistinguishable from zero. However, there is a suggestive pattern showing that males with stronger preferences for assisted schools obtain relatively better results from attending an assisted school.

5. Conclusions and Policy Implications

Privately managed public schools have gained significant attention as a policy option towards increasing education quality. Many countries have implemented these arrangements, but there is little international evidence of their effectiveness regarding academic outcomes. Most of the evidence comes from charter schools in the United States with mixed results. Despite the important policy implications associated with the potential effectiveness of granting private administration of public schools, virtually no rigorous evidence incorporating the whole student population of a country has been produced. Owing to the unique setup of the education system and the data in Trinidad and Tobago, we were able to deal with the identification challenges that could plague isolating the academic effects of attending a privately managed public secondary school (or assisted school) versus a traditional public secondary school managed by the government (or government school).

We find that a failure to account for student selection can lead to large spurious estimated benefits to attending assisted schools. Once student selection is accounted for, attending an assisted school is not associated with any meaningful improvement in test performance. However, we do find modest positive effects on the number of exams passed and on earning a CSEC certificate (which is a requirement for tertiary education). This suggests that assisted schools have no achievement effects but may have an impact on university admissions. No significant differential treatment effects, however, were found by gender or preferences for assisted schools.

From a policy perspective, the results do not suggest that attending an assisted school provides academic benefits across the board. Within the context of Trinidad and Tobago, private management of public secondary schools does not appear to make substantive differences in terms of academic achievement, so policy options suggesting a broad migration of government schools to assisted school regimes may have little academic benefit. However, while we have

identified that assisted schools *per se* do not provide much academic value added, it is important not only to identify which schools are creating more value added, but also what school inputs are associated with higher value added. This is an open venue for policy relevant future research.

Finally, while the results suggest that assisted schools may not be highly effective for most students in terms of test performance, attending an assisted school appears to increase the likelihood of obtaining a CSEC certificate. Since this certificate is a requirement for entering tertiary education, there could be effects on attaining university education. If so, possible benefits of attending assisted schools linked to tertiary education admissions and further labor market outcomes may well exist, which provides grounds for future research.

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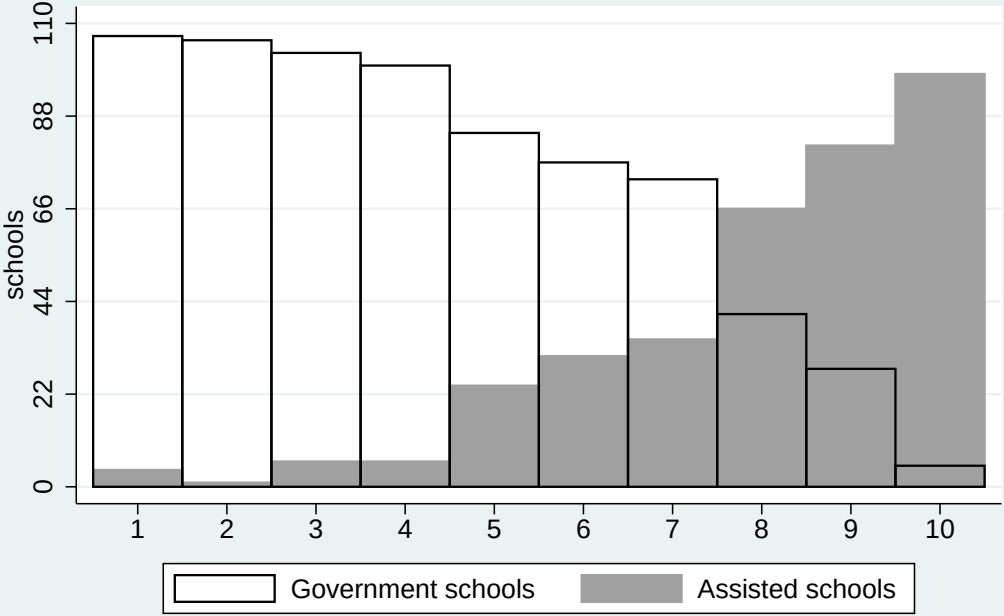
Figure 1: Distribution of Peer Achievement Across School Types
SEA Cohorts 2002-2009



Note: Data placed into 10 equally spaced bins of SEA scores. Observation is a school year

Figure 2: Likelihood of Being Assigned to a Preferred School

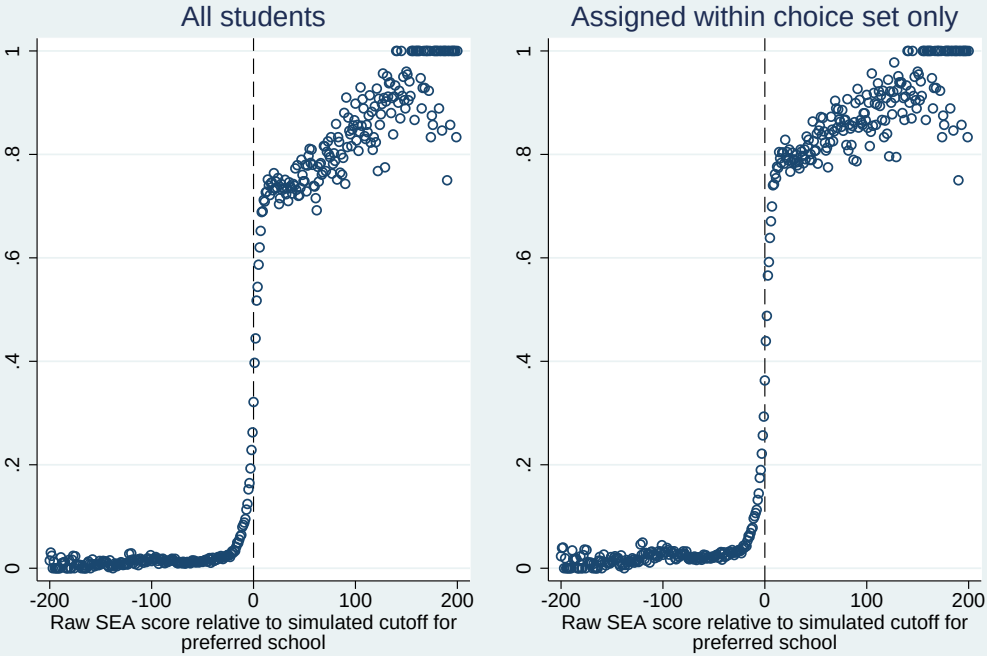
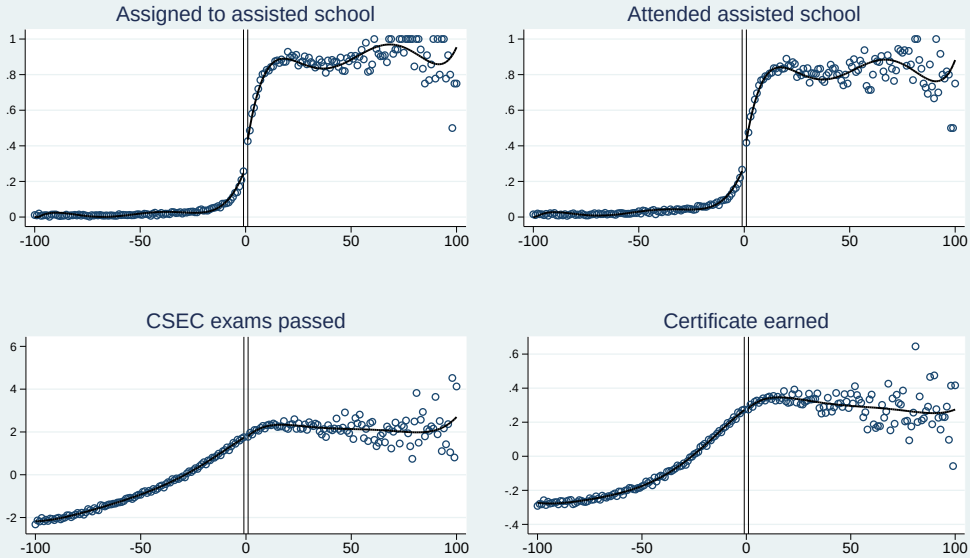


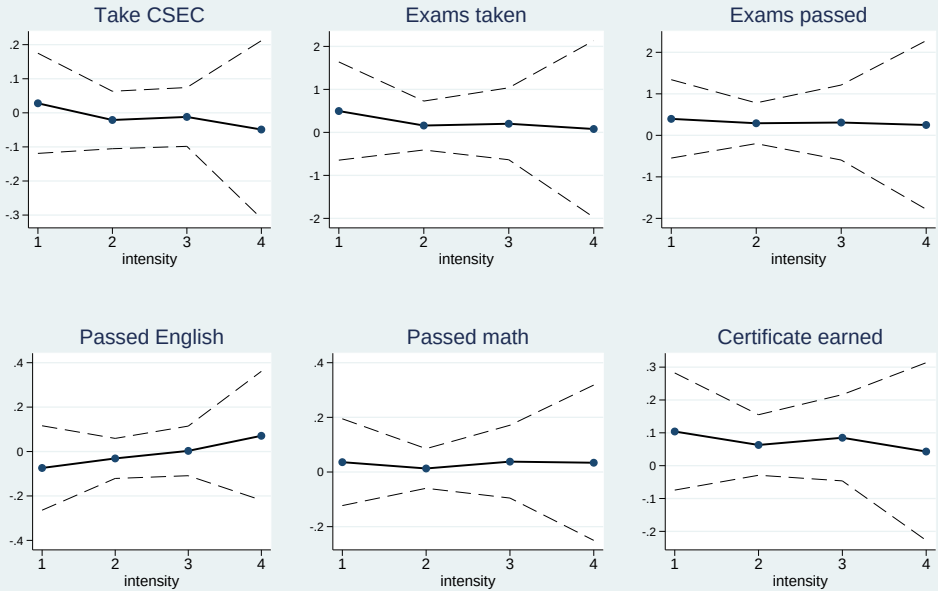
Figure 3: Change in Treatment and Outcomes Through Simulated Cutoffs, Applicants to Assisted Schools



Note: The X-axis is the score relative to the simulated cutoff. The Y-axis is the mean outcome for each relative score (net of the mean for the cutoff)

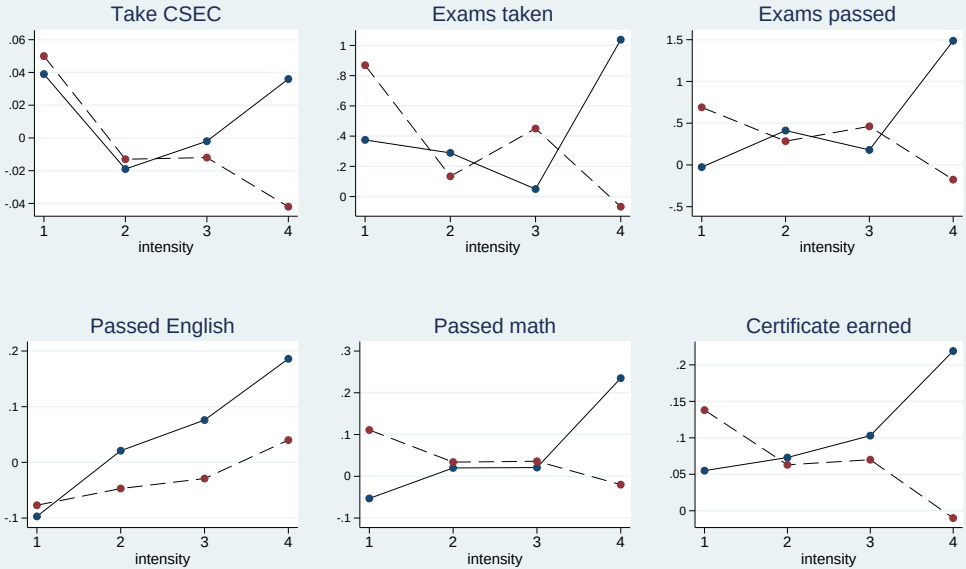
Figure 4: Graphical Illustration of the Difference-in-Difference Variation

Figure 5: Effects by Intensity of Preferences for Assisted Schools



Note: The X-axis is the number of assisted schools listed within the students' choice sets. Estimated effects shown in solid lines, while 95% confidence intervals shown in dashed lines.

Figure 6: Effects by Gender and Intensity of Preferences for Assisted Schools



The X-axis is the number of assisted schools listed within the students' choice sets.
Estimated effects for females shown in dashed lines, while effects for males shown in solid lines.
None of the estimated coefficients shown are statistically significant.

Table 1: Comparison of Inputs Between Assisted and Government Schools

Variable	Assisted (1)	Government (2)	Difference (1) - (2)
Teachers: %BA	0.74	0.43	0.31***
			(0.04)
Teachers: %MA	0.06	0.02	0.04***
			(0.01)
Teachers: years of experience	10.98	14.07	-3.09***
			(0.79)
Number of teachers	39.73	56.67	-16.93***
			(4.21)
Number of academic teachers	28.62	33.76	-5.14*
			(2.65)
School enrollment	642	783.84	-141.84**
			(56.10)
Grade 6 enrollment	116.48	151.47	-35.00**
			(13.43)
Pupils/(# teachers)	17.32	13.82	3.50***
			(0.66)
Pupils/(# academic teachers)	25.17	26.74	-1.57
			(3.01)
Single-sex	0.77	0.03	0.74***
			(0.05)
Mean incoming score (in SD)	0.79	-0.30	1.08***
			(0.12)
Number of schools	44	93	137

Note: Data from the 2005–2006 academic year. Estimated standard errors in parentheses below estimated differences. Significance at the 1, 5, and 10 percent levels is indicated by ***, **, and *, respectively.

Table 2: Summary Statistics

	All students		Students with a simulated assignment		
	Attend assisted	Attend government	Assigned assisted	Assigned government	Assigned private
	(1)	(2)	(3)	(4)	(5)
Standardized SEA score	0.82	-0.31	0.94	-0.25	0.25
	(0.65)	(0.92)	(0.56)	(0.93)	(0.68)
Female	0.52	0.49	0.53	0.49	0.63
	(0.50)	(0.50)	(0.50)	(0.50)	(0.48)
Take CSEC exams	0.84	0.65	0.83	0.66	0.82
	(0.37)	(0.48)	(0.38)	(0.47)	(0.39)
Exams taken	6.07	3.63	5.97	3.78	5.14
	(3.05)	(3.02)	(3.09)	(3.09)	(2.89)
Exams passed	5.11	1.90	5.15	2.09	3.25
	(3.17)	(2.47)	(3.19)	(2.61)	(2.75)
Pass CSEC English	0.72	0.31	0.72	0.33	0.54
	(0.45)	(0.46)	(0.45)	(0.47)	(0.50)
Pass CSEC math	0.66	0.21	0.67	0.23	0.37
	(0.48)	(0.40)	(0.47)	(0.42)	(0.48)
Certificate	0.59	0.14	0.61	0.17	0.27
	(0.49)	(0.35)	(0.49)	(0.38)	(0.44)
Number of students	40,816	101,560	36,260	83,047	858

Note: Standard deviations in parentheses below means. Sample sizes for the simulated assignment are smaller than the full sample because students with very low scores will have no simulated assignment. In reality, such students are assigned to schools based on availability and proximity. Earning a certificate is a prerequisite to entering tertiary education and entails passing 5 CSEC exams including English and math. Students who attended a private school were excluded from the sample. Although no student in the sample attended a private school, 858 students were simulated to attend one using the assignment rule. Of these, 220 ended up attending an assisted school and 638 a government school.

Table 3: Effects of Attending Preferred Assisted Schools

	Peer achievement	Take CSEC	Exams taken	Exams passed	Passed English	Passed math	Certificate
Panel A: OLS - No controls (142,376 observations)							
Assisted	1.130***	0.183***	2.443***	3.211***	0.409***	0.449***	0.448***
	(0.100)	(0.022)	(0.215)	(0.270)	(0.034)	(0.037)	(0.039)
Panel B: OLS - Fifth-order polynomial in SEA scores, and demographic controls (142,376 observations)							
Assisted	0.472***	0.049***	1.032***	1.184***	0.097***	0.134***	0.168***
	(0.057)	(0.010)	(0.133)	(0.156)	(0.015)	(0.020)	(0.026)
Panel C: OLS - Fifth-order polynomial in SEA scores, demographic controls, and preference-fixed effects (142,376 observations)							
Assisted	0.405***	0.056**	0.955***	0.971***	0.085***	0.095***	0.126***
	(0.121)	(0.024)	(0.282)	(0.261)	(0.032)	(0.031)	(0.036)
Panel D: 2SLS-DID - Individual SEA test score fixed effects, demographic controls, and preference-fixed effects (120,165 observations)							
Assisted	0.126**	-0.001	0.297	0.278	-0.002	0.017	0.041
	(0.055)	(0.027)	(0.235)	(0.256)	(0.032)	(0.037)	(0.042)
Panel E: 2SLS - Fifth-order polynomial in SEA scores, demographic controls, and preference-fixed effects (120,165 observations)							
Assisted	0.170**	-0.007	0.332	0.460**	-0.008	0.047	0.100**
	(0.070)	(0.027)	(0.223)	(0.224)	(0.032)	(0.039)	(0.046)
Panel F: 2SLS - Fifth-order polynomial in SEA scores, demographic controls, preference-fixed effects, and single-sex school (120,141 observations)							
Assisted	0.181**	-0.022	0.168	0.298	0.001	0.063	0.111
	(0.091)	(0.039)	(0.297)	(0.269)	(0.036)	(0.057)	(0.072)
Panel G: 2SLS - Fifth-order polynomial in SEA scores, demographics, preference-fixed effects, single-sex school, and choice attained fixed effects (120,141 obs.)							
Assisted	0.200*	-0.018	0.225	0.334	0.008	0.069	0.114
	(0.104)	(0.036)	(0.276)	(0.264)	(0.035)	(0.059)	(0.072)
Panel H: 2SLS - Fifth-order polynomial in SEA scores, demographic controls, preference-fixed effects, and peer quality (120,165 observations)							
Assisted		-0.017	0.223	0.368*	-0.026	0.039	0.095**
		(0.030)	(0.234)	(0.222)	(0.034)	(0.038)	(0.046)

Note: Estimated standard errors clustered at the attended school level in the OLS models and at the simulated assigned school level in 2SLS-DID and 2SLS models in parenthesis. Sample sizes for the simulated assignment are smaller than the full sample because students who score very low will have no simulated assignment. Demographic controls include gender and primary school district fixed effects. Significance at the 1, 5, and 10 percent levels is indicated by ***, **, and *, respectively.

Table 4: LATE Effect of Attending a Preferred Assisted School Using Discontinuity Variation Only

Outcomes																
Peer achievement		Take CSEC		Exams taken		Exams passed		Passed English		Passed math		Certificate		Bandwidth (SD)	Polynomial of SEA score	Obs.
0.790**	(0.361)	0.009	(0.281)	1.152	(2.331)	-0.352	(2.126)	0.069	(0.316)	-0.034	(0.325)	-0.212	(0.349)	0.5	5	57,073
0.485***	(0.168)	-0.122	(0.185)	-0.137	(1.406)	-0.806	(1.352)	-0.035	(0.196)	-0.094	(0.207)	-0.135	(0.214)	0.5	4	57,073
0.315***	(0.105)	-0.089	(0.128)	-0.154	(0.987)	-0.590	(0.943)	-0.046	(0.138)	-0.053	(0.145)	-0.103	(0.150)	0.5	3	57,073
0.262***	(0.039)	-0.041	(0.044)	-0.150	(0.362)	-0.261	(0.345)	-0.071	(0.048)	-0.046	(0.050)	-0.066	(0.052)	0.5	2	57,073
0.245***	(0.057)	-0.046	(0.066)	-0.248	(0.520)	-0.482	(0.495)	-0.088	(0.072)	-0.032	(0.072)	-0.076	(0.076)	1	5	91,733
0.249***	(0.039)	-0.048	(0.043)	-0.191	(0.358)	-0.289	(0.340)	-0.057	(0.048)	-0.067	(0.049)	-0.063	(0.051)	1	4	91,733
0.180***	(0.032)	-0.029	(0.034)	-0.123	(0.279)	-0.234	(0.261)	-0.051	(0.037)	-0.060	(0.038)	-0.064	(0.040)	1	3	91,733
0.254***	(0.021)	-0.020	(0.017)	0.096	(0.158)	0.147	(0.155)	-0.038*	(0.021)	-0.042*	(0.022)	-0.006	(0.024)	1	2	91,733
0.197***	(0.033)	-0.032	(0.033)	-0.145	(0.276)	-0.152	(0.262)	-0.039	(0.038)	-0.038	(0.039)	-0.033	(0.040)	1.5	5	116,822
0.252***	(0.027)	-0.013	(0.024)	0.062	(0.214)	0.061	(0.204)	-0.033	(0.028)	-0.041	(0.030)	-0.017	(0.032)	1.5	4	116,822
0.166***	(0.023)	-0.007	(0.019)	0.024	(0.168)	-0.017	(0.159)	-0.019	(0.022)	-0.039*	(0.023)	-0.032	(0.025)	1.5	3	116,822
0.270***	(0.017)	-0.028**	(0.012)	0.139	(0.111)	0.309***	(0.113)	-0.047***	(0.015)	-0.032**	(0.016)	0.019	(0.018)	1.5	2	116,822

Note: Estimated standard errors clustered at the cutoff level in parenthesis. Estimates are presented from a model using the subsample of the stacked dataset described in Section 3.3 that involves cutoffs for preferred assisted schools. Outcomes are modeled as a function of attending a preferred assisted school, smooth functions of the SEA score, and cutoff fixed effects. Attending a preferred assisted school is instrumented for with scoring above the cutoff for the preferred assisted school. The second stage coefficient on "*assisted* " is presented for each outcome. Significance at the 1, 5, and 10 percent levels is indicated by ***, **, and *, respectively.

Table 5: LATE Effect of Attending a Preferred Government School Using Discontinuity Variation Only

Outcomes																
Peer achievement		Take CSEC		Exams taken		Exams passed		Passed English		Passed math		Certificate		Bandwidth (SD)	Polynomial of SEA score	Obs.
0.177	(0.662)	0.294	(0.701)	0.783	(4.604)	-3.911	(4.551)	-0.321	(0.701)	-0.454	(0.684)	-0.766	(0.811)	0.5	5	55,617
0.231	(0.350)	0.129	(0.355)	0.250	(2.466)	-1.181	(2.120)	-0.013	(0.378)	-0.300	(0.358)	-0.373	(0.362)	0.5	4	55,617
0.289	(0.331)	0.033	(0.345)	-0.139	(2.356)	-1.424	(2.056)	-0.083	(0.363)	-0.381	(0.355)	-0.403	(0.351)	0.5	3	55,617
0.298***	(0.098)	0.043	(0.093)	0.162	(0.652)	0.009	(0.555)	0.017	(0.100)	-0.078	(0.098)	-0.052	(0.089)	0.5	2	55,617
0.251	(0.163)	0.093	(0.157)	0.324	(1.083)	-0.127	(0.886)	0.028	(0.160)	-0.165	(0.164)	-0.150	(0.151)	1	5	101,043
0.390***	(0.090)	0.056	(0.085)	0.486	(0.604)	0.277	(0.505)	0.025	(0.088)	-0.042	(0.088)	0.003	(0.081)	1	4	101,043
0.352***	(0.081)	0.061	(0.078)	0.372	(0.554)	0.128	(0.457)	0.029	(0.080)	-0.040	(0.080)	-0.004	(0.073)	1	3	101,043
0.474***	(0.040)	-0.002	(0.029)	0.351*	(0.211)	0.670***	(0.191)	0.060*	(0.032)	0.091***	(0.034)	0.139***	(0.031)	1	2	101,043
0.398***	(0.081)	0.063	(0.079)	0.467	(0.567)	0.220	(0.474)	0.070	(0.081)	-0.028	(0.082)	0.012	(0.074)	1.5	5	134,456
0.428***	(0.053)	0.032	(0.041)	0.431	(0.296)	0.485*	(0.261)	0.045	(0.045)	0.040	(0.045)	0.084**	(0.042)	1.5	4	134,456
0.385***	(0.050)	0.028	(0.038)	0.312	(0.273)	0.270	(0.240)	0.036	(0.041)	0.006	(0.042)	0.041	(0.039)	1.5	3	134,456
0.509***	(0.031)	-0.025	(0.018)	0.319**	(0.129)	0.841***	(0.129)	0.068***	(0.022)	0.136***	(0.025)	0.176***	(0.024)	1.5	2	134,456

Note: Estimated standard errors clustered at the cutoff level in parenthesis. Estimates are presented from a model using the subsample of the stacked dataset described in Section 3.3 that involves cutoffs for preferred government schools. Outcomes are modeled as a function of attending a preferred government school, smooth functions of the SEA score, and cutoff fixed effects. Attending a preferred government school is instrumented for with scoring above the cutoff for the preferred government school. The second stage coefficient on "*Government school* " is presented for each outcome. Significance at the 1, 5, and 10 percent levels is indicated by ***, **, and *, respectively.

Table 6: Effects of Attending Preferred Assisted Schools, Female Sample

	Take CSEC	Exams taken	Exams passed	Passed English	Passed math	Certificate
Panel A: OLS - No controls (71,014 observations)						
Assisted	0.145***	2.206***	3.293***	0.373***	0.459***	0.482***
	(0.020)	(0.212)	(0.289)	(0.034)	(0.041)	(0.043)
Panel B: OLS - Fifth-order polynomial in SEA scores, and demographic controls (71,014 observations)						
Assisted	0.044***	1.028***	1.311***	0.083***	0.142***	0.185***
	(0.009)	(0.131)	(0.160)	(0.014)	(0.020)	(0.026)
Panel C: OLS - Fifth-order polynomial in SEA scores, demographic controls, and preference fixed effects (71,014 observations)						
Assisted	0.067***	1.131***	1.248***	0.088**	0.131***	0.163***
	(0.025)	(0.292)	(0.302)	(0.034)	(0.035)	(0.040)
Panel D: 2SLS-DID - Individual SEA test score fixed effects, demographic controls, and preference fixed effects (60,567 observations)						
Assisted	-0.018	0.222	0.358	-0.013	0.062	0.072
	(0.037)	(0.347)	(0.369)	(0.045)	(0.061)	(0.062)
Panel E: 2SLS - Fifth-order polynomial in SEA scores, demographic controls, and preference fixed effects (60,567 observations)						
Assisted	-0.004	0.424	0.486	-0.027	0.058	0.090*
	(0.032)	(0.300)	(0.308)	(0.042)	(0.050)	(0.050)
Panel F: 2SLS - Fifth-order polynomial in SEA scores, demographic controls, preference fixed effects, and single-sex school (60,558 observations)						
Assisted	-0.017	0.152	0.193	-0.046	0.072	0.087
	(0.045)	(0.396)	(0.369)	(0.046)	(0.082)	(0.092)
Panel G: 2SLS - Fifth-order polynomial in SEA scores, demographics, preference fixed effects, single-sex school, and choice attained fixed effects (60,558 obs.)						
Assisted	-0.014	0.220	0.281	-0.037	0.084	0.098
	(0.045)	(0.392)	(0.364)	(0.049)	(0.083)	(0.092)
Panel H: 2SLS - Fifth-order polynomial in SEA scores, demographic controls, preference fixed effects, and peer quality (60,567 observations)						
Assisted	-0.010	0.297	0.351	-0.055	0.047	0.083
	(0.041)	(0.329)	(0.303)	(0.046)	(0.051)	(0.053)

Note: Estimated standard errors clustered at the attended school level in the OLS models and at the simulated assigned school level in 2SLS-DID and 2SLS models in parenthesis. Sample sizes for the simulated assignment are smaller than the full sample because students who score very low will have no simulated assignment. Demographic controls include gender and primary school district fixed effects. Significance at the 1, 5, and 10 percent levels is indicated by ***, **, and *, respectively.

Table 7: Effects of Attending Preferred Assisted Schools, Male Sample

	Take CSEC	Exams taken	Exams passed	Passed English	Passed math	Certificate
Panel A: OLS - No controls (71,362 observations)						
Assisted	0.218***	2.638***	3.058***	0.436***	0.433***	0.405***
	(0.027)	(0.272)	(0.348)	(0.044)	(0.048)	(0.052)
Panel B: OLS - Fifth-order polynomial in SEA scores, and demographic controls (71,362 observations)						
Assisted	0.049***	1.012***	1.049***	0.118***	0.121***	0.147***
	(0.013)	(0.181)	(0.218)	(0.023)	(0.028)	(0.037)
Panel C: OLS - Fifth-order polynomial in SEA scores, demographic controls, and preference fixed effects (71,362 observations)						
Assisted	0.044	0.769**	0.687**	0.090**	0.056	0.089*
	(0.034)	(0.357)	(0.326)	(0.042)	(0.040)	(0.049)
Panel D: 2SLS-DID - Individual SEA test score fixed effects, demographic controls, and preference fixed effects (59,598 observations)						
Assisted	-0.003	0.310	0.198	0.004	-0.042	0.028
	(0.040)	(0.365)	(0.413)	(0.051)	(0.051)	(0.060)
Panel E: 2SLS - Fifth-order polynomial in SEA scores, demographic controls, and preference fixed effects (59,598 observations)						
Assisted	-0.001	0.348	0.454	0.036	0.034	0.105
	(0.039)	(0.325)	(0.334)	(0.051)	(0.055)	(0.074)
Panel F: 2SLS - Fifth-order polynomial in SEA scores, demographic controls, preference fixed effects, and single-sex school (59,583 observations)						
Assisted	-0.012	0.366	0.372	0.063	0.046	0.117
	(0.049)	(0.409)	(0.416)	(0.063)	(0.077)	(0.104)
Panel G: 2SLS - Fifth-order polynomial in SEA scores, demographics, preference fixed effects, single-sex school, and choice attained fixed effects (59,583 observations)						
Assisted	-0.011	0.390	0.358	0.067	0.048	0.114
	(0.047)	(0.397)	(0.379)	(0.062)	(0.078)	(0.095)
Panel H: 2SLS - Fifth-order polynomial in SEA scores, demographic controls, preference fixed effects, and peer quality (59,598 observations)						
Assisted	-0.013	0.249	0.382	0.023	0.027	0.102
	(0.039)	(0.324)	(0.334)	(0.050)	(0.054)	(0.073)

Note: Estimated standard errors clustered at the attended school level in the OLS models and at the simulated assigned school level in 2SLS-DID and 2SLS models in parenthesis. Sample sizes for the simulated assignment are smaller than the full sample because students who score very low will have no simulated assignment. Demographic controls include gender and primary school district fixed effects. Significance at the 1, 5, and 10 percent levels is indicated by ***, **, and *, respectively.

Figure A1: Test for Smoothness Through the Simulated Cutoffs

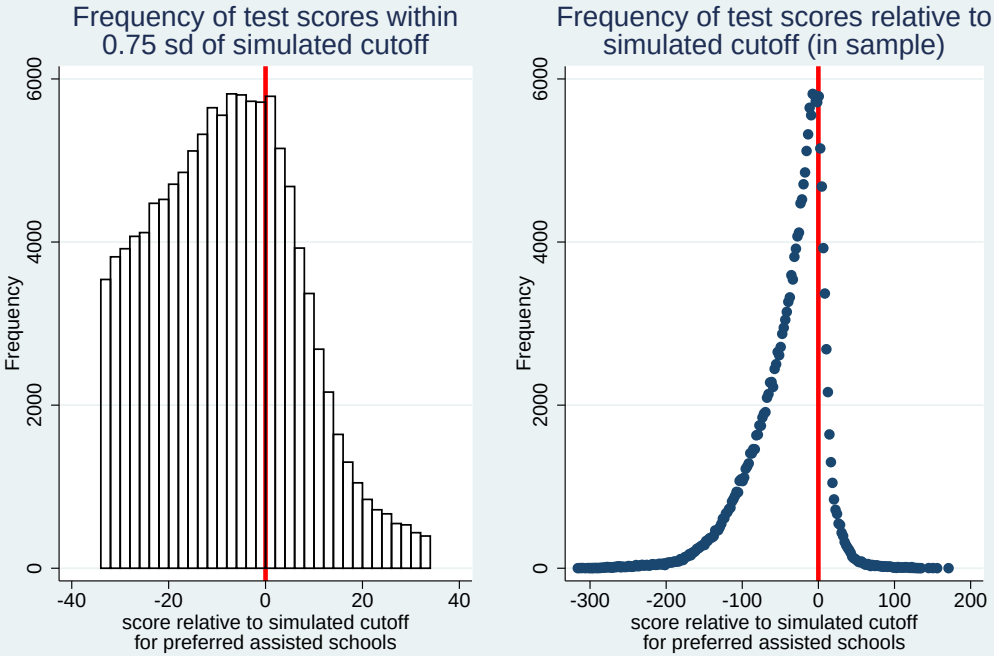


Table A1: Testing for Smoothness of Observable Characteristics Across Cutoffs

	Coefficient on:			
	Above cutoff Bandwidth: 0.5 sd	Above cutoff Bandwidth: 1 sd	Above cutoff Bandwidth: 1.5 sd	Simulated Assisted
Peer scores at choice 1	0.000	0.000	0.000	-
	(0.004)	(0.003)	(0.003)	-
Peer scores at choice 2	0.002	-0.000	0.001	-
	(0.005)	(0.004)	(0.003)	-
Peer scores at choice 3	-0.006	-0.001	-0.001	-
	(0.006)	(0.005)	(0.004)	-
Peer scores at choice 4	0.005	0.006	0.005	-
	(0.009)	(0.007)	(0.006)	-
Religion 1	-0.001	-0.003	-0.003	0.000
	(0.006)	(0.004)	(0.004)	(0.008)
Religion 2	-0.003	-0.005	-0.004	-0.003
	(0.004)	(0.003)	(0.003)	(0.010)
Religion 3	0.008	0.004	-0.002	0.002
	(0.010)	(0.008)	(0.007)	(0.015)
Religion 4	-0.002	-0.002	-0.002*	-0.001
	(0.002)	(0.001)	(0.001)	(0.003)
Religion 5	0.005	0.005	0.002	-0.007
	(0.007)	(0.005)	(0.004)	(0.009)
Religion 6	0.005	0.003	-0.001	0.012
	(0.012)	(0.008)	(0.007)	(0.017)
Religion 7	-0.001	0.005	0.003	-0.001
	(0.006)	(0.005)	(0.004)	(0.008)
Religion 8	-0.003	-0.003	-0.001	-0.007
	(0.005)	(0.003)	(0.003)	(0.008)
Religion 9	-0.004	-0.004*	-0.001	0.002
	(0.003)	(0.002)	(0.002)	(0.004)
Religion 10	-0.001	0.000	0.010	-0.004
	(0.010)	(0.007)	(0.006)	(0.015)
Religion 11	-0.007	-0.002	-0.002	0.013
	(0.006)	(0.005)	(0.004)	(0.009)

Note: Each estimate represents a separate regression of the simulated instruments (scoring above the simulated cutoff or the simulated assisted assignment) on a separate covariate. The estimated effects of scoring above a simulated cutoff are based on models similar to equation (2) using the stacked discontinuity sample with bandwidths of 0.5, 1, and 1.5 standard deviations with estimated standard errors clustered at the cutoff level. The estimated effects of simulated assisted are based on models similar to equation (4): controlling for a fifth-order polynomial in SEA, preferences fixed effects, gender and primary school district with estimated standard errors clustered at the simulated assignment school level. In this model, peer scores at the different school choices are absorbed by the preference fixed effects. Significance at the 1, 5, and 10 percent levels is indicated by ***, **, and *, respectively.

Table A2: 2SLS-DID Model Including Interactions Between Coarse Measures of Test Scores and School Preferences

	Peer achievement	Take CSEC	Exams taken	Exams passed	Passed English	Passed math	Certificate
Panel A: Complete sample (118,199 observations)							
Assisted	0.131**	-0.002	0.252	0.230	-0.002	0.016	0.032
	(0.055)	(0.031)	(0.272)	(0.303)	(0.037)	(0.045)	(0.050)
Panel B: Female sample (59,513 observations)							
Assisted	-	-0.022	0.166	0.290	-0.017	0.060	0.059
	-	(0.042)	(0.395)	(0.423)	(0.051)	(0.069)	(0.069)
Panel C: Male sample (59,686 observations)							
Assisted	-	-0.006	0.241	0.087	0.000	-0.059	0.003
	-	(0.047)	(0.401)	(0.478)	(0.062)	(0.061)	(0.075)

Note: Estimated standard errors clustered at the simulated assigned school level in parenthesis. Models include include group indicator variables defined by the unique combination of five indicators for the student's test score quintile, and four indicators for whether the first-, second-, third-, and fourth-choice school is assisted. Demographic controls include gender and primary school district fixed effects. Significance at the 1, 5, and 10 percent levels is indicated by ***, **, and *, respectively.

Table A3: Differential Effects of Attending Preferred Assisted Schools by Intensity of Preferences for Assisted Schools, Female and Male

	Take CSEC	Exams taken	Exams passed	Passed English	Passed math	Certificate
Panel A: 2SLS - Fifth-order polynomial in SEA scores, demographic controls, and preference fixed effects						
1 Assisted	0.015	0.478	0.352	-0.045	0.018	0.091
Obs= 30,372	(0.069)	(0.495)	(0.413)	(0.074)	(0.095)	(0.096)
2 Assisted	0.000	0.348	0.449*	-0.009	0.032	0.077
Obs= 33,682	(0.032)	(0.273)	(0.256)	(0.044)	(0.040)	(0.048)
3 Assisted	-0.011	0.251	0.374	0.007	0.051	0.087
Obs= 24,029	(0.042)	(0.409)	(0.459)	(0.058)	(0.065)	(0.066)
4+ Assisted	-0.032	0.248	0.430	0.073	0.044	0.049
Obs= 14,549	(0.139)	(1.069)	(1.049)	(0.155)	(0.147)	(0.145)
Panel B: 2SLS - Fifth-order polynomial in SEA scores, demographic controls, preference fixed effects, and peer quality						
1 Assisted	0.028	0.497	0.397	-0.074	0.036	0.104
Obs= 30,372	(0.075)	(0.583)	(0.482)	(0.097)	(0.081)	(0.091)
2 Assisted	-0.021	0.160	0.292	-0.031	0.013	0.063
Obs= 33,682	(0.043)	(0.290)	(0.250)	(0.046)	(0.037)	(0.047)
3 Assisted	-0.012	0.201	0.309	0.003	0.038	0.085
Obs= 24,029	(0.044)	(0.427)	(0.461)	(0.057)	(0.068)	(0.067)
4+ Assisted	-0.049	0.079	0.250	0.071	0.034	0.043
Obs= 14,549	(0.133)	(1.048)	(1.036)	(0.148)	(0.145)	(0.138)

Note: Estimated standard errors clustered at the simulated assigned school level in parenthesis. Demographic controls include gender and primary school district fixed effects. Significance at the 1, 5, and 10 percent levels is indicated by ***, **, and *, respectively.

Table A4: Differential Effects of Attending Preferred Assisted Schools by Intensity of Preferences for Assisted Schools, Females

	Take CSEC	Exams taken	Exams passed	Passed English	Passed math	Certificate
Panel A: 2SLS - Fifth-order polynomial in SEA scores, demographic controls, and preference fixed effects						
1 Assisted	0.025	0.729	0.488	-0.070	0.085	0.115
Obs= 13,911	(0.103)	(0.869)	(0.782)	(0.118)	(0.123)	(0.119)
2 Assisted	0.011	0.408	0.496	-0.014	0.053	0.081
Obs= 16,628	(0.048)	(0.426)	(0.391)	(0.059)	(0.067)	(0.064)
3 Assisted	-0.011	0.464	0.635	-0.008	0.069	0.094
Obs= 13,151	(0.054)	(0.501)	(0.527)	(0.069)	(0.075)	(0.077)
4+ Assisted	-0.054	0.001	0.029	0.038	-0.015	-0.008
Obs= 9,117	(0.133)	(1.037)	(1.049)	(0.153)	(0.136)	(0.144)
Panel B: 2SLS - Fifth-order polynomial in SEA scores, demographic controls, preference fixed effects, and peer quality						
1 Assisted	0.050	0.869	0.690	-0.077	0.111	0.138
Obs= 13,911	(0.101)	(0.830)	(0.709)	(0.128)	(0.102)	(0.093)
2 Assisted	-0.013	0.134	0.285	-0.047	0.034	0.063
Obs= 16,628	(0.055)	(0.436)	(0.370)	(0.063)	(0.060)	(0.061)
3 Assisted	-0.012	0.450	0.462	-0.029	0.036	0.070
Obs= 13,151	(0.067)	(0.597)	(0.599)	(0.084)	(0.095)	(0.088)
4+ Assisted	-0.042	-0.067	-0.176	0.040	-0.020	-0.010
Obs= 9,117	(0.129)	(1.046)	(1.047)	(0.144)	(0.142)	(0.130)

Note: Estimated standard errors clustered at the simulated assigned school level in parenthesis. Demographic controls include primary school district fixed effects. Significance at the 1, 5, and 10 percent levels is indicated by ***, **, and *, respectively.

Table A5: Differential Effects of Attending Preferred Assisted Schools by Intensity of Preferences for Assisted Schools, Males

	Take CSEC	Exams taken	Exams passed	Passed English	Passed math	Certificate
Panel A: 2SLS - Fifth-order polynomial in SEA scores, demographic controls, and preference fixed effects						
1 Assisted	0.030	0.460	0.120	-0.038	-0.066	0.045
Obs= 16,641	(0.087)	(0.642)	(0.552)	(0.087)	(0.090)	(0.120)
2 Assisted	-0.001	0.402	0.497	0.027	0.033	0.080
Obs= 17,054	(0.045)	(0.403)	(0.352)	(0.059)	(0.052)	(0.072)
3 Assisted	-0.004	0.017	0.175	0.075	0.019	0.105
Obs= 10,878	(0.081)	(0.673)	(0.760)	(0.109)	(0.113)	(0.114)
4+ Assisted	0.050	1.123	1.539	0.188	0.241	0.223
Obs= 5,477	(0.300)	(2.400)	(2.176)	(0.349)	(0.359)	(0.335)
Panel B: 2SLS - Fifth-order polynomial in SEA scores, demographic controls, preference fixed effects, and peer quality						
1 Assisted	0.039	0.375	-0.026	-0.097	-0.053	0.055
Obs= 16,641	(0.109)	(0.782)	(0.687)	(0.112)	(0.103)	(0.134)
2 Assisted	-0.019	0.289	0.412	0.021	0.020	0.073
Obs= 17,054	(0.047)	(0.397)	(0.339)	(0.057)	(0.048)	(0.069)
3 Assisted	-0.002	0.050	0.180	0.076	0.021	0.103
Obs= 10,878	(0.084)	(0.689)	(0.791)	(0.112)	(0.118)	(0.119)
4+ Assisted	0.036	1.038	1.487	0.186	0.235	0.219
Obs= 5,477	(0.296)	(2.325)	(2.110)	(0.346)	(0.353)	(0.332)

Note: Estimated standard errors clustered at the simulated assigned school level in parenthesis. Demographic controls include primary school district fixed effects. Significance at the 1, 5, and 10 percent levels is indicated by ***, **, and *, respectively.