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SINGLE-SEX SCHOOLS, STUDENT ACHIEVEMENT, AND COURSE SELECTION: EVIDENCE FROM RULE-BASED STUDENT ASSIGNMENTS IN TRINIDAD AND TOBAGO

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ABSTRACT

Existing studies on single-sex schooling suffer from biases due to student selection to schools and single-sex schools being better in unmeasured ways. In Trinidad and Tobago students are assigned to secondary schools based on an algorithm allowing one to address self-selection bias and cleanly estimate an upper-bound single-sex school effect.

The upper-bound effects show that while students (particularly females) with strong expressed preferences for single-sex schools may benefit from attending them, most students perform no better at single sex schools. I show that the treatment effect for the typical single-sex student differs greatly from that of the average student. Girls at single-sex-schools take fewer sciences courses and more traditionally female subjects.

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Existing studies on single-sex schooling suffer from biases due to student selection to schools and single-sex schools being better in unmeasured ways. In Trinidad and Tobago students are assigned to secondary schools based on an algorithm allowing one to address self-selection bias and cleanly estimate an upper-bound single-sex school effect. The upper bound effects show that while students (particularly females) with strong expressed preferences for single-sex schools *may* benefit from attending them, *most* students perform no better at single sex schools. I show that the treatment effect for the typical single-sex student differs greatly from that of the average student. Girls at single-sex-schools take fewer sciences courses and more traditionally female subjects.

The relative merits of single-sex schooling versus coeducational schooling have been fiercely debated. Proponents of single-sex schools typically make one of three arguments: The first is that girls and boys learn in fundamentally different ways, so that single-sex schooling allows teachers to tailor their lessons to the particular needs of each gender. The second argument is that the presence of the opposite sex is inherently distracting,. The third argument is that coeducational schools tend to reinforce gender stereotypes. Based on this notion, increased single-sex schooling is thought to be a way to increase female representation in the hard sciences. This debate has been going on for years in European, Latin American, and Caribbean nations where single-sex-schools are prevalent and has recently been ignited in the US with the passage of Title IX[1] regulations that made it easier for school districts to provide single-sex schools (Weil 2008, Medina 2009, Rasicot 2009).

If students have better academic outcomes in single-sex-schools then by merely reshuffling students across schools to attain complete sex segregation, overall educational attainment can be increased. In this scenario, with no increase in spending one can either have a better educated labor force or cost-savings that can be put into other productive sectors of the economy. Also, if students are more likely to take subjects at which they have high aptitude in single-sex schools, then single-sex schooling could lead to more efficient allocations of talent to courses and subsequent occupations. As such, single-sex schooling is a potentially important technological advance in human capital production.

Despite theory suggesting that single-sex schools are good for students, and their potential importance for education policy, there is very little conclusive empirical evidence on the effects of single-sex schooling on student outcomes. In describing the 2221 studies on single-sex schooling, a meta-

[1] On November 24th, 2006 existing Title IX regulations of the Education Amendments of 1972 were amended. While the previous regulations permitted school districts to provide single-sex public schools to students of one sex only if they provided comparable single-sex public schools to students of the other sex, the new regulations only required providing equal coeducational schooling to students of the other sex. For more details, see http://www.ed.gov/news/pressreleases/2006/10/10242006.html.

analysis (Mael, et al. 2005) conducted by the US Department of Education in 2005 states:

> "According to the guidelines of the [What Works Clearinghouse] WWC, all studies other than randomized controlled trials, quasi-experimental designs (QED) with matching, or regression discontinuity designs would be excluded prior to Phase III. Under the WWC criteria for inclusion, virtually all single-sex studies would have been eliminated from the review process because of the lack of experimental research on this topic."

All of the empirical evidence on single-sex schooling (to my knowledge) is based on comparisons between children who chose to attend single-sex schools and those who do not.[2] This evidence is unlikely to isolate the effect of single-sex schooling on student outcomes for three important reasons: First, because students who *decide to* attend single-sex schools may differ from those who *decide to* attend coeducational schools in important unobserved ways, such comparisons may be subject to self-selection bias. Second, because single-sex schools are often different in important unobserved ways from coeducational schools (e.g. curriculum, academic calendar) these comparisons may confound a single-sex school effect with other important differences. Third, because single-sex schools are often perceived as being better schools and are therefore more selective than coeducational schools, these studies may confound a "single-sex schooling" effect with a "school selectivity effect".

I attempt to overcome these obstacles by using data from Trinidad and Tobago to investigate the following empirical questions: (1) Do students, on average, benefit from attending single-sex schools on a range of academic outcomes? (2) Are there heterogeneous effects based on preferences for single-sex schools? (3) Do the effects vary by gender? (4) Are the effects for the typical single-sex school student similar to those of the average student in the population who may not chose to attend a single-sex school and, (5) Do single-sex schools affect the course selection of girls and boys?

The Trinidad and Tobago education system is well-suited for studying the effect of attending a single-sex school because (a) roughly ten percent of all public secondary schools are single-sex, (b) all secondary schools share the same national curriculum and (c) students are assigned to secondary schools by the Ministry of Education based on their performance on a secondary school entrance exam and a list of school choices ─ so that conditional on exam scores and the list of school choices, attendance to

[2] Several studies compare the outcomes of students who attend single-sex Catholic schools to those who attend coeducational traditional public schools. Acknowledging the high likelihood of institutional differences other than being single-sex across these schools, a few studies have attempted to deal with these selection issues by looking at Catholic high school students who attend single-sex vs. coeducational schools. Based on such comparisons, (Lee and Bryk 1986) find that girls at single-sex schools do better while there is no effect for boys. However, (Marsh 1989) using the same data and similar methodology finds that Catholic high schools have no effect on achievement once one controls for baseline scores. Consistent with this (LePore and Warren 1997) compare the outcomes of students who attend single-sex and coeducational Catholic secondary schools and control for selection by including lagged test scores and find no statistically significant single-sex school effect. None of these studies control for the selectivity level of the school or adjust for selection to schools (other than controlling for lagged achievement). The international evidence is also decidedly mixed: In studies that do include controls for prior achievement, (Jimenez and Lockheed 1989) find that girls in Thai single-sex secondary school classes do better at math while boys do worse, while (Harker 2000) finds that single-sex secondary schools in New Zealand have little impact, (Malacova 2007) finds that both boys and girls at selective single-sex schools in the United Kingdom achieve higher progress in single-sex schools. None of these studies explicitly account for the selectivity level of the school or adjust for selection to schools (other than controlling for lagged achievement).

single-sex schools is partially beyond their control and therefore not subject to self-selection bias.

To address the self-selection bias that makes it difficult to obtain credible causal effects when comparing observationally similar students who attend different schools, I use rule-based instrumental variables in the spirit of (Campbell 1969) based on the student school assignment rules used by the Ministry of Education. The assignment rules (described in Section 2) are deterministic, non-linear functions of student choices and incoming test scores that are determined by *the interaction* between student choices and student test scores. That is, (1) conditional on two students having the same test score, differences in school assignments are due to their choices, and (2) conditional on two students with the same choices, differences in school assignments are due to their test scores. As such, I employ a difference-in-differences type instrumental variables strategy to isolate exogenous variation in school attendance. To show that my identification strategy is likely valid, I show that, conditional on test scores and choices, the instruments are not correlated with incoming student characteristics, and I present a variety of tests indicating the variation exploited is exogenous.

To address the concern that single-sex schools may differ from coeducational schools in other important ways, I focus the analysis to public schools that share the same curriculum and institutional details. The remaining concern is that single-sex schools are typically more selective than coeducational schools and may also have better inputs. Because non-experimental variation is not ideal for credibly decoupling a school selectivity effect from a single-sex school effect, I demonstrate that in Trinidad and Tobago single-sex schools are more selective than and have better inputs than coeducational schools, and I focus on credibly identifying an *upper-bound* effect of attending a single-sex school.[3] Insofar as this upper bound effect is close to zero for all students or large portions of the student population, the estimates, while imperfect, will provide valuable new credible information.

One unique feature of these data is that I can observe the number of single-sex schools a student lists in her school choices, so that I can test for response heterogeneity by the intensity of preferences for single-sex schools. Because this preference measure is strongly associated with actual single sex school attendance, this allows me to (a) determine the extent to which the local treatment effects obtained for those who typically apply to single-sex schools differ from those of the average student and (b) speak to whether any improved outcomes reflect better student-school matching or a technological improvement

[3] In principle, one could separately identify a single-sex school effect from a school selectivity effect with a randomized experiment that took existing schools and randomly assigned some to be single sex and other to be coeducational. However, such an experiment would identify the *short-run* effect of making some schools single-sex, which may be very different from the policy relevant effect of attending a school that has been singe sex for several years (that may have changed pedagogical styles and management styles etc. in the long-run to take advantage of the single-sex environment). As such, while looking at actual schools does not allow one to separate a school selectivity effect from a single-sex schools, it does allows one to say something about the policy relevant long-run effects (which a randomized experiment would not).

that would benefit all students. To my knowledge, the analysis is unique in this regard.

Using instrumental variables to account for student-selection yields effects that are half as large as naive ordinary least squares effects; indicating positive selection to single-sex schools. The *upper-bound* instrumental variables effects suggest modest benefits to attending single-sex schools but mask considerable response heterogeneity by preference and gender. For students with weak preferences for single-sex schools (about 85 percent of all students) the upper-bound effects are close to zero ─ indicating that the majority of students do not benefit from attending a single-sex school. However, for students with strong preferences for single-sex schools (about 12 percent of all students and almost 60 percent of those who are assigned to and attend single-sex schools), the upper-bound effects suggest large benefits. These benefits persist in models that condition on school selectivity explicitly ─ suggestive of a real single-sex school effect that does not operate through school selectivity *for this sub-sample of students*. This pattern of results suggest that the effects for students who typically attend single-sex schools grossly overstate the effect for the average student in the population. An analysis by gender reveals that most of the estimated benefits to attending single-sex schools are driven by girls with strong preferences for single-sex schools. Contrary to the belief that single-sex schools cause girls to take more math and science classes, single-sex schools led girls to take *fewer* science courses and *more* traditionally female subjects.

This is the first study, to my knowledge, to investigate the causal effect of single-sex schooling on student outcomes. The results suggest that previous studies on single sex schools may suffer from sizable student-selection bias. The findings also highlight that local treatment effects of schools (based on applicants who are likely to benefit more than others) can be very misleading about effects for the average student. The results suggest making single-sex schools available to those few students with strong preferences for single-sex schools *may* improve academic outcomes *for these few students*, but that expanding single-sex secondary schools to all students will have little effect on overall achievement, and will not be an effective tool for increasing female representation in math, science, and engineering fields.

The remainder of the paper is as follows: Section 2 lays out the theoretical justifications for and against single-sex schooling. Section 3 describes the Trinidad and Tobago education system, the assignment mechanism, and the data. Section 4 describes the empirical framework, section 5 presents the results, and Section 6 concludes.

2. *Theoretical Justifications for Single-Sex Schooling*

Much of the justification for single-sex education stems from the notion that boys and girls learn in different ways so that single-sex schools allow for the teacher to focus the lesson plans to the particular strengths and learning styles of each sex. This is, in essence, a special case of the focus model of classroom peer effects where heterogeneity in learning styles in the classroom is inherently crippling for a teacher. Studies have found that less classroom heterogeneity in ability levels is associated with better student outcomes (Hoxby and Weingarth 2006, Ding and Lehrer 2007, Duflo, Dupas and Kremer 2009). Some researchers suggest that the differences in learning styles are biological in nature. Specifically, girls' and boys' brains grow in mass at different rates such that girls complete about half of their brain development by age 11 compared to age 15 years for boys (Lenroot, et al. 2007). There is also evidence based on functional MRI scans that in girls the language areas of the brain develop before the areas used for spatial relations and for geometry, while in boys, it's the other way around (Killgore and Yurgelun-Todd 2004). Taking these differences as given, a curriculum which ignores these biological differences in the timing of brain development between boys and girls *may* (a) cause boys who are not as developmentally advanced as girls to perform poorly academically and disengage from school, and (b) produce boys who think that "writing is for girls" and girls who think they are "bad at math".

Behavioral psychology also suggests differences in learning styles between girls and boys. For example there is a deep literature documenting that girls are under-confident in their abilities while boys are overconfident (Pomerantz, Altermatt and Saxon 2002, Beyer 1990, Beyer and Bowden 1997) — suggesting that girls should be encouraged while boys need to be challenged. Also psychologists have found that girls are more concerned than boys with pleasing authority figures such as parents and teachers (Higgins 1991, Cross and Madson 1997, Maccoby and Jacklin 1974, Eagly 1978) while most boys are only motivated to study when the material itself interests them. Insofar as there are real differences in learning styles between boys and girls, the greater classroom homogeneity afforded by single-sex schools may be associated with improved academic outcomes for both boys and girls.

In addition to the "difference in learning styles" argument, it is argued that the mere presence of the opposite sex is inherently distracting. Both boys and girls may be more concerned with social status in coeducational environments than in single-sex environments. Supporting this notion, (Coleman 1961) finds that both boys and girls in coeducational schools were less concerned about academic achievement and more concerned about appearance and popularity, (Riordian 1990) argues that girls in coeducational schools do not want to seem “too smart” because they do not want to lose their appeal to boys, and (Trickett and Trickett 1982) find that students at single-sex schools had more positive attitudes toward academics and were more involved in classroom activities.

There is common perception that single-sex schools help girls but hurt boys.[4] This notion is driven, in part, by the belief that boys are disruptive and alter the classroom dynamic in ways that are not conducive to learning. Supporting this idea, researchers have found that larger shares of boys are associated with worse academic outcome for all students (Lavy and Schlosser 2009, C. Hoxby 2000) —

[4] For example, Mary Bousted of the UK's Association of Teachers and Lecturers states that "All the research shows single-sex schools are good for girls but bad for boys – both in terms of academic performance and socialization." (Garner 2009).

implying that, *ceteris paribus* relative to coeducational schools, girls might perform better in all-girls schools while boys may perform worse in all-boys schools.

Regarding course selection, it is argued that single-sex schooling increases the likelihood that boys participate in traditionally female subjects and girls take math and science classes. There is suggestive observational evidence supporting this notion (Spielhofer, Benton and Schagen 2004, James and Richards 2003). There are two explanations to rationalize this. The first is based on the neurological differences between boys and girls. Specifically, because girls develop the portions of the brain used in math later than boys, they are more likely to underperform in math and science at early ages and therefore disengage from and avoid these subjects in a coeducational one-size-fits-all education system. By the same logic, because boys develop the linguistic portions of the brain later than girls, boys are more likely to underperform in English and literature at early ages and therefore disengage from and avoid these subject in a coeducational one-size-fits-all education system. The second explanation is rooted in a model similar to (Akerlof and Kranton 2000) where identity is associated with different social categories, dictates how people in these categories should behave, and enters the utility function directly. In this context, either to avoid social sanctions from other group members or to avoid acting in ways inconsistent with one's identity, boys will avoid "girls' subjects" such as poetry, while girls will avoid "boys' subjects" such as math and hard sciences. The argument for single-sex schools is that in the absence of the opposite sex, the gendered nature of subjects is no longer salient, therefore removing the disutility or stigma associated with particular subjects.[5] However, a simple contagion model of social interactions suggests that single-sex schools may reinforce pre-existing gender differences in course participation, so that the effect of single-sex schooling on course taking is theoretically ambiguous.

3. *The Trinidad and Tobago Education System and the Data*

The Trinidad and Tobago education system evolved from the English education system. Secondary school begins in grade 6 and ends at grade 10 when students take the Caribbean Secondary Education Certification (CSEC) examinations. These are the Caribbean equivalent of the British Ordinary levels examinations[6] and are externally graded by examiners appointed by the Caribbean Examinations Council. Students seeking to continue their education typically take five or more subjects, and virtually all testers take the English language and mathematics exams.[7]

[5] There is empirical evidence of such dynamics for black students in (Fryer 2009). In schools that are mixed black students face a social penalty for doing well in school (i.e. acting white), while they do not face such a penalty in predominantly black schools.

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

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

There are eight educational school districts. Unlike in many countries where private schools are often of higher perceived quality, private schools in Trinidad and Tobago account for a small share of student enrollment and tend to serve those who "fall through the cracks" in the public system.[8] There are three types of public secondary schools: Government schools, Government assisted schools (referred to as assisted schools) and Comprehensive schools. Government schools are secondary schools that provide instruction from 6th through 10th grade and often continue to 12th grade. These schools teach the national curriculum and are fully funded and operated by the Government. Government assisted schools, often the more elite schools, are like Government schools but differ along a few key dimensions. They are run by private bodies (usually a religious board) and, while capital expenses are publicly funded, their teacher costs are not paid for by the Government. Along all other dimensions, Government and Government assisted schools are virtually identical. The third type of schools, Comprehensive schools, are Government schools that were *historically* vocational in focus. In the past, students with low test scores after 5th grade were assigned to such schools and after 3 years took an exam to gain admission to a senior secondary school (or possibly a regular Government school) which would prepare them for the CSEC examinations. During the relevant sample period Comprehensive schools differed from Government schools only in name. All schools taught the same academic curriculum, and only a few Comprehensive schools did not provide instruction through to the CSEC exams.[9]

3.2. *Student Assignment Rules (Algorithm)*

Students in Trinidad and Tobago compete for a limited number of places at premium schools. After 5th grade, students take the Secondary Entrance Assessment (SEA) examinations. Each student lists four ordered secondary school choices. These choices and their SEA score are used by the Ministry of Education to assign them to schools using the following algorithm. Each secondary school has a predetermined number of open slots each year and these slots are filled sequentially such that the most highly subscribed/ranked school fills its spots first, then the next highly ranked school fills is slots, and so on until all school slots are filled. This is done as follows: (1) the number of school slots at each school is predetermined based on capacity constraints (this is a time-invariant school specific characteristic) (2) Each student is tentatively placed in the applicant pool for his or her first choice school and is ranked by SEA score. (3) The school that is oversubscribed with the highest "cut off" score fills its slots first. For

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learning for those seeking to continue academic studies). The CAPE is the Caribbean equivalent of the English Advanced Levels (A-Levels) examinations.

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

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

example, suppose both schools A and B have 100 slots, and 150 students list each of them as their top choice. If the 100^{th} student at school A has a score of 93 (school B's cut-off score) while the 100^{th} student at school B has a score of 89 (school B's cut-off score), school A is ranked first and fills all its spots first. (4) Those filled school slots and the students who are assigned to the highest ranked school are removed from the applicant pool and the process is repeated, where a student's second choice now becomes their first choice if the first choice school has been filled. This is repeated until all slots are filled.

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

3.3. *Simulating the Student Assignments Using the Rules*

Unfortunately, the actual cut-off scores for each school are not released to the public and those student who were hand-picked cannot be identified in the data. However, because the rules are known and I have the same information that the Ministry of Education uses to assign students, I can determine where the cut-offs *would have been* (and therefore the schools students would have been assigned to) if Assisted schools could not hand-pick students. The simulated school assignments and cut-offs are constructed sequentially as follows: (1) All secondary school sizes are given[10], (2) all students are put in the applicant pool for their top choice school and are ranked by incoming SEA scores, (3) the school with the highest

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

cut off (as described in section 3.2) fills all its slots with the highest scoring students who listed that school as their first choice[11], (4) the students who were rejected from the top choice school are placed back into the applicant pool and their second choice school becomes their first choice school, (5) Steps 2 through 5 are repeated, after removing previously assigned students and school slots until the lowest ranked school is filled. The *only* difference between how students are actually assigned and the "tweaked" rule-based assignment is that at step (3) the "tweaked" rule does not allow any students to be hand-picked while, in fact, some students are hand-picked by principals only at Government assisted schools.

To show the validity of the simulation, using the "simulated" school assignments and cut-offs, I estimate the likelihood of being assigned to a preferred school as a function of one's score relative to the simulated cut-off for that school. To do this I combine several discontinuities into one. Specifically, for each school I find all students who list that school as the top choice, re-center all those students' test scores around the cut-off for that school, and then create a sample of applicants for each school. To mimic the sequential nature of the assignment mechanism (i.e. the top ranked school fills its slots before the applicant pool for the second rank school is determined), I then remove students who were assigned to their top choice schools, replace students' first choice with their second choice, and repeat this process with the second choice, third choice, and fourth choice. The applicant samples for all schools are then stacked so that every student has one observation for each school for which they were an actual applicant. For example, a student who attends their top choice school will only be in the data once for his or her top choice school, while a student who gets into their second choice school will be in the data twice (once for the top choice school and once for the second choice school). Because all scores are re-centered, scoring above zero means scoring above the cut-off for a preferred school.

Using this stacked dataset, I present the relationship between being assigned to one's preferred school as a function of one's incoming test score relative to the simulated cut-off for the preferred school in Figure 1. On the left I show the figure using all the observations, while on the right I exclude those students whose school assignment is not one of their four choices (i.e. I exclude those students for whom scoring above a cut-off could not result in any treatment differential). If there were no self-selection or hand picking of students by principals, the figure would be a step function. However, as one can see, there is a rapid increase in the likelihood of being assigned to a preferred school as one's score goes from below to above the simulated cut off ─ indicating that the assignments operate as described. This suggests that there are meaningful differences in schooling environments associated scoring just above versus just below a simulated cut-off that are not due to selection. The fact that the assignment rules lead to exogenous cut-offs that are well approximated by the simulated cut-offs (which are orthogonal to student

[11]To deal with ties, I assume that all students who score at or above the cut-off are assigned. The results are unchanged if were to make the alternate assumption that only those scoring above are assigned.

self-selection by construction) plays a central role in my identification strategy.

Because students are more likely to attend a preferred school when they score above a cut-off, it is important to better understand what a preferred school looks like. Students' school choices are based largely on their own perceived ability, geography, and religion. Specifically, higher ability students tend to have higher achievement schools in their list, students often request schools with the same religious affiliation as their own, and students typically list schools that are geographically close to their homes. Also, students tend to put schools with higher-achieving peers higher up on their preference ranking. On average the difference between the mean incoming SEA scores at a student's top choice school and their second, third, and fourth choice school is 0.277, 0.531, and 0.82 standard deviations, respectively. This pattern is shown graphically in Appendix Figure A1.

Among students with preferences for single-sex schools the cut-offs create arguably exogenous variation in single-sex school attendance (I document this in section 3). As such, it is instructive to get a sense of what preferences for single-sex schools are like. Roughly 58 percent of all students have a single-sex school as one of their four secondary school choices and students tend to put single-sex schools higher up on their list. Specifically, roughly half of all students list a single-sex school as their top choice school (47 percent for boys and 52 percent for girls), while about one third list a single-sex school as their second choice (29 percent for boys and 33 percent for girls), one fifth list a single-sex school as their third choice (18 percent for boys and 21 percent for girls), and about one tenth list a single-sex school as their fourth choice (9 percent for boys and 10 percent for girls).

3.4 Data and Summary Statistics

The data used in this study come from two sources: the official SEA test score data (5th grade) for the 1995 through 2002 cohorts and the official 2000 through 2007 CSEC test score data (10th grade). The SEA data contain each of the nation's student's SEA test scores, 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. The SEA exam is comprised of five subjects that all students take: math, English, science, social studies, and an essay. To track these 5th grade students through to secondary school in 10th grade, I link the SEA data with the CSEC examination data both four and five years later. Roughly two-thirds of SEA test takers were linked to CSEC exam data.[12] The CSEC data contain each student's grades on each CSEC exam and secondary school they attended. In the data, there are 123 public

[12] Students were matched based on name, gender and date of birth. The match rate was just over 70 percent, which is consistent with the national high school dropout rate of one third. Note that students with missing CSEC data are coded as having zero passes *and are included in the regression sample* so that the results are not affected by sample selection bias. In section V, I present results on the effect on CSEC taking (the extensive margin) and show that the results on the number of exams passed are driven primarily by the intensive margin (i.e. improvements among those who would have taken the CSEC exams regardless of school or peer quality).

secondary schools and several small test taking centers and private schools. Of these schools, 34 are single-sex schools which are split almost evenly between all-boys and all-girls schools. Among students linked to CSEC data, under seven percent attended a private institution, were home schooled, or were unaffiliated with any public education institution. I determine whether a student took the CSEC exams, compute the number of examinations taken and passed, and determine the courses taken. The resulting dataset contains 219,849 students across seven cohorts and 126 school assignments.

Table 1 summarizes the final dataset, broken up by single-sex school attendance, assignment, and gender. One clear pattern in these data are that students who are assigned to single-sex schools have much higher incoming achievement than those who are assigned to coeducational schools. Specifically, girls and boys who are *simulated to be assigned* to all-girls and all-boys schools have test scores that are 1.45 and 0.99 standard deviations higher than those of girls and boys assigned to coeducational schools, respectively. As one might expect, the average outcomes are much better among students simulated to be assigned to single-sex schools. About 90.3 percent of girls assigned to all-girls schools remain in secondary school to take the CSEC exams five years after entering secondary school compared to only 63 percent at coeducational schools. Among boys the differences are even larger where 87.3 percent of boys assigned to all boys schools remain in secondary school to take the CSEC exams five years after entering secondary school compared to only 51.9 percent at coeducational schools. Girls and boys simulated to be assigned to single-sex schools pass 6.18 and 5 CSEC exams respectively, compared to only 2.09 and 1.22 at coeducational schools for girls and boys, respectively. An important academic outcome is earning a certificate (passing at least 5 exams including math and English) because it is the prerequisite to continuing to tertiary education. Girls and boys who are *simulated to be assigned* to all-girls and all-boys schools have a likelihood of earning a certificate of 0.796 and 0.633 compared to 0.16 and 0.09 for girls and boys assigned to coeducational schools, respectively. These represent very large and important differences in academic outcomes across school types.

I classify courses into three groups: (1) Sciences— biology, chemistry, physics, information technology, and integrated sciences; (2) Hard Sciences— chemistry and physics; and (3) Female dominated subjects— defined as any subject where more than two thirds of all participants were female in 1999 (a pre-sample year). These subjects are literature, history, biology, integrated sciences, French, Spanish, principles of accounts, principles of business, Information technology, Office procedures, Food and nutrition, typewriting, home economics, shorthand, clothing and textiles. The summary statistics show that both boys and girls simulated to be assigned to single-sex schools take more female dominated subjects, sciences, and hard sciences than boys and girls assigned to coeducational schools. This is also true for those who take the CSEC exams and attend single-sex schools— suggesting that these differences do not merely reflect students being more likely to take the CSEC exams at single-sex schools.

It is clear from Table 1 that single-sex schools are more selective than coeducational schools so that even if one were to remove self-selection bias to schools, a comparison of outcomes across single-sex schools and coeducational schools will confound single-sex schooling with more selective schooling (high achieving peers). Using the same set of schools, Jackson (2010) finds a large positive effects of attending a school with higher-achieving peers. To get a sense of the distribution of peer achievement across schools, in Figure 2 I put the peer achievement across all schools in all years into ten equally spaced bins, and I show the number of single-sex schools and coeducational schools that fall into each of these bins. The unit of observation is a school year so that a school that existed for all seven years of the data will be represented seven times, while a school that opened in 2002 will only be in the data once. Figure 2 shows that while there is substantial overlap in the distribution of peer achievement between single-sex schools and coeducational schools, the schools with the highest achieving peers are disproportionally single-sex schools. This indicates that in comparing students who attend single-sex schools to those who attend coeducational schools, one will get an *upper-bound* estimate because the single-sex school effect will be confounded with a school selectivity effect.

3.5 *Direct Evidence of Positive Selection into Single-sex Schools*

Because I can observe student school choices, I am able to assess the degree of selection into single-sex schools. To gauge this I compare the incoming achievement levels of students *who express preferences for* single-sex schools to those of students who do not. Students who list a single-sex schools as their top choice have incoming test scores one standard deviation higher than those who do not. This could merely be an artifact of the fact that single-sex schools are more selective and better prepared students put more selective schools on their list. To test for this, I compare the incoming test scores of students who list a single-sex school as their top choice school to those who do not while controlling for the mean peer achievement level of the top choice school. *Taking the selectivity of the choices into account*, students who list a single-sex school as their top choice have incoming test scores that are 0.56 standard deviations higher than those who do not. If one predicts incoming test scores as a function of the selectivity of each of the school choices and also whether each of the choices are single-sex schools, those who chose a single-sex top, second, third, and fourth choice have test scores 0.06, 0.1, 0.1, and 0.017 standard deviations higher than those who do not (each conditional on the other school choices). This is direct evidence of positive selection into single-sex schools that is not merely due to single-sex schools being more selective and highlights the need for exogenous variation in school attendance.

4 *Econometric Framework*

As shown in Figure 1 and Table 1, single sex schools are more selective than coeducational schools. Jackson (2010) demonstrates that school selectivity has a positive effect on students outcomes, so that it is clear that where one does not control for differences in input quality across schools that the estimated single-sex schooling effect is more positive than the true effect. I demonstrate that this is the case empirically. If I had data on school inputs (which I do not), I could control for these inputs and hope that there are no omitted inputs that are correlated with single-sex schools; this is clearly unsatisfactory. Also, one may be tempted to control for school selectivity directly. This approach, while not unreasonable, is undesirable because school selectivity may be endogenous to single-sex schooling. This would likely lead one to find no single-sex school effect even if there were one. I therefore focus on credibly identifying an *upper bound* effect by removing the bias due to student selection. While a large positive upper bound would be uninformative, an upper-bound effect close to zero would indicate that there are no benefits to attending single sex schools. My aim then is to say something meaningful about single sex schools by estimating upper-bound effects for different sub-samples of the population.

Effects on the Typical Single-Sex School Student vs. the Typical Student in the Population

If some individuals benefit from single sex schools while others do not, and if individuals select to schools based on their private benefits of attending a school, then the typical single-sex school student may have a very different treatment effect than the typical student in the population. Both are important to know because if only students with strong preferences for single-sex schools benefit from them, then the successes of the existing single-sex schools will not be scalable or replicable for the average student. On the other hand, if all students benefit equally (even those who have weak preferences for single sex schools) then turning all schools into single sex schools would improve average student achievement. In most credible designs (such as lottery assignment) researchers are only able to estimate a Local Average Treatment Effect (LATE) and are unable to speak to how the results may generalize to those students who do not self-select into the admissions pool for schools.

The fact that I am able to observe student choices allows me to get around this problem and asses the degree to which the LATE (based on students with strong preferences for single sex schools) is similar to the treatment effect for the typical student who may not have strong preferences for single-sex schools. This allows me to asses if single sex-schools will improve outcomes for the typical student.

I infer the intensity of a student's preferences for single-sex schools based on the number of single-sex schools they put in their list. *In the population*, 40 percent of students do not list any single-sex schools in their choices. However, 27 percent list one, 17 percent list two, 10 percent list three, and 4 percent list four single-sex schools. As expected, those who actually attend single-sex schools have stronger preferences for single-sex schools than the average student. *For those who attend single-sex schools* only 16 percent do not list any single-sex schools, 23 percent list one, 28 percent list two, 24 percent list three, and 10 percent list four single-sex schools. For those who are used to estimate the

LATE described below (students both assigned to and who attended single-sex schools) 9 percent list one, 31 percent list two, 38 percent list 3, and 20 percent list four single-sex schools.

Because only14 percent of students have strong preferences for single-sex schools (list 3 of 4 schools) while almost 60 percent of those who are used to estimate the LATE have strong preferences for single sex schools, the LATE may be very misleading about the effects for a typical student response heterogeneity by the intensity of preferences for single-sex schools. To investigate this I (a) determine if the upper-bound effects are largest for students who typically attend single-sex schools, (b) determine if the upper-bound effect is distinguishable from zero for students with weak preferences for single-sex schools (86 percent of the student population), and (c) estimate of what fraction of the population may benefit from attending single-sex schools.

4.2 *Identification Strategy*

The major obstacle to credibly identifying an upper-bound single-sex school effect is removing the effect of student self-selection. In this section I describe how I overcome this obstacle. To estimate the effect of attending a single-sex school, I compare the outcomes of students with similar incoming characteristics who attend different schools. For the baseline specification, I model the outcome of student *i* at a school *j* with the following equation.

$$Y_{ij} = SEA_i \cdot \beta + \text{single}_{ij}\sigma_{upper} + X_i\delta + \sum_{p=1} I_{ip} \cdot \theta_p + \varepsilon_{ij} \quad [1]$$

In [1], single_{ij} is an indicator variable equal to 1 if the student attends a single-sex school and equal to 0 otherwise, SEA_i is a matrix of incoming test scores (a fifth order polynomial in the student's total SEA score[13]), X_i is student gender, I_{ip} is an indicator variable denoting the school choice list of student *i* (that is a indicator variable identifying each unique list of four choices)[14], and ε_{ij} is the idiosyncratic error term. The coefficient σ_{upper} is the estimate of the *upper* bound effect of attending a single-sex school.

While including individual SEA scores should remove a large amount of self-selection bias, OLS without preferences may still be biased because students may know more about their ability and aspirations beyond their SEA scores. Adding preferences should remove that bias. However, OLS

[13] A fifth order polynomial was chosen by starting with a linear model and increasing the order of the polynomial until the last order is no longer statistically significant at the 5 percent level. As such, one can reject a model with an order less than a fifth order polynomial at the 5 percent level. While not presented in this paper, all results are robust to using a third, fourth, fifth, or sixth order polynomial.

[14] Each preference group is defined by a distinct preference ordering of schools. All students who list schools A,B,C,D in that order form a group, while students who list schools B,A,C,D are in a different group because even though they have the same list of schools, the ordering is different. There are 22649 preference groups with more than one student. Among these groups, the average group has 63 students.

estimates of σ_{upper} from [1] may still suffer from bias because students who are unhappy with their initial school assignment may be able to subsequently transfer across schools (recall that outcomes are measured five years after student are initially assigned to schools), and assisted schools can hand-pick 20% of their incoming class.[15] Because this opens the door to selection, I propose rule-based instrumental variables strategies to deal with this endogeneity concern.

4.3 *Rule-Based Instrument*

To remove self-selection bias from the actual school attended, I use the simulated school assignments described in section 3.3 to construct exogenous instruments. That is, I use the initial school assignment that would prevail if Government assisted schools could not select students. This assignment can be constructed by "tweaking" the school assignment mechanism to impose the deterministic portion of the assignment mechanism on *all* students. Since the deterministic portion of the assignment mechanism is used to assign most students to schools, the assignments based on the "tweaked" assignment mechanism should be correlated with the schools students actually attend. However, since the deterministic portion of the assignment mechanism cannot be manipulated by students or school principals, the "tweaked" assignments should be uncorrelated with *unobserved* student characteristics such as motivation and ability, conditional on student test scores and school choices. As such, following Jackson (2010) I propose an instrumental variables strategy based on these "tweaked" assignments.

I exploit the fact that the school assigned, and therefore single-sex school attendance, is partly based on a deterministic function of the student's total SEA score and the student's school preferences. Since this deterministic function is non-linear and non-smooth, it can be used as an instrument while directly controlling for smooth functions of the underlying covariates themselves (Fisher 1976). For each school student pair, I define the variable $Rule_{ij}$ that is equal to 1 if student *i* would have been assigned to school *j* had there been no student self-selection or school selection of students and *0* otherwise. That is, $Rule_{ij}$ is equal to *1* if student *i* is assigned to school *j* based on the simulations described in Section 3.3 and *0* otherwise. $Rule_{ij}$ is the simulated school assignment and is the deterministic portion of the student assignment algorithm. This simulated assignment is determined by student test scores and choices, *but also by the interaction between the two*. This fact plays a central role in my identification strategy.

4.3. *Sources of Exogenous Variation and the Econometric Models*

Conditional on incoming test scores and choices, $Rule_{is}$ captures two plausibly exogenous sources of variation in single-sex school assignment (and attendance). I discuss these two distinct sources

[15] About 60% of students take the CSEC exams at the school to which they were initially assigned.

of exogenous variation, and describe an instrumental variables estimation strategy that exploits them.

Exogenous variation due to test scores: According to the simulated assignment mechanism the only reason two students with the same set of school choices are assigned to different schools would be due to differences in their tests scores. Specifically, *conditional on school choices*, the assignment rule creates test score cut-offs above which student are assigned to one school and below which they are assigned to another. As such, the first source of exogenous variation comes from comparing the outcomes of students assigned to different schools (one of which is a single-sex school) who score just above and just below a cut-off. The logic is similar to a regression discontinuity design. *Among students who chose a single-sex school*, the likelihood of being assigned to (and attending) a single-sex school increases in a sudden and discontinuous manner as one's score goes from below to above the cut-off for that single-sex school (see Figure 1). If the location of the cut-offs are orthogonal to student characteristics, and the effect of test scores on outcomes are smooth through the cut-offs, one can attribute any sudden jumps in the outcomes as one's score goes from below to above the cut-offs to the sudden increased likelihood of attending one's preferred single-sex school. Appendix Note 1 shows that results using the discontinuity variation alone are similar to those using all the exogenous variation but are much less precise and more sensitive to specific modeling assumptions.

Exogenous variation due to school choices: According to the simulated assignment mechanism the only reason two students with the same test score are assigned to different schools would be due to differences in their school choices. Because school choices are directly observed, and not all students with the same preferences attend the same schools (owing to the test score variation) one can exploit variation due to differences in school choices while directly controlling for student school choices (a relatively rare feature of the Trinidad and Tobago data). The logic of this source of variation is as follows: Consider two students (A and B) with the same test score X who are assigned to schools S (single-sex) and C (coeducational) respectively. Suppose both students A and B list the same first choice school F (an arbitrary first choice school), but list S and C as their second choice schools. If they both just miss the cut-off for their top choice school (school F), then student A ends up in single-sex school S and student B ends up in coeducational school C. The difference in outcomes of students A and B reflects *both* differences in choices and differences in single-sex school assignment (and therefore attendance). Consider now, two similar students (A' and B') such that A' has the same choices as A, and B' has the same choices as B, but A' and B' have the same score X' that is higher than X. If X' is above the cut-off for the top choice school, then A' and B' will both be assigned to the same top choice school (school F) even though they listed different second choice schools. The difference in outcomes between A' and B' reflects only the differences in their choices, because they have the same test score and have the same school assignment. Under the assumption that differences in outcomes due to test scores are the same across all choice groups, one can subtract the difference between A' and B' from the difference between A and B to isolate the differences in outcomes associated with attending single-sex school S relative to coeducational school C. This is difference-in-difference variation.

Rule-Based Instrument Using all Exogenous Variation: Both sources of plausibly exogenous variation come from the fact that the simulated assignment is a non-smooth function of *the interaction between school choices and incoming test scores*, so that conditional on both test scores and school choices, there is useful exogenous variation in simulated school assignments. To exploit both sources of variation for identification, I use a 2SLS strategy that estimates the effect of attending a single-sex school after controlling for a full set of choice indicator variables (i.e. controlling for the underlying choices that generate variation in school assignments), and smooth functions of the incoming SEA tests scores (i.e. controlling for the underlying test score that generates sudden non-smooth variation in school assignments). I then instrument for single-sex school attendance with an indicator variable denoting whether the simulated school is a single-sex school. To obtain a clean estimate of the upper bound effect I estimate the following system of equations by 2SLS.

$$\begin{aligned} \text{single}_{ij} &= f_1(SEA_i) + \gamma_1(\overline{\overline{\text{single}}} \mid Rule_{ij}) + X_i\delta_1 + \sum_{p=1} I_{i,p}\cdot\theta_{p1} + \varepsilon_{i,j,1} \\ Y_{ij} &= f_2(SEA_i) + \text{single}_{ij}\sigma_{upper} + X_i\delta_2 + \sum_{p=1} I_{i,p}\cdot\theta_{p2} + \varepsilon_{ij2} \end{aligned} \qquad [2]$$

In [2], single_{ij} is an indicator variable denoting whether a student attends a single-sex school, X_i is student sex, $I_{i,p}$ is an indicator variable equal to 1 if a student's rank ordering is preference group *p* and equal to zero otherwise[16], SEA_i is a fifth order polynomial in the student's total SEA score, and $(\overline{\overline{\text{single}}} \mid Rule_{ij})$ denotes whether the students simulated school assignment is single-sex. Simulated single-sex assignment $(\overline{\overline{\text{single}}} \mid Rule_{is})$ is the excluded instrument. Standard errors are clustered at the simulated school assignment level. The coefficient σ_{upper} from equation [2] should yield an upper-bound LATE of the effect of attending a single-sex school that is not subject to selection bias.

To see if the LATE is likely to be similar to the effect for the typical student, by testing for heterogeneity by the intensity of preferences for single sex schools, I estimate equation (2) for sub-samples based on the number of single sex school listed in their set of choices.

[16] Each preference group is defined by a distinct preference ordering of schools. All students who list schools A,B,C,D in that order form a group, while students who list schools B,A,C,D are in a different group because even though they have the same list of schools, the ordering is different. There are 22649 preference groups with more than one student. Among these groups, the average group has 63 students.

4.4 *Specification Tests and Falsification Tests*

To show that my identification strategy is valid, I first present evidence that the discontinuities created by the simulated school assignment mechanism are exogenous. The first test of the exogeneity of the cut-offs is to see if there is less density than would be expected by random chance right below a cut-off and more density right above the cut-off than would be expected by random chance. Such a pattern would be consistent with gaming of the cut-offs. Using the dataset from stacking all the cut-offs into one aggregate cut-off (as described in section 3.3) I test this possibility. Appendix Figure A2 shows the density of incoming test scores and the vertical line is the cut-off. There is little evidence of such a pattern visually. Following McCrary (2008), I test for discontinuity in the density of the total score at the simulated cut-off 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 cut-off” is a statistically and economically insignificant -0.003 (*p*-value=0.2) ─ suggests no gaming.

Another test of the exogeneity of the instrument is to see if scoring above a simulated cut-off or haveing a simulated single-sex assignment are associated with a shift in preferences. If the simulated assignments are exogenous, then preferences should be roughly balanced above and below the cut-off and there should be no difference in the selectivity of school choices for those assigned to single-sex schools conditional on school choices and student test scores. To test for differences in preferences through the simulated cut-offs, I regress the mean peer test scores at the first choice school on a fifth order polynomial in the relative score and an indicator variable denoting “above cut-off”. Such a model yields a coefficient on scoring above the threshold of -0.008 (se= 0.019). The same exercise with the second, third, and fourth choice schools yield similarly small and statistically insignificant coefficients (none yield *p*-values below 0.3). Testing the actual instruments, I test if having a simulated single-sex assignment is correlated with having preferences for more selective schools conditional on smooth functions of test scores and choice indicator variables. For the mean test score of the first, second, third, and fourth choices, the coefficients on "simulated same sex" are small and none yield *p*-values smaller than 0.4.

There is also no evidence of shifts in other observables (religion and primary school district) associated with the cut-offs or the simulated school assignments. Of the nineteen covariates tested across two models (38 regressions), all point estimates are economically small and none yield *p*-values below 0.05. Consistent with sampling variation only three yield *p*-values below 0.1. These results summarized in Appendix Table A1, show little evidence of any correlation between the instruments and student characteristics. Because religion is used by principals when hand-picking students at religious schools, the fact that religion is not correlated with the instruments lends credibility to the identification strategy.

Another test of the main rule-based instrumental variables strategy is if the results obtained using the different sourses of variation separately are similar ─ that is if results using only the discontinuity variation is similar to that obtained using only the difference in difference variation. To do this, I estimate the upper-bound results that use the discontinuity variation only under various bandwidth sizes and orders of the polynomial for control for the underlying SEA test scores. I also estimate upper bound results that use only the difference in difference variation (by including indicator variables for each distinct SEA score and not relying on any variation due to non-smooth or rapid changes in likelihood of attending a single-sex school through a cut-off). The upper bound estimates obtained using both distinct sources of variation are similar (detailed in Appendix Note 1) ─ suggesting that the main rule-based instrument that exploits both sources of variation is valid. The similarities across these two approaches has also been shown in Jackson (2010) when looking at the effects of school selectivity on student outcomes.

5 Results

Estimated Average Effects: To illustrate of the importance of addressing student self-selection in both observed and *unobserved* dimensions, I first present naive upper bound estimates of the effects of attending a single-sex school and then show how the results change as one takes various sources of selection bias into account. Table 2 presents the coefficient on attending a single-sex school on the main academic outcomes analyzed: Taking the CSEC exams (a proxy for not dropping out of school), the number of exams taken, the number of exams passed (a summary statistic for overall academic achievement that is sensitive to the CSEC taking margins and performance on the CSEC), passing the math CSEC exams, passing the English CSEC exams, and earning a certificate (i.e. passing 5 subjects including English and Math. This is the prerequisite to attending tertiary education). Given that Jackson (2010) documents that attending a school with higher-achieving peers is associated with better academic outcomes, I also include mean peer achievement as an outcome to give a sense of how much more selective single-sex schools are to the coeducational schools they are being compared to.

The results indicate that single-sex schools are much more selective than coeducational schools. Peer achievement is a full standard deviation higher, *on average*, for students who attend single-sex schools than those who do not. Not surprisingly, students who attend single-sex schools also have much better outcomes than those who do not. Conditional on incoming test scores and choice fixed effects there are still large differences in school selectivity and outcomes. Conditional on incoming test scores and choice fixed effects (third row), students who attend single-sex schools are exposed to peers with 0.5 standard deviations higher incoming test scores, are 40 percentage points more likely to take the CSEC exams (not drop out of school), take 2.47 more exams, pass 1.66 more exams, are 24 percentage points more likely to pass their CSEC English exam, are 20 percentage points more likely to pass their CSEC math exam, and are 17 percentage points more likely to earn a certificate (the prerequisites to tertiary education). Relative to the means in the population, these naively estimated effects are large.

Using the rule-based instrument to remove self-selection bias from selection on unobservables leads to a sizable reduction in the estimated benefits of attending a single-sex school ─ indicating that there is much positive selection in *unobservables* to single-sex schools. The 2SLS results indicate that after accounting for student self-selection, students who attend single-sex schools are exposed to peers with 0.336 standard deviations higher incoming test scores, are 7.3 percentage points more likely to take the CSEC exams (not drop out of school), take 0.736 more exams, pass 0.615 more exams, are 5.4 percentage points more likely to pass their CSEC English exam, are 5.4 percentage points more likely to pass their CSEC math exam, and are 5.7 percentage points more likely to earn a certificate (the prerequisites to tertiary education). These effects, while positive, are orders of magnitudes smaller than the OLS estimates, underscoring the importance of exploiting exogenous variation when analyzing the effects of single-sex schools. Because these models do not control for other school inputs, and single-sex schools are more selective, these estimates provide an upper-bound LATE of the direct effect of single-sex schools on outcomes for students who are on the margin of attending a single sex school.

Local Average Treatment Effects Conditional on School Selectivity: As stated previously, there is no credible way to isolate a single sex school effect from a school selectivity effect. However, it is instructive to see if single-sex schools have better outcomes compared to schools that are equally selective. Note that conditioning on school selectivity does not credibly isolate the single sex-school effect and likely yield a lower bound estimate because (a) single-sex schools being better may be reflected in their being more selective so that conditioning on selectivity would be "over-controlling" and, (b) if school selectivity is a near-perfect measure of school quality and if single-sex schools do improve outcomes then a single-sex school must have worse inputs than and equally selective coeducational school so that the "single sex" will be correlated with worse inputs and be downward biased. However, if school selectivity is a poor measure input quality, then unaccounted for input quality may be positively correlated with single sex schools so that the estimate is still biased upward. Because school selectivity and school value-added track very closely together in Trinidad and Tobago (Jackson 2010, 2011) models that condition on school selectivity likely yield a lower bound. However, because I cannot say this with certainty, conditional results must be interpreted with caution. The fact that there is positive selection to single-sex school even after accounting for school selectivity suggests that parent and students perceive some benefit to attending single sex schools above and beyond student selectivity. From the parent or student perspective, these conditional results are important.

In the bottom panel of Table 2, I present 2SLS results that condition of school selectivity (comparing students at single-sex schools to students at equally selective coeducational schools that have the same mean incoming student SEA scores). Because peer quality (average incoming SEA scores at the school) is a characteristic of the school and students self-select to schools, to deal with this self-selection to school selectivity, I instrument for peer quality at the school *actually* attended with the peer quality at the *simulated assigned* peer quality. These conditional effects yield results that are all close to zero, and not statistically significant. This implies that attending a single-sex school provides no benefit over attending an equally selective coeducational school. However because these results likely yield a lower bound, this does not mean that there is no single-sex school effect because the single-sex school effect may already be captured by these schools being more selective.

5.2 *Response Heterogeneity by Preferences for Single-sex Schools*

As mentioned previously, while only14 percent of students have strong preferences for single sex schools (list 3 of 4 schools) almost 60 percent of those who are used to estimate the Local Average Treatment Effect have strong preferences for single sex schools. If the response to attending single-sex schools varies with ones preferences for single sex schools then the positive upper-bound LATE may obscure an average treatment effect that is close to zero. To see if there is response heterogeneity be the intensity of preferences for single-sex schools I estimate the 2SLS upper and conditional effect for students who list 1,2,3, and 4 single-sex schools in their choices separately. I cannot estimate the effect for those who do not list any single-sex school choices because students who do not list ant single-sex schools in the choice list will not be *assigned* to a single-sex school based on the simulated assignment algorithm (they can however transfer to single-sex schools or be hand-picked by principals). I present the upper and conditional effects graphically in Figure 3 (the point estimates are in Appendix Table A2). I focus the analysis on the upper-bound effects.

Across all outcomes there is a pattern of upper bound effects close to zero for students who only list one or two single-sex schools with modest positive effects for students who list three single-sex schools, and large positive effects for students who list four single-sex schools. For all outcomes, one cannot reject the null hypothesis that the upper-bound effect of single sex schools is zero for students with one or two single sex school choices. Moreover, the upper bound effects are sufficiently small for students who list one and two single-sex schools that one can rule out any meaningful single-sex school effect for these students. This is important because only 13.5 percent of all students (34 percent of those who attend single-sex schools) list three or four single-sex schools ─ suggesting that for 85 percent of the student population there is no positive effect of attending a single sex school on academic outcomes.

Even though most students do not benefit from attending single-sex schools, for students with strong preferences for single-sex schools (list 3 or 4 schools), the upper-bound effects are large enough, and the estimates statistically significant enough, that one can rule out that there is no upper-bound single school effect for these students. In fact, the results show that even conditional on school selectivity, this group of students benefit from attending single sex schools. This conditional result is important because it provides a justification for the positive selection into single sex schools even conditional on the level of school selectivity (at least for those students and parents with strong preferences for single sex schools). The pattern of results suggests that students respond differently to different types of schools and select into schools that best match their learning style. This is important because the results are inconsistent with a world in which there are constant treatment effects for all students, and they show that the treatment effect on the treated is likely to provide a gross overestimate of the single-school effect for the entire population. In sum, the results show that only 13.5 percent of the full population may derive any benefit from attending singe sex schools. They also suggest that the positive upper bound LATE reflects a student-school match effect, rather than a generalizable benefit to attending single-sex schools.

5.3 *Effects by gender*

While findings are mixed, some observational studies have found that single-sex schools have large benefits for girls but not necessarily for boys ((Malacova 2007) (Spielhofer, Benton and Schagen 2004)). To assess the extent to which this might be true, I estimate the upper-bound effects and conditional effects by the number of single-sex school choices for males and females separately (appendix Tables A3 and A4). Figure 4 shows the upper-bound effect for each outcome separately for females and males. For all outcomes the upper-bound effect is larger for females than for males, and it is clear that for most outcomes almost all of the positive LATEs were driven by females. Specifically, for the important outcomes of interest (the number of exams passed and earning a certificate) one can reject the null hypothesis that the upper-bound treatment effect is zero for females with strong preferences for single sex schools (at the 1 percent level), while one cannot reject the null hypothesis that the upper-bound effect for males is zero for any preference intensity group (at the 20 percent level). Given that these are upper-bound estimates, the lack of either economic or statistical significance for males is telling.

In sum, the evidence suggests no benefit to attending single-sex schools for the vast majority of males (only 3 percent list 4 single-sex schools). In contrast, there are large positive single-sex school effects for females with 3 of 4 single-sex school choices (15 percent of females) but little effect for females with weak preferences for single-sex schools. Females that have 4 single-sex choices pass 2.8 more exams (p-value<0.01) and are 0.39 percentage points more likely to earn a certificate (p-value<0.05). For these girls, the effects persist even after conditioning on school selectivity; suggestive of a real positive effect for this subsample of girls. These results are consistent with the commonly held belief that single sex-schools improve the outcomes of girls but have little effect on the outcomes of boys.

5.4 *Effect of Single-sex Schooling on Course Selection*

One major justification for single-sex schools is that females may be more likely to take science and math courses in all-girls schools, and boys may be more likely to take traditionally female subjects in all-boys schools. The evidence on this is observational, and therefore only suggestive, and has yielded mixed results. From a macroeconomic standpoint if single-sex schooling causes students to pursue subjects (and careers) consistent with their innate talents, then single-sex schooling could lead to greater allocative efficiency in the economy even with no effect on average achievement. To assess whether attending a single-sex school has an effect on the course selections of students, I present both the upper-bound and conditional 2SLS estimates on the number of female courses, and science classes taken by preference strength for males and females separately in Figure 5. The upper-bound results suggest that attending a single-sex school increases the number of female dominated subjects taken for both males and females. As with the other outcomes, this effect is increasing in the number of single-sex schools the students lists and the effects are large and statistically significant for females while they are small and not statistically significant for boys. Looking to the number of science subjects taken the upper bound results suggest that attending a single-sex school decreases the number of sciences taken for all females except those with four choices (where the effect is not distinguishable from zero) while there is no systematic or statistically significant effect on the number of science courses taken by boys. The pattern of results indicate that for females that do not have strong preferences for single-sex schools, attending an all-girls school reduces the number of science courses taken by 0.6.

In sum, the results do not support the notion that single-sex schools help reduce gender differences in course selection. In fact the results show that females take more female dominated courses and take fewer science courses in single-sex schools. The results are inconsistent with the argument that single-sex schooling reduces traditional gender roles and that girls are more likely to take math and science courses at single-sex schools.

6 *Conclusions*

Single-sex schooling is prevalent in many nations, and is becoming increasingly popular in the United States. Whether single-sex schooling improves student outcomes has large and important implications for the macro-economy as a large single-sex schooling effect on student achievement would imply that that by merely reorganizing the education system (i.e. reshuffling students across schools to obtain complete sex segregation) human capital can be increased with no additional expenditure. Also, if single-sex schooling makes it more likely that students pursue subjects and careers consistent with their innate talents, then single-sex schooling could lead to greater allocative efficiency in the economy even with no effect on achievement. Given that government spending on education constitutes more than 5 percent of GDP in most industrialized nations and human capital is responsible about one third of all economic output the potential cost savings or increases in human capital or allocative efficiencies could have important macroeconomic effects.

Despite the important policy implications associated with whether single-sex schools improve educational outcomes and psychological, sociological, and biological reasons why both boys and girls *might* benefit from attending single-sex schools, there is no clean credible evidence that single-sex schooling actually improves student outcomes. Previous studies on the effect of attending a single-sex school have either not accounted for student selection to schools in unobserved dimensions or the fact that single-sex schools and coeducational schools are typically incomparable in terms of curriculum, input quality, or selectivity. Owing to the unique setup of the education system and the data in Trinidad and Tobago, I am able to deal with some of the challenges plaguing the extant literature. Moreover, I am able to test for heterogeneous treatment effects by the intensity of the desire to attend single-sex schools—allowing me to say something meaningful about (a) how different the treatment effect for the treated is from the average treatment effect and (b) gain a credible prediction of the effect of a policy that replaced all coeducations schools with single-sex schools.

I find that a failure to account for student selection can lead to large spurious positive effects of attending single-sex schools. Once student selection is accounted for, I find the upper-bound local average local treatment effect of single sex schools (i.e. the effect of attending a single sex school for those students who typically attend single sex schools) is positive. However, these overall results mask considerable heterogeneity across students, such that those students who exhibit strong preferences for single-sex schools (and are more likely to be marginal) enjoy large benefits to attending single-sex schools, while for most students (who have weak or modest preferences for single-sex schools) there is little or no effect on student achievement. That is, I can rule out positive single-sex school effects for more than eighty percent of the student population.

Looking for heterogeneity by gender reveals that much of the benefit to attending single-sex schools was driven by large effects for those girls with strong preferences for single-sex schools, while the effects for boys are small. Looking to course selection, the results show that females take more female dominated courses and take *fewer* science courses in single-sex schools.

From a policy perspective, the results suggest that single-sex schools may improve the academic outcomes only for those students who tend to select to single-sex schools. More importantly, the results also suggest that most students do not benefit from attending single-sex schools. As such, while the results suggest that a school choice policy that included single-sex schools as an <u>option</u> for students is likely to improve student outcomes, the results suggest that an expansion of single-sex schools to a large segment of the population may have little benefit. The heterogeneous treatment effects also suggest that the single sex-school effect is driven by a student-school match rather than a technological advance that would benefit all students.

There is little evidence that single-sex schools lead to a more efficient allocation of talent to subject areas or may be an effective tool for increasing female representation in math, science fields. While the results suggest that single-sex school may not be effective for most students, single-sex school attendance could have effects on important social outcomes not measured in this study. Also, because this study looks at secondary schools, they do not speak to the possible benefits of single-sex elementary schools or single-sex colleges. However, given that I find evidence of substantial positive self-selection to single sex schools, and I find that the benefits are much larger for the typical student who attends a single-sex school than the average student, the results suggest that policy-makers should be skeptical in their reading of studies on single-sex schools using observational data, and that studies based on credible research designs but only identify the effects among school *applicants* should be interpreted with caution. It is evident that we need more studies using credible empirical designs to deepen our understanding of single-sex schools. This paper represents a useful first step in this direction.

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Table 1: *Summary Statistics*

Summary Statistics	All Students				All Students With a Simulated Assignment			
	Attend all-girls	Attend mixed: Female	Attend all boys	Attend mixed: Male	Assigned all-girls	Assigned mixed: Female	Assigned all boys	Assigned mixed: Male
Normalized SEA Score	0.836	-0.074	0.792	-0.343	1.498	-0.041	1.315	-0.316
	(0.868)	(0.882)	(0.882)	(0.938)	(0.457)	(0.862)	(0.569)	(0.922)
Female	1.000	1.000	0.000	0.000	1.000	1.000	0.000	0.000
	(0.0)	(0.0)	(0.0)	(0.0)	(0.0)	(0.0)	(0.0)	(0.0)
Take CSEC	1.000	0.564	1.000	0.459	0.903	0.630	0.873	0.519
	(0.0)	(0.496)	(0.0)	(0.498)	(0.296)	(0.483)	(0.333)	(0.5)
Exams Taken	6.582	3.339	6.288	2.464	6.641	3.736	6.153	2.795
	(1.607)	(3.15)	(1.77)	(2.919)	(2.395)	(3.119)	(2.633)	(2.969)
Exams Passed	5.121	1.809	4.290	1.053	6.178	2.094	5.002	1.228
	(2.707)	(2.462)	(2.822)	(1.985)	(2.58)	(2.569)	(2.874)	(2.1)
Pass CSEC	0.782	0.311	0.669	0.163	0.860	0.360	0.756	0.194
	(0.413)	(0.463)	(0.471)	(0.37)	(0.347)	(0.48)	(0.429)	(0.395)
Pass CSEC	0.620	0.184	0.597	0.130	0.825	0.214	0.721	0.152
	(0.485)	(0.388)	(0.491)	(0.336)	(0.38)	(0.41)	(0.448)	(0.359)
Certificate	0.568	0.141	0.481	0.078	0.796	0.166	0.633	0.092
	(0.495)	(0.348)	(0.5)	(0.268)	(0.403)	(0.372)	(0.482)	(0.288)
Female Subjects	3.099	1.362	2.171	0.657	3.004	1.590	2.300	0.765
	(1.365)	(1.576)	(1.452)	(1.139)	(1.566)	(1.642)	(1.471)	(1.217)
Female Subjects	2.473	0.810	1.566	0.332	2.810	0.977	1.855	0.396
	(1.609)	(1.292)	(1.426)	(0.814)	(1.618)	(1.396)	(1.451)	(0.886)
Sciences Taken	1.097	0.419	1.283	0.349	1.485	0.456	1.487	0.399
	(1.147)	(0.836)	(1.247)	(0.775)	(1.229)	(0.852)	(1.343)	(0.813)
Sciences Passed	0.929	0.229	0.949	0.173	1.393	0.260	1.245	0.199
	(1.13)	(0.618)	(1.222)	(0.564)	(1.232)	(0.647)	(1.335)	(0.598)
Hard Sciences	0.443	0.145	0.671	0.153	0.710	0.150	0.838	0.174
	(0.762)	(0.477)	(0.873)	(0.476)	(0.876)	(0.482)	(0.929)	(0.503)
Hard Sciences	0.381	0.066	0.495	0.061	0.664	0.071	0.705	0.070
	(0.727)	(0.326)	(0.813)	(0.317)	(0.867)	(0.337)	(0.899)	(0.338)
Observations	24648	87625	19689	86642	12162	67903	11961	60231

Notes: Earning a certificate is a prerequisite to entering tertiary education and it entails passing 5 CSEC exams including English and math. Sciences subject are biology, chemistry, physics, information technology, and integrated sciences. Hard Science subjects are chemistry and physics. Female subjects are English literature, Caribbean history, biology, integrated sciences, French, Spanish, principles of accounts, principles of business, Information technology, Office procedures, Food and nutrition, typewriting, home economics, shorthand, and clothing and textiles.

Table 2

	1	2	3	4	5	6	7
	Peer Achievement	Take the CSEC exams	CSEC Exams Taken	CSEC Exams Passed	Passed CSEC English	Passed CSEC Math	Earned Certificate
	No controls (218604 obs.)						
Single-sex	1.027	0.488	3.547	3.319	0.496	0.454	0.421
(upper bound)	[0.064]**	[0.049]**	[0.217]**	[0.205]**	[0.023]**	[0.035]**	[0.043]**
	Fifth order polynomial in SEA scores (218604 obs.)						
Single-sex	0.501	0.375	2.266	1.493	0.224	0.181	0.152
(upper bound)	[0.027]**	[0.031]**	[0.148]**	[0.081]**	[0.015]**	[0.011]**	[0.013]**
	Fifth order polynomial in SEA scores and preference fixed Effects (218584 obs.)						
Single-sex	0.54	0.407	2.471	1.664	0.244	0.201	0.171
(upper bound)	[0.038]**	[0.028]**	[0.139]**	[0.109]**	[0.018]**	[0.016]**	[0.018]**
	2SLS: Fifth order polynomial in SEA scores and preference fixed effects (179710 obs. [a])						
Single-sex	0.336	0.073	0.736	0.615	0.054	0.054	0.057
(upper bound)	[0.054]**	[0.018]**	[0.163]**	[0.154]**	[0.020]**	[0.017]**	[0.025]*
	2SLS: Fifth order polynomial in SEA scores, preference fixed effects, and peer quality (179710 obs.)						
Single-sex	-	-0.039	-0.158	0.009	-0.008	-0.001	-0.011
(conditional)	-	[0.040]	[0.216]	[0.145]	[0.022]	[0.018]	[0.020]

Standard errors in brackets adjusted for clustering at the simulated assigned school level.
a. Sample sizes are smaller in the 2SLS models because some observations are dropped due to co-linearity.
+ significant at 10%; * significant at 5%; ** significant at 1%

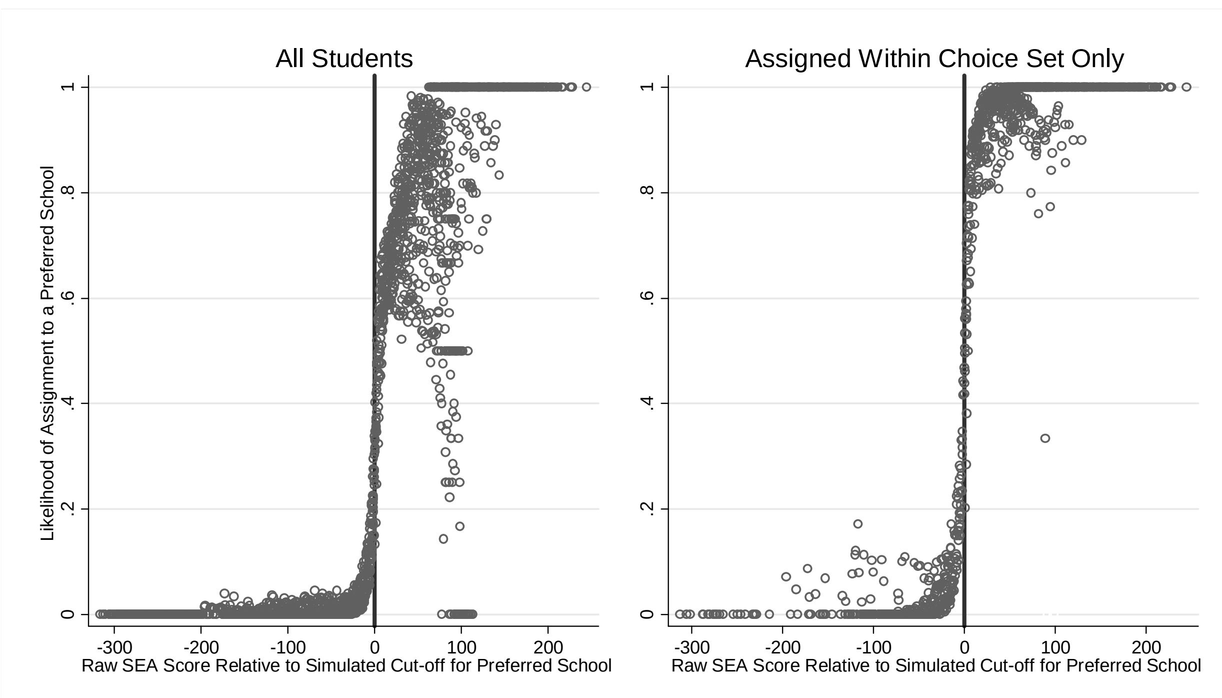


Figure 1. *Likelihood of Being Assigned to a Preferred School*

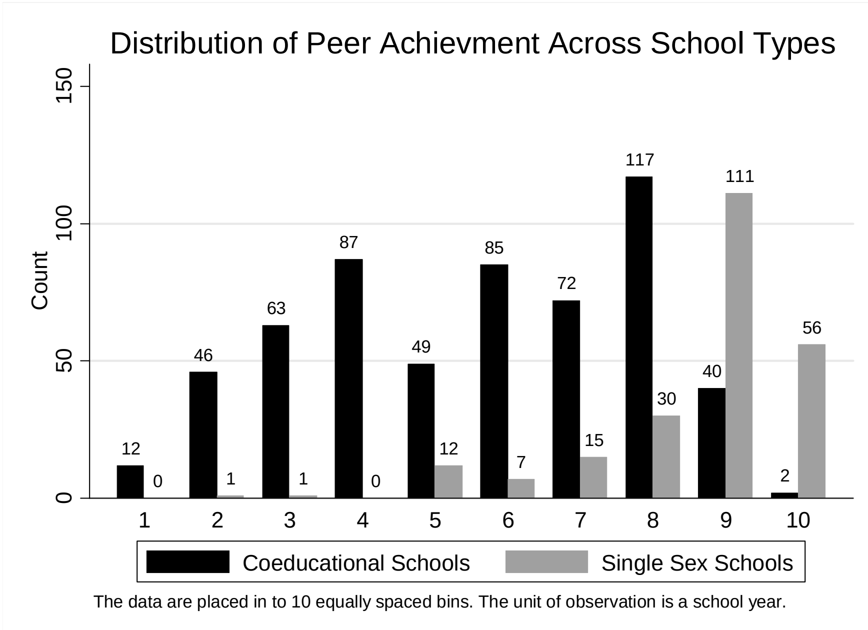


Figure 2: *Distribution of Peer Achievement Across School Types*

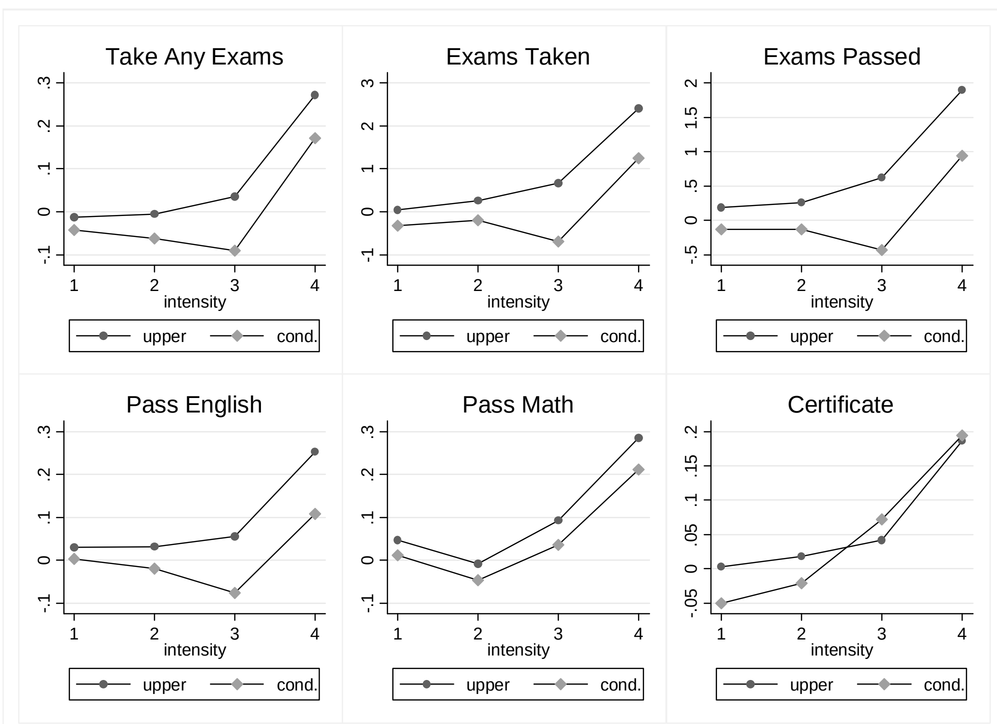
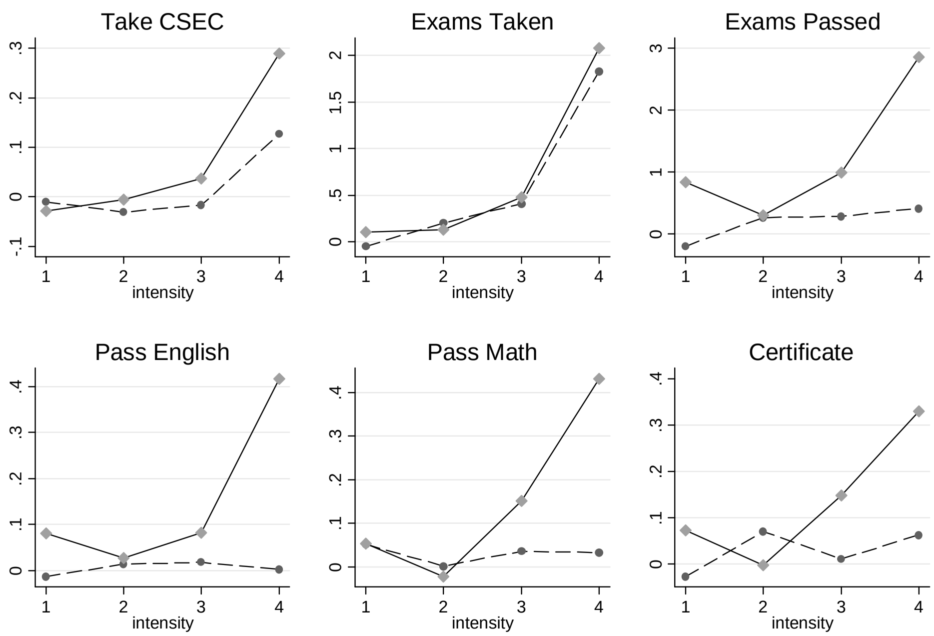


Figure 3: *Effects by intensity of preference for single-sex schools*



Females are solid lines and Males are dashed lines.

Figure 4: *Upper bound effects by gender and preference intensity*

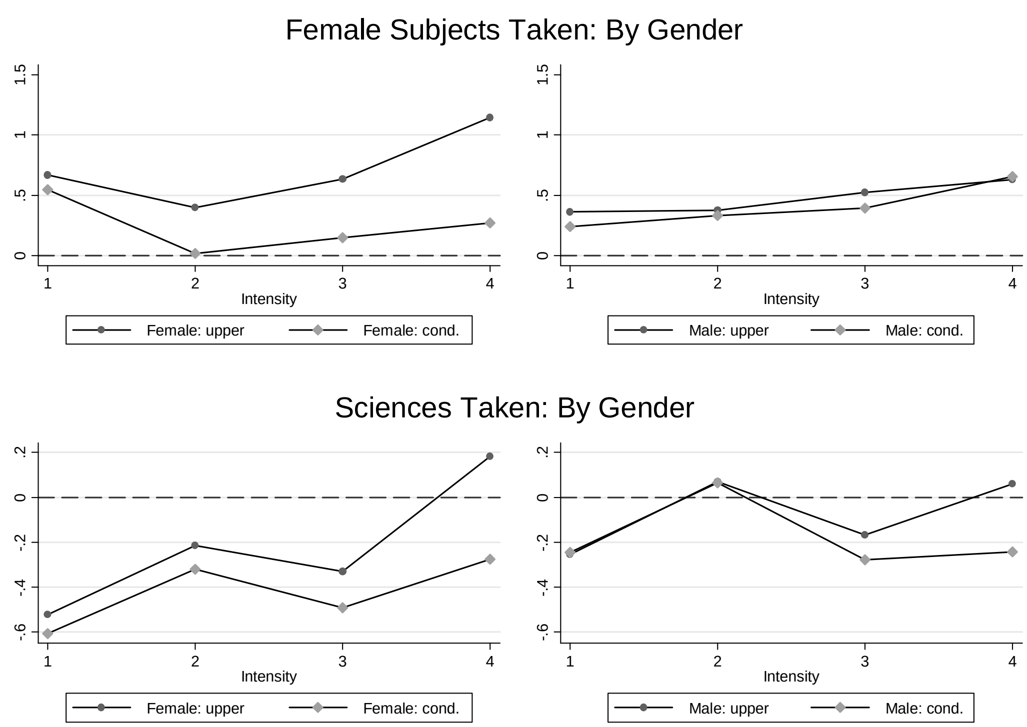


Figure 5: *Effect on course taking by gender*

Appendix

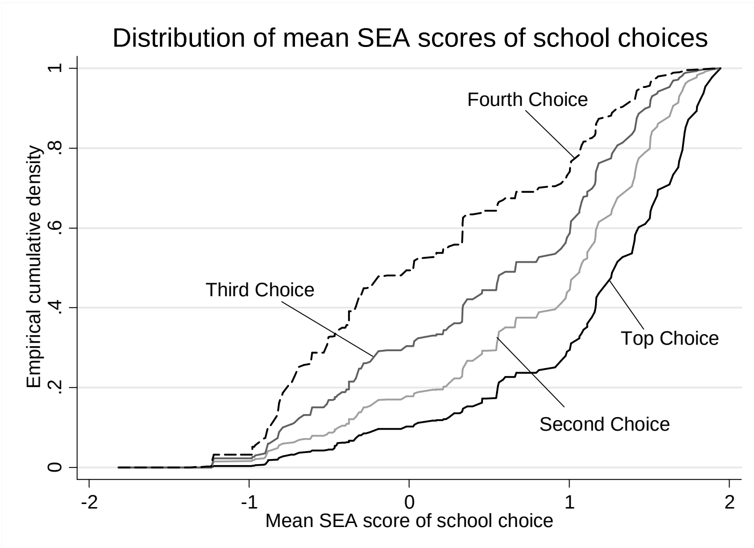


Figure A1: *Distribution of Peer Quality by School Choice Rank*

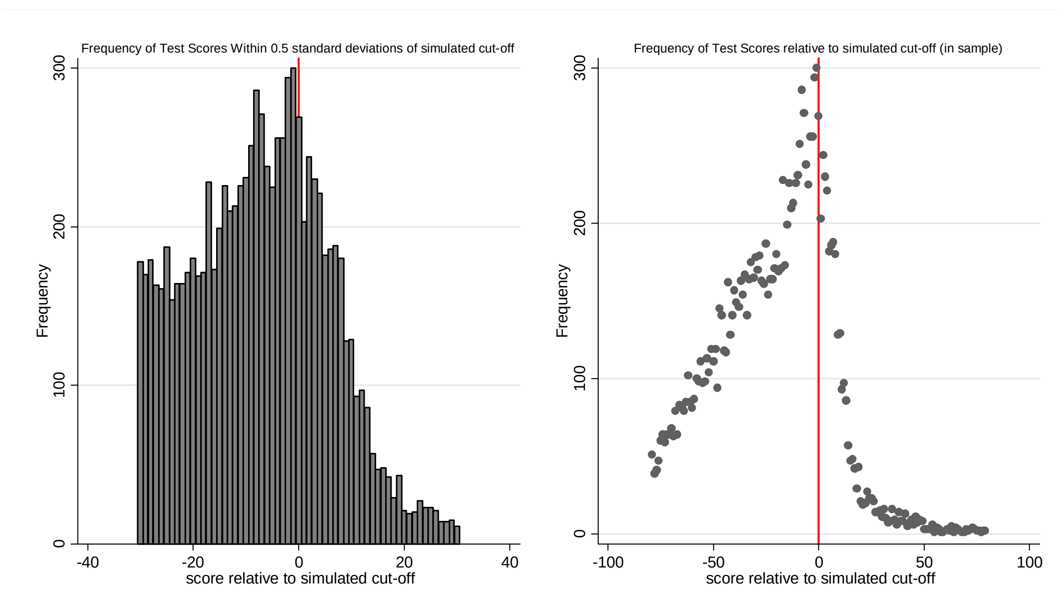


Figure A2: *Test for Smoothness Through the Simulated Cut Offs*

Table A1

	Coefficient on:			Coefficient on:			Coefficient on:	
	Above Simulated cut-off	Simulated Same sex		Above Simulated cut-off	Simulated Same sex		Above Simulated cut-off	Simulated Same sex
	-0.008	-0.003	District 1	0.008	0.001	Religion 1	0.004	0.002
Peer Scores at Choice 1	[0.019]	[0.006]		[0.027]	[0.003]		[0.012]	[0.004]
	0.017	-0.001	District 2	0.014	0.002	Religion 2	-0.003	0.005
Peer Scores at Choice 2	[0.021]	[0.009]		[0.042]	[0.001]		[0.007]	[0.004]
	0.004	<0.001	District 3	-0.021	**-0.005**	Religion 3	-0.002	-0.008
Peer Scores at Choice 3	[0.031]	[0.007]		[0.029]	**[0.003]+**		[0.017]	[0.007]
	-0.01	-0.006	District 4	-0.007	0.003	Religion 4	-0.002	<0.001
Peer Scores at Choice 4	[0.048]	[0.007]		[0.055]	[0.003]		[0.003]	[0.001]
			District 5	-0.006	0.002	Religion 5	0.01	0.001
				[0.030]	[0.002]		[0.010]	[0.005]
			District 6	0.004	-0.003	Religion 6	0.012	0.001
				[0.011]	[0.002]		[0.010]	[0.001]
			District 7	0.008	<0.001	Religion 7	-0.003	**-0.018**
				[0.032]	[0.003]		[0.008]	**[0.010]+**
			District 8	-0.001	<0.001	Religion 8	-0.013	0.002
				[0.001]	[0.000]		[0.015]	[0.004]
						Religion 9	-0.006	0.003
							[0.006]	[0.003]
						Religion 10	-0.002	**0.014**
							[0.001]	**[0.009]+**
						Religion 11	0.005	-0.001
							[0.004]	[0.001]

Robust standard errors in brackets adjusted for clustering at the assigned school level.
+ significant at 10%; * significant at 5%; ** significant at 1%

Each estimate represents a separate regression of the simulated instruments (scoring above the simulated cut off or the simulated same sex assignment) on a separate covariate.

Appendix Table A2: *Estimated effects by Intensity of Preferences for Single-Sex Schools*

	Take CSEC				Exams Taken			
	1 choice	2 choices	3 choices	4 choices	1 choice	2 choices	3 choices	4 choices
Single	-0.012	-0.006	0.035	0.272	0.039	0.261	0.673	2.417
upper	[0.037]	[0.032]	[0.023]	[0.071]**	[0.268]	[0.216]	[0.181]**	[0.589]**
Single	-0.043	-0.062	-0.091	0.172	-0.33	-0.194	-0.696	1.246
cond.	[0.046]	[0.028]*	[0.039]*	[0.079]*	[0.299]	[0.181]	[0.325]*	[0.309]**
Observation	44460	29615	17396	6597	44460	29615	17396	6597

	Exams Passed				Pass English			
	1 choice	2 choices	3 choices	4 choices	1 choice	2 choices	3 choices	4 choices
Single	0.187	0.258	0.623	1.898	0.03	0.032	0.056	0.253
upper	[0.259]	[0.200]	[0.203]**	[0.458]**	[0.045]	[0.039]	[0.025]*	[0.042]**
Single	-0.13	-0.133	-0.433	0.937	0.003	-0.02	-0.077	0.108
cond.	[0.240]	[0.166]	[0.365]	[0.552]+	[0.042]	[0.037]	[0.050]	[0.063]+
Observation	44460	29615	17396	6597	44460	29615	17396	6597

	Pass Math				Certificate			
	1 choice	2 choices	3 choices	4 choices	1 choice	2 choices	3 choices	4 choices
Single	0.047	-0.008	0.092	0.285	0.003	0.018	0.042	0.186
upper	[0.026]+	[0.033]	[0.036]*	[0.085]**	[0.033]	[0.035]	[0.052]	[0.068]**
Single	0.011	-0.047	0.035	0.212	-0.05	-0.021	0.072	0.194
cond.	[0.024]	[0.031]	[0.069]	[0.104]*	[0.031]+	[0.030]	[0.073]	[0.134]
Observation	44460	29615	17396	6597	44460	29615	17396	6597

Standard errors in brackets clustered at the simulated school level.
+ significant at 10%; * significant at 5%; ** significant at 1%

Each estimate comes from a separate instrumental variables regression. The upper bound estimates include no controls for school selectivity, while the conditional effects include mean peer quality as a covariate.

Table A3: *Effects by Intensity of Preferences for Males*

	Males									
	Take CSEC					Exams Taken				
	All	1 choice	2 choices	3 choices	4 choices	All	1 choice	2 choices	3 choices	4 choices
Single attend upper	0.012	-0.01	-0.031	-0.017	0.127	0.461	-0.049	0.203	0.409	1.831
	[0.023]	[0.040]	[0.043]	[0.043]	[0.056]*	[0.215]*	[0.382]	[0.287]	[0.288]	[0.605]**
Single attend cond.	-0.048	-0.045	-0.051	-0.07	0.12	-0.042	-0.363	0.021	-0.364	1.375
	[0.029]	[0.040]	[0.035]	[0.034]*	[0.073]	[0.185]	[0.348]	[0.241]	[0.263]	[0.355]**
Observations	84875	21353	13797	7723	2700	84875	21353	13797	7723	2700

	Exams Passed					Pass English				
	All	1 choice	2 choices	3 choices	4 choices	All	1 choice	2 choices	3 choices	4 choices
Single attend upper	0.362	-0.2	0.264	0.286	0.412	0.014	-0.013	0.014	0.018	0.003
	[0.214]+	[0.342]	[0.285]	[0.438]	[0.634]	[0.032]	[0.044]	[0.052]	[0.049]	[0.100]
Single attend cond.	0.056	-0.402	0.109	-0.269	0.173	-0.008	-0.03	-0.003	-0.074	0.026
	[0.162]	[0.305]	[0.238]	[0.340]	[0.625]	[0.025]	[0.040]	[0.045]	[0.048]	[0.128]
Observations	84875	21353	13797	7723	2700	84875	21353	13797	7723	2700

	Pass Math					Certificate				
	All	1 choice	2 choices	3 choices	4 choices	All	1 choice	2 choices	3 choices	4 choices
Single attend upper	0.048	0.052	0.002	0.036	0.033	0.076	-0.027	0.07	0.011	0.063
	[0.027]+	[0.038]	[0.039]	[0.059]	[0.079]	[0.037]*	[0.055]	[0.046]	[0.102]	[0.072]
Single attend cond.	0.02	0.021	-0.004	-0.023	0.008	0.039	-0.06	0.059	0.016	0.048
	[0.022]	[0.036]	[0.038]	[0.056]	[0.094]	[0.029]	[0.048]	[0.042]	[0.086]	[0.103]
Observations	84875	21353	13797	7723	2700	84875	21353	13797	7723	2700

Standard errors in brackets clustered at the simulated school level.
+ significant at 10%; * significant at 5%; ** significant at 1%

Each estimate comes from a separate instrumental variables regression. The upper bound estimates include no controls for school selectivity, while the conditional effects include mean peer quality as a covariate.

Table A4: *Effects by Intensity of Preferences for Females*

	Females									
	Take CSEC					Exams Taken				
	All	1 choice	2 choices	3 choices	4 choices	All	1 choice	2 choices	3 choices	4 choices
Single attend upper	0.132	-0.029	-0.006	0.037	0.289	0.986	0.104	0.128	0.48	2.078
	[0.032]**	[0.053]	[0.041]	[0.041]	[0.106]**	[0.202]**	[0.338]	[0.235]	[0.293]	[0.647]**
Single attend cond.	-0.03	-0.043	-0.093	-0.134	0.21	-0.23	-0.248	-0.525	-1.141	1.51
	[0.054]	[0.072]	[0.032]**	[0.073]+	[0.122]+	[0.301]	[0.422]	[0.167]**	[0.553]*	[1.018]
Observations	90573	23099	15816	9673	3897	90573	23099	15816	9673	3897

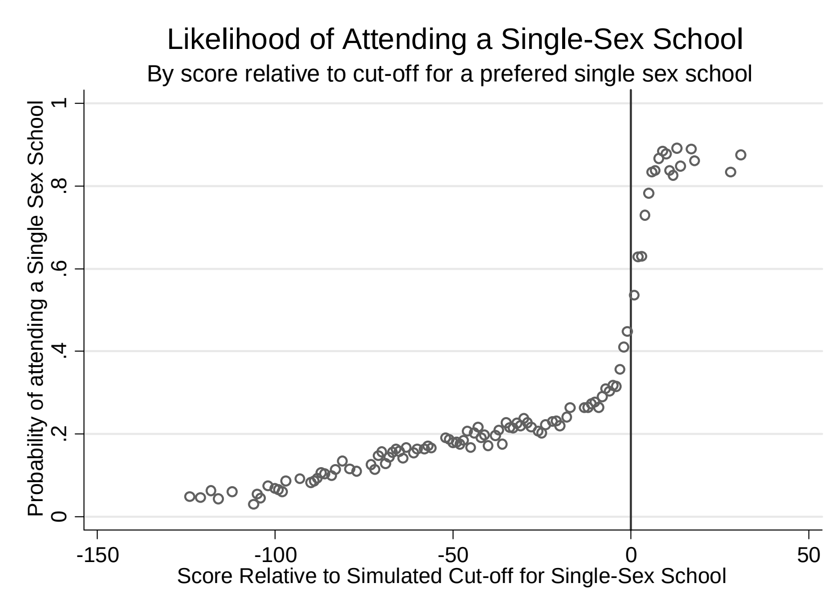
	Exams Passed					Pass English				
	All	1 choice	2 choices	3 choices	4 choices	All	1 choice	2 choices	3 choices	4 choices
Single attend upper	1.011	0.835	0.298	0.988	2.857	0.115	0.081	0.027	0.082	0.417
	[0.167]**	[0.311]**	[0.215]	[0.265]**	[0.567]**	[0.025]**	[0.044]+	[0.038]	[0.042]*	[0.108]**
Single attend cond.	0.129	0.402	-0.269	-0.382	3.243	0.006	0.046	-0.047	-0.051	0.488
	[0.166]	[0.298]	[0.162]+	[0.575]	[1.249]**	[0.025]	[0.037]	[0.035]	[0.076]	[0.201]*
Observations	90573	23099	15816	9673	3897	90573	23099	15816	9673	3897

	Pass Math					Certificate				
	All	1 choice	2 choices	3 choices	4 choices	All	1 choice	2 choices	3 choices	4 choices
Single attend upper	0.065	0.054	-0.022	0.152	0.432	0.068	0.073	-0.002	0.148	0.329
	[0.025]**	[0.035]	[0.050]	[0.051]**	[0.155]**	[0.022]**	[0.030]*	[0.045]	[0.048]**	[0.146]*
Single attend cond.	-0.02	0.021	-0.097	0.155	0.491	-0.027	0.002	-0.065	0.222	0.391
	[0.029]	[0.035]	[0.045]*	[0.142]	[0.257]+	[0.026]	[0.033]	[0.041]	[0.156]	[0.274]
Observations	90573	23099	15816	9673	3897	90573	23099	15816	9673	3897

Standard errors in brackets clustered at the simulated school level.
+ significant at 10%; * significant at 5%; ** significant at 1%
Each estimate comes from a separate instrumental variables regression. The upper bound estimates include no controls for school selectivity, while the conditional effects include mean peer quality as a covariate.

Appendix Note 1: *Results using discontinuity variation and difference in difference variation separately.*

Based on the stacked dataset described in section 3.3, I can create a sub-sample of cut-offs for preferred single-sex schools to see if scoring above the simulated cut-off for a preferred single-sex school is associated with a sudden increase in the likelihood of attending a single-sex school. This is shown in the figure above. This figure presents visual evidence of a relatively rapid increase in the likelihood of attending a single-sex school through the simulated cut-off for a preferred single-sex school. However, the figure also shows that the increase in likelihood is somewhat smooth, suggesting that results using variation through the cut-offs may be sensitive to how one controls for smoothness through the cut-offs, making it suboptimal for a fuzzy regression discontinuity type design. In any case, given that a rapid increase in likelihood does exists, it is worthwhile to see what a fuzzy RD type design might yield, and to see if the results are similar to what one obtains using other sources of clean variation.



Using the stacked dataset, I use scoring above the cut-off for a preferred single-sex school as an instrument for attending a single-sex. Specifically, I estimate (a) by 2SLS.

$$\begin{aligned} \text{single}_{ij} &= f(SEA_i) + Above_{ij} \cdot \varphi_1 + \nu_{j1} + \varepsilon_{ij1} \\ Y_{ij} &= f(SEA_i) + \text{single}_{ij}\sigma_{j,2} + \nu_{j2} + \varepsilon_{ij2} \end{aligned} \quad . \qquad \text{(a)}$$

All variables are defined as before, $Above_{ij}$ is an indicator variable that is equal to *1* if student *i* has a SEA score above the simulated cut-off for single-sex school *j* and *0* otherwise, and ν_j is a fixed effect for each cut-off (preferred school) to account for the fact that students in the admission pool for the top single-sex school may have very different characteristics as those in the applicant pool for a less selective single-sex school.[17] The excluded instrument yields a first stage F-statistic of 173.2 in predicting attending a single-sex school. It is worth noting that while the setup looks a lot like a fuzzy-regression discontinuity approach, it is not. Because the location of the discontinuities are not known, they are simulated. This introduces additional noise. As such, this strategy is best described as an instrumental variables strategy that lives of the discontinuities inherent in the assignment process.

To show the range of results on obtains under different choices of bandwidth and different polynomial orders of the SEA scores. These results are presented in the table below. As one can see the estimates do vary depending on the modeling assumption made. However, the pattern of the results are robust for the number of SEA exams passed and the earning a certificate. Moreover, for all the outcomes, the range of point estimates are similar to those obtained using the full rule based instrumental variables model.

[17] A fifth order polynomial of the SEA score was chosen by starting with a linear model and increasing the order of the polynomial until the last order is no longer statistically significant at the 5 percent level. As such, one can reject a model with an order less than a fifth order polynomial at the 5 percent level. While not presented in this paper, all results are robust to using a fourth, fifth, or sixth order polynomial.

Take		Exams Taken		Exams Passed		Math Pass		English Pass		Certificate		Obs.	Bandwidth	Polynomial of SEA scores
-0.05	[0.048]	0.367	[0.362]	0.895	[0.387]*	-0.049	[0.061]	-0.017	[0.063]	0.101	[0.071]	63125	50	5
-0.051	[0.043]	0.288	[0.327]	0.803	[0.345]*	-0.049	[0.056]	-0.038	[0.058]	0.07	[0.064]	63125	50	4
-0.02	[0.027]	0.362	[0.217]+	0.79	[0.248]**	-0.091	[0.061]	-0.044	[0.042]	0.095	[0.047]*	63125	50	3
-0.026	[0.026]	0.272	[0.209]	0.707	[0.236]**	-0.092	[0.035]**	-0.048	[0.038]	0.085	[0.047]+	63125	50	2
-0.094	[0.035]**	-0.183	[0.249]	0.501	[0.286]+	-0.072	[0.040]+	-0.078	[0.048]	0.024	[0.056]	96274	100	5
-0.016	[0.029]	0.495	[0.210]*	1.023	[0.249]**	-0.048	[0.033]	-0.038	[0.042]	0.127	[0.049]**	96274	100	4
0.003	[0.025]	0.415	[0.194]*	0.602	[0.242]*	-0.066	[0.041]+	-0.117	[0.041]**	0.023	[0.051]	96274	100	3
0.13	[0.024]**	0.953	[0.167]**	0.411	[0.190]*	-0.142	[0.024]**	-0.125	[0.032]**	0.035	[0.038]	96274	100	2
-0.035	[0.028]	0.278	[0.201]	0.755	[0.252]**	-0.045	[0.032]	-0.083	[0.042]+	0.057	[0.050]	104229	200	5
-0.008	[0.028]	0.481	[0.208]*	0.922	[0.249]**	-0.056	[0.032]+	-0.059	[0.043]	0.107	[0.050]*	104229	200	4
0.102	[0.026]**	0.847	[0.179]**	0.467	[0.220]*	-0.092	[0.028]**	-0.141	[0.038]**	0.002	[0.046]	104229	200	3
0.098	[0.021]**	0.773	[0.141]**	0.395	[0.197]*	-0.171	[0.026]**	-0.087	[0.036]*	0.089	[0.042]*	104229	200	2
-0.026	[0.026]	0.3	[0.195]	0.775	[0.255]**	-0.047	[0.033]	-0.073	[0.043]+	0.072	[0.050]	104887	300	5
0.006	[0.027]	0.511	[0.201]*	0.863	[0.249]**	-0.062	[0.033]+	-0.068	[0.043]	0.099	[0.050]*	104887	300	4
0.097	[0.026]**	0.829	[0.176]**	0.524	[0.221]*	-0.089	[0.028]**	-0.12	[0.039]**	0.026	[0.046]	104887	300	3
0.1	[0.021]**	0.763	[0.140]**	0.351	[0.202]+	-0.18	[0.027]**	-0.093	[0.037]*	0.083	[0.042]*	104887	300	2

Standard errors in brackets are adjusted for clustering at the assigned school level.
+ significant at 10%; * significant at 5%; ** significant at 1%

To capture only the difference in difference (DID) variation obtained by looking at the difference in scores for students with exactly the same test scores but who attend different schools because of difference in their school choices, I use a DID-2SLS strategy that estimates the effect of attending a single-sex school after controlling for a full set of preference indicator variables, a full set of test score indicator variables (i.e. an indicator variable for each distinct total SEA score for each test year- there are 1430 such values). Including indicator variables for each distinct test score removes all variation due to sudden changes in outcome through a cut-off. the upper bound estimates are presented in the table below.

	1	2	3	4	5	6
	Take CSEC	Exams Taken	Exams Passed	Pass Math	Pass English	Certificate
Single-sex	0.024	0.372	0.408	0.04	0.051	0.058
	[0.021]	[0.220]+	[0.178]*	[0.032]	[0.036]	[0.025]*
Observations	152242	152242	152242	152242	152242	152242

Standard errors in brackets
+ significant at 10%; * significant at 5%; ** significant at 1%

The results are similar across both models that exploit different sources of variation. While the discontinuity estimates are noisy and are sensitive to the bandwidth chosen and exactly how one controls for incoming test scores, the discontinuity results show a consistent positive effect on the number of exams taken between 0.25 and 0.8, a consistent positive effect on the number of exams passed between 0.4 and 1, and a consistent positive effect on the likelihood of earning a certificate of between 3 and 10 percentage points. These are consistent with the statistically significant positive effects obtain using the DID variation in the number of exams taken, exams passed, and earning a certificate. The statistically insignificant DID effect for taking the CSEC is consistent with the discontinuity estimates that range from -0.094 to +0.097. The only outcomes for which there is some slight disagreement across models is for passing math and English. While the discontinuity results suggest that students at single-sex schools rare slightly worse in single-sex schools in the likelihood of passing math and English, the DID results suggests there is no effect. By and large the results are consistent across the two sources of variation and indicate that students who attend single-sex schools pass more exams and are more likely to earn a certificate. Moreover, the fact that I find strong evidence of response heterogeneity, slight difference across models would be expected.