Single-Sex Schools, Student Achievement, and Course Selection: Evidence from Rule-Based Student Assignments in Trinidad and Tobago

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Existing studies on single-sex schooling suffer from biases because students who attend single-sex schools differ in unmeasured ways from those who do not. In Trinidad and Tobago students are assigned to secondary schools based on an algorithm allowing one to address self-selection bias and estimate the causal effect of attending a single-sex school versus a similar coeducational school. While students (particularly females) with strong expressed preferences for single-sex schools benefit, *most* students perform no better at single-sex schools. Girls at single-sex-schools take fewer sciences courses and more traditionally female subjects.

The merits of single-sex schooling have been fiercely debated in European, Latin American, and Caribbean nations where single-sex schools are prevalent. This debate has recently been ignited in the US with the passage of Title IX[1] regulations making it easier for school districts to provide single-sex schools (Weil 2008, Medina 2009). If students have better academic outcomes in single-sex than coeducational schools, then overall educational attainment can be increased by merely shuffling students across schools to achieve sex-segregation. Also, if single-sex schooling reduces gendered course taking, it may lead to more efficient allocations of talent to courses and improved matching of workers to occupations. Under these scenarios, by making all schools single-sex, with no increased spending one can have a better educated population and cost-savings that can be put into productive sectors of the economy.

Using data from Trinidad and Tobago I aim to answer the following questions: (1) Does attending a single-sex secondary school improve student academic outcomes? (2) Do students with stronger preferences for single-sex schools experience larger benefits? (3) Do the effects vary by gender? (4) Do single-sex schools affect the course selection of girls and boys? This context is attractive for studying single-sex schools because about one quarter of public secondary schools are single-sex and institutional details allow one to remove self-selection bias while comparing students at coeducational and single-sex schools that are similar along key dimensions.

One justification for single-sex education stems from the notion that boys and girls learn in different ways either due to different socialization (Pomerantz, Altermatt and Saxon 2002, Beyer 1990, Beyer and Bowden 1997, Higgins 1991, Cross and Madson 1997, Maccoby and Jacklin 1974, Eagly 1978)[2] or biological differences (Lenroot, et al. 2007, Killgore and Yurgelun-Todd 2004)[3] so that single-

[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. Source: http://www.ed.gov/news/pressreleases/2006/10/10242006.html.
[2] It has been found that girls tend to be under-confident in their abilities while boys are overconfident (Pomerantz, Altermatt and Saxon 2002, Beyer 1990, Beyer and Bowden 1997) also girls tend to be more concerned than boys with pleasing authority figures

sex schools allow teachers to tailor instruction to the particular needs of each sex. Another justification is that the presence of the opposite sex is distracting and leads to lower academic engagement.[4] This is thought to be particularly important for girls because larger shares of boys within coeducational classrooms have been found to be associated with lower classroom achievement (Lavy and Schlosser 2009, C. Hoxby 2000). It is also argued that single-sex schooling increases the likelihood that boys/girls participate in traditionally female/male subjects either due to the salience of gender identities in coeducational settings[5] or by deemphasizing differences in the *timing* of neurological development between boys and girls (Spielhofer, Benton and Schagen 2004, James and Richards 2003).[6]

Despite theory suggesting benefits of single-sex schools, and the potential importance for education policy, there is little conclusive empirical evidence on the effects of single-sex schooling on student outcomes.[7] The empirical evidence, to date, on single-sex schooling is based on comparisons between children who chose to attend single-sex schools and those who do not.[8] This evidence is unlikely to isolate the effect of single-sex schooling on student outcomes due to two important limitations. First, because students who *decide to* attend single-sex schools may differ from those who *decide to* attend

such as parents and teachers (Higgins 1991, Cross and Madson 1997, Maccoby and Jacklin 1974, Eagly 1978).

[3] (Lenroot, et al. 2007) find that girls complete about half of their brain development (as measured by adult mass) by age 11 compared to age 15 years for boys, and (Killgore and Yurgelun-Todd 2004) find 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.

[4] Supporting this notion, (Coleman 1961) finds that both boys and girls in coeducational schools were less concerned with academics and more concerned with 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.

[5] This is 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 coeducational environments, where gender is salient, 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.

[6] Because girls develop the math portions of the brain later than boys (Killgore and Yurgelun-Todd 2004), they are more likely to underperform in math and science at early ages and thus disengage from and avoid these subjects in a coeducational one-size-fits-all 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 disengage from and avoid these subject in coeducational settings.

[7] In describing the 2221 studies on single-sex schooling, a meta-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."

[8] 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 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 in the United Kingdom achieve higher progress in selective single-sex schools. None of these studies adjust for selection to schools (other than controlling for lagged achievement).

coeducational schools in important unobserved ways, such comparisons may be subject to self-selection bias. Second, because single-sex schools often differ in important unobserved ways from coeducational schools (e.g. curriculum, academic calendar, selectivity) these comparisons may confound a single-sex school effect with other differences. I propose solutions to both of these limitations in this study.

To address the self-selection bias, I exploit the fact that students in Trinidad and Tobago 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 attendance to single-sex schools is partially beyond their control. I use the algorithm used to assign students to schools to form rule-based instrumental variables that predict single-sex school attendance, but are not subject to selection. Under the assignment rules the likelihood of assignment to a single-sex school is a deterministic, non-linear, non-monotonic, non-smooth function of student choices and incoming test scores. Specifically, (a) conditional on two students having the same test score, differences in school assignments are due to their different choices, and (b) conditional on two students having the same choices, differences in school assignments are due to small differences in their test scores. This allows for a difference-in-differences instrumental variables strategy that identifies the causal relationship off *the interaction* between student choices and test scores. I show that each of the two distinct sources of variation ((a) and (b)) independently yield similar results to each other and the strategy that exploits them both. I also show that conditional on test scores and school choices the instruments are not correlated with incoming student characteristics, and I present additional tests indicating the instrument are exogenous.

To address the concern that single-sex schools may differ from coeducational schools in other important ways, I focus the analysis to coeducational and single-sex public secondary schools that share the same curriculum, are subject to the same oversight, and follow the same national regulations. As such, the single-sex schools and coeducational schools analyzed will not differ in most important dimensions that typically confound the relationship between single-sex and coeducational schools in other contexts. While focusing on similar schools removes numerous sources of bias, there may still remain unobserved differences across schools that affect the interpretation of the findings.[9] I document that single-sex school are more selective than coeducational schools and they attract higher quality teachers. Based on this, I argue that the effects presented likely overstate the pure single-sex schooling effect.

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[9] Because single-sex schools and coeducational school have different students (by definition) and school inputs change *in response* to student characteristics (e.g. teachers, teacher behavior, parental behaviors, peer etc.) one could never reasonably expect to find a real-world situation where the *only* thing that differs between schools is whether they are single sex. In principle, only in the short-run, one could separately identify a single-sex school effect from other factors 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 single sex for several years (that may have changed pedagogical and management styles in the long-run to take advantage of the single-sex environment). As such, while looking at actual schools may not isolate the short run single-sex school effect, it does allows one to say something about the policy relevant long-run effects (which a randomized experiment would not).

If students or parents are aware of their own suitability for a single-sex school, one would expect larger treatment effects for those with stronger preferences for single-sex schools. A unique feature of these data is that I can observe the number of single-sex schools a student lists in her school choices. Because this preference measure is also strongly associated with actual single-sex school attendance, this allows me to (a) determine if the treatment effect varies with preferences for single sex schools, (b) determine if the treatment effect for those who typically apply to single-sex schools differ from that of the average student, and (c) speak to whether improved outcomes reflect better student-school matching or a technological improvement that benefits all students. The analysis is unique in this regard.

While naive ordinary least squares yield large treatment effects, the instrumental variables results that account for selection show modest positive effects of gaining admission to a preferred single-sex school over a less preferred coeducational school. However, models that condition on gaining admission to a preferred school (of any type) yield treatment effects close to zero ─ indicating that the modest effects were due to gaining admission to a preferred school rather than attending a single-sex school *per se*. These average null effects mask considerable response heterogeneity. For students with weak preferences for single-sex schools (86 percent of all students) the effects are close to zero. However, for students with strong preferences for single-sex schools (14 percent of all students and 60 percent of those assigned to and who attend single-sex schools), there are sizable benefits. 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 girls take more math and science classes at single-sex, girls took *fewer* science courses and *more* traditionally female subjects at single-sex schools.

This is the first study, to my knowledge, to identify a causal effect of single-sex schooling on student outcomes. The results suggest that previous studies may have suffered from student-selection bias. The finding of heterogeneous treatment effects highlight that local treatment effects of schools for the typical applicant can be very misleading about effects for the average student. The results suggest that 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 may have little effect on overall achievement, and may 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 describes the Trinidad and Tobago education system, the assignment mechanism, and the data. Section 3 describes the empirical framework, section 4 presents the results, and Section 5 concludes.

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

The Trinidad and Tobago education system evolved from the English education system. At the end of primary school (after grade 5) students take the Secondary Entrance Assessment (SEA) and are assigned to a secondary school based on scores on this exams a list of four school choices by the Ministry of Education. Secondary school begins in grade 6 and ends at grade 10 when students take the Caribbean Secondary Education Certification (CSEC) examinations.[10] These are the Caribbean equivalent of the British Ordinary levels examinations 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.[11]

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.[12] There are two types of public secondary schools: Government schools and Government assisted schools (assisted schools).[13] All secondary schools provide instruction from 6th through 10th grade, and teach the same national curriculum. The key difference between Government schools and government assisted schools is that Government schools are fully funded and operated by the Government while Assisted school are run by private bodies (usually a religious board) and *at least* half of their expenses are paid for by the Government. While assisted schools are often considered more elite schools, along all other dimensions, Government and assisted schools are the same.

2.1 Single Sex Schools in Trinidad and Tobago

There are 34 single-sex schools spread out across the country. While they are geographically spread out, single-sex schools are located primarily in larger districts. In St Georges West 11 out of 32 secondary schools are single sex, compared to 9 out of 31 for Victoria, 6 out of 18 for St Georges East, 6 out of 17 in Caroni, and 2 out of 14 for St. Patrick. There are no single sex schools in the smaller districts of St. David, Tobago, and Mayaro which have 9, 8, and 4 secondary schools, respectively. It is worth noting that Trinidad is sufficiently small (about 37 by 50 miles in size) that, with the exception of Tobago

[10] 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.

[11] The CSEC examinations are accepted as an entry qualification for higher education in Canada, the UK and the United States. After taking the CSEC, students may continue to take the Caribbean Advanced Proficiency Examinations (CAPE), at the end 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 the primary institution of academic higher learning).

[12] 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.

[13] Historically, there was a third type of vocationally focused school (Comprehensive schools). 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 largely in name. The slight differences are due to the fact that there were a few (5) junior comprehensive schools that did not provide instruction through to the CSEC exams because students attended the associated senior 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.

which is a small island, a single sex school is located within about 20 miles of any location. About 91 percent of single sex-schools are assisted, and 74 percent of assisted schools are single-sex. If there is an assisted school effect, it is important to isolate this from a single-sex school effect.

While there is limited information on school inputs, Table 1 summarizes teacher and peer inputs by school type for the 2004-5 academic year. Single-sex schools have a student to faculty ratio of 17.1 compared to 12.26 for coeducational schools. This is due to the fact that coeducational schools hire more guidance officers, assistant teachers, and vocational teachers. To make comparisons across schools comparable, I focus on classroom teachers for academic courses. The students to teacher ratio is slightly higher at single sex school (23.9 versus 22.59 at coed schools) and there are large differences in education level. Unlike in the US where all teachers must poses a college degree, in Trinidad and Tobago some secondary school teachers only hold a high school degree themselves. While all teachers at single-sex schools posses a BA degree only 85 percent do at coed schools. However, the average years of experience at single sex schools is 11.98 years compared to 12.87 for coeducational schools. Given that the difference between possessing a high-school degree and a college degree is vast, the small differences in experience are likely trumped by the differences in education. While teachers at single sex schools are better educated and less experienced that those at coeducational schools as a whole, they are very similar to teachers at coeducational assisted schools ─ suggesting that conditioning on school type may remove any teacher quality differences.

The largest observable input difference across schools is peer quality. The average student assigned to a single-sex school has incoming test scores that are 1.4 standard deviations higher than those of students assigned to coed schools. In fact single sex schools are so selective that assigned students had 0.475 standard deviations higher incoming test scores than those assigned to coed assisted schools. To get a sense of the distribution of peer achievement across schools, in Figure 1 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. Figure 1 shows that while there is overlap in the distribution of peer achievement between single-sex schools and coeducational schools, schools with the highest achieving peers are disproportionally single-sex schools. Using similar data, Jackson (2010) finds large positive effects of attending a school with higher-achieving peers ─ so that the estimated single-sex school effects are likely to include a positive selectivity effect. This, in conjunction with the differences in teacher education suggest that any estimated single-sex school effects will likely overstate the effects of single sex schooling *per se*.

2.2 Data and Summary Statistics

The data used in this study come from two sources: the official SEA test score data (5^{th} grade) for the 1995 through 2002 cohorts and the official 2000 through 2007 CSEC test score data (10^{th} 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 5^{th} grade students through to secondary school in 10^{th} 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.[14] The CSEC data contain each student's grades on each CSEC exam and secondary school they attended. The school attended is defined as the school at which the student took the CSEC. For those who do not take the CSEC I use the official school assignment from the Ministry of Education. In the data, there are 123 public 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.[15] Taking a subject is defined as taking a CSEC exam in the subject. The resulting dataset contains 219,849 students across seven cohorts and 123 school assignments.

Table 1 summarizes the achievement data, 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 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 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 coed schools. While 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 only 51.9 percent do at coed schools. Girls and boys assigned to single-sex schools pass 6.18 and 5 CSEC exams respectively, compared to only 2.09 and

[14] 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 and show that the results on the number of exams passed are driven primarily by improvements among those who would have taken the CSEC exams regardless of school attended.

[15] Taking a subject is defined as taking a CSEC exam in the subject.

1.22 at coed schools for girls and boys, respectively. An important academic outcome is earning a certificate (passing 5 exams including math and English) because it is the prerequisite to tertiary education. Girls and boys assigned to single-sex schools have a likelihood of earning a certificate of 0.796 and 0.63 compared to 0.16 and 0.09 for girls and boys assigned to coeducational schools, respectively.

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 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.

A key variable in this analysis is student choices. Students' school choices are based largely on their own perceived ability, geography, and religion. 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. 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 *Econometric Framework*

3.1 *Identification Strategy*

In this section I describe how I aim to remove the effect of student selection to credibly identify the effect of attending a single-sex school. To do this, 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 school j with the following equation.

[1] $$Y_{ij} = f(SEA_i) + \text{single}_{ij}\sigma + X_i\delta + \sum_{c=1} I_{ic} \cdot \theta_c + \varepsilon_{ij}$$

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, X_i is student gender, I_{ic} is an indicator variable denoting the school choice list of student i (that is a indicator variable identifying each unique list of four choices)[16], and ε_{ij} is the idiosyncratic error term. The coefficient σ is the effect of attending a single-sex school. While including individual SEA scores should remove a large amount of self-selection bias, and adding preferences should remove bias due to students knowing more about their ability and aspirations beyond their SEA scores, OLS estimates of σ may suffer from bias if students can select to single-sex schools based on characteristics not captured by test scores and school choices. In the following sections I (a) detail how students are assigned to schools, (b) explain why there may be selection to single-sex schools, and (c) detail how I use the assignment rules to form exogenous instruments to remove selection bias and identify the causal effect of attending a single-sex school relative to a coed school.

3.2. *Student Assignment Rules*

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 an algorithm. School slots are assigned in successive rounds such that the most highly subscribed/ranked school fills its spots in the first round, then the next highly subscribed school fills its slots in the second round, and so on until all school slots are filled. This is done as follows: (1) The number of school slots at each school n_j is predetermined based on capacity constraints (this is a time-invariant school-specific characteristic). (2) Students are tentatively placed in the applicant pools for their first choice schools and are ranked in descending order by SEA score within each application pool. (3) The school at which the n_j^{th} ranked applicant has the highest SEA score is determined to be the most highly subscribed/ranked school, this score becomes the cut-off score for this school, and the top n_{j1} students in the applicant pool for top-ranked school j_1 are admitted to school j_1. (4) The top ranked school slots and the admitted students are removed from the process, and the second choice becomes the new "first choice" for students who had the top ranked school as their first choice but did not gain admission. (5) This process is repeated in round two to assign students to the second highest

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

ranked school j_2 and determine the cut-off score for the second ranked school. This is repeated in subsequent rounds 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 might not be used. Assisted schools (which account for about 16% of school slots) can 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% can be 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 an assisted school. The top 80 applicants to that school will be assigned to that school while the principal can hand pick 20 other students at their discretion. The remaining 20 students would be chosen based on family alumni connections, being relatives of teachers or religious affiliation (because 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 the deterministic function of student test scores and student choice described in the paragraph above (which is beyond students' control after taking the SEA exams), and partly on the endogenous selection of students by school principals.

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, using only the deterministic portion of algorithm described above 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 *only* difference between how students are actually assigned and the simulated "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 assisted schools.

To show the validity of the simulation, I estimate the likelihood of assignment 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 cut-offs into one. Specifically, for each school I find all students who list that school as the top choice, re-center those students' scores around the cut-off for that school, and create a sample of applicants for each school. To mimic the sequential nature of the assignment mechanism, 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 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 once for the top choice school and once for the second choice school. Because 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 2. On the left I use 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 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 self-selection by construction) plays a central role in my identification strategy.

3.4 *Rule-Based Instrument*

To remove selection bias from the actual school attended, I use the school assignments that would prevail if assisted schools could not select students. For each school student pair, I define $Rule_{ij}$ that is equal to 1 if student *i* would have been assigned to school *j* had there been no student selection or principal hand-picking and *0* otherwise. That is, $Rule_{ij}$ is equal to *1* if student *i* is assigned to school *j* based on the simulations described above and *0* otherwise. $Rule_{ij}$ is the deterministic portion of the student assignment algorithm. Because this deterministic portion of the assignment mechanism is used to assign most students to schools, the simulated assignments are correlated with the schools students attend.[17] However, since the deterministic portion of the assignment mechanism cannot be manipulated by students or school principals, the simulated assignments should be uncorrelated with *unobserved* student characteristics such as motivation and ability, conditional on student test scores and school choices. I propose instrumental variables strategies based on these simulated assignments.

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

3.4.a *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 of exogenous variation, and describe an instrumental variables estimation strategies that exploit 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, one 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 2). 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.

To isolate the discontinuity based variation in a regression setting I implement something similar to a fuzzy RD-design. Using the stacked dataset described in section 3.3, I create a sub-sample of cut-offs for preferred single-sex schools. Using this sub-sample, Figure 3 presents visual evidence of a rapid increase in the likelihood of attending a preferred single-sex school through the simulated cut-off for a preferred single-sex school. The figure also shows that the increase in likelihood is somewhat smooth, suggesting that results using variation through the cut-offs alone may be sensitive to how one controls for smoothness through the cut-offs, making it suboptimal for a fuzzy regression discontinuity type design.
While not very sharp Figure 3 also shows suggestive visual evidence of a discontinuity through the main outcomes (number of exams passed and earning a certificate). Due to the noisiness of this procedure, this is not the preferred source of variation. However, it is worthwhile to see what a discontinuity-type design might yield, and 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 simulated cut-off for a preferred single-sex school as an instrument for attending a preferred single-sex school. Specifically, I estimate [2] by 2SLS.

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

All variables are defined as before, $Above_{ij}$ is an indicator variable equal to *1* if student *i* has a SEA score above the simulated 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.[18] The first stage F-statistic is 173.2. Standard errors are clustered at the cut-off level.

***Exogenous variation due to school choices*:** The second source of variation comes from the fact that different schools have different cut-offs so that students with the same test scores but different choices are assigned to different schools. This variation is best illustrated with a simple example. Consider a world with two single-sex schools 1 and 2 and one coeducational school 3. There are two choice groups; choice group 1 who list school 1 as their top choice and school 3 as their second; and choice group 2 who list school 2 as their top choice and school 3 as their second choice. Applicants to school 1 who score above 82 on the SEA are granted admission, while school 2 has a higher cut-off such that applicants to school 2 who score above 92 on the SEA are granted admission. One can put all students into one of three test score groups: group A with scores of 82 and below; group B with scores between 83 and 92; and group C with scores of 93 and above. This is illustrated in figure 4.

Students in test score group A (with scores below the cut-offs for both single-sex schools) are never admitted to a single sex school whether they are in choice group 1 or choice group 2. Similarly, students in test score group C (with scores above the cut-offs for both single-sex schools) are all admitted to a single sex school whether they are in choice group 1 or choice group 2. However, those in test score group B (with scores above the cut-off for school 1 but below the cutoff for school 2) who are in choice group 1 are admitted to a single sex school while those in choice group 2 are not admitted to a single sex school. As such, if the choice group effects is additively separable from that of test scores, one can use a difference in difference approach to identify the effect of attending a single sex school.

Specifically, because the difference in choices do not lead to a difference in single-sex school attendance within test score groups A and C, the difference in outcomes between choice groups 1 and 2 within test score groups A and C cannot be due to differences in test scores or differences in single-sex school attendance and must therefore be due to differences in choices. However, because the difference in choices lead to a differences in single-sex school attendance within test score range B, the difference in outcomes between choice groups 1 and 2 within test score group B reflects both differences in single-sex school attendance and differences in choices. As long as the effect of choices is the same across all test score levels then the difference in outcomes between choice groups 1 and 2 within test score group B (single sex effect + choice group effect), minus the difference in outcomes between choice groups 1 and 2 within test score groups A or C (choice group effect), reflects the effect of attending a single sex school.

[18] I present results using a second, third, fourth, fifth order polynomial.

To capture only the difference in difference (DID) variation obtained by looking at the difference in outcomes for students with exactly the same test scores but who attend different schools because of differences in their school choices, I use a DID-2SLS strategy that estimates the effect of attending a preferred single-sex school after controlling for a full set of choice indicator variables, and 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 outcomes through cut-offs.

$$\begin{aligned} \text{single}_{ij} &= \sum_{t=1} \theta_{1t} \cdot I_{SEA_i=t} + \gamma_1(\widetilde{\text{single}} \mid Rule_{ij}) + X_i\delta_1 + \sum_{c=1} I_{i,c} \cdot \theta_{c1} + \varepsilon_{i,j,1} \\ Y_{ij} &= \sum_{t=1} \theta_{2t} \cdot I_{SEA_i=t} + \text{single}_{ij}\sigma + X_i\delta_2 + \sum_{c=1} I_{i,c} \cdot \theta_{c2} + \varepsilon_{ij2} \end{aligned} \quad [3]$$

In [3], single_{ij} is an indicator variable denoting whether a student attends a single-sex school, X_i is student sex, $I_{i,c}$ is an indicator variable equal to 1 if a student's rank ordering is choice group c and equal to zero otherwise[19], $I_{SEA_i=t}$ is an indicator variable equal to 1 if the students SEA score is equal to t, and $(\widetilde{\text{single}} \mid Rule_{ij})$ denotes whether the students simulated school assignment is single-sex. The simulated single-sex assignment is the excluded instrument. The coefficient σ from equation [3] should yield the causal effect of attending a single-sex school. Standard errors are clustered at the simulated school level.

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 simultaneously 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 scores that generate variation in school assignments in a smooth manner to allow for identification due to non-smoothness in the outcomes through cut-offs). I instrument for single-sex school attendance with an indicator variable denoting whether the simulated school is single-sex. Specifically, I estimate the following system of equations by 2SLS where all variables are defined as in [3] where instead of indicator variables for each test score $f(SEA_i)$ is a fifth order polynomial in the student's total SEA score.[20]

[19] Each choice 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 the ordering of schools is different. There are 22649 preference groups with more than one student. Among these groups, the average group has 63 students.
[20] All results are robust to using a third, fourth, fifth, or sixth order polynomial.

$$
\begin{aligned}
\text{single}_{ij} &= f_1(SEA_i) + \gamma_1(\widetilde{\text{single}} \mid Rule_{ij}) + X_i\delta_1 + \sum_{c=1} I_{i,c} \cdot \theta_{c1} + \varepsilon_{i,j,1} \\
Y_{ij} &= f_2(SEA_i) + \text{single}_{ij}\sigma + X_i\delta_2 + \sum_{p=1} I_{i,c} \cdot \theta_{c2} + \varepsilon_{ij2}
\end{aligned} \qquad [4]
$$

3.5 *Interpretation of the Local Average Treatment Effects*

In this context, the coefficients on "single" from [2], [3], and [4] provide a selection free estimate of the effect of gaining admission to and therefore attending a preferred single-sex school for those students who apply to single sex schools (a local treatment effect for the compliers). As such, the coefficients on "single" from [2], [3], and [4] may not isolate a single-sex schooling effect because attending a preferred school (irrespective of type) may have an effect on student outcomes and attending a single-sex school may also be associated with attending a religious assisted school. The majority of single sex school are also assisted schools (which are more selective, have better funding, and have a religious affiliation) so that this comparison of single-sex versus non-single sex is potentially confounded with the effect of attending an assisted school. Also, due to the nature of the assignment mechanism, students are more likely to attend a single sex school when they gain admission to a preferred school. Given that missing ones top choice school may have an independent effect on student motivation and effort, and could lead to changes in parental inputs (such as extra tutoring or help with homework) it is possible that part of the effect of attending a preferred single-sex school is driven by the psychological or behavioral effects associated with attending a preferred school.

Fortunately, one can exploit variation across cut-offs to remove these confounding factors. Because one quarter of assisted schools are coed and one tenth of single sex schools are government schools, some cut-offs are associated with exogenous variation to assisted schools but not single sex schools while others are associated with exogenous variation to singe sex schools but not assisted schools. Similarly, because there are several cut-offs for several schools, I can leverage the fact that some cut-offs do not entail being admitted to a single sex school, to isolate the effect of being admitted to a single sex school from that of scoring above a cut-off for a preferred school. By exploiting variation *across* cut-offs I can remove the effects of gaining admission (and therefore attending) a single sex school from that of an effect of attending an assisted school and the effects of attending a preferred school (irrespective of type).

One can move closer to isolating a single sex schooling effect by augmenting equation [5] to include an indicator for whether the students attends an assisted school and indicators for whether a student attend their first, second, third, or fourth choice school. To account for selection, I instrument for attending an assisted school with an indicator denoting whether the students was assigned to an assisted school based on the simulation, and I instrument for whether a student attends their first, second, third, or fourth choice school with whether a student was assigned to their first, second, third, or fourth choice school based on the simulation. The interpretation of the coefficient on "single" with these additional covariates would be the effect of gaining admission to and therefore attending a preferred single-sex school *above and beyond the effect of gaining admission to (and attending) a preferred school, or an assisted school* ─ an estimate that can arguably be seen as a single-sex schooling effect.

After presenting clean estimates of the effect of gaining admission to and therefore attending a preferred single-sex school, I will then present results that condition on other attributes to illustrate the underlying mechanisms through which this effect operates and also to move closer to isolating something that can be plausibly interpreted as a single-sex school effect.

3.6 *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 is 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).

I also test if having a simulated single-sex assignment $(\widetilde{\text{single}} \mid Rule_{ij})$ 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 explicitly used by principals when hand-picking students, the fact that religion is not correlated with the instruments lends credibility to the identification strategy.

4 Results

4.1 Naive Estimates of the Effect of Attending a Preferred Single Sex School: To illustrate the importance of addressing student selection in both observed and *unobserved* dimensions, I first present naive estimates of the effects of attending a single-sex school and then show how the results change as one accounts for selection. Table 3 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). Because Jackson (2010) documents that attending a school with higher incoming peer-achievement 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 naive OLS results indicate that incoming peer achievement is one 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 (third row) there are still large differences in school selectivity and outcomes such that 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.

4.1.A ***Direct Evidence of Positive Selection into Single-sex Schools:*** Because I can observe student school choices, I can 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 school as their top choice have incoming test scores one standard deviation higher than those who do not. This could be because 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 selectivity of the choices into account, students with a top choice single-sex school 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 (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.

4.2 Selection Free Effects of Attending A Preferred Single Sex School :

Because my preferred specification uses both the discontinuity and DID variation simultaneously, it is important that show that each source of variation independently gives similar answers. If both the discontinuity and DID strategies yield similar results it would suggest that each strategy yields the true relationship (a natural robustness check), and that the use of both sources of variation together is appropriate (a useful specification check for the preferred model). I show this below.

Discontinuity Variation Only: In table 4 I present the range of results one obtains using only the discontinuity variation under different choices of bandwidth and different polynomial orders of the SEA scores. As expected given the noisiness of the discontinuity only model, the estimates do vary depending on the modeling assumptions made. However, there are general patterns that are reasonably robust. First, for all models attending a single-sex school as a result of scoring above a cut-off for a preferred single sex school is associated with peer achievement that is between 0.415 and 0.779 standard deviations higher than at a coeducational school (all estimates are statistically significant at the 1 percent level). While there appears to be little effect on taking the CSEC exams, for the two main overall outcomes the results suggest that students have better outcomes at single-sex schools. Specifically, the point estimates for taking the CSEC (not dropping out of school) range from 0.095 to -0.116, about half are positive and the other half negative, and one positive point estimate is statistically significant while one negative point estimate is statistically significant. This suggest a high degree of noise, and is suggestive of no effect on this outcome. However, the point estimates for the number of CSEC exams passed are all positive, ranging between 0.16 and 1.012, and many of the specification are statistically significantly different from zero ─ suggesting a real positive effect on this outcome. For earning a certificate almost all of the point estimates are positive (but are noisier leading to a range from -0.058 and 0.102) and a few positive estimates are statistically significantly different from zero ─ suggesting an imprecisely estimated but real positive effect. The lower panel of Figure 3 shows visually the discontinuity evidence through the cut-offs for the two main outcomes. Consistent with the regression estimates there appear to be positive effects of attending a single sex school on these two main outcomes. While these discontinuity based estimates are imprecise, the range of estimate are centered at about half the size of the naive OLS estimates in Table 3 and the *highest* discontinuity based point estimates are *at least* forty percent smaller than the OLS estimate. This provides compelling evidence that the OLS estimates are overstated due to positive selection to single-sex schools. While this is informative about the probable size and sign of the true casual effect, the exact point estimates are sensitive to exactly how one controls for smoothness through the cut-offs. This motivates the use of the DID strategy that yields much more precise estimates.

Difference in Difference Variation Only: Row 4 of Table 3 presents the results from the DID model that instrument attending a single sex school with whether one was assigned to a single sex school based on the simulation. This model is conditional on indicator variables for each unique test score and each unique combination of school choices orderings. The estimates are consistent with those obtained using the discontinuity variation only while the standard errors are about one quarter the size ─ leading to much greater precision. The estimates indicate that attending a single-sex school is associated with a school with 0.296 standard deviations higher incoming peer achievement (a more selective school). Consistent with the discontinuity variation, attending a single sex school has no effect on taking the CSEC exams but is associated with passing 0.408 more exams and being 5.8 percentage point more likely to earn a certificate (both significant at the 5 percent level). Consistent with the discontinuity results, the DID results yield treatment effects that are about one third the size of the naively estimated treatment effects ─ again indicative of sizable positive selection bias on unobservables. The only outcomes for which there is some disagreement across models is for passing math and English. While the discontinuity results suggest that students at single-sex schools fare slightly worse 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 for the main overall outcomes both sets of results indicate that students who attend single-sex schools are no more likely to take the CSEC exams but do pass more exams and are more likely to earn a certificate. Given the similarity of the results across distinct sources of variation, it is now reasonable to turn to the preferred model that exploits both sources of variation simultaneously and focus the remaining analysis on this preferred model.

As a final check on the DID variation, while one cannot estimate model with the full interaction between all test scores and all choices (because this is the level of the variation), I can estimate models with the interactions between course measures of test scores and coarse measures of choices. That is, because readers may worry that an average student's willingness to list a single-sex school among her preferred set of schools may mean this student should not be compared to a similarly average student that does not list a single-sex school in her choice set, I include indicator variables defined by the unique combination of the student test score quintile, whether the first choice school is single-sex, whether the second choice school is single-sex, whether the third choice school is single-sex, and whether the fourth choice school is single-sex. These group indicators control for coarse interactions between test score and choice such that the comparisons are made among students with similar test scores and similar choices within which the assumption of additivity is most likely to hold. Results from this specification (not shown) are almost identical to those from the models without these indicator variable, suggesting that the DID identifying assumption of additive separability are valid.

All Exogenous Variation: The rule-based instrument strategy exploiting all the exogenous variation yields results that are largely similar to those from the previous models (5th row of Table 3). The 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 (and on the same order of magnitudes as the discontinuity-only and DID only results), underscoring the importance of exploiting exogenous variation when analyzing the effects of single-sex schools. In sum, the results show that there are real positive effect associated with attending a single sex school that cannot be attributed to student selection. In the following section I will investigate the mechanisms through which these positive treatment effects emerge in order to determine whether this true positive effects reflects a single-sex school effect or is due to other factors that are associated with attending single sex schools that affect how the results should be interpreted.

4.3 *Isolating the Single-Sex Schooling Effect*

The effect of attending a preferred single sex school may not isolate a single sex schooling effect because it may be confounded with the effect of attending an assisted school and the effect of attending a preferred school (of any type). I present results that account for these sources of confounding below.

<u>Are the results driven by an assisted school effect?:</u> To remove the effect of attending a preferred assisted school from that of attending a preferred single-sex school I instrument for and include attending an assisted school as a covariate (presented in Row 6 of Table 3). For all outcomes conditioning on school

type reduces the estimated effects by approximately 20 percent, but one cannot reject the null hypothesis that the effects are the same with and without conditioning on school type. This indicates that while a small portion of the estimated benefits to attending a single-sex school may have to do with the fact that such school are assisted schools, this does not drive the results.

Are the results driven by benefits to gaining admission to a preferred school of any kind?: Due to the nature of choices and the assignment mechanism, students are more likely to attend a single sex school when they gain admission to a preferred school by scoring above a cut-off. To isolate the effect of scoring above the cut-off for a preferred school from that of attending a single-sex school, I instrument for and include attending one's first, second, third, and fourth choice school as a covariate (presented in Row 7 of Table 2). The results show that conditional on the choice attained, students who attend a preferred single sex school and up with peers that are 15 percent of a standard deviations better than those who attend a coed school. Despite attending more selective schools such students experience no statistically significant benefits to attending a single sex school. The point estimates are much smaller than the unconditional casual estimates suggesting that the benefits to attending a single-sex school *per se*, if any, are small.

To assuage concerns that this result is driven by idiosyncrasies in the DID specification, I also estimate the effect of attending a preferred school where the preferred school is a coeducational school using the discontinuity variation. Specifically I estimate models similar to [2] using cut-offs where the preferred school is a coeducational school, where attending a preferred school is the endogenous treatment, and scoring above the cut-off for a preferred school is the excluded instrument. This identifies the causal effect of attending a preferred school (because both the preferred and the comparison schools are coeducational schools). The results are presented in Table 5 for various bandwidths and polynomial orders of the running SEA variable. Comparing the estimates in Tables 3 and 4, it is clear that attending a preferred single-sex school leads to improvements that are very similar to (and often smaller than) the effects of attending a preferred coed school— suggesting that benefits associated with scoring above a cut-off for a preferred school can explain all the benefits to attending a preferred single sex school.

Do students perform better at single-sex schools than equally selective coeducational schools? The results thus far indicate that despite attending schools that are more selective on average, conditional on school type and being admitted to a preferred school there is little to no benefit to attending a single sex-school. There is no credible way to isolate a single sex school effect from school selectivity effect because school selectivity may be a function of it being single sex. Even though school selectivity may be endogenous to whether the school is single-sex (unlike attending a preferred school or the type of school which are not endogenous), it is instructive to see if single-sex schools have better outcomes compared to equally selective coeducational schools.[21] The bottom panel of Table 3 presents 2SLS results that condition on 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.

Taken together the pattern of results suggests that single-sex schooling *per se* is not associated with better outcomes on average. That is, the effect of gaining admission to a preferred single sex assisted school is not statistically distinguishable from the effect of gaining admission to a preferred coeducational assisted school. The fact that single sex school shave better peers and better teacher inputs (see Table 1) than assisted schools suggests that this near zero effect might be an upper bound on the real single sex schooling effect. If there were real positive effects of attending single-sex schools due to the schools being single-sex, there should be some benefit to attending a preferred single sex school above and beyond the effect attending any preferred school *per se* -- but this is not the case. It is then not surprising that attending a single-sex school provides no benefit over attending an equally selective coeducational school on average. This finding of no single-sex schooling effect on average does not mean that expanding single sex school will not improve students outcome because there may be subpopulations that enjoy real large benefits to attending single sex schools. I investigate this in the following section.

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

If individuals rationally select to schools based on their private benefits of attending a school, then the treatment effect for students with strong preferences for single sex schools may be very different from those of students with weak preferences for single sex schools. This is important to asses because (a) the finding of no single-sex effect conditional on attending a preferred school and school type may mask considerable benefits to single sex schooling for sub-samples of the population, and (b) if only students

[21] 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 good measure of school quality and if single-sex schools do improve outcomes then a single-sex school must have worse inputs than an equally selective coed school so that the "single sex" will be correlated with worse inputs and be downward biased. 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 parents and students perceive some benefit to attending single sex schools above and beyond student selectivity.

with strong preferences for single-sex schools benefit from them, then any successes of single-sex schools will not be scalable or replicable for the average student because the treatment effect for the average treated students would be larger than that of the average student. The fact that I observe student choices allows me to investigate these issues. 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 list zero single-sex schools in their choices, 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. Among students who are 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 only 14 percent of all students have strong preferences for single-sex schools (list 3 of 4 schools) while almost 60 percent of those who actually attend single sex schools have strong preferences for single sex schools, the treatment effect for the marginal student, may be very different from that of the average treated student, which may be very different from that of the average student in the population.

To see if there is response heterogeneity be the intensity of preferences for single-sex schools I estimate both the preferred 2SLS models (that do not condition on school type, choice attended, or selectivity) and the preferred 2SLS models conditional on peer selectivity 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). Because I aim to document non-zero effects for certain sub-populations, I estimate the more conservative 2SLS model that conditions on incoming peer achievement and yields estimates that are closer to zero than those that condition on school type and attending a preferred school.[22] I present these unconditional and conditional effects graphically in Figure 5 (the point estimates are in Appendix Table A2).

Across all outcomes there is a pattern of unconditional effects close to zero for students who only list one or two single-sex schools, 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 unconditional effect of single sex schools is zero for students with one or two single sex school choices. Moreover, the unconditional (likely an upper bound) effects are sufficiently small that one can rule out any meaningful single-sex school effect for students who list one and two single-sex schools. This is important because it suggests that for 85 percent of the student population

[22] However, to assuage worries that this modeling assumption drives the results I have verified that the pattern of results are very similar in models that condition on school type and choice rank attended.

there is no positive effect of attending a single-sex school on academic outcomes.

For the 14 percent of students with strong preferences for single-sex schools the unconditional effects are large enough, and the estimates statistically significant enough, that one cannot rule out that there is no single school effect for these students. I therefore turn to the more conservative results for this sub-population. The conservative results that condition on school selectivity show that this group of students do benefit from attending single sex schools. Specifically, the point estimates for those with three and four choices are large and very similar to the unconditional estimates (which are statistically significant at the 5 percent level). This indicates that the lack of statistical significance in the conditional models for these students is due to decreased power with the inclusion of an additional endogenous variables rather than bias. In fact in a Housman test one fails to reject equality of the conditional and unconditional estimates for these students, so that one should trust the unconditional estimates which are large and statistically significant. 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).

Readers may wonder whether a small subset of the single-sex schools drive the effect for high-preference students. To test for this, I compute the fraction of students assigned to each school in the high preference group (4 choices) and then reweight the sample for the other groups to reflect the contribution of each of the assigned schools that exists in the high preference group. If the results were driven by those with 4 choices attending better single-sex schools than those with 1 or 2, then the re-weighted regressions for the low preference groups would yield similar estimates to that of the 4 choice sample. While not shown here, the opposite is true. The re-weighted results make the results even more different across the groups — indicating that differential school quality does not explain the response heterogeneity.

The pattern of results suggests that students respond differently to different types of schools and select into schools that best match their specific needs. 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 overstate the single-school effect for the entire population. In sum, the results show that only 14 percent of the full population may derive any benefit from attending singe sex schools such that while providing the option for single sex school should increase academic outcomes for these students, turning all schools into single sex schools will not improve outcomes for the average student.

4.5 *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 (Spielhofer, Benton and Schagen 2004, Malacova 2007). To assess the extent to which this might be true, I estimate the unconditional effects and conditional effects by the number of single-sex school choices for males and females separately (appendix Tables A3 and A4). Figure 6 shows the unconditional effect for each outcome separately for females and males. For all outcomes the unconditional effect is larger for females than for males, and it is clear that for most outcomes almost all of the positive effects 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 unconditional 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 unconditional effect for males is zero for any preference intensity group (at the 20 percent level). Given that these unconditional estimates are likely upper-bound estimates, the lack of either economic or statistical significance for males is telling. The conditional estimates also suggest real positive effects for females. The conservative conditional estimates indicate that females with four single sex school choices at single sex schools pass 3.2 more exams in total and are 39 percentage points more likely to earn a certificate that they would at a similarly selective coed school. While these effects are not very precisely estimated, they are *larger* than the unconditional estimates which are both statistically significant at the 5 percent level ─ suggesting that there is a real positive effect for this sub-population.

In sum, the evidence suggests no benefit to attending single-sex schools for the vast majority of males. 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. 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.

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

One 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. To assess whether attending a single-sex school has an effect on the course selections of students, I present both the unconditional and conditional 2SLS estimates on the number of female courses, and science classes taken by preference strength for males and females separately in Figure 7. The unconditional 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, both the unconditional and the conditional 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 may actually reduce the number of science courses taken by 0.6. Potentially, the larger effects on female courses for girls may reflect the fact that girls at single sex schools take more courses in general. However, this increased course taking explanations would not explain the reduction in the number of science courses taken. It is important to note that all schools are to offer the same courses so that any differences in course taking reflect differences in demand rather difference in supply across schools.

The results do not support the notion that single-sex schools reduce gender differences in course selection. In fact, they suggest 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 take more math and science courses at single-sex schools.

5 *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 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, increases in human capital, or allocative efficiencies could be sizable.

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 was, to date, 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.

I find that a failure to account for student selection can lead to large spurious benefits to attending single-sex schools. Once student selection is accounted for, I find that attending a single-sex school is associated with better educational outcomes. An investigation into the mechanisms however, reveals that this positive effect is not due to the school being single sex per se, but is due to benefits associated with being admitted to a preferred school (irrespective of the type of school) and to a smaller extent single sex schools being government assisted schools as opposed to traditional government schools. These small overall effects mask considerable heterogeneity across students, such that those students who exhibit strong preferences for single-sex schools enjoy large benefits to attending single-sex schools, while for most students (who have weak or modest preferences for single-sex schools) there is no effect on student achievement. Looking for heterogeneity by gender reveals that much of the benefit to attending single-sex schools was driven by large effects for girls with strong preferences for single-sex schools, while the effects for boys are small. Looking to course selection, 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. Contrary to popular belief, the results show that females may take *more* female dominated courses and *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 a school choice policy that included single-sex schools as an option for students is likely to improve student outcomes, 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, in large part, by a student-school match rather than a technological advance that would benefit all students.

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 study represents a useful first step in this direction.

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Table 1: *Comparison of Inputs at Single Sex and Coeducational Schools*

Variable	Single sex	Not single sex	Assisted and not single sex	Not assisted and not single sex
Pupils/(# Faculty)	17.110	12.256	16.136	11.771
	(3.475)	(4.619)	(7.105)	(4.011)
Pupils/(# Academic Teachers)	23.922	22.597	23.908	22.308
	(11.106)	(16.531)	(9.412)	(17.231)
Teachers: % BA	0.738	0.461	0.654	0.436
	(0.176)	(0.241)	(0.235)	(0.232)
Teachers: % MA	0.060	0.024	0.035	0.022
	(0.047)	(0.034)	(0.048)	(0.032)
Academic Teachers: % BA	1.000	0.846	1.000	0.825
	-	(0.130)	-	(0.144)
Academic Teachers: % MA	0.080	0.040	0.059	0.038
	(0.075)	(0.038)	(0.056)	(0.036)
Assisted	0.912	0.111	1.000	0.000
	(0.288)	(0.316)	-	-
Mean years of teaching experience	11.987	12.876	10.681	13.400
	(4.492)	(4.278)	(3.672)	(4.069)
School enrollment (2004)	664.588	638.879	524.546	653.171
	(167.478)	(263.568)	(208.199)	(267.226)
Grade 6 enrollment (2004)	108.471	114.111	96.182	116.352
	(28.404)	(49.243)	(42.944)	(49.737)
Mean score of assigned (2004)	1.212	-0.183	0.736	-0.282
	(0.595)	(0.903)	(0.752)	(0.862)
Standard dev. of score of assigned (2004)	0.446	0.709	0.504	0.753
	(0. 200)	(0.196)	(0.132)	(0.178)
Number of schools	34	99	11	88

Standard deviations in parentheses below sample means

Table 2: *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: The sample sizes for the simulated assignment are smaller than the full sample because students who score very low will have no simulated assignment. In the real world such students are assigned to schools based on arability and proximity.
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 3: *Effect of Attending A Preferred Single-Sex Schools*

	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
	[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
	[0.027]**	[0.031]**	[0.148]**	[0.081]**	[0.015]**	[0.011]**	[0.013]**
Fifth order polynomial in SEA and choice fixed Effects (218584 obs.)							
Single-sex	0.54	0.407	2.471	1.664	0.244	0.201	0.171
	[0.038]**	[0.028]**	[0.139]**	[0.109]**	[0.018]**	[0.016]**	[0.018]**
2SLS-DID: individual SEA test score fixed effects and choice fixed effects (152242 obs. [a])							
Single-sex	0.296	0.024	0.372	0.408	0.04	0.051	0.058
	[0.060]**	[0.021]	[0.220]+	[0.178]*	[0.032]	[0.036]	[0.025]*
2SLS: Fifth order polynomial in SEA and preference fixed effects (179710 obs. [a])							
Single-sex	0.336	0.073	0.736	0.615	0.054	0.054	0.057
	[0.054]**	[0.018]**	[0.163]**	[0.154]**	[0.020]**	[0.017]**	[0.025]*
2SLS: Fifth order polynomial in SEA, choice fixed effects, and assisted school (179710 obs. [a])							
Single-sex	0.268	0.065	0.585	0.442	0.043	0.042	0.04
	[0.056]**	[0.020]**	[0.160]**	[0.158]**	[0.021]*	[0.020]*	[0.027]
2SLS: Fifth order polynomial in SEA, choice fixed effects, and choice attained fixed effects (179710 obs. [a])							
Single-sex	0.157	-0.082	-0.213	0.239	0.018	0.031	0.026
	[0.053]**	[0.044]+	[0.265]	[0.180]	[0.023]	[0.019]	[0.025]
2SLS: Fifth order polynomial in SEA, choice fixed effects, and assisted, and choice attained fixed effects (179710 obs. [a])							
Single-sex	0.121	-0.059	-0.190	0.130	0.013	0.024	0.014
	[0.053]*	[0.042]	[0.263]	[0.185]	[0.024]	[0.021]	[0.027]
2SLS: Fifth order polynomial in SEA, preference fixed effects, and peer quality (179710 obs.)							
Single-sex	-	-0.039	-0.158	0.009	-0.008	-0.001	-0.011
	-	[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. The sample sizes for the simulated assignment are smaller than the full sample because students who score very low will have no simulated assignment. In the real world such students are assigned to schools based on arability and proximity.
+ significant at 10%; * significant at 5%; ** significant at 1%

Table 4: *LATE Effect of Attending Preferred Single-Sex Schools using Discontinuity Variation Only*

Peer achievement	Take CSEC	Exams Taken	Pass English	Pass math'	Exams Passed	Certificate	Bandwidth (SD)	Polynomial of SEA score	Obs.
0.607 [0.259]*	-0.16 [0.202]	-0.26 [1.353]	-0.042 [0.211]	0.036 [0.171]	0.486 [1.221]	0.022 [0.211]	0.5	5	71941
0.723 [0.160]**	-0.051 [0.103]	0.453 [0.758]	0.023 [0.115]	0.075 [0.111]	1.015 [0.712]	0.098 [0.127]	0.5	4	71941
0.531 [0.141]**	-0.139 [0.102]	-0.361 [0.725]	-0.18 [0.126]	-0.19 [0.144]	0.161 [0.678]	0.1 [0.143]	0.5	3	71941
0.779 [0.097]**	0.046 [0.052]	0.469 [0.346]	-0.147 [0.086]+	-0.073 [0.076]	1.012 [0.324]**	0.102 [0.084]	0.5	2	71941
0.618 [0.131]**	-0.115 [0.088]	0.018 [0.566]	0.101 [0.093]	0.109 [0.101]	0.577 [0.559]	0.023 [0.076]	1	5	96208
0.415 [0.108]**	-0.116 [0.065]+	-0.289 [0.472]	-0.11 [0.083]	-0.12 [0.093]	0.437 [0.437]	0.082 [0.072]	1	4	96208
0.644 [0.086]**	-0.03 [0.033]	0.372 [0.267]	-0.142 [0.055]*	-0.123 [0.059]*	0.889 [0.310]**	0.057 [0.074]	1	3	96208
0.445 [0.124]**	0.008 [0.041]	0.353 [0.299]	-0.178 [0.050]**	-0.237 [0.083]**	0.404 [0.388]	-0.058 [0.115]	1	2	96208
0.366 [0.115]**	0.111 [0.071]	-0.401 [0.533]	-0.156 [0.089]+	-0.204 [0.102]*	0.224 [0.488]	0.096 [0.043]*	1.5	5	107203
0.589 [0.080]**	-0.042 [0.037]	0.362 [0.291]	-0.134 [0.055]*	-0.146 [0.064]*	0.902 [0.280]**	0.084 [0.043]+	1.5	4	107203
0.546 [0.144]**	-0.024 [0.039]	0.349 [0.292]	-0.144 [0.075]+	-0.125 [0.084]	0.691 [0.409]+	0.052 [0.046]	1.5	3	107203
0.447 [0.078]**	0.095 [0.031]**	0.692 [0.212]**	-0.251 [0.044]**	-0.28 [0.071]**	0.126 [0.295]	-0.007 [0.040]	1.5	2	107203

Standard errors in brackets are adjusted for clustering at cut-off level.
+ significant at 10%; * significant at 5%; ** significant at 1%

Table 5: *LATE Effect of Attending A Preferred Coed School using Discontinuity Variation Only*

Peer achievement	Take CSEC	Exams Taken	Pass English	Pass math'	Exams Passed	Certificate	Bandwidth (SD)	Polynomial of SEA score	Obs.
0.407 [0.136]**	-0.0413 [0.0835]	0.521 [0.474]	0.0507 [0.0730]	-0.0532 [0.0670]	0.641 [0.403]	0.0658 [0.0709]	0.5	5	203375
0.415 [0.111]**	0.0942 [0.0766]	0.966 [0.418]*	-0.0603 [0.0710]	-0.0708 [0.0769]	0.331 [0.384]	0.0634 [0.0778]	0.5	4	203375
0.563 [0.0937]**	0.00133 [0.0662]	0.636 [0.371]+	-0.00365 [0.0684]	0.121 [0.0762]	0.875 [0.362]*	0.247 [0.0733]**	0.5	3	203375
0.421 [0.0835]**	0.0507 [0.0738]	0.566 [0.367]	-0.107 [0.0791]	0.0287 [0.0837]	0.284 [0.363]	0.167 [0.0746]*	0.5	2	203375
0.505 [0.103]**	0.00646 [0.0678]	0.679 [0.380]+	-0.00888 [0.0660]	0.0523 [0.0718]	0.747 [0.362]*	0.186 [0.0725]*	1	5	231311
0.474 [0.0955]**	0.0295 [0.0732]	0.714 [0.380]+	-0.0293 [0.0678]	0.0335 [0.0742]	0.625 [0.347]+	0.166 [0.0719]*	1	4	231311
0.449 [0.0858]**	0.0487 [0.0783]	0.638 [0.367]+	-0.0749 [0.0733]	0.0735 [0.0891]	0.498 [0.371]	0.198 [0.0839]*	1	3	231311
0.603 [0.0764]**	-0.0273 [0.0712]	0.564 [0.323]+	0.084 [0.0700]	0.236 [0.0842]**	1.308 [0.347]**	0.332 [0.0773]**	1	2	231311
0.458 [0.0995]**	0.035 [0.0730]	0.718 [0.387]+	-0.0424 [0.0677]	0.0223 [0.0763]	0.558 [0.354]	0.152 [0.0753]*	1.5	5	234619
0.516 [0.0889]**	0.00665 [0.0715]	0.613 [0.360]+	-0.00933 [0.0679]	0.1 [0.0796]	0.787 [0.345]*	0.226 [0.0751]**	1.5	4	234619
0.428 [0.0805]**	0.049 [0.0749]	0.604 [0.350]+	-0.0709 [0.0711]	0.0715 [0.0854]	0.462 [0.352]	0.193 [0.0800]*	1.5	3	234619
0.615 [0.0753]**	-0.0225 [0.0669]	0.61 [0.301]*	0.112 [0.0664]+	0.248 [0.0817]**	1.395 [0.340]**	0.333 [0.0760]**	1.5	2	234619

Standard errors in brackets are adjusted for clustering at cut-off level.
+ significant at 10%; * significant at 5%; ** significant at 1%

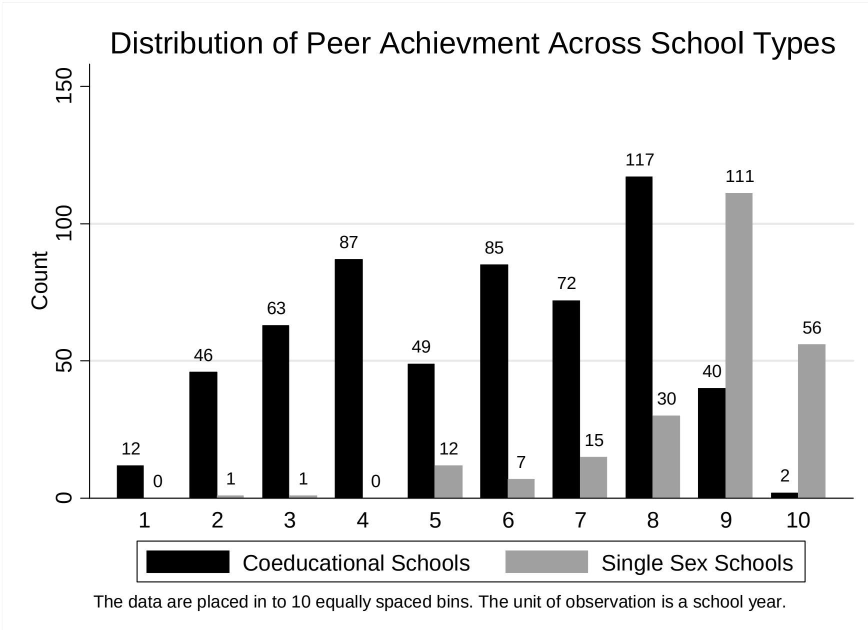

Figure 1: *Distribution of Peer Achievement Across School Types*

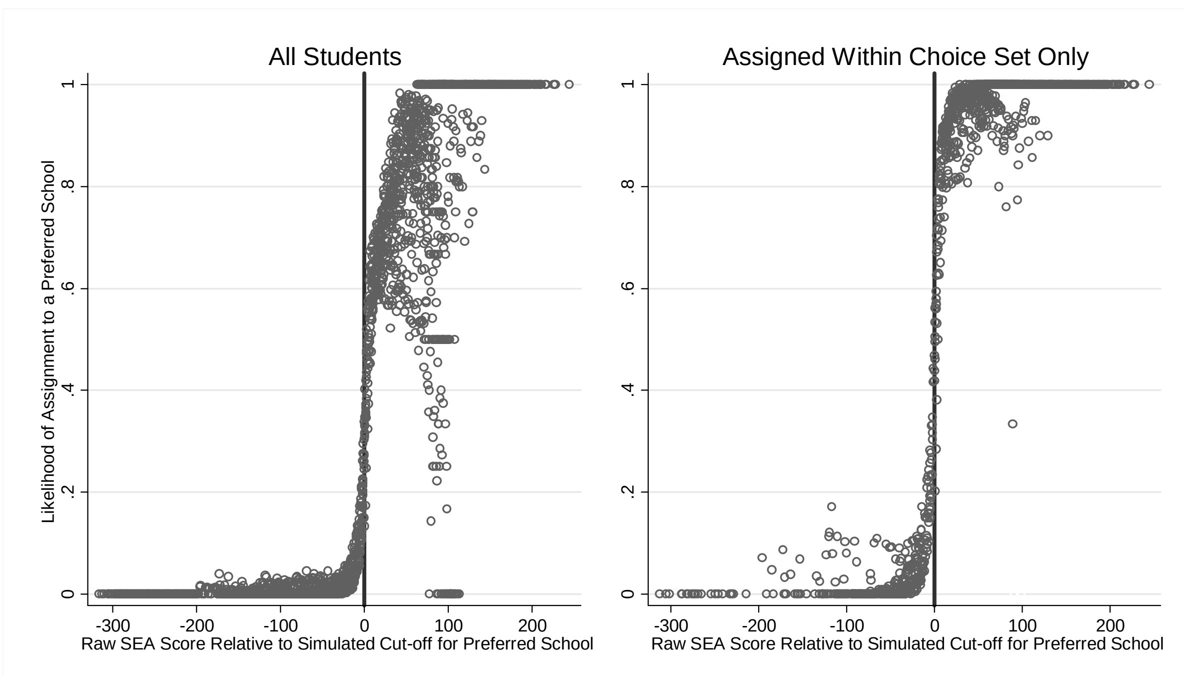

Figure 2: *Likelihood of Being Assigned to a Preferred School*

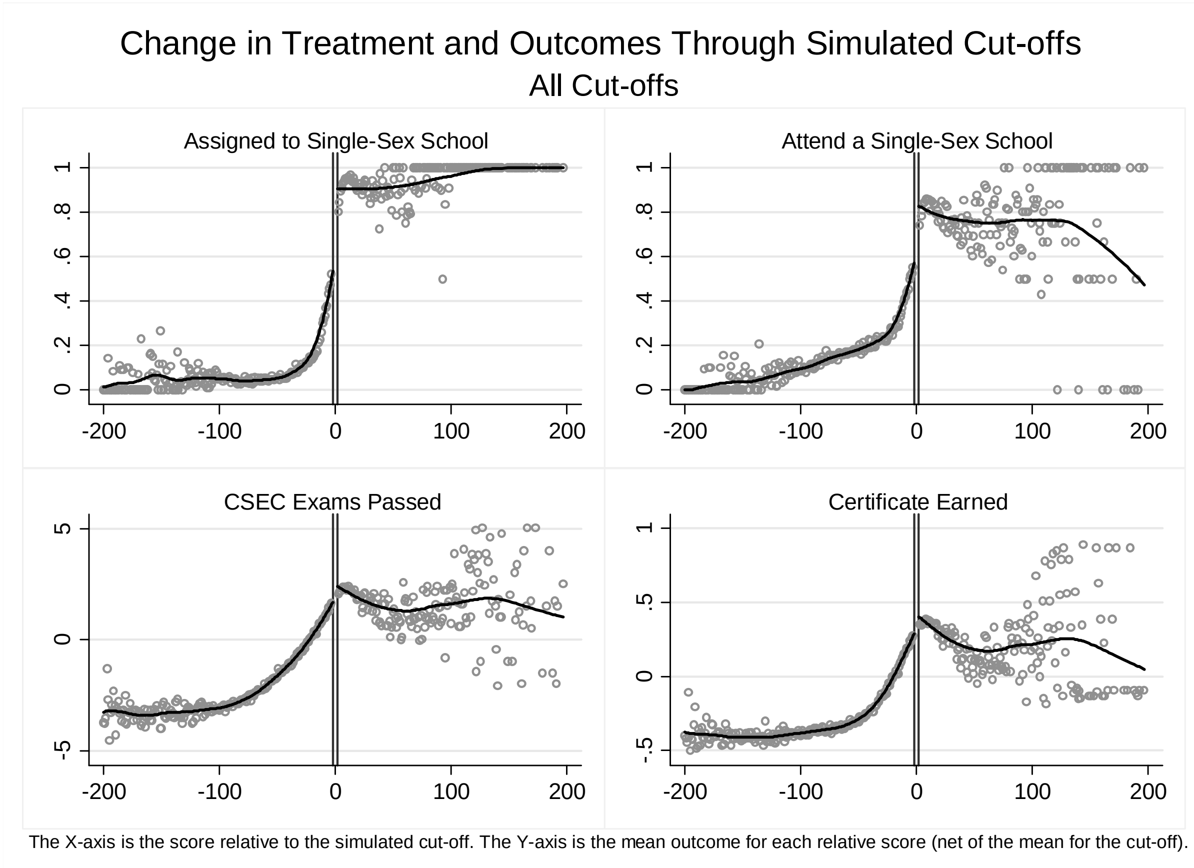

Figure 3: *Aggregate cut-offs*

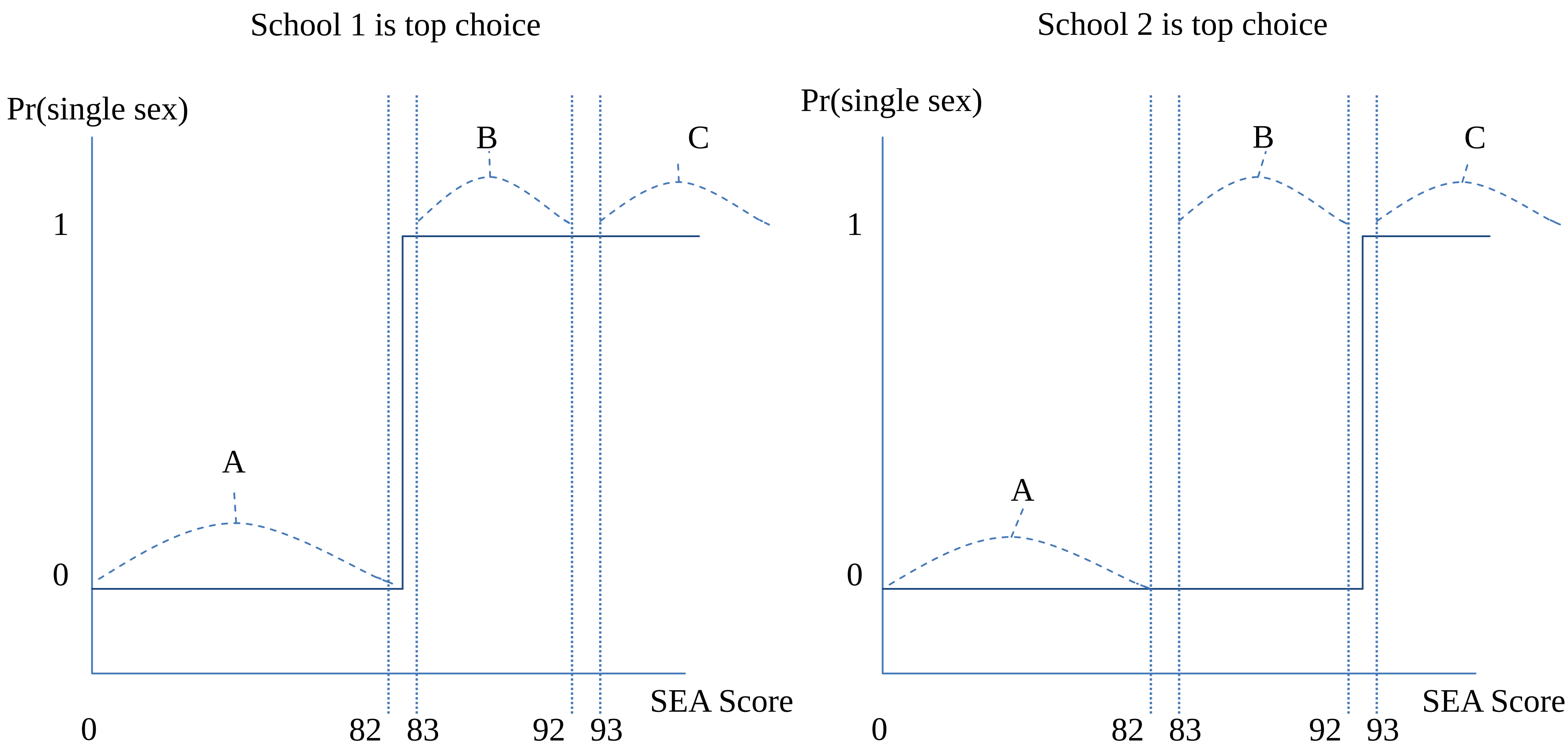

Figure 4: *A graphical illustration of the Difference in Difference variation*

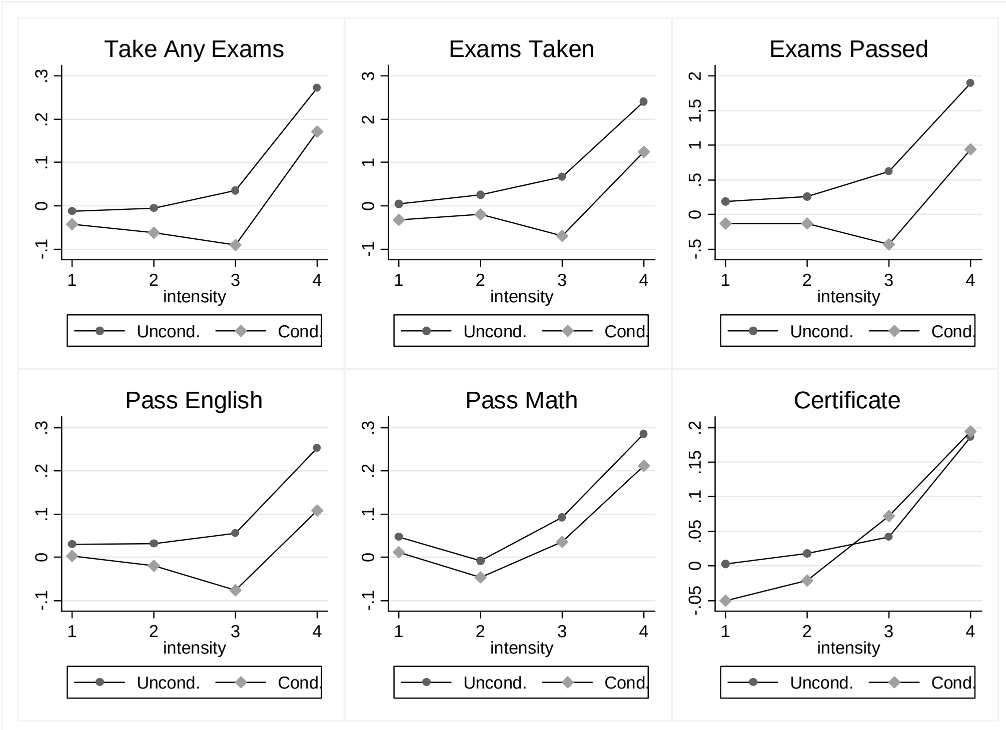

Figure 5: *Effects by intensity of preference for single-sex schools*

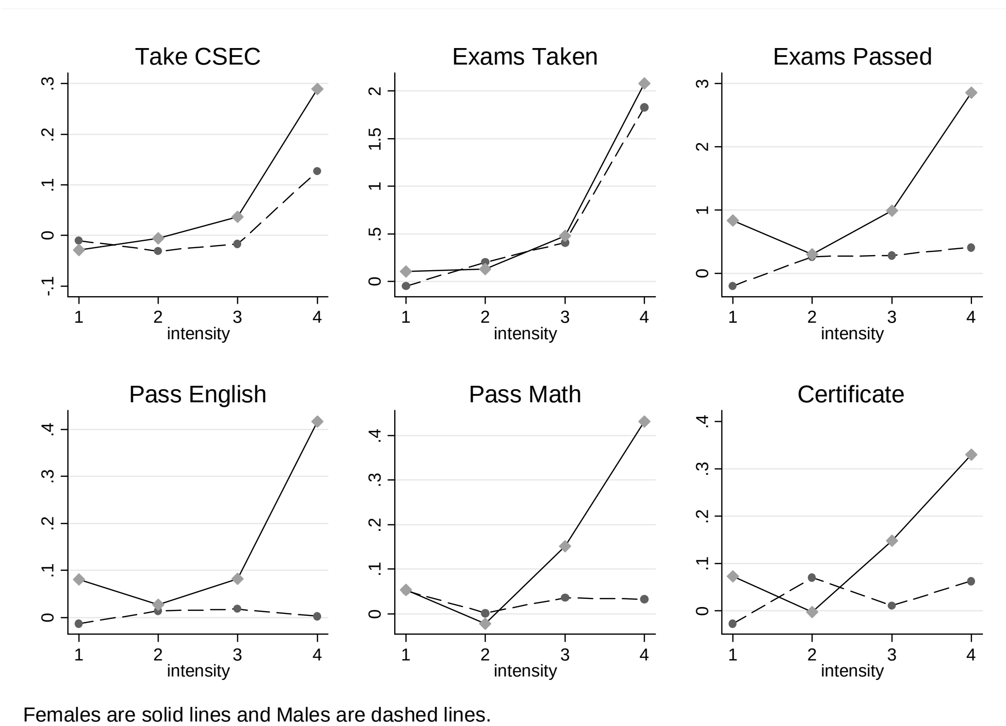

Figure 6: *Unconditional effects of attending a preferred single-sex school by gender and preference intensity*

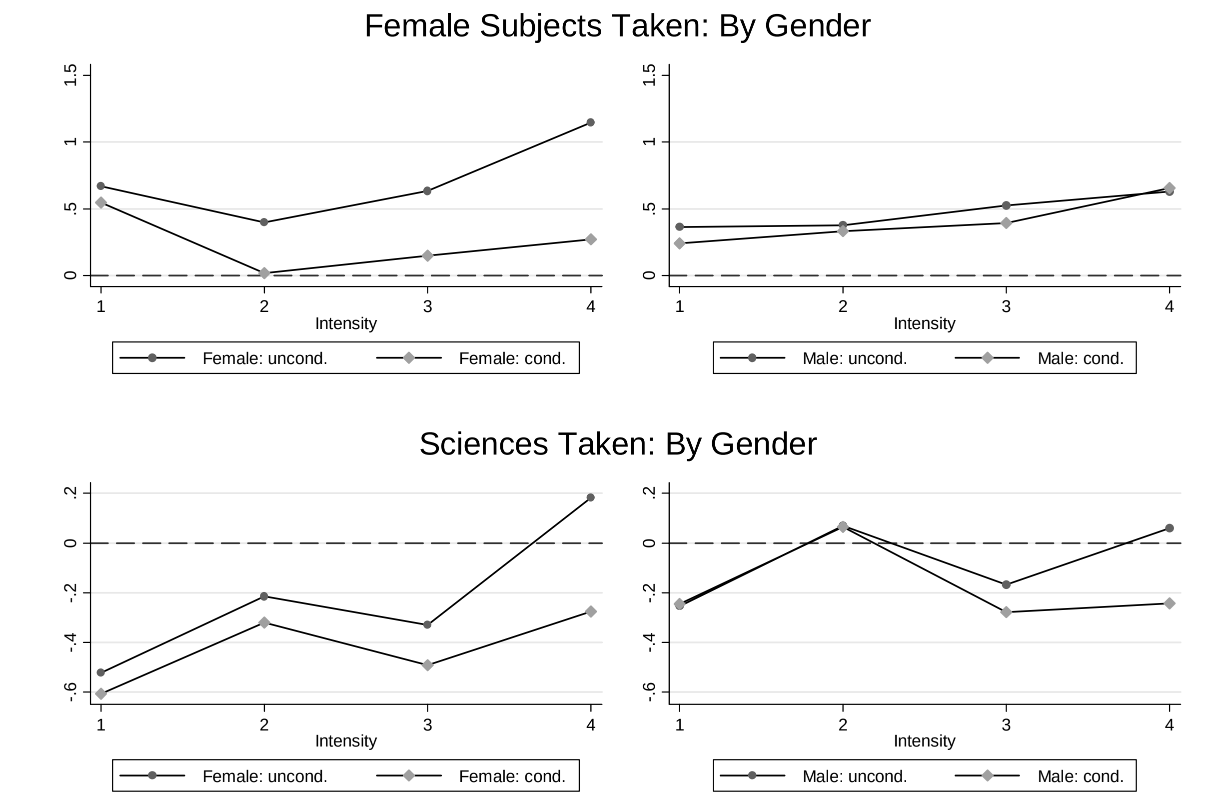

Figure 7: *Effect on course taking by gender*

Appendix

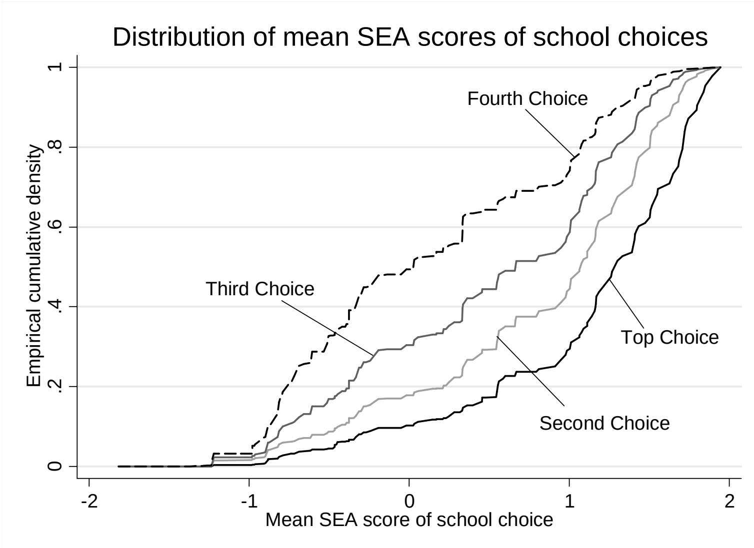

Figure A1: *Distribution of Peer Quality by School Choice Rank*

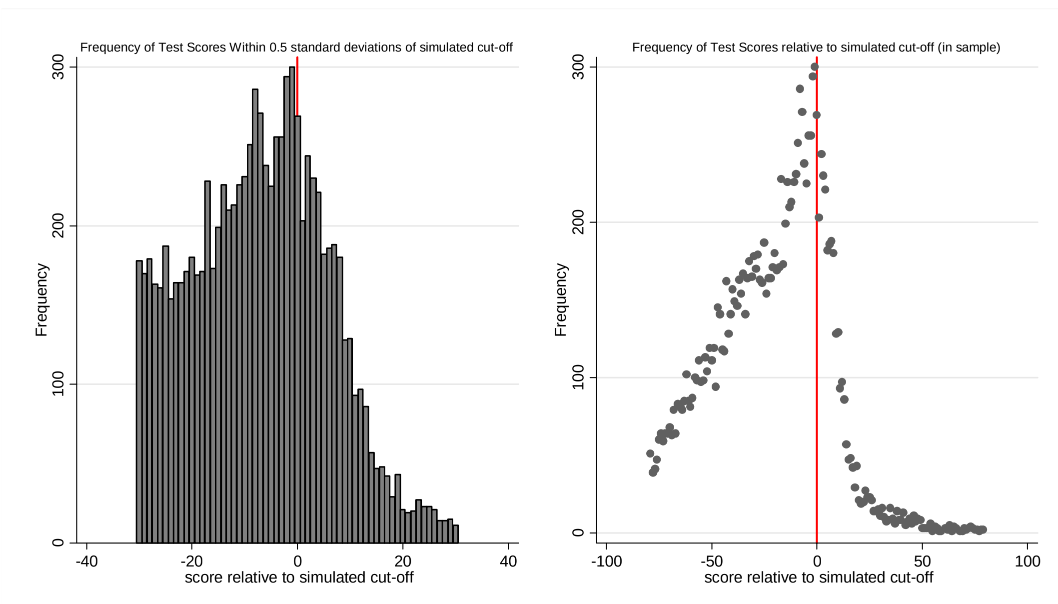

Figure A2: *Test for Smoothness Through the Simulated Cut Offs*

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 attend unconditional	-0.012	-0.006	0.035	0.272	0.039	0.261	0.673	2.417
	[0.037]	[0.032]	[0.023]	[0.071]**	[0.268]	[0.216]	[0.181]**	[0.589]**
Single attend conditional	-0.043	-0.062	-0.091	0.172	-0.33	-0.194	-0.696	1.246
	[0.046]	[0.028]*	[0.039]*	[0.079]*	[0.299]	[0.181]	[0.325]*	[0.309]**
Observations	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 attend unconditional	0.187	0.258	0.623	1.898	0.03	0.032	0.056	0.253
	[0.259]	[0.200]	[0.203]**	[0.458]**	[0.045]	[0.039]	[0.025]*	[0.042]**
Single attend conditional	-0.13	-0.133	-0.433	0.937	0.003	-0.02	-0.077	0.108
	[0.240]	[0.166]	[0.365]	[0.552]+	[0.042]	[0.037]	[0.050]	[0.063]+
Observations	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 attend unconditional	0.047	-0.008	0.092	0.285	0.003	0.018	0.042	0.186
	[0.026]+	[0.033]	[0.036]*	[0.085]**	[0.033]	[0.035]	[0.052]	[0.068]**
Single attend conditional	0.011	-0.047	0.035	0.212	-0.05	-0.021	0.072	0.194
	[0.024]	[0.031]	[0.069]	[0.104]*	[0.031]+	[0.030]	[0.073]	[0.134]
Observations	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	0.012	-0.01	-0.031	-0.017	0.127	0.461	-0.049	0.203	0.409	1.831
unconditional	[0.023]	[0.040]	[0.043]	[0.043]	[0.056]*	[0.215]*	[0.382]	[0.287]	[0.288]	[0.605]**
Single attend	-0.048	-0.045	-0.051	-0.07	0.12	-0.042	-0.363	0.021	-0.364	1.375
conditional	[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	0.362	-0.2	0.264	0.286	0.412	0.014	-0.013	0.014	0.018	0.003
unconditional	[0.214]+	[0.342]	[0.285]	[0.438]	[0.634]	[0.032]	[0.044]	[0.052]	[0.049]	[0.100]
Single attend	0.056	-0.402	0.109	-0.269	0.173	-0.008	-0.03	-0.003	-0.074	0.026
conditional	[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	0.048	0.052	0.002	0.036	0.033	0.076	-0.027	0.07	0.011	0.063
unconditional	[0.027]+	[0.038]	[0.039]	[0.059]	[0.079]	[0.037]*	[0.055]	[0.046]	[0.102]	[0.072]
Single attend	0.02	0.021	-0.004	-0.023	0.008	0.039	-0.06	0.059	0.016	0.048
conditional	[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 unconditional	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 conditional	-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 unconditional	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 conditional	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 unconditional	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 conditional	-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.