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PEER QUALITY OR INPUT QUALITY?: EVIDENCE FROM TRINIDAD AND TOBAGO

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ABSTRACT

Using exogenous secondary school assignments to remove self-selection bias to schools and peers, I obtain credible estimates of (1) the effect of attending schools with higher-achieving peers, and (2) the direct effect of peer quality improvements within schools, on the same population. While students at schools with higher-achieving peers have better academic achievement, within-school increases in peer achievement improve outcomes only at high-achievement schools. Peer quality can account for about one tenth of school value-added on average, but over one-third among the top quartile of schools. The results reveal large and important differences by gender.

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Peer Quality or Input Quality?: Evidence from Trinidad and Tobago

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Abstract: Using exogenous secondary school assignments to remove self-selection bias to schools and peers, I obtain credible estimates of (1) the effect of attending schools with higher-achieving peers, and (2) the direct effect of peer quality improvements within schools, *on the same population*. While students at schools with higher-achieving peers have better academic achievement, within-school increases in peer achievement improve outcomes only at high-achievement schools. Peer quality can account for about one tenth of school value-added on average, but over one-third among the top quartile of schools. The results reveal large and important differences by gender.

There is mounting evidence that attending higher-achieving schools improves student test scores (Hastings and Weinstein 2007, Pop-Eleches and Urquiola 2008, Jackson 2009) and broader outcomes (Culllen, Jacob and Levitt 2006, Clark 2007, Deming 2010).[2]It is also true that high-achieving schools tend to have student bodies with above average incoming achievement.[3] Since students may benefit directly from higher-achieving peers (C. Hoxby 2000, Hoxby and Weingarth 2006, Sacerdote 2001, Zimmerman 2003), part of the benefit to attending a high-achievement school may be attributed to the direct benefits of having higher-achieving peers.[4,5]

Without knowing how much of the benefit associated with "better" schools is due directly to those schools having "better" peers, it is impossible to know whether successful school models can be replicated in other settings. For example, if high concentrations of Catholic students (whose parents place a high value on education) engenders an environment particularly conducive to learning, it would imply that adopting practices used in Catholic schools (e.g. strict discipline and uniforms) in traditional public schools may not yield improved results as much of the "Catholic school effect" *could* be a "Catholic peer effect".[6] Despite the need to understand

[1] I thank Ron Ehrenberg, David Figlio, Caroline Hoxby, Brian Jacob, Jordan Matsudaira, Jonah Rockoff, and Miguel Urquiola for helpful comments and suggestions.

[2] This is related to studies showing positive effects of attending private schools (Angrist, Bettinger, et al. 2002, Rouse 1998) and the effect of attending Catholic schools (Evans and Schwab 1995, Neal 1997).

[3] This may be due to residential sorting by socioeconomic status in conjunction with local funding for public schools, explicit across-school ability-grouping, or selection into schools.

[4] Also, in the long run, the quality of other school inputs such as teachers may be endogenous to peer quality (Hanushek, Kain and Rivkin 2004, Jackson 2009, Boyd, et al. 2008). As such, it is unclear how much of the benefits from attending schools with higher achieving peers are through the direct effect of being exposed to such peers.

[5] Supporting this notion, researchers have found that parents may chose schools based on the potential peer group rather than a productive advantage (Willms and Echols 1992, Black 1999, Rothstein 2006).

[6] This channel could potentially explain some of the varied results in the charter school literature, and the fact that start-ups (that admit new student bodies) are found to be more successful than conversion charter schools.

the relationship between peer quality and school effectiveness, no empirical evidence exists on the extent to which peer effects can account for school value-added. That is, there is no evidence on whether successful school models can be replicated.

To decompose a school effect into a peer effect and an input effect poses empirical difficulties because; (a) students select to schools, (b) students select to peers, and (c) school quality and peer quality generally move together. One needs clean estimates of both peer and school quality based on independent sources of variation *on the same outcomes for the same student population*. While impossible in most settings, the peculiarities of the Trinidad and Tobago education system provide a unique opportunity to overcome these difficulties.

Exploiting these peculiarities (detailed below), I use data from Trinidad and Tobago to analyze the relationship between school value-added and peer quality. Specifically, I (1) estimate the effect of attending a secondary school with higher-achieving peers, (2) estimate the effect of increases in peer achievement within a school, and (3) compare these two estimates to gain some understanding of how much of the benefits to attending a high-achievement school can be attributed to the direct effect of having higher-achieving peers, and how much could potentially be explained by other factors that can be manipulated by school leaders and policy-makers.

The Trinidad and Tobago context is well suited to addressing these questions because: (1) students with high incoming test scores are assigned to secondary schools with other high-achieving students and *vice versa* ─ creating large differences in peer quality across schools; (2) Secondary schools have long-standing reputations so that differences in peer achievement across schools are associated with meaningful differences in inputs across schools that developed over time[7] ─ something that cannot be created in a randomized experiment; and (3) Students are assigned to secondary schools by the Ministry of Education so that one can remove self-selection bias and obtain credible estimates of *both* the effect of schools and the effect of peers.

To address concerns that peer quality and input quality are highly collinear, I identify the effect of attending a school with higher-achieving peers using variation across schools, and I identify the direct effect of exposure to higher-achieving peers using variation in peer

Researchers often find negative or no effect of charter schools relative to traditional schools where charters schools draw students with lower initial achievement than traditional school students (E. A. Hanushek, J. F. Kain, et al. 2005, Bifulco and Ladd 2006) modest gains relative to traditional schools for charter schools that draw students similar to traditional school students (Hoxby and Rockoff 2004, Hoxby and Murarka 2007) and large positive effects for charter schools that attract high-achieving students (Abdulkadiroglu, et al. 2009).

[7] For example, *all else equal*, since teachers may prefer to teach at the more elite high-achieving secondary schools, there may be large differences in the observable characteristics of teacher across schools.

achievement across cohorts within schools ─ effectively holding input quality constant.[8] To address concerns that students may self-select to schools and peers, I restrict analysis to a sub-sample where students are assigned to schools by the Ministry of education based on observable characteristics that I can directly control for ─ precluding self-selection to one's assigned school or assigned peers. I then use the exogenous (but not random) assignments to construct instruments for students' actual schools attended and students' actual peers.[9]

In Trinidad and Tobago, students take the Secondary Entrance Examination (SEA) at the end of 5th grade and list four ordered secondary school choices. These test scores and school choices are sent to the Ministry of Education, and based on these two pieces of information students are assigned to secondary schools. I present empirical tests suggesting that the school assignments are conditionally exogenous. Note that the vast majority of variation in secondary school attendance is driven by student ability and student school preferences (unlike other national contexts where most variation comes from differences in neighborhoods or other unobserved factors). As such, having data on student school choices and incoming test scores allows me to control *directly* for sources of student selection not possible in most datasets.

One remaining concern is that changes in peer quality over time within a school may be correlated with changes in input quality if schools that fall out of favor lose resources at the same time that high-achieving students no longer apply to that school. Fortunately, given that I can observe students' school choices, a school's desirability is directly observable in the data. To assuage such concerns, I show that schools' *rankings* in peer achievement level are virtually unchanging over time so that changes in peer quality are driven by idiosyncratic perturbations in population sizes, demographics, and preferences. I also present other empirical tests suggesting that endogenous peer achievement changes are unlikely. Furthermore, the sample of schools used are centrally operated by the Ministry of Education so that improvements in school inputs are slow and are not undertaken on a school-by-school basis ─ making a correlation between year-to-year changes in assigned peer quality and year-to-year changes in inputs unlikely.

[8] Similar approaches to identifying peer effects has been used successfully by Ammermueller and Pischke (2006), Lavy and Schlosser (2009), Hoxby (2000), and Hanushek, et al. (2003).

[9] Part of the benefits to attending a school with higher achieving peers has to do with the fact that schools that always have high-achieving students can attract better teachers, wealthier students, and lobby for better school inputs. Randomization of students to schools will get rid of this long-run effect that occurs *in the real world* where schools attract students based on historical performance or reputation. As such, the institutional setup in Trinidad and Tobago (where variation is conditionally exogenous, but not random) is much closer to the ideal setup for addressing this particular question than a randomized experiment.

Students are assigned to secondary school after 5th grade and take a series of secondary school-leaving exams at the end of 10th grade. I use the number of these secondary school-leaving exams passed as the main outcome. This variable is a nice summary statistic for overall educational output because it is sensitive to dropping out of school, the number of exams attempted, and performance on a given exam. Because school prestige is highly correlated with incoming peer achievement I classify schools based on the achievement of peers *before entering secondary school*.[10] Classifying schools and peers in the same manner allows for a decomposition of a school effect into a direct peer effect and an other-input effect.

All specifications indicate that attending schools with higher incoming peer achievement increases the number of exams passed and that increases in mean peer achievement *within a school* increases the number of exams passed. These marginal effects vary considerably across schools. Specifically, (1) the benefits to attending a school with higher-achieving peers are largest among schools high peer achievement levels, and (2) the benefits of increases in peer achievement within schools are largest at those high peer achievement schools. Direct peer quality can account for as much as two-thirds of school value-added among the top quartile of schools, but little for the lower three quartiles of schools. Analyses by gender provide further evidence that a sizable part of the school effect is a peer effect. Specifically, females benefit thirty percent more than males from attending schools with higher achieving peers, and gender differences in the response to peers completely explain gender differences in response to schools.

Understanding the extent to which peer quality can account for school value-added is key to understating whether models of successful school are replicable. While the Trinidad and Tobago context may not generalize to all settings, the findings are an important contribution to the education literature because this is the first paper to credibly show that, among the most successful schools, much of the value-added can be directly attributed to the quality of the peers. The findings underscore that to understand how to improve student outcomes we must not only know which schools are successful, but must also understand why.

Section II lays out the theoretical framework. Section III describes the Trinidad and Tobago education system and the data. Section IV lays out the identification strategies. Section V presents specification tests, results, and robustness checks. Section VI concludes.

[10] This also avoids the reflection problem (Manski 1993).

II Econometric Framework

I present a model showing that, under reasonable assumptions, the ratio of the coefficient on peer quality obtained using variation across schools and the coefficient on peer quality obtained using variation within-schools yields an estimate of the proportion of the effect of attending a school with higher-achieving peers that can be directly attributed to those higher-achieving peers (versus other inputs that vary across schools).[11]

Input quality at school *j* at time *t*, denoted I_{jt}, is a linear function of long-run peer quality $\overline{P}_j$ and idiosyncratic determinants u_{jt}. This is written as [1] below.

[1] $$I_{jt} = \pi \overline{P}_j + u_{jt} \quad \text{where} \quad \mathrm{E}[u_{jt} \mid \overline{P}_j] = \mathrm{E}[u_{jt}] = 0 \, .$$

This captures the fact that school inputs are endogenous to the student body. For example, teacher quality and alumni donations may depend on the characteristics of students. Input quality is not a function of contemporaneous peer quality because input quality changes are likely not sensitive to transitory shocks to peer quality and changes in input quality in response to changes in peer quality will exhibit a considerable lag.

Peer quality is comprised of long-run component $\overline{P}_j$ and idiosyncratic component μ_{jt}.

[2] $$P_{jt} = \overline{P}_j + \mu_{jt} \quad \text{where} \quad \mathrm{E}[\mu_{jt} \mid \overline{P}_j, u_{jt}] = \mathrm{E}[\mu_{jt}] = 0 \, .$$

The long-run component plus random error captures the fact that the schools that attract the highest/lowest achieving students have done so for years. This modeling assumption would seem natural for those familiar with secondary schools in Trinidad and Tobago as it is akin to saying that Harvard and Yale (or Oxford and Cambridge) always attract the top students in any given year and have done so for several years. To support this assumption, in section V.1, I show that school rankings in mean peer quality have been very stable over the sample period.

Under the assumption of additive separability, the structural statistical model of student achievement as a function of input quality and peer quality is given by [3] below, where Y_{ijt} is the achievement level of student *i* at school *j* at time *t*, and ε_{ijt} is an idiosyncratic error term.

[3] $$Y_{ijt} = \alpha + \beta P_{jt} + \gamma I_{jt} + \varepsilon_{ijt} \quad \text{where} \quad \mathrm{E}[\varepsilon_{ijt} \mid P_{jt}, I_{jt}] = E[\varepsilon_{ijt}] = 0 \, .$$

[11] It is important to note that *like all studies on peer effects*, the direct effect of peers will reflect a combination of direct peer interactions, teacher interactions that are affected directly by the classroom composition, and any other contemporaneous changes in the school environment directly associated with peers. This is distinct from indirect input quality effects that are a function of long-run peer quality.

Substituting [1] into [3], the expected difference in student outcomes across schools *j* and school *j'* conditional on peer quality is given by [4] below.

[4] $$E[Y_{ijt} - Y_{i'j't} \mid P_{jt}, P_{j't}] = \beta(P_{jt} - P_{j't}) + \gamma(I_{jt} - I_{j't}) = (\beta + \gamma\pi)(P_{jt} - P_{j't}).$$

Equation [4] illustrates that, under the identifying assumptions in [1],[2], and [3][12], differences in student achievement across schools with different levels of peer achievement will reflect both the direct marginal effect of peers on student achievement β, and the indirect marginal effect of peers through peers affecting the quality of school inputs $\gamma\pi$.

Consider now the expected difference in outcomes across cohorts within the same school exposed to different peers over time. Because *in expectation* there are no systematic differences in input quality within schools over time, the expected difference in student outcomes across times *t* and time *t-1*, within school *j* conditional on peer quality is given by [5] below.

[5] $$E[Y_{ijt} - Y_{i'jt-1} \mid P_{jt}, P_{jt-1}, J = j] = \beta(P_{jt} - P_{jt-1}).$$

Equation 5 illustrates that, under the identifying assumptions in [1],[2], and [3][13], comparing the outcomes of observationally similar students attending the same school but exposed to different peers because they attend at different times, yields the direct effect of peers on outcomes β.

The difference between the coefficient on peer quality obtained across schools and that obtained within schools will be $\gamma\pi$, the effect of a marginal increase in long-run peer quality through its effect on the level of other inputs. As such, $\beta/(\beta + \gamma\pi)$, the coefficient on within-school changes in peer quality divided by the coefficient on peer quality across schools, provides an estimate of the fraction of the benefits to attending a school with higher-achieving peers that can be attributed directly to the achievement level of those peers. In section IV, I detail strategies that will hopefully yield consistent estimates of β and $\beta + \gamma\pi$.

The decomposition relies on the assumptions of linearity and the additive separability of inputs and peers. While these assumptions are somewhat restrictive, they allow one to say something meaningful about the important relationship between peer effects and school effects. While these assumptions may be accurate for small changes in peer achievement, they may not hold for the entire universe of schools. As such, in the final preferred set of results I relax the

[12] That peer quality at a school and input quality at a school are uncorrelated with *__unobserved__* determinants of student achievement.

[13] That changes in peer quality within a school over time are uncorrelated with changes in *__unobserved__* determinants of student achievement and changes in other unobserved school inputs over time.

assumption of linearity and additive separability and present findings for sub-samples of schools with similar levels of achievement (within which these assumptions are likely to hold).

III The Trinidad and Tobago Education System and the Data.

The Trinidad and Tobago education system evolved from the English education system. Secondary school begins in first form (the equivalent of 6^{th} grade) and ends at fifth form (the equivalent of 10^{th} grade) when students take the Caribbean Secondary Education Certification (CSEC) examinations. These are the Caribbean equivalent of the British Ordinary levels (O-levels) examinations.[14] The CSEC exams are externally graded by examiners appointed by the Caribbean Examinations Council. Students seeking to continue their education take five or more subjects, and the vast majority of testers take the English language and mathematics exams.[15]

In Trinidad and Tobago, there are eight educational school districts. Unlike in many countries where private schools are often of higher perceived quality, private schools in Trinidad and Tobago account for a small share of student enrollment and tend to serve those who "fall through the cracks" in the public system.[16] There are three types of public secondary schools: Government schools, Comprehensive schools, and Government Assisted schools. Government schools are secondary schools that provide instruction from 6^{th} through 10^{th} grade and often continue to 12^{th} grade (called upper-sixth form). These schools teach the national curriculum and are fully funded and operated by the Government. The second type of school, Comprehensive schools, are Government schools that were *historically* vocational in focus. In the past, students with low test scores after 5^{th} 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. Senior secondary schools have been phased out, so that by 1995, the relevant sample period, all schools taught the same academic curriculum and

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[14] There are 31 CSEC subjects covering a range of purely academic subjects such as Physics, Chemistry and Geography, and vocational subjects such as Technical Drawing and Principles of Business and Office Procedures.
[15] The CSEC examinations are accepted as an entry qualification for higher education in Canada, the United Kingdom and the United States. After taking the CSEC, students may continue to take the Caribbean Advanced Proficiency Examinations (CAPE), at the end of sixth form (the equivalent of grade 12), which is considered tertiary level education but is a prerequisite for admission to the University of the West Indies (the largest University in the Caribbean and is the primary institution of higher learning for those seeking to continue academic studies). The CAPE is the Caribbean equivalent of the English Advanced Levels (A-Levels) examinations.
[16] This is evidenced by the fact that students who attend private secondary schools have test scores that are a third of a standard deviation lower than the average SEA taking student, and half a standard deviation lower than the average among those students who take the CSEC exams.

only a handful of Comprehensive schools did not provide instruction through to the CSEC exams.[17] The third type of school, Government Assisted schools (Assisted schools), are often the more elite schools and are like Government schools but differ along a few key dimensions. Assisted schools are run by private bodies (usually a religious board) and, while capital expenses are publicly funded, their teacher costs are not paid for by the Government. Another key difference between Assisted schools and government or Comprehensive schools is that the Ministry of education only assigns 80 percent of the student body at Assisted schools, while the Ministry assigns all slots at Government and comprehensive schools. This distinction is key in finding a sample of students within which variation in schools attended and changes in peer quality are not subject to self-selection bias. Along all other dimensions, Government and Government Assisted schools are identical.

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

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

This process is used to assign over 95 percent of all students. However, there is a group of students for whom this mechanism is *not necessarily* be used. Government Assisted schools (which account for about 16 percent of school slots) are allowed to admit 20 percent of their incoming class at the principal's discretion. As such, the rule is used to assign 80 percent of the students at these schools, while the remaining 20 percent can be handpicked by the school principal before the next highest ranked school fills any of its slots.[18] Only after all the spots (the assigned 80 percent and the hand-picked 20 percent) at the highest ranked school have been filled will the process be repeated for the remaining schools.

Unfortunately, the actual cut-off scores for each school are not released to the public. However, because the rules are known and I have the same information that the Ministry of Education used to assign students, I can determine where the cut-offs would have been if Assisted schools could not hand pick students.[19] Using this "simulated" cut-off, I estimate the likelihood of attending one's top choice school as a function of one's score relative to the cut-off for one's top choice school. I present this in Figure 1. There is a sudden increase in the likelihood of assignment to one's top choice school as one's score goes from below to above the simulated cut off — indicating that the assignments operate as described. The fact that assignments are orthogonal to unobserved student characteristics (where student school choices and incoming test scores are observed and can be controlled for) within the sub-sample of students assigned to government and comprehensive schools plays a crucial role for identification in this paper. [20]

In general students tend to put schools with higher-achieving peers higher up on their preference ranking. Figure 2 shows the cumulative distribution of the mean peer incoming SEA scores of students' school choices. The distribution of mean SEA scores of first choice schools is to the right of the second choice schools which is to the right of the third choice schools which, in turn, is to the right of the fourth choice schools. On average the difference between the mean incoming SEA scores at a student's top choice school and second, third and fourth choice school

[18] For example, suppose the highest ranked school has 100 slots and is a Government Assisted school. The top 80 students will be assigned to that school while the principal will be able to hand pick 20 other students who listed the school as their top choice. The remaining 20 students would be chosen based on family alumni connections, being relatives of teachers or religious affiliation (Since Government assisted schools are run by religious bodies).
[19] I detail how this is constructed in Appendix Note 1.
[20] This sudden change in the likelihood of attending one top choice school is amenable to a regression discontinuity (RD) type analysis. However, this is not the approach taken in this paper because (a) there are other useful sources of exogenous variation described in section IV, and (b) the within-school analysis to identify the effect of peers cannot be conducted in an RD-type model. However, as a robustness check, I do show that the cross-sectional results based on my preferred strategy and an RD-type design are essentially the same.

is 0.277, 0.531, and 0.91 standard deviations, respectively.

Student school choices are also based on their own perceived ability, preferences for schools, geography, and religion. Higher ability students tend to have higher achievement schools in their list, students request schools with the same religious affiliation as their own, and students typically list schools that are geographically close to their homes. It is important to note that Trinidad and Tobago is small so that attending school "far" from home is feasible. The fact that the set of school choices is a summary statistic for a student's aspirations, preferences for schools, expectations about their own ability, parental aspirations, parental expectations, religious affiliation and geographic location makes these choices a powerful set of controls.

By making inferences among students with the same school choices, one can control for sources of bias not possible in many datasets. The fact that differences in school assignments among students with the same school choices are driven by the exogenously set cut-offs discussed above, deals with concerns that not all persons with the same choices are *identical*.

III.2. Data and Summary Statistics: The data used in this study come from two sources: the official SEA test score data (5th grade) for the 1995 through 2002 cohorts and the official 2000 through 2007 CSEC test score data (10th grade). The SEA data contain each of the nation's student's SEA test scores, their list of preferred secondary schools, their gender, age, religion code,[21] primary school district, and the secondary school to which they were assigned by the Ministry of Education. The SEA exam is comprised of five subjects that all students take: math, English, science, social studies, and an essay. To track these 5th grade students through to secondary school, I link the SEA data with the CSEC examination data both four and five years later. About 72 percent of SEA test takers were linked to CSEC exam data.[22] The CSEC data contain each student's grades on each CSEC exam and secondary school they attended. In the data, there are 123 public secondary schools, some small test-taking centers and private 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.

[21] To preserve confidentiality I was not given access to the actual religion, but a religion code.

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

Because students who are assigned to Assisted schools could have been hand-picked by the school's principal, I drop students assigned to Assisted schools. Dropping students who actually *attend* Assisted schools would generate a sample selection bias so I only drop those who are *assigned* to Assisted schools and present instrumental variables estimates based on the exogenous assignments. Students who scored too low on the SEA exams to be assigned to schools are dropped from the sample. Students assigned to temporary or private schools (at times the government will purchase private school slots) are also dropped from the sample. This accounts for 2 percent of the sample and has no impact on the estimates. The resulting analytical dataset contains 150,701 students across seven cohorts and 158 school assignments.[23]

Table 1 summarizes the final dataset, broken up by the assigned secondary schools' rankings in incoming SEA scores (i.e. the school with the highest average incoming total SEA scores is ranked first and the school with the lowest average total SEA scores is ranked last). The SEA scores have been normalized each year to have a mean of zero and a standard deviation of one. In this sample, females make up about half of students in each school group. There is substantial variation in school and peer quality in Trinidad and Tobago. Schools ranked in the top 30 had students with about one standard deviation higher incoming SEA scores than schools ranked between 31 and 90, which in turn had students with average incoming scores over half a standard deviation higher than schools ranked below 90. The difference in mean test scores between the top and bottom schools is 3.04 standard deviations. Appendix Figure A1 shows the distribution of total SEA scores for schools with different ranks in mean peer quality.

As one might expect, those schools that have the brightest peers also have the best outcomes. About 87 percent of students at schools ranked better than 30 took the CSEC exams compared to 71 percent for schools ranked 31 to 90, and 59 percent for schools ranked below 90. The average student at a top 30 school passes 4.44 exams, compared to passing 1.9 exams in schools ranked between 31 and 90 and passing only 1 exam at schools ranked below 90. Some of

[23] There are two quirks in the data that must be addressed. First, there are more assignments than high schools because some Comprehensive schools feed into the same Government secondary school and when there are more students than available spots the government assigns students with the lowest SEA scores to small "temporary" schools or purchases seats in private schools. Omitting students assigned to such schools does not affect the results. The second quirk is that only for the year 2000 all students were assigned to schools irrespective of their SEA scores. As such, in the analytic dataset there are approximately 18,000 students in all years except 2000 when there were 25,496 students. To ensure that the unusually large 2000 cohort does not drive any of the findings, I verify that the results are robust to excluding the year 2000 cohort and also excluding all years after 1999.

these differences reflect the fact that students who do not take the CSEC exams have no passes or exams attempted (this issue is addressed in Section V). The outcome "obtaining a certificate" denotes passing five CSEC subjects including math and English and is a prerequisite for continuing education. Even though I do not analyze this outcome, the large differences in this important outcome across schools are instructive. Half of the students at the top 30 schools earn a certificate, compared to only 12 percent at schools ranked between 31 and 90, and 3.7 percent at schools ranked below 90. Surprisingly, virtually no student who attends a school ranked below 90 satisfies the requirement to continue to 11th and 12th grades.

In Trinidad and Tobago, as in many nations, the schools that attract the brightest students typically have the best school resources. Table 1 documents that schools with the highest achieving students are on average smaller with cohort sizes being about 120 students at the top 30 schools and about 440 students in both other groups of schools. The one input for which there is aggregate data across broad school types (but not at the school level) is teachers. In 1999, 70 percent of teachers at Government schools (where mean total SEA scores were 0.57) had a Bachelors degree compared to only 64 percent for Comprehensive schools (where mean total SEA scores were -0.34). Similarly, 28 percent of teachers at Government schools had an education degree compared to 12 percent for Comprehensive schools (Source: National Institute of Higher Education and Science and Technology 1999).

IV Identification Strategy

Because students who are assigned to non-Assisted schools cannot self-select into the assignment, conditional on the variables used to create the school assignment (incoming test scores and student choices) the actual school assignment within the group of non-Assisted schools is exogenous. As such, even though students may transfer out of their initial school assignment or parents may subsequently use their political and economic influence to affect the schools that students actually attend, I can use the mean peer quality that would prevail if all students complied with their initial school assignment as an instrument for the actual mean peer quality at the school attended. In sections IV.1 and IV.2 below, I detail how I aim to identify (1) the effect of attending a secondary school with marginally higher-achieving peers, and (2) the effect of marginal increases in peer achievement within a school.

IV.1 Across-school model: The logic underlying the identification of school effects is as follows: Conditional on two students having the same test score, differences in school assignments are due to their school choices. Conditional on two students having the same school choices, differences in school assignments are due to their test scores. As such, under the assumption that the interactions between school choices and test scores only affect student achievement via their effect on the assigned school, one can use differences in differences (DID).

Figure 1 shows the variation in assignments due to test score cut-offs generated by the assignment rules conditional student school choices. These cut-offs lead to exogenous variation because individuals with the same school choices may be assigned to different schools because one scored above a school threshold for a preferred school while the other did not. While this sounds amenable to a regression discontinuity type analysis, because (a) I do not know where the actual cutoffs are (b) there is exogenous variation due to differences in student choices, I do not employ such a strategy. The fact that I can observe student choices and control for them directly allows me to use a Difference in Difference (DID) identification strategy to isolate the causal effect of schools.[24] I explain this exogenous DID variation below.

Consider two students (A and A') with the same school choices but with different test scores such that A' is above the cut-off for a preferred school and A is not. Using the same RD-type logic, the only reason that A and A' are in different schools is that one scored above a cut-off while another did not. Consider now two students (B and B') with different school choices from A and A' but with the same school choices as each other. B and B' have different test scores such that B' is above the cut-off for a preferred school and B is not. If the within-pair difference in incoming test scores is the same (that is, the difference in incoming test scores between A and A' is the same as that of B and B'), but the within-pair difference in peer quality is different (that is, the difference in peer achievement between A and A' is different from that of B and B') one can difference out the effect of school choices by making comparisons within pairs, and one can difference out the effect of incoming test scores by making comparison across pairs ─ isolating the effect of pair achievement. Specifically, the difference in difference $(Y_{A'}-Y_{A})$ - $(Y_{B'}-Y_{B})$,

[24] Jackson (2009) simulates where the cut-offs would be had there been no self selection and uses the simulated cut-offs to estimate a simulated RD type model. He finds that results using the RD based model are very similar to those obtained using the DID type variation used in this paper but are much less precise. As a robustness check, in section V, I also show that results based on the discontinuities are statistically indistinguishable from results using the more efficient DID type variation. It is important to note that any discontinuity based strategy used where the cut-offs are not know is not a true regression discontinuity model, and is therefore just another instrumental variables procedure.

where Y_i is the outcome for person i, will reflect the effect of the difference in the within-peer difference in peer quality. The identifying assumption is that the impact of a student's test scores is the same across all sets of choices. In section V.1, I present evidence that identification based on this variation is likely valid, and in section V.3 I show that results from a discontinuity based strategy are statistically indistinguishable from those obtained using the DID variation.

To identify the effect of attending a school with higher-achieving peers, I implement a 2SLS model based on the DID variation described above. Because the school attended, and therefore the average peer quality at the school attended, is subject to self-selection bias, I use *assigned* peer quality (i.e. the average incoming test scores of other students assigned to the same assigned school *j** as student *i* in cohort *c*), $\widetilde{SEA}_{ij^*c}$, as an instrument for *actual* peer quality. Specifically, I predict the outcome of student *i* from cohort *c*, at school *j* with the following system of equations by two-stage-least-squares.

$$[6] \qquad \begin{aligned} \widehat{SEA}_{ijc} &= \sum_{k=1}^{50} I_{SEA_i=k} \cdot \theta_{k1} + \widetilde{SEA}_{ij^*c} \cdot \delta_1 + \sum I_{P_i=p} \cdot \theta_{p1} + X_i\rho_1 + \theta_{c1} + \varepsilon_{ijc1} \\ Y_{ijc} &= \sum_{k=1}^{50} I_{SEA_i=k} \cdot \theta_{k2} + \widehat{SEA}_{ijc} \cdot (\beta + \gamma\pi) + \sum I_{P_i=p} \cdot \theta_{p2} + X_i\rho_2 + \theta_{c2} + \varepsilon_{ijc2} \end{aligned} .$$

In [6], $\widehat{SEA}_{ijc}$ is the mean total SEA scores for students attending the same school *j* as student *i* in cohort *c*, $I_{SEA_i=k}$ is an indicator variable equal to one if the student's SEA score is in test score bin *k (SEA scores are put into 50 bins per year to allow for a flexible relationship between incoming test score and the outcome)* so that θ_{k2} is a test score bin fixed effect, $I_{P_i=p}$ is an indicator variable denoting whether a student has a particular school preference ordering (choices), and $\theta_{p,2}$ is a preference ordering (choices) fixed effect (i.e. there is an indicator variable denoting each distinct ordered list of schools. For example there is a dummy variable for all students who list schools A,B,C,D as their first, second, third and fourth choice schools, and another different indicator variable for all students who list A,B,D,C as their first, second, third and fourth choice schools.), X_i includes student gender, θ_c is a SEA test taking cohort fixed effect, and ε_{ijc} is the idiosyncratic error term.

The 2SLS estimate of the coefficient on peer achievement, $\beta + \gamma\pi$, from [5] should be unbiased since (1) the analytic sample only includes those schools and students for whom the initial school assignment is exogenous conditional on incoming test scores and choices, (2) the excluded instrument, mean peer quality of the students assigned school (based on other students assigned to the school), is not affected by students subsequently transferring to schools they prefer, and (3) the model is based on comparisons within groups of students with *exactly the same set of school choices* — a potentially large source of bias in many observational studies. Inference is based on comparisons within groups of students who are similar in important ways but who were assigned to different schools for reasons beyond their control.

IV.2 Within-school model: To identify the effect of peers independent from that of other school inputs, the empirical strategy is to compare the outcomes of students with the same incoming test scores and the same preferences who were assigned to the same schools at different times and therefore assigned to the same school inputs but different peers. To deal with the fact that students may self-select to peers and the fact that peer quality at a school could also be the result of *other* students self-selecting to schools, I use changes in the assigned peer quality within a school over time (that is, the change in average incoming test scores of all other students *assigned* to the same *assigned* school over time) as an instrument for changes in actual peer quality. To do this I augment the cross-sectional equation [6] to include an assigned school fixed effect θ_{j*} where all other variables are defined as before.

[7]
$$\begin{aligned} \widehat{SEA}_{ijc} &= \sum_{k=1}^{50} I_{SEA_i=k} \cdot \theta_{k1} + \widetilde{SEA}_{ij*c} \cdot \delta_1 + \sum I_{P_i=p} \cdot \theta_{p1} + X_i \rho_1 + \theta_{c1} + \theta_{j*1} + \varepsilon_{ijc1} \\ Y_{ijc} &= \sum_{k=1}^{50} I_{SEA_i=k} \cdot \theta_{k2} + \widehat{SEA}_{ijc} \cdot \beta + \sum I_{P_i=p} \cdot \theta_{p2} + X_i \rho_2 + \theta_{c2} + \theta_{j*2} + \varepsilon_{ijc2} \end{aligned} .$$

In equation [7], I instrument for changes in actual peer quality within assigned schools over time with the changes in peer quality that would prevail if all students complied with their school assignments. The 2SLS estimates of the coefficient on peer achievement from [7] is now only β because the within-school model conditions on the indirect effect of peer quality through input quality. Because there is no opportunity for students to self-select into school assignments or to assigned peers, equation [7] should yield a consistent estimate of the direct effect of peers on student achievement as long as changes in peer achievement are not correlated with *changes* in unobserved school inputs (I address this in the next section). Identification in [7] comes from comparing the outcomes of students who were assigned to the same school, with the same school choices, and the same test score, but who faced different peers as a result of idiosyncratic perturbations in the student assignment mechanism over time.

Variation in assigned peer achievement within assigned schools over time comes from two sources: (1) variation in cohort sizes and the distribution of test scores that affect the applicant pools for each school's fixed number of slots (it is important to note that in the entire country there are only about 30,000 students taking the SEA exams every year so that changes in cohort sizes can and do result in changes in peer quality within schools), and (2) idiosyncratic variation in the distribution of student preferences over time.

Holding preferences fixed, small variation in cohort sizes and small changes in the distribution of test scores affect peer quality within a school because the number of slots in each school is fixed. To illustrate this point I look at the average test scores for students ranked 1 to 100, students ranked 3000 to 3500, and 10000 to 10500 for each year between 1995 and 1998. Figures 3 and 4 show that due to small changes in the distribution of test scores, the average incoming test scores of students ranked 1 to 100 decreased by 0.1 standard deviations between 1995 and 1996 and increased by 0.06 standard deviation between 1996 and 1997. In comparison, mean test scores of students ranked 10000 to 10500 increased by 0.05 standard deviation between 1995 and 1996 and fell by 0.02 standard deviations between 1996 and 1997. If one were to do a similar exercise within a educational district, the year-to-year changes are much larger. For example, the mean test scores of the top 100 students in Mayaro district (accounts for 22 percent of the student population) increased by 0.21 standard deviations between 1995 and 1996, while the mean test scores of students ranked between 2000 and 2500 fell by 0.11 standard deviations during the same time period. These examples illustrate that relatively small changes from year to year in the distribution of test scores can result in sizable changes in peer quality within schools because schools take small slices (between 100 and 500 students) out of the distribution based in large part on student rank.

Since some variation in peer achievement is driven by *aggregate* changes in cohort size, there may be a concern that changes in peer quality may be correlated with changes in cohort size within a school. Since schools must fill a fixed number of school slots every year there should be no correlation between peer quality changes and cohort size within a school over time. I test this empirically. The null hypothesis that within-school changes in assigned mean peer quality are not correlated with within school changes in cohort size yields a p-value of 0.65.

The second source of variation is due to changes in student preferences from year to year that will be reflected in changes in assigned peer quality within a school. Because the assignment mechanism fills schools sequentially and a schools order is based on the score of the last admitted student, small changes in preferences, demographics, and scores can cause a school that fills its slots first in one year to be second or third the following year. As such, small perturbations in the distribution of student choices and test scores play out into meaningful differences in peer quality within schools over time.

This source of variation would be problematic if students knew that certain schools were losing resources (such as good teachers) and, as such, no longer list that school in their choices. There are important reasons to think that this is not a problem in these data. First, there are *ex-ante* reasons to expect that changes in peer quality are not correlated with changes in inputs within schools over time. Because the Government and Comprehensive schools in Trinidad and Tobago receive their funds centrally from the Ministry of Education, *all* funding changes, salary increases, and personnel practices apply to all schools in the analytic sample. The fact that school policies that would affect input quality come from the top down make it unlikely that idiosyncratic changes in assigned peer achievement within assigned schools over time are correlated with changes in input quality within assigned schools over time. Second, because school choices are observed in the data, this scenario is testable. In section V.1 I show that students' rankings of schools over time are very stable so that changes in peer quality are not the result of systematic changes in students expectations about a schools quality, but rather, *idiosyncratic* changes in the distribution of preferences over time.

V Results

V.1 Specification and Falsification Tests: In this section I address the two major potential threats to the validity of the results, and present evidence that the models used are likely valid.

(1) Students may self-select to schools and to peers: I argue that, conditional on test scores and student choice orderings, within the sub-sample of government schools and comprehensive schools, the school assignments, assigned peer quality, and changes in assigned peer quality within an assigned school over time are orthogonal to unobserved student attributes. While this should be true if the assignment mechanism functions correctly, I test the validity of this assumption by seeing if the mean assigned peer achievement at the assigned school is correlated with other observable student characteristics before entering secondary school.

I also estimate the magnitude of any possible bias.[25] Specifically, I run a regression of the outcome on each religion indicator variable, each primary school district indicator variable, and the number of times a student attempted the SEA exams. I then run the falsification test of the pre-treatment covariate on the arguably exogenous assigned peer quality. The product of the coefficient of the pre-treatment covariate on the outcome and the coefficient of mean peer quality on the pre-treatment covariate will yield an estimate of the bias in the estimated relationship between assigned peer quality and the outcome associated with excluding the particular pre-treatment covariate. I calculate the implied bias using both the cross sectional model and the within-school model. The falsification tests and the implied bias are presented in Table 2.

In the cross section, assigned peer quality is not associated with student religion, the number of SEA attempts, or the students' primary school district at the five percent level. There is, however, a marginally statistically significant relationship with the number of SEA attempts, but the implied bias is negligible. In the within-school model, changes in assigned peer quality are not correlated with any of the pre-treatment characteristics at the five percent level, and the implied bias is negligible. In sum, among the 28 models estimated, none are significant at the five percent level and only two are significant at the 10 percent level. One would expect between 2 and 3 to be marginally statistically significant at the 10 percent level due to random chance, so the specification tests suggest that both the within- and across-school models may be valid. In addition, for all variables, the correlations between the instruments and pre high school covariates are too small in magnitude to not affect the results in any meaningful way. [26] It seems likely that the specifications outlined in [5] and [6] may yield consistent and unbiased estimates.

(2) Changes in peer quality could be correlated with changes in input quality: While I show that students do not select to their schools or their peers, to identify the direct effect of peers also requires that changes in peer quality are not correlated with changes in input quality. Since more desirable schools attract higher-achieving students and therefore have brighter peers,

[25] This exercise is similar in spirit to Altonji, Elder and Taber 2005.

[26] For example, the coefficient of -0.008 on religion 5 suggests that students who are assigned to a school where assigned peer quality is one standard deviation higher are 0.8 of a percentage point more likely to be of religion 5. Given that being of religion 5 is associated with passing 0.05 more exams, the implied bias is only -0.00041. This is less than one thousandth of the size of the actual estimated cross sectional effect and about one hundredth the size of the within school coefficient and is therefore of virtually no consequence. For both the cross-sectional models and the within-school models the maximum estimated implied bias is less than 0.003, which is about one eight of the size of the *standard error* for the estimated cross sectional effects and less than on tenth of the of the size of the *standard error* for the estimated within school effects.

one may worry that improvements in input quality at a particular school in a particular year may cause students to rank that school more highly in their preference lists generating a correlation between changes in input quality and changes in peer quality within a school over time. In such a scenario, changes in preferences could *potentially* reflect a declining/rising school that is also losing/gaining quality inputs. While I do not observe input quality directly, all scenarios where changes in inputs lead to changes in the peer quality and *vice versa* involve schools moving up or down the rankings in desirability and therefore peer quality. Fortunately, because I can observe students' school choices and as such schools' rankings in the assignment mechanism, I can test for this possibility directly. To show that this is not a source of bias, I show that such changes in school rankings from the assignment mechanism essentially do not occur in these data.

Table 3 shows the correlation between a school's rank in simulated cut-off scores across years. The correlation between a school's rank across any two adjacent years in the data is at least 0.98 and the correlation between a school's rank in 1995 and seven years later in 2002 is 0.96 — so that it is clear that systematic changes in school rankings are not driving the variation in assigned peer achievement within schools over time. As mentioned previously, this would not be surprising to those familiar with schools in Trinidad and Tobago because these schools have well established reputations and the perceived pecking order of schools have been very stable over a long period of time. The stability of school rankings over time corroborates the *a priori* notion that changes in peer achievement within a school over time are driven by idiosyncratic year-to-year perturbations in cohort size, demographics, and preferences.

Given the stability of school rankings over time, one may worry that there is no within-school variation in assigned peer quality to use for identification. The standard deviation of mean peer achievement overall is 0.67, and the standard deviation of residual mean peer quality (after taking out assigned school fixed effects and cohort fixed effects) is 0.2. This indicates that while much of the variation in peer achievement is *across* schools, there still remains substantial variation in peer achievement *within* schools. As such, I am reasonably confident that the within-school model is identified.

V.2 Main Results

As discussed in section II, imposing the assumption of linearity and additive separability over the full sample of schools could lead to very misleading results if peer effects are non-linear or if inputs and peer quality are not additive in the production function. As such, estimates that allow for non-linearity and complementarities are preferred. However, as a first pass analysis I first present the results that impose these restrictive assumptions on all school and then show how the conclusions change when the restrictions are relaxed.

Table 4 presents the cross-sectional and within-school estimates for the outcome of interest, the number of exams passed, for the full analytic sample. The top panel presents naive OLS results based on students' actual schools attended, the second panel presents reduced forms (RF) effect of assigned peer quality on students' outcomes, and the third panel presents the Instrumental Variables (IV) estimates that use assigned peer quality as an exogenous predictor of actual peer quality. Columns 1 through 3 present the across school models, while columns 4 through 7 present the within school models.

A parsimonious OLS model of the number of exams passed as a function of the mean total scores of the students at the actual school, SEA cohort fixed effects, the student's gender, and a cubic in the total SEA score (column 1 top panel) yields an across school coefficient of 1.044 (se = 0.082). Controlling for student choices (column 2) and including indicator variables for 50 SEA test score groups (column 3) yield very similar estimates of 1.229 (se = 0.073) and 1.235 (se = 0.073), respectively. These OLS results indicate that a student who attends a school where peer test scores 0.2 standard deviations higher (roughly the within school variance in peer quality and the mean difference in peer achievement between a student's top choice and second choice schools) would pass about 0.244 more exams. This is an effect size of 0.11.

The second panel presents the reduced form effect of being assigned to a school with higher achieving peers that should be free from self-selection bias. The reduced form estimates range between 0.437 and 0.574 and are all statistically significant at the 1 percent level ─ suggesting that there is a causal effect of attending a higher achievement school on the number of exams passed. The instrumental variables results in the third panel (row) are similar to the OLS results yielding statistically significant coefficient estimates between 0.85 and 1.177 ─ indicating that after taking self-selection into account, a student who attends a school where peer test scores are 0.2 standard deviations higher would pass between 0.17 and 0.23 more exams.

Columns 4 through 7 present the within-school results that should identify the direct effect of peer achievement on outcomes. The OLS results are based on actual incoming peer achievement at students' actual schools attended and include indicator variables for students' actual school attended. The parsimonious model that includes SEA cohort fixed effects, the student's gender, a cubic of their total SEA score, and fixed effects for the actual school attended (column 4 top panel) yields a within-school coefficient of 0.14 (se = 0.067). Controlling for score group dummies (column 5), student choices (columns 6) and both score group dummies and choices (column 7) yield very similar within-school estimates of 0.144 (se = 0.067), 0.135 (se = 0.072) and 0.141 (se=0.074), respectively. These OLS results indicate that a student would pass about 0.028 more exams than an observationally similar student who attends the same school when peer achievement was 0.2 standard deviations lower. The ratio of interest, the preferred across-school OLS coefficient divided by the preferred within-school OLS coefficient is a statistically significant 0.114 (se = 0.58) ─ suggesting that roughly 11 percent of school value-added can be attributed directly to peer quality differences across schools.[27]

Unlike the across school results, the reduced form within-school estimates (second row, in columns 4 through 7) yield statistically insignificant coefficients between 0.039 and 0.047. Therefore, as one would expect, the IV estimates are also statistically insignificant. These range between 0.069 and 0.085. Taken literally, the point estimates suggest that after taking self-selection bias into account, a student would pass between 0.014 and 0.017 more exams than an observationally similar student who was attended the same school when peer achievement was 0.2 standard deviations lower. The IV estimates are about half the size of the OLS estimates, suggesting that there is positive selection on unobservables to high achieving peers. Note that the first stage F-statistic on assigned peer quality (conditional on assigned school fixed effects) ranges between 400 and 666, so that the results do not suffer from a weak instruments problem. The ratio of the preferred within-school and across-school IV estimates is 0.072 (se=0.122) ─ suggesting that on average roughly 7.2 percent of school value-added can be attributed directly to peer quality differences across schools. Given that peer effects may not be linear and peer quality and inputs may be complementary, the results in Table 4 prove a nice starting point, but are not as conclusive as the more flexible models that I will present.

Effects by gender: Recent findings in the economics literature indicate that girls are more likely to benefit from interventions, neighborhood quality, and school quality than boys (Kling,

[27] The standard error of this ratio of coefficients was computed by estimating both the across school model and the within school models simultaneously (a two-equation model) and then computing the standard error of the nonlinear combination of coefficients using the delta method. This computation is dome by STATA's "nlcom" command.

Liebman and Katz 2007, Angrist, Lang and Oreopoulos 2009, Angrist and Lavy forthcoming, Jackson 2009, Hastings, Kane and Staiger 2006). Also recent papers find that while females benefit from exposure to higher achieving peers, males may actually fare worse. For example (Lavy, Silva and Weinhardt 2010) find that English girls benefit from having a high proportion of very bright while boys are significantly *negatively* affected by having a high proportion of very bright students at school. Also, (Han and Li 2009) find that female Chinese college students benefit from exposure better peers while male do not. Finally, (Deming, Hastings, et al. 2010) find that girls who win admission lotteries to their preferred school are more likely to attend a four-year college, while boys are less likely to do so, and they attribute this effect to a differential gender response to peer achievement. As such, it is reasonable to expect that the effect of school and the effect of peers may differ by gender.

To test for this, I present the same specification as in Table 4 with the inclusion of the interaction between being female and peer quality. As such, the coefficient on mean peer scores is the effect for males, while the coefficient on the interaction between female and mean peer scores is the difference in the marginal effect for males and females. I also compute and present the marginal effect for females (for ease of interpretation). The results are presented in Table 5.

The top panel show the OLS results and columns 1 through 3 present the across school estimates. Across all OLS specifications both males and females benefit from attending schools with higher achieving peers and females benefit more than males. In the preferred model (with preference and score group fixed effects) in column 3, the coefficient on peer scores is 0.931 (se=0.069) and that for the interactions between peer scores and female is 0.579 (se=0.068). This suggest that males and females who attend a school with 0.2 standard deviations higher peer test scores will pass 0.18 and 0.3 more exams, respectively. The effect for females is 62 percent larger than that for males and the difference is statistically significant at the one percent level.

The RF and IV results tell a similar story. The point estimates in the preferred IV model (with preference and score group fixed effects) in column 3 (lowest panel) suggest that males and females who attend a school with 0.2 standard deviations higher peer test scores will pass 0.2 and 0.27 more exams, respectively. The effect for females is 38 percent larger than that for males and the difference is statistically significant at the one percent level.

Columns 4 through 7 present the within school estimates of the direct effect of peers. The OLS results show a consistent pattern of no direct peer effect for males with large peer effects for females. In the preferred OLS model (top panel, column 7) , the coefficient on peer scores is -0.041 (se=0.079) and that for the interactions between peer scores and female is 0.358 (se=0.073). This suggest that females who attend a school during a time when peer test scores are 0.2 standard deviations higher would pass 0.064 more exams while males would be unaffected. One noteworthy finding is that the gender difference in the marginal effect across schools is very similar to the gender difference in the marginal effect within schools─ suggesting that the larger female response to schools may be due to the fact that while all student benefit from better school inputs, females benefit from higher-achieving peers on average while males do not. As mentioned above, this gender difference in response to peers has been found in other contexts.

The within school IV results (bottom panel, columns 4 through 7) tell a similar story to the within school OLS results, but are noisier. Across all but one of the IV models, the coefficient on mean peer quality is negative and not statistically significantly different from zero, while the coefficient on the interaction with female is positive. In the preferred specification (bottom panel, column 7) the coefficient on peer scores is -0.157 (se=0.138) and that for the interaction between peer scores and female is 0.286 (se=0.134). While the direct effects are imprecisely estimated they suggest that females who attend a school during a period when peer test scores are 0.2 standard deviations higher would pass 0.026 more exams while males would pass 0.031 *fewer* exams. Even though the effects by gender are imprecisely estimated, the difference in the marginal effects across gender is precisely estimated and is statistically significant at the 5 percent level.

The similarity between the gender differences across the cross-section and the within-school models is notable. In the preferred instrumental variables models (bottom panel, columns 3 and 7) the gender difference in the school effect is 0.365 while that for the direct effect of peers is 0.286 ─ suggesting that almost 80 percent of the gender difference in response to schools can be explained by gender differences in response to peers. Moreover, one cannot reject the null hypothesis that peer effects account for all of the gender gap in response to schools at the 10 percent level.[28] Consistent with this notion, for the reduced form results, *all* of the gender difference in being assigned to schools can be explained by gender differences in response to assigned peers. The pattern of results are consistent with a psychology literature suggesting that

[28] The difference between the cross-school difference and the within-school gender difference is 0.08. The standard errors on the across school and within school gender differences are 0.12 and 0.07, respectively.

females may be more responsive to peers than males (Cross and Madson 1997, Maccoby and Jacklin 1974, Eagly 1978). Moreover, the results indicate that at least part of the explanation for gender differences in response to schools (in other studies and contexts) has to do with females responding differently to their peers.

In sum, the results in Table 5 indicate that $\beta/(\beta+\gamma\pi)$, the coefficient on within-school changes in peer quality divided by the coefficient on peer quality in the cross section, a measure of the fraction of the benefits to attending a school with higher achieving peers can be attributed to direct peer effects, is about 9.5 percent for females and 15.6 (negative) percent for males.[29] Females who benefit more from exposure to higher achieving peers within the same school benefit more than males from attending schools with high-achieving peers, and the differential peer response can explain all of the differential response to schools ─ compelling evidence that between 9.5 and 9.5+15.6=25.1 percent of the school effect can be directly attributable to peers. Also, the similarity of the gender differences in response to schools $\beta+\gamma\pi$, and the gender differences in response to peers β, suggest that the assumption of additive separability of inputs and peer quality may be a good first approximation.

Relaxing the Additive Separability and Linearity Assumptions: Because peer effects may be non-linear, and peer inputs may be complementary to other inputs, in this section I move away from the restrictive specification that assumes that peer effects and input effects are the same across all schools to a more flexible model that allows for *both non-linearity and additive separability*. Because linearity and additive separability are approximated *locally* by the first order terms of a Taylor expansion of a continuously differentiable function, the assumptions of additive separability and linearity are likely to be a good approximation among schools with similar levels of peer achievement (even if they do not hold *globally*). Within sub-samples of similar schools the *local* cross-sectional effect is approximately $\beta+\gamma\pi$, and the *local* within-school effect is approximately β. Therefore, one can represent global non-linearity and non separability by piecewise linear functions applied to different regions of the data. By doing this one can present estimates of the fraction of school value-added that one can attribute to peers, that are not driven by the modeling assumptions presented in section II.

[29] Readers should note that omitting the 2000 through 2002 SEA cohorts does not qualitatively change the results.

I do this in two ways. First, in a simple commonsense approach, I present the reduced form and instrumental variables results broken up by subsamples of similar schools by the level of peer incoming achievement. The second approach is to present flexible semi-parametric reduced form estimates of the effects of being assigned to schools with higher-achieving peers and the effects of increases in assigned peer achievement within assigned schools over time. These approaches allows one to see if peer quality plays a more important role for school value added for certain schools that others, and if the global estimates pertain to all schools.

Because peer quality and input quality are both higher at high-achieving schools, non-linearity in the across-school effect (i.e. the across-school effect varies with the level of peer achievement at the school) could be the result of either (a) peer effects being non-linear, (b) the marginal effect of peers being a function of input quality (i.e. complementarity of peer inputs and other inputs), or (c) the marginal effect in input effects being non-linear. In contrast, non-linearity in the within-school effects will reflect only (a) and (b) ─ non-linearity in the direct effect of peers. This is helpful because it means that that similarities in the non-linearity in across-school models and within-school models can provide further evidence of the importance of peers in explaining school effects. That is, if the across-school effects are largest among schools for which the within-school effects are largest and *vice versa*, it would imply that direct peer effects are an important component of school value-added. Specifically, if direct peer effects are an important component of school value-added, then one would expect than non-linearity in β will also be present in $\beta + \gamma\pi$. I present evidence that this is the case below.

The top panel of Table 6 presents the across-school estimates and the second panel presents the within school-estimates. Within each panel, the top row shows the reduced form results and the second row presents the instrumental variables results. In columns 1,2, and 3, I present linear peer effect estimates for different subsamples of schools based on rank (while controlling for gender, preference fixed effects, total SEA score, and a cubic in the total SEA score). The reduced-form across-school coefficient on mean peer achievement is 2.7 for the top 30 schools, 0.45 for schools ranked 31 through 90, and 0.313 for the bottom 68 schools (ranked below 90). Consistent with the RF results, the instrumental variables across school coefficient on mean peer achievement is 2.526 for the top 30 schools, 0.826 for schools ranked 31 through 90, and 0.542 for the bottom 68 schools (all effects are significant at the 1 percent level) ─ suggesting that being assigned to a school with marginally higher-achieving peers has a larger positive effect on the number of exams passed among schools with high-achieving peers.

The lower panel of Table 6 presents within-school estimates of the direct contribution of peers for the same groups of schools. All models include assigned school fixed effects, preference fixed effects, gender fixed effects and a cubic in the total SEA score. For increased efficiency, I also include interactions of incoming test scores and cohort indicator variables with gender ─ this has little effect on the point estimates but does reduce the size of the standard errors. The reduced-form within-school coefficients on mean peer achievement is a 1.186 for the top 30 schools (*p*-value= 0.035), a statistically insignificant -0.0165 for schools ranked 31 through 90, and a statistically insignificant 0.047 for the bottom 68 schools (ranked below 90).[30] The instrumental variables within-school coefficients on mean peer achievement is a sizable 1.959 for the top 30 schools (*p*-value= 0.015), a statistically insignificant -0.163 for schools ranked 31 through 90, and a statistically insignificant 0.078 for the bottom 68 schools (ranked below 90). In words, while increases in peer quality have little or no effect in most schools, increases in peer quality have a large positive effect on achievement among the top 30 schools.[31]

The fact that the non-linearity in the school effects track closely the non-linearity in the direct peer effect suggests that among the top 30 schools, some of the increased value-added can be attributed to the direct contribution of peers on outcomes. Consistent with this interpretation, based on the IV results, $\beta / (\beta + \gamma\pi)$, the fraction of the across school effect explained by direct peer influences is 0.78 (*p*-value=0.03) in the top 30 schools, and is not statistically distinguishable from zero at other schools.

These non-linearities across different schools could be driven by heterogeneity in the response to peers across students with different ability levels who typically attend different schools. As one can see in appendix Figure 1, there is substantial overlap in the distributions of incoming test scores across schools of different achievement levels. As such, one can test for this possibility by estimating the within-school model among students with different levels of incoming test scores (irrespective of the schools they attend). Columns 4 through 7 present the results broken up by quartile of the student in incoming SEA scores. None of the within-school models yield results that are close to statistical significance and the pattern of point estimates are not consistent with the results in columns 1 through 3 ─ suggesting that response heterogeneity

[30] If one looks at males and female separately one sees the same pattern.
[31] While not present in here results by gender yield a similar pattern, however the marginal benefits of peers are always higher for females than for males.

by student ability does not drive the non-linear peer effects.

To provide visual evidence of the nonlinear peer effects, the left panel of Figure 5 shows the local polynomial fit of the number of exams passed (after taking out the effects of own incoming test scores, choices and gender) on the mean assigned peer level of the assigned school (this is a semi-parametric representation of the reduced form). Between -2 and 0, there are small increases in the number of examinations passed, however among schools with assigned peer achievement above 0, there are large benefits to attending a school with higher-achieving peers. This is consistent with the findings of Table 6. On the right panel of Figure 5, I show the relationship between the within-school effect of increases in assigned peer achievement over time and the mean peer achievement of the school. Specifically, I estimate the reduced form within-school model for each school, and then fit a local polynomial of the estimated *betas* to the mean assigned peer achievement level of the school. Since the *betas* are the effect of a marginal increase in peer quality holding all else constant, the right panel is a plot of the first partial derivative of achievement with respect to peer quality by initial peer quality. While the relationship is not precisely estimated, one can see that the marginal effect of increasing peer quality within a school is highest among high-achievement schools. Both patterns are consistent with the regression evidence, and both suggest that non-linearity in the cross-sectional effects are driven, in part, by non-linearity in the within-school effects — evidence that direct peer effects are responsible for much of the very high value-added among high-achieving schools but do not explain differences in school value-added for middle- and low-achievement schools.

Interpretation of the non-linear peer effects: Because peer quality and input quality are both higher at high-achieving schools, the non-linearity of the direct peer effects either reflect that marginal increases in peer quality within a school are more effective when peer achievement is already high, or that marginal increases in peer quality within a school are most effective when input quality is high. Because I do not observe input quality I am unable to distinguish these two scenarios. This distinction does not affect the interpretation of the ratio $\beta/(\beta+\gamma\pi)$, but it does have direct implications for how improvements in input quality (or peer quality) may increases school value added. I explain below.

If the non-linearity in the within-school effect is driven by non-linearity in the marginal effect of peers, it would imply that school value-added can be increased by increasing input quality at all schools (and the distribution of inputs across schools would only have distributional effects and would be irrelevant for overall outcomes). It would also imply that one could increase overall student achievement in the population by stratifying students across schools by ability. However, if the non-linearity in the within-school effect is driven by complementarity between peer quality and other inputs, it would imply that the marginal effect of improved inputs will be highest at schools with the highest achieving students. If would also imply that overall education output would be highest if high ability students were sent to the schools with the best inputs.

While being able to determine the optimal distribution of inputs and students across schools is important, the objective of this paper is to establish how mush school value added can be directly attributed to the quality of the peers at the school. As such, this discussion is merely to highlight how the nonlinear peer effects should be interpreted, and to point out that the underlying reasons for the non-linearity has important policy implications. The policy implications behind the non-linearity is beyond the scope of this paper.

Intensive or Extensive Margin?: While the number of exams passed is a good measure of overall academic achievement, one may wonder if these effects are driven by students being less likely to drop out at schools that have higher achieving peers or due to improvements in outcomes conditional on taking the CSEC exams. To get a sense of this, I re-estimate the main preferred specifications using "taking the CSEC exams" as the dependent variable. In the cross-school model one gets a reduced form coefficient of 0.17, indicating that attending a school where peers have 0.2 standard deviations higher achievement would increase the likelihood of taking the CSEC exams by as much as 3.4 percentage points. To get some sense of how this may affect the estimates, I follow a procedure outlined in Angrist (1995) to obtain consistent estimates of the effect on those students who would have taken the CSEC exams irrespective of school or peer quality. This entails estimating the likelihood of taking the CSEC exams based on observed covariates, and then estimating the effect on the number of examinations passed on the sample of CSEC takers while including the estimated likelihood of taking the CSEC exams as a covariate. Such a model yields an extensive margin across school coefficient of 1.16 (se = 0.119) ─ indicating that much of the cross school effect was on the intensive margin.

In contrast, the within-school model on CSEC taking yields a coefficient of -.0001496 with a *p*-value of 0.99. This suggests that none of the direct effects of peers, on average, are driven by changes in the extensive margin, but are through improvements in achievement conditional on taking the CSEC exams. Even at the top 30 schools where there are large positive effects on the number of exams passed, the coefficient on taking the CSEC exams is a statistically insignificant 0.142 (se = 0.141). In a model that includes the estimated propensity score and uses only CSEC takers, the within school extensive margin coefficient is 1.46 (se = 1.01), very similar to the effect in Table 6─ suggesting that much of the direct peer effect is on intensive margin. The loss of statistical significance for the intensive margin within school effect is likely due to collinearity with the estimated propensity which has a *p*-value of 0.7.

V.3 Robustness Checks

While there are a priori reasons to believe that the results presented in section V.2 are not driven by self-selection and reflect true causal effects, there are a few lingering concerns one may have. I present these concerns and address them in turn.

(1) ***The difference in difference variation may not be clean*****:** Given that the source of the exogenous variation exploited in this paper is driven *in part* by the test score cut-offs, it is helpful to show that the main cross sectional results are robust to exploiting this discontinuity only, and not relying on explicit controls for school choices. To do this, I simulate where the cut offs for each school would be if no students could be handpicked by school principals.[32] One can see from Figure 6 that while one *can* tease out a discontinuous increase in peer quality associated with scoring just above a simulated cut off for the first choice or second choice school econometrically, the visual evidence of a sharp discontinuity is not very strong (this is due to the fact that the cut-offs are simulated and there is not always full compliance). However, econometric models reject the null hypothesis of a smooth increase in peer quality through the simulated cut-offs at less than the one percent level. To present a formal test that the variation due to the cut-offs yields similar results to using the school assignments as instruments, I create three variables that denote whether a student scores above the simulated cut-off for their first second, and third, choice schools. I then use these three variables as instruments *en lieu* of the assigned peer quality. The results are presented in Table 7. Looking to the first stage regression, Scoring above the cut-off for the first, second and third choice schools are associated with attending school with peers with 0.033, 0.056 and 0.076 standard deviations higher incoming test

[32] See appendix note 1 for details.

scores, respectively. The reduced form regression indicates that scoring above the cut-off for the first, second and third choice schools are associated with passing -0.003 (se=0.44), 0.052 (se=0.36) and 0.072 (0.033) more exams, respectively. Using these three variables as instruments yields a coefficient on mean peer quality of 0.867 (se=0.254) ─ similar to the estimates obtained in Table 4. As a formal test that the assigned peer quality yield similar results to the cut-off instruments, I include the assigned peer quality as an additional instrument. In such a model (column 5) the coefficient is 1.009 (se=0.074), and the test of overidentifying restrictions yields a *p*-value of 0.79 ─ indicating that the assigned peer quality instrument is consistent with discontinuity based instruments that rely on variation due to the cut-offs.[33] Jackson (2010) uses a single cross-section from the 2001 cohort to implement a discontinuity based model and find estimates that range between 0.42 and 1.02 depending on how one controls for smooth function of the SEA scores and the range of observations used on either side of the simulated cut-off ─ results that are in line with those found in these data. The similarity of the discontinuity based results and the results obtained using the difference in difference model suggest that the assumption that one can control for preference orderings (choices) to remove selection may be valid.

(2) ***The estimated peer effects may be spurious*****:** I argue that the gender differences in response to peers and the differences by the school rank reflect a true causal relationship. As a further test of the validity of the results, I implement a test similar to Mas and Moretti (2009) and Jackson and Bruegmann (2009) where I include the current peers (for which there should be a true treatment effect) and the peer quality of the preceding cohort and the following cohort. If the estimated results are spurious, the coefficients on both peer quality for the preceding and the following cohorts should be similar to the estimated contemporaneous peer effects. Also, insofar as the effect of preceding and following cohorts reflect some underlying spurious relationship, the difference between the magnitudes of these effects will be informative about the size of any bias that may exist in the estimates in Tables 4 through 6. The estimates that include current peers and peer quality in the following and preceding cohorts are presented in columns 1 and 5 of Table 8. To test for gender differences I show the coefficient on current peers and peer quality in the following and preceding cohorts interacted with whether the student is female.

[33] Of the 14 pre-treatment covariates across the three simulated cut-offs (i.e. 42 point estimates) only 3 were statistically significant at the 10 percent level. This is consistent with what one would expect by random chance.

For all models, one rejects the null hypothesis that the subsequent peers and the preceding peers are jointly statistically significant at the 20 percent level. However, similar to Tables 5 and 6, one can reject the null hypothesis that current peer quality has zero effect among the top 30 schools (column 2) and the null hypothesis that females and males have the same response to peers (column 5) at the 10 percent level. While the p-value on the contemporaneous effects for these two models is below 0.1, those for the joint significance of the lag and lead are above 0.7 for both models. If we take the conservative view that the effect on the lag reflects some underlying spurious association, then we could subtract that from the contemporaneous effect to obtain a conservative causal estimate. Doing this for the female interaction results in a conservative reduced form peer effect estimate of 0.085. This is about two thirds of the reduced form estimate obtained in the preferred model in Table 5 ─ suggesting that there is a true gender difference. The same calculation for the top 30 schools yields a conservative reduced form current peer effect estimate of 0.99. This is about 70 percent of the within school reduced form coefficient in Table 6. This conservative estimate implies that 37 percent (as opposed to 78 percent) of the across school effect among the top quartile of schools can be attributed to peers.

(3) ***The effects are consistent with random sampling variation:*** In difference in difference models there is always the concern that inference based on estimated effects could be biased by underlying serial correlation in the data. To asses this problem I follow an approach used by Bertrand, Duflo and Mullainathan (2004). Specifically I create placebo treatments by taking each school and rearranging the actual peer achievement values for a given cohort so that the actual peer achievement are not lined up with the corresponding outcome for that year. I estimate the placebo treatment models based on 100 replications of this reshuffling. I then compare the actual estimate obtained to the distribution of placebo estimates. Since the gender differences and the positive effect of peers among the top 30 school are the estimates that are statistically significant, these are the two models tested. In both cases, none of the 100 replications yielded parameter estimates larger than the actual estimated coefficients, suggesting that the estimates obtained were probably not some random artifact of the data and would not have been obtained merely due to sampling variation.

(4) ***Changes in peer quality could be correlated with changes in input quality over a long time period*:** A concern with *all* within-entity models is that they rely on the assumption that important unobservable characteristics of the entity do not change over time. While this assumption may be plausible over short time periods such as two or three years, it is less plausible over long periods of time. As such, to ensure that the gender differences and the positive peer effect among the top 30 schools are not an artifact of changing inputs, I present results based on two separate time periods (1995 through 1998) and (1999 through 2003). If the within-school results are similar within these two shorter time periods it will suggest that the results were not driven by comparing entities over long periods of time, and it will ensure that the patterns observed are not an artifact of any particular time period. The results by school rank are presented in Table 9. While the first stage regressions are somewhat weak in the sample of the top 30 schools, the 2SLS results tell a consistent story. Among the top 30 schools the 2SLS coefficients on peer quality are large (but imprecisely estimated) for both time periods. In contrast, for the lower ranked schools the 2SLS coefficients are small (and imprecisely estimated) for both time periods. The fact that one obtains large positive coefficients for the top 30 schools in both time periods and small coefficients for the lower ranked schools in both time periods, suggests that the non-linear peer effects documented are not driven by long term changes in inputs over time. More generally, the consistency in the non-linearity (with the caveat that the estimates are not precise) across both time periods gives one confidence that the patterns are real. A similar test for gender differences (not shown in the table) yields a coefficient on the interaction between peer achievement and being female of 0.37 (se=0.18) for 1995 through 1998 data and a coefficient of 0.183 (se=0.12) for the 1999 through 2003 data. Again this is a good indication that the results are not driven by any one time period and that they are probably not driven by changes in unobserved school inputs over time.

VI Conclusions

There is a growing body of evidence based on credible research designs indicating that attending higher achieving-schools may improve students' test scores and may also improve broader outcomes such as course taking, secondary-school graduation, disciplinary incidents, and arrest rates. It is also well accepted that high-achieving schools tend to have student bodies that have better observable or unobservable characteristics than the average school, and there is some credible evidence that students benefit directly from being in schools or classrooms with higher-achieving peers. As such, part of the benefits to attending a high-achievement school may be attributed to the direct benefits associated with having higher-achieving peers. However, due to the difficulty of obtaining data that allows one to credibly estimate both peer effects and the effect of attending particular schools, there is no previous empirical evidence on the relationship between school value-added and peer quality.

Using a unique dataset from Trinidad and Tobago and a carefully selected group of students where there is no self-selection of students to assigned schools or assigned peers, I attempt to overcome a variety of econometric obstacles to estimating credible school value-added effects and direct peer effects. I estimate the effect of attending a school with higher achieving peers in the cross-section, and I estimate the direct effect of peers by comparing students attending the same school over time as the achievement of the assigned peers change. In both models, I use exogenous school assignments to remove self-selection bias. Even though the school assignments should preclude student self-selection *by construction*, I present specification tests indicating that these results are unlikely to be subject to bias. Within this subsample where there is no self-selection of students to assigned schools or peers, I find that being assigned to, and attending, a school with higher-achieving peers is associated with substantial improvements in academic outcomes. However, I find that, on average, improvements in assigned and actual peer quality within a school are associated with small, statistically insignificant improvements. The point estimates suggest that, on average, between 8 and 20 percent of the school effect can be directly attributed to peer quality differences across schools.

Similar to Hastings, Kane and Staiger (2006) the marginal effects of attending a school with higher achieving peers are about 50 percent more for females than for males. Looking at the direct effect of peers, the result suggests that, on average males may perform slightly worse when peer achievement increases within a school while females perform slightly better. These differences in response by gender are statistically significant. Positive effect for females and negative effect for males have also been found in other studies. The gender differences in response to peers can account for all of the gender differences in response to schools — evidence that part of the school effect can be explained by the direct contribution of peers.

There is substantial non-linearity in the effect of attending a school with higher achieving peers. Similar to Ding and Lehrer (2007) and Pop-Eleches and Urquiola (2008), the marginal effect of attending a school with marginally higher achieving peers is greatest among high peer achievement schools. Looking at the direct effect of peers, the marginal effect of improvements in peer achievement within a school is largest at high peer achievement levels. Consistent with direct peer effects being responsible for some of the effect of attending schools with higher achieving peers, the schools within which the marginal effect of attending a school with higher achieving peers is highest are exactly those schools for which the direct contribution of peers is estimated to be the highest. This symmetry in the non-linearity leads me to conclude that that while direct peer effects may explain little of the benefits to attending a school with higher-achieving peers among the bottom 75 percent of schools, over half of the benefit to attending a school with higher-achieving peers among the quarter of schools can be attributed directly to peer quality.

Owing to the uniqueness of the institutional setup in Trinidad and Tobago, this paper is the first to rely on independent exogenous variation in schools attended *and* peer quality on the same student population ─ allowing for a credible decomposition of a school effect into an input affect and a peer effect. While these findings are particular to Trinidad and Tobago, they highlight the importance of understanding the underlying mechanisms through which schools may improve student outcomes. From a policy perspective, the finding that for most schools differences in peer quality do not account for much of the differences in value-added is encouraging, as it leaves open the possibility that schools with higher-achieving students bodies confer greater benefit to students due to school inputs or differences in educational technologies across schools. As such, outcomes of students at the lowest performing school can be improved by adopting some of the practices of more successful higher-achievement schools. However, the finding that peer quality can account for more than half of the value-added at the highest achieving schools suggests that the success associated with transplanting practices and technologies from the *most* successful schools to regular schools may be limited. One important implication of these findings is that the models used at successful schools may not be as successful if taken to scale.

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Tables and Figures

Table 1: *Summary Statistics*

School is ranked 1 through 30 in mean peer quality over time			
Variable	Obs.	Mean	Std. Dev.
Total SEA score	17811	1.102142	0.332909
Female	17811	0.490259	0.499919
Take the CSEC Exams	17811	0.871259	0.334922
Number of Exams Passed	17811	4.443546	2.62827
Certificate [a]	17811	0.507664	0.499955
Cohort size	17811	119.0497	63.57304
School is ranked 31 through 90 in mean peer quality over time			
Variable	Obs.	Mean	Std. Dev.
Total SEA score	84746	0.122312	0.693467
Female	84746	0.498997	0.500002
Take the CSEC Exams	84746	0.705638	0.455758
Number of Exams Passed	84746	1.939006	2.308109
Certificate	84746	0.127593	0.333638
Cohort size	84746	439.6736	218.533
School is ranked below 91 in mean peer quality over time			
Variable	Obs.	Mean	Std. Dev.
Total SEA score	48144	-0.56061	0.654401
Female	48144	0.505089	0.499979
Take the CSEC Exams	48144	0.58593	0.492566
Number of Exams Passed	48144	1.00459	1.746091
Certificate	48144	0.037284	0.189459
Cohort size	48144	443.8953	241.9063

a. Certificate is variable that is equal to 1 if the student passes five CSEC exams including English and Mathematics.

Table 2 : *Falsification Tests and Implied Bias*

Cross-Section Bias tests

Dependent Variable	Religions					SEA Attempts
	1	2	3	4	5	attempts
Mean Total	0.001	0	-0.005	0.009	-0.008	0.013
	[0.004]	[0.006]	[0.005]	[0.006]	[0.005]	[0.007]+
Implied bias	-0.00001	<0.00000	-0.00033	-0.00082	-0.00041	-0.0021

	Districts							
	1	2	3	4	5	6	7	8
Mean Total	-0.003	-0.003	0.004	<0.00000	-0.002	-0.001	<0.00000	<0.00000
	[0.003]	[0.002]	[0.003]	[0.002]	[0.002]	[0.005]	[0.000]	[0.000]
Implied bias	-0.00111	-0.00035	-0.0009	<0.00000	0.00025	-0.00005	0.00004	<0.00000

Within-School Bias tests

Dependent Variable	Religions					SEA Attempts
	1	2	3	4	5	attempts
Mean Total	-0.004	<0.00000	0.019	-0.014	<0.00000	0.005
	[0.005]	[0.008]	[0.015]	[0.012]	[0.009]	[0.012]
Implied bias	-0.00013	0.00001	0.00063	0.00104	-0.00003	-0.001

	Districts							
	1	2	3	4	5	6	7	8
Mean Total	0.004	0.008	-0.024	-0.007	0.004	-0.021	<0.00000	<0.00000
	[0.007]	[0.008]	[0.018]	[0.004]+	[0.013]	[0.016]	[0.000]	[0.000]
Implied bias	0.00077	0.00078	0.00242	-0.00011	-0.00024	-0.00206	0.00001	-0.00003

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

Table 3: *Correlations between schools' ranks across years*

Rank in	1995	1996	1997	1998	1999	2000	2001	2002
1995	1							
1996	0.993	1						
1997	0.9855	0.9914	1					
1998	0.9838	0.9888	0.9929	1				
1999	0.9779	0.9835	0.991	0.9933	1			
2000	0.9724	0.9793	0.981	0.9877	0.9875	1		
2001	0.9622	0.971	0.9717	0.9754	0.9754	0.9833	1	
2002	0.9618	0.9697	0.9701	0.9721	0.9736	0.9824	0.9951	1

Table 4

Effects on the Number of Exams Passed: Full Sample

	1	2	3	4	5	6	7	Ratio[b]
	Cross sectional results			Within School results				
Actual (OLS)	1.044	1.229	1.235	0.14	0.144	0.135	0.141	0.114
	[0.082]**	[0.073]**	[0.073]**	[0.067]*	[0.067]*	[0.072]*	[0.074]*	[0.058]*
Assigned (RF)	0.437	0.57	0.574	0.039	0.043	0.038	0.047	0.082
	[0.062]**	[0.057]**	[0.057]**	[0.058]	[0.059]	[0.079]	[0.079]	[0.77]
Actual (2SLS)	0.851	1.171	1.177	0.07	0.076	0.069	0.085	0.072
	[0.124]**	[0.094]**	[0.092]**	[0.105]	[0.105]	[0.144]	[0.144]	[0.122]
First stage F-Statistic	578.48	390.3	396.23	666.88	453.67	513.023	519.67	
Cohort Fixed Effect?	YES	YES	YES	YES	YES	YES	YES	-
Preference Effects? [a]		YES	YES			YES	YES	-
Score Group Dummies?			YES		YES		YES	-
School Fixed Effects?				YES	YES	YES	YES	-
								-
Observations	150701	150695	150695	150701	150701	150695	150695	-
R-squared	0.39	0.62	0.62	0.41	0.42			-

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

a. note that preferences include gender so that all models with preference fixed effect are within both preference and gender.

b. The estimate of $\beta/(\beta+\pi\delta)$ ─ the coefficient in column 7 divided by the coefficient in column 3. The standard error was computed by stacking the data to estimate both the within and across model simultaneously and then using the delta method.

Table 5

Effects on the Number of Exams Passed: Effects by Gender

	1	2	3	4	5	6	7
	Cross sectional results			Within School results			
	Ordinary Least Squares						
Peer SEA scores	0.771	0.926	0.931	0.002	-0.007	-0.041	-0.041
	[0.071]**	[0.069]**	[0.069]**	[0.049]	[0.049]	[0.078]	[0.079]
Female*Peer SEA scores	0.533	0.578	0.579	0.277	0.3	0.348	0.358
	[0.069]**	[0.068]**	[0.068]**	[0.061]**	[0.062]**	[0.071]**	[0.073]**
Female Effect	1.304	1.504	1.509	0.279	0.293	0.307	0.317
[se]	[0.101]**	[0.086]**	[0.086]**	[0.064]**	[0.064]**	[0.08]**	[0.08]**
	Reduced Form Results						
Assigned Peer SEA scores	0.349	0.515	0.512	-0.08	-0.051	-0.082	-0.08
	[0.053]**	[0.053]**	[0.052]**	[0.065]	[0.057]	[0.082]	[0.079]
Female*Assigned Peer SEA scores	0.128	0.122	0.133	0.066	0.091	0.123	0.139
	[0.076]+	[0.058]*	[0.057]*	[0.073]	[0.063]	[0.062]*	[0.063]*
Female Effect	0.477	0.638	0.645	-0.015	0.04	0.041	0.059
[se]	[0.083]**	[0.054]**	[0.055]**	[0.066]	[0.066]	[0.084]	[0.085]
	Instrumental Variables Results						
Peer SEA scores (2SLS)	0.659	1.013	1.005	-0.143	-0.096	-0.157	-0.157
	[0.105]**	[0.105]**	[0.101]**	[0.114]	[0.098]	[0.142]	[0.138]
Female*Peer SEA scores (2SLS)	0.315	0.34	0.365	0.12	0.176	0.25	0.286
	[0.148]*	[0.121]**	[0.121]**	[0.146]	[0.126]	[0.130]+	[0.134]*
Female Effect	0.974	1.353	1.37	-0.024	0.08	0.092	0.129
[se]	[0.17]**	[0.12]**	[0.12]**	[0.129]	[0.126]	[0.167]	[0.17]
Cohort Fixed Effect?	YES	YES	YES	YES	YES	YES	YES
Preference Effects? [a]	-	YES	YES	-	-	YES	YES
Score Group Dummies?	-	-	YES	-	YES	-	YES
School Fixed Effects?	-	-	-	YES	YES	YES	YES
Observations	150701	150695	150695	150701	150701	150695	150695

Robust standard errors in brackets. Standard errors are adjusted for clustering at the assigned school level.
+ significant at 10%; * significant at 5%; ** significant at 1%
a. note that preferences include gender so that all models with preference fixed effect are within both preference and gender.

Table 6

	Dependent Variable is the Number of Exams Passed						
	Across School Variation						
	1	2	3	4	5	6	7
	Schools rank 1-30	Schools rank 31-90	Schools rank 91+	Students in top SEA quartile	Students in third SEA quartile	Students in second SEA quartile	Students in bottom SEA quartile
Reduced form	2.701	0.453	0.313	1.308	0.466	0.228	0.028
	[0.366]**	[0.081]**	[0.075]**	[0.142]**	[0.075]**	[0.088]**	[0.032]
Mean peer scores (2SLS)	2.526	0.826	0.543	2.457	1.046	0.526	0.079
	[0.323]**	[0.161]**	[0.168]**	[0.262]**	[0.171]**	[0.183]**	[0.058]
	Within School Variation						
Reduced form	1.186	-0.0165	0.047	<0.001	-0.092	-0.164	-0.06
	[0.563]*	[0.099]	[0.118]	[0.245]	[0.162]	[0.121]	[0.243]
Mean peer scores (2SLS)	1.959	-0.163	0.078	0.084	-0.166	-0.329	-0.042
	[0.797]*	[0.184]	[0.215]	[0.490]	[0.292]	[0.232]	[0.087]
Observations	17811	84740	48144	26454	50348	42249	27521
Ratio (2SLS)	0.78	-0.197	0.145	0.034	-0.16	-0.626	-0.537
[se]	[0.33]*	[0.225]	[0.399]	[0.200]	[0.280]	[0.492]	[1.17]

Robust standard errors in brackets. Standard errors are adjusted for clustering at the assigned school level.

+ significant at 10%; * significant at 5%; ** significant at 1%.
All models include preference ordering fixed effects, and control for the total SEA score, its quadratic and its cubic, and gender.

Table 7

2SLS Results using Scoring Above a Simulated Cut-off for a Preferred School as Instruments

	1	2	3	4	5
Depended Variable	Assigned Mean Peer Scores	Actual Mean Peer Scores	Exams Passed	Exams Passed	Exams Passed
	OLS	OLS	OLS	2SLS	2SLS
Mean Peer Score (actual)	-	-	-	0.867	1.009
	-	-	-	[0.254]**	[0.074]**
Above first choice cut-off	0.049	0.033	-0.003	-	-
	[0.021]*	[0.016]*	[0.044]	-	-
Above second choice cut-off	0.07	0.056	0.054	-	-
	[0.022]**	[0.015]**	[0.033]+	-	-
Above third choice cut-off	0.14	0.076	0.072	-	-
	[0.023]**	[0.016]**	[0.033]*	-	-
Polynomial order of Total	5	5	5	5	5
Cohort fixed Effects	YES	YES	YES	YES	YES
Preference fixed effects	YES	YES	YES	YES	YES
P-value of J-Stat	-	-	-	0.63	0.79
Observations	150695	150695	150695	114062	114062

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

Column 4 includes scoring above the cut-offs as excluded instruments. Column 5 includes scoring above the cut-offs and simulated peer quality as excluded instruments.

Table 8

Dependent Variable is the Number of Exams Passed

	1	2	3	4		5
Sample:	Overall	Schools rank 1-30	Schools rank 31-90	Schools rank 91+		All
$Peers_{c-1}$	-0.019	0.735	-0.06	-0.231	$Female*Peers_{c-1}$	0.094
	[0.075]	[1.043]	[0.102]	[0.224]		[0.069]
Peers	-0.064	**1.733**	-0.17	-0.223	Female*Peers	**0.1793**
	[0.075]	**[0.991]+**	[0.112]	[0.142]		**[0.010]+**
$Peers_{c+1}$	0.026	0.109	0.027	0.319	$Female*Peers_{c+1}$	0.057
	[0.058]	[0.713]	[0.092]	[0.200]		[0.135]
Current minus lag	-0.046	0.997	-0.109	0.008		0.0853
Current minus lead	-0.09	1.623	-0.143	-0.542		0.1223
P-value on lag and lead	0.73	0.7	0.75	0.23		0.73
P-value on current	0.39	**0.08**	0.13	0.14		**0.1**

Robust standard errors in brackets are adjusted for clustering at the assigned school level. P-values in parentheses.
+ significant at 10%; * significant at 5%; ** significant at 1%.
All models include preference fixed effects, and control for the total SEA score, its quadratic and its cubic, and gender.
Model 6 also includes the first order effect of the lag and lead of peer quality.

Table 9

	Dependent Variable is the Number of Exams Passed					
	1	2	3	4	5	6
	Within School Variation - 2SLS regressions					
	School rank 1-30		Schools rank 31 - 90		Schools rank 90+	
Year Included	1995 - 1999	2000 - 2003	1995 - 1999	2000 - 2003	1995 - 1999	2000 - 2003
Mean peer scores	4.441	4.038	-0.27	-0.083	0.234	0.093
	[2.866]	[7.178]	[0.356]	[0.138]	[0.338]	[0.150]
First stage F-Statistic	10.2	6.1	41.7	>100	>100	>100
Observations	8608	9203	40495	44245	23629	24515

Standard errors in brackets

+ significant at 10%; * significant at 5%; ** significant at 1%
All models include preference fixed effects, cohort fixed effects, assigned school fixed effects, and controls for gender and the quartic of the incoming Sea score. The excluded instrument is the assigned peer quality at the assigned school.

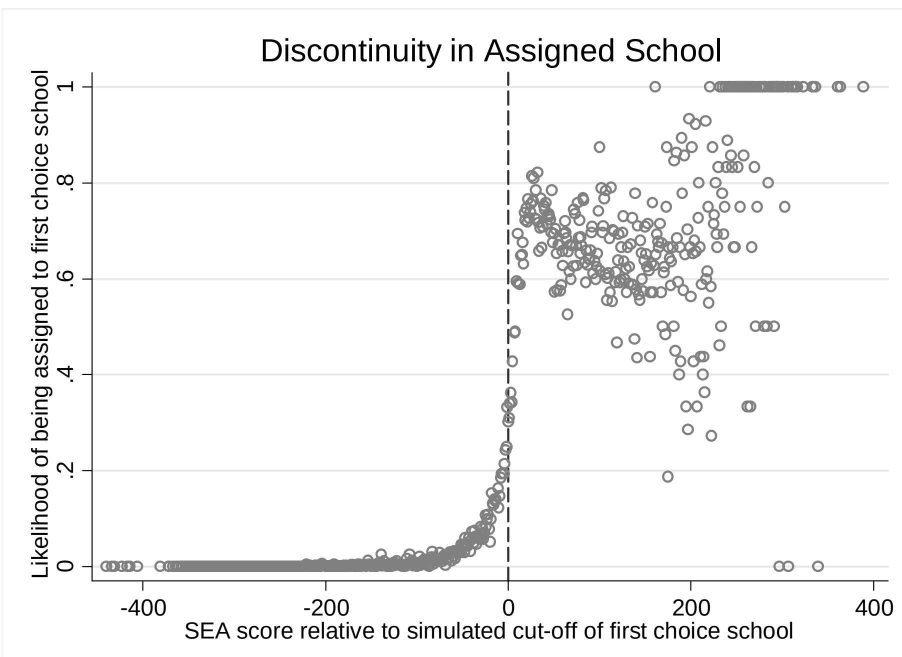

Figure 1: *Likelihood of being Assigned to One's Top Choice School*

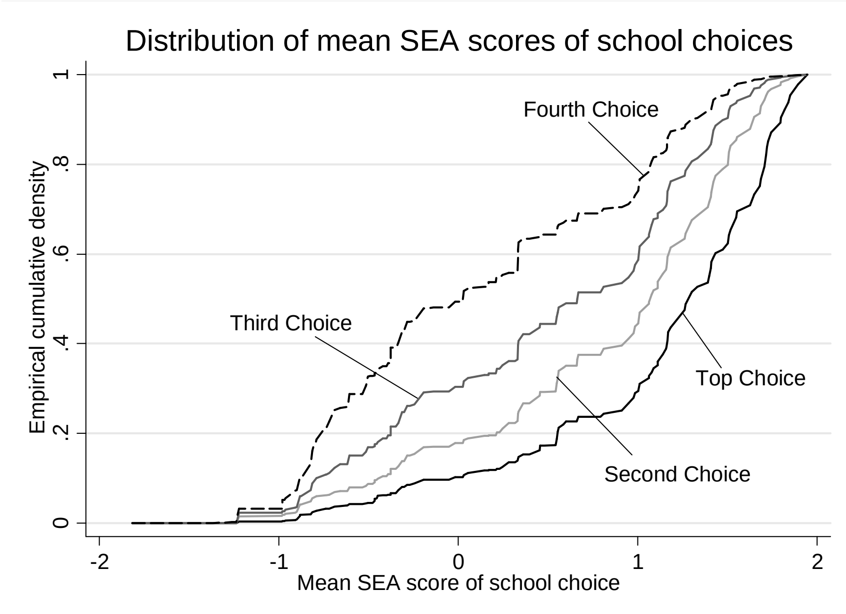

Figure 2: *Distribution of Peer Quality by School Choice Rank*

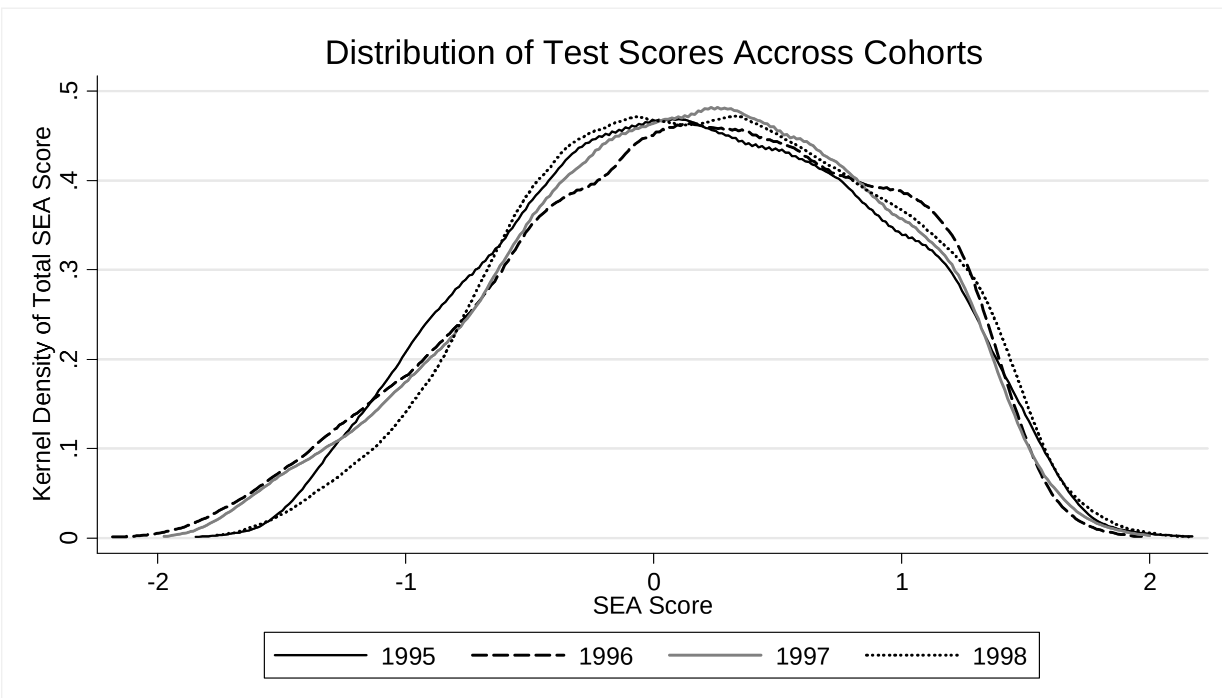

Figure 3: *Distribution of SEA Scores Across Cohorts*

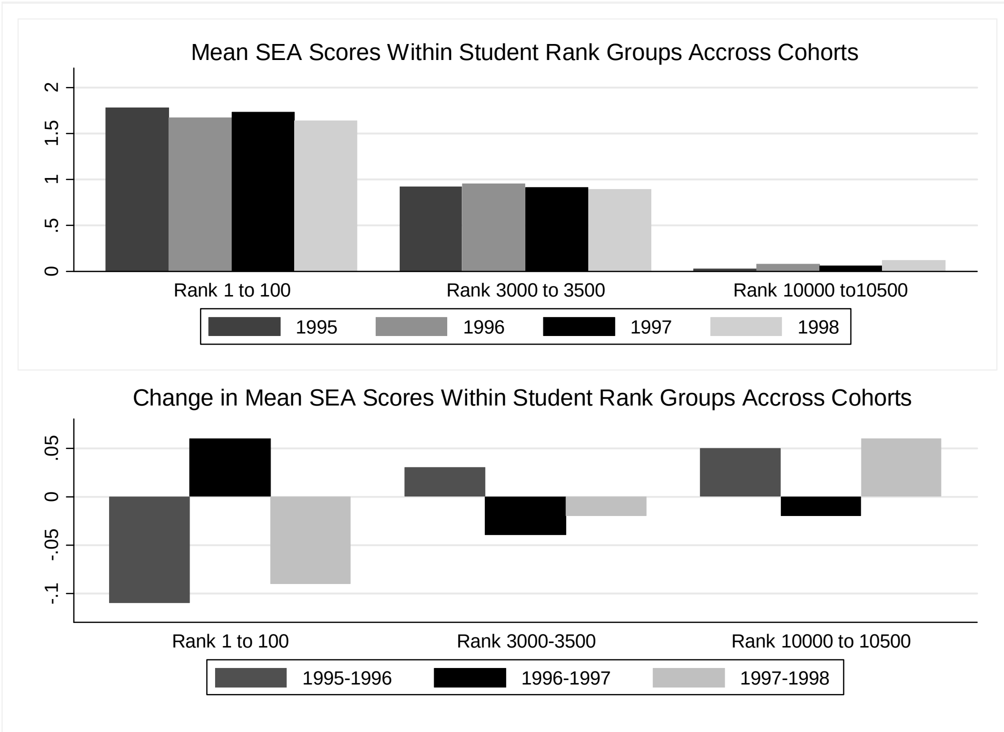

Figure 4: *Hypothetical Changes in Mean Test Scores Driven Only by Changes in SEA Test score Distribution Across Cohorts.*

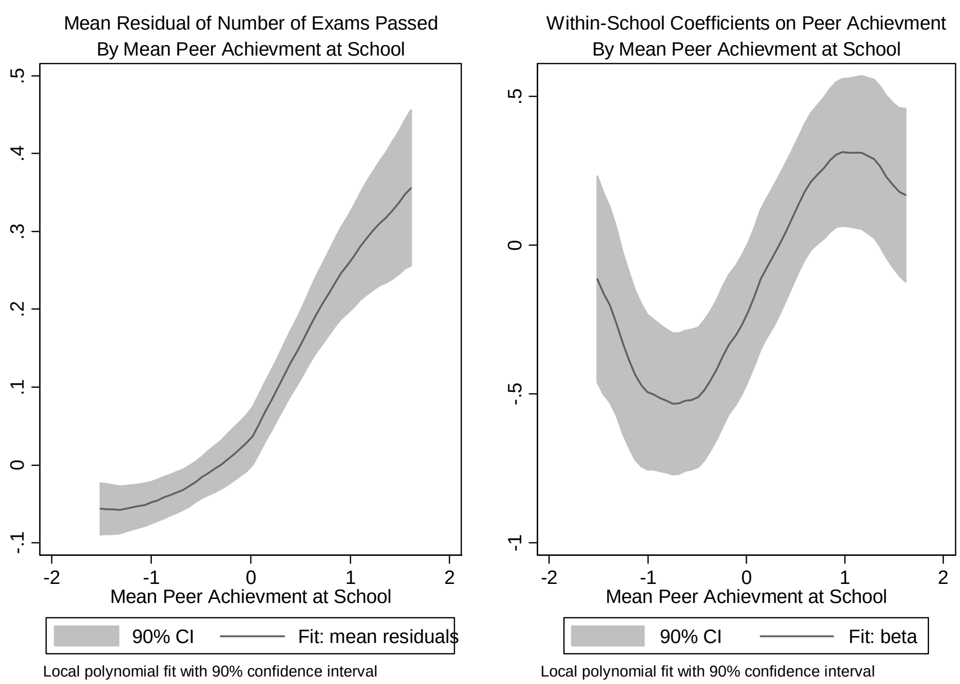

Figure 5: *Graphical evidence of non-linear peer effects*

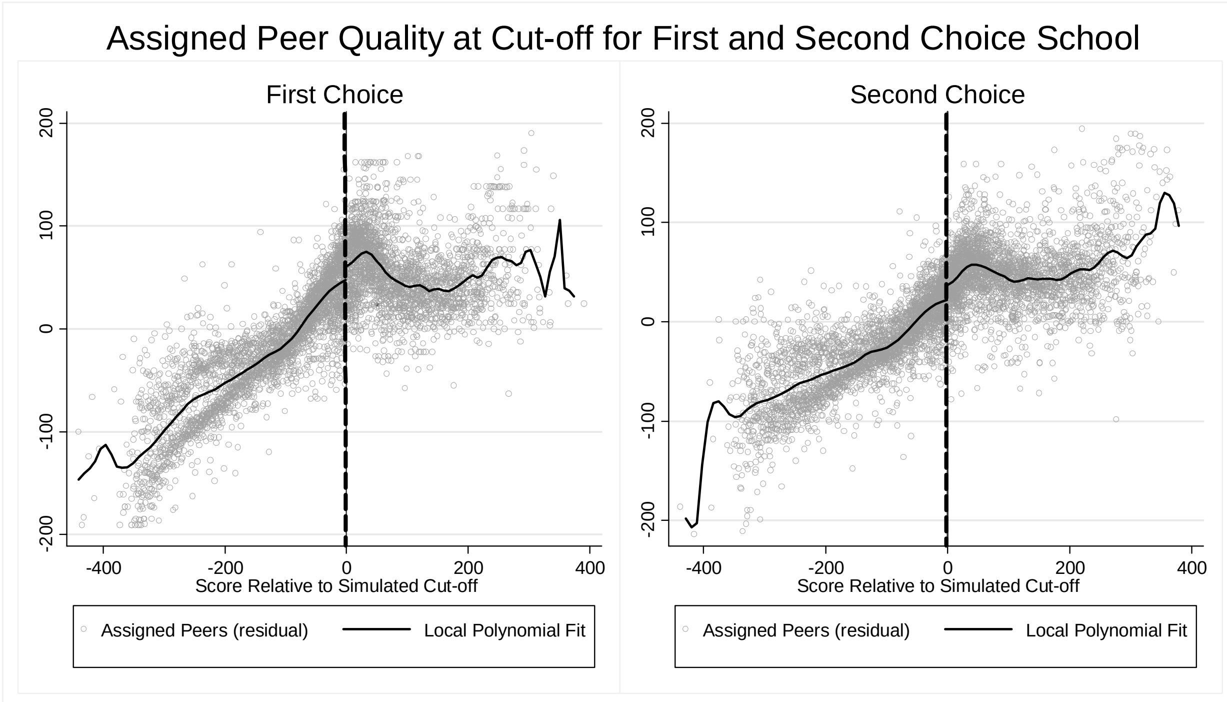

Figure 6: *Discontinuity in assigned peer quality (raw SEA scores are shown) through the assigned cut-off for the first and second choice school.*

Appendix

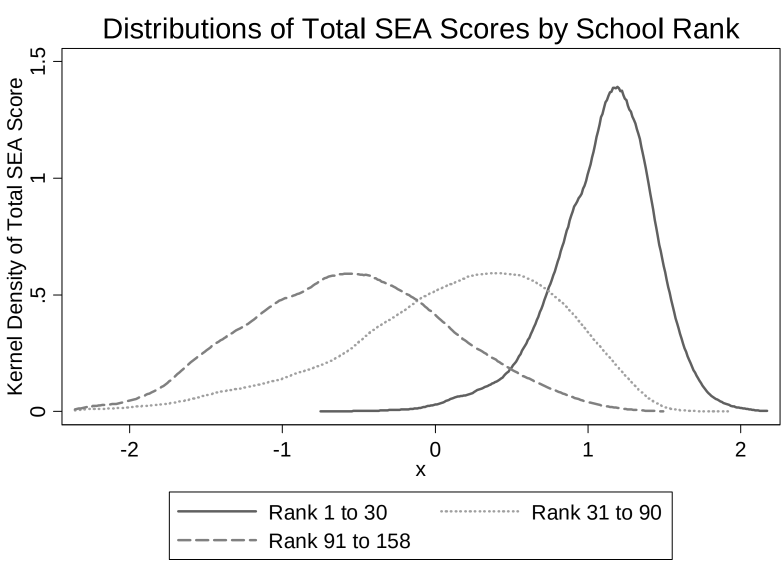

Figure A1: *Distribution of total SEA scores by school rank*

Appendix Note 1: *Constructing the Simulated Cut-off*

The simulated cut-off is constructed sequentially as follows: (1) All secondary school sizes are given,[34] (2) all students are put in the applicant pool for their top choice school, (3) the school for which the first rejected student has the highest test score fills all its slots (with the highest scoring students who listed that school as their first choice), (4) the students who were rejected from the top choice school are placed back into the applicant pool and their second choice school becomes their first choice school, (5) Steps 2 through 5 are repeated, after removing previously assigned students and school slots until the lowest ranked school is filled. The *only* difference between how students are actually assigned and the "tweaked" rule-based assignment is that at step (3) the "tweaked" rule does not allow any students to be hand-picked while, in fact, some students are hand-picked by principals only at Government assisted schools. Jackson (2009) exploits the discontinuities inherent in the assignment mechanisms to identify the effect of attending schools with higher achieving peers. In this paper, I use the school assignments (to government and comprehensive schools) that I know are not driven by any endogenous gaming or self-selection.

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