

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 a growing body of evidence based on credible research designs indicating that attending higher-achieving schools improves students' test scores (Hastings and Weinstein 2007, Pop-Eleches and Urquiola 2008, Jackson 2009) and broader outcomes such as course taking, secondary-school graduation, disciplinary incidents, and arrest rates (Culllen, Jacob and Levitt 2006, Clark 2007).¹In most nations, high-achieving schools tend to have student bodies with better incoming achievement than the average school. 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. Since students may benefit directly from exposure to 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.² 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) — suggesting that an important part of the benefits to attending a school with higher-achieving peers *has to do with the peers themselves*.

From a policy perspective understanding the relative contribution of direct peer effects is of great importance, as it speaks to the ability to replicate success across schools. For example, if

¹ This is closely 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).

² 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, while students may benefit from attending schools with higher achieving peers, it is unclear how much of the benefits to attending a better school are through the direct effect of being exposed to higher achieving peers.

having high concentrations of Catholic students (whose parents place a higher value than average on education) engenders an environment particularly conducive to learning, it would imply that adopting practices used in Catholic schools (e.g. stricter discipline and mandatory uniforms) in traditional public schools may not yield improved results as much of the "Catholic school effect" *could* be a "Catholic peers effect".³ Since it is natural to look at the practices of successful schools for guidance on how to improve outcomes at other schools, it is imperative to understand how much of the success of these school are merely an artifact of the student body, and how much could potentially be replicated in other schooling environments. However, there is no empirical evidence on the extent to which peer effects can account for school value-added.

I aim to some light on the relationship between school value-added and peer quality using data from Trinidad and Tobago. Specifically, I aim to (1) estimate the effect of attending a secondary school with higher-achieving peers, (2) identify the effect of marginal 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 affect of having higher-achieving peers, and how much could potentially be explained by other factors that can be manipulated by policy-makers. The Trinidad and Tobago context is particularly well suited to addressing this question for a few important reasons: (1) Trinidad and Tobago has an ability-grouping system such that 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 in Trinidad and Tobago have long-standing reputations such that those schools that admit high achieving students have done so for several years so that large differences in peer achievement across schools led to meaningful differences in inputs across schools that would have developed over time⁴ — something that cannot be created in a randomized control trial; (3) Students are assigned to secondary schools by the Ministry of education so that one can remove much self-selection bias and obtain credible estimates of the causal effect of schools and the effect of peers.

³ 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 conversions charter schools. 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).

⁴ For example, even though teacher salaries are the same at all public school, since teachers prefer to teach at the more elite high achieving secondary schools, there are large differences in the observable characteristics of teacher across schools.

To decompose a school effect into a peer effect and an input effect, one needs clean estimates of *both* peer quality and school quality *on the same outcomes for the same population of students*. To address the concern that peer quality and input quality are highly collinear, I identify the effect of attending a school with higher-achieving peers in the cross-section and estimate the direct effect of peers based on within-school variation in peer achievement across cohorts — effectively holding input quality constant. This approach to identifying peer effects has been used by (Ammermueller and Pischke 2006) (Lavy and Schlosser 2009) (C. Hoxby 2000) (E. Hanushek, et al. 2003). To address concerns that students may self-select to schools and peers, I restrict my analysis to a sample of students and schools 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 school assignment. I use the exogenous (but not random) assignments to construct instruments (the schools students would attend if there were no self-selection, and the peers students would be exposed to if there were no student self selection) for the actual schools attended and the actual peers students are exposed to.⁵

In Trinidad and Tobago, students take the Secondary Entrance Examination (SEA) at the end of 5th grade and they 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 show that the nature of the exogenous variation due to the Ministry's assignments, and I present empirical tests supporting the assertion that the school assignments are conditionally exogenous. It is important to point out that in Trinidad and Tobago, *the majority* of the variation in secondary school attendance is driven by student ability and student preferences for schools (unlike other national contexts where most variation comes from differences in neighborhoods or other unobserved factors). As such, even without an instrumental variables strategy, 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

⁵ Part of the benefits to attending a school with higher achieving peers has to do with the fact that school that always have high achieving students can attract better teachers, attract wealthier student bodies, 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 question than a randomized control trial. While randomized control trials are a powerful tool, such a research design would be inappropriate for the particular question at hand.

time that high-achieving students no longer apply to that school. Fortunately, given that I can observe students school choices, how desirable a school is deemed to be 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 and preferences, and I present several 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.

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 my main outcome. This variable is a nice summary statistic for overall educational output because it is sensitive to numerous margins such as dropping out of school, the number of exams attempted, and performance conditional on taking a given exam. Because school prestige and school achievement are highly correlated with incoming peer achievement, I classify schools based on the achievement of the peers *before entering secondary school* (thus avoiding the reflection problem (Manski 1993)). Classifying schools and peers in the same manner allows for a useful decomposition of a school effect into a peer effect and an input effect. Across all specifications, observationally similar students who are assigned to, and attend, schools with higher incoming peer achievement pass more exams. While OLS results show that increases in mean peer achievement *within a school over time* are associated with improvements in student outcomes, instrumental variables (IV) results are about half as large and are imprecisely estimated. However, both the OLS and the IV results indicate that, on average, only about 10 percent of school value-added can be attributed to peer quality.

An analysis by gender provides further evidence that part of the school effect is a peer effect. Specifically, females benefit about 30 percent more than males from attending schools with higher achieving peers, and gender differences in response to peers can completely explain gender differences in response to schools. Models that allow for non-linearity show that the benefits to attending a school with marginally higher-achieving peers are larger at high peer achievement levels, and among high achievement schools increases in peer quality have large

positive statistically significant effects — suggesting that much of the value-added associated with attending the highest achievement schools can be attributed directly to peers.

Understanding whether peer quality can account for 2 percent of school value added as opposed to 92 percent of school value added is tremendously important for understating how to improve ailing schools and what the optimal allocation of students across schools should be. However, there is no empirical evidence to guide us. The institutional setup in Trinidad and Tobago, the availability of data on student choices, and credible sources of exogenous variation in *both* schools attended *and* exposure to peers *for the same population of students* provides a unique opportunity to analyze the relationship between school value-added and peer quality. This is the first study to decompose the school effect into a direct peer effect and an input effect *based on exogenous variation*, and the first study to show that, *on average*, only a modest proportion of the benefits to attending a higher-achievement school is due directly to the fact that such schools have higher-achieving peers. However, the results suggest that much value-added at the highest-achieving schools can be attributed to having high-achieving peers — so that adopting the practices of the highest achieving schools may not yield the same success in the average school.

Section II lays out the econometric framework and the assumptions necessary to decompose the school effect into a direct peer effect and an other inputs effect. Section III describes the education system in Trinidad and Tobago and describes the data. Section IV lays out the identification strategy and discusses the exogenous variation. Section V presents specification tests, the results, and robustness checks. Section VI concludes.

II Econometric Framework

In this section I present a model showing that, under reasonable assumptions, the ratio of the coefficient on peer quality obtained in the cross-section and the coefficient on peer quality obtained in a within-school regression will yield the proportion of the effect of attending a school with higher achieving peers that can be attributed to direct peer effects. Input quality at school j , denoted I_{jt} , is a function of the long-run peer quality \bar{P}_j and idiosyncratic determinants u_{jt} . This is written as [1] below.

$$[1] \quad I_{jt} = f(\bar{P}_j) + u_{jt}.$$

This captures the fact that school inputs may be endogenous to the characteristics of the student body. For example, teacher quality may depend on the characteristics of students, the amount of

funding a school gets may be a function of alumni donations and political suasion of alumni, or the donation and contribution of students parents. I assume that input quality is not a function of contemporaneous peer quality since (a) input quality changes are likely not sensitive to transitory shocks to peer quality and (b) changes in input quality in response to changes in peer quality will exhibit a considerable lag. Peer quality at school j at time t , $P_{jt} = \bar{P}_j + \mu_{jt}$, is comprised of an idiosyncratic component μ_{jt} and a long run component \bar{P}_j that captures the fact that the schools that attract the highest/lowest achieving students have typically done so for several years. This modeling assumption would seem very natural for those familiar with secondary schools in Trinidad and Tobago as it would be akin to saying that Harvard and Yale (or Oxford and Cambridge) have always attracted 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.

The structural statistical model of student achievement as a function of input quality and peer quality is given by [2] 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.

$$[2] \quad Y_{ijt} = \alpha + \beta P_{jt} + \gamma I_{jt} + \varepsilon_{ijt}.$$

The conditional expectation of [2] with respect to peer quality is given by [3] below when $f(X) = \pi X$, and the conditional expectations of both ε_{ijt} and μ_{jt} are zero.

$$[3] \quad E[Y_{ijt} | P_{jt}] = \alpha + \beta P_{jt} + \gamma f(E[\bar{P}_j + \mu_{jt} | P_{jt}]) + E(\varepsilon_{ijt} | P_{jt}) \equiv \alpha + P_{jt}(\beta + \gamma\pi).$$

Equation [4] illustrates that if the school attended is exogenous to unobserved student characteristics, then an OLS regression of student achievement on peer achievement will reflect both the direct effect of peers on student achievement $\beta \cdot P_{jt}$ and the indirect effect of peers through peers affecting the quality of school inputs $\gamma\pi \cdot P_{jt}$.

Consider now the expected change in student achievement over time within the same school conditional on changes in peer achievement within the school.

$$[4] \quad E[Y_{ijt} - Y_{ijt-1} | P_{jt} - P_{jt-1}, J = j] = \beta(P_{jt} - P_{jt-1}) + E(\mu_{ijt} - \mu_{ijt-1} + \varepsilon_{ijt} - \varepsilon_{ijt-1} | P_{jt} - P_{jt-1}) \equiv \beta(P_{jt} - P_{jt-1}).$$

Equation 4 illustrates that if changes in peer quality within a school over time are uncorrelated with changes in unobserved determinants of student achievement and changes in inputs over

time, one can identify the direct contribution of peers using within-school variation in peer quality — comparing the outcomes of observationally similar students who attend the same school but are exposed to different peers because they attend at different times.

The difference between the coefficient on peer quality obtained in the cross section and that obtained using within-school variation will be $\gamma\pi$, the effect of a marginal increase in the long-run peer quality through its effect on the level of inputs. As such, $\beta / (\beta + \gamma\pi)$, the coefficient on within-school changes in peer quality divided by the coefficient on peer quality in the cross section, is a measure of the fraction of the benefits to attending a school with higher achieving peers that can be attributed to direct peer effects. 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 additive separability of inputs and peers. While these assumptions are somewhat restrictive, they allow one to say something meaningful about the relationship between peer effects and school effects. While these assumptions may be accurate for small changes in peer achievement, it may not hold for the entire universe of schools. As such, I also present results for sub-samples of schools with similar levels of achievement. In Section V I also present evidence consistent with additive separability of inputs and peers. I believe that while the modeling assumptions are not perfect, the decomposition that falls out of the model is nonetheless useful.

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 grade 6, hereinafter referred to as 6th grade) and ends at fifth form (the equivalent of grade 10, hereinafter referred to as 10th grade) when students take the Caribbean Secondary Education Certification (CSEC) examinations. These are the Caribbean equivalent of the British Ordinary levels (O-levels) examinations.⁶ The CSEC exams are externally graded by examiners appointed by the Caribbean Examinations Council. Students seeking to continue their education typically take five or more subjects, and

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

the vast majority of testers take the English language and mathematics exams.⁷

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.⁸ 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 6th through 10th grade and often continue to 12th grade (called upper-sixth form). These schools teach the national curriculum and are fully funded and operated by the Government. 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 5th grade were assigned to such schools and after 3 years took an exam to gain admission to a senior secondary school (or possibly a regular Government school) which would prepare them for the CSEC examinations. Senior secondary schools have been phased out, so that by 1995, the relevant sample period, all schools taught the same academic curriculum and only a handful of Comprehensive schools did not provide instruction through to the CSEC exams.⁹ 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 will be key

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

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

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

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 and so forth 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.

This process is used to assign over 95 percent of all students. However, there is a group of students for whom this mechanism may not be used. Government Assisted schools (which account for about 16 percent of school slots) are allowed to admit 20 percent of their incoming class at the principal’s discretion. As such, the rule is used to assign 80 percent of the students at these schools, while the remaining 20 percent are handpicked by the school principal before the next highest ranked school fills any of its slots. For example, suppose the highest ranked school has 100 slots and is a Government Assisted school. The top 80 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). 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.¹⁰ 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. As one can see, there is a clear rapid increase on the likelihood of being assigned to one's top choice school as one 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.¹¹

Students' school choices are based largely on their own perceived ability, preferences for schools, geography, and religion. Specifically, 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 very 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 within groups of students who made the same school choices, one is able to control for sources of bias not possible in many datasets. The fact that differences in school assignments among student 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*. 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

¹⁰ I detail how this is constructed in Appendix Note 1.

¹¹ This discontinuous 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 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.

choice schools is to the right of the second choice schools which is to the right of the third choice schools which, in turn, is to the right of the fourth choice schools. In other words, students tend to put schools with higher-achieving peers higher up on their preference ranking. In fact, on average the difference between the mean incoming SEA scores at a student's top choice school and second choice school is 0.277 standard deviations, between the top choice school and the third choice school is 0.531 standard deviations, and between the top choice school and the fourth choice school is 0.82 standard deviations.

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,¹² primary school district, and the secondary school to which they were assigned by the Ministry of Education. The SEA exam is comprised of five subjects that all students take: math, English, science, social studies, and an essay. To track these 5th grade students through to secondary school in 10th grade, I link the SEA data with the CSEC examination data both four and five years later. Roughly 72 percent of SEA test takers were linked to CSEC exam data.¹³ 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 and several 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. I determine whether a student took the CSEC exams, compute the number of examinations taken and passed, and determine if they obtained the pre-requirements for tertiary education (passing five CSEC exams including English and mathematics).

Because students who are assigned to Assisted schools could have been hand-picked by the school's principal, I drop all students assigned to Assisted schools (I do not drop students

¹² To preserve confidentiality I was not given access to the actual religion, but a code that identified students' religions.

¹³ 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 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).

who *attend* Assisted schools unless they were *assigned* to Assisted schools). Dropping students who actually attend Assisted school would generate a sample selection bias so I only drop those who are assigned to Assisted schools and present clean instrumental variables estimates based on the exogenous assignments. All students who scored too low on the SEA exams to be assigned to schools are dropped from the sample. Students who were assigned to temporary schools or private schools (at times the government will purchase private school slots) are also dropped from the sample. This accounts for about 2 percent of the sample and has virtually no impact on the estimates. The resulting analytical dataset contains 150,701 students across seven cohorts and 158 school assignments.

There are two quirks in the data that must be addressed. First, there are more assignments than high schools because (a) some Comprehensive schools feed into the same Government secondary school and (b) when there are more students than available school spots the government will assign those students with the lowest SEA scores to small "temporary" schools or purchase seats in private schools. Omitting students assigned to such schools does not change the results in any meaningful way. The second quirk is that only for the year 2000 the government decided to assign all students 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 is not driving any of the findings, I verify that all results are robust to excluding the year 2000 cohort and also excluding all years after 1999.

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. As one can see in Table 1, there is substantial variation in school and peer quality in Trinidad and Tobago. The average total SEA scores are a full 1.66 standard deviations higher at the top 30 schools than the bottom 67 schools. The difference between the top and bottom ranked schools is a full 3.04 standard deviations. 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. To provide a deeper sense of the variation in peer quality across

schools in Trinidad and Tobago, Appendix Figure A1 shows the distribution of total SEA scores for schools with different ranks in mean peer quality.

In this sample, females make up about half of students in each school group. 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. Students in the top 30 schools pass several more exams than students at lower ranked schools, such that 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 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. This is a common prerequisite for continuing education. Even though I do not analyze this outcome, the large differences in this important outcome across schools is 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 student at the top 30 schools and about 440 in both other groups of schools. The one input for which there is aggregate data across broad school types 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 and only 64 percent for Comprehensive schools (where mean total SEA scores were -0.34). Similarly, 28 percent of teacher at for 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 did not self-select into the assignment, conditional on the variables used to create the school assignment (incoming test

scores and student preferences) the actual school assignment within the group of non-Assisted schools is exogenous. Within the sub-sample of students assigned to non-Assisted schools, the student assignment algorithm precludes student self-selection to the assigned school. As such, even though students may transfer out of their initial school assignment or parents may subsequently use their political and economic influence to influence 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 section IV.1 and IV.2 below, I detail how I use the mean peer quality that would prevail if all students complied with their initial school assignment identification strategies to identify (1) the effect of attending a secondary school with higher-achieving peers, and (2) the effect of marginal increases in peer achievement within a school.

IV .1 Across-school model: In the cross section, exogenous variation in school assignments comes from the fact that conditional on incoming test score and secondary school choices, student have no control over their assignments, so that any variation in school assignments conditional on incoming test score and secondary school choices must be exogenous. While this is true, it is important to illustrate where this exogenous variation comes from. As detailed in section III.1, the assignment rules generate cut-offs such that those who are in the applicant pool for that school who score above the cut-off are assigned and those who are in the applicant pool for that school who score below the cut-off are not assigned. These cut-offs lead to exogenous variation because individuals with the same school preferences may be assigned to different schools because one scored above a school threshold while the other did not. While this sounds like a situation amenable to a regression discontinuity type analysis, because I do not know where the actual cutoffs are and there are other sources of exogenous variation, I do not employ such an identification strategy. The fact that I am able to observe student preferences and control for them directly (in addition to test scores), a unique feature of the Trinidad and Tobago data, allows me to use a Difference in Difference (DID) identification strategy to isolate the causal effect of schools.¹⁴ I explain this exogenous DID variation below.

¹⁴ 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

Consider two students (A and B) with the same test score X . Suppose both A and B list the same first choice school, but list different second choice schools. Assume all students attend their assigned school. If they both just missed the cut-off for their top choice school, then they will both end up attending their second choice schools. A comparison of the outcomes of A and B across their different schools will reflect both differences in preferences and differences in schools. Consider now, two other students (A' and B') such that A' has the same preferences as A, and B' has the same preferences as B, but A' and B' have the same score X' that is higher than X . If X' is above the cut-off for the top choice school, then A' and B' will both attend the same top choice school even though they listed different second choice schools. Any difference in outcomes between A' and B' must reflect only their preferences, since they have the same test scores and attend the same school. Under the assumption that differences in outcomes due to preferences are the same across all levels of achievement, one can subtract the difference between A' and B' from the difference between A and B to isolate the differences in outcomes associated with different schools. In section V.1, I present evidence that identification based on this variation is likely valid. To further show that this difference in difference type strategy is valid, as a robustness check in section V.3 I show that results from a discontinuity based strategy that simulates where the cut-offs would be if there were no hand picking of students 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 models based on the Difference in Difference variation described above. The basic empirical strategy is to compare the outcomes of students with similar incoming test scores and school preferences who attend different schools using cross-sectional variation. Since the actual school attended, and therefore the average peer quality at the school attended, may be subject to some self-selection bias, I use the assigned peer quality (that is, the average incoming test scores of all other students assigned to the same assigned school j^* as student i in cohort c), $\overline{SEA_{j^*c}}$, as an instrument for actual peer quality. Specifically, I estimate the outcome of student i from cohort c , at school j with the following system of equations by two-stage-least-squares.

efficient DID type variation. It is important to note that any discontinuity based strategy used where the cut-offs are not known is not a true regression discontinuity model, and is therefore just another instrumental variables procedure.

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

In [5], \overline{SEA}_{ijc} is the mean total SEA scores for students attending the same school j as student i in cohort c , SEA_i is a matrix of incoming test scores (the student's total SEA score, total SEA score squared, total SEA score cubed), $I_{SEA_i=k}$ 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 set of school preferences, and $\theta_{p,2}$ is a preferences 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 estimates 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 preferences, (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 by construction, 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. In this model inference is based on comparisons within groups of students who are similar in very important ways (i.e. there are powerful controls for most sources of selection bias) but who were assigned to different schools for reasons entirely beyond their control (i.e. they were on either side of a school's cut-off).

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 section equation [5] to include an assigned school fixed effect θ_{j*2} where all other variables are defined as before.

$$\begin{aligned}
 \overline{SEA}_{ijc} &= \sum_{k=1}^{50} I_{SEA_i=k} \cdot \theta_{k1} + \overline{\overline{SEA}_{ij*c}} \cdot \delta_1 + \sum I_{P_i=p} \cdot \theta_{p1} + X_i \rho_1 + \theta_{c1} + \theta_{j*1} + \varepsilon_{ijc1} \\
 Y_{ijc} &= \sum_{k=1}^{50} I_{SEA_i=k} \cdot \theta_{k2} + \overline{SEA}_{jc} \cdot (\beta + \gamma\pi) + \sum I_{P_i=p} \cdot \theta_{p2} + X_i \rho_2 + \theta_{c2} + \theta_{j*2} + \varepsilon_{ijc2}
 \end{aligned}
 \tag{6}$$

In equation [6], I instrument for changes in actual peer quality within assigned schools over time the with changes in peer quality that would prevail if all students complied with their school assignments. Because there is no opportunity for students to self-select into school assignments or to assigned peers, equation [6] 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 [6] 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.

Variation in assigned peer achievement within assigned schools over time comes from two sources: (1) variation in aggregate cohort sizes 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, variation in cohort sizes affect peer quality within a school because the number of slots in each school is fixed. For example, if a cohort were to double in size, then twice as many students will apply to the top school. Since the cut-off score for each school is the score of the last person admitted, the cut-off score would be higher when there is a larger test taking cohort and more applicants. 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. However, I test this possibility 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 other 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. This 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 all 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 rather 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, in section V.1, I present empirical tests suggesting that changes in assigned peer quality within a school over time are not correlated with within school changes in input quality. Third, because school choices are observed in the data, this scenario is testable. In section V.1 I show that students rankings of school over time are very stable over time 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: While I have described the sources of variation in assigned peer achievement both in the cross section and within assigned schools over time, and I have asserted that such variation should yield consistent estimates, it is important to present evidence that this may be the case. In this section I address the three 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 preferences, on the sub-sample of government schools and comprehensive schools, the school assignments, assigned peer quality, and changes in assigned peer quality within a 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.

In addition to testing for correlations between assigned peer achievement and pre-treatment characteristics, I also estimate the magnitude of any possible bias.¹⁵ Specifically, I run a separate 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 correlated religion, the number of SEA attempts, or the primary school district the student is from at the five percent level. There is, however, a marginally statistically significant relationship with the number of SEA attempts. 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, but is correlated with changes in the likelihood of being from district 4 at the 10 percent level. In other words, 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 are valid. In addition to the fact that peer achievement and within-school changes in peer achievement, are not correlated with any pre-treatment covariates or within-school changes in pre-treatment covariates at the five percent level, the correlations are very small in

¹⁵ This exercise is similar in spirit to (Altonji, Elder and Taber 2005).

magnitude.¹⁶ Unlike scenarios where the implied bias may be similar to the estimated coefficient of interest, the most pessimistic estimates of the bias due to unobserved covariates are so insignificant relative to estimated effects and the natural variability of the estimates that it could not possibly affect the results in any meaningful way. As such, the specifications outlined in [5] and [6] are likely to 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, readers 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 within a school over time 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 school's ranking in the assignment mechanism, I can test for this possibility directly. To show that this is not a source of bias in these data, 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 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

¹⁶ 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 eighth of the size of the *standard error* for the estimated cross sectional effects and less than one tenth of the size of the *standard error* for the estimated within school effects.

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 and preferences.¹⁷

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. I present evidence that there is sufficient within-school variation to identify meaningful peer effects. 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 school, there still remains substantial variation in peer achievement within schools. As such, I am reasonably confident that the within-school model can be identified.

V.2 Main Results

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 the naive OLS results based on students actual schools attended, the second panel presents the 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 preferences (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 are 0.5 standard deviations higher (roughly the variance in peer quality in the cross section and the mean difference in peer achievement between a

¹⁷ One may wonder if there is sufficient variation in peer achievement within schools over time. A simple variance decomposition indicates that about one quarter of the overall variation in peer achievement in the data are from within school variation.

student's top choice and third choice schools) would pass about 0.61 more exams.

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 RF 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 IV results in the third panel (row) are slightly smaller than the OLS results yielding statistically significant coefficient estimates between 0.85 and 1.177 — indicating that a student who is assigned to a school where peer test scores are 0.5 standard deviations higher would pass between about 0.425 and 0.585 more exams.

Columns 4 through 7 present the within-school results. 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 preferences (columns 6) and both score group dummies and preferences (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 0.07 more exams than an observationally similar student who attends the same school when peer achievement was 0.5 standard deviations lower. If one were to divide the preferred across school OLS coefficient by the preferred within-school coefficient, one gets a ratio of 0.114 — suggesting that roughly 11 percent of school value-added can be attributed directly to peer quality differences across schools.

Unlike the across school results, the reduced form within-school estimates (second row, in columns 4 through 7) are all small and statistically insignificant. The RF estimates all yield statistically insignificant coefficients between 0.039 and 0.047. 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 0.035 and 0.042 more exams than an observationally similar student who was assigned to the same school when peer achievement was 0.5 standard deviations lower. The instrumental variables estimates are about half the size of the OLS estimates, suggesting that there is self-selection bias in the OLS within-school estimates. It is worth noting that the first

stage F-statistic on assigned peer quality (conditional on assigned school fixed effects) range between 400 and 666, so that the results do not suffer from a weak instruments problem. If one uses the instrumental variables estimates to calculate the ratio of the cross school effect that can be attributed directly to peer quality, one obtains a ratio of 0.072 — suggesting that on average roughly 7.2 percent of school value-added can be attributed directly to peer quality differences across schools.

Effects by gender: In light of recent findings in the economics literature of gender differences in response to interventions (Kling, Liebman and Katz 2007, Angrist, Lang and Oreopoulos 2009, Angrist and Lavy forthcoming, Han and Li 2009, Jackson 2009), I present the main results for males and females separately. In Table 5 I present the reduced form results (the top panel) and the instrumental variables results (the bottom panel) for same models as Table 4. The top and second row of each panel present the results for males and females, respectively.

For both the RF and IV results, the estimated coefficients in the cross-section (columns 1 through 3) are larger for females than for males. The IV estimates for males are all statistically significant at the one-percent level and range between 0.709 and 1.005, while those for females range between 1.047 and 1.371. These estimates suggests that a male who attends a school where peer test scores are 0.5 standard deviations higher would pass between 0.35 and 0.5 more exams while a female who attends a school where peer test scores are 0.5 standard deviations higher would pass between 0.5 and 0.69 more exams. The difference in the marginal effect between males and females range between 0.338 and 0.366 (these cross sectional gender differences are statistically significant at the one percent level)¹⁸, indicating that the marginal effects are about 50 percent larger for females than that for males.

The within-school RF and IV results also show differences by gender. While none of the point estimates for males are statistically significant at the 5 percent level, they are all negative and one is significant at the 10 percent level. The within school 2SLS coefficients for males range between -0.069 and -0.168 suggesting that a male student would pass between 0.033 and 0.084 *fewer* exams that an observationally similar male who attends the same school when peer achievement was 0.5 standard deviations lower. Unlike the negative estimates for males, all the

¹⁸ To test the statistical significance between the marginal effect for males and females, I estimate a model that interacts all covariates with a female indicator variable. I then test the statistical significance of the interactions between peer achievement and the female indicator variable.

point estimates for females are positive, albeit not statistically significant. The within school coefficients for females range between 0.013 and 0.119, suggesting that a female student would pass between 0.0065 and 0.06 *more* exams than an observationally similar female who attends the same school when peer achievement was 0.5 standard deviations lower. The difference in the marginal effect between males and females range between 0.09 and 0.276 (these within-school gender differences are statistically significant at the one percent level).

The similarity between the gender differences across the cross-section and the within-school models is notable. In the preferred models (columns 3 and 7) the gender difference in the school effect is 0.366 while that for the direct effect of peers is 0.262 — suggesting that about 72 percent of the gender difference in response to schools can be explained by gender differences in response to peers. While the point estimates indicate as much as 72 percent of the gender gap in school effects can be explained by peers, statistically, one cannot reject the hypothesis that peer effects account for all of the gender gap.¹⁹ 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. Results in which researchers have found gender differences where one would expect there to be a social interaction effect, and a psychology literature suggesting that females may be more responsive to peers than males (Cross and Madson 1997, Maccoby and Jacklin 1974, Eagly 1978) lend credibility to this interpretation.

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 8.5 percent for females and 16.8 (negative) percent for males. In other words, the point estimates suggest that the direct contribution of peers to achievement can explain between 8 and 16 percent of the effect of attending a school with higher-achieving peers.²⁰ The differential effects by gender corroborate this calculation. 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 most (if not all) the differential response to schools — compelling evidence that between 8 and 8+16=24 of the school effect can be directly attributable to peers. Also, the similarity in the

¹⁹ The difference between the cross-school difference and the within-school gender difference is 0.104. The standard errors on the across school and within school gender differences are 0.12 and 0.07, respectively.

²⁰ Readers should note that omitting the 2000 through 2002 SEA cohorts does not qualitatively change the results.

gender differences in response to schools $\beta + \gamma\pi$, and the gender differences in response to peers β (particularly in the reduced form results), suggest that the assumption of additive separability of inputs and peer quality may be a good first approximation.

Non-linear effects: Given recent research documenting that peer effects may be non-linear and heterogeneous across different populations and students of different ability levels, it is worthwhile to move away from the restrictive specification that assumes that the effect is the same across all students and all types of schools to a more flexible model. 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. This mitigates error due to linear extrapolation across schools with very different levels of peer 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. Similarities in the non-linearity of the effect between the across-school models and the within-school models may provide further evidence of the importance of direct peer effects in explaining school effects. Since the full cross sectional effect (under additive separability) is $\beta + \gamma\pi$, then one might expect that any non-linearity in β will also be present in $\beta + \gamma\pi$. I present evidence of this 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 2.614 for the top 30 schools, 0.853 for schools ranked 31 through 90, and 0.662 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 achievement levels.

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 sizable 1.416 for the top 30 schools (p -value= 0.012), a statistically insignificant -0.091 for schools ranked 31 through 90, and a statistically insignificant 0.005 for the bottom 68 schools (ranked below 90).²¹ The instrumental variables within school coefficients on mean peer achievement is a sizable 1.902 for the top 30 schools (p -value= 0.03), a statistically insignificant -0.032 for schools ranked 31 through 90, and a statistically insignificant 0.088 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.²²

The fact that the non-linearity in the school effects track very closely the non-linearity in the direct peer effect strongly 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 $\beta / (\beta + \gamma\pi)$, the fraction of the across school effect explained by direct peer influences is between 0.73 (based on the IV results) and 0.57 (based on the reduced form results) in the top 30 schools, and essentially 0 at other schools (since the within-school point estimates for schools ranked 31 through 158 are not statistically significant).

These non-linearities across different schools could be driven by (a) non-linearity in the effect of peers or (b) 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 the second 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

²¹ If one looks at males and female separately one sees the same pattern. However, the direct positive effect of peers for females at top 30 schools is statistically significant at the 5 percent level, while that for males is not significant at the 10 percent level.

²² 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.

columns 1 through 3 — suggesting that response heterogeneity 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 3 shows the local polynomial fit of the number of exams passed (after taking out the effects of own incoming test scores, preferences and gender) on the mean assigned peer level of the assigned school (this is semi-parametric representation of the reduced form). It would appear that 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 3, I show the relationship between the within-school effect of increases in assigned peer achievement and the mean peer achievement of the school over time. 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 clearly 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 — compelling evidence that direct peer effects are responsible for much of the very high value-added among high-achieving schools. As with the evidence by gender, the non-linear analysis provides empirical support for the assumption of additive separability of inputs and peers.

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 half a standard deviation higher achievement would increase the likelihood of taking the CSEC exams by as much as 8 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 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 preferences. To do this, I simulate where the cut offs for each school would be if no students could be handpicked by school principals.²³ One can see from Figure 4 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

²³ See appendix note 1 for details.

fact that the cut-offs are simulated and there is not always full compliance). However, econometric models fail to reject the null hypothesis of a discontinuous increase in peer quality through the simulated cut-offs at less than the one percent level. To present a formal test of 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 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) — very 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 instrumental variables 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).²⁴ Jackson (2009) 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 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 preferences to remove selection is 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

²⁴ 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.

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 effect 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 Table 8.

For all models, one rejects the null hypothesis that the following peers and the previous peers are jointly statistically significant at the 20 percent level. However, similar to Tables 5 and 6, the negative effect of peers for males and the positive effect of peers for the top 30 schools are statistically significant at the 1 and 10 percent level, respectively. Even though the coefficients on peer quality in the previous cohort yield p -values of 0.48 for both these models, the estimated coefficients are not zero and are the same sign as the actual estimated peer effect. If we take the conservative view that the effect on the lag reflects some underlying spurious association, then we could subtract that from the estimated peer effect to obtain a conservative causal estimate. Doing this for males results in a conservative reduced form current peer effect estimate of -0.161. This is actually larger than the reduced form estimate obtained in the preferred model in Table 5 — suggesting that there is a true negative effect on average for males. 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. Even this conservative estimate implies that 37 percent (as opposed to 52 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 concerns that inference based on estimated effects could be biased by underlying serial correlation in the data. To assess 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 outcomes estimated. In both cases, none of the 100 replications yielded parameter estimates larger than the actual estimated coefficients, suggesting that the estimated 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 are 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 ensure 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. To obtain peer effect estimates that are not confounded with school input quality, 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 level of the assigned peers change. In both these 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.

The effects by gender reveal some interesting patterns. Similar to results found in (Hastings, Kane and Staiger 2006) the marginal effects of being assigned to and attending a school with higher achieving peers are about 50 percent more for females than for males. The differences are statistically significant. 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. The differences in response to peers are also statistically significant. Positive effect for females and negative effect for males have also been found in experimental studies on neighborhood effects (Kling, Liebman and Katz 2007). The gender differences in response to peers can account for most of the gender differences in response to school achievement — suggesting that some of the school effect can be explained by the direct contribution of peers.

I also find that 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), I find that 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 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* school inputs (in this case 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 students' 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.

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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 [0.082]**	1.229 [0.073]**	1.235 [0.073]**	0.14 [0.067]*	0.144 [0.067]*	0.135 [0.072]*	0.141 [0.074]*	0.114
Assigned (RF)	0.437 [0.062]**	0.57 [0.057]**	0.574 [0.057]**	0.039 [0.058]	0.043 [0.059]	0.038 [0.079]	0.047 [0.079]	0.082
Actual (2SLS)	0.851 [0.124]**	1.171 [0.094]**	1.177 [0.092]**	0.07 [0.105]	0.076 [0.105]	0.069 [0.144]	0.085 [0.144]	0.072
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.

Table 5

Reduced Form and 2SLS Results on the Number of Exams Passed: By Gender							
	1	2	3	4	5	6	7
	Cross sectional results			Within School results			
	Reduced Form OLS Regressions						
Mean Total (assigned)	0.38	0.515	0.512	-0.048	-0.04	-0.097	-0.083
Male	[0.054]**	[0.067]**	[0.065]**	[0.060]	[0.057]	[0.059]+	[0.079]
Mean Total (assigned)	0.519	0.638	0.646	0.007	0.012	0.056	0.062
Female	[0.081]**	[0.068]**	[0.069]**	[0.064]	[0.065]	[0.100]	[0.100]
Female-Male	0.139	0.123	0.134	0.055	0.052	0.153	0.145
P-value Male=Female	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01
	2SLS Regressions						
Mean Total (actual)	0.709	1.013	1.005	-0.084	-0.069	-0.168	-0.143
Male	[0.103]**	[0.105]**	[0.101]**	[0.103]	[0.097]	[0.102]+	[0.102]
Mean Total (actual)	1.047	1.353	1.371	0.013	0.021	0.108	0.119
Female	[0.169]**	[0.120]**	[0.120]**	[0.119]	[0.119]	[0.129]	[0.129]
Female-Male	0.338	0.34	0.366	0.097	0.09	0.276	0.262
P-value Male=Female	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01
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

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. The regressions on the sample of females have 75337 observations and that for males have 75361 observations.

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 [0.254]**	0.453 [0.041]**	0.313 [0.056]**	1.308 [0.084]**	0.466 [0.050]**	0.228 [0.056]**	0.028 [0.032]
Mean peer scores (2SLS)	2.614 [0.635]**	0.853 [0.113]**	0.662 [0.170]**	2.438 [0.195]**	1.052 [0.128]**	0.497 [0.129]**	0.057 [0.038]
Within School Variation							
Reduced form	1.416 [0.581]*	-0.0913 [0.099]	0.005 [0.122]	-0.03 [0.056]	-0.157 [0.118]	-0.084 [0.165]	-0.06 [0.243]
Mean peer scores (2SLS)	1.902 [0.912]*	-0.032 [0.192]	0.088 [0.220]	<0.001 [0.480]	-0.166 [0.294]	-0.330 [0.246]	-0.075 [0.082]
Observations	17811	84740	48144	26454	50348	42249	27521
Ratio (2SLS)	0.73	-0.04	0.13	0.00	-0.16	-0.66	-1.32

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 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

Depended Variable	1	2	3	4 ^a	5 ^b
	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	0.052	-	-
	[0.022]**	[0.015]**	[0.036]	-	-
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

Sample:				Schools rank	Schools rank	Schools rank
	Overall	Males	Females	1-30	31-90	rank 91+
Peers _{c-1}	-0.019	-0.058	-0.007	0.735	-0.060	-0.231
	[0.075]	[0.083]	[0.112]	[1.043]	[0.102]	[0.224]
	(0.8)	(0.48)	(0.95)	(0.48)	(0.55)	(0.3)
Peers	-0.064	-0.220	-0.034	1.733	-0.170	-0.223
	[0.075]	[0.081]	[0.096]	[0.991]	[0.112]	[0.142]
	(0.39)	<0.01	(0.72)	(0.08)	(0.13)	(0.14)
Peers _{c+1}	0.026	0.088	0.001	0.109	-0.027	0.319
	[0.058]	[0.058]	[0.114]	[0.713]	[0.092]	[0.200]
	(0.44)	(0.13)	(0.99)	(0.88)	(0.77)	(0.12)
Current minus lag	-0.046	-0.161	-0.027	0.997	-0.109	0.008
Current minus lead	-0.090	-0.308	-0.035	1.623	-0.143	-0.542
P-value on lag and lead	0.73	0.29	0.99	0.70	0.75	0.23
P-value on current	0.39	<0.01	0.72	0.08	0.13	0.14

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 fixed effects, and control for the total SEA score, its quadratic and its cubic, and gender.

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.

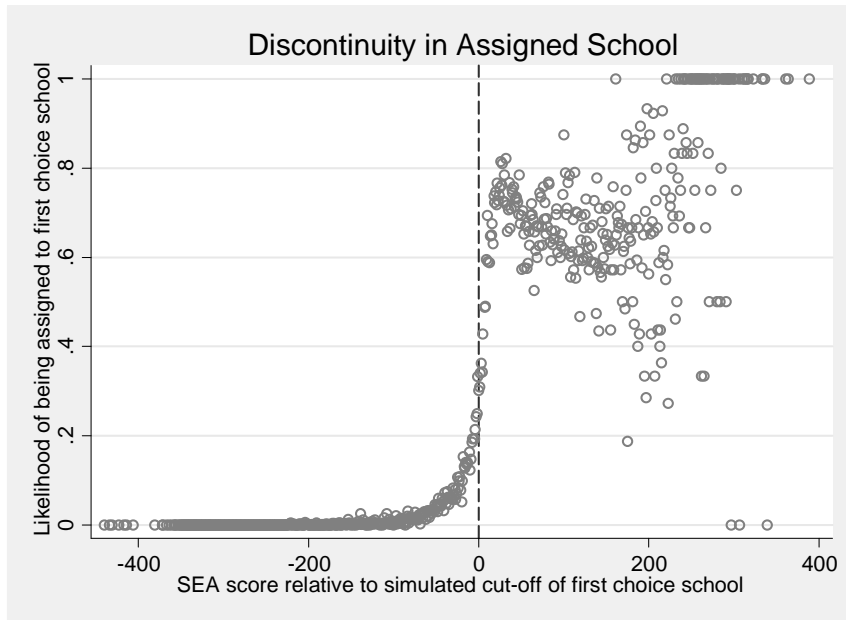


Figure 1: Likelihood of being Assigned to One's Top Choice School

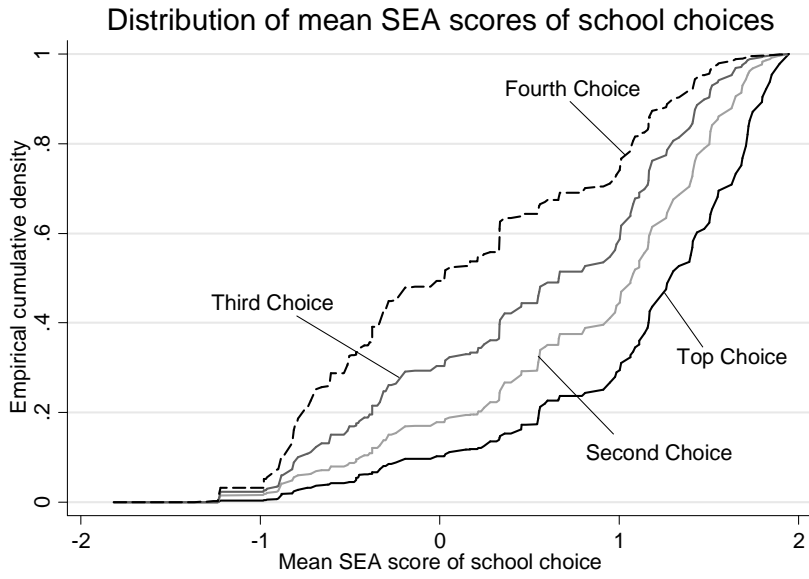


Figure 2: *Distribution of Peer Quality by School Choice Rank*

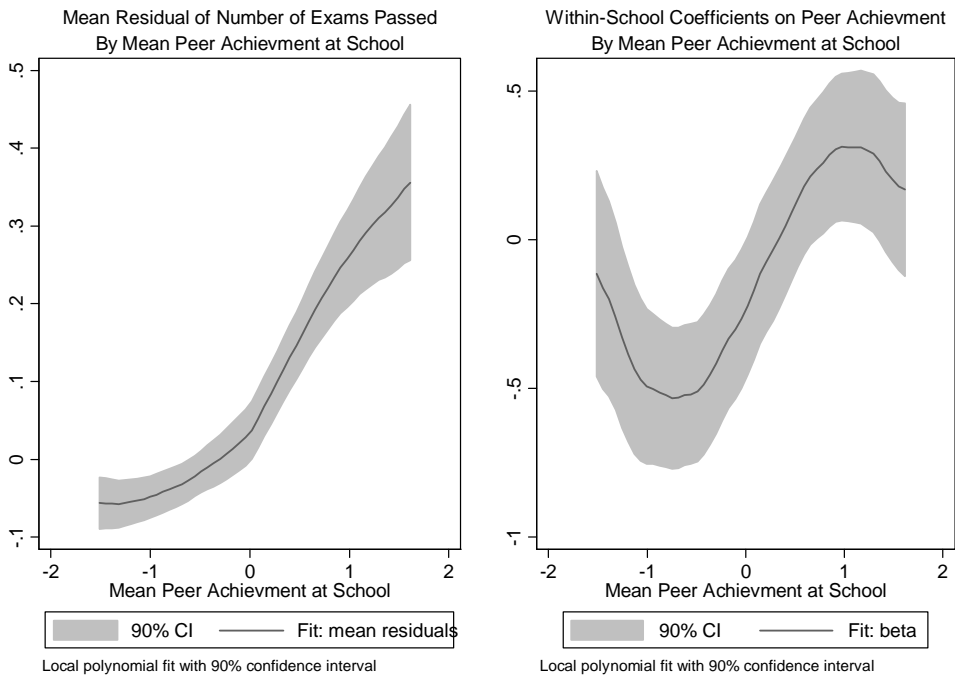


Figure 3: *Graphical evidence of non-linear peer effects*

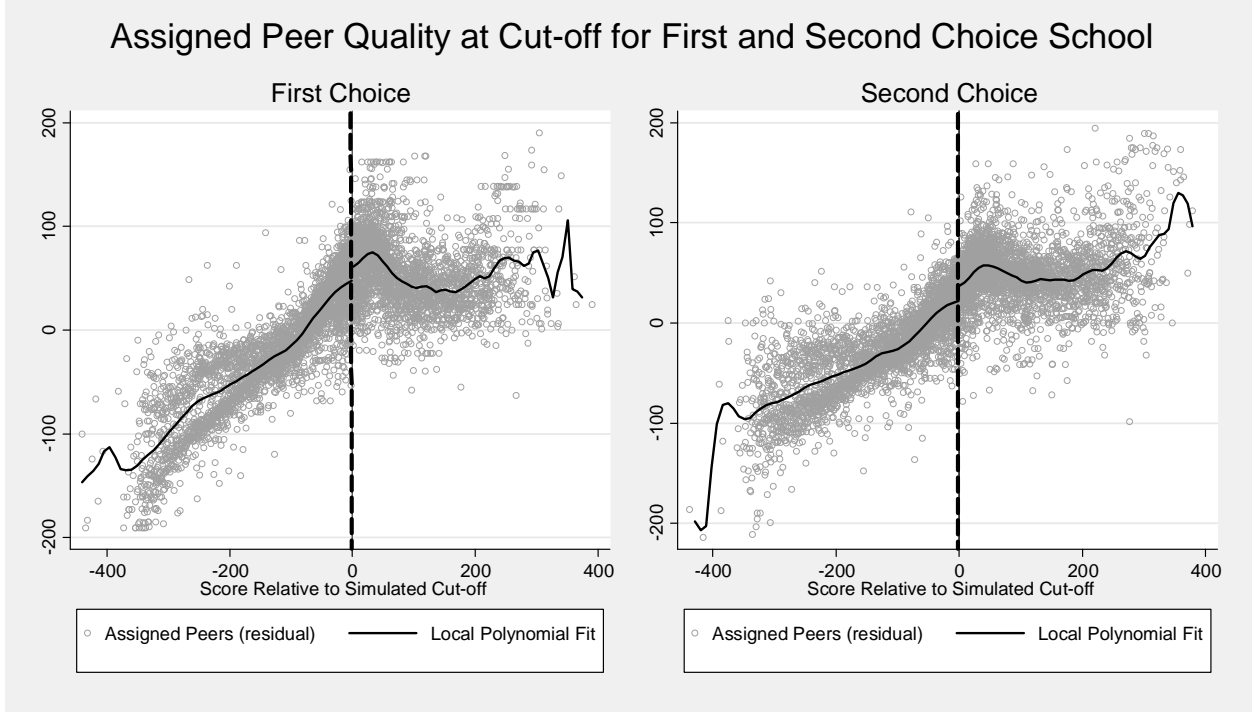


Figure 4: *Discontinuity in assigned peer quality (raw SEA scores are shown) through the assigned cut-off for the first and second choice school.*

Appendix

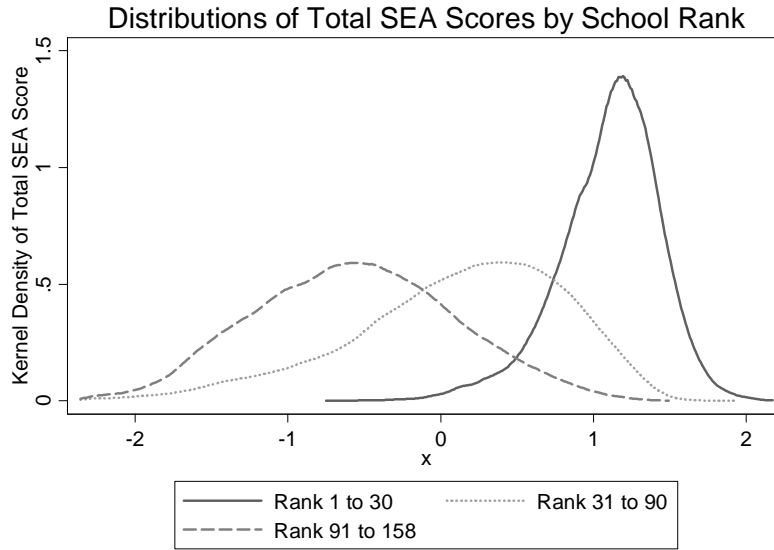


Figure A1: *Distribution of total SEA scores by school rank*

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

The simulated cut-off is constructed sequentially as follows: (1) All secondary school sizes are given,²⁵ (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.

²⁵ 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.