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Can higher-achieving peers explain the benefits to attending selective schools? Evidence from Trinidad and Tobago

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ABSTRACT

Using exogenous secondary school assignments to remove self-selection bias to schools and peers within schools, I credibly estimate both (1) the effect of attending schools with higher-achieving peers, and (2) the direct effect of short-run peer quality improvements within schools, *on the same population*. While students at schools with higher-achieving peers have better academic achievement, within-school short-run increases in peer achievement improve outcomes only at high-achievement schools. Short-run (direct) peer quality accounts for only one tenth of school value-added on average, but at least one-third among the most selective schools. There are large and important differences by gender.

1. Introduction

In many nations, there is fierce competition for scarce slots at selective schools (Hastings and Weinstein, 2007; Hsieh and Urquiola, 2006). This is, in part, because students at more selective schools typically have better outcomes — giving the impression of sizable benefits to attending selective, and often prestigious, schools. However, because motivated and high-achieving students tend to select to these schools, these differences may reflect selection rather than selective schools providing greater value-added. Addressing this selection problem, Jackson (2010) uses a quasi-experimental design and finds that attending a more selective school in Trinidad and Tobago has positive effects on exam performance and high-school graduation. While not all studies find positive selective school effects (e.g. Abdulkadiroglu et al., 2009), similar positive effects have been found in Romania (Pop-Eleches and Urquiola, 2008), the United Kingdom (Clark, 2007), and the United States (Dobbie and Fryer, 2011).

The effect of attending a more selective school reflects both short-run peer effects (that arise from contemporaneous interactions with higher-achieving classmates *in addition to interactions with parents and teachers*) and the effect of inputs that may be endogenous to long-run differences in peer quality across schools (such as teacher quality, superior management style, or funding).[2] As such, if students benefit directly from higher-achieving classmates (Hanushek et al., 2003; Hoxby and Weingarth, 2006; Lavy et al., 2010), part of the benefit to attending a selective school may be attributed to the direct benefits of having higher-achieving peers. If the high concentrations of high-achieving students afforded by selective schools engender an environment particularly conducive to learning, then adopting the practices of selective schools (such as strict discipline and educated teachers) in other schools will not yield similarly impressive results. Without knowing how much of the benefits associated with successful selective schools is due to those schools providing higher-achieving peers, we will have little indication of whether successful school models can be replicated in other settings.

I aim to shed new light on this issue by investigating the extent to which the positive selective school effects documented in Trinidad and Tobago (Jackson, 2010) can be attributed to selective schools providing higher-achieving contemporaneous peers.[3] This poses empirical difficulties because; (a) school selectivity and peer quality generally move together, (b) students select to schools, and (c) students select to peers. However, peculiarities of the Trinidad and Tobago education system provide a rare opportunity to overcome these difficulties.

To address concerns that peer quality and input quality tend to move together, I identify the effects of attending a selective school (a school with higher-achieving peers) using variation across schools, and I identify the direct effect of exposure to higher-achieving peers using only the variation in peer achievement across cohorts within

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[2] Jackson (2009) presents evidence that teacher quality is determined in part by student characteristics.

[3] This paper does not investigate the broader related question of how much school value-added can be explained by peer quality. This would be very ambitious research to undertake. The data available and the nature of the exogenous variation preclude a rigorous treatment of this broader question.

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schools — effectively holding input quality constant.[4] I present an analytical framework that shows under certain conditions, the marginal effect of higher achieving peers obtained within schools divided by the marginal effect of higher achieving peers obtained across schools will yield the fraction of the school selectivity effect that can be directly attributed to that school providing higher-achieving peers. The intuition is as follows: Suppose attending a school with 0.1σ higher quality peers is associated with passing 0.3 more exams. If the same 0.1σ increase in peer quality *within schools* (where other inputs are held constant) is associated with passing 0.2 more exams, it would suggest that $0.2/0.3 = 66\%$ of the school selectivity effect can be explained by the differences in peer quality across schools.

To address concerns that students may self-select to schools and peers, I restrict analysis to a sub-sample where students are assigned to schools by the Ministry of Education (MOE) based on observable characteristics that I can control for directly — precluding self-selection to one's assigned school or assigned peers. I use the assignments to construct instruments for (a) the selectivity of the schools that students attend and (b) changes in the incoming achievement of peers across cohorts within schools. I present tests indicating that (a) school assignments are conditionally exogenous, and (b) changes in assigned peer quality across cohorts within schools are conditionally exogenous. One remaining concern is that changes in peer achievement within schools across cohorts could be correlated with changes in unobserved school inputs. This is unlikely because the schools used are centrally operated by the MOE that rarely alters school policies, spending, or inputs on a school-by-school basis. Moreover, I show that changes in assigned peer quality over time within schools are unrelated to changes in a school's desirability (a measure of perceived long-run school quality) or observed teacher quality.

I use the number of secondary school-leaving exams passed at the end of 10th grade as the main outcome. This variable is a summary statistic for overall educational attainment because it is sensitive to dropping out of school, the number of exams attempted, and performance on a given exam. Echoing the findings of Jackson (2010), attending schools with higher incoming peer achievement increases the number of exams passed. Also, increases in mean peer achievement within schools increase the number of exams passed. The marginal effect of increases in mean peer achievement across schools is about 10 times larger than the marginal effect of increases in mean peer achievement across cohorts within schools — implying that approximately $1/10 = 10\%$ of the selective schooling effect can be attributed to the achievement level of peers. However, the direct marginal effect of peers varies considerably across schools such that peer achievement can account for two-thirds of the school selectivity effect among the most selective schools, but explains little of the effect for the lower three quartiles of schools. Four additional empirical facts support the notion that a sizable part of the selective school effect is a peer effect: (1) among high-achieving schools, both the benefits to attending a more selective school and the benefits of increased peer achievement within schools are large; (2) among low-achieving schools both the benefits to attending a school with higher-achieving peers and benefits of increased peer achievement within schools are small; (3) females, who benefit more from attending selective schools also enjoy the largest benefits to marginal increases in peer achievement within schools, and (4) gender differences in the response to peers can completely explain gender differences in response to schools.

In this paper, I present a framework within which one can determine how much of a school selectivity effect is directly attributable to a peer achievement effect. Using this framework, this paper presents evidence that, among the most selective schools, much of the school selectivity effect can be directly attributed to the incoming achievement level of the peers — such that the successes at the most selective schools may not be scalable. Conversely, I show that incoming peer achievement can only explain a small fraction of the school selectivity effect on average, so that school attributes that generate the large differences in value-added between low- and middle-achieving schools are *potentially* scalable.

The remainder of the paper proceeds as follows: Section 2 describes the Trinidad and Tobago education system and the data. Section 3 lays out the analytic framework. Section 4 outlines the empirical strategy. Section 5 presents the results, specification tests, and robustness checks. Section 6 concludes.

2. The Trinidad and Tobago education system and the data

In Trinidad and Tobago secondary school begins in first form (6th grade) and ends at fifth form (10th grade) when students take the Caribbean Secondary Education Certification (CSEC) examinations. The exams are given in 31 subjects and are externally graded. Students who pass five or more subjects including English language and mathematics exams meet the requirements for secondary school graduation.[5] There are eight public school districts, and private schools account for a small share of student enrollment and tend to serve those who “fall through the cracks” in the public system.[6] There are two types of public secondary schools: Government schools, and Government Assisted schools (assisted schools). Government schools provide instruction from 6th through 10th grade and often continue to 12th grade. These schools teach the national curriculum and are fully funded and operated by the Government. Assisted schools are almost identical to Government schools but differ along the following key dimensions: (a) Assisted schools are run by religious boards and are often single-sex schools; (b) all operating expenses except teacher costs are publicly funded; (c) while the MOE assigns students to fill all available slots at Government schools, the MOE assigns students to 80% of the open slots at Assisted schools. The remaining 20% of school slots at Assisted schools are assigned by the principal. This last distinction is key because unlike assignment to Assisted schools, assignment to Government schools is not subject to self-selection bias.

2.1. Data and summary statistics

The data used in this study come from two sources: the official Secondary Entrance Assessment (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, primary school district, and the secondary school to which they were assigned by the MOE. The SEA exam is comprised of five subjects that all students take: math, English, science, social studies, and an essay. To track these 5th grade students through to secondary school, I link the SEA data with the CSEC examination data both four and five years later. About 72% of SEA test takers were linked to CSEC exam data (consistent with national dropout rate of approximately 30%).[7] The CSEC data contain each student's grades on each CSEC exam and the secondary school they attended. In the data, there are 123 public secondary schools, some small test-taking centers and private schools. Among students linked to the CSEC data, under 7% attended a private institution, were home schooled, or were unaffiliated with any public education institution. To ensure that I have a sample

[4] Similar approaches to identifying direct peer effects are used in Ammermueller and Pischke (2006), Lavy and Schlosser (2009), Hoxby and Weingarth (2006), and Hanushek et al. (2003).

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

[6] Students at private secondary schools have SEA scores 0.33 standard deviations below the average.

[7] Students were matched on name, gender, and date of birth. The match rate is consistent with the national dropout rate of one third. Students with missing outcomes are coded as having zero passes *and included in the sample*.

within which the school assignments are not subject to any selection, I drop students who are *assigned* to Assisted schools and private schools. The resulting analytical dataset contains 150,701 students across seven cohorts and 158 school assignments.[8]

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). SEA scores are in standard deviation units. Females make up about half of students in each school group. There is much variation in school and peer quality. The 30 most selective schools 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. As expected, selective schools have better outcomes. About 87% of students at schools ranked better than 30 took the CSEC exams compared to 71% for schools ranked 31 to 90, and 59% for schools ranked below 90. The average student at a top 30 school passes 4.44 exams, compared to 1.9 exams in schools ranked between 31 and 90, and passing only 1 exam at schools ranked below 90. Some of these differences are due to students not taking the CSEC exams having no passes.

The schools that attract the brightest students typically have the best school resources. Schools with the highest achieving students are on average smaller with cohort sizes being about 120 students at the top 30 schools and about 440 students in both other groups of schools. Similarly, about 58% of teachers at schools ranked better than 30 have a bachelor's degree compared to 55% for schools ranked 31 to 90, and 36% for schools ranked below 90. Given that having a university or college degree likely has an important effect on teaching ability, this may translate into sizable teacher quality differences across schools. Higher ranked schools also have fewer inexperienced teachers. Specifically, 14% of teachers at schools ranked better than 30 have between 0 and 3 years of teaching experience compared to 16% for schools ranked 31 to 90, and 24% for schools ranked below 90.

Table 1
Summary statistics.

Variable	Rank of school in mean incoming SEA scores 1 to 30	31 through 90	Above 90
Total SEA score	1.102	0.122	−0.561
	(0.333)	(0.693)	(0.654)
Female	0.490	0.499	0.505
	(0.500)	(0.500)	(0.500)
Take the CSEC exams	0.871	0.706	0.586
	(0.335)	(0.456)	(0.493)
Number of exams passed	4.444	1.939	1.005
	(2.628)	(2.308)	(1.746)
Certificate [a]	0.508	0.128	0.037
	(0.500)	(0.334)	(0.189)
Cohort size	119.1	439.7	443.9
	(63.6)	(218.5)	(241.9)
% teacher with BA	0.584	0.552	0.360
	(0.232)	(0.197)	(0.248)
% teacher 0 to 3 years experience	0.137	0.162	0.240
	(0.093)	(0.120)	(0.136)
% Teachers 4 to 20 years experience	0.399	0.418	0.402
	(0.169)	(0.111)	(0.113)
% Teacher 20 plus years experience	0.309	0.356	0.279
	(0.153)	(0.188)	(0.145)
Observations	17,811	84,746	48,144

Note that the teacher variables are only available for 2000, 2001, and 2002.
[a] Certificate is variable that is equal to 1 if the student passes five CSEC exams including English and Mathematics.

3. Econometric framework

I present a model showing that, under reasonable assumptions, the ratio of the coefficient on peer quality obtained using variation within-schools across cohorts and the coefficient on peer quality obtained using variation across schools yields an estimate of the proportion of the effect of attending a school with higher-achieving peers that can be directly attributed to contemporaneous exposure to higher-achieving peers (versus other school-level input or practice differences that can be replicated in other schools). The intuition is as follows: Suppose attending a school with 0.1σ higher quality peers is associated with passing 0.3 more exams. If the same 0.1σ increase in peer quality *within schools* (reflecting direct peer interactions, contextual effects, and teacher and parent interactions that may all be directly affected by the contemporaneous classroom composition) is also associated with passing 0.3 more exams, it would suggest that 0.3/0.3 = 100% of the school selectivity effect can be explained by the differences in peer quality across schools. However, if a 0.1σ increase in peer quality within schools is associated with passing only 0.03 more exams, it implies that only 0.03/0.3 = 10% of the school selectivity effect can be explained by the differences in peer quality across schools. The framework below formalizes this intuition.

Input quality at school j at time t, denoted I_{jt}, is an increasing function of permanent (long-run) peer quality $\overline{P}_j$ and idiosyncratic determinants u_{jt}. This is written in Eq. (1) below.

$$I_{jt} \equiv g\left(\overline{P}_j\right) + u_{jt} \quad \text{where} \quad \mathrm{E}\left[u_{jt}|\overline{P}_j\right] = \mathrm{E}\left[u_{jt}\right] = 0. \tag{1}$$

This captures the fact that school inputs (such as teacher quality and alumni donations) are endogenous to *persistent* characteristics of the student body. Input quality is not a function of contemporaneous peer quality because input quality changes are likely not sensitive to transitory shocks to peer quality. I present empirical support of this assumption in Section 8.

Peer quality, P_{jt}, includes a long-run component $\overline{P}_j$ and idiosyncratic component μ_{jt}.

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

The long-run component plus random error captures the fact that the schools that attract the highest/lowest achieving students have done so for years. This modeling assumption is akin to saying that Harvard and Yale (or Oxford and Cambridge) always attract the top students in any given year. I present empirical support of this assumption in Section 8.

Achievement of student i at school j at time t Y_{ijt} is an increasing function of input quality and peer quality as in Eq. (3) below, where ε_{ijt} is an idiosyncratic error term.

$$Y_{ijt} = f\left(P_{jt}, I_{jt}\right) + \varepsilon_{ijt} = f\left(P_{jt}, g\left(P_{jt}\right) + u_{jt}\right) + \varepsilon_{ijt} \tag{3}$$
$$\text{where} \quad \mathrm{E}\left[\varepsilon_{ijt}|P_{jt}, I_{jt}, u_{jt}\right] = E\left[\varepsilon_{ijt}\right] = 0.$$

For any two schools j and j' such that $\left(P_{jt} - P_{j't}\right)$ is sufficiently small, as long as f and g are continuously differentiable functions, the expected difference in student outcomes *across* schools is approximated by the first order Taylor expansion in Eq. (4).

$$\begin{aligned} E\left[Y_{ijt} - Y_{i'j't}|P_{jt}, P_{j't}\right] &\approx f_P \cdot \left(P_{jt} - P_{j't}\right) + f_I \cdot g_P \cdot \left(P_{jt} - P_{j't}\right) \\ &= \left(f_P + f_I \cdot g_P\right) \cdot \left(P_{jt} - P_{j't}\right). \end{aligned} \tag{4}$$

That is, under the identifying assumptions in Eqs. (1), (2), and (3), differences in student achievement across similarly selective schools associated with peer quality differences reflect the direct marginal effect of short-run variation in peers f_P plus the marginal

[8] There are more assignments than high schools because some when there are more students than available spots, the Government assigns students with the lowest SEA scores to small “temporary” schools or purchases seats in private schools. Omitting students assigned to such schools does not affect the results.

effect of input differences across schools that exist in equilibrium due to the long-run differences in peer quality $f_I \cdot g_P$, all times the difference in peer quality across the two schools.

Because *in expectation* there are no systematic differences in input quality within schools over time, where $(P_{jt} - P_{jt-1})$ is sufficiently small, the expected difference in student outcomes across cohorts t and $t - 1$, within school j conditional on peer quality is approximated by the first order Taylor expansion in Eq. (5) below (note: all terms involving changes in inputs are zero).

$$E\left[Y_{ijt} - Y_{i'jt-1} | P_{jt}, P_{jt-1}, J = j\right] = f_P\left(P_{jt} - P_{jt-1}\right). \tag{5}$$

Eq. (5) illustrates that the differences in outcomes of observationally similar students attending the same school but exposed to different peers because they attend at different times reflect only the direct marginal effect of short-run variation in peers f_P times the difference in peer quality across cohorts within the same school. As such, β, the coefficient on *within-school* changes in peer quality divided by the coefficient on peer quality obtained *across schools* γ, yields $\beta/\gamma \approx f_P/(f_P + f_I \cdot g_P)$, which is the fraction of the benefits to attending a marginally more selective school that can be directly attributed to exposure to marginally higher-achieving peers.

This intuitive interpretation is valid for marginal changes in peer quality and school selectivity. However, for large differences in peer quality (e.g. comparing the most and least selective schools) one must consider the higher order terms of the Taylor expansion. If (a) there are important complementarities between peer quality and inputs, or (b) important non-linearities in the marginal effect of peers or inputs, the average marginal effects (obtained across a large range of peer quality) *may* not accurately represent the marginal effects for any particular school. As such, I present both naïve estimates that make comparisons across all schools (where complementarities and non-linearities may muddy interpretation) and also the preferred estimates based on similarly selective schools that will be valid even in the presence of complementarities and non-linearities. I detail the strategies to uncover estimates of $\beta \approx f_P$ and $\gamma \approx (f_P + f_I \cdot g_P)$.

4. Empirical strategy

4.1. Estimating the effect of attending a more selective school

To estimate the effect of attending a school with higher-achieving peers, γ, the basic approach is to compare the outcomes of observationally similar students at different schools. For the naïve baseline specification, I model the outcome of student i at a school j in cohort c with the following equation.

$$Y_{ijc} = f(\mathrm{SEA}_i) + \overline{\mathrm{SEA}}_{ijc} \cdot \gamma + \theta_c + \varepsilon_{ijc} \tag{6}$$

where $f(\mathrm{SEA}_i)$ is a function of a student's incoming SEA score, $\overline{\mathrm{SEA}}_{ijc}$ is the mean incoming SEA score of all other students at school j in cohort c with student i, θ_c is a cohort fixed effect and ε_{ijc} is the unobserved determinants of student achievement. Because students may select to schools based on unobserved determinants of achievement, naïve estimation of Eq. (6) might not uncover the parameter γ. This motivates an instrumental variable strategy that uses students' initial school assignments by the MOE to remove bias due to student selection to schools.

4.2. Estimating the direct effect of contemporaneous peer achievement

To estimate the effect of contemporaneous exposure to higher-achieving peers, β, I compare the outcomes of observationally similar students at the same school but who are exposed to peers with different levels of incoming achievement because they are in different cohorts. In the naïve specification, I model the outcome of student i at a school j in cohort c with Eq. (7).

$$Y_{ijc} = f(\mathrm{SEA}_i) + \overline{\mathrm{SEA}}_{ijc} \cdot \beta + \theta_c + \theta_j + \varepsilon_{ijc}. \tag{7}$$

All variable are defined as in Eq. (6). The difference between Eqs. (6) and (7) is the inclusion of a school fixed effect θ_j, that absorbs time-invariant variation in peer quality — so that estimation is based on only idiosyncratic transitory variation in peer quality within schools across cohorts. This same strategy has successfully been employed in several papers including Ammermueller and Pischke (2006), Lavy and Schlosser (2009), and Hoxby and Weingarth (2006). However, because (a) students may select to schools based on unobserved determinants of achievement and this selection may change over time, and (b) mean peer achievement could change within schools for reasons other than random transitory shocks, naïve estimation of Eq. (7) by OLS may not uncover β. This motivates an instrumental variable strategy that uses students' initial school assignments, and the initial school assignments of students' peers, to remove bias due to student selection to schools and bias due to endogenous changes in peer achievement over time.

5. Student assignment rules

After 5th grade, *all* students take the SEA examinations. Each student submits an ordered list of four secondary school choices *before* taking the SEA examinations (i.e. before they have any indication about their performance or their final scores). These choices and the SEA scores are used by the MOE to assign students to schools. School slots are assigned in successive rounds such that the most highly subscribed/ranked school fills its spots first, then the next highly subscribed school fills its slots in the second round, and so on until all school slots are filled. This is done as follows: (1) The number of school slots at each school n_j is predetermined based on capacity. (2) Students are tentatively placed in the applicant pools for their first choice schools and are ranked in descending order by SEA score within each application pool. (3) The school at which the n_jth ranked applicant has the highest SEA score is deemed the most highly subscribed/ranked school j_1, this score is the cut-off score for this school, and the top n_{j1} students in the applicant pool for top-ranked school j_1 are admitted to school j_1. (4) The top ranked school's slots and the admitted students are removed from the pool, and the second choice becomes the new "first choice" for students who had the top ranked school as their first choice but did not gain admission. (5) This process is repeated in round two to assign students to the second highest ranked school j_2 and determine the cut-off score for the second ranked school. This is repeated in subsequent rounds until all slots are filled. Students are assigned to the highest ranked school they are eligible for in their choice set and receive a single placement. This is a deferred acceptance algorithm (Gale and Shapley, 1962). While the optimal set of school choices is difficult to solve, Chade, and Smith (2006) demonstrate that the choice set should include the school with the largest expected payoff (utility conditional on attendance times the likelihood of admission),[9] students should rank selected schools in order of actual preferences,[10] and should include a "reach" school for which admission is unlikely but the utility conditional on attendance is high. This process applies to over 95% of students. However, assisted schools (16% of school slots) can admit 20% of their incoming class at the principal's discretion. The rule is used to assign 80% of the students at these schools,

[9] This condition suggests that better prepared students (who have higher admission probabilities) should have more selective choices in their choice set.

[10] Because students will never be admitted to their second choice school if they are admitted to their first choice school, the chances of being assigned to a school is never increased by placing that school lower in one's rankings.

while the remaining 20% can be hand-picked by the principal before the next-highest ranked school fills any of its slots.[11]

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 MOE used to assign students, I can simulate where the cut-offs would have been if Assisted schools could not hand pick students (see Appendix Note 1). After simulating the clean cut-offs, I estimated the likelihood of attending one's top choice school as a function of one's score relative to the simulated cut-off for one's top choice school. Fig. 1 depicts a sudden increase in the likelihood of assignment to one's top choice school as one's score goes from below to above the simulated cut off — indicating that (a) the assignments operate as described, and (b) there are meaningful differences in school assignments associated with scoring above/below a cut-off that are not due to selection.[12] The fact that assignments to *Government schools* are orthogonal to unobserved student characteristics (where both choices and SEA scores are observed) plays a crucial role for identification, and motivates my using only those students with exogenous assignments to Government schools.

In general, students tend to put schools with higher-achieving peers higher up on their preference ranking. On average, the difference between the mean incoming SEA scores at a student's top choice school and second, third and fourth choice school is 0.277, 0.531, and 0.91 standard deviations, respectively. Also, higher-achieving students tend to have more selective schools in their list, students request schools with the same religious affiliation as their own, and students typically list schools geographically close to home. Because school choices are a summary statistic for student/parental aspirations, preferences, expectations about ability, religious affiliation, and geographic location, these choices are a powerful set of controls.

6. Identification strategy

Because students assigned to Government schools cannot self-select into the assignment, conditional on incoming test scores and student choices, the school assignments within the group of Government schools is exogenous. I detail how I exploit this fact to identify (a) the effect of attending a selective secondary school, and (b) the effect of marginal increases in incoming peer achievement within a school.

6.1. Using instruments to estimate the instruments to estimate effect of attending a more selective school

The problem with merely comparing outcomes of observationally similar students who attend different schools (as in Eq. (6)) is that students may select or transfer into selective schools based on unobserved characteristics that directly affect student outcomes. As such, one needs variation in school attendance that is beyond students' control. Conditional on school choices, the assignment rule creates test score cut-offs above which students are assigned to one school and below which they are assigned to another. Among students who chose a selective school, the likelihood of being assigned to (and attending) a more selective school increases in a sudden and discontinuous manner as one's score

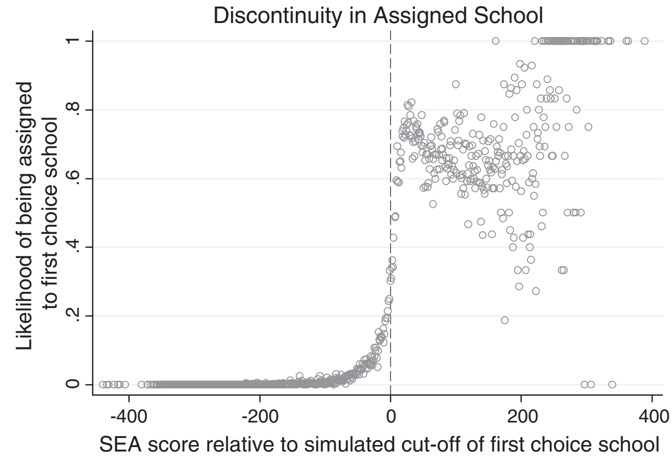


Fig. 1. Likelihood of being assigned to one's top choice school.

goes from below to above the cut-off for that selective school. As such, conditional on school choices, mean peer quality increases suddenly as one's SEA score goes from below to above the cut-off for one's preferred school (as illustrated in Fig. 1). If the locations of the cut-offs are orthogonal to student characteristics, and the effect of test scores on outcomes is smooth through the cut-offs, one can attribute any sudden jumps in outcomes as one's score goes from below to above the cut-offs to the sudden exogenous increased likelihood of attending a selective school. This is amenable to a regression discontinuity (RD) type design.

To capture this discontinuity variation, I create variables that denote whether a student scores above the simulated cut-off for their first, second, and third choice schools as instruments for mean peer quality at the school attended while controlling for smooth functions the incoming SEA test score. Because the cut-offs are only exogenous among students with the same school choices, I also condition on school choices. Formally, I estimate the outcome of student i from cohort c, at school j with the following equations by two-stage-least-squares (2SLS).

$$\begin{aligned}\widehat{\mathrm{SEA}}_{ijc} &= f(\mathrm{SEA}_i) + \pi_1 \cdot above1_i + \pi_2 \cdot above2_i + \pi_3 \cdot above3_i + X_i\rho_1 \\ &\quad + \sum I_{P_i=p} \cdot \theta_{p1} + \theta_{c1} + \varepsilon_{ijc1}. \qquad (8)\\ Y_{ijc} &= f(\mathrm{SEA}_i) + \widehat{\mathrm{SEA}}_{ijc} \cdot \gamma + X_i\rho_1 + \sum I_{P_i=p} \cdot \theta_{p2} + \theta_{c2} + \varepsilon_{ijc2}\end{aligned}$$

In Eq. (8), $\widehat{\mathrm{SEA}}_{ijc}$ is the mean total SEA scores for students attending the same school j as student i in cohort c. The excluded instruments *above*1, *above*2, and *above*3 are indicator variables denoting if student i's SEA score is above the cut-off for their first, second, or third choice school, respectively. $f(\mathrm{SEA}_i)$ is a smooth function of student SEA scores, $I_{P_i=p}$ is an indicator variable denoting whether a student has a particular school preference ordering (choices), and θ_{p2} is a preference ordering (choices) fixed effect (i.e. there is an indicator variable denoting each distinct ordered list of schools. For example there is an indicator for all students who list schools A, B, C and D as their first, second, third and fourth choice schools, and a different indicator for all students who list A, B, D and C as their first, second, third and fourth choice schools.), θ_c is a SEA test taking cohort fixed effect, X_i is student gender, and ε_{ijc} is the idiosyncratic error term.

While this RD model should yield consistent estimates of γ, it is not the preferred model because it ignores important exogenous variation across cut-offs. That is, it ignores the fact that some cut-offs are associated with larger increases in peer quality than others. For example, consider a group of students (group 1) whose first and second choice schools have peer quality of 95 and 90 points, respectively. Within group 1, the difference in peer quality associated with scoring above the cut-off for the top choice school is 5 points. Consider now a different group of students (group 2) whose first and

[11] These hand-picked students are chosen based on family alumni connections, being relatives of teachers, or religion. These students need not list the school as their top choice. Students receive one assignment and are never made aware of other schools they would have been assigned to had they not been hand-picked.

[12] Note that there is some fuzziness in the cut-off depicted in Fig. 1. This is for two reasons: First, there is some noise due to the fact that the cut-offs are estimated. Because this figure combines several estimated cut-offs into one (where the cut-off may be slightly off for each school) the aggregate combined cut-off is somewhat fuzzy within the range of the simulation error. Second, even if the cut-offs were not simulated, the cutoff would be somewhat fuzzy because this figure includes all schools (including Assisted schools where there is only 80% compliance with the cut-offs). Note that because assisted school do not have to comply with the assignment rules, some students are assigned to an assisted school even if it is not their top choice school. Accordingly, some students score above the cut-off for their preferred school and are not assigned to these schools.

second choice schools have peer quality of 95 and 80, respectively. Within group 2, the difference in peer quality associated with scoring just above the cut-off for the top choice school is 15 points. If attending a school with higher-achieving peers is truly associated with better outcomes, there should be larger improvements in outcomes associated with scoring above the top choice cut-off for group 2 than for group 1. The RD model treats all cut-offs the same and ignores this variation.

In the preferred strategy, I exploit *both* the existence of the exogenous cut-offs and also the differences in treatment intensity across cut-offs for students with different school choices. To do this, I use *assigned* peer quality (i.e. the average incoming test scores of other students assigned to the same school j^* as student i in cohort c), $\widetilde{\text{SEA}}_{ij*c}$, as an instrument for *actual* peer quality while controlling for a full set of choice indicator variables, and flexible functions of the SEA score. Variation in assigned peer quality comes from two sources: (a) discontinuity variation that causes assigned peer quality to jump suddenly as one's score goes from below to above the cut-off for a preferred school, and (b) differences in the treatment intensity (i.e. the jump in assigned peer quality) that exist across cut-offs by imposing the condition that cut-offs with larger increases in assigned peer quality have larger effects on the outcome.[13] To implement the preferred strategy, I estimate the outcome of student i from cohort c, at school j with the following system of equations by two-stage-least-squares (2SLS).

$$\begin{aligned}\widehat{\text{SEA}}_{ijc} &= f(\text{SEA}_i) + \widetilde{\text{SEA}}_{ij*c}\cdot\delta_1 + \sum I_{P_i=p}\cdot\theta_{p1} + X_i\rho_1 + \theta_{c1}\\ Y_{ijc} &= f(\text{SEA}_i) + \widehat{\text{SEA}}_{ijc}\cdot\gamma + \sum I_{P_i=p}\cdot\theta_{p2} + X_i\rho_2 + \theta_{c2} + \varepsilon_{ijc2}.\end{aligned} \tag{9}$$

All variables are defined in Eq. (8). The 2SLS estimate of the coefficient on peer achievement, γ from Eq. (9) should be an unbiased estimate of the school selectivity effect because (1) the analytic sample only includes those schools and students for whom the initial school assignment is exogenous conditional on incoming test scores and choices, (2) the excluded instrument, mean peer quality of the students assigned school (based on other students assigned to the school), is not affected by students subsequently transferring to schools they prefer, and (3) inference is based on comparisons within groups of students who are similar in important ways but who were assigned to different schools for reasons beyond their control.

While there is no way to test for a correlation between the instruments and *unobserved* student characteristics, I present evidence consistent with the identifying assumption by showing that *observed* covariates are uncorrelated with the instruments. To show that the above cut-off indicator variables are exogenous, I estimate the 2SLS model predicting the observed covariates (each religion, each primary school district, gender, and the number of times a student attempted the SEA exams) while instrumenting for mean peer quality with the indicator variables that connote scoring above the cut off for one's first, second, or third choice school. I present the results in Table 2. Mean peer quality (as predicted by the cut-off indicators) is not correlated with any of these covariates at the 5% level-indicating balance of covariates above and below the cut-offs. As a further test of the exogeneity of the cut-offs, I test for smoothness in the density of observations through the cut-offs and find no evidence of a discontinuity (not presented). To test for the exogeneity of *assigned* peer quality (i.e. that cut-offs with larger changes in peer quality are uncorrelated with other characteristics), I run a regression with SEA score fixed effects, choice fixed effects, and the assigned peer quality to predict the covariates. Assigned peer quality is not associated with student religion, gender, number of SEA attempts, or the students' primary school district at the 5% level — suggesting that the conditional exogeneity assumption is valid. I present further robustness checks in Section 8.

6.2. Using instruments to estimate the effect of improved peer quality within schools

The problem with simply comparing the outcomes of observationally similar students at the same school exposed to different peers is that (a) students select to schools and (b) peers select to schools. To address this problem, I use variation in peer quality that is not subject to student or peer selection. The fact that students assigned to Government schools cannot self-select into their assignment allows this. I use across-cohort within-school changes in average incoming test scores of other students *assigned* to the same *assigned* school as an instrument for changes in actual peer quality within student's actual schools. To do this, I augment the cross-sectional Eq. (8) to include an assigned school fixed effect θ_{j*}.

$$\begin{aligned}\widehat{\text{SEA}}_{ijc} &= f(\text{SEA}_i) + \widetilde{\text{SEA}}_{ij*c}\cdot\delta_1 + \sum I_{P_i=p}\cdot\theta_{p1} + X_i\rho_1 + \theta_{c1} + \theta_{j*1}\\ Y_{ijc} &= f(\text{SEA}_i) + \widehat{\text{SEA}}_{ijc}\cdot\beta + \sum I_{P_i=p}\cdot\theta_{p2} + X_i\rho_2 + \theta_{c2} + \theta_{j*2} + \varepsilon_{ijc2}.\end{aligned} \tag{10}$$

The difference between Eq. (10) above and the naïve model of Eq. (7) is that I include fixed effects for the student's assigned secondary school (as opposed to the actual school) and I instrument for the actual peers at the actual school with the assigned peers at the assigned school. As long as the school assignments are exogenous, conditional on test scores and student choices, Eq. (10) will remove bias due to the selection of students and peers and will yield an unbiased estimate of β. Note that the instruments do not use all the cohort-to-cohort variation, but use only that variation across cohorts within schools that is predicted by the exogenous school assignments. As such, the instruments eliminate not only bias due to own selection to peers, but also bias due to any changes in peer quality within a school that is driven by changing peer selection to schools. Also note that unlike the across-school model that is based on the marginal admit to a selective school, the direct peer effects are identified from all students at the school (not only the marginal admit).

The variation in within-school changes in peer quality exploited by the instruments come from two sources: (a) different sets of students deciding to apply to the school from year to year and (b) from modest changes in overall (across schools) cohort quality or size. I discuss each source of arguably exogenous variation in turn. The first plausible source of exogenous variation in peer quality within schools over time comes from modest year-to-year changes in overall (across schools) cohort quality and cohort size. Because there are only about 20 thousand students taking the SEA each year and schools take small slices (between 100 and 500 students) out of the SEA distribution based largely on student rank, both small transitory changes in the distribution of test scores and idiosyncratic variation in the distribution of student preferences over time lead to meaningful exogenous variation in assigned peer achievement within assigned schools across cohorts. To illustrate this variation due to small changes in the SEA distribution, I look at the mean SEA scores for students ranked 1 to 100, students ranked 3000 to 3500, and 10,000 to 10,500 for each year between 1995 and 1998. Figs. 2 and 3 show that due to small changes in the distribution of test scores, the average incoming test scores of students ranked 1 to 100 decreased by 0.1σ between 1995 and 1996 and increased by 0.06σ between 1996 and 1997. In comparison, mean test scores of students ranked 10,000 to 10,500 increased by 0.05σ between 1995 and 1996 and fell by 0.02σ between 1996 and 1997. Within district changes are even larger. Mean test scores of the top 100 students in the largest district increased by 0.21σ between 1995 and 1996, while the mean test scores of students ranked between 2000 and 2500 fell by 0.11σ.[14]

[13] This is because all variation in the assigned school come from the cut-offs. Indeed assigned peer quality is just the interaction between scoring above the cut-off for a school in one's choice set multiplied by the assigned peer quality at that school (for the highest school the student scores above the cut-off for).

[14] Because schools must fill a fixed number of school slots every year there is no correlation between peer quality changes and cohort size within a school over time. 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.

Table 2
Tests for balance of covariates.

Regression discontinuity cross-sectional model balance test (using scoring above a cut-off as an instrument)								
Dependent variable	Religion indicators						SEA attempts	Female
	1	2	3	4	5			
Mean total (instrumented)[a]	−0.003 [0.006]	0.007 [0.007]	−0.005 [0.011]	<0.00000 [0.012]	0.006 [0.018]		−0.005 [0.016]	0.01 [0.024]
	District indicators							
	1	2	3	4	5	6	7	8
Mean total (instrumented) [a]	0.003 [0.013]	−0.012 [0.010]	0.004 [0.009]	−0.008 [0.008]	−0.002 [0.007]	−0.001 [0.013]	0.015 [0.009]$^{+}$	0.002 [0.003]
Preferred cross-sectional model balance tests								
Dependent variable	Religion indicators						SEA attempts	Female
	1	2	3	4	5			
Mean total assigned	0.001 [0.004]	<0.000 [0.006]	−0.005 [0.005]	0.009 [0.006]	−0.008 [0.005]		0.013 [0.007]$^{+}$	0.002 [0.004]
	District indicators							
	1	2	3	4	5	6	7	8
Mean total assigned	−0.003 [0.003]	−0.003 [0.002]	0.004 [0.003]	<0.00000 [0.002]	−0.002 [0.002]	−0.001 [0.005]	<0.00000 [0.000]	<0.00000 [0.000]
Within-school balance tests (using assigned peer quality as an instrument)								
Dependent variable	Religion indicators						SEA attempts	Female
	1	2	3	4	5			
Mean total assigned	−0.004 [0.005]	<0.000 [0.008]	0.019 [0.015]	−0.014 [0.012]	<0.000 [0.009]		0.005 [0.012]	−0.005 [0.011]
	District indicators							
	1	2	3	4	5	6	7	8
Mean total assigned	0.004 [0.007]	0.008 [0.008]	−0.024 [0.018]	−0.007 [0.004]$^{+}$	0.004 [0.013]	−0.021 [0.016]	<0.000 [0.000]	<0.000 [0.000]

Robust standard errors in brackets.
These models are based on running regressions predicting indicator variables for each religion and primary school district, gender and the number of time the student took the SEA exams as a function of the exogenous instruments.
[a] In these models, the independent variable is the fitted values for mean peer quality as predicted by the instruments.
$^{+}$ Significant at 10%.
* Significant at 5%.
** Significant at 1%.

The second plausible source of exogenous variation in peer quality within schools over time comes from there being slightly different sets of students deciding to apply to the school from year-to-year. Because the assignment mechanism fills schools sequentially and a school's order is based on the score of the last admitted student, small changes in preferences, demographics, and scores can cause a school that fills

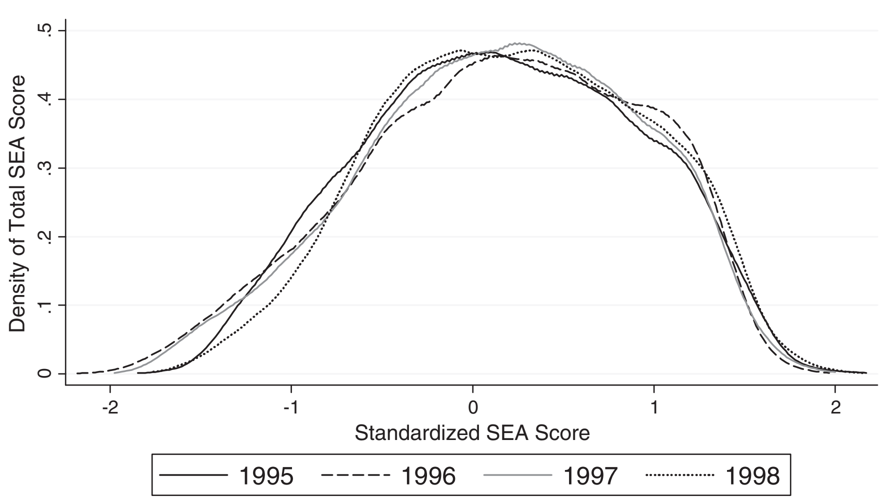


Fig. 2. Distribution of SEA scores across cohorts.

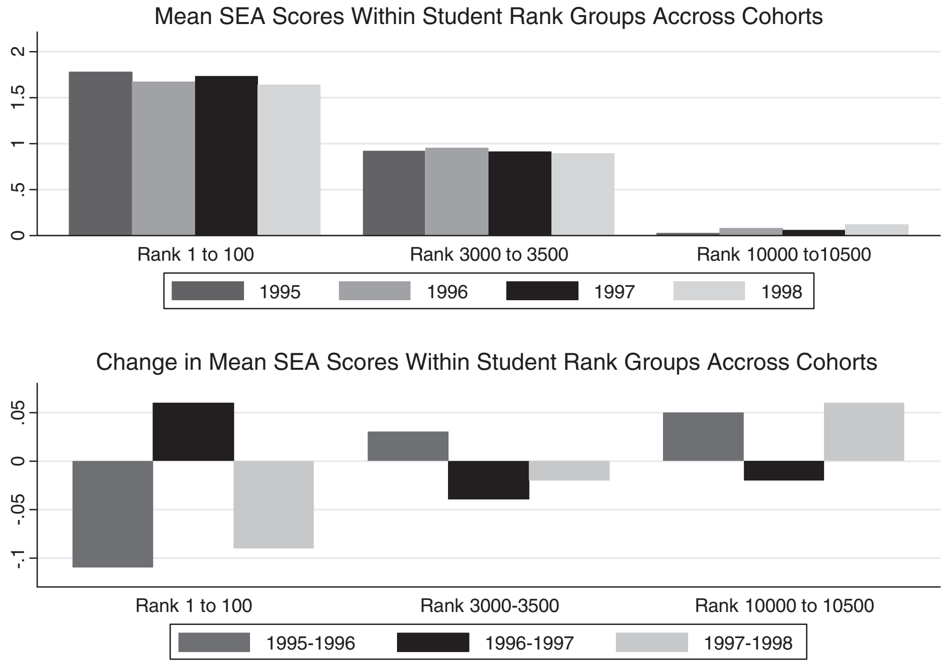


Fig. 3. Hypothetical changes in mean test scores driven only by changes in SEA test score distribution across cohorts.

its slots first in one year to be second or third the following year. As such, small perturbations in the distribution of student choices can play out into meaningful differences in peer quality within schools over time. This source of variation would be problematic if changes in student choices were correlated with changes in unobserved school characteristics. I present empirical evidence that this is not the case in Section 8.

To present evidence consistent with no selection, I show that *observed* covariates are uncorrelated with changes in assigned peers within assigned schools across cohorts. Specifically, I run a regression with functions of the SEA score, choice fixed effects, assigned school fixed effects, and the assigned peer quality to predict each covariate (Table 2). Assigned peer quality is not associated with any observable characteristic at the 5% level — suggesting that the conditional exogeneity assumption is valid. While this strategy may remove selection bias, there remains the concern that changes in peer achievement could be correlated with *changes* in unobserved school inputs. In Section 8, I present evidence that this is not the case.

7. Results

7.1. Preliminary discontinuity results

Before presenting the results from the preferred model, I present the cross-sectional results based on the discontinuity variation only in Table 3. In the first stage, scoring above the cut-off for the first, second and third choice schools is associated with attending school with peers with 0.033, 0.056 and 0.076σ higher incoming test scores, respectively. The reduced form regression indicates that scoring above the cut-off for the first, second and third choice schools is 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). As a formal test that the assigned peer quality instrument yields similar results to the cut-off instruments, I include assigned peer quality as an additional instrument (column 5). The coefficient is 1.009 (se = 0.074), and the test of overidentifying restrictions yields a *p*-value of 0.79 — indicating that the instrumental variable results based on variation in assigned peer quality are consistent with the discontinuity based instruments that rely only on variation due to the cut-offs. Note that the standard errors of the estimates are about three times smaller based on the assigned peer quality instruments. Given the increased precision associated with using all the variation, and the fact that both sets of instruments yield similar results, I now present the results from the preferred model that uses assigned peer quality as the excluded instrument.

7.2. Main results

Table 4 presents the cross-school and within-school estimates 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 form (RF) effect of assigned peer quality on students' outcomes, and the third panel presents the instrumental variable (IV) estimates that use assigned peer quality as an exogenous predictor of actual peer quality. Columns 1 and 2 present the across school models, while columns 3 and 4 present the within-school models.

A parsimonious OLS model of the number of exams passed as a function of the mean total scores of the students at the actual school, SEA cohort fixed effects, the student's gender, and a cubic in the total SEA score (column 1 top panel) yields an across school coefficient of 1.044 (se = 0.082). Controlling for student choices (column 2) yields a very similar estimate of 1.229 (se = 0.073). These OLS results indicate that a student who attends a school where peer test scores 0.2 standard deviations higher (roughly the within-school variance in peer quality) would pass about 0.244 more exams. These findings echo Jackson (2010) and Jackson (2012).

The second panel presents the reduced form effect of being assigned to a school with higher achieving peers (whether or not the student actually attended the school) that should be free from self-selection bias. If there were full compliance with the initial assignment the reduced form effect of assigned peer quality would be the same as the instrumental variables estimate of peer quality at the school *attended*. However, because a 1σ increase is assigned, peer quality is associated with less than 1σ increase in actual peer quality, the effect of assigned peer quality will be smaller than those for actual peer quality. The reduced form estimates range between 0.437 and 0.57 and are all statistically significant at the 1% level. The instrumental variable results based on assigned peer quality (lower panel) are similar to the OLS results yielding statistically

Table 3
Effects using discontinuity variation only.

	2SLS results using scoring above a simulated cut-off for a preferred school as instruments				
	1	2	3	4	5
Depended variable	Assigned mean peer scores OLS	Actual mean peer scores OLS	Exams passed OLS	Exams passed 2SLS	Exams passed 2SLS
Mean peer score	–	–	–	0.867	1.009
(actual)	–	–	–	[0.254]**	[0.074]**
Above first	0.049	0.033	−0.003	–	–
choice cut-off	[0.021]*	[0.016]*	[0.044]	–	–
Above second	0.07	0.056	0.054	–	–
choice cut-off	[0.022]**	[0.015]**	[0.033]+	–	–
Above third	0.14	0.076	0.072	–	–
choice cut-off	[0.023]**	[0.016]**	[0.033]*	–	–
p-Value of J-Stat	–	–	–	0.63	0.79
Observations	150,695	150,695	150,695	114,062	114,062

Robust standard errors in brackets.
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. All models include a 5th order polynomial in total SEA scores, cohorts fixed effects and school choice fixed effects.
+ Significant at 10%.
* Significant at 5%.
** Significant at 1%.

significant coefficient estimates between 0.85 and 1.17 — indicating that after taking self-selection into account, a student who attends a school where peer test scores are 0.2 standard deviations higher would pass between 0.17 and 0.23 more exams.

Columns 3 and 4 present the within-school results that should identify the direct effect of peer achievement on outcomes. The OLS results are based on actual incoming peer achievement at students' actual schools attended and include indicator variables for students' actual school attended. The parsimonious model that includes SEA cohort fixed effects, gender, a cubic of their total SEA score, and fixed effects for the actual school attended (column 3 top panel) yields a within-school coefficient of 0.14 (se = 0.067). Controlling for choices (column 4) yields a very similar within-school estimates of 0.135 (se = 0.072). These OLS results indicate that a student would pass about 0.028 more exams than an observationally similar student who attends the same school when peer achievement was 0.2 standard deviations lower. The ratio of interest, the preferred across-school OLS coefficient divided by the preferred within-school OLS coefficient is a statistically significant 0.114 (se = 0.058) — suggesting that 11% of school selectivity effect can be attributed directly to peer quality differences across schools on average.[15]

The IV estimates are about half the size of the OLS estimates, suggesting that there is a positive selection on unobservables to high-achieving peers. The IV estimates are not statistically significant, and are about 0.072. The first stage *F*-statistics are both above 100. Taken literally, the point estimates suggest that after taking self-selection bias into account, a student would pass about 0.014 more exams than an observationally similar student who attended the same school when peer achievement was 0.2 standard deviations lower. The ratio of the preferred within-school and across-school IV estimates is 0.062 (se = 0.120) — suggesting that on average, roughly 6.2% of school value-added can be attributed directly to peer quality differences across schools. The results in Table 4 would imply that most of the school selectivity effect on average is not due to contemporaneous exposure to higher achieving peers, so that much of the differences across schools may be scalable *on average*. However, these average effects may mask considerable heterogeneity by gender and the level of school selectivity.

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

7.3. Effects by gender

Recent findings indicate that girls are more likely to benefit from attending better schools than boys (Deming et al., 2010; Hastings et al., 2006). Also, using similar data, Jackson (2010) finds that the selectivity effect is larger for girls than for boys. A psychology literature suggests that females may be more responsive to peers than males (Cross and Madson, 1997; Eagly, 1978; Maccoby and Jacklin, 1974). As such, a differential gender response to schools might be due to a differential gender response to peers. Indeed recent papers find that while females benefit from exposure to higher achieving peers, males may not (e.g. Han and Li, 2009; Lavy et al., 2010).

To test for whether gender differences in response to peers can explain gender differences in response to schools, I present the preferred specifications with the inclusion of the interaction between being female and peer quality in Table 5. As such, the coefficient on mean peer scores is the effect for males, while the coefficient on the interaction between female and mean peer scores is the difference in the marginal effect for males and females. Columns 1 and 2 present the across school estimates. In the OLS model, both males and females benefit from attending schools with higher achieving peers and females benefit more than males. In the OLS model in column 1, the coefficient on peer scores is 0.931 (se = 0.069) and that for the interaction between peer scores and female is 0.579 (se = 0.068). The IV results tell a similar story. The point estimates in the preferred IV model (with preference and score group fixed effects) in column 2 suggest that males and females who attend a school with 0.2 standard deviations higher peer test scores will pass 0.2 and 0.27 more exams, respectively. The effect for females is 38% larger than that for males, and the difference is statistically significant at the 1% level.

Columns 3 and 4 present the within school estimates of the direct effect of peers by gender. The OLS results suggest no direct peer effect for males and large peer effects for females. In the within-school OLS model (column 3), the coefficient on peer scores is −0.041 (se = 0.079) and that for the interaction between peer scores and female is 0.358 (se = 0.073). This suggests that females who attend a school during a time when peer test scores are 0.2 standard deviations higher would pass 0.064 more exams while males would be unaffected. The within school IV results (column 4) tell a similar story to the within school OLS results, but are noisier. The coefficient on mean peer quality is

Table 4
Main results.

	Effects on the number of exams passed: full sample				
	1	2	3	4	Ratio[a]
	Cross sectional results		Within School results		
Actual (OLS)	1.044	1.229	0.14	0.135	0.11
	[0.082]**	[0.073]**	[0.067]*	[0.072]*	[0.055]*
Assigned (RF)	0.437	0.57	0.039	0.038	0.067
	[0.062]**	[0.057]**	[0.058]	[0.079]	[0.067]
Actual (2SLS)	0.851	1.17	0.07	0.072	0.062
	[0.124]**	[0.094]**	[0.105]	[0.144]	[0.120]
First stage F-statistic	578.48	390.3	666.88	513.023	
Cohort fixed effect?	Yes	Yes	Yes	Yes	–
Preference effects? [d]		Yes		Yes	–
School fixed effects?			Yes	Yes	–
Observations	150,701[e]	150,695	150,701	150,695	–

Robust standard errors in brackets. Standard errors are adjusted for clustering at the assigned school level.
[a] The estimate of β/γ — the coefficient in column 7 divided by the coefficient in column 3. The standard error was computed by stacking the data to estimate both the within and across model simultaneously and then using the delta method.
+ Significant at 10%.
** Significant at 1%.
* Significant at 5%.
[b] Note that preferences include gender so that all models with preference fixed effect are within both preference and gender.

negative and not statistically significantly different from zero, while the coefficient on the interaction with female is positive and statistically significant at the 5% level. The IV coefficient on peer scores is −0.067 (se = 0.138) and that for the interaction between peer scores and female is 0.265 (se = 0.134). While the direct effects are imprecisely estimated, they suggest that females who attend a school during a period when peer test scores are 0.2 standard deviations higher would pass 0.039 more exams while males would pass 0.0134 *fewer* exams.

The similarity between the gender differences across the cross-section and the within-school models is notable. In the instrumental variable models, the gender difference in the school effect is 0.365 while that for the direct effect of peers is similar at 0.286. In fact, one cannot reject the null hypothesis that peer effects account for all of the gender gap in response to schools at the 10% level.[16] The pattern of results suggest that much of the explanation for gender differences in response to schools (in this and other studies) has to do with females responding differently to their peers. 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 may explain all of the differential response to schools — compelling evidence that between 6.67 and 6.67 + 14.4 = 21.1% of the school effect can be directly attributable to peers.[17]

Table 5
Testing for gender differences.

Effects on the number of exams passed: effects by gender	Across schools		Within schools	
	OLS	2SLS	OLS	2SLS
	1	2	3	4
Peer SEA scores	0.931	1.005	−0.041	−0.067
	[0.069]**	[0.101]**	[0.079]	[0.138]
Female ∗ peer SEA scores	0.579	0.365	0.358	0.265
	[0.068]**	[0.121]**	[0.073]**	[0.134]*
Female effect	1.509	1.37	0.317	0.198
[se]	[0.086]**	[0.116]**	[0.081]**	[0.128]
School fixed effects?	–	–	Yes	Yes
Observations	150,695	150,695	150,695	150,695

Robust standard errors in brackets. Standard errors are adjusted for clustering at the assigned school level.
All models include cohort fixed effects, choice fixed effects, and the third order polynomial in total SEA scores.
+ Significant at 10%.
* Significant at 5%.
** Significant at 1%.

7.4. Looking among similar schools

Because the decomposition for schools *on average* may mask considerable heterogeneity by school selectivity, and the first order Taylor expansion is most accurate for small changes in peer quality, I move to a more flexible model that will be valid even in the presence of non-trivial non-linearities and complementarities in inputs.[18] I do this in two ways. First I present the instrumental variable results broken up by subsamples of similar schools by the level of peer incoming achievement. The second approach is to present flexible semi-parametric reduced form estimates of the effects of being assigned to schools with higher-achieving peers and the effects of increases in assigned peer achievement within assigned schools over time. These approaches allow one to see if peer quality plays a more important role for certain schools than others, and if the global estimates pertain to all schools.

Because peer quality and input quality are both higher at high-achieving schools, non-linearity in the across-school effect could be due to (a) peer effects being non-linear, (b) the effect of other inputs being non-linear, or (c) complementarity of peer inputs and other inputs. In contrast, non-linearity in the within-school effects will reflect only (a) and (c). This implies that similarities in the non-linearity in across-school models and within-school models can provide further evidence of the importance of peers in explaining school effects. That is, if the across-school effects of $\gamma \approx (f_P + f_I \cdot g_P)$ are largest among schools for which the within-school effects $\beta \approx f_P$ are largest and vice versa, it would imply that the direct peer effects are an important component of the school selectivity effect. I show 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 the linear peer effect estimates for different subsamples of schools based on rank (while controlling for gender, choices, and SEA score). Both the RF and IV estimates suggest that attending a school with marginally higher-achieving peers has a larger positive effect among schools with high-achieving peers. The IV across school coefficient on mean peer achievement is 2.526 for the top 30 schools, 0.826 for schools ranked 31 through 90, and 0.542 for the bottom 68 schools (all effects are significant at the 1% level).

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 and control for choices, gender and SEA score.[19] The IV within-school coefficient on mean peer achievement is 1.959 for the top 30 schools (p-value = 0.015), and is statistically insignificant and small for schools ranked below 30. 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 most selective schools.[20] The fact that the non-linearity in the school effects track closely the non-linearity in the direct peer effect suggests that among the top 30 schools, some of the increased value-added can be attributed to the direct contribution of peers on outcomes. Consistent with this interpretation, based on the IV results, $\beta/\gamma \approx f_P/(f_P + f_I \cdot g_P)$, the fraction of the across school effect explained by direct peer influences is 0.78 (p-value = 0.03) in the top 30 schools, and is not statistically distinguishable from zero at other schools. While this point estimate is large, the lower bound of the 95% confidence interval is 0.14.

To ensure that these effects are driven by heterogeneity by school selectivity and not by heterogeneity by student achievement, I estimate the within-school model among students with different levels of incoming test scores (pooling all schools). Because there is sizable variation in student ability by school rank (as shown in Appendix Fig. 2), there is a sizable overlap in the distributions of students ability across school selectivity levels.[21] This allows one to possibly separate the effects of larger peer effects at more selective schools from larger peer effects for higher achieving students. 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

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

[17] These numbers are calculated from the fraction of the school effect attributable to peers by gender from Table 4.

[18] Because linearity and additive separability are approximated *locally* by the first order terms of a Taylor expansion of a continuously differentiable function, one can represent global non-linearity and non-separability by piecewise linear functions applied to different regions of the data.

[19] 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.

[20] 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.

[21] Indeed a decomposition reveals that 66% of the variation in total SEA scores occurs across schools. Of the remaining 34% of the variation in SEA scores, roughly have is attributable to cohort level variation within schools and the other half is due to heterogeneity in the student body within a school in a given year.

Table 6
Effect by school selectivity.

	Dependent variable is the number of exams passed						
	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
Across school variation							
Reduced form	2.701	0.453	0.313	1.308	0.466	0.228	0.028
	[0.366]**	[0.081]**	[0.075]**	[0.142]**	[0.075]**	[0.088]**	[0.032]
Mean peer scores (2SLS)	2.526	0.826	0.543	2.457	1.046	0.526	0.079
	[0.323]**	[0.161]**	[0.168]**	[0.262]**	[0.171]**	[0.183]**	[0.058]
Within school variation							
Reduced form	1.186	−0.0165	0.047	<0.001	−0.092	−0.164	−0.06
	[0.563]*	[0.099]	[0.118]	[0.245]	[0.162]	[0.121]	[0.243]
Mean peer scores (2SLS)	1.959	−0.163	0.078	0.084	−0.166	−0.329	−0.042
	[0.797]*	[0.184]	[0.215]	[0.490]	[0.292]	[0.232]	[0.087]
Observations	17,811	84,740	48,144	26,454	50,348	42,249	27,521
Ratio (2SLS)	0.78	−0.197	0.145	0.034	−0.16	−0.626	−0.537
[se]	[0.33]*	[0.225]	[0.399]	[0.200]	[0.280]	[0.492]	[1.17]

Robust standard errors in brackets. Standard errors are adjusted for clustering at the assigned school level.
All models include preference ordering fixed effects, and control for the total SEA score, its quadratic and its cubic, and gender.
+ Significant at 10%.
* Significant at 5%.
** Significant at 1%.

consistent with the results in 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 school selectivity effects, the left panel of Fig. 4 shows the local polynomial fit of the number of exams passed (after taking out the effects of own incoming test scores, choices and gender) on the mean assigned peer level of the assigned school (this is a semi-parametric representation of the reduced form). Among schools with mean peer quality between −2 and 0, there are small increases in the number of examinations passed associated with attending a marginally more selective school. However among schools with assigned peer achievement above 0 (i.e. above average schools), there are large marginal benefits to attending a school with higher-achieving peers — consistent with Table 6.

On the right panel of Fig. 4, to see whether peer effects are larger at more selective schools, I show the relationship between the contemporaneous peer effect at a school and the level of selectivity at that school. Specifically, I estimate the reduced form within-school model for each school β_j, and then fit a local polynomial of the estimated β_js to the mean assigned peer achievement of school j. If the marginal effect of contemporaneous peers (obtained using within school variation in cohort quality over time) is the same for all schools, there will be no relationship (a horizontal line). However, if peer effects are larger in schools with higher quality peers (non-linear peer effects) or schools with higher quality inputs (complementarity between input quality and peer quality), the depicted relationship will be positive. The marginal effect of within school increases in peer quality is highest among high-achievement schools — consistent with the regression evidence. Fig. 4 suggests that non-linearity in the selectivity effects are driven, in part, by non-linearity in the within-school effects — evidence that direct peer effects are responsible for much of the large selectivity effects among selective schools but do not explain the selectivity effects for middle- and low-achievement schools.

While the objective of this paper is to establish how much of the benefits to attending a selective school can be directly attributed to the quality of the peers at the school, it is helpful to discuss the policy implications behind the documented non-linear peer effects. Because peer quality and input quality are both higher at high-achieving schools, the non-linearity of the direct peer effects either reflects that marginal increases in peer quality within a school are more effective when peer achievement is already high, or that marginal increases in peer quality within a school are most effective when input quality is high. Because I do not observe input quality, I am unable to distinguish these two scenarios. This distinction does not affect the interpretation of the ratio β/γ, but it does have direct implications for how improvements in input quality (or peer quality) may increase school effectiveness. If the non-linearity in the within-school effect is driven by non-linearity in the marginal effect of peers, it would imply that school value-added can be increased by increasing input quality at all schools (and the distribution of inputs across schools would only have distributional effects). It would also imply that one could increase overall achievement by stratifying students across schools by ability. However, if the non-linearity in the direct peer effect reflects complementarity between peer quality and other inputs, it would imply that the marginal effect of improved inputs will be highest at schools with the highest achieving students. It would also imply that overall education output would be highest if high ability students attend schools with the best inputs. Despite clear policy implications, data limitations preclude rigorous investigation into the source of the non-linearity.

7.5. 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 the IV estimate is small and not statistically significant — indicating that most, if not all, of the cross-school effect was on the intensive margin, as found in Jackson (2010). Similarly, the within-school model of CSEC taking yields a very small statistically insignificant point estimate. 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 small and statistically insignificant — suggesting that much of the direct peer effect is on the intensive margin.

8. Robustness checks

While there are a priori reasons to believe that the results presented reflect true causal effects, there remain lingering concerns. I present these concerns and address them in turn.

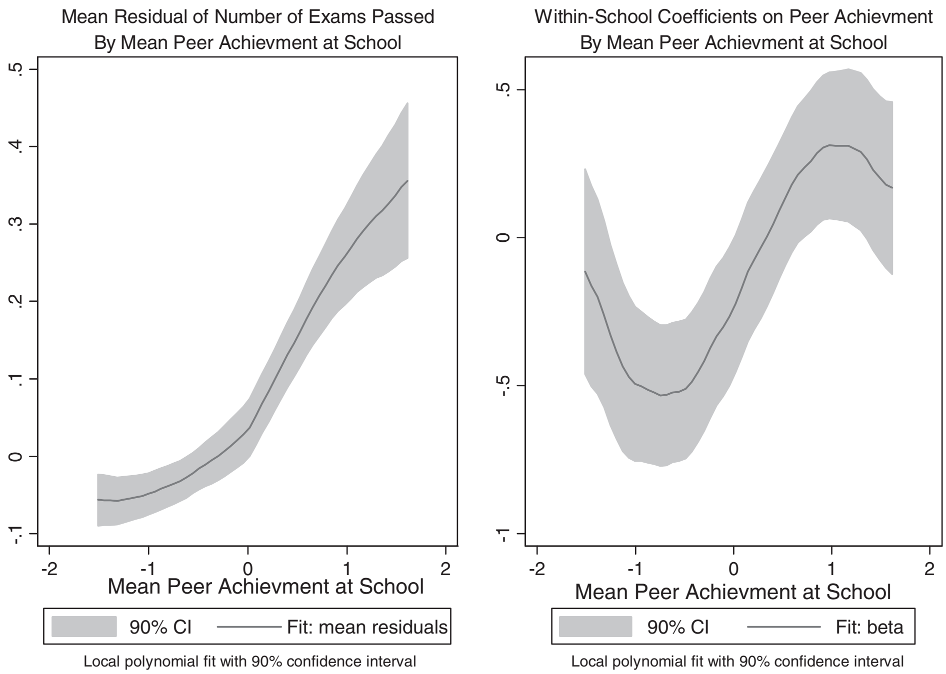


Fig. 4. Graphical evidence of non-linear peer effects.

8.1. 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 Jackson and Bruegmann (2009) and Lavy and Schlosser (2009) where I include the current peers (for which there should be a true treatment effect) and the peer quality of the preceding cohort and the following cohort (for which there should be little to no effect). The estimates that include current peers and peer quality in the following and preceding cohorts are presented in Appendix Table A1. To test for gender differences I show the coefficient on current peers and peer quality in the following and preceding cohorts interacted with whether the student is female. Under the null hypothesis of no real effect, the effect of peer quality in the preceding cohort and the following cohorts (which should be close to zero if the estimated effects are real) might be similar to those in the contemporaneous cohort (which should be non-zero if there is a real effect). Consistent with the effects being real one can reject the null hypothesis that contemporaneous peer quality has zero effect among the top 30 schools (column 1) and the null hypothesis that females and males have the same response to peers (column 4) at the 10% level. Also consistent with the effects being real, while the *p*-value on the contemporaneous effects for these two models is below 0.1, those for the joint significance of the lag and lead are above 0.7 for these two same models.[22]

8.2. The peer effects may be driven by sampling variation

In difference models there is always the concern that inference based on estimated effects could be biased by underlying serial correlation in the data. To assess this problem I follow an approach used by Bertrand et al. (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 is not lined up with the corresponding outcome for that year. I estimate the placebo treatments based on 100 replications of this reshuffling. I compare the actual estimates to the distribution of placebo estimates. Since the gender differences and the positive effect of peers among the top 30 schools are the estimates that are statistically significant, I test these two models. In both cases, none of the 100 replications yielded parameter estimates larger than the actual estimated coefficients, suggesting that the estimates obtained were not some artifact of the sample and would not have been obtained merely due to sampling variation.

8.3. Changes in peer quality within schools could be correlated with changes in input quality within schools

Because more desirable schools attract higher-achieving students and thus brighter peers, one may worry that improvements in input quality at a particular school in a particular year may cause students to rank that school more highly in their preference lists generating a correlation between changes in input quality and changes in peer quality within a school over time. 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. I can test for this possibility directly. To show that this is not a source of bias, I show that such changes in school rankings essentially do not occur in these data. Appendix Table A2 shows the correlation between a school's rank in simulated cut-off scores across years. The correlation between a school's rank across any two adjacent years in the data is at least 0.98 and the correlation between a school's rank in 1995 and seven years later in 2002 is 0.96 — so that systematic changes in school rankings are not driving the variation in assigned peer achievement within schools over time.

As a more direct test, I test for a correlation between changes in mean assigned peer achievement and teacher quality (one of the most important school inputs). Specifically, I run regressions of teacher characteristics on the mean SEA score of students assigned to the school

[22] If we take the conservative view that the effect on the lag reflects some underlying spurious association, we can subtract that from the contemporaneous effect to obtain a conservative estimate. Doing this for the female interaction results in a conservative reduced form peer effect estimate of 0.085. This is about two thirds of the reduced form estimate obtained in Table 4 — suggesting that there is a true gender difference. The same calculation for the top 30 schools yields a conservative reduced form current peer effect estimate of 0.99. This is about 70% of the within school reduced form coefficient in Table 5. This conservative estimate implies that 37% (versus 78%) of the across school effect among the top quartile of schools can be attributed to peers.

while including both cohort and school fixed effects. The results (Appendix Table A3) indicate no relationship between changes in peer quality and changes in teacher quality. Together, the results show that changes in peer quality within schools are due to idiosyncratic shocks around a long-run mean that are not correlated with changes in other school inputs within schools.

9. Conclusions

There is a growing body of evidence based on credible research designs indicating that attending selective schools may improve student outcomes. However, we have little understanding of *why*. Using a unique dataset from Trinidad and Tobago, I investigate the extent to which the positive selective school effects can be attributed to selective schools providing higher-achieving contemporaneous peers. Using 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 selectivity effects and direct peer effects *on the same student population*.

Using instrumental variable strategies, I find that attending a school with higher-achieving peers is associated with substantial improvements in academic outcomes. However, on average, improvements in incoming peer achievement within a school are associated with small improvements. The point estimates suggest that, on average, between 7 and 14% of the school effect can be directly attributed to peer quality differences across schools. Echoing other studies, the marginal effects of attending a school with higher achieving peers are larger for females than for males. I find that the gender differences in response to peers can account for all of the gender differences in response to schools — evidence that part of the school effect can be explained by the direct contribution of peers. I also find substantial non-linearity in the effects. Similar to Ding and Lehrer (2007) and Pop-Eleches and Urquiola (2008), the marginal effect of attending a more selective school is greatest among the most selective schools. Looking at the direct effect of peers, this non-linear school selectivity effect appears to be driven by the fact that the marginal effect of improvements in peer achievement within a school is largest at selective schools — further evidence that direct peer effects are responsible for some of the effect of attending schools with higher-achieving peers. The symmetry in the non-linearity leads me to conclude that while direct peer effects may explain little of the benefits to attending a more selective school among the bottom three-quarters of schools, at least one-third of the benefits to attending a more selective school among the top quarter of schools can be attributed directly to the achievement level of the peers.

The finding that at least one-third of the estimated school selectivity effect can be directly explained by peer achievement for the top quartile of schools is sobering because it implies that very little of the large estimated success at these selective schools can be scaled up to all schools. These findings underscore the fact that identifying highly successful schools may not be informative about how to improve outcomes for the average school. However, the finding that peer achievement explains almost none of the benefits to attending a more selective school among the bottom three-quarters of schools suggests that the relative successes at average schools may be scalable to low-performing schools.

Owing to the uniqueness of the institutional setup in Trinidad and Tobago, this paper is the first to rely on independent exogenous variation in schools attended *and* peer quality on the same student population — allowing one to credibly estimate the extent to which positive selective school effects can be attributed to selective schools providing higher-achieving contemporaneous peers. The findings highlight the importance of understanding the mechanisms through which selective schools may improve student outcomes.

Appendix A

Table A1
Placebo peer treatments.

Dependent variable is the number of exams passed	1	2	3		4
Sample:	Schools rank 1–30	Schools rank 31–90	Schools rank 91+		All
$Peers_{c-1}$	0.735	−0.06	−0.231	Female $Peers_{c-1}$	0.094
	[1.043]	[0.102]	[0.224]		[0.069]
Peers	1.733	−0.17	−0.223	Female Peers	0.1793
	[0.991]+	[0.112]	[0.142]		[0.010]+
$Peers_{c+1}$	0.109	0.027	0.319	Female $Peers_{c+1}$	0.057
	[0.713]	[0.092]	[0.200]		[0.135]
Current minus lag	0.997	−0.109	0.008		0.0853
Current minus lead	1.623	−0.143	−0.542		0.1223
p-Value on lag and lead	0.7	0.75	0.23		0.73
p-Value on current	0.08	0.13	0.14		0.09

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

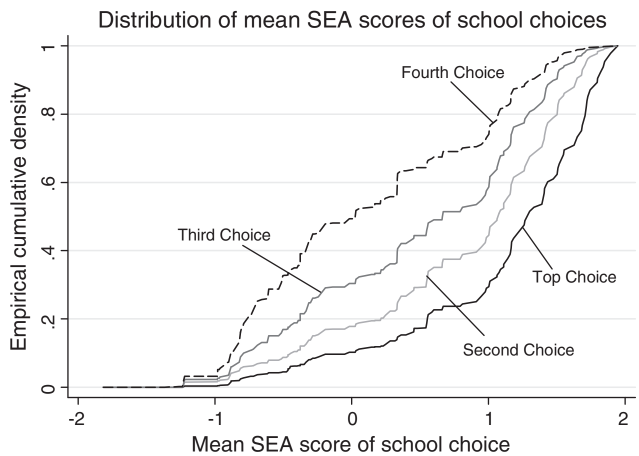
Table A2
Correlations between schools' ranks across years.

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

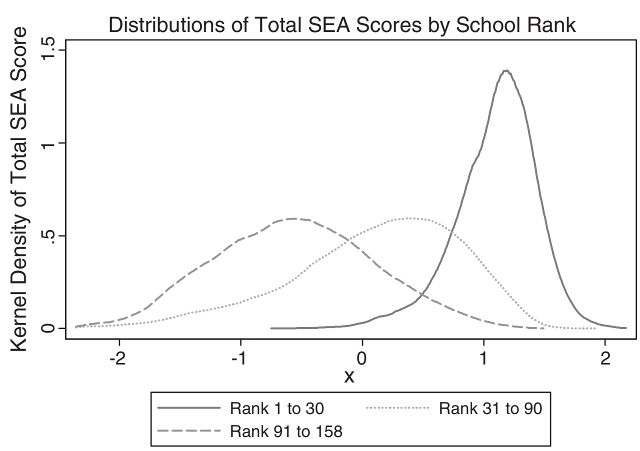
Table A3
Relationship between changes in peer quality and changes in teacher quality.

	1	2	3	4
	% teachers with 1–3 years experience	% teachers with 1–3 years experience	% teachers with 1–3 years experience	% teachers with a BA degree
Mean total SEA of assigned students	−0.01	−0.001	−0.08	0.004
	[0.022]	[0.041]	[0.059]	[0.097]
Observations	25,962	25,962	25,962	25,962

Robust standard errors in brackets are adjusted for clustering at the school level.
All models include assigned school fixed effects and cohort fixed effects.
+ Significant at 10%.
* Significant at 5%.
** Significant at 1%.



Appendix Fig. 1. Distribution of peer quality by school choice rank.



Appendix Fig. 2. Distribution of total SEA scores by school rank.

Appendix Note 1. Constructing the simulated cut-off

The simulated cut-offs are constructed sequentially as follows: (1) All secondary school sizes are fixed based on capacity, (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, 2010, 2012) 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 schools) that are not driven by any gaming or selection.

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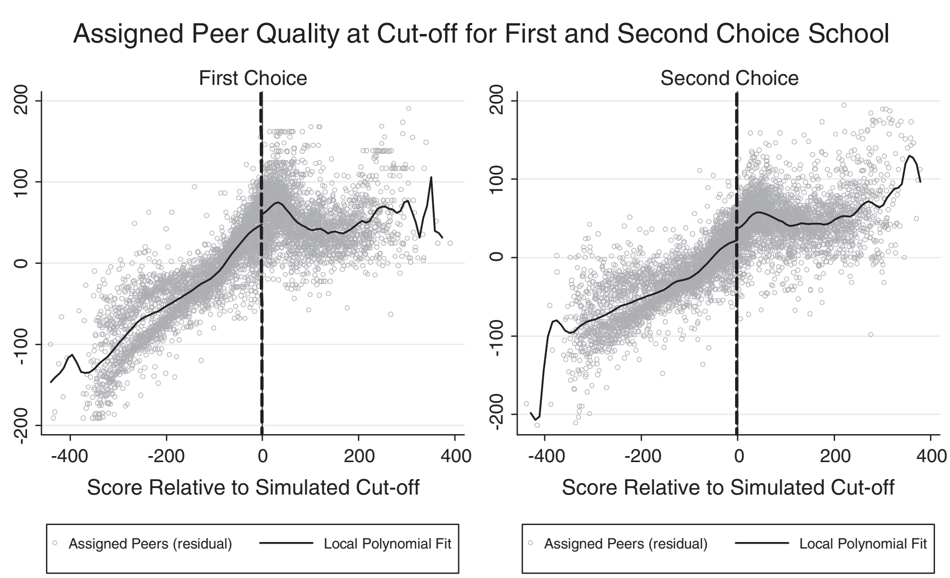

Appendix Fig. 3. Discontinuity in assigned peer quality (raw SEA scores are shown) through the assigned cut-off for the first and second choice school.

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