Appendix: NOT FOR PUBLICATION

Appendix A: School Placement Rules and Validity of the Discontinuity Variation

The School Assignment Algorithm

School slots are assigned in rounds such that the most highly subscribed/ranked school fills its spots in the first round, then the next highly subscribed school fills its slots in the second round, and so on until all school slots are filled. This is done as follows: (1) the number of school slots at each school n_j is predetermined based on capacity constraints. (2) Each student is tentatively placed in the applicant pool for her first choice school and is ranked by SEA score. (3) The school at which the n_j^{th} ranked student has the highest SEA score is determined to be the most highly subscribed/ranked school and the top n_{j1} students in the applicant pool for top-ranked school j_1 are assigned to school j_1. The SEA score of the n_{j1}^{th} student is the cutoff score for school j_1. (4) The top-ranked school slots and the assigned students are removed from the process, and the second choice becomes the new "first choice" for students who had the top-ranked school as their first choice but did not gain admission. (5) This process is repeated in round two to assign students to the second highest ranked school j_2 and determine the cutoff score for the second-ranked school, and this is repeated in subsequent rounds until all slots are filled. This assignment mechanism is a deferred acceptance algorithm (Gale and Shapley 1962) in which students have incentives to truthfully reveal their rankings among chosen schools.

However, there is an important exception to the school assignment algorithm-based rule. Specifically, Government assisted schools (which are privately managed public schools – akin to Charter schools in the US) can admit up to 20 percent of their incoming class at the principal's discretion. As such, the rule is used to admit at least 80 percent of the students at these schools, while the remaining students can be hand-picked by the principal before the next-highest ranked school fills any of its slots. For example, suppose the highest ranked school has 100 slots and is a Government assisted school. The top 80 applicants to that school will be admitted, while the principal can hand-pick up to 20 other students at her discretion. The remaining 20 students would be chosen based on for example family alumni connections, being relatives of teachers, religious affiliation, and so on. These hand-picked students may list the school as their top choice, but this need not be the case. Students receive one admission decision and are never made aware of other schools they would have been admitted to had they not been hand-picked. Only after all the spots (including both admitted students based on the algorithm and on the hand-picking) at the highest ranked school have been filled will the process be repeated for the remaining schools. As such, school admissions are based partly on the described deterministic function of student test scores and student choices and partly on the endogenous selection of students by school principals at Government assisted schools.

In addition, there are other circumstances by which the attended school would differ from the algorithm-based assigned school. First, students who do not score high enough to be assigned to a school on their choice list receive an administrative placement from the Ministry of Education (made to the administrative school zoned to the students' residential location). Finally, due to unforeseen circumstances some schools may have less capacity than expected or may close (this may happen due to flooding etc.). In such rare cases, the Ministry will place students to schools based on open slots in nearby schools, open slots in other schools in the choice list, and proximity.

Simulating the School Assignments Using the Algorithm-Based Rule

Because the assignment algorithm is known and we have the same data used by the Ministry of Education to tentatively assign students, we can identify the algorithm-based assignment cutoffs and, therefore, the algorithm-based school assignments (i.e. those that would have been the actual school allocations if Government assisted schools could not select any of their own students). This algorithm-based or tentative assignment removes the part of the actual admission process that may be driven by endogenous selection and leaves only the variation in the assignments that are known deterministic functions of students' test scores and school choices.

Following Jackson (2010) and Pop-Eleches and Urquiola (2013), we stack the data across all application pools for each year to each school (that is, we stack data for all the cutoffs into a single cutoff) into one single database. As such, we stack all application cutoffs and re-center the SEA scores for applicants to each school in each year around the algorithm-based assignment cutoff for that school-year.[1] Scoring above zero means scoring above the cutoff for a preferred school. Figure A1 shows the relationship between actually *attending* to one's preferred school as a function of one's incoming test score relative to the assignment cutoff for that school.[2] Consistent with our assignment cutoffs capturing real exogenous variation in actual school attendance, there is a sudden increase in the likelihood of attending a preferred school as one's score goes from below to above the assignment cutoff. Table A1 reports this first-stage estimated coefficient showing its high significance. This shows that there are meaningful differences in preferred school attendance associated with scoring above versus below an assignment cutoff that are not due to selection or hand-picking. Next, we provide direct supporting evidence on the exogeneity of the algorithm-based assignment cutoffs.

Testing the Exogeneity of the Assignment Cutoffs

The RD variation used in this paper is driven by the assignment cutoffs. As such, here we present evidence that this identification strategy is likely valid. One key diagnostic is to test for smoothness of density across the simuled cutoffs (McCrary 2008). As such, we formally test for any differential density across simulated cutoffs within each of our SEA cohorts by regressing the density of observations at each relative SEA score on an indicator for scoring above the cutoff along with smooth functions of the relative score.[3] We estimate this for each school cutoff in each year and report the average *t*-statistic associated with the null hypothesis of no differential density for each cohort along with the proportion of cutoffs that yield p-values smaller than 0.1. As one can see in Table A2, these tests reveal no statistically significant relationship between scoring above the cutoff and the density. Therefore, there is little evidence of gaming around the cutoffs regarding the density of observations at each test score.

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[1]Specifically, for each school we find all students who list that school as their top choice, re-center those students' SEA scores around the simulated cutoff for that school, and create a sample of applicants for each school. To mimic the sequential nature of the algorithm, we remove students assigned to their top choice schools, replace students' first choice with their second choice, and repeat this process with their second, third, fourth, fifth, and sixth choices. The applicant samples for all schools are then stacked so that every student has one observation for each school for which she/he was an applicant. We use four or six choices, as relevant per cohort limit. Only for SEA cohorts 2001-2006 students were allowed to list up to 6 school choices. Therefore, most of SEA cohorts in our data (1995-2000 and 2007-2012) could list up to 4 school choices.

[2]We consider that one student attended school j if the student was enrolled in school j at the time of writing the CSEC examinations.

[3]We implement these tests using the *rddensity* command in Stata.

The validity of the identification strategy also requires that there be no sorting of students around the cutoff (i.e. that latent outcomes are smooth through the cutoff). Given that students are unaware of the location of the cutoffs and are forced to make school choices before they take the SEA examinations, it is very unlikely that there is any sorting around the test score cutoffs. However, to provide further evidence that the variation employed (due to the cutoffs) is valid, we compute predicted outcomes (using the available baseline information) and test for whether scoring above the assignment cutoff is associated with any significant change in predicted outcomes.

Specifically, we first regress our outcomes on the number of SEA attempts (repeater status in 5th grade), the student's sex, the student's religion, selectivity of the student's primary school (measured by the average SEA scores of each primary school-year), selectivity of the student's secondary school choices (measured by the average SEA scores of the incoming class to each school choice-year), month of birth (to measure quarter of birth effects), age at SEA, and SEA cohorts fixed effects. These variables are relatively good predictors of the examination indexes such that, as shown in column 1 of Table A1, they yield adjusted R-squares ranging from 0.27 to 0.31. However, the predictive power for the nonacademic binary outcomes is low.

We then take the fitted values from these prediction regressions as our predicted outcomes. If there was some gaming of the cutoff, one would likely see that scoring above the cutoff (conditional on smooth functions of the relative SEA score) should be associated with better "predicted" scores. However, with no gaming there should be no relationship between scoring above the cutoff and one's predicted outcomes. To test for this, we estimate the following model using our stacked database:

$$Y_{ijt}^p = \pi \cdot Above_{i\tau t} + f(SEA_{it}) + F_{\tau t} + \varepsilon_{ijt} \tag{1}$$

where Y_{ijt}^p is the predicted outcome for individual i who attended school j at time t. $Above_{i\tau t}$ is an indicator for scoring above the algorithm-based assignment cutoff for school τ. Among those who comply with the cutoff, $j=\tau$. $f(SEA_{it})$ is a 5th order polynomial of the incoming SEA score net of the cutoff score for preferred school τ. $F_{\tau t}$ is a cutoff fixed effect for applicants to school τ in year t. The inclusion of cutoff fixed effects ensures that all comparisons are among students who applied to the same school in the same year. Because the same individual can enter the data for multiple cutoffs, the estimated standard errors are clustered at the individual level.

Consistent with no gaming, column 2 of Table A1, shows that there is no relationship between scoring above the cutoff and one's predicted outcomes. The estimated coefficients,$\hat{\pi}$, are small in magnitude and statistically indistinguishable from zero – indicating no gaming across the assignment cutoffs. As an additional check on this model, we estimated model (1) for different bandwidths around the cutoff. Figure A2 presents these results visually. As one can see for any choice of bandwidth, there are no effects of scoring above the cutoff on predicted outcomes. Taken together, the patterns suggest that the variation due to the algorithm-based assignment cutoffs is likely exogenous and, therefore, valid to identify causal school impacts.

Finally, Table A1 also reports the estimated RD effects on the actual outcomes (columns 4 -7) showing that reduced-form effects of scoring above the school assignment cutoff are associated, on average, with significant improvements in students' examination indexes and that these estimates are not sensitive to the inclusion of baseline sociodemographic controls in the model.

Reconciling Null Pooled Cutoff Impacts with Other Results

One may wonder how scoring above a cut-off for a preferred school on average could be unrelated to outcomes while there are real differences in value-added across schools. This could occur if (a) school choices are unrelated to value-added, (b) school preferences are heterogeneous, or (c) there is little variability in value-added among choices. We discuss each below.

- (a) Consider the hypothetical scenario where school choices are entirely random. In this hypothetical scenario, preferred schools are not higher value-added on average, and there would be no pooled cut-off effect even if there are real differences in value-added across schools. This hypothetical scenario highlights that there could be no aggregate cut-off effects if preferences for school (among those at the cut-offs) are not strongly related to value-added in all dimensions. This example is not meant to explain why the results differ between Table 2 and Table A2, but rather to underscore the fact that a lack of an aggregate cut-off effect does not imply a lack of school effects.

- (b) A more plausible explanation for small cut-off effects despite there being real differences in value-added is that preferences for schools are heterogeneous. That is, some people may rank high value-added schools more highly, while others do not so that on average the pooled threshold effect is small. Indeed, we document clear differences in the choice behaviors of parents of children at the top versus the bottom of the incoming test score distribution so that this is not unreasonable.

- (c) Another possibility is that many parents may choose schools that are very similar in value-added along those dimensions. For example, a parent who wishes to have their child attend a school that reduces crime may list four schools that are all high value-added in that dimension. In such a case, there are real differences in school value-added, choices are heavily influenced by school value-added, but there would be no effect on value-added through an admission threshold. This scenario may hold (or approximately hold) for many parents in the data such that many individual cut-offs end up having very low signal-to-noise ratios.

A related question (in light of scenario (a) above) is whether the lack of an aggregate cut-off effect implies that our choice models must be wrong (i.e., that parents do choose schools with impacts on non-academic outcomes conditional on proximity, admission probability, peer characteristics, and average outcomes). We argue that it does not for three reasons.

1. As we discuss above, the pooled average results may conceal considerable heterogeneity and could be explained by the heterogeneity in preferences for schools that are suggested by our choice patterns (scenario b above).

2. Also, given scenario (c) above, if parents have strong preferences for an attribute, the schools listed may all have similar levels of value-added for that attribute. In this scenario, that attribute will strongly predict being listed as a top choice school, but there may be little difference in value-added among the choices made (and thus no cut-off effect in the RD model).

4

3. It is important to point out that our choice models show relationships that are conditional on peer quality, proximity, school-level averages and include school impacts in multiple dimensions. As such, it is possible that the preferred school is not a higher value-added school unconditionally (which is what Table 2 will speak to), but is only higher value-added conditional on all the other predictors of choices. To give a hypothetical example, suppose John values proximity and arrests value-added. In the choice models with both included, one would find that John is more likely to rank schools with high proximity and high value-added. However, if for some reason, John lives far away from the high value-added schools and has a heavy weight on proximity, there may be little difference in the arrests value-added of John's choices (and he may even list a lower value-added school above a higher one). This example is merely meant to illustrate that because the choice models represent conditional relationships, the patterns we document are not directly relatable to the pooled cut-off effects.

Table A1. First Stage and Reduced-Form Effects

|	Predicted Outcomes			Actual Outcomes			
	Prediction R2	Effect	*p-value*	Effect	*p-value*	Effect	*p-value*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
First Stage:							
Attended preferred school				0.200	<0.001	0.200	<0.001
Reduced-Form Effects:							
High-Stakes Index	0.31	-0.003	0.173	0.062	<0.001	0.062	<0.001
Low-Stakes Index	0.27	0.000	0.968	0.048	<0.001	0.050	<0.001
No Dropout by 14	0.10	-0.001	0.095	-0.004	0.08	-0.004	0.07
No live birth by 19	0.03	0.000	0.578	-0.001	0.86	-0.001	0.79
Not arrested by 18	0.04	0.000	0.634	0.000	0.83	0.000	0.86
Formally employed 27+	0.02	-0.001	0.173	-0.004	0.38	-0.004	0.36
Cutoff fixed effects		Yes		Yes		Yes	
Sociodemographics		No		No		Yes	

Notes: This table reports estimated coefficients on 'Above' from equation (1). Models were estimated using observations within a bandwidth of +/-1.25 standard deviations from the school assignment cutoff. Sociodemographics include sex, primary school district fixed effects, and religion fixed effects. Estimated standard errors are clustered at the individual level in all regressions. *P-values* for the null of $\pi=0$ shown next to the estimated coefficients.

Table A2. Testing for differential density around the school assignment cutoff

| SEA Cohort	*p-value* < .1	*mean* T-stat	SEA Cohort	*p-value* < .1	*mean* T-stat
1995	9.46%	-0.04	2004	12.77%	-0.19
1996	10.81%	-0.02	2005	7.69%	0.03
1997	16.22%	-0.16	2006	11.46%	-0.02
1998	10.53%	-0.09	2007	11.70%	-0.02
1999	17.11%	0.11	2008	9.89%	0.22
2000	11.11%	0.06	2009	5.43%	-0.04
2001	11.58%	-0.05	2010	11.39%	-0.08
2002	13.68%	0.18	2011	8.86%	-0.01
2003	15.96%	0.21	2012	15.79%	0.21

Notes: This table shows the percentage of school assignment cutoffs for which the *p-value* corresponding to the test of differential density across the cutoff is less than 10% in each SEA cohort. For all cohorts together it is 11.69%. The table also reports the average of *T-Statistics* of differential density tests of all school assignment cutoffs for each SEA cohort included in the study.

Figure A1. Discontinuity in Preferred School Attendance Through Assignment Cutoffs
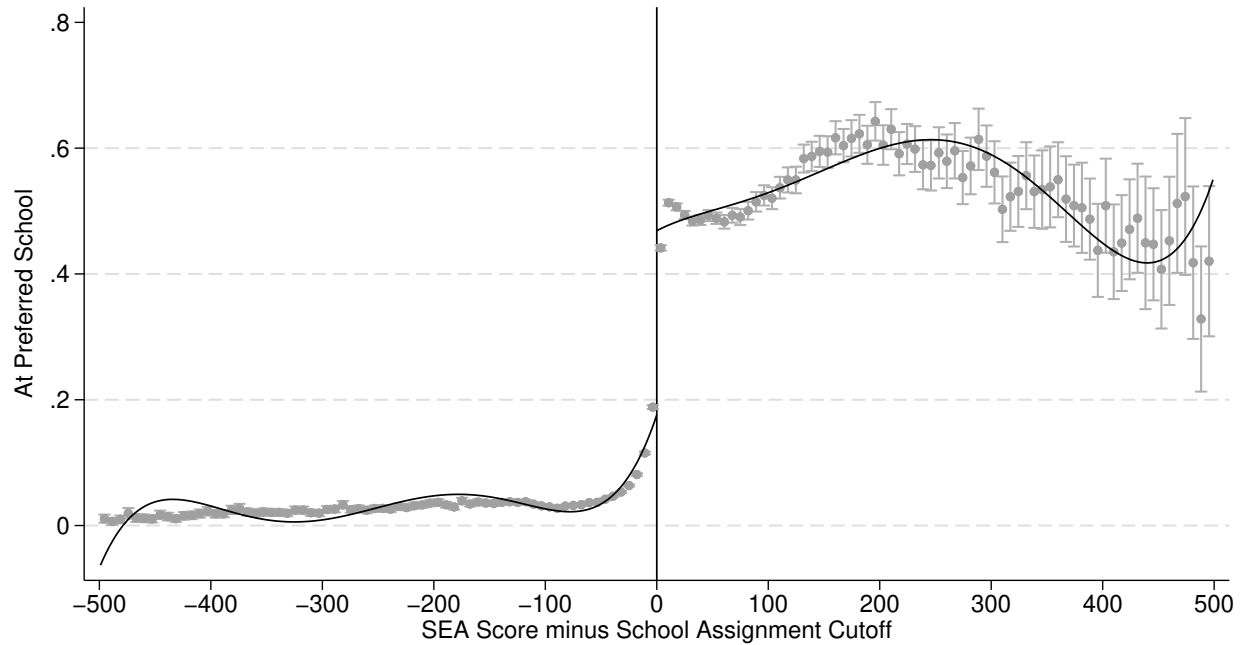
Notes: The Y-axis represents the likelihood of preferred school attendance (i.e. the school where the student was enrolled at the time of taking the CSEC examinations). The X-axis is the SEA score relative to the deferred acceptance rule-based assignment cutoff. The circles are means corresponding to 7-point bins of the relative score. The solid lines are the fitted school attendance rates generated by fitting a fifth degree polynomial of the relative score fully interacted with an indicator for scoring above the school assignment cutoff. The gray vertical bars depict the 90 percent confidence intervals for each bin average.

Figure A2. Reduced-form effects on predicted outcomes by bandwidth
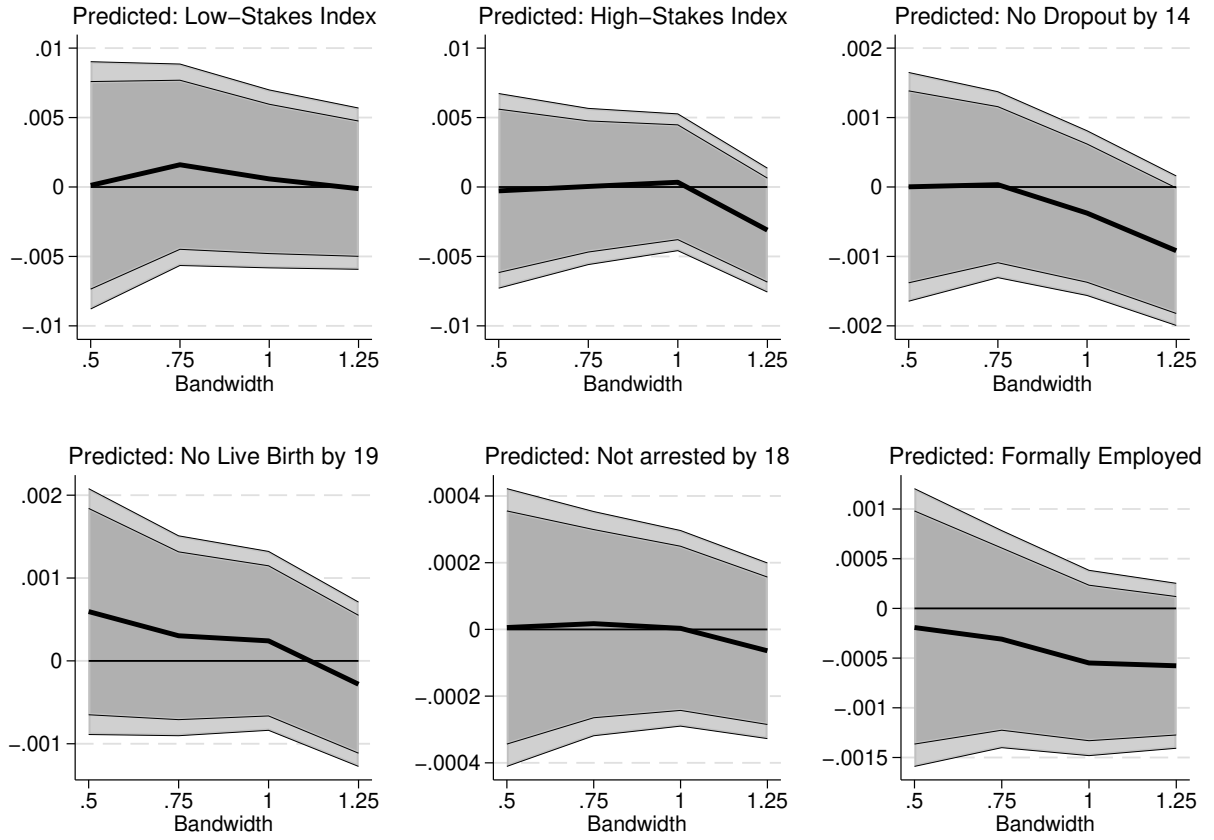
Notes: This figure reports estimated coefficients on 'Above' resulting from equation (1). The estimated coefficients are reported for each bandwidth between +/-0.5sd an +/-1.25sd from the school assignment cutoff. The 90 (95) percent confidence intervals of the estimated coefficients are presented in dark (light) gray.

Appendix B: Additional Appendix Tables and Figures

Table B1. Minimum and maximum cutoffs by district

| District | Cutoff by SEA percentile | |
| | Minimum | Maximum |
	(1)	(2)
Caroni	4	97
North Eastern	1	82
Port of Spain	7	97
South Eastern	1	91
St. George East	1	98
St. Patrick	1	92
Tobago	3	77
Victoria	1	98

Notes: We show the minimum and maximum cutoffs as SEA percentiles for each district across all years.

Table B2: Weights Used to Compute Indexes

High-Stakes Index	Weight
Number of CSEC subjects passed	0.202
CSEC tertiary qualification	0.192
CSEC tertiary qualification attempt	0.140
CAPE scholarship	0.068
CAPE scholarship attempt	0.213
Number of CAPE units passed	0.219
CAPE Associate's degree	0.213

Low-Stakes Index	Weight
NCSE Total Academic	0.546
NCSE Total Non academic	0.546

Notes: Indexes are computed from a separate factor analysis (using the principal-component factor method) applied to the individual outcomes that integrate each index. The weights for individual outcomes within the indexes are determined by predicting the first underlying principal-component applied separately to each group of outcomes that integrate each index. The computed indexes are standardized to have zero mean and unit variance. CSEC tertiary qualification is obtained when passing 5 subjects including English language and mathematics. "CSEC tertiary qualification attempt" denotes that the student took 5 subjects including English language and mathematics. CAPE scholarship is awarded when passing eight CAPE units (including Caribbean and Communication studies) with the maximum possible grade. "CAPE scholarship attempt" denotes that the student took eight CAPE units (including Caribbean and Communication studies). CAPE associate's degree is awarded when passing seven CAPE units (including Caribbean and Communication studies). NCSE academic subjects include mathematics, English, Spanish, sciences, and social studies. NCSE non academic subjects include arts, physical education, and technical studies.

Table B3: Standard Deviation of Persistent School Impacts and Maximum Likelihood Correlations Between 2SLS School Impacts

	School Level ($\sigma_{\theta^{TOT}}$)				School Level (correlations)			
	Size of impacts for Students at the				Correlations with High-Stakes at the			
Outcome	Size of Impact	25th %ile of the achievement distribution	median of the achievement distribution	75th %ile of the achievement distribution	Correlation	25th %ile of the achievement distribution	median of the achievement distribution	75th %ile of the achievement distribution
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Standardized Outcomes								
High-Stakes Index								
All Schools	0.441	0.516	0.495	0.444	1.000	1.000	1.000	1.000
	[0.397 , 0.483]	[0.464 , 0.562]	[0.445 , 0.534]	[0.400 , 0.481]				
Dropping Outliers	0.441	0.516	0.495	0.444	1.000	1.000	1.000	1.000
	[0.397 , 0.483]	[0.464 , 0.562]	[0.445 , 0.534]	[0.400 , 0.481]				
Low-Stakes Index								
All Schools	0.493	0.864	0.814	0.441	0.091	0.102	0.160	0.000
	[0.452 , 0.530]	[0.715 , 0.962]	[0.759 , 0.861]	[0.389 , 0.481]	[-0.045 , 0.219]	[-0.028 , 0.253]	[0.037 , 0.257]	[-0.136 , 0.141]
Dropping Outliers	0.473	0.764	0.814	0.410	0.100	0.173	0.173	0.037
	[0.433 , 0.508]	[0.683 , 0.826]	[0.760 , 0.859]	[0.365 , 0.448]	[-0.042 , 0.224]	[0.047 , 0.289]	[0.041 , 0.285]	[-0.110 , 0.167]
Low-Stakes Index (Academic)								
All Schools	0.390	0.699	0.608	0.324	0.196	0.184	0.147	0.207
	[0.360 , 0.414]	[0.637 , 0.754]	[0.566 , 0.646]	[0.283 , 0.357]	[0.076 , 0.312]	[0.047 , 0.292]	[0.020 , 0.260]	[0.086 , 0.337]
Dropping Outliers	0.390	0.699	0.608	0.324	0.196	0.184	0.147	0.207
	[0.360 , 0.414]	[0.637 , 0.754]	[0.566 , 0.646]	[0.283 , 0.357]	[0.076 , 0.312]	[0.047 , 0.292]	[0.020 , 0.260]	[0.086 , 0.337]
Low-Stakes Index (Non Academic)								
All Schools	0.618	0.950	0.971	0.600	0.011	0.068	0.151	-0.098
	[0.560 , 0.664]	[0.778 , 1.055]	[0.898 , 1.032]	[0.538 , 0.653]	[-0.141 , 0.145]	[-0.067 , 0.214]	[0.035 , 0.275]	[-0.248 , 0.037]
Dropping Outliers	0.607	0.831	0.970	0.597	0.024	0.132	0.166	-0.087
	[0.549 , 0.657]	[0.743 , 0.903]	[0.898 , 1.036]	[0.542 , 0.650]	[-0.129 , 0.159]	[-0.014 , 0.262]	[0.046 , 0.287]	[-0.229 , 0.047]

Notes: All estimates shown were computed by bootstrap with 1,000 repetitions of the maximum likelihood approach described in the text. We report the median as the point estimate, as well as the 5th and 95th percentiles for the confidence intervals. Columns (1) - (4) report estimated standard deviations of the persistent school impacts for each outcome; where school impacts have been estimated without weights (column 1), and with weights centered at the 25th, 50th and 75th percentile of the incoming achievement distribution (columns 2 to 4 respectively). Column (5) - (8) report estimated correlations of the persistent school impacts on the high-stakes index with the persistent school impacts on other outcomes; where school impacts have been estimated without weights (column 5), and with weights centered at the 25th, 50th and 75th percentile of the incoming achievement distribution (columns 6 to 8 respectively). We do this using $weight_i = (1 + \frac{(X - pct_i)^2}{100})^{-1}$, where $X = 25, 50, 75$ and pct_i is the student's percentile in the achievement distribution. For the first row of each outcome, we removed schools with outlier estimated impacts (i.e. beyond 4σ of the median school). Estimates reported in the second row of each outcome were obtained without removing outliers.

Table B3 (continued): Standard Deviation of Persistent School Impacts and Maximum Likelihood Correlations Between 2SLS School Impacts

	School Level ($\sigma_{\theta^{TOT}}$)				School Level (correlations)			
		Size of impacts for Students at the				Correlations with High-Stakes at the		
Outcome	Size of Impact	25th %ile of the achievement distribution	median of the achievement distribution	75th %ile of the achievement distribution	Correlation	25th %ile of the achievement distribution	median of the achievement distribution	75th %ile of the achievement distribution
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Binary Outcomes								
No Dropout by 14								
All Schools	0.148	0.197	0.135	0.170	0.117	0.002	0.102	0.238
	[0.091 , 0.162]	[0.175 , 0.215]	[0.089 , 0.146]	[0.074 , 0.183]	[-0.009 , 0.266]	[-0.166 , 0.135]	[-0.065 , 0.354]	[0.006 , 0.487]
Dropping Outliers	0.090	0.190	0.090	0.070	0.121	-0.012	0.238	0.117
	[0.077 , 0.100]	[0.171 , 0.206]	[0.081 , 0.098]	[0.058 , 0.080]	[0.019 , 0.228]	[-0.142 , 0.117]	[0.121 , 0.345]	[-0.044 , 0.290]
No live birth by 19								
All Schools	0.367	0.287	0.324	0.450	-0.022	0.030	-0.115	-0.079
	[0.295 , 0.414]	[0.254 , 0.311]	[0.200 , 0.369]	[0.339 , 0.527]	[-0.115 , 0.092]	[-0.110 , 0.151]	[-0.230 , -0.004]	[-0.260 , 0.099]
Dropping Outliers	0.173	0.287	0.202	0.131	-0.036	0.025	-0.064	0.174
	[0.147 , 0.193]	[0.254 , 0.313]	[0.165 , 0.232]	[0.107 , 0.150]	[-0.171 , 0.087]	[-0.124 , 0.161]	[-0.188 , 0.071]	[0.021 , 0.311]
Not arrested by 18								
All Schools	0.068	0.072	0.134	0.075	0.084	-0.099	-0.006	-0.085
	[0.041 , 0.077]	[0.056 , 0.083]	[0.051 , 0.150]	[0.033 , 0.083]	[0.004 , 0.262]	[-0.235 , 0.182]	[-0.112 , 0.199]	[-0.220 , 0.257]
Dropping Outliers	0.037	0.053	0.045	0.020	0.282	0.055	0.097	0.407
	[0.032 , 0.041]	[0.048 , 0.059]	[0.039 , 0.050]	[0.017 , 0.023]	[0.164 , 0.419]	[-0.059 , 0.205]	[-0.022 , 0.226]	[0.210 , 0.619]
Formally employed 27+								
All Schools	0.160	0.132	0.453	0.306	0.076	0.193	-0.429	-0.049
	[0.078 , 0.191]	[0.086 , 0.149]	[0.093 , 0.484]	[0.115 , 0.349]	[-0.014 , 0.233]	[0.091 , 0.298]	[-0.493 , 0.184]	[-0.245 , 0.308]
Dropping Outliers	0.070	0.083	0.086	0.090	0.152	0.165	0.106	0.014
	[0.058 , 0.079]	[0.065 , 0.098]	[0.072 , 0.097]	[0.076 , 0.103]	[0.025 , 0.294]	[0.046 , 0.297]	[-0.043 , 0.237]	[-0.148 , 0.157]

Notes: All estimates shown were computed by bootstrap with 1,000 repetitions of the maximum likelihood approach described in the text. We report the median as the point estimate, as well as the 5th and 95th percentiles for the confidence intervals. Columns (1) - (4) report estimated standard deviations of the persistent school impacts for each outcome; where school impacts have been estimated without weights (column 1), and with weights centered at the 25th, 50th and 75th percentile of the incoming achievement distribution (columns 2 to 4 respectively). Column (5) - (8) report estimated correlations of the persistent school impacts on the high-stakes index with the persistent school impacts on other outcomes; where school impacts have been estimated without weights (column 5), and with weights centered at the 25th, 50th and 75th percentile of the incoming achievement distribution (columns 6 to 8 respectively). We do this using $weight_i = (1 + \frac{(X - pct_i)^2}{100})^{-1}$, where $X = 25, 50, 75$ and pct_i is the student's percentile in the achievement distribution. For the first row of each outcome, we removed schools with outlier estimated impacts (i.e. beyond 4σ of the median school). Estimates reported in the second row of each outcome were obtained without removing outliers.

Table B4. Standard Deviation of Persistent School Impacts and even-odd year Correlations Between 2SLS School Impacts

Outcome	School Level ($\sigma_{\theta_j^{TOT}}$)	School Level correlations with high-stakes	
	Size of Impact	Average	75th %ile of the achievement distribution
	(1)	(2)	(3)
Standardized outcomes			
High-Stakes Index	0.432	1.000	1.000
	[0.373 , 0.488]		
Low-Stakes Index	0.454	0.105	0.073
	[0.406 , 0.505]	[-0.066 , 0.272]	[-0.146 , 0.271]
Binary outcomes			
No Dropout by 14	0.084	0.088	0.183
	[0.069 , 0.099]	[-0.050 , 0.228]	[-0.103 , 0.413]
No live birth by 19	0.149	-0.001	0.172
	[0.117 , 0.178]	[-0.180 , 0.163]	[-0.051 , 0.447]
Not arrested by 18	0.034	0.272	0.353
	[0.028 , 0.040]	[0.112 , 0.418]	[0.117 , 0.604]
Formally employed 27+	0.053	0.291	-0.013
	[0.035 , 0.069]	[0.055 , 0.536]	[-0.246 , 0.215]

Notes: The table reports standard deviations and correlations of persistent school effects across different outcomes. Correlations were computed comparing even and odd years as described in the text. To dissatenutate this raw correlation, one must divide by the square root of the product of the reliability ratios for each measure. Correlations in the second column were computed with estimated school impacts for the average student and those in the third column were computed using school impacts with weights centered around the 75th percentiles of the achievement distribution. We do this using $weight_i = (1 + \frac{(75-pct_i)^2}{100})^{-1}$, where pct_i is the student's percentile in the achievement distribution. We removed schools with outlier estimated impacts (i.e. beyond 4σ of the median school). We bootstrap the even-odd correlations approach for 1,000 repetitions. Results shown correspond to the median (for the point estimates) and the 5th and 95th percentiles for the confidence intervals.

Table B5. Maximum Likelihood Correlations Using Different Weights for 2SLS estimates

	Correlation between average school impacts and school impacts weighted around the		
	25th %ile of the achievement distribution	median of the achievement distribution	75th %ile of the achievement distribution
	(1)	(2)	(3)
High-Stakes Index	0.99	0.99	0.97
	[0.96 , 1.00]	[0.98 , 1.00]	[0.95 , 0.99]
Low-Stakes Index	0.90	0.91	0.91
	[0.86 , 0.94]	[0.88 , 0.94]	[0.86 , 0.94]
No Dropout by 14	0.95	0.86	0.52
	[0.86 , 1.00]	[0.80 , 0.93]	[0.34 , 0.65]
No live birth by 19	0.92	0.89	0.93
	[0.87 , 0.96]	[0.77 , 0.99]	[0.82 , 1.00]
Not arrested by 18	0.93	0.73	0.74
	[0.86 , 0.99]	[0.65 , 0.80]	[0.60 , 0.85]
Formally employed 27+	0.99	1.00	0.92
	[0.85 , 1.00]	[0.94 , 1.00]	[0.83 , 1.00]

Notes: The table reports correlations of persistent school effects for each outcome across the different weighted estimates. Correlations were computed using the maximum likelihood approach described in the text. We show correlations between estimated school effects for the average student and those weighted around the 25th percentile, the median, and the 75th percentile of the students' SEA score distribution. Weighted school impacts are estimated using $weight_i = (1 + \frac{(X - pct_i)^2}{100})^{-1}$, where $X = 25, 50, 75$ and pct_i is the student's percentile in the achievement distribution. We removed schools with outlier estimated impacts (i.e. beyond 4σ of the median school). We bootstrap the maximum likelihood approach for 1,000 repetitions. Results shown correspond to the median (for the point estimates) and the 5th and 95th percentiles for the confidence intervals.

Figure B1. Distribution of cutoffs by district
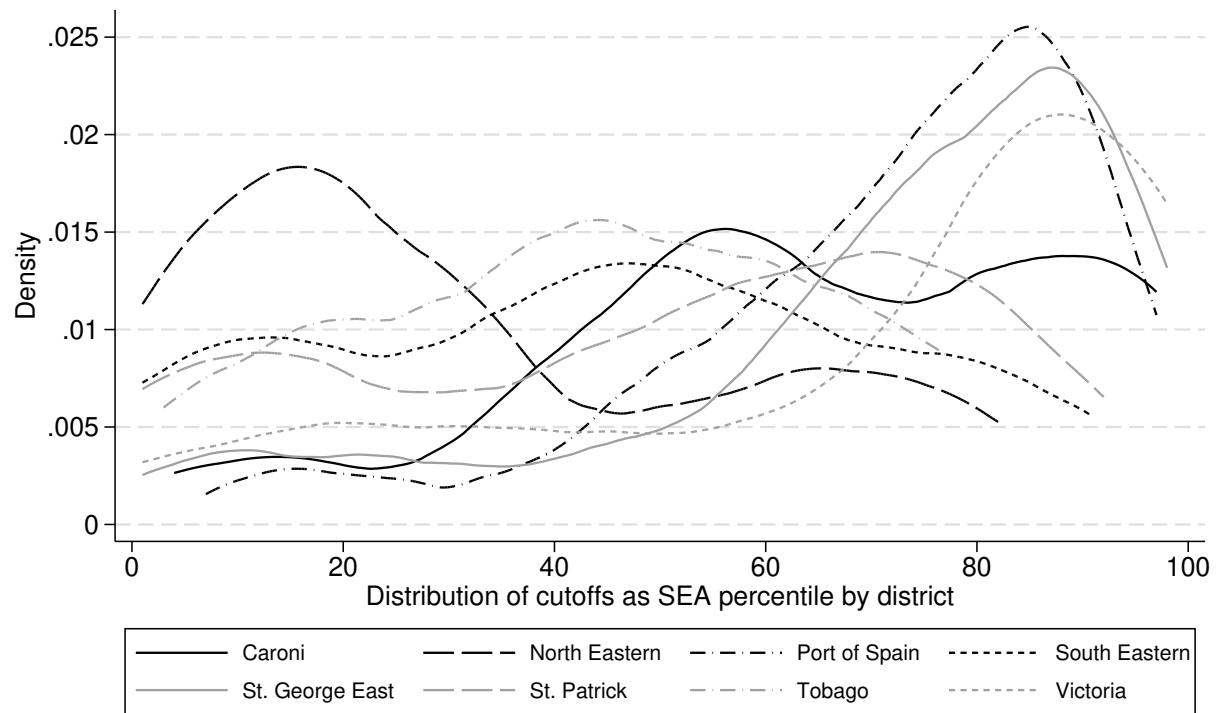
Notes: We show the distribution of cutoffs for all school-years by district. Cutoffs are shown as the SEA percentile they correspond to in each year.

Figure B2. Estimated School Impacts with alternative SEA polynomials
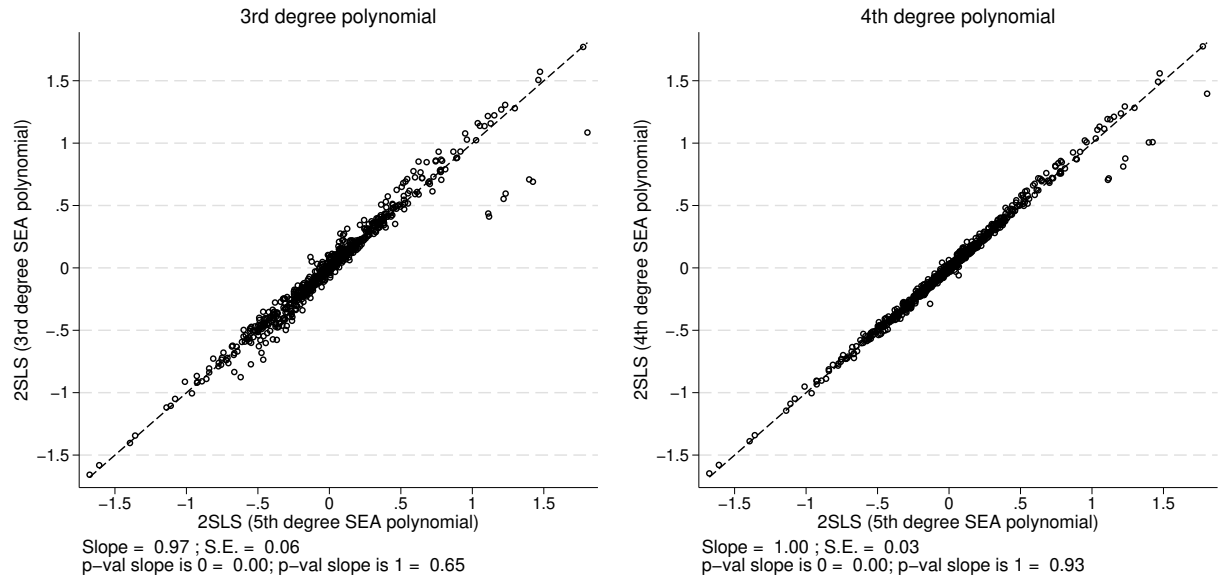
Notes: The left panel displays estimated 2SLS school impacts with a 3rd degree polynomial of the SEA score (Y-axis) against the preferred estimated school impacts with a 5th degree polynomial of the SEA score (X-axis). The right panel displays estimated 2SLS school impacts with a 4th degree polynomial of the SEA score (Y-axis) against the preferred estimated school impacts with a 5th degree polynomial of the SEA score (X-axis). Estimated slopes and tests for whether they differ from 0 and 1 are shown below each panel.

Figure B3. Scatter plots of the raw school effects
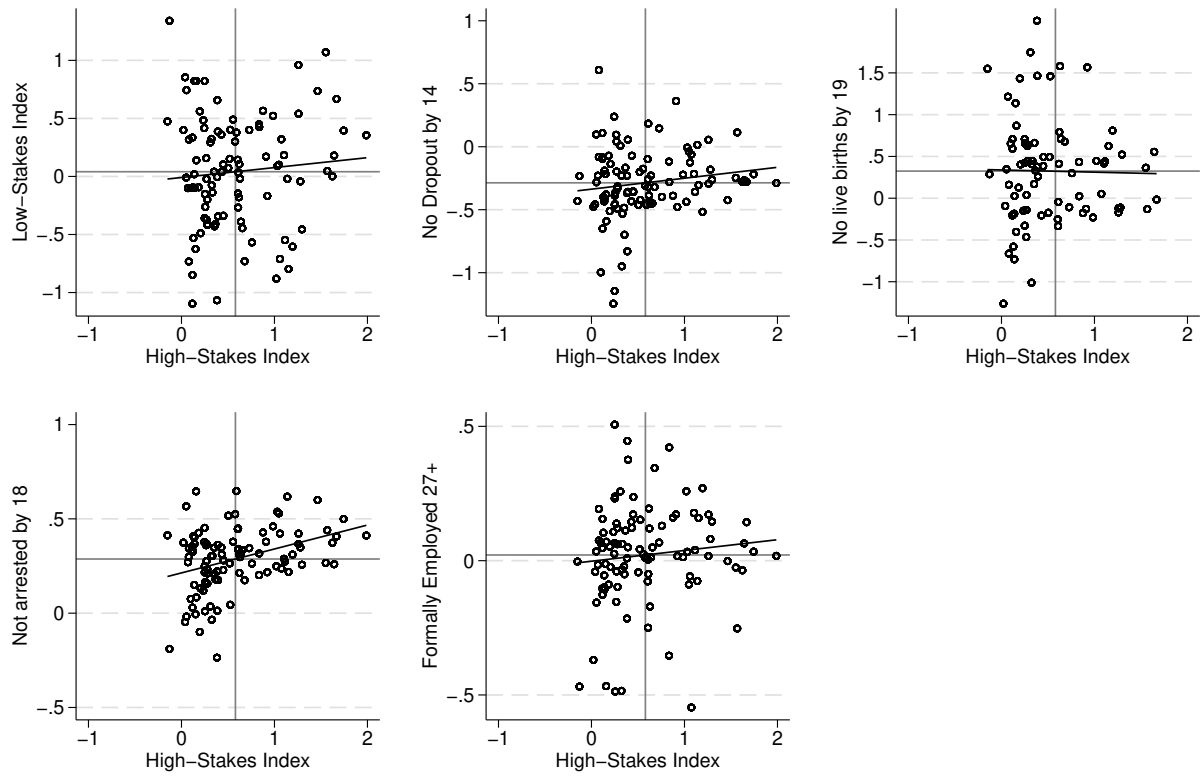
Notes: The X-axis plots the High-Stakes value-added for each school. The Y-axis represents the value-added for each school for the other outcomes.

Figure B4. Outcomes by SEA score ventile
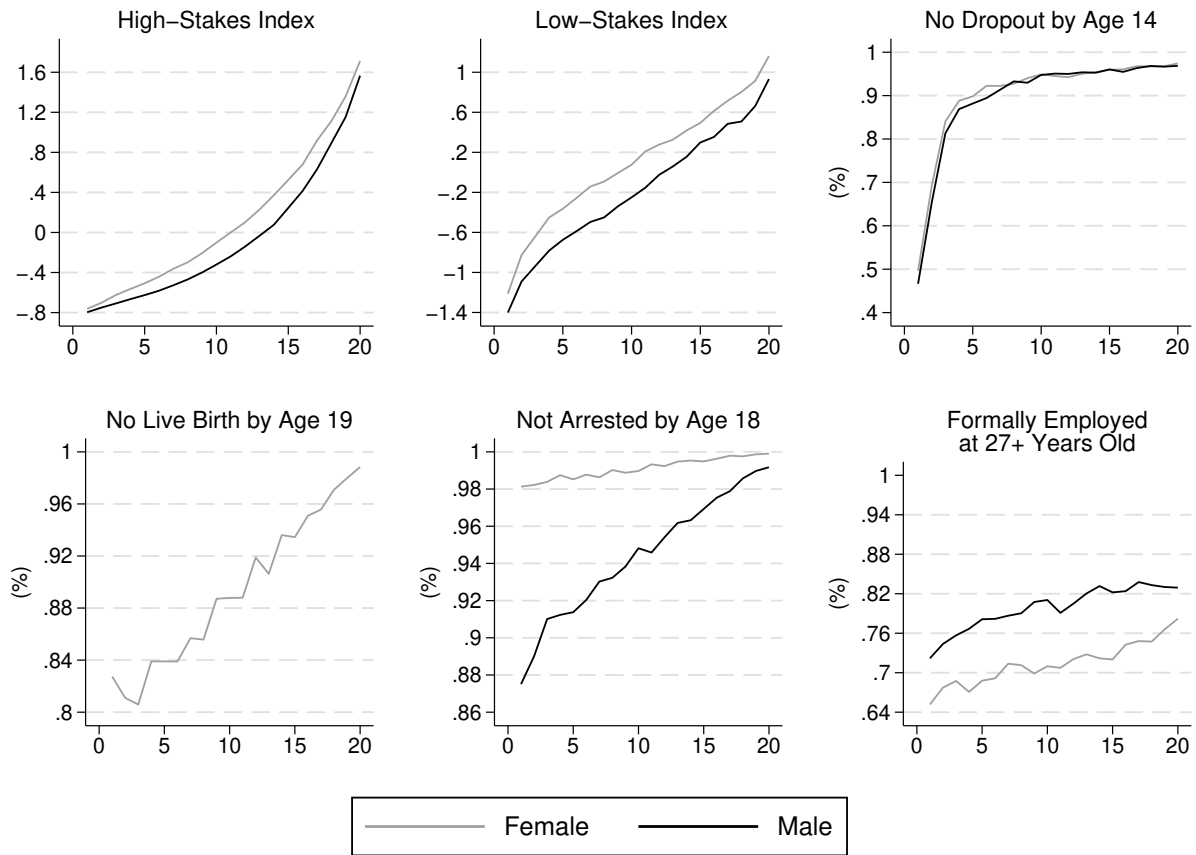
Notes: The X-axis represents the SEA score ventile. The Y-axis represents each outcome for students in standardized units for high-stakes and low-stakes indexes and in percentages for the other outcomes. Arrests are much higher in males and in the lower ventiles, but it still occurs in the top ventiles of the SEA score.

Appendix C: Selection of Excluded Schools

Our analysis focuses on all public secondary schools in Trinidad and Tobago between the years 1995 and 2012. Therefore, all private schools, temporary schools, and non-traditional secondary schools (such as junior life centers that focus on life skills rather than the national academic curriculum) are excluded. Our analysis therefore focuses on 134 public secondary schools (which account for over 95 percent of student enrolment).

In our 2SLS models, we estimate one first stage regression for each of these 134 schools, where the school attendance indicator is the dependent variable; while each of the 134 schools algorithm-based *assignment* indicators plus all controls listed in equation (**??**) enter as regressors. For each school, we assess the point estimates on the own-school algorithm-based assignment indicator from each first stage regression. If the the own-school assignment indicator resulted is less than a 15 percentage-point increase in the likelihood of attendance, the school was identified as having a weak fist stage and we, therefore, excluded both the assignment and attendance indicators when estimating individual school's causal impacts. In addition, for those outcomes that are only observed for more recent cohorts (such as low stakes, dropout, and pregnancy which are only observed for cohorts after 2004 compared to 1995 for crime and formal employment, and 1999 for high stakes exams), we drop the next lowest compliance school in Tobago.

This threshold of 15 percentage points was chosen because lower thresholds resulted in the inclusion of schools that triggered weak identification for all schools. Using higher thresholds yielded very similar results. Therefore, we used the lowest threshold for which the full 2SLS model was well identified. Because some outcomes are not observed for all SEA cohorts, the first stages are somewhat different. As such, in order to avoid weak identification (only for those outcomes observed with recent cohorts only) an additional low-compliance school in Tobago needed to be dropped.

We do not exclude any individual observations when estimating the final 2SLS model. Therefore, the low-compliance schools for which the assignment and attendance indicators were excluded serve as the omitted category. Note that because not all schools were operating across during the entire analysis period, and because not all outcomes are observed for all the SEA cohorts; the total number of schools for which value-added estimates are observable varies across outcomes.

After estimating the school effects for each outcome, the main analysis removes any remaining outlier estimates (those more than 4σ away from the median estimate for that outcome). Table C1 shows the total number of schools for which we can observe each outcome (column 1), the total number of schools for which we estimate value-added for each outcome (column 2), and the total number of schools for which we have value-added for each outcome *after removing outliers* (column 3). The outcome with the most remaining schools is teen arrests, with 127 schools and the least is teen motherhood with 103 (note that this relatively low number is mechanical as this outcome is missing for the 17 all-boys schools).

Table C1. Schools by outcome

	Non-private schools		
	(1)	(2)	(3)
High-Stakes Index	134	126	126
Low-Stakes Index	134	124	124
No Dropout by 14	134	127	124
No live birth by 19	115	103	103
Not arrested by 18	134	130	127
Formally employed 27+	117	111	108

Notes: We show the total number of schools available for each outcome. The first column shows the total number of non-private schools in the cohorts where we have information for each outcome. In the second column we show the remaining amount of schools once we take out those with low first stages. We estimate value-addeds for all these. Finally, we take out outliers from our estimates and show the remaining number of schools in the last column.

Appendix D: Similar School Effects by Incoming Achievement

The first parametric assumption was that the relative school effects are the same for all incoming achievement levels. Recent work (i.e., Oosterbeek et al. 2020) shows this need not hold. We test this using a re-weighting method motivated by Solon et al. (2015). Because we use variation across cutoffs, we use variation among all admitted students to compute relative school effects. Because schools admit students across a wide range of incoming scores (especially non-selective schools – see Figure D1), one can estimate relative school effects only among students with (or around) a particular incoming score. We approximate this by estimating equation system (??) and (??) of the main text while weighting the regression on individuals at different points in the incoming test score distribution.[4] That is, where pct_i is the percentile of student i in the SEA distribution, we estimate each school's treatment effect ($\theta_j^{TOT_{IV}}$) while weighting each observation by $(1 + \frac{(X - pct_i)^2}{100})^{-1}$. This puts heavy weight on students with incoming scores close to the Xth percentile and low weight on those far away from that percentile (see Figure D2 for the weights by SEA score). We do this for the 25th, 50th, and 75th percentiles. As pointed out in Solon et al. (2015), because weighting uncovers effects that are typical for individuals who receive more weight, the differences between unweighted and weighted estimates will be informative on the extent of heterogeneity by incoming achievement.

We document minimal heterogeneity by incoming achievement in two ways. First, we assess the extent to which unweighted and weighted estimated school effects differ using paired t-tests (Table D1).[5] Based on 1,399 pairwise t-tests (by school-outcome-percentile), in less than 2 percent of cases does one reject that the estimates are the same (at the 5 percent significance level).[6] Second, we show that the unweighted estimates are very good predictors of the weighted ones. In Figure D3, we plot the weighted effects (and the prediction 95% confidence interval) against the unweighted (or average) school impacts. While there is some evidence that the average effects are not always the same as those evaluated at the 75th percentile, they are very similar so that for all three percentiles, the average-weighted effects are close to the $45°$ line.[7] As such, our assumption that school effects are the same by incoming achievement is largely supported by the data.

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[4]We can only approximate this because not all schools admit students at all incoming achievement levels. See Figure D1 for the distributions of incoming test scores by school selectivity. While almost all schools admit students around the 75th percentile, effects weighted at that percentile will be largely representative of effects for students close to that percentile. However, because the most selective schools do not admit students at the 25th percentile, school effects weighted at this percentile will be largely representative of effects for students close to that percentile for the least selective half of schools, but will reflect marginal effects for the lowest-scoring admits for schools that admit student exclusively above this achievement level.

[5]Note that we implement a conservative estimate of the standard error of the differences by assuming that the correlation between the errors for the two estimates of the same school is 1.

[6]Looking at individual outcomes, for all outcomes one cannot reject equality of effects between the average and those evaluated at the 25th percentile or the median *for any school*. The only estimates that appear to differ are for dropout and arrests evaluated at the 75th percentile. Even among these, the vast majority are the same – but insofar as out average effect may not reflect estimates for all students, it would only be for a small number of school effects for only these two outcomes among the highest achieving students.

[7]The slope predicting the percentile-weighted effects using the average effect is 0.998 for the 25th percentile, 0.923 for the 50th percentile, and 0.707 for the 75th percentile. Only for the 75th percentile does one reject that the slope is equal to one at the 5 percent level.

Table D1: Two sample test for average and weighted 2SLS

	Assuming Correlation = 0			Assuming Correlation = 1		
	Avg - 25th	Avg - 50th	Avg - 75th	Avg - 25th	Avg - 50th	Avg - 75th
	(1)	(2)	(3)	(4)	(5)	(6)
All Outcomes	0.00	0.00	0.00	0.00	0.00	4.63
High-Stakes Index	0.00	0.00	0.00	0.00	0.00	0.00
Low-Stakes Index	0.00	0.00	0.00	0.00	0.00	2.02
No Dropout by 14	0.00	0.00	0.00	0.00	0.00	8.25
No live birth by 19	0.00	0.00	0.00	0.00	0.00	0.00
Not arrested by 18	0.00	0.00	0.00	0.00	0.00	16.49
Formally employed 27+	0.00	0.00	0.00	0.00	0.00	0.00

Notes: We report the percentage of schools where the difference in the average and weighted value-added is significant at the 95% level. We adjust the difference dividing it by its standard error, which is estimated for the following two scenarios: when the correlation between the estimated value-addeds is assumed to be 0 (left panel) and when it is assumed to be 1 (right panel). This will affect the variance of the difference through the covariance term. We then report the percentage of cases where the adjusted difference is greater than 1.96.

Figure D1. Common support of incoming scores by selectivity of assigned school (10% groups)
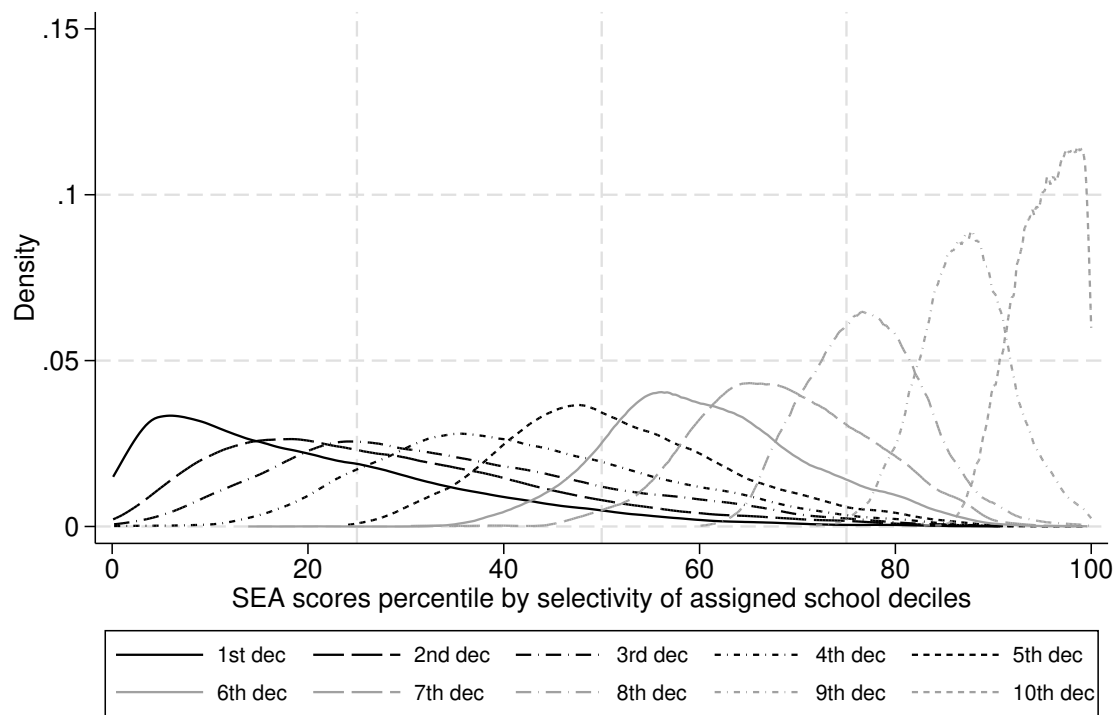
Notes: The X-axis represents the SEA score percentile. The Y-axis represents the density of students for each SEA score percentile. We plot one curve for different levels of school selectivity, going from the least selective on the left to the most selective schools on the right. As expected, as the selectivity of the school increases, the distribution of students by SEA percentiles shifts to the right. However, we can see there is still overlap of students between less and more selective schools.

Figure D2. Distribution of weights centered around 25th, median and 75th percentile
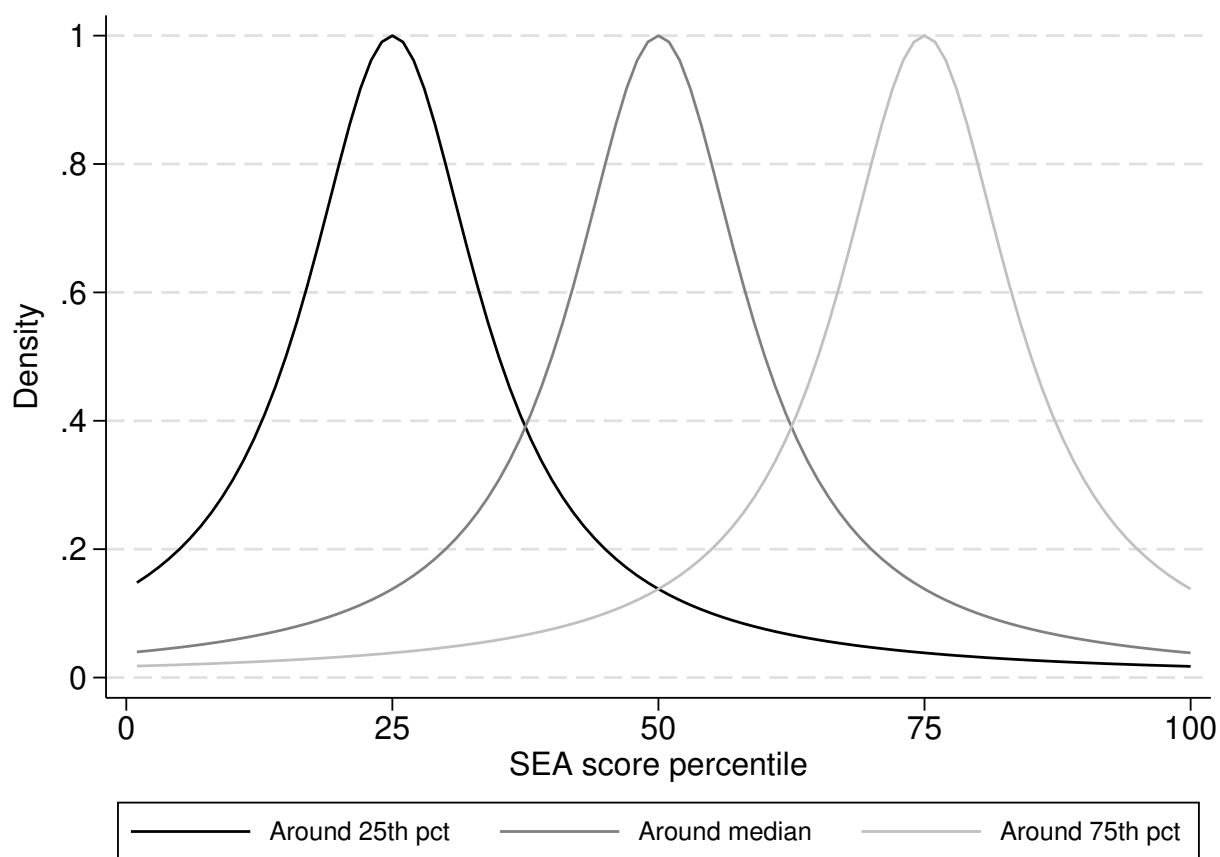
Notes: The X-axis represents the SEA score percentile. The Y-axis represents the density of students for each SEA score percentile. Each of the three weights is highly concentrated around the respective percentile (25th, 50th, 75th) but includes the whole range of students.

Figure D3. Predicted weighted 2SLS effects from average 2SLS effects

Slope = 0.998 ; CI 95% = [0.662;1.333]
p–val slope is 0 = 0.000; p–val slope is 1 = 0.990

Slope = 0.923 ; CI 95% = [0.819;1.028]
p–val slope is 0 = 0.000; p–val slope is 1 = 0.148

Slope = 0.707 ; CI 95% = [0.656;0.757]
p–val slope is 0 = 0.000; p–val slope is 1 = 0.000
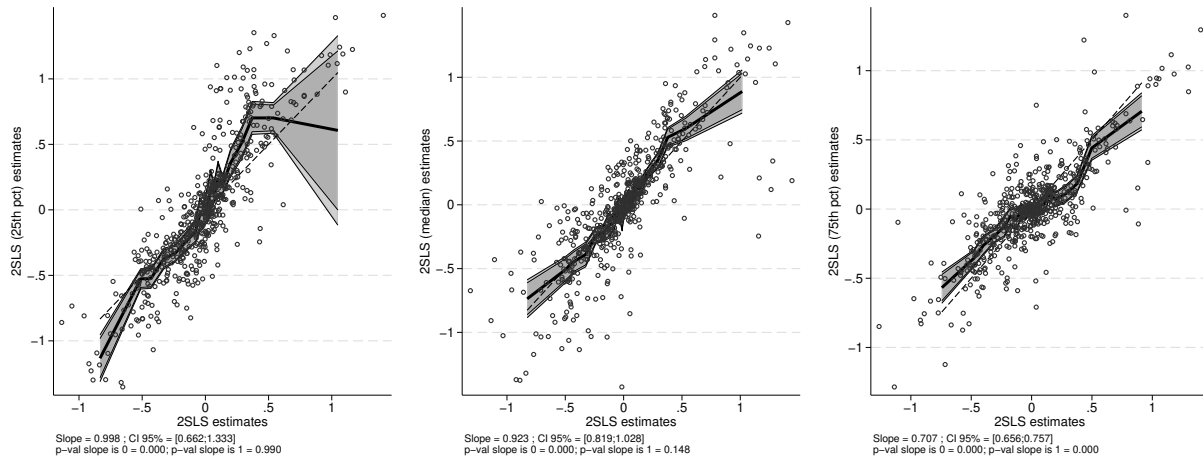
Notes: We plot weighted 2SLS estimated school impacts (around the 25th percentile, median and 75th percentile) in the Y-axis against the average 2SLS estimated school impacts (X-axis) in each panel. Prediction intervals have been computed by grouping the school estimates in 20 bins across the X-axis.

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Appendix E: Robustness to Interactions

The second assumption was that there are no interaction effects between incoming SEA scores and school choices. A key difference between a model that uses the DiD variation (as we do) and one that relies only on variation at cutoffs (not across) is that our model excludes interactions between school choices and incoming test scores. If the interactions are important for identification, then our school estimates (using the DiD variation without interactions) will be biased relative to flexible models that rely only on variation at cutoffs by accounting for interactions. As such, one can test the importance of the additive separability assumption between school choices and incoming scores by seeing if our estimates are robust to the inclusion of interactions between test scores and school choices. Note that we cannot fully control for all the possible interactions (because this is the level of the variation). However, the stability of our estimates to the inclusion of interactions between coarse measures of test scores and choices may be informative. We, therefore, estimate models controlling for these interactions and compare the resulting estimated school impacts, $\hat{\theta}_j^{TOT_{IV}}$, to those that we obtained without these extra controls. As we show here, in all cases the correlations between the resulting effects is close to 1.

We estimate our base model defined in (**??**) and (**??**) in the main text with four different interactions between choices and SEA scores. The *first interaction* considers the selectivity of choices. For this, we include an interaction between the polynomial of the student's own SEA score and the selectivity of each of the first 4 choices, this being approximated by the average peer SEA score of those assigned to that choice.

$$I_{i,j} = \Sigma(I_{i,\tau} \cdot \pi_{\tau j}) + f_j(SEA_i) + \Sigma(g_{j,k}(SEA_i) \cdot \overline{SEA}_{choice_k}) + \lambda_{j,c} + \mathbf{X}'_{it}\delta_{\mathbf{j}} + S_{j,t} + \upsilon_{ijct}, \quad \text{for each } j \in J \tag{2}$$

$$Y_{ijct} = \Sigma(\hat{I_{i,j}} \cdot \theta_j^{TOT_{IV}}) + f(SEA_i) + \Sigma(g_k(SEA_i) \cdot \overline{SEA}_{choice_k}) + \lambda_c + \mathbf{X}'_{it}\delta + S_t + \varepsilon_{ijct} \tag{3}$$

Where $g_k(SEA_i)$ is a fifth order polynomial and $\overline{SEA}_{choice_k}$ is the mean total SEA scores for incoming assigned students for each choice k.

The *second interaction* is between SEA scores and student's choice sets fixed effects. Instead of using one fixed effect per choice set, we use two, one for those below the median SEA score in that choice set and one for those above.

$$I_{i,j} = \Sigma(I_{i,\tau} \cdot \pi_{\tau j}) + f_j(SEA_i) + \lambda_{j,ci} + \mathbf{X}'_{it}\delta_{\mathbf{j}} + S_{j,t} + \upsilon_{ijct}, \quad \text{for each } j \in J \tag{4}$$

$$Y_{ijct} = \Sigma(\hat{I_{i,j}} \cdot \theta_j^{TOT_{IV}}) + f(SEA_i) + \lambda_{ci} + \mathbf{X}'_{it}\delta + S_t + \varepsilon_{ijct} \tag{5}$$

Now, instead of λ_c we will have λ_{ci} which will take one value if student i with choiceset c is above the median SEA score within that choice set and another if it is below.

The *third interaction* is between the student's SEA score and its *n-th* choice of school:

$$I_{i,j} = \Sigma(I_{i,\tau} \cdot \pi_{\tau j}) + f_j(SEA_i) + \Sigma(SEA_i \cdot I_{j,i,k}) + \lambda_{j,c} + \mathbf{X}'_{it}\delta_{\mathbf{j}} + S_{j,t} + \upsilon_{ijct}, \quad \text{for each } j \in J \tag{6}$$

$$Y_{ijct} = \Sigma(\hat{I_{i,j}} \cdot \theta_j^{TOT_{IV}}) + f(SEA_i) + \Sigma(SEA_i \cdot I_{i,k}) + \lambda_c + \mathbf{X}'_{it}\delta + S_t + \varepsilon_{ijct} \tag{7}$$

Where $I_{i,k}$ is an indicator equal to 1 if the *n-th* choice of student i is school k. This is done for the first 4 choices.
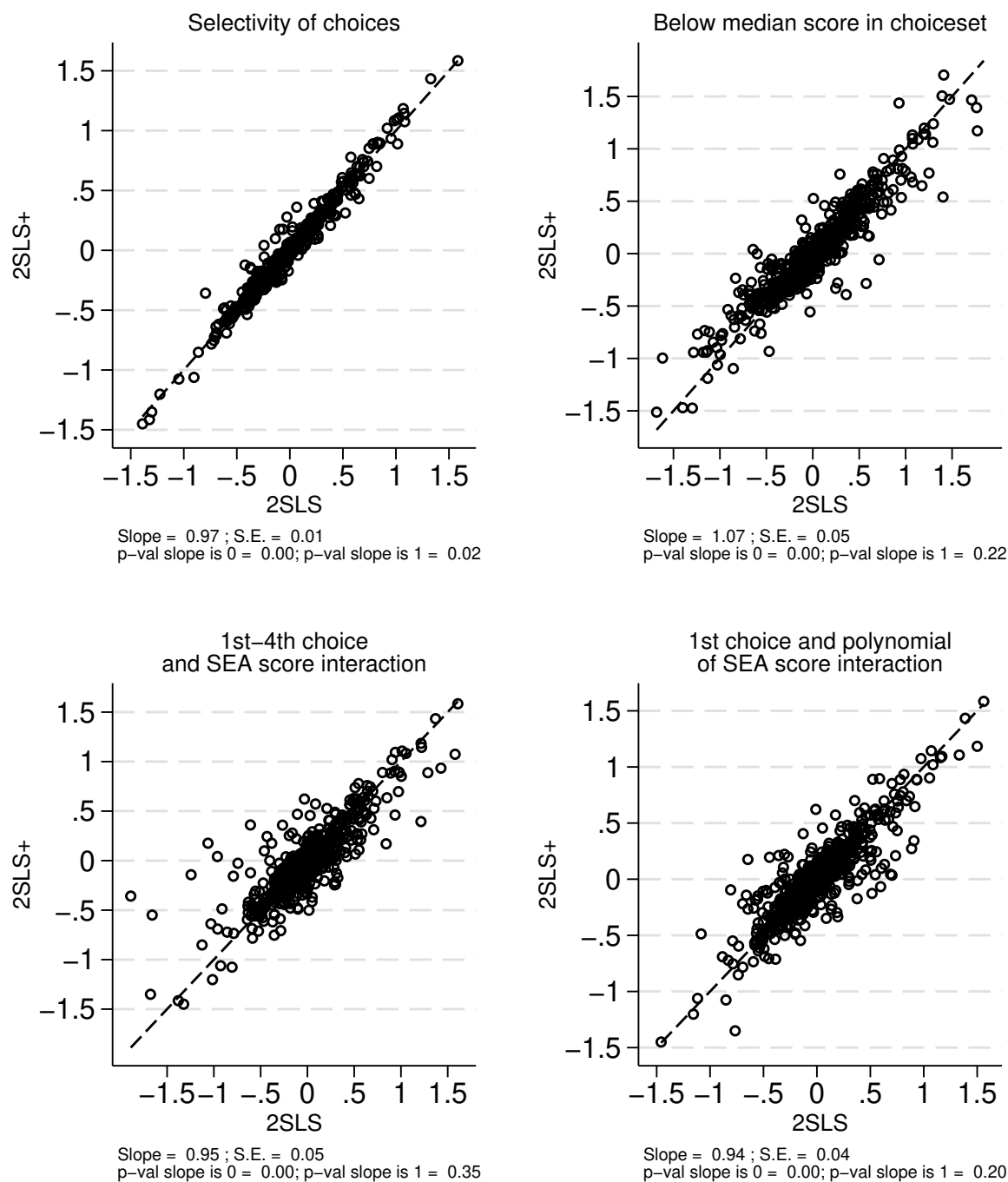
Finally, the *fourth interaction* is between the student's SEA score polynomial and its 1st choice of school:

$$I_{i,j} = \Sigma(I_{i,\tau} \cdot \pi_{\tau j}) + f_j(SEA_i) + \Sigma(g_{j,k}(SEA_i) \cdot I_{j,i,k}) + \lambda_{j,c} + \mathbf{X}'_{\mathbf{it}}\delta_{\mathbf{j}} + S_{j,t} + \upsilon_{ijct}, \quad \text{for each } j \in J \quad (8)$$

$$Y_{ijct} = \Sigma(\hat{I}_{i,j} \cdot \theta_j^{TOT_{IV}}) + f(SEA_i) + \Sigma(g_k(SEA_i) \cdot I_{i,k}) + \lambda_c + \mathbf{X}'_{\mathbf{it}}\delta + S_t + \varepsilon_{ijct} \quad (9)$$

Where $g_k(SEA_i)$ is a fifth order polynomial and $I_{i,k}$ is an indicator equal to 1 if the first choice of student i is school k. Results for all four models are shown in Figure E1

Figure E1: Robustness of School Impacts to Interactions Between Choices and Incoming Scores

Selectivity of choices

Slope = 0.97 ; S.E. = 0.01
p–val slope is 0 = 0.00; p–val slope is 1 = 0.02

Below median score in choiceset

Slope = 1.07 ; S.E. = 0.05
p–val slope is 0 = 0.00; p–val slope is 1 = 0.22

1st–4th choice
and SEA score interaction

Slope = 0.95 ; S.E. = 0.05
p–val slope is 0 = 0.00; p–val slope is 1 = 0.35

1st choice and polynomial
of SEA score interaction

Slope = 0.94 ; S.E. = 0.04
p–val slope is 0 = 0.00; p–val slope is 1 = 0.20

Notes: We show the relationship between the estimated school impacts with the interacted model (y-axis) and the model without interactions (x-axis). Each panel shows results for one of the interacted models. Estimated slope and p-values resulting from testing for whether the slope differs from both 0 and 1 are shown below the graph. The short dashed line shows the 45° relationship.

Appendix F: Regression Discontinuity Variation vs. All Variation

Existing papers that have explored parental preferences for school causal impacts have either relied on school average outcomes (which may not reflect their impacts *per se*) or estimated school impacts that may be biased due to selection.[8] If one's measures of school effectiveness do not accurately reflect schools' causal impacts, it may distort one's conclusions regarding parental preferences for school effectiveness. For this reason, validating the estimated school impacts as reflecting *causal* impacts is important. A key strength of our context and data is that we are able to validate our estimated school impacts using exogenous variation only. We test the validity of our value-added estimates by exploring if they are consistent with what one would obtain using plausibly exogenous variation only.

Under the algorithm used to create the tentative school assignments (discussed in Appendix A), each school has a minimum score above which applicants are tentatively admitted and below which they are not. As such, the marginal effect of being tentatively assigned to each school (relative to the next lowest ranked school) can be estimated with a regression discontinuity design. That is, among students who are applicants to a given school τ, the causal effect of being tentatively assigned to school τ is simply the effect of scoring above the admission cutoff for school τ (conditional on smooth functions of ones incoming SEA score). In our setup, students are considered to be applicants to a school if that school is in their ranked list and they do not score above the cutoff for a more preferred school. Note, therefore, that students can be applicants to more than one school.[9]

To obtain the reduced-form Regression Discontinuity (RD) effect of being tentatively assigned to any school τ, we estimate RD models for each outcome among all applicants to school τ.[10] Under the RD identifying assumptions, the reduced-form effect of being tentatively assigned to school τ on outcome Y, is captured by estimating the equation below.

$$Y_{ij} = Above_{i\tau} \cdot \gamma_\tau + f(SEA_i) + \mathbf{X_i'}\delta + \varepsilon_{ij} \tag{10}$$

Where Y_{ij} is the outcome of student i who attended school j, and $Above_{i\tau}$ is an indicator for scoring above the algorithm-based assignment cutoff for school τ. Among those who comply with the cutoff, $j=\tau$. The parameter γ_τ captures the difference in outcomes (all else equal) between those exogenously assigned to a preferred school τ (due to scoring above the cutoff) versus scoring below the cut off and attending the student's counterfactual school q (that is, the school that the students would have attended had they not scored above the cutoff for school τ). As such, in the neighborhood of the cutoff,

$$E[\hat{\gamma}_\tau|X_i, SEA_i] = E(Y_{ij}|Above = 1) - E(Y_{iq}|Above = 0) \tag{11}$$

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[8]In related work Abdulkadiroğlu et al. (2020) examine parents responsiveness to school impacts that rely on selection on observables assumptions (similar to our estimates). However, they are unable to validate these school impact estimates using exogenous variation in school attendance.

[9]For example, a student that was assigned to her first choice will only appear once as a (successful) applicant to her top choice school. However, a student who is assigned to her second choice school will appear twice: as an (unsuccessful) applicant to her top choice school and as a (successful) applicant to her second choice school.

[10]That is we estimate separate reduced-form models where, in each one, we consider all persons who applied to a particular school τ in each year.

To simplify equation (11), we consider this expression for compliers and for non-compliers. Under the assumption of unconfoundedness (Rubin 1990), it follows that $E[Y_{ij} - Y_{iq}|X_i, SEA_i] = \theta_j^{TOT} - \theta_q^{TOT}$. That is, if there is no selection on observables, the average difference in outcomes between observationally equivalent individuals who attended school j and school q reflects the difference in causal impacts between school j and school q. Among the compliers, school j is school τ if they score above the cutoff.[11] As such, for compliers, $E[\hat{\gamma}_\tau|X_i, SEA_i] = \theta_\tau^{TOT} - E[\theta_q^{TOT}]$, where $E[\theta_q^{TOT}]$ is the average impact of the counterfactual schools for the applicants to school τ. Among non-compliers, the cutoff does not change the school attended so that $E[\hat{\gamma}_\tau|X_i, SEA_i] = 0$. It follows that for the average applicant to school τ, equation (11) can be written as equation (12) below.

$$E[\hat{\gamma}_\tau|X_i, SEA_i] = \bar{p}_\tau \times (\theta_\tau^{TOT} - E[\theta_q^{TOT}]) \qquad (12)$$

In words, in expectation, the estimated effect of scoring above the cutoff for school τ is the difference between the impact of attending preferred school τ and that of attending the average counterfactual school q, all times the compliance rate ($\bar{p}_\tau$). This is simply the weighted cutoff effect for the compliers and the non-compliers.

Consider now, estimating this same model, but replacing each student's actual outcome with the predicted TOT impact of the school they attended, $\hat{\theta}_j^{TOT_{IV}}$, as below.

$$\hat{\theta}_j^{TOT_{IV}} = Above_{i\tau} \cdot \zeta_\tau + f(SEA_i) + \mathbf{X_i'}\delta + \varepsilon_{ij} \qquad (13)$$

The parameter ζ_τ is the difference in predicted TOT school impacts (all else equal) between those scoring above the cutoff for preferred school τ versus not. In the neighborhood of the cutoff, the RD effect on the predicted TOT impacts of an individual's attended school is $E[\zeta_\tau|X_i, SEA_i] = E(\hat{\theta}_j^{TOT_{IV}}|Above = 1) - E(\hat{\theta}_q^{TOT_{IV}}|Above = 0)$. Using the same logic as above for compliers and non-compliers, it follows that

$$E[\hat{\zeta}_\tau|X_i, SEA_i] = \bar{p}_\tau \times (E[\hat{\theta}_\tau^{TOT_{IV}}] - E[\hat{\theta}_q^{TOT_{IV}}]) \qquad (14)$$

In words, in expectation, the estimated difference in predicted school TOT impacts of scoring above the cutoff for school τ is the difference between the estimated TOT impact of attending preferred school τ and that of the average counterfactual school q, all times the compliance rate ($\bar{p}_\tau$).

Inspection of (12) and (14) reveals that if our treatment on the treated estimated impacts for attended school j and the average counterfactual school q are unbiased, then by the law of iterated expectations, $E[\hat{\zeta}_\tau|X_i, SEA_i] = E[\hat{\gamma}_\tau|X_i, SEA_i]$. In words, if the estimated TOT school impacts ($\hat{\theta}_\tau^{TOT_{IV}}$) are consistent estimates of the causal effect of attending school τ, then for each applicant school, the RD estimates using the actual outcomes as left hand side variables and the RD estimates using the TOT school impacts of the attended school j as left hand side variables, should be equal in expectation.

This motivates a validation test of our TOT school estimates. In related work, Hastings et al. (2015) implement a very similar test to validate the reliability of using predicted versus actual earnings when disseminating information on the expected returns to attend alternative colleges and majors. To implement this test, first, we estimate $\hat{\gamma}_\tau$ and $\hat{\zeta}_\tau$ for each preferred school τ (using the

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[11]In most instances school q will be the next ranked school in the choice list, but could be any fallback school (such as a private school or Government Assisted school that can admit students irrespective of their SEA scores).

optimal bandwidth from Imbens and Kalyanaraman (2012)). Following Hastings et al. (2015), to account for estimation errors in the RD effects on school impacts, we implement Empirical Bayes estimates of each cutoff effect.[12] We then regress the former estimated coefficients on the latter (to account for noisiness in the RD estimated effects, we weight each estimate by the inverse of its squared standard error when doing this regression). Finally, we test for whether the estimated slope is statistically indistinguishable from 1.

The results from this approach are reported in Figure F1. The binned scatter-plot, pooled across outcomes presents the relationship for the raw school impacts (left) and the Empirical Bayes estimates (right) – the conclusions are the same. Using the raw estimates, we fail to reject that the estimated slope coefficient is different from 1 (*p*-value = 0.48). The slopes using the Empirical Bayes estimates (with or without weights) are close to 1, and both yield *p*-values well above 0.5. This test (in addition to the two previous tests) indicate that the "no selection" condition which we showed holds for the RD variation likely also holds for our preferred models.

---

[12]Specifically, for any particular outcome, the *predicted* RD effect for school j is the weighted difference in estimated school impacts between those just above and below the cutoff for school j. We can express the estimated parameter as $\hat{\zeta}_{\hat{\theta}_j} = \sum_{k \in A} a_k(\hat{\theta}_k) - \sum_{k \in B} b_k(\hat{\theta}_k) = \sum_{k \in A} a_k(\theta_k + \varepsilon_k) - \sum_{k \in B} b_k(\theta_k + \varepsilon_k)$, where A is the set of schools the students attend above the cutoff, B the set of schools the students attend below the cutoff, a_k and b_k the proportions in which they do so and ε_k the estimation error for the school impact θ_k. If we assume that the school impacts are not independent within each cutoff, but that the estimation errors are, we can approximate the variance of this estimated parameter as $Var(\hat{\zeta}_{\hat{\theta}_j}) = \sum u_k \sigma_\theta^2 + \sum v_{lm} Cov_\theta + \sum w_k SE_k^2$, where σ_θ^2 is given by the magnitude of the school impacts, Cov_θ is approximated by the covariance between each pair of schools the students applied to and SE_k^2 is given by the square of the standard error of the school impact. If we also include the squared standard error of the RD estimate, $SE_{\hat{\zeta}}$, the reliability ratio of our RD estimate is given by

$$\lambda_j = \frac{\sum u_k \sigma_\theta^2 + \sum v_{lm} Cov_\theta}{(\sum u_k \sigma_\theta^2 + \sum v_{lm} Cov_\theta) + (\sum w_k SE_k^2 + SE_{\hat{\zeta}}^2)} \tag{15}$$

Our Empirical Bayes estimate of the *predicted* effect of cutoff j is therefore $[\lambda_j \times \hat{\zeta}_{\hat{\theta}_j}]$

Figure F1: Predicted Cutoff Effects versus Actual Cutoff Effects

Slope = 0.761; S.E. = 0.309
p–val slope is 0 = 0.057; p–val slope is 1 = 0.475

Slope (unweighted) = 0.903 ; S.E. = 0.424
p–val slope is 0 = 0.086; p–val slope is 1 = 0.829
Slope (weighted) = 0.915 ; S.E. = 0.305
p–val slope is 0 = 0.030; p–val slope is 1 = 0.791
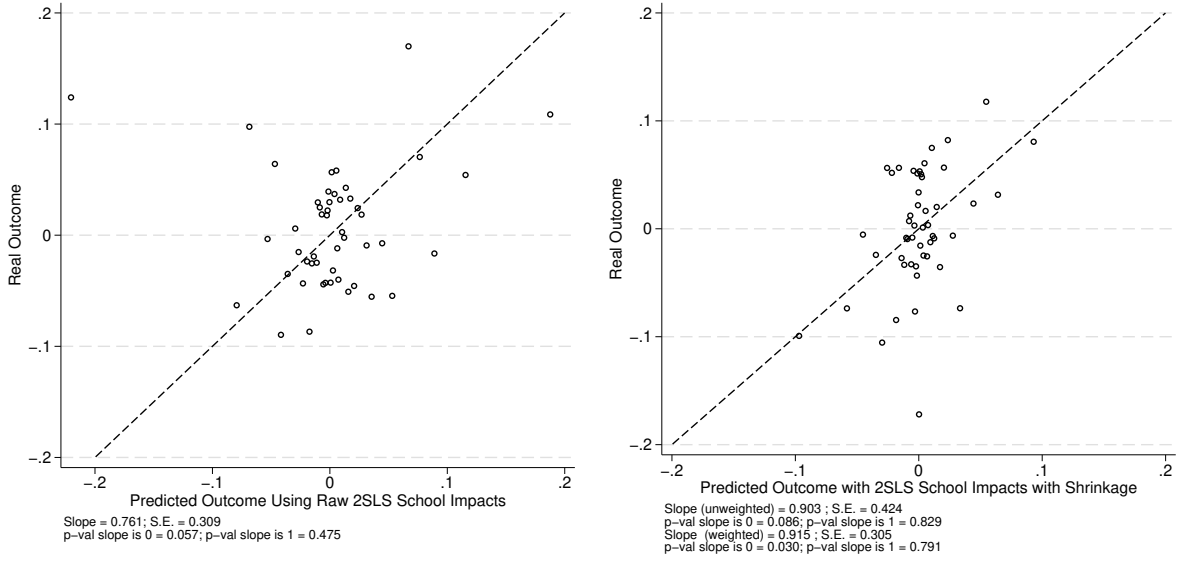
Notes: In both panels, the X-axis represents the estimated coefficients on an indicator for scoring above the rule-based cutoff resulting from an RD model that controls for a fifth degree polynomial of the SEA score, gender, district of residence at SEA registration, and religion; estimated for each school j and for each outcome where the estimated TOT school impacts ($\hat{\theta}_j^{TOT_{IV}}$) enter as dependent variables. The Y-axis represents the estimated coefficients on an indicator for scoring above the rule-based cutoff resulting from an RD model that controls for a fifth degree polynomial of the SEA score, gender, district of residence at SEA registration, and religion; estimated for each school j and for each outcome where the individual level outcomes enter as dependent variables. Estimated slope and p-values resulting from testing for whether the slope differs from both 0 and 1 are shown below the graph. The short dashed line shows the 45° relationship. Schools have been grouped in 50 bins across the X-axis. Outliers above 4 standard deviations away from the median were removed. In the left panel, we show unweighted results. In the right panel, all school estimated effects are adjusted by the reliability ratio described in the text. We show unweighted results and results were all school estimated effects are weighted by the inverse of the squared standard error of each estimated coefficient for the real outcome.

Appendix G: Testing for Differential Match Effects

While we assumed that value-added is fixed for exposition purposes, here we test the specific identifying assumption we require: no differential match effects on average across schools. Following Kirkeboen et al. (2016) we test for differential match bias within choice groups (what they refer to as comparative advantage) and assess whether students who list school m over school k experience larger benefits from attending school m versus school k than those who ranked school k over school m. To this aim, for every pair of schools m and k, we estimate (16) among individuals who list both schools in their choices. All common variables are defined as in the main text.

$$Y = I_m \cdot \theta_{m,k}^{ITT} + (I_m \cdot I_{k-b-m}) \cdot \theta_{k,m}^{Pref} + I_{\neq m,k} \cdot \theta_{n,k}^{ITT} + f(SEA) + \lambda_c + \mathbf{X}'\delta + S_t + \varepsilon \qquad (16)$$

In (16), I_m is an indicator denoting being assigned to school m, and $I_{\neq m,k}$ is an indicator denoting whether the individual was *neither assigned* to school m nor school k. Because the omitted school assignment is school k, $\theta_{m,k}^{ITT}$ (the coefficient on I_m) captures the effect of being assigned to school m relative to school k. The variable I_{k-b-m} is an indicator that denotes individuals who list school k above m (as opposed to school m above k). The direct effect of this difference in preferences is already accounted for in λ_c. The interaction effect between being assigned to school m relative to k and listing school k above m (i.e., $\theta_{k,m}^{Pref}$) captures the *differential* effect of being assigned to school m relative to k for those who chose k before m relative to those who listed m before k.

If, for any pair of schools, students who prefer school m over k experience larger relative effects from attending school m over k, then $\theta_{k,m}^{Pref}$ would be negative. We estimate this model for all pairs of schools, and capture $\hat{\theta}_{k,m}^{Pref}$ for each pair. We then test for whether the average and the median of these estimates differ from zero.[13] As shown in Table G1, we find small average and median effects failing to reject that these are equal to zero at the 10 percent level – suggesting minimal differential match effects of this sort.

Table G1: Testing for Differential Match Effects

	Mean	p-value	Median	p-value
	(1)	(2)	(3)	(4)
High-Stakes Index	0.702	0.323	0.015	0.179
Low-Stakes Index	0.2	0.146	-0.016	0.486
No Dropout by 14	0.051	0.502	0.006	0.735
No live birth by 19	-2.199	0.1	-0.036	0.185
Not arrested by 18	-2.939	0.304	0.003	0.558
Formally employed 27+	0.08	0.295	0.015	0.443

Notes: We show results for a mean and median test for the interaction effect, $\theta_{k,m}^{Pref}$, from (16). Neither is rejected for being equal to zero at the 10% level. Hence, we find no evidence of *differential effect* of being assigned to school m relative to k for those who chose k before m relative to those who listed m before k.

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[13]Because we do not account for estimation errors, this test is biasing *toward* rejecting the null of no match effects.

Appendix H: Testing for Additivity of School Effects

Even if we can identify relative school effects among individuals who make similar choices, our ability to compare *all* schools against each other relies on the assumption that school effects are additive. This condition holds with fixed value-added, but here we show that this particular condition holds empirically. We define the parameter $\theta_{m,k}^{ITT}$ as the effect of being assigned to school m relative to being assigned to school k. If school effects are additive, then $\theta_{m,k}^{ITT} = \theta_{m,l}^{ITT} + \theta_{l,k}^{ITT}$. If school effects are not additive, then this condition will generally not hold. As such, to test for additivity, we implement the sample analog of this test. That is, we test whether $\hat{\theta}_{m,k}^{ITT} = \hat{\theta}_{m,l}^{ITT} + \hat{\theta}_{l,k}^{ITT}$. Specifically, for each pair of schools m and k, we restrict the data to students that had both schools in their choices. We then estimate equation (17), where I_m is an indicator for being assigned to school m, and $I_{\neq m,k}$ is an indicator connoting assignment to a school other than school m or school k. [14]

$$Y = I_m \cdot \theta_{m,k}^{ITT} + I_{\neq m,k} \cdot \theta_{n,k}^{ITT} + f(SEA) + \lambda_c + \mathbf{X}'\delta + S_t + \varepsilon \tag{17}$$

In equation (17), because the omitted school assignment is school k, $\theta_{m,k}^{ITT}$ captures the effect of being assigned to school m relative to school k. We then find all intermediate schools l such that $\hat{\theta}_{m,k}^{ITT_{sum}} = \hat{\theta}_{m,l}^{ITT} + \hat{\theta}_{l,k}^{ITT}$ can be computed. As is typical in the value-added literature, because we will use this sum of estimates as a regressor, we form Empirical Bayes estimates by multiplying each raw sum of effects $\hat{\theta}_{m,k}^{ITT_{sum}}$ by an estimate of their reliability $\hat{\lambda}_{m,k}$.[15] That is, our Empirical Bayes estimate of the indirect effect is $\hat{\lambda}_{mk}\hat{\theta}_{m,k}^{ITT_{sum}}$. Using (18) we test whether $\beta = 1$, to implement a formal test of additivity of school effects.

$$\hat{\theta}_{m,k}^{ITT} = \alpha + \beta[\hat{\lambda}_{mk}\hat{\theta}_{m,k}^{ITT_{sum}}] + \varepsilon \tag{18}$$

We implement this test on sets of school pairs that have at least 100 observations assigned to school m, school k, or school l across all SEA cohorts – this corresponds to about 15 observations per school per cohort. A binned scatterplot of the direct estimates against the indirect estimate (i.e., the sum) is presented in Figure H1. The datapoints line up remarkably well along the 45 degree line, and the null hypothesis that the effects are additive (i.e. $\beta = 1$) is not rejected. This suggests that our school effects are additive – indicating that the fixed value-added assumption is reasonable.

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[14] Estimated standard errors are clustered at the assigned school level.

[15] Where $\sigma_{\theta_{sum}}^2$ is the variance of the sum of the two school effects to create the indirect estimate, and $\sigma_{e,sum}^2$ is the variance of the estimation error for the indirect effect, the reliability ratio of our indirect school effect can be written as: $\lambda_{mk} = \frac{\sigma_{\theta_{sum}}^2}{\sigma_{\theta_{sum}}^2 + \sigma_{e,sum}^2}$ To compute an estimate of the reliability ratio, we assume that the estimation errors are uncorrelated across schools. Therefore, our empirical estimate of $\sigma_{e,sum}^2$ is $(SE_{\hat{\theta}_{ml}}^2 + SE_{\hat{\theta}_{lk}}^2)$. We then make the additional standard assumption that the estimation errors are uncorrelated with the true effects. Therefore, the total variance of the estimated indirect school effects is $\sigma_{\hat{\theta}_{sum}}^2 = \sigma_{\theta_{sum}}^2 + \mathbf{E}(SE_{\hat{\theta}})^2$, where $\mathbf{E}(SE_{\hat{\theta}})^2$ is the sum of the squared expected standard errors $SE_{\hat{\theta}_{ml}}$ and $SE_{\hat{\theta}_{lk}}$, which are approximated by the mean standard error of the estimates (alternatively, we also approximated them with the median standard error of the estimates yielding equivalent results). By subtracting our estimates of $\mathbf{E}(SE_{\hat{\theta}})^2$ from the overall variance of the estimated indirect effects, we estimate $\sigma_{\theta_{sum}}^2$. The estimated reliability ratio, $\hat{\lambda}_{m,k}$, is then computed using these two estimates (Kane and Staiger, 2008).

Figure H1: Additivity of the School Effects.
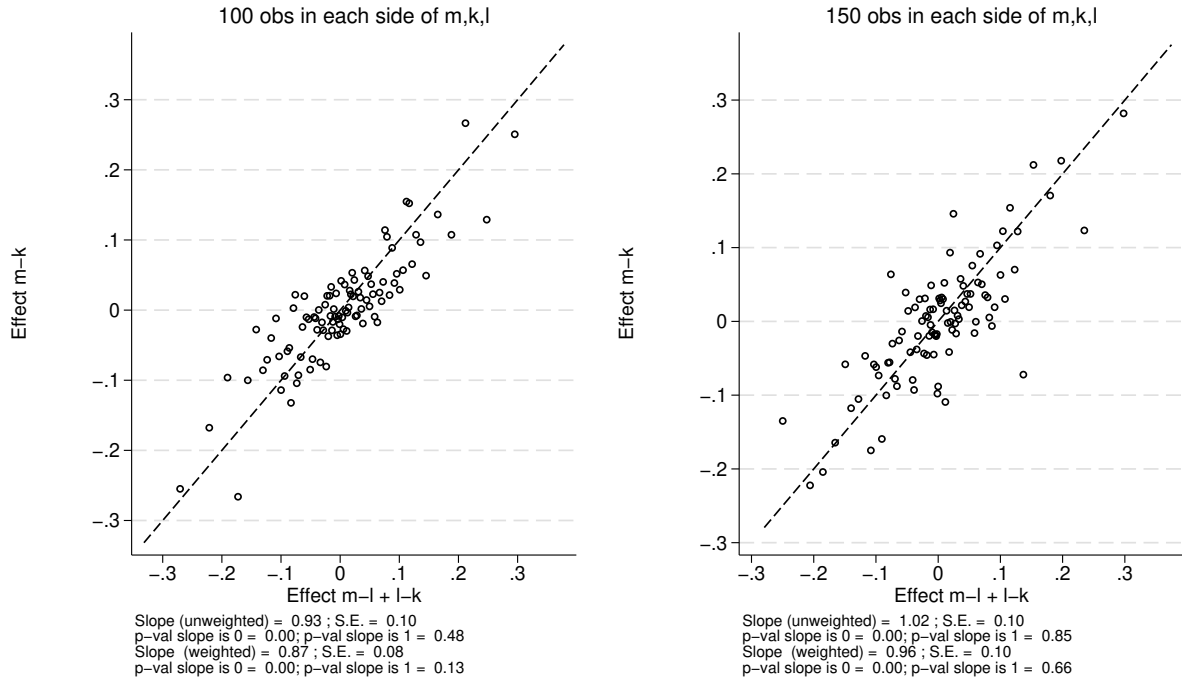
Notes: Both panels display estimated relative school effects pooled across all outcomes. The left panel shows results where the estimated effects were obtained with least 100 students assigned to m, l or k. The right panel restricts this to at least 150 students. We show slopes and standard errors for the unweighted relationship and weighted by the sum of observations used to estimate each pair $m - l$ and $l - k$. The p-values associated with the hypothesis that the slope is 0 and that the slope is 1 are shown below these for each estimation. The short dashed line shows the 45° relationship. We use the mean of the estimated standard errors of the school effects when computing the reliability ratio. Similar results are obtained when employing the median of the standard errors. In this case, slopes are slightly lower, in average 0.03 units less than when using the mean and we still fail to reject all slopes being different from 1 at the 5% level. Observations have been grouped in 100 bins across the X-axis.

Appendix I: Estimating Admission Probabilities

When we compare the top choice school to all un-chosen schools within our main choice model, we account for the probability that student i would have been assigned to each school j had they applied. In many research settings, this probability is difficult to uncover. Fortunately, because we have many years of admissions data and students are assigned to schools based on a known algorithm, we can approximate this probability with the historical likelihood that student i would have scored above the cutoff for school j given their own incoming SEA score. We report these assignment probabilities for four different schools by the percentile of incoming SEA scores in Figure I1. School A is a very selective secondary school. Across all years, no student below the 82nd percentile scored above the cuttoff for that school and all students above the 92nd percentile did. We show similar figures for less selective schools in other panels. Depending on their incoming SEA score, students may be marginal admits for some schools (predicted probabilities greater than zero and less than 1), be virtually guaranteed assignment at some schools, and have virtually no chance at others.

Because there is uncertainty for any student regarding their exact score, we compute the likelihood that student i is within "striking distance" of a given cutoff as follows: For each school in each year and each SEA score, we code the "rough" likelihood as 0 if the score was more than 5 percentile points below the cutoff; 0.5 if it was within 5 percentiles of the cutoff; and 1 if it was above the cutoff by more than 5 percentiles. We then compute the probability of student i with a particular SEA score being assigned to school j (p_{ij}) as the average of these "rough" likelihoods for that SEA score across all years (excluding the year when the student actually applied). We also show these probabilities for the selected schools in Figure I1. In our choice models, we use these rough probabilities and, to avoid extreme values, we truncate the rough probability at 0.05 and 0.95. So long as students are somewhat aware of these relationships based on historical precedent, our estimated probabilities proxy for the real admission probabilities used when making choices.

Figure I1: Estimated Admission Probabilities for Selected Schools
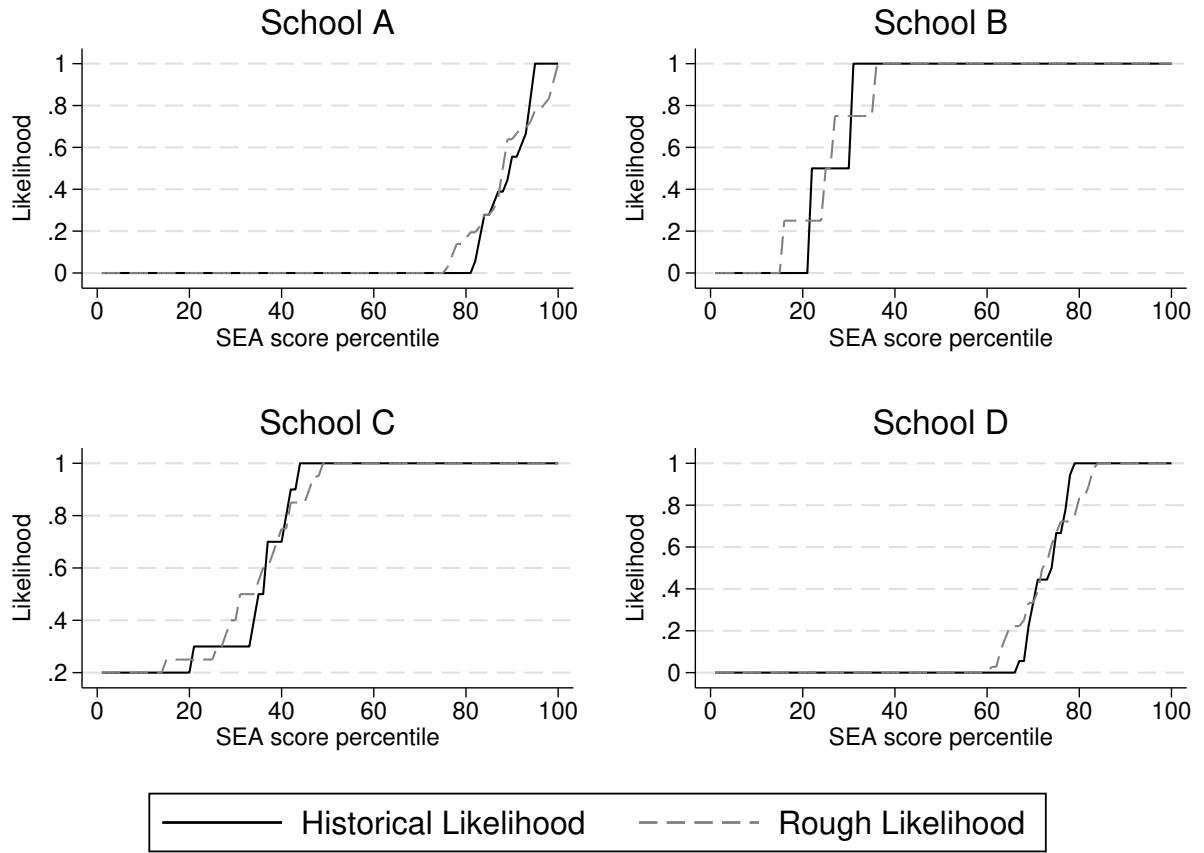
Notes: The X-axis represents the SEA score percentile. The Y-axis represents the likelihood of being assigned to a given school. School A is a very selective secondary school, where the school algorithm-based assignment cutoff has always been above the 82nd percentile. School B, on the other hand, is a less selective school where students above the 30th percentile have always scored above the cutoff. Historical probabilities are depicted with black hollow circles. "Rough" probabilities (calculated as described in the text) are depicted with gray diamonds.

Appendix J: Choice Model using Alternative Specifications

Rank-Ordered Logit

We first present estimates from this standard model which neither includes the first pseudo-observation nor the rejection probabilities. See Figures J1-J3.

Figure J1. Academic Outcomes using Rank-Ordered Logit Model

(a) Causal Impact (Impacts Only Model)

(b) Causal Impact (Full Model)

> ▬ Males   -- Males 95% CI   ▬ Females   -- Females 95% CI

Notes: The X-axis represent the individual SEA score ventile. The connected lines represent the estimated coefficients, computed separately for each (SEA score ventile)×(gender) cell, for two main models: Panel (a) displays estimates from the Impacts Only Rank Ordered Logit Model, which includes schools' causal impact estimates for all outcomes, peer quality, and log distance and Panel (b) displays estimates from the Full Rank Ordered Logit Model, which includes schools' causal impact estimates for all outcomes, the school-level averages for all outcomes, peer quality, and log distance. All specifications include control variables for whether the secondary school is on the same island, whether it is all-girls, whether it is all-boys, only comparing ranked schools. The dashed lines represent the associated 95% confidence intervals.

Figure J2. Non-Academic Outcomes using Rank-Ordered Logit Model

### (a) Causal Impact (Impacts Only Model)

(b) Causal Impact (Full Model)

Notes: The X-axis represent the individual SEA score ventile. The connected lines represent the estimated coefficients, computed separately for each (SEA score ventile)×(gender) cell, for two main models: Panel (a) displays estimates from the Impacts Only Rank Ordered Logit Model, which includes schools' causal impact estimates for all outcomes, peer quality, and log distance and Panel (b) displays estimates from the Full Rank Ordered Logit Model, which includes schools' causal impact estimates for all outcomes, the school-level averages for all outcomes, peer quality, and log distance. All specifications include control variables for whether the secondary school is on the same island, whether it is all-girls, whether it is all-boys, only comparing ranked schools. The dashed lines represent the associated 95% confidence intervals.

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Figure J3. Comparison of Choice Model's Estimated Coefficients (Rank-Ordered Logit Model)
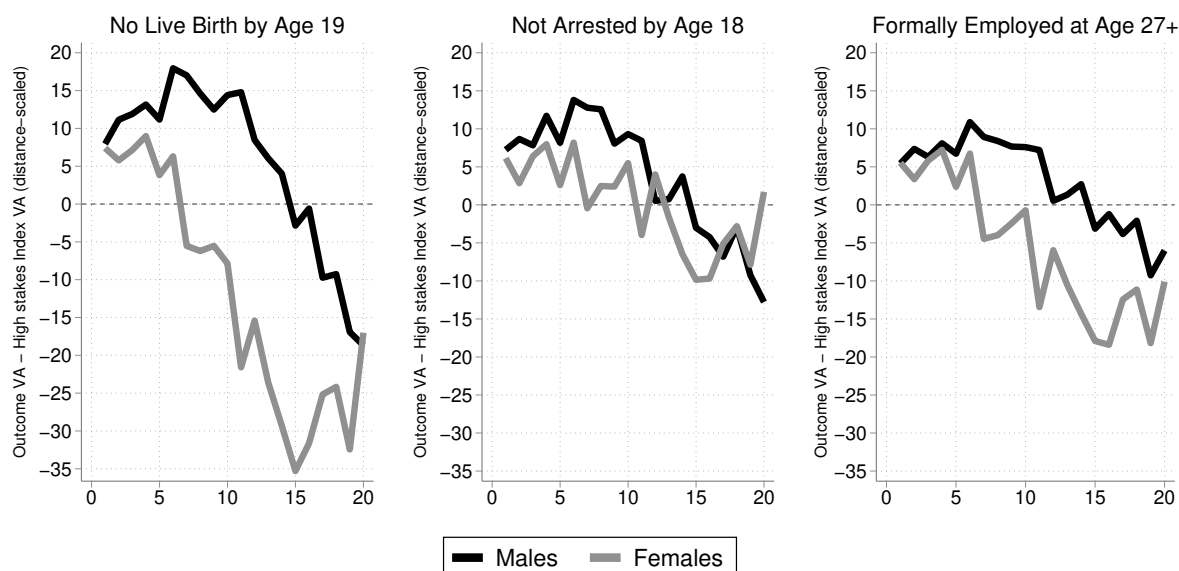
Notes: This figure presents the difference between the choice model's estimates on the school impacts of three non-academic outcomes and the choice model's estimate on the school impacts of the high-stakes index, scaled by the log distance estimate. The X-axis represents the individual score ventile. The connected lines represent the difference between the choice model estimate on the non-academic impacts and the choice model estimate on the high-stakes impacts divided by the log distance cell estimate (and scaled by 6 for ease of interpretation). This difference is computed separately for each (SEA score ventile)×(gender) cell. The estimates result the Ranked Ordered Logit Model, which includes schools' causal impact estimates for all outcomes, the school-level averages for all outcomes, peer quality and log distance, control variables (whether the secondary school is on the same island, whether it is all-girls, whether it is all boys) only for the ranked schools.

No Rejection Probabilities

We also present results from a model that includes the first pseudo-observation but does not account for rejection probabilities in any form. See Figures J4-J6.
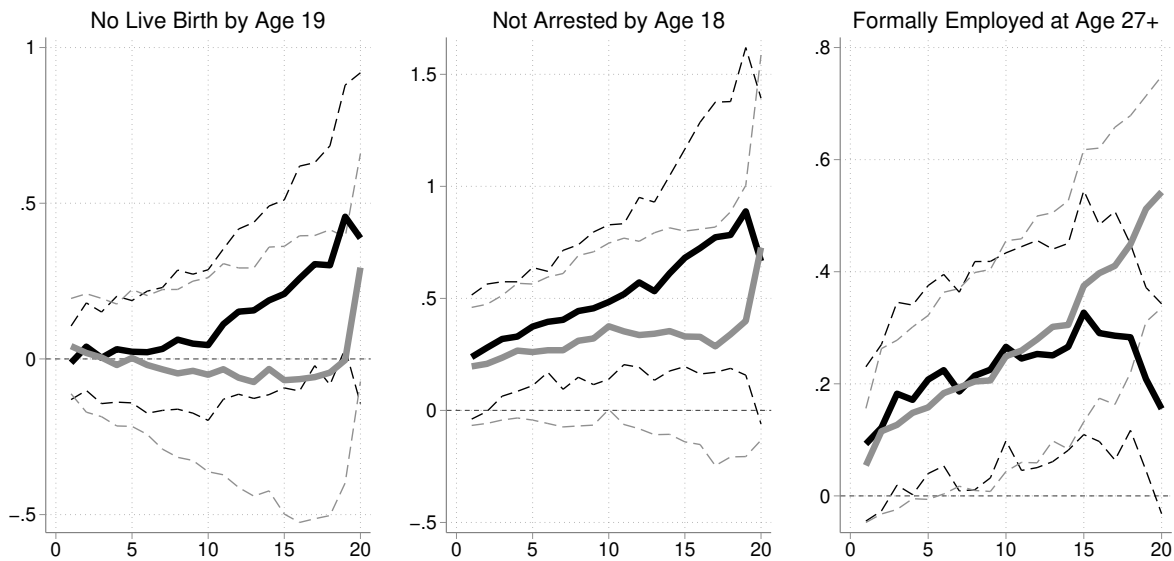
Figure J4. Academic Outcomes using The Model without Rejection Probabilities
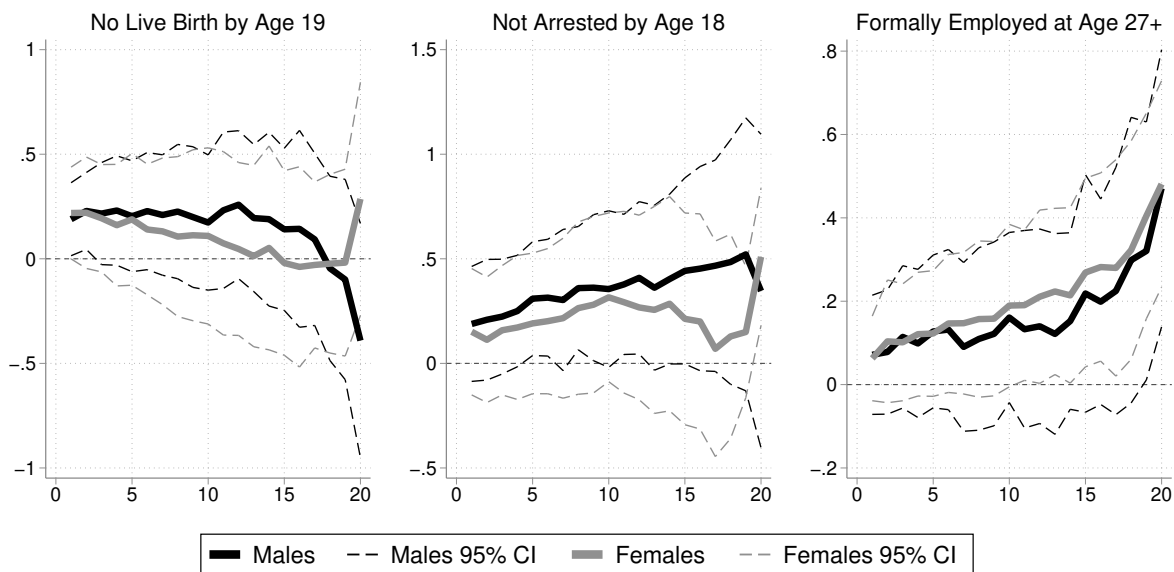
(a) Causal Impact (Impacts Only Model)

(b) Causal Impact (Full Model)

Notes: The X-axis represent the individual SEA score ventile. The connected lines represent the estimated coefficients, computed separately for each (SEA score ventile)×(gender) cell, for two main models: Panel (a) displays estimates from the Impacts Only Model using the model without rejection probabilities, which includes schools' causal impact estimates for all outcomes, peer quality, and log distance and Panel (b) displays estimates from the Full Model using the model without rejection probabilities, which includes schools' causal impact estimates for all outcomes, the school-level averages for all outcomes, peer quality, and log distance. All specifications include control variables for whether the secondary school is on the same island, whether it is all-girls, and whether it is all-boys. The dashed lines represent the associated 95% confidence intervals.

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Figure J5. Non-Academic Outcomes using The Model without Rejection Probabilities

(a) Causal Impact (Impacts Only Model)

(b) Causal Impact (Full Model)

| ━━ Males | − − Males 95% CI | ━━ Females | − − Females 95% CI |

Notes: The X-axis represent the individual SEA score ventile. The connected lines represent the estimated coefficients, computed separately for each (SEA score ventile)×(gender) cell, for two main models: Panel (a) displays estimates from the Impacts Only Model using the model without rejection probabilities, which includes schools' causal impact estimates for all outcomes, peer quality, and log distance and Panel (b) displays estimates from the Full Model using the model without rejection probabilities, which includes schools' causal impact estimates for all outcomes, the school-level averages for all outcomes, peer quality, and log distance. All specifications include control variables for whether the secondary school is on the same island, whether it is all-girls, and whether it is all-boys. The dashed lines represent the associated 95% confidence intervals.

Figure J6. Comparison of Choice Model's Estimated Coefficients (Model without Rejection Probabilities)


Notes: This figure presents the difference between the choice model's estimates on the school impacts of three non-academic outcomes and the choice model's estimate on the school impacts of the high-stakes index, scaled by the log distance estimate. The X-axis represents the individual score ventile. The connected lines represent the difference between the choice model estimate on the non-academic impacts and the choice model estimate on the high-stakes impacts divided by the log distance cell estimate (and scaled by 6 for ease of interpretation). This difference is computed separately for each (SEA score ventile)×(gender) cell. The estimates result from the Full Model (but without the inclusion of rejection probabilities), which includes schools' causal impact estimates for all outcomes, the school-level averages for all outcomes, peer quality and log distance, control variables (whether the secondary school is on the same island, whether it is all-girls, whether it is all boys).

Appendix K: Influence of School-Level Average Outcomes on School Choices

This appendix plots the estimated coefficients on the school average outcomes from the Full Model described in the main text, which includes schools' causal impact estimates for all outcomes, the school-level averages for all outcomes, peer quality and log distance, control variables (whether the secondary school is on the same island, whether it is all-girls, whether it is all boys).

Figure K1. Average Outcomes (Full Model)
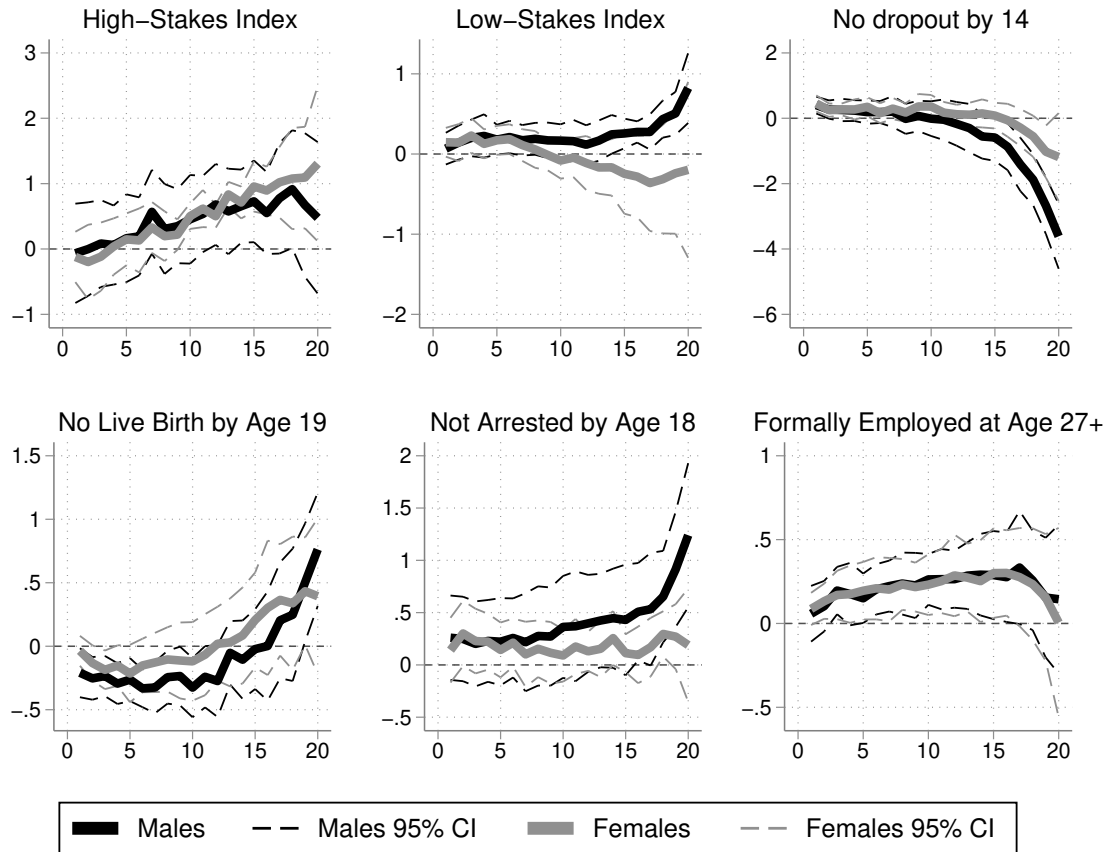
Notes: The X-axis represents the SEA score ventile. The connected lines represent the estimated coefficients of the standalone average outcomes, computed separately for each (SEA score ventile)×(gender) cell, for the Full Model, which includes schools' causal impact estimates for all outcomes, the school-level averages for all outcomes, peer quality, and log distance. All specifications include control variables for whether the secondary school is on the same island, whether it is all-girls, whether it is all-boys, the estimated likelihoods of school rejection only when comparing the top choice with all unranked schools, and interactions of these likelihoods with all school attributes. The dashed lines represent the associated 95% confidence intervals.

Appendix L: Influence of Admission Probabilities on School Choices

As discussed in the main text, a key conditioning variable in our analysis is the admission probability. Figure L1 plots admission probabilities for all the range of SEA scores differentiated by the priorities of the submitted school choices. Consistent with rational behavior, students rank more selective schools (where the probability of accessing them is almost null for students below the 80th percentile of the incoming test score distribution) higher within their choices.

Figure L2 shows the coefficients on the rejection probabilities from a choice model that includes these probabilities (only in first pseudo-observation) alone but without interacting these probabilities with the school's causal impacts or average outcomes.[16]

More desirable schools (for both observed and unobserved reasons) are those with higher rejection probabilities, by construction. As such, positive coefficients on rejection probabilities would indicate that there are unaccounted-for school attributes that are positively correlated with the rejection probabilities. However, if the school attributes included accurately reflect those dimensions of school quality that parents care for, the coefficients on the rejection probabilities should be negative. In the impacts only model, the point estimates for all groups are negative (Figure L2, left) – that is, conditional on peer quality, proximity and impacts on key outcomes, parents are less likely to choose schools to which their child is less likely to be admitted. In the full model(Figure L2, right), as expected, the coefficients on the rejection probabilities become more negative – indicating that schools that are more desirable tend to have better average outcomes. The fact that the coefficients on the rejection probabilities are negative for all groups provides empirical validation of the theoretical predictions from Chade and Smith (2006), and suggests that our included variables appropriately capture relevant determinants of parents' school choices.

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[16]We estimate this model to provide a more easily understandable relation between school choices and rejection probabilities (as opposed to the main model with interactions where the estimated coefficients on the rejection probabilities are not directly interpretable.

Figure L1. Likelihood of getting into each choice by SEA score percentile
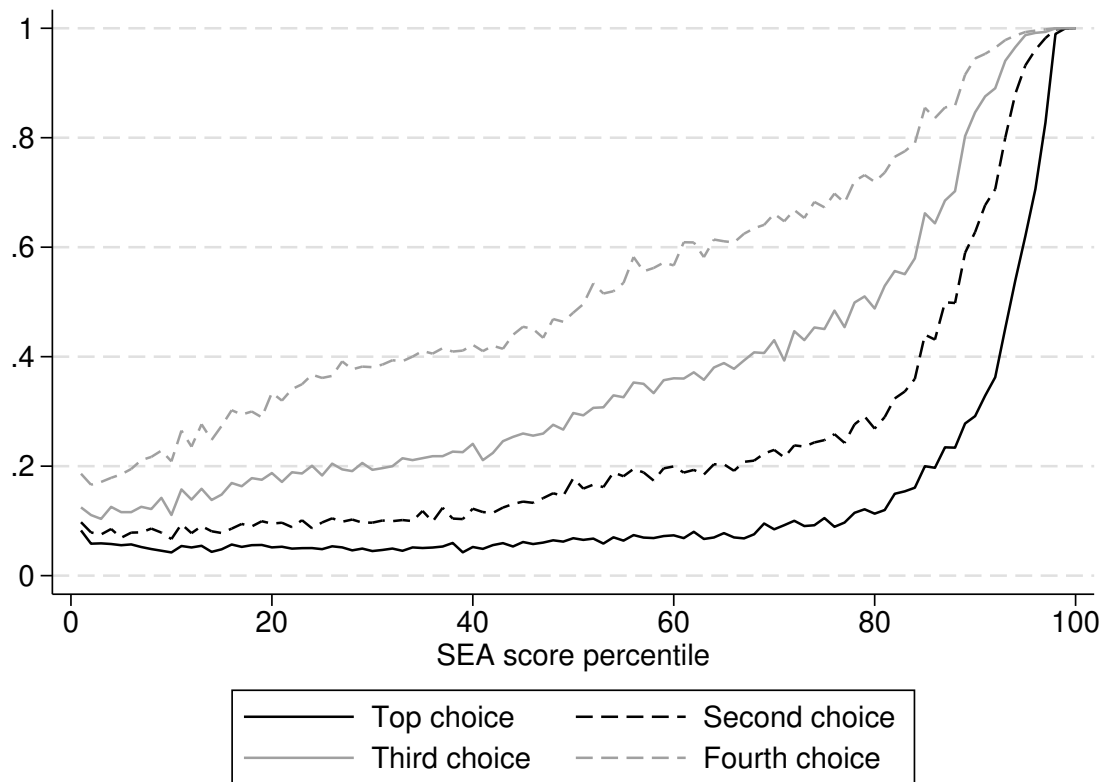
Notes: The X-axis represents the SEA score percentile. The Y-axis represents the likelihood of being assigned to a given choice. This probability is higher for the top SEA percentiles. The top choice, which are usually more selective schools, have higher cutoffs and a lower probability of assignment for each SEA percentile in the distribution. As we move to the next preferred choices, the probability of being assigned to that given choice increases for all SEA percentiles.

Figure L2: Rejection Probability
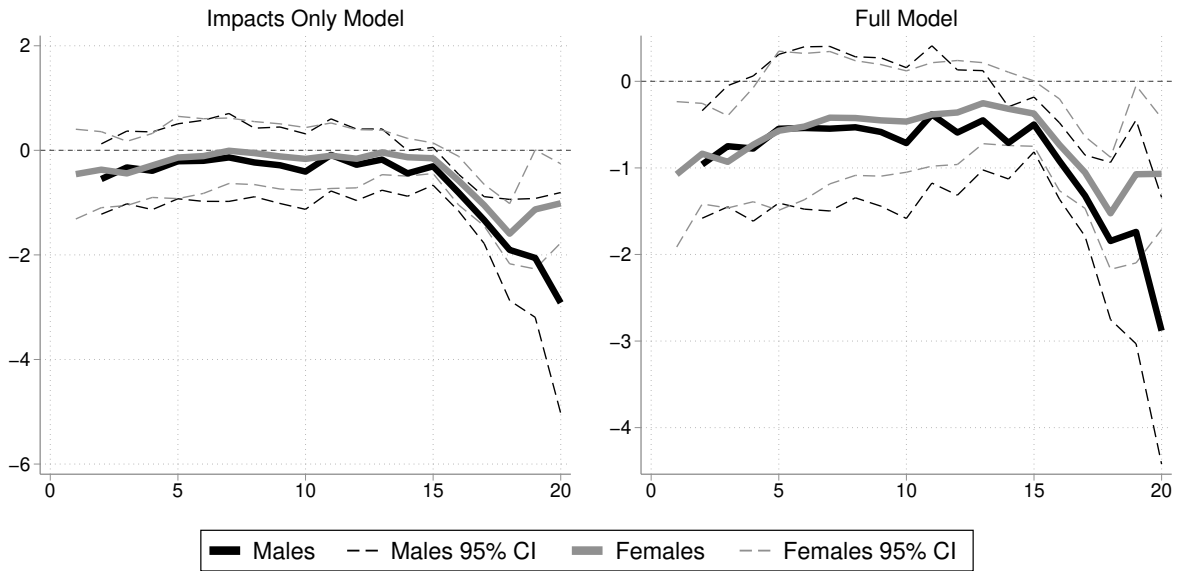
Notes: The X-axis represent the individual SEA score ventile. The connected lines represent the estimated coefficients, computed separately for each (SEA score ventile)×(gender) cell, for two main models: The left panel displays estimates from the Impacts Only Model, which includes schools' causal impact estimates for all outcomes, peer quality, and log distance and the right panel displays estimates from the Full Model, which includes schools' causal impact estimates for all outcomes, the school-level averages for all outcomes, peer quality, and log distance. All specifications include control variables for whether the secondary school is on the same island, whether it is all-girls, whether it is all-boys, and the estimated likelihoods of school rejection only when comparing the top choice with all unranked schools. The dashed lines represent the associated 95% confidence intervals.

Appendix M: Influence of Proximity and Peer Quality on School Choices

Here we document the importance of proximity and incoming peer quality in shaping school choices. The coefficient estimates for each cell are presented in Figure M1. With respect to distance, the figure reveals three key patterns. First, all students rank closer schools more highly - for all cells the estimate on distance is negative (with p-value<0.01) and the pooled models (Table M1) both have t-statistics over 20. Second, parents of the highest-achieving students (the top 20 percent) are somewhat more responsive to distance than those of lower-achieving students. Note that the patterns for the impacts only model (panel a) and the full model (panel b) are largely the same – indicating that distance to school is largely unrelated to other school attributes when shaping school preferences. Third, the relationship between choices and proximity for parents of boys and girls are similar within each achievement group.
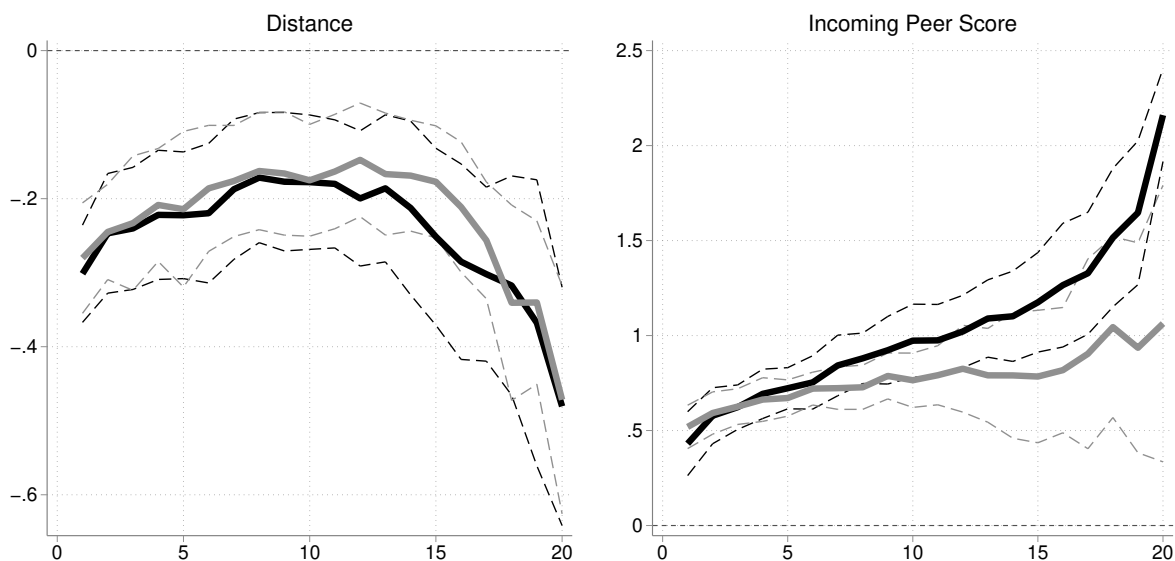
A second key attribute when choosing a school is the academic achievement of peers (Hastings et al. 2005; Hastings et al. 2006; Hastings and Weinstein 2008). Figure M1 shows the coefficients on the potential peers' academic quality (the average SEA score of the incoming cohort). In both models, one rejects that choices are unrelated to peer achievement for every cell at the 5 percent level, and the pooled models (Table M1) both have t-statistics over 20. In the impacts only model (panel a), parents of higher-achieving boys are more responsive to peer quality than low-achieving counterparts. However, this difference largely goes away in the full model (panel b). Also, note that the magnitude of the estimated coefficients from the full model fall by over a third relative to the impacts only model – suggesting that the association between school preferences and better peers (higher incoming achievement) may partially reflect preferences for better average outcomes.

The importance of proximity and peer quality in shaping the schooling decision is consistent with other studies (e.g., Hastings et al. 2005; Abdulkadiroğlu et al. 2020). A comparison of the coefficients on peer quality and proximity from the full models implies that, on average, increasing peer quality by about 0.445 standard deviations (roughly the difference between a student's top choice school and the second choice school) is associated with the same difference in choices as doubling the distance between the primary and secondary school. That is, the choice parameters imply that parents may be willing to travel about 3.2 times farther to attend a secondary school with one standard deviation higher in incoming peer scores. The average distance is about 6 kilometers, so that this amounts to travelling an *additional* 13 kilometers (or 8 miles) to attend a school with one standard deviation higher peer achievement. This relative relationship is largely similar throughout the achievement distribution for both males and females.
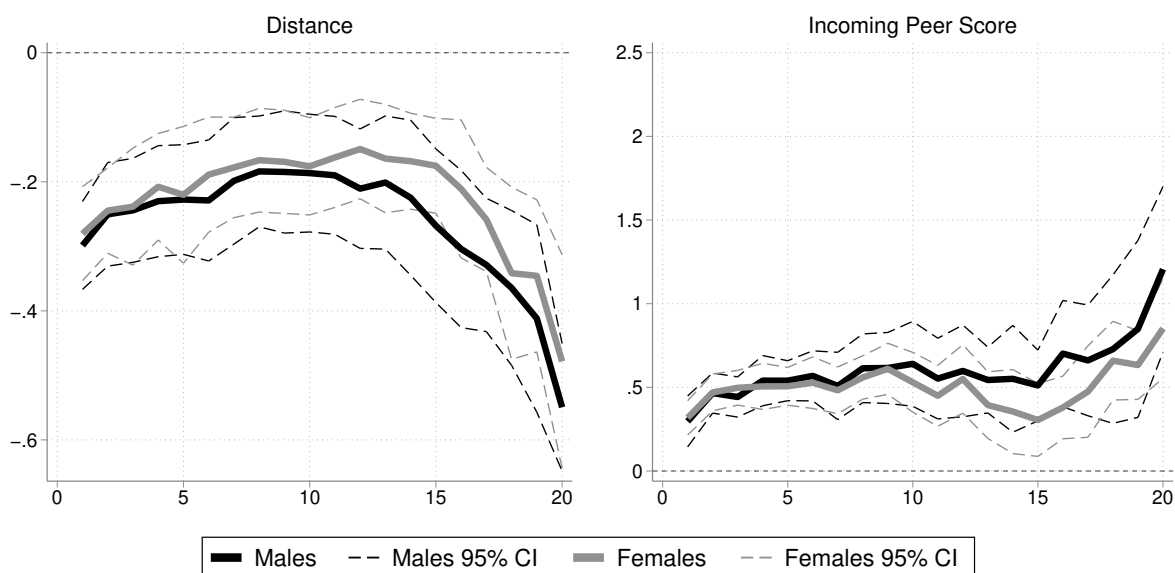
Figure M1: Proximity and Incoming Peer Quality

(a) Impacts Only Model

(b) Full Model

■■ Males　－－ Males 95% CI　■■ Females　－－ Females 95% CI

Notes: The X-axis represent the individual SEA score ventile. The connected lines represent the estimated coefficients, computed separately for each (SEA score ventile)×(gender) cell, for two main models: Panel (a) displays estimates from the Impacts Only Model, which includes schools' causal impact estimates for all outcomes, peer quality, and log distance and Panel (b) displays estimates from the Full Model, which includes schools' causal impact estimates for all outcomes, the school-level averages for all outcomes, peer quality, and log distance. All specifications include control variables for whether the secondary school is on the same island, whether it is all-girls, whether it is all-boys, the estimated likelihoods of school rejection only when comparing the top choice with all unranked schools, and interactions of these likelihoods with all school attributes. The dashed lines represent the associated 95% confidence intervals.
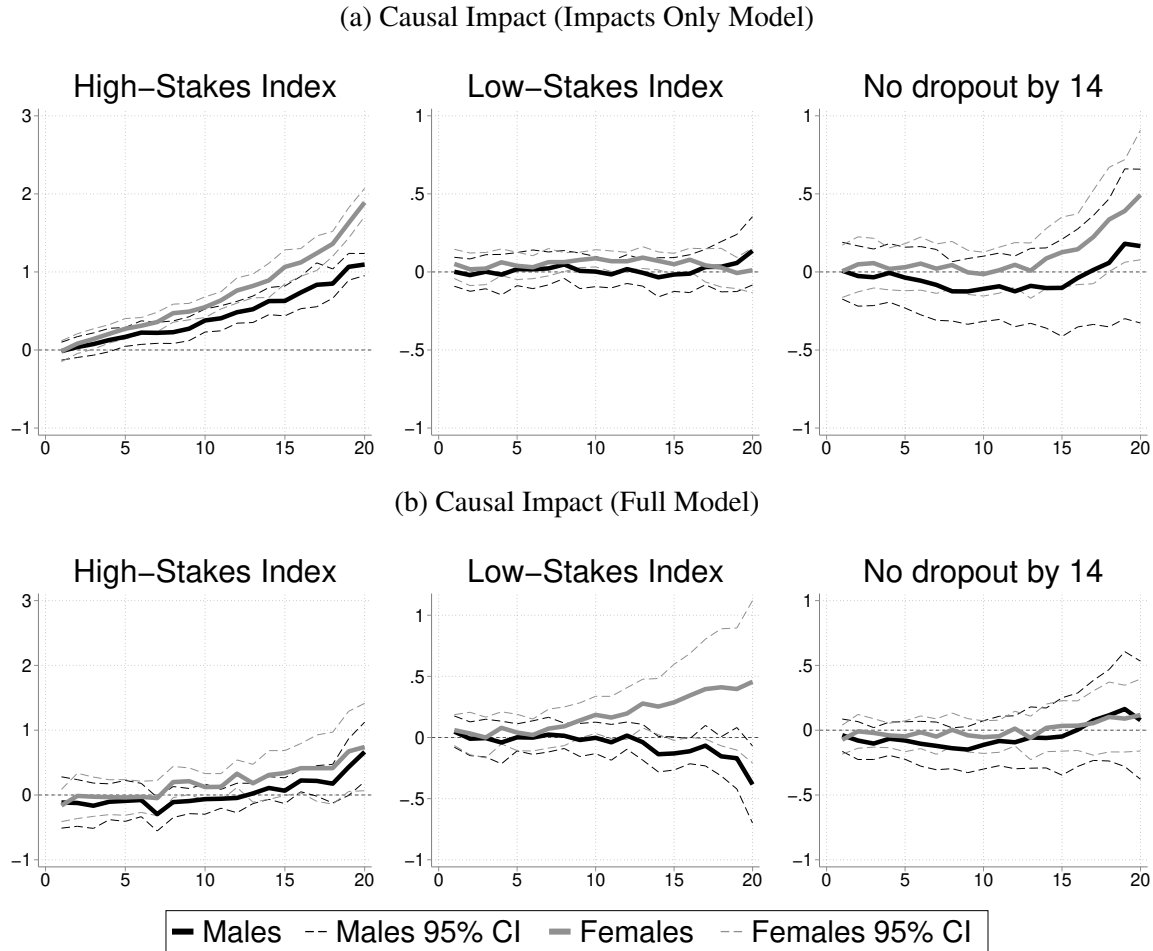
Table M1: Inference Tests

	Impacts Only Model			Full Model				
	Pooled average	Above median score	Below median score	Pooled average	Above median score	Below median score	Female	Male
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Distance	-0.236	-0.261	-0.211	-0.245	-0.275	-0.215	-0.226	-0.264
	(0.008)	(0.013)	(0.010)	(0.008)	(0.012)	(0.010)	(0.011)	(0.011)
Peer quality	0.906	1.101	0.711	0.555	0.598	0.512	0.503	0.607
	(0.022)	(0.041)	(0.015)	(0.019)	(0.034)	(0.018)	(0.021)	(0.032)

Notes: This table presents pooled averages of the choice model estimates on log distance to the school, and peer quality (i.e., β_1 from equation **??** in the main text) for two main models: (a) an Impacts Only Model, which includes schools' causal impact estimates for all outcomes, peer quality, and log distance (presenting the aggregated pooled average (Column 1) and the average by incoming achievement level (Columns 2 and 3)) and (b) a Full Model that includes schools' causal impact estimates for all outcomes, the school-level averages for all outcomes, peer quality, and log distance (presenting the aggregated overall average (Column 4), and the averages by incoming achievement level (Columns 5 and 6), and by gender (Columns 7 and 8)). All specifications include control variables for whether the secondary school is on the same island, whether it is all-girls, whether it is all-boys, the estimated likelihoods of school rejection only when comparing the top choice with all unranked schools, and interactions of these likelihoods with all school attributes. The pooled means are computed by taking the average across the point-estimates of all cells in a subgroup. Assuming independence across cells, the pooled standard error is computed by summing all subgroup variances, dividing by the number of cells in the subgroup, and taking the squared-root of the ratio. The pooled standard errors are presented in parentheses below each pooled average estimate.

Appendix N: Choice Model with Out of Sample Impacts and Averages

This appendix reports estimates from the same choice models described in the text but using leave-year-out school impacts and average outcomes. That is, for each SEA cohort, we estimate schools' causal impacts and compute average outcomes disregarding the information of the cohort for which these calculations are being implemented.
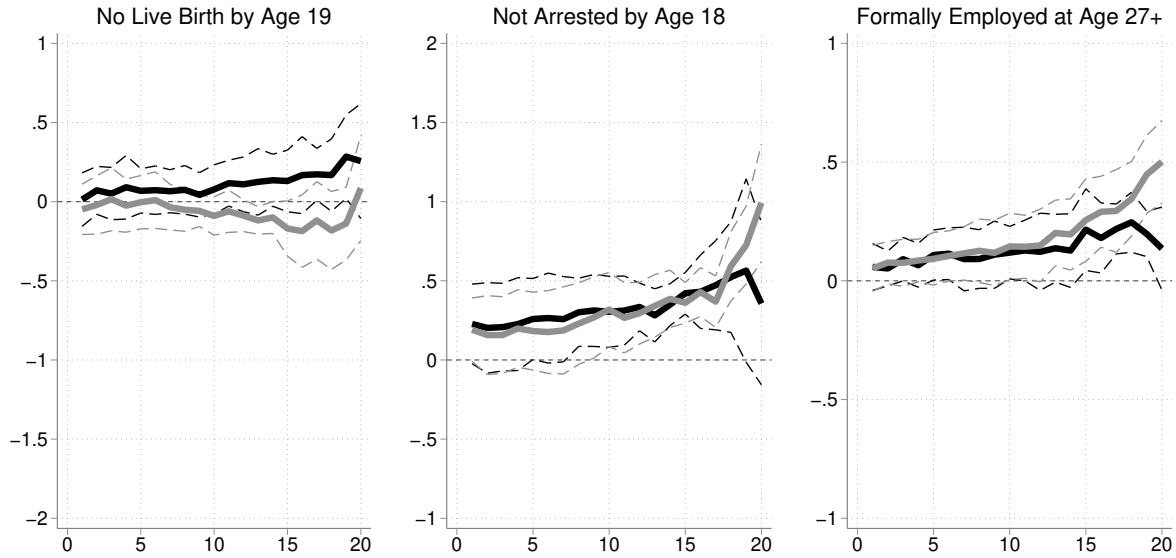
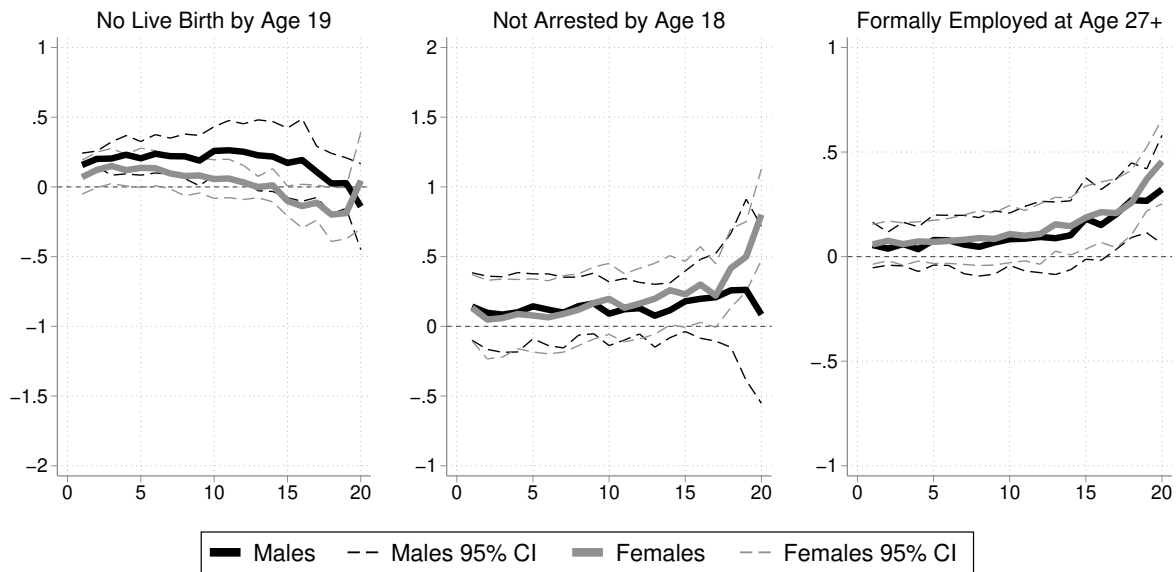Figure N1. Academic Outcomes using Out of Sample Estimated Impacts and Average Outcomes

(a) Causal Impact (Impacts Only Model)

(b) Causal Impact (Full Model)

Notes: The X-axis represent the individual SEA score ventile. The connected lines represent the estimated coefficients, computed separately for each (SEA score ventile)×(gender) cell, for two main models: Panel (a) displays estimates from the Impacts Only Model using out of sample estimates of schools' causal impacts for all outcomes, peer quality, and log distance and Panel (b) displays estimates from the Full Model using out of sample estimates of schools' causal impacts for all outcomes, out of sample school-level averages for all outcomes, peer quality, and log distance. All specifications include control variables for whether the secondary school is on the same island, whether it is all-girls, whether it is all-boys, the estimated likelihoods of school rejection only when comparing the top choice with all unranked schools, and interactions of these likelihoods with all school attributes. The dashed lines represent the associated 95% confidence intervals.

Figure N2. Non-Academic Outcomes using Out of Sample Estimated Impacts and Average Outcomes

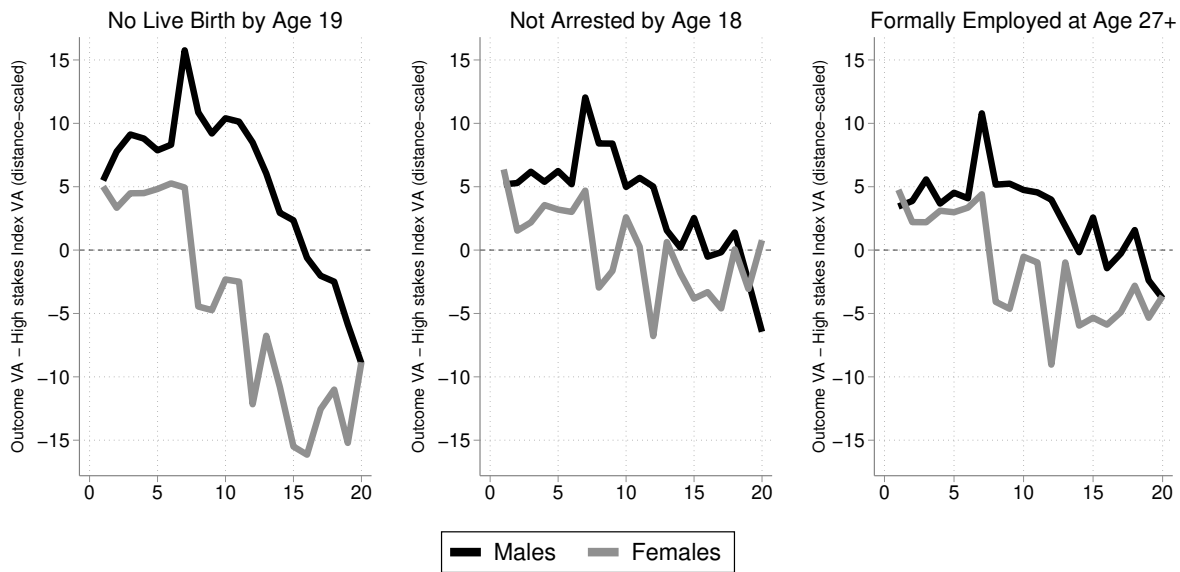(a) Causal Impact (Impacts Only Model)

(b) Causal Impact (Full Model)

Notes: The X-axis represent the individual SEA score ventile. The connected lines represent the estimated coefficients, computed separately for each (SEA score ventile)×(gender) cell, for two main models: Panel (a) displays estimates from the Impacts Only Model using out of sample estimates of schools' causal impacts for all outcomes, peer quality, and log distance and Panel (b) displays estimates from the Full Model using out of sample estimates of schools' causal impacts for all outcomes, out of sample school-level averages for all outcomes, peer quality, and log distance. All specifications include control variables for whether the secondary school is on the same island, whether it is all-girls, whether it is all-boys, the estimated likelihoods of school rejection only when comparing the top choice with all unranked schools, and interactions of these likelihoods with all school attributes. The dashed lines represent the associated 95% confidence intervals.

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Figure N3. Comparison of Choice Model's Estimated Coefficients (Out of Sample Estimated Impacts and Average Outcomes)

Notes: This figure presents the difference between the choice model's estimates on the school impacts of three non-academic outcomes and the choice model's estimate on the school impacts of the high-stakes index, scaled by the log distance estimate. The X-axis represents the individual score ventile. The connected lines represent the difference between the choice model estimate on the non-academic impacts and the choice model estimate on the high-stakes impacts divided by the log distance cell estimate (and scaled by 6 for ease of interpretation). This difference is computed separately for each (SEA score ventile)×(gender) cell. The estimates result from the Full Model using out of sample estimates, which includes schools' causal impact estimates for all outcomes, the school-level averages for all outcomes, peer quality and log distance, control variables (whether the secondary school is on the same island, whether it is all-girls, whether it is all boys), the estimated likelihoods of school rejection only when comparing the top choice with all unranked schools, and interactions of these likelihoods with all school attributes.

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