The Short and Long-Run Effects of Attending The Schools that Parents Prefer
ONLINE APPENDIX*

Diether W. Beuermann
C. Kirabo Jackson

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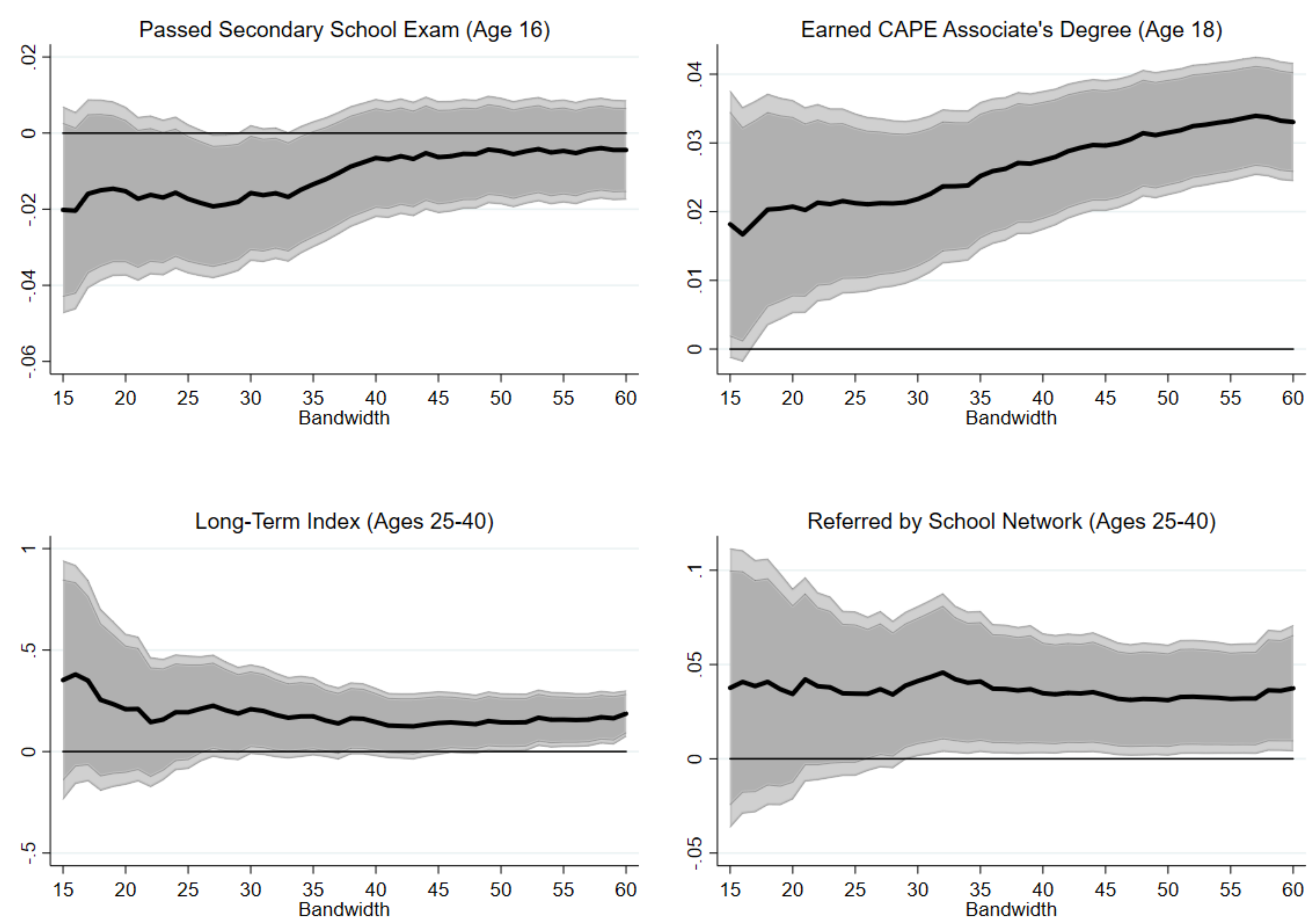
*Diether W. Beuermann is a Senior Research Economist at the Inter-American Development Bank, 1300 New York Avenue, NW, Washington DC 20577 (e-mail: dietherbe@iadb.org). C. Kirabo Jackson is a Abraham Harris Professor of Education and Social Policy Faculty Fellow at Northwestern University, 2120 Campus Drive, Evanston IL 60208 (e-mail: kirabo-jackson@northwestern.edu).

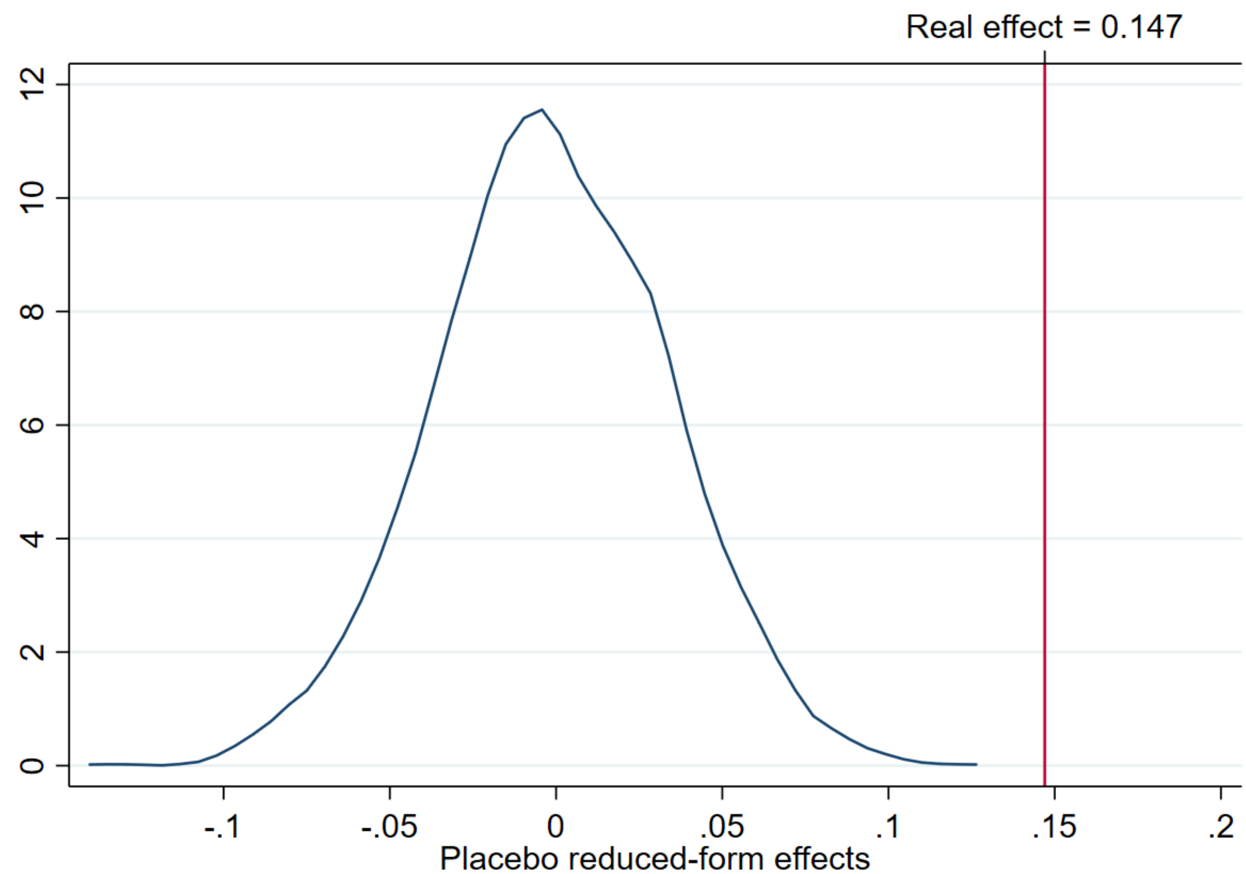
Appendix A. Appendix Figures and Tables

Figure A1. 2SLS Effects by Bandwidth



Notes: This figure depicts estimated 2SLS coefficients on 'Attend' a preferred school using 'Above' as the excluded instrument (resulting from equation system (1) - (2) in the text). The estimated 2SLS effects are reported for each bandwidth between +/-15 (+/-0.6sd) and +/-60 (+/-2.5sd). The 90 (95) percent confidence interval of the estimated effects is presented in dark (light) gray.

Figure A2. Cutoff Falsification Test (Long-Term Index)



Notes: This figure depicts the distribution of 2,000 placebo estimated coefficients on the 'Above' indicator resulting from reduced-form models as in equation (1) along with the real estimated reduced-form effect on the Long-Term Index (equivalent to 0.147 standard deviations with p-value$<$0.01). Each placebo estimate was generated with the following steps: (1) A randomly chosen cutoff admission score was generated for each applicant pool (i.e. applicants to each school-year); (2) A placebo relative score and 'Above' indicator were generated with respect to the randomly chosen admission cutoff; (3) A reduced form model was estimated using the placebo relative scores and the 'Above' indicator.

Table A1: Survey Representativeness

Survey Status:	Not Surveyed	Matched Face to Face to Survey	(1) = (2)
	(1)	(2)	(3)
Panel A: Sociodemographics			
Female	0.497	0.483	0.346
	(0.500)	(0.500)	
Month of birth: Jan - Mar	0.241	0.239	0.814
	(0.428)	(0.424)	
Month of birth: Apr - Jun	0.218	0.210	0.604
	(0.413)	(0.410)	
Month of birth: Jul - Sep	0.249	0.248	0.913
	(0.433)	(0.433)	
Month of birth: Oct - Dec	0.292	0.303	0.432
	(0.454)	(0.459)	
Panel B: Selectivity of Secondary School Choices (BSSEE score of incoming class)			
Choice 1	1.164	1.124	0.188
	(0.622)	(0.622)	
Choice 2	0.934	0.898	0.408
	(0.669)	(0.681)	
Choice 3	0.891	0.856	0.175
	(0.587)	(0.604)	
Choice 4	0.611	0.562	0.218
	(0.621)	(0.637)	
Choice 5	0.334	0.294	0.779
	(0.660)	(0.652)	
Choice 6	0.078	0.029	0.908
	(0.724)	(0.699)	
Choice 7	-0.151	-0.214	0.982
	(0.771)	(0.761)	
Choice 8	-0.272	-0.331	0.476
	(0.844)	(0.854)	
Choice 9	-0.369	-0.432	0.795
	(0.889)	(0.896)	
Panel C: Parish of Residency (before admission to secondary school)			
Parish 1	0.021	0.015	0.062
	(0.144)	(0.177)	
Parish 2	0.042	0.047	0.257
	(0.200)	(0.248)	
Parish 3	0.066	0.071	0.532
	(0.248)	(0.257)	

cont'd. Table A1: Survey Representativeness

Parish 4	0.036	0.031	0.225
	(0.188)	(0.208)	
Parish 5	0.036	0.047	0.052
	(0.186)	(0.226)	
Parish 6	0.363	0.354	0.763
	(0.481)	(0.451)	
Parish 7	0.023	0.026	0.411
	(0.149)	(0.192)	
Parish 8	0.087	0.085	0.703
	(0.281)	(0.291)	
Parish 9	0.077	0.088	0.189
	(0.267)	(0.287)	
Parish 10	0.047	0.045	0.784
	(0.211)	(0.236)	
Parish 11	0.198	0.190	0.268
	(0.399)	(0.372)	
Panel D: CSEC Performance (after 5 years of secondary school)			
Took at least 1 subject	0.676	0.692	0.232
	(0.468)	(0.436)	
Number of subjects taken	3.036	3.268	0.120
	(3.328)	(3.291)	
Number of subjects passed	2.188	2.282	0.633
	(2.912)	(2.809)	
Qualified for tertiary	0.268	0.285	0.456
	(0.443)	(0.445)	
Individuals	93,846	1,545	
Panel E: CAPE Performance (after 2 years of post-secondary studies)			
Took at least 1 unit	0.152	0.159	0.731
	(0.359)	(0.351)	
Number of units taken	0.944	1.003	0.714
	(2.467)	(2.359)	
Number of units passed	0.866	0.906	0.830
	(2.340)	(2.220)	
Earned Associate's Degree	0.080	0.085	0.884
	(0.272)	(0.261)	
Individuals	43,192	792	

Notes: All individuals who took the BSSEE between 1987 and 2011 are included in panels A, B, C, and D. However, panel E only includes individuals who took the BSSEE between 1998 and 2009. This because the earliest CAPE outcome data corresponds to year 2005 which is associated with the 1998 BSSEE cohort; while the latest CAPE data corresponds to year 2016 which is associated with the 2009 BSSEE cohort. Standard deviations are reported in parentheses below the means. Column (1) reports means and standard deviations of individuals who were not surveyed. Column (2) reports means and standard deviations of individuals who were surveyed and matched with the BSSEE administrative dataset. Estimates in column (2) are weighted by the inverse of sampling probability to reflect survey design. Column (3) reports the p-value of a test for the equality of means reported in columns (1) and (2) adjusting for BSSEE cohorts fixed effects.

Table A2: 2SLS Effects on Secondary and Tertiary Examinations - Alternative Samples

Estimation Sample:	All		Women		Men		(4) = (6)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: CSEC Performance. Sample: BSSEE cohorts 1987 - 2002 (Administrative data +/- 0.75 SD from cutoff)							
Took at least 1 subject	-0.02	-0.019	-0.009	-0.008	-0.03	-0.029	0.279
	(0.010)	(0.010)	(0.013)	(0.013)	(0.015)	(0.015)	
Number of subjects taken	-0.121	-0.116	-0.014	-0.006	-0.237	-0.237	0.057
	(0.061)	(0.061)	(0.086)	(0.086)	(0.086)	(0.086)	
Number of subjects passed	-0.071	-0.069	0.005	0.006	-0.162	-0.16	0.110
	(0.052)	(0.052)	(0.076)	(0.076)	(0.071)	(0.071)	
Qualified for tertiary	-0.004	-0.004	0.003	0.002	-0.012	-0.011	0.440
	(0.009)	(0.009)	(0.013)	(0.013)	(0.012)	(0.012)	
Observations	184,648	184,648	94,385	94,385	90,263	90,263	
Panel B: CSEC Performance. Sample: BSSEE cohorts 1987 - 2002 (Full matched survey observations)							
Took at least 1 subject	0.039	0.041	0.124	0.072	-0.048	0.010	0.358
	(0.045)	(0.037)	(0.062)	(0.045)	(0.064)	(0.052)	
Number of subjects taken	-0.174	-0.005	0.301	0.330	-0.582	-0.287	0.167
	(0.266)	(0.230)	(0.347)	(0.286)	(0.389)	(0.344)	
Number of subjects passed	-0.049	-0.022	0.463	0.299	-0.481	-0.286	0.134
	(0.239)	(0.199)	(0.327)	(0.272)	(0.336)	(0.277)	
Qualified for tertiary	0.021	0.028	0.066	0.020	-0.020	0.036	0.835
	(0.044)	(0.036)	(0.062)	(0.053)	(0.062)	(0.051)	
Observations	5,610	5,610	2,616	2,616	2,994	2,994	
Panel C: CAPE Performance. Sample: BSSEE cohorts 1998 - 2002 (Administrative data +/- 0.75 SD from cutoff)							
Took at least 1 unit	0.014	0.016	0.007	0.008	0.021	0.022	0.609
	(0.013)	(0.013)	(0.021)	(0.021)	(0.016)	(0.016)	
Number of units taken	0.153	0.158	0.078	0.076	0.217	0.228	0.351
	(0.081)	(0.081)	(0.132)	(0.132)	(0.096)	(0.096)	
Number of units passed	0.13	0.135	0.071	0.067	0.178	0.19	0.424
	(0.076)	(0.076)	(0.125)	(0.126)	(0.089)	(0.089)	
Earned Associate's Degree	0.014	0.014	0.017	0.016	0.010	0.011	0.750
	(0.009)	(0.009)	(0.015)	(0.015)	(0.010)	(0.010)	
Observations	45,885	45,885	23,306	23,306	22,579	22,579	
Sociodemographics	Yes	Yes	Yes	Yes	Yes	Yes	
BSSEE cubic spline	Yes	Yes	Yes	Yes	Yes	Yes	
Cutoff fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	
Preferences fixed effects	No	Yes	No	Yes	No	Yes	

Notes: This table reports estimated 2SLS coefficients on 'Attend' a preferred school using 'Above' as the excluded instrument (resulting from equation system (1) - (2) in the text). Estimated standard errors in parenthesis are clustered at the individual level. Sociodemographic controls include student gender and parish fixed-effects. Panel B shows estimated effects using only observations that belong to the matched survey data and regressions are weighted by the inverse of sampling probability to reflect survey design. Column (7) reports the p-value of a test for the equality of estimates reported in columns (4) and (6).

Table A3: Reduced Form Estimates on Baseline Characteristics

Estimation Sample:	Administrative Data: +/- 0.75 SD from cutoff			Matched Face to Face Survey		
	All	Women	Men	All	Women	Men
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Month of Birth						
January	0.008	-0.004	0.022	-0.016	-0.038	-0.008
	(0.005)	(0.007)	(0.007)	(0.020)	(0.022)	(0.026)
February	-0.007	0.002	-0.016	-0.032	-0.012	-0.027
	(0.004)	(0.006)	(0.006)	(0.024)	(0.027)	(0.027)
March	-0.001	0.003	-0.006	0.032	-0.014	0.043
	(0.004)	(0.006)	(0.007)	(0.022)	(0.021)	(0.030)
April	<0.001	0.005	-0.005	0.013	-0.019	0.037
	(0.004)	(0.006)	(0.006)	(0.023)	(0.033)	(0.022)
May	-0.001	-0.004	0.001	0.010	0.031	-0.028
	(0.005)	(0.006)	(0.007)	(0.022)	(0.027)	(0.023)
June	0.004	<0.001	0.009	0.007	0.007	-0.020
	(0.004)	(0.006)	(0.006)	(0.022)	(0.027)	(0.022)
July	-0.003	-0.001	-0.005	0.011	0.002	0.020
	(0.004)	(0.006)	(0.006)	(0.022)	(0.025)	(0.024)
August	0.002	0.005	-0.002	-0.007	0.014	-0.007
	(0.005)	(0.006)	(0.007)	(0.018)	(0.023)	(0.023)
September	0.001	-0.003	0.006	0.014	-0.014	0.026
	(0.005)	(0.007)	(0.007)	(0.021)	(0.022)	(0.024)
October	-0.001	0.007	-0.009	-0.025	-0.004	-0.048
	(0.005)	(0.007)	(0.007)	(0.023)	(0.027)	(0.026)
November	0.001	-0.009	0.011	-0.016	0.024	-0.007
	(0.005)	(0.007)	(0.007)	(0.020)	(0.025)	(0.025)
December	-0.003	-0.001	-0.005	0.008	0.023	0.019
	(0.005)	(0.007)	(0.007)	(0.025)	(0.028)	(0.030)
Panel B: Selectivity of Secondary School Choices (BSSEE score of incoming class)						
Choice 1	0.001	0.001	0.001	-0.002	0.001	0.003
	(0.001)	(0.001)	(0.001)	(0.004)	(0.004)	(0.005)
Choice 2	<0.001	0.001	-0.001	0.001	<0.001	-0.013
	(0.001)	(0.001)	(0.001)	(0.004)	(0.003)	(0.005)
Choice 3	<0.001	-0.001	<0.001	0.007	0.006	0.004
	(0.001)	(0.001)	(0.001)	(0.004)	(0.004)	(0.005)
Choice 4	-0.002	-0.001	-0.002	0.002	0.003	-0.000
	(0.001)	(0.002)	(0.002)	(0.005)	(0.005)	(0.006)

cont'd. Table A3: Reduced Form Estimates on Baseline Characteristics

Choice 5	<0.001	0.001	-0.001	-0.008	-0.012	-0.01
	(0.001)	(0.002)	(0.002)	(0.006)	(0.007)	(0.005)
Choice 6	-0.002	-0.002	-0.001	0.001	0.002	-0.005
	(0.001)	(0.002)	(0.002)	(0.006)	(0.005)	(0.007)
Choice 7	-0.001	<0.001	-0.002	0.011	0.017	-0.002
	(0.002)	(0.002)	(0.002)	(0.007)	(0.006)	(0.006)
Choice 8	0.001	<0.001	0.002	<0.001	-0.002	0.004
	(0.002)	(0.002)	(0.002)	(0.006)	(0.008)	(0.006)
Choice 9	-0.002	-0.003	-0.001	0.012	0.019	0.011
	(0.002)	(0.003)	(0.003)	(0.007)	(0.007)	(0.008)
Panel C: Parish of Residency (before admission to secondary school)						
Parish 1	<0.001	<0.001	<0.001	-0.004	<0.001	-0.008
	(0.001)	(0.001)	(0.002)	(0.003)	(<0.001)	(0.005)
Parish 2	<0.001	<0.001	<0.001	-0.002	<0.001	-0.007
	(0.001)	(0.001)	(0.002)	(0.002)	(<0.001)	(0.005)
Parish 3	<0.001	<0.001	0.001	-0.002	<0.001	-0.008
	(0.001)	(0.001)	(0.002)	(0.002)	(<0.001)	(0.005)
Parish 4	<0.001	<0.001	-0.001	-0.003	<0.001	-0.003
	(0.001)	(0.001)	(0.002)	(0.003)	(<0.001)	(0.004)
Parish 5	<0.001	<0.001	0.001	-0.003	<0.001	-0.008
	(0.001)	(0.001)	(0.002)	(0.003)	(<0.001)	(0.005)
Parish 6	<0.001	<0.001	<0.001	-0.003	<0.001	-0.007
	(0.001)	(0.001)	(0.002)	(0.003)	(<0.001)	(0.005)
Parish 7	-0.001	<0.001	-0.001	-0.003	<0.001	-0.008
	(0.001)	(0.001)	(0.002)	(0.002)	(<0.001)	(0.005)
Parish 8	<0.001	<0.001	<0.001	-0.004	<0.001	-0.010
	(0.001)	(0.001)	(0.002)	(0.003)	(<0.001)	(0.006)
Parish 9	<0.001	<0.001	0.001	-0.003	<0.001	-0.009
	(0.001)	(0.001)	(0.002)	(0.003)	(<0.001)	(0.006)
Parish 10	-0.001	<0.001	-0.001	-0.002	<0.001	-0.006
	(0.001)	(0.001)	(0.002)	(0.002)	(<0.001)	(0.004)
Parish 11	<0.001	<0.001	<0.001	-0.003	<0.001	-0.009
	(0.001)	(0.001)	(0.002)	(0.003)	(<0.001)	(0.006)
BSSEE cubic spline	Yes	Yes	Yes	Yes	Yes	Yes
Cutoff fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	184,648	94,385	90,263	5,610	2,616	2,994

Notes: This table reports estimated coefficients on the 'Above' indicator resulting from reduced form models as in equation (1) of the text. Estimated standard errors in parenthesis are clustered at the individual level. Sample corresponds to BSSEE cohorts 1987 - 2002 (25 - 40 years old when surveyed). Regressions in columns 4-6 are weighted by the inverse of sampling probability to reflect survey design.

Table A4: Reduced Form Estimates on Predicted Outcomes

	All	Women	Men	(2) = (3)	Prediction R2
	(1)	(2)	(3)	(4)	(5)
Panel A: Predicted CSEC Performance. BSSEE cohorts 1998 - 2009 (Administrative data +/- 0.75 SD from cutoff)					
Predicted: Took at least 1 subject	$<$0.001	$<$0.001	-0.001	0.978	0.21
	(0.002)	(0.003)	(0.004)		
Predicted: Number of subjects taken	-0.014	-0.008	-0.013	0.883	0.26
	(0.016)	(0.025)	(0.020)		
Predicted: Number of subjects passed	-0.013	-0.009	-0.010	0.951	0.21
	(0.013)	(0.021)	(0.016)		
Predicted: Qualified for tertiary	-0.001	-0.001	-0.001	0.864	0.12
	(0.002)	(0.003)	(0.002)		
Observations	106,701	54,649	52,052		
Panel B: Predicted CAPE Performance. BSSEE cohorts 1998 - 2009 (Administrative data +/- 0.75 SD from cutoff)					
Predicted: Took at least 1 unit	-0.001	$<$0.001	-0.001	0.834	0.08
	(0.001)	(0.002)	(0.001)		
Predicted: Number of units taken	-0.006	-0.004	-0.005	0.948	0.08
	(0.007)	(0.010)	(0.009)		
Predicted: Number of units passed	-0.005	-0.004	-0.004	0.965	0.08
	(0.006)	(0.009)	(0.008)		
Predicted: Earned Associate's Degree	$<$0.001	$<$0.001	$<$0.001	0.960	0.05
	(0.001)	(0.001)	(0.001)		
Observations	106,701	54,649	52,052		
Panel C: Predicted Indexes. BSSEE cohorts 1987 - 2002 (25 - 40 Years old at Survey - all observations)					
Predicted: Long-term index	0.019	0.013	0.019	0.885	0.22
	(0.019)	(0.030)	(0.023)		
Predicted: Educational attainment index	0.029	0.023	0.026	0.949	0.23
	(0.030)	(0.043)	(0.039)		
Predicted: Labor market index	0.004	0.022	-0.019	0.398	0.17
	(0.025)	(0.034)	(0.035)		
Predicted: Health index	0.027	0.006	0.048	0.258	0.17
	(0.019)	(0.027)	(0.025)		
Observations	5,598	2,613	2,985		
BSSEE cubic spline	Yes	Yes	Yes		
Cutoff fixed effects	Yes	Yes	Yes		

Notes: This table reports estimated coefficients on the 'Above' indicator resulting from reduced form models as in equation (1) of the text. Estimated standard errors in parenthesis are clustered at the individual level. Samples in Panels A and B correspond to BSSEE cohorts that have both CSEC and CAPE data available (BSSEE cohorts 1998 - 2009). This because the earliest CAPE outcome data corresponds to year 2005 which is associated with the 1998 BSSEE cohort; while the latest CAPE data corresponds to year 2016 which is associated with the 2009 BSSEE cohort. Panel C shows estimated effects on predicted longer term indexes obtained from the matched survey data covering BSSEE cohorts 1987-2002 (25-40 years old when surveyed). Regressions do not control for preferences as the selectivity of preferences were used when predicting the outcomes. Regressions in Panel C are weighted by the inverse of sampling probability to reflect survey design. Column (4) reports the p-value of a test for the equality of estimates reported in columns (2) and (3). Column (5) reports the adjusted coefficient of determination of the prediction regression for each outcome.

Table A5: 2SLS Effects on CSEC and CAPE Outcomes

	All		Women	Men	(3) = (4)
	(1)	(2)	(3)	(4)	(5)
Panel A: CSEC Performance. Sample: BSSEE cohorts 1998 - 2009 (Administrative data +/- 0.75 SD from cutoff)					
Took at least 1 subject	0.002	0.000	0.005	-0.005	0.592
	(0.010)	(0.010)	(0.013)	(0.015)	
Number of subjects taken	-0.128	-0.129	0.077	-0.314	0.007
	(0.072)	(0.072)	(0.105)	(0.099)	
Number of subjects passed	-0.027	-0.025	0.108	-0.146	0.035
	(0.060)	(0.060)	(0.091)	(0.078)	
Qualified for tertiary	-0.017	-0.017	-0.019	-0.014	0.788
	(0.010)	(0.010)	(0.017)	(0.013)	
Observations	106,701	106,701	54,649	52,052	
Panel B: CAPE Performance. Sample: BSSEE cohorts 1998 - 2009 (Administrative data +/- 0.75 SD from cutoff)					
Took at least 1 unit	0.019	0.019	0.018	0.019	0.980
	(0.009)	(0.009)	(0.015)	(0.011)	
Number of units taken	0.201	0.196	0.164	0.226	0.624
	(0.062)	(0.062)	(0.103)	(0.071)	
Number of units passed	0.195	0.191	0.17	0.21	0.739
	(0.059)	(0.059)	(0.098)	(0.066)	
Earned Associate's Degree	0.021	0.021	0.026	0.016	0.476
	(0.007)	(0.007)	(0.012)	(0.008)	
Observations	106,701	106,701	54,649	52,052	
Sociodemographics	Yes	Yes	Yes	Yes	
BSSEE cubic spline	Yes	Yes	Yes	Yes	
Cutoff fixed effects	Yes	Yes	Yes	Yes	
Preferences fixed effects	No	Yes	Yes	Yes	

Notes: This table reports estimated 2SLS coefficients on 'Attend' a preferred school using 'Above' as the excluded instrument (resulting from equation system (1) - (2) in the text). Estimated standard errors in parenthesis are clustered at the individual level. Sociodemographic controls include student gender and parish fixed-effects. The population corresponds to BSSEE cohorts that have both CSEC and CAPE data available (BSSEE cohorts 1998 - 2009). Column (5) reports the p-value of a test for the equality of estimates reported in columns (3) and (4).

Table A6: 2SLS Effects on Educational Attainment and Labor Market Indicators

BSSEE Cohorts:	1987 - 2002: 25 - 40 Years old at Survey (Full Matched Data)						
	All		Women		Men		(4) = (6)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Educational Attainment							
Years of education	0.615	0.677	1.409	1.644	-0.189	-0.238	0.015
	(0.386)	(0.382)	(0.573)	(0.575)	(0.529)	(0.514)	
University degree	0.043	0.045	0.168	0.174	-0.063	-0.066	0.006
	(0.044)	(0.043)	(0.070)	(0.070)	(0.052)	(0.052)	
Observations	5,277	5,277	2,510	2,510	2,767	2,767	
Panel B: Main Occupation							
Employed	0.097	0.101	0.14	0.133	0.065	0.076	0.582
	(0.054)	(0.051)	(0.074)	(0.067)	(0.078)	(0.076)	
Unemployed	-0.022	-0.039	-0.11	-0.126	0.052	0.037	0.043
	(0.044)	(0.040)	(0.063)	(0.055)	(0.061)	(0.057)	
Out of labor force	-0.075	-0.062	-0.030	-0.007	-0.117	-0.114	0.180
	(0.040)	(0.039)	(0.054)	(0.054)	(0.058)	(0.057)	
Observations	5,610	5,610	2,616	2,616	2,994	2,994	
Panel C: Employment Quality (only employed persons)							
Manager or professional	0.057	0.056	0.248	0.246	-0.094	-0.088	0.002
	(0.050)	(0.051)	(0.086)	(0.087)	(0.058)	(0.059)	
Log monthly wage	0.176	0.158	0.42	0.413	-0.015	-0.043	0.027
	(0.101)	(0.105)	(0.172)	(0.173)	(0.119)	(0.123)	
Observations	4,048	4,048	1,774	1,774	2,274	2,274	
Sociodemographics	Yes	Yes	Yes	Yes	Yes	Yes	
BSSEE cubic spline	Yes	Yes	Yes	Yes	Yes	Yes	
Cutoff fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	
Preferences fixed effects	No	Yes	No	Yes	No	Yes	

Notes: This table reports estimated 2SLS coefficients on 'Attend' a preferred school using 'Above' as the excluded instrument (resulting from equation system (1) - (2) in the text). Estimated standard errors in parenthesis are clustered at the individual level. Sociodemographic controls include student gender and parish fixed-effects. Regressions are weighted by the inverse of sampling probability to reflect survey design. Column (7) reports the p-value of a test for the equality of estimates reported in columns (4) and (6).

Table A7: 2SLS Effects on Health Indicators

BSSEE Cohorts:	1987 - 2002: 25 - 40 Years old at Survey (Full Matched Data)						
	All		Women		Men		(4) = (6)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Preventive Health Behaviors							
Weekly gym	0.117	0.125	0.14	0.136	0.092	0.107	0.706
	(0.040)	(0.040)	(0.055)	(0.054)	(0.058)	(0.056)	
Medical insurance	0.027	0.028	0.136	0.102	-0.065	-0.035	0.190
	(0.054)	(0.052)	(0.079)	(0.078)	(0.073)	(0.068)	
Yearly dental checkup	0.107	0.111	0.006	0.043	0.2	0.176	0.237
	(0.058)	(0.058)	(0.083)	(0.080)	(0.081)	(0.081)	
Observations	5,610	5,610	2,616	2,616	2,994	2,994	
Panel B: Objective Health Outcomes (based on BMI)							
Normal weight	0.183	0.167	0.186	0.2	0.19	0.129	0.593
	(0.068)	(0.067)	(0.098)	(0.098)	(0.093)	(0.087)	
Overweight or Obese	-0.145	-0.141	-0.171	-0.191	-0.126	-0.082	0.397
	(0.067)	(0.066)	(0.097)	(0.095)	(0.090)	(0.085)	
Underweight	-0.038	-0.026	-0.015	-0.009	-0.064	-0.047	0.440
	(0.026)	(0.024)	(0.038)	(0.038)	(0.036)	(0.030)	
Observations	4,361	4,361	2,146	2,146	2,215	2,215	
Sociodemographics	Yes	Yes	Yes	Yes	Yes	Yes	
BSSEE cubic spline	Yes	Yes	Yes	Yes	Yes	Yes	
Cutoff fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	
Preferences fixed effects	No	Yes	No	Yes	No	Yes	

Notes: This table reports estimated 2SLS coefficients on 'Attend' a preferred school using 'Above' as the excluded instrument (resulting from equation system (1) - (2) in the text). Estimated standard errors in parenthesis are clustered at the individual level. Sociodemographic controls include student gender and parish fixed-effects. Regressions are weighted by the inverse of sampling probability to reflect survey design. Column (7) reports the p-value of a test for the equality of estimates reported in columns (4) and (6).

Table A8: Sensitivity of 2SLS Effects on Main Outcomes to Alternative BSSEE Polynomial Specifications

	All			Women			Men		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: CSEC Performance. Sample: BSSEE cohorts 1998 - 2009 (Administrative data +/- 0.75 SD from cutoff)									
Took at least 1 subject	-0.02	-0.014	0.000	-0.016	-0.007	0.005	-0.025	-0.019	-0.005
	(0.004)	(0.007)	(0.010)	(0.005)	(0.009)	(0.013)	(0.007)	(0.011)	(0.015)
Qualified for tertiary	0.004	-0.012	-0.017	0.004	-0.018	-0.019	0.004	-0.007	-0.014
	(0.004)	(0.007)	(0.010)	(0.006)	(0.011)	(0.017)	(0.006)	(0.009)	(0.013)
Panel B: CAPE Performance. Sample: BSSEE cohorts 1998 - 2009 (Administrative data +/- 0.75 SD from cutoff)									
Took at least 1 unit	0.058	0.031	0.019	0.065	0.039	0.018	0.053	0.022	0.019
	(0.004)	(0.006)	(0.009)	(0.006)	(0.010)	(0.015)	(0.005)	(0.008)	(0.011)
Earned Associate's Degree	0.049	0.028	0.021	0.061	0.041	0.026	0.037	0.015	0.016
	(0.003)	(0.005)	(0.007)	(0.004)	(0.008)	(0.012)	(0.003)	(0.005)	(0.008)
Panel C: Survey Sample BSSEE Cohorts 1987 - 2002 (25 - 40 years old when surveyed - all observations)									
Long-term index	0.121	0.167	0.187	0.161	0.274	0.298	0.084	0.071	0.089
	(0.032)	(0.042)	(0.057)	(0.050)	(0.059)	(0.085)	(0.041)	(0.058)	(0.078)
Educational attainment index	0.179	0.187	0.108	0.227	0.383	0.376	0.136	0.012	-0.129
	(0.049)	(0.067)	(0.092)	(0.074)	(0.100)	(0.144)	(0.066)	(0.088)	(0.115)
Labor market index	0.106	0.124	0.181	0.219	0.31	0.389	0.002	-0.045	0.000
	(0.045)	(0.062)	(0.082)	(0.067)	(0.086)	(0.121)	(0.062)	(0.090)	(0.116)
Health index	0.072	0.148	0.217	0.070	0.175	0.207	0.077	0.126	0.224
	(0.041)	(0.047)	(0.060)	(0.067)	(0.070)	(0.088)	(0.047)	(0.063)	(0.082)
BSSEE polynomial order	1	2	3	1	2	3	1	2	3

Notes: This table reports estimated 2SLS coefficients on 'Attend' a preferred school using 'Above' as the excluded instrument (resulting from equation system (1) - (2) in the text) using alternative polynomial specifications of the BSSEE relative score. Estimated standard errors in parenthesis are clustered at the individual level. Sociodemographic controls include student gender and parish fixed-effects. All regressions include interactions between the BSSEE polynomial and the the 'Above' indicator, cutoff fixed effects and preference fixed effects. Samples in Panels A and B correspond to BSSEE cohorts that have both CSEC and CAPE data available (BSSEE cohorts 1998 - 2009). Panel C uses the full matched survey data covering BSSEE cohorts 1987-2002 (25 - 40 years old when surveyed) and these regressions are weighted by the inverse of sampling probability to reflect survey design. Summary Indexes are expressed in standard deviations and combine the following outcomes: Educational attainment (years of education, university degree); Labor market (employed, manager or professional, monthly wage); Health (medical insurance, yearly dental checkup, weekly gym, normal BMI); Long-term (all outcomes included in the previous indexes).

Table A9: 2SLS Effects on Main Outcomes (two-way clustering at the individual and BSSEE score levels)

	All		Women		Men		(4) = (6)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: CSEC Performance. Sample: BSSEE cohorts 1998 - 2009 (Administrative data +/- 0.75 SD from cutoff)							
Took at least 1 subject	0.002	0.000	0.006	0.005	-0.003	-0.005	0.602
	(0.010)	(0.010)	(0.013)	(0.012)	(0.015)	(0.015)	
Qualified for tertiary	-0.017	-0.017	-0.019	-0.019	-0.015	-0.014	0.792
	(0.010)	(0.010)	(0.016)	(0.016)	(0.013)	(0.013)	
Observations	106,701	106,701	54,649	54,649	52,052	52,052	
Panel B: CAPE Performance. Sample: BSSEE cohorts 1998 - 2009 (Administrative data +/- 0.75 SD from cutoff)							
Took at least 1 unit	0.019	0.019	0.019	0.018	0.02	0.019	0.98
	(0.010)	(0.010)	(0.015)	(0.016)	(0.012)	(0.012)	
Earned Associate Degree	0.021	0.021	0.026	0.026	0.016	0.016	0.48
	(0.007)	(0.007)	(0.012)	(0.012)	(0.008)	(0.008)	
Observations	106,701	106,701	54,649	54,649	52,052	52,052	
Panel C: Survey Sample BSSEE Cohorts 1987 - 2002 (25 - 40 years old when surveyed - all observations)							
Long-term index	0.189	0.187	0.285	0.298	0.101	0.089	0.027
	(0.052)	(0.052)	(0.061)	(0.058)	(0.081)	(0.081)	
Educational attainment index	0.101	0.108	0.341	0.376	-0.118	-0.129	0.009
	(0.090)	(0.089)	(0.142)	(0.140)	(0.123)	(0.123)	
Labor market index	0.17	0.181	0.4	0.389	-0.030	<0.001	0.007
	(0.074)	(0.068)	(0.097)	(0.085)	(0.117)	(0.113)	
Health index	0.221	0.217	0.219	0.207	0.224	0.224	0.876
	(0.058)	(0.056)	(0.071)	(0.069)	(0.088)	(0.084)	
Observations	5,610	5,610	2,616	2,616	2,994	2,994	
Sociodemographics	Yes	Yes	Yes	Yes	Yes	Yes	
BSSEE cubic spline	Yes	Yes	Yes	Yes	Yes	Yes	
Cutoff fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	
Preferences fixed effects	No	Yes	No	Yes	No	Yes	

Notes: This table reports estimated 2SLS coefficients on 'Attend' a preferred school using 'Above' as the excluded instrument (resulting from equation system (1) - (2) in the text). Estimated standard errors in parenthesis are two-way clustered at the individual and BSSEE score levels. Sociodemographic controls include student gender and parish fixed-effects. Regressions in Panel C are weighted by the inverse of sampling probability to reflect survey design. Column (7) reports the p-value of a test for the equality of estimates reported in columns (4) and (6). Summary Indexes are expressed in standard deviations and combine the following outcomes: Educational attainment (years of education, university degree); Labor market (employed, manager or professional, monthly wage); Health (medical insurance, yearly dental checkup, weekly gym, normal BMI); Long-term (all outcomes included in the previous indexes).

Table A10: 2SLS Effects on Fertility

Sample	Women	
	(1)	(2)
Panel A: Sample: BSSEE cohorts 1987 - 2002 (25 - 40 years old when surveyed)		
Baby by 25	0.023	0.018
	(0.077)	(0.052)
At least 1 baby ever	0.049	-0.001
	(0.071)	(0.049)
Total fertility	0.032	-0.114
	(0.186)	(0.134)
Observations	2,341	2,341
Panel B: Sample: BSSEE cohorts 1987 - 1992 (35 - 40 years old when surveyed)		
At least 1 baby ever	0.133	0.084
	(0.105)	(0.092)
Total fertility	0.504	0.328
	(0.389)	(0.329)
Observations	909	909
Panel C: Sample: BSSEE cohorts 1987 - 1991 (36 - 40 years old when surveyed)		
At least 1 baby ever	0.108	0.005
	(0.110)	(0.091)
Total fertility	0.455	0.393
	(0.408)	(0.371)
Observations	729	729
Sociodemographics	Yes	Yes
BSSEE cubic spline	Yes	Yes
Cutoff fixed effects	Yes	Yes
Preferences fixed effects	No	Yes

Notes: This table reports estimated 2SLS coefficients on 'Attend' a preferred school using 'Above' as the excluded instrument (resulting from equation system (1) - (2) in the text). Estimated standard errors in parenthesis are clustered at the individual level. Sociodemographic controls include student gender and parish fixed-effects. Regressions are weighted by the inverse of sampling probability to reflect survey design.

Appendix B. Meta-Analysis Methodology

We perform meta-analysis on all publicly-available studies using quasi-random assignment to a preferred (non-charter) public school (either through lottery or selective enrollment exam). We focus on studies that examine test score impacts. Studies for the meta-analysis were obtained through a targeted literature search. This involved searching through the bibliographies of known papers that fit the inclusion criteria and a search of papers citing these papers. To locate additional papers that may have been missed using this first approach, we also conducted a keyword search on ”causal” and “elite” or “selective” for additional papers. We found 17 papers that fit our criteria.

Following Borenstein, Hedges, Higgins, and Rothstein (2009), the meta-analytic methodology employed can be summarized in two main stages. First, the computation of the estimated effect from each study and, second, the computation of the overall average effect across all studies. For the first stage, because studies tend to report multiple estimated effects, we consider the following criteria: (i) The estimate resulting from the preferred identification strategy and model specification by the author if specified. If no preference is specified, we take into account estimates from all identification strategies used (considering the model specification with most controls within each strategy).[1] (ii) Treatment on the treated effects whenever possible. If not reported, intention to treat effects are used. (iii) Test scores closer to secondary school completion (e.g. 11th grade preferred to 9th grade). (iv) Full population effects. If effects are reported only for sub-populations separately, we record all of them and compute a weighted aggregate effect. (v) Overall examination scores (of exams that evaluate mathematics and language together) if available. If not, separate scores on mathematics and language examinations are recorded and then averaged. If only one overall score of an examination that evaluates mathematics, language, and other additional subjects is available, we consider that one.

After following these criteria, some studies will have multiple effects using different identification strategies, sub-populations or examinations. Therefore, following Borenstein, Hedges, Higgins, and Rothstein (2009), we compute one single overall average effect for each study. For this, it is important to recognize whether different effects presented within the same study are expected to be correlated. This is the case when math and language effects are estimated for the same sample or when different identification strategies are used for the same outcomes. In such cases, we compute as the combined effect the mean of the estimated effects and we assume a correlation of 0.5 between their standard errors to compute the variance of the combined effect. If different sub-populations are used, such as gender, cohorts or regions and no overall effects are presented, then (also following Borenstein, Hedges, Higgins, and Rothstein (2009)) we assume them to be independent and we generate a combined effect for each study j by performing a fixed-effects meta-regression. This procedure estimates a weighted average effect (ES_j) and average variance $({SE_j}^2)$ for each study j as follows:

$$ES_j = \frac{\sum \frac{es_i}{{se_i}^2}}{\sum {se_i}^2} \tag{3}$$

$${SE_j}^2 = \left(\sum \frac{1}{{se_i}^2}\right)^{-1} \tag{4}$$

[1] Such is the case of Bui, Craig, and Imberman (2014) where we use the estimated effects derived from both the RD and Admission Lottery identification strategies.

where es_i denotes the i^{th} estimated effect of study j and ${se_i}^2$ denotes the variance of such estimated effect.

In the second stage, we compute an overall average effect across all studies through a random-effects meta-regression of all the studies' estimated effects obtained within the first stage (see Borenstein, Hedges, Higgins, and Rothstein (2009), chapter 12 for a detailed explanation). The random-effects meta regression estimates a weighted average effect (μ) of the included studies where the weight for each study j is computed as follows:

$$W_j = \frac{1}{SE_j^2 + T^2} \tag{5}$$

where ${SE_j}^2$ is the variance of the estimated effect from study j (or the within study variance) and T^2 is the between-study variance. We estimate the between-study variance using a method of moments approach.

Finally, we compute the prediction interval for the weighted average effect across all studies $,\mu$, as follows:

$$\mu \pm z\sqrt{T^2 + SE^2} \tag{6}$$

where SE^2 denotes the variance of the weighted average effect (μ) and T^2 denotes the between-study variance resulting from the random-effects meta-regression of all the studies' estimated effects (estimated with a method of moments approach). The prediction interval presents the expected range of true effects in similar studies.

Our analysis includes the following studies assessing the test score effects of attending any preferred public (non-charter) secondary school: Abdulkadiroğlu, Angrist, and Pathak (2014), Abdulkadiroglu, Angrist, Narita, Pathak, and Zarate (2017), Anderson, Gong, Hong, and Zhang (2016), Ajayi (2015), Cullen, Jacob, and Levitt (2006), Deming (2011), Hastings, Kane, and Staiger (2009), Hoekstra, Mouganie, and Wang (2018), Jackson (2010), Pop-Eleches and Urquiola (2013). In addition, we also consider the following studies assessing the test score effects of attending a preferred elite public secondary school: Barrow, Sartain, and de la Torre (2017), Bui, Craig, and Imberman (2014), Clark (2010), Dee and Lan (2015), Dustan, de Janvry, and Sadoulet (2017), Lucas and Mbiti (2014), Park, Shi, Hsieh, and An (2015).

Appendix C. The Revealed Preference Ranking of Schools

While existing studies have examined the impacts of attending a more selective school or an elite school, it is not clear that *most* parents actually prefer such schools. If all parents prefer more elite (or selective) schools, the lack of an elite schooling effect would speak to the choices made by all parents. Conversely, if parents hold very different views regarding what schools they prefer, then elite schools would not necessarily be preferred by many (or even most) parents. In this second scenario, the lack of an elite schooling effect would speak to the choices made by some parents, but would not speak to the choices made my most parents. Related to this, if parents agree on which schools are preferred, the benefits to attending a preferred school reflect the relative impacts of a certain set of schools. Conversely, if parents have very heterogeneous preferences for schools, the benefits to attending a preferred school might reflect student-school matching effects rather than the effectiveness of a certain set of schools relative to others. From a policy perspective, and to aid interpretation, these are important distinctions that we examine below.

To better understand parental preferences for schools, we follow Avery, Glickman, Hoxby, and Metrick (2013) and exploit the choice data to construct a revealed-preference ranking of secondary schools in Barbados. Intuitively, because each student lists a set of schools they wish to attend, in order of desirability, and the allocation algorithm is truth revealing *among the set of choices submitted*, one can determine which are more preferred schools by seeing which individual schools tend to be systematically higher in individuals' choices. The ranking approach is similar to that used for ranking players in tournaments where players are observed in several head-to-head match-ups. Schools that tend to be preferred in many head-to-head comparisons (i.e. ranked above other schools on the list) are more highly ranked, and schools that are preferred over more highly ranked schools are themselves more highly ranked. Because each list of X ranked schools includes $\sum_{n=1}^{X-1}(X-n)$ such head-to-head comparisons and thousands of students submit such lists each year, constructing such rankings from the choice data is feasible. We expand on the model below.

Each student i, has a utility value, U_{ij}, for each secondary school j, given by (1) below.

$$U_{ij} = \theta_j + \varepsilon_{ij} \tag{7}$$

The parameter θ_j is an index of the overall desirability of school j, and the random error term is ε_{ij}. The parameter θ_j does not vary at the student level and therefore represents a school's average desirability. Let $\theta_j^{r_{is}}$ be the desirably of the school j that individual i ranked s ($r_i = s$) in their list of options R_i. Let $U_{ij}^{r_{is}}$ be the utility individual i gets from school j that she ranked in position s, so that $U_{ij}^{r_{i1}}$ is her utility from the school she ranked first, $U_{ij}^{r_{i2}}$ her utility for the school ranked second, and so on. Because the assignment mechanism is truthfully revealing within the set of submitted choices, we make the simple behavioral assumption that higher ranked schools are preferred to lower ranked schools. It therefore follows that the probability that an individual i submits a particular ranking over the set of listed schools is

$$Pr\big[(U_{ij}^{r_{i1}} > U_{ik}^{r_{im}}, 1 < m, \forall m \in \{2,\ldots,R_i\}) \cap \ldots \cap (U_{ij}^{r_{iR_i-1}} > U_{ik}^{r_{iR_i}})\big] \tag{8}$$

As is common practice in the discrete-choice literature, we assume that ε_{ij} follows an extreme value distribution so that the probability that an individual i submits a particular ranking over all ranked schools is a product of standard logit formulas. The likelihood (or probability) that individual i

chooses ranking $\{r_{i1}, r_{i2}, \ldots, R_i\}$ is now:

$$(9) \quad l_i(\theta) = Prob[r_{i1}, r_{i2}, \ldots, R_i] = \frac{\exp(\theta_j^{r_{i1}})}{\sum_{k=1}^{R_i} \exp(\theta_{ik}^{r_{ik}})} \cdot \frac{\exp(\theta_j^{r_{i2}})}{\sum_{k=2}^{R_i} \exp(\theta_{ik}^{r_{ik}})} \cdots \frac{\exp(\theta_j^{r_{iR-1}})}{\exp(\theta_j^{r_{iR-1}}) + \exp(\theta_k^{r_{iR_i}})}$$

The full log likelihood of observing all the choices is simply the sum of the log of the individual likelihoods across all individuals.

$$(10) \qquad \log L(\theta) = \sum_{i=1}^{N} \log l_i(\theta)$$

One can obtain estimated preferences for each school $\hat{\theta}_j$ by finding the θ_js that maximize the full log likelihood. This is achieved by estimating a rank-ordered logit model with a full set of indicator variables for each school in Barbados. Because proximity is a strong predictor of parents' choices, we obtained rankings based on models that both include and exclude proximity to each school choice as a covariate. Reassuringly, the rankings are identical across both models. Schools with larger $\hat{\theta}_j$s are those that tend to be listed higher up in individuals' ordered lists. The school with the highest $\hat{\theta}_j$ will be the school that is most likely to be preferred (on average) in head-to-head comparisons with other schools. After running this model, we rank schools by their estimated desirability to obtain a revealed-preference ranking over all schools. If students who list both schools A and B tend to list school A above school B, and students who list both schools B and C tend to list school B above school C, our approach will rank school A above B and B above C.

The Estimated School Rankings

To determine whether the preference rankings are meaningful, we first establish that they are stable over time. The top five schools in 1987 remain the top five schools in 2011 with the only difference being that the top two schools swapped places.[2] While there is some movement among the lower-ranked schools, the rankings are quite stable across this 25 year period. Overall, the correlation between the revealed preference rank in 1987 and the revealed preference rank in 2011 is 0.96. The similarity in rankings when parents can rank all schools (and therefore truthful revelation is a dominant strategy) and when they can rank up to nine schools, indicates that one can reliably infer parental preferences from choice data when parents can only rank nine choices. A scatter-plot of the rankings across these two years is presented in the left panel of Figure C1. The regression predicting the rank in 2011 based on the rank in 1987 has a slope of 0.97 and an R-squared of 0.91. The p-value for the test that the slope is equal to 1 is 0.7. This suggests that the average view regarding what schools are most desirable has been very stable over time.

Having established that aggregate school rankings are stable over time, we now explore how much the *average* view is shared among *individuals*. To do this, we rank schools in each year, and then estimate the likelihood of a given school being listed as a preferred school in a given year as a function of its aggregate ranking in that year.[3] If there is widespread agreement among parents about what the most desirable schools are, aggregate rankings would predict being ranked more

[2]Using the reveled preference rankings, the five top ranked schools in 1987 were (1) Harrison College (HC), (2) Queens College (QC), (3) Combermere School (CS), (4) St. Michaels School (SM), (5) Christ Church Foundation (CF). A quarter century later in 2011, the top ranked schools were (1) QC, (2) HC, (3) CS, (4) SM, (5) CF.

[3]We estimate a rank ordered logit model in which the aggregate ranking enters the model as the sole predictor

highly by parents, and rank reversals (i.e. putting a lower-ranked school higher in one's choice list) would be uncommon. Conversely, if there is considerable heterogeneity in parents' views regarding which schools are more desirable, aggregate rankings may predict being ranked more highly by parents *on average*, but rank reversals would be common. The average ranking in a given year is a very strong predictor of individual choices in that year. A school is 44 percent more likely to be more highly ranked by an individual if it is one rank higher in the aggregate, 3 times as likely to be more highly ranked if it is three ranks higher in the aggregate, and 38 times as likely to be more highly ranked if it is 10 ranks higher in the aggregate.

To assuage concerns that the analysis above uses an in-sample prediction (for which there may be some mechanical correlation), we also we rank schools based on the choice lists in 1987, and then estimate the likelihood of a given school being listed as a preferred school in 2011 as a function of its ranking in 1987. We estimate this using a rank ordered logit model on the 2011 choices in which the 1987 ranking enters the model as the sole predictor. Because we use the rankings from a different year, this model will understate the extent to which the individual choices are similar to the average view. However, the patterns are very similar. The 1987 ranking is a powerful predictor of rankings in subsequent years. A school is 33 percent more likely to be more highly ranked in 2011 if it is one rank higher in 1987, 2.4 times as likely to be more highly ranked in 2011 if it is three ranks higher in 1987, and more than 19 times as likely to be more highly ranked in 2011 if it is 10 ranks higher in 1987.[4] These patterns suggests that while parents may disagree regarding which schools are most desirable among very similarly ranked schools, there is considerable agreement regarding which group of schools are most desirable. To allow for the possibility that boys and girls may have different preferences for schools, we examined differences by student gender and the results are virtually identical.[5]

Because the highest-achieving students are admitted to their top choices first, if most students rank schools similarly, then the more preferred schools will also be more selective than the less preferred schools. To show that this is borne out in the data, the right panel of Figure C1 presents the cumulative distribution of the mean peer incoming BSSEE scores of students' school choices. The distribution of mean BSSEE scores of first-choice schools is to the right of the second-choice schools, which is to the right of the third-choice schools, and so on. That is, parents and students tend to place schools with higher-achieving peers higher up on their preference ranking. This is further evidence that most parents agree on which schools are most desirable. As above, we allow for the possibility that boys and girls may have different preferences for schools, we examined differences by student gender, and the results are virtually identical. Given that the impact of schools may differ by student gender, this is an important finding.

[4] Put differently, a rank reversal would occur only about 42 percent of the time for schools that were one rank apart in 1987, under 30 percent of the time for schools that were four ranks apart in 1987, and less than six percent of the time for schools that were ten ranks apart. Figure C2 shows the estimated likelihood that a parent would rank a school above another school in 2011 as a function of the difference in the school rankings in 1987.

[5] We calculated revealed preference rankings pooling all BSSEE cohorts separately by gender. The correlation between girls' rankings and boys' rankings is 0.996.

Figure C1. School Choices

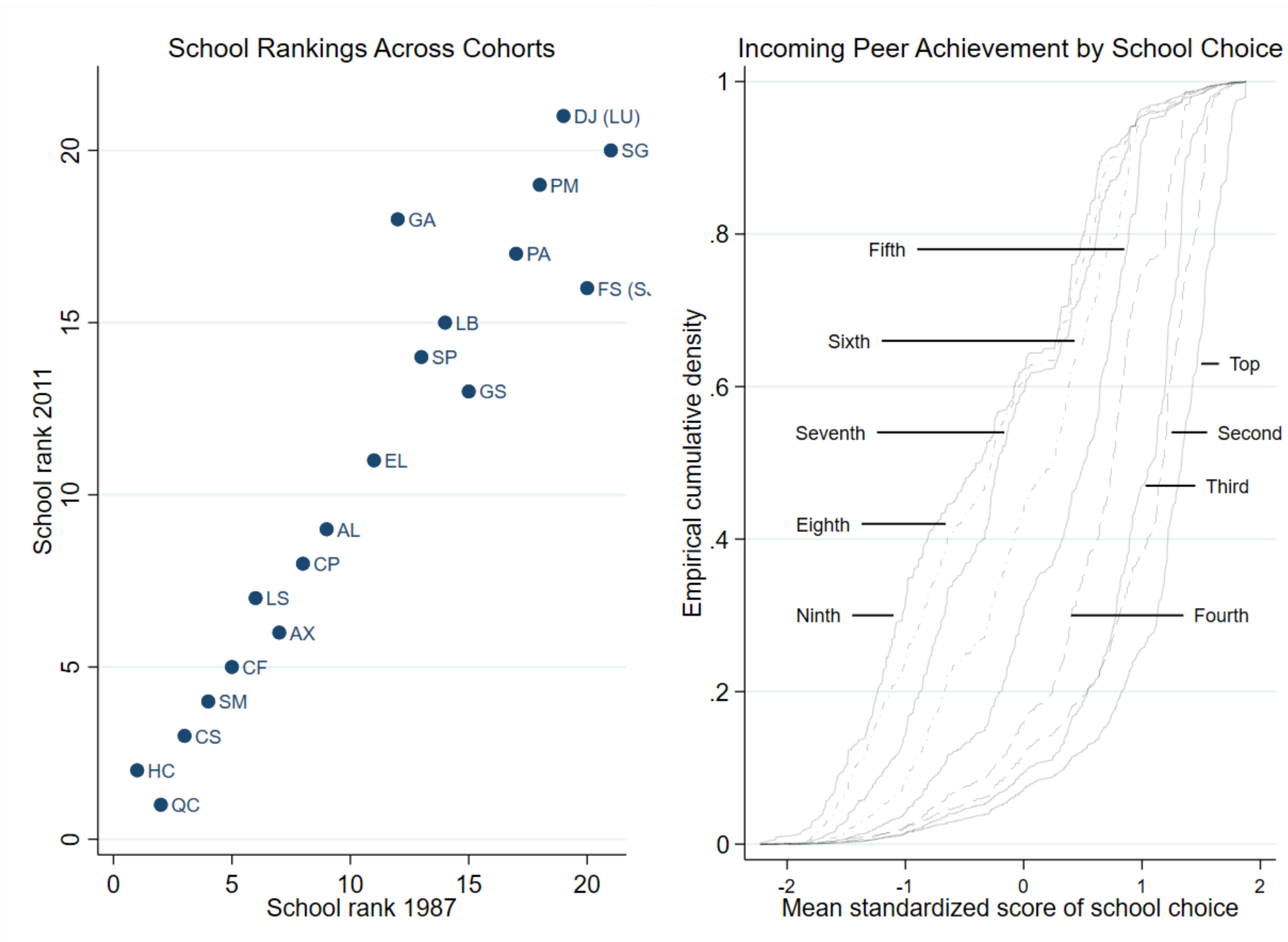
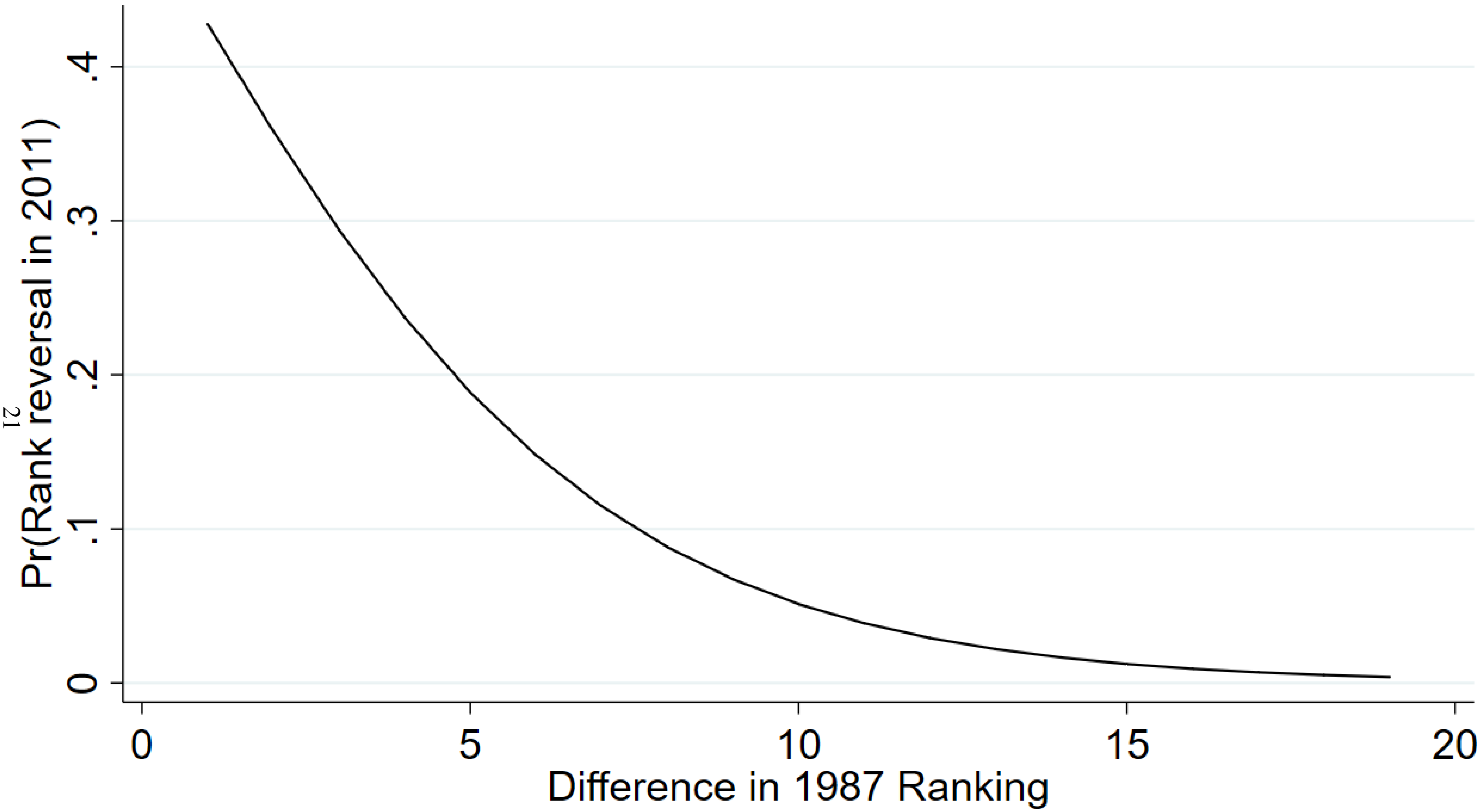


Figure C2. Probability of Rank Reversal



Appendix D. Do the Null Effects Generalize to the Average Student?

Because our estimated preferred school effects are based on applicants who score just above or just below the cutoff for a preferred school, this local treatment effect may not reflect the experiences of the average student at a preferred school. This limitation applies to 13 out of the 17 studies in Table 1 that rely on test score cutoffs to identify school impacts. In these studies (as here) the estimated treatment effect is *the impact of being the lowest scoring student at a preferred school relative to being a more typical student at a less preferred school* which may be different from *the average effect of attending a preferred school relative to a less preferred school.* If so, the small benefits to attending a preferred school for the marginal admit could be reconciled with strong parental preferences for such schools if the average impacts were more positive than those for the marginal student who scores just above the cutoff. Contributing to this literature methodologically, we implement a test for whether the estimated school impacts for the marginal students are similar to those for the average student. While used for a different purpose, our proposed test is similar to those used in Deming (2014) and Hastings, Neilson, and Zimmerman (2015) to validate observational school impacts with quasi-random variation. This test will help potentially explain the null impacts we find in Barbados and possibly other settings.

The Empirical Test

We now introduce some notation. The impact of attending school j for the average student is μ_{j1} while that for the marginal student is μ_{j2}. The outcome for marginal student i at school j is

$$Y_{ijt} = \mu_{j2} + f(BSEE_{it}) + X_{ijt}\gamma + C_{jt} + P_{ijt} + \varepsilon_{ijt} \tag{11}$$

The estimated effect on outcome Y_{ijt} of scoring above the admissions cutoff for any school j is $\Gamma_{j,actual} = E(Y_{ijt}|Above = 1) - E(Y_{ijt}|Above = 0)$. Substituting (11) into this expression and taking expectations, in the neighborhood of the cutoff yields

$$E[\Gamma_{j,actual}] = E(\mu_{j2}|Above = 1) - E(\mu_{j2}|Above = 0) \tag{12}$$

In expectation, the RD estimate of scoring above the cutoff for school j simply reflects the difference through that cutoff in the attended school impacts *for the marginal students*. This is intuitive; scoring above the cutoff for school j increases the likelihood of attending school j and reduces the likelihood of attending the next preferred schools. If school j is no more effective for the marginal admit (on average) than the next preferred schools, then the cutoff effect for school j will be zero. Conversely, the cutoff for school j will only have a positive impact if school j is more effective at improving outcomes for the marginal admit (on average) than the next preferred schools.

Now consider impacts for the average admit, μ_{j1}. One can estimate the impact of school j for the *average* student, μ_{j1}, in a value-added framework. Where $I_{i,J=j}$ is an indicator variable equal to 1 if student i attends school j, the outcome for average student i at school j is

$$Y_{ijt} = I_{i,J=j} \cdot \mu_{j1} + f(BSSEE_{it}) + X_{ijt}\gamma + C_{jt} + P_{ijt} + \varepsilon_{ijt} \tag{13}$$

One can obtain an estimate of the value-added of school j for the average attendee by estimating equation (13) by OLS. The resulting estimate $\hat{\mu}_{j1}$ is simply a school fixed effect that reflects the school-level average outcomes after accounting for observable student characteristics such as incoming test scores, choices, and demographics.[6]

As discussed above, the RD estimate of scoring above the cutoff for school j on outcomes reflects the difference through that cutoff in the attended school impacts (i.e. $\delta(\mu_{j2})/\delta(\text{Above})$) for the marginal admits. We define $\Gamma_{j,predicted}$ as the difference through that cutoff in the estimated value-added of the attended school (i.e. $\delta(\hat{\mu}_{j1})/\delta(\text{Above})$) *among those same marginal admits*. If (i) the value-added estimate is unbiased such that $E[\hat{\mu}_{j1}] = \mu_{j1}$, and (ii) the effect of school j for the marginal admit is the same as the average admit such that $\mu_{j1} = \mu_{j2}$, then (iii) in expectation, the change in the average estimated value-added of the school attended through the cutoff for school j should be equal to the actual change in outcomes through that cutoff.[7] We test this empirically by estimating $\Gamma_{j,actual}$ and $\Gamma_{j,predicted}$ for each school j across all the CSEC outcomes, and then we regress one on the other. **To avoid endogeneity, we use out-of-sample (or leave-year-out) estimates of school value-added.** If our school value-added estimates are biased, then the slope of this regression will differ from 1. In addition, if the school impacts are different for the marginal student from those for the average student, then this slope will also differ from 1. However, if (a) our school value-added estimates are unbiased, and (b) the school impacts are the same for the marginal student as for the average student, then the slope *will* be equal to 1.[8]

Pooling the estimated impacts for each cutoff (preferred school) across all CSEC outcomes, we plot the estimated impacts against the difference in school value-added in Figure D1. The estimated slope is 0.97, revealing that on average the predicted impacts are very similar to the actual impacts. The p-value associated with the null hypothesis that the slope is zero has a p-value of less than 0.001, and the p-value associated with the null hypothesis that the slope is 1 has a p-value of 0.836. This is compelling evidence that the null impacts on short-run test scores are not because the impact for the marginal student is more negative than that for the average student.[9]

[6]Under the assumption that $E[\varepsilon_{ijt}|I_{i,J=j}, BSSEE_{it}, X_{ijt}, C_{jt}, P_{ijt}] = 0$, this will be an unbiased estimate.

[7]One can estimate the impact of scoring above the cutoff for school j on the average value-added of the schools students attend, $\hat{\mu}_{j1}$, by replacing the actual outcomes with the estimated value-added of the attended school and estimating the model below.

$$\hat{\mu}_{j1} = \zeta_j \cdot Above_{ijt} + f(BSEE_{it}) + X_{ijt}\gamma + C_{jt} + P_{ijt} + \varepsilon_{ijt} \tag{14}$$

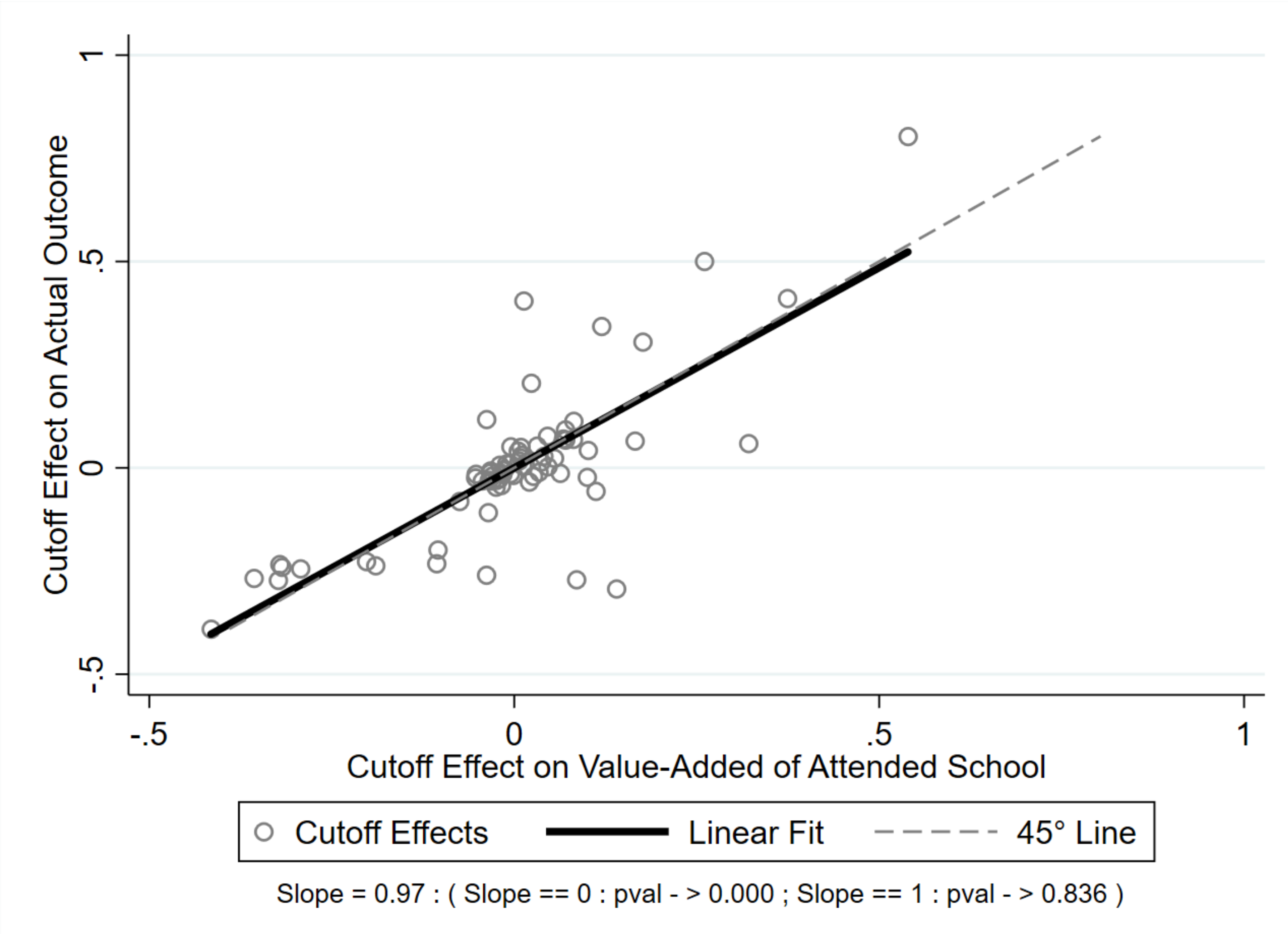
The parameter ζ_j is the difference in school value-added between those who score just above the cutoff for school j and those who score just below. *In the neighborhood of the cutoff*, this is

$$E[\zeta_j|X_i, BSEE_i] = E(\mu_{j1}|Above = 1) - E(\mu_{j1}|Above = 0) \tag{15}$$

[8]It is *possible* that biases in the school effect estimates are exactly offset by differences between the marginal and average treatment effects so that the estimated coefficient appears to be 1 even when the marginal and average impacts differ. This would be a razor's edge result and is extraordinarily unlikely. Accordingly, we assume that the probability of such a situation is essentially zero.

[9]This test also serves as a validation of the school fixed effects (i.e. value-added estimates).

Figure D1. Predicted Cutoff Effects vs Actual Cutoff Effects - CSEC Outcomes



Notes: The X-axis represents the estimated coefficients on the 'Above' indicator resulting from equation (14); estimated for each school *j* and for each CSEC outcome (school value-added measures enter as dependent variables). The Y-axis represents the estimated coefficients on the 'Above' indicator resulting from reduced form models as in equation (1); estimated for each school *j* and for each CSEC outcome (individual level outcomes enter as dependent variables).

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