Abstract Title Page

Title: Stand and Deliver: Effects of Boston's Charter High Schools on College Preparation, Entry, and Choice

Authors and Affiliations: Joshua D. Angrist, MIT and NBER; Sarah R. Cohodes, Harvard University; Susan M. Dynarski, University of Michigan and NBER; Parag A. Pathak, MIT and NBER; Christopher R. Walters, University of California Berkeley

Abstract Body

Background / Context:

One of the most important questions in education research is whether the gains from interventions for which we see short-term success can be sustained. For example, test score gains generated by pre-school interventions, highly effective teachers, and elementary-school class size reductions often appear to fade as students progress through school, though some of these gains may re-emerge later in non-cognitive outcomes. The possibility of short-lived impacts is especially relevant for research on charter schools, where charter operators who face high-stakes assessments have an incentive to "teach to the test." The fact that charters are subject to intense scrutiny and evaluation may even create incentives for cheating (Jacob and Levitt, 2003), strategic instruction (Jacob, 2007), and a focus on small groups of students that are pivotal for official accountability measures (Neal and Schanzenbach, 2010). Assessing charter school impacts on outcomes other than state test scores used for accountability purposes would allow us to assess whether charter school test score gains persist to outcomes more closely related to job market success and less related to school accountability incentives.

Charter schools are a recent innovation. Not surprisingly, therefore, most evidence on charter effectiveness comes from outcomes measured while children are still enrolled in elementary and secondary school. An exception is Dobbie and Fryer's (2012) recent lottery-based study, which follows applicants to a single charter middle school in the Harlem Children's Zone, estimating effects on college enrollment while also looking at non- educational outcomes related to crime and teen pregnancy. Dobbie and Fryer (2012) find that Promise Academy students are more likely to go to college, while girls are less likely to get pregnant and boys are less likely to be incarcerated. Earlier work by Booker et al. (2008) uses statistical controls and distance instruments to identify the effects of charter school attendance on high school graduation and college enrollment. Both empirical strategies suggest gains for charter students. We complement this earlier work with new results on post-secondary preparation, enrollment, and choice for a large cohort of charter high school lottery applicants.

Purpose / Objective / Research Question / Focus of Study:

The purpose of this paper is to assess the impact of attendance at Boston's charter high schools on outcomes where the link with human capital and future earnings seems likely to be sustained and strong. Specifically, we focus on outcomes that are either essential to or facilitate post-secondary schooling: high school graduation, the attainment of state competency thresholds, scholarship qualification, Advanced Placement (AP) and SAT scores, college enrollment, and college persistence. Importantly, most of these outcomes are less subject to strategic manipulation than are the state's test-based assessments. As in earlier work, the research design implemented here exploits randomized enrollment lotteries at over-subscribed charter schools. The resulting estimates are likely to provide reliable measures of average causal effects for charter applicants.

Setting:

Boston's over-subscribed charter schools generate impressive score gains. Lottery estimates show that each year spent at a charter middle school boosts scores by about a fifth of a standard deviation in English Language Arts (ELA) and over a third of a standard deviation in math. High school gains are just as large (Abdulkadiroglu et al., 2011). These effects are in line with those generated by urban charters elsewhere in Massachusetts, as shown in studies of a Knowledge is Power Program (KIPP) school in Lynn, Massachusetts (Angrist et al., 2010, 2012), and in an analysis of charter lottery results from around the state (Angrist et al., 2011a,b).

Population / Participants / Subjects:

Schools: We set out to study the effects of attendance at six charter high schools in Boston. These schools

generated the lottery-based estimates of charter high school achievement effects reported in an earlier study (Abdulkadiroglu et al., 2011), and account for the bulk of charter high school enrollment in Boston today. Two additional charter high schools serving Boston students in the same period are now closed. One school that is still open has poor records and appears unsuitable for a lottery-based analysis.

Students: There are 3,671 charter school lottery applicants in our randomized sample. Applicants tend to have higher baseline test scores than the traditional BPS population, and are more likely to be black. Limited English proficient students are under- represented among charter applicants, but the proportion of applicants identified as qualifying for special education services is almost as high among applicants as in the traditional BPS population. Most lottery applicants qualify for free or reduced price lunch.

Intervention / Program / Practice:

A defining feature of Massachusetts' successful urban charter schools appears to be adherence to No Excuses pedagogy, an approach to urban education described in a book of the same name (Thernstrom and Thernstrom, 2003). No Excuses schools emphasize discipline and comportment, traditional reading and math skills, extended instruction time, and selective teacher hiring. Massachusetts' No Excuses charters also make heavy use of Teach for America (TFA) participants and alumni and provide extensive and ongoing feedback to teachers. Like most Boston charter schools, the high schools studied here largely identify with the No Excuses approach. Another feature of the Massachusetts charter landscape is the state's rigorous charter authorization and monitoring process. To date the state has closed 12 charter schools after they began operations and an additional 4 schools before they opened (Massachusetts Department of Elementary and Secondary Education, 2013).

Research Design:

Our lottery-based empirical strategy is motivated by the observation that charter attendance is a choice variable that may be correlated with motivation, ability, or family background. Conventional regression estimates of the effects of charter attendance may therefore fail to capture causal effects. To eliminate selection bias, our empirical strategy uses randomly assigned charter lottery offers to estimate the effects of attending charter schools. The second-stage equation for our lottery-based two-stage least squares (2SLS) analysis links charter school attendance with outcomes as follows:

$$y_{it} = \alpha_t + \sum_j \delta_j d_{ij} + \gamma' X_i + \rho C_{it} + \varepsilon_{it}$$

where y_{it} is the outcome of interest for student i, X_i is a vector including 10th-grade-year dummies and a set of pre-lottery demographic characteristics (race, special education, limited English proficiency, subsidized lunch status, and a female-minority interaction) and ε_i is an error term. The d_{ij} are dummy variables for all combinations of charter school lotteries (indexed by j) seen in the lottery sample. In what follows, we refer to these combinations as "risk sets." These are included because the application mix determines the probability of receiving an offer, even when offers at each school are randomly assigned.[10] The variable of interest, C_i, indicates attendance at any of the six charter schools in our lottery sample in 9th or 10th grade. The parameter ρ captures the causal effect of charter school attendance.

We use charter offer variables as instruments. The initial offer instrument, Z_{i1}, is a dummy variable indicating offers made on the day of the charter school lottery. Because some applicants who don't receive offers on lottery day do so at a later date when their names are reached on a randomly ordered wait list, we also code a second instrument, denoted ever offer, or Z_{i2}. The ever offer instrument indicates applicants who receive an offer at any time, whether on lottery day or later. Applicants who receive an initial offer thus have both instruments switched on, while those who receive later offers without an initial offer have only the ever offer instrument switched on. Missing values for either instrument are coded as no offer. Because the model controls for the pattern of schools and cohorts with lottery data of each type

through application risk sets, this convention is innocuous.

The first stage for our 2SLS procedure is:

$$C_{it} = \lambda_t + \sum_j \mu_j d_{ij} + \beta' X_i + \pi_1 Z_{i1} + \pi_2 Z_{i2} + \eta_{it},$$

where two separate parameters, π_1 and π_2, capture the effects of initial and eventual offers. As in the second stage equation, the first stage includes risk set controls, 10th-grade-year dummies, and baseline demographic characteristics. With two instruments used to estimate a single causal effect, we can interpret 2SLS estimates as a statistically efficient weighted average of what we would get from an estimation strategy that uses the instruments one at a time. Standard errors are clustered at the 10th-grade-school-by-year level.

Randomly-assigned lottery offers are likely to be independent of student ability or family background (within risk sets). Consistent with presumed random assignment, an analysis of covariate balance shows that pre-lottery demographics and test scores are similar for offered and non-offered students. Differences in baseline characteristics by offer status are small and statistically insignificant for all variables tested, and p-values from joint tests are high. Additionally, there is no significant difference in availability of outcomes (test scores, college attendance, and college choice) between offered and non-offered students.

Data Collection and Analysis:

Massachusetts charter schools admit students by lottery when they have more applicants than seats. We collected lists of charter school applicants and information on the results of admissions lotteries from individual charter schools. These lists were then matched to administrative records covering all Massachusetts public school students. Our analysis sample is limited to charter applicants who applied for a charter school seat from Fall 2002 through Fall 2009.

We matched applicant records to administrative data using applicants' names, cohorts, and grades of application. Where available, information on date of birth, town of residence, race or ethnicity, and gender was used to break ties. Among applicants eligible for our study, 94 percent were matched to state data. Applicants were excluded from the analysis if they were disqualified from the lottery (these are mostly applicants to the wrong grade). We also omit siblings of current charter students, late applicants, and some out-of-area applicants. Students submitting charter applications in multiple years appear only once in the sample, with data recorded for the first application only. Information on baseline demographics and test scores comes from the most recent pre-lottery data available in the state database.

In addition to providing demographic information and scores on state assessments, state administrative records include AP and SAT scores for all public school students tested in Massachusetts. Information on college enrollment and choice comes from the National Student Clearinghouse (NSC). The Massachusetts Department of Elementary and Secondary Education routinely requests an NSC match for Massachusetts high school graduates. NSC data record enrollment spells at participating post-secondary institutions, which account for 94% of Massachusetts undergraduates. The cleaned lottery files merged to the state and NSC outcome data provides our analytic sample.

Findings / Results:

Our estimates suggest that the effects of Boston's charters are remarkably persistent. Specifically, charter attendance raises the probability that students pass the score thresholds for high-stakes exams required for high-school graduation (Table 4), boosts the likelihood that students qualify for an exam-based college scholarship (Table 4), increases SAT scores (Table 5), increases the frequency of AP test- taking with modest gains in scores (Table 6), and shifts students away from attending two-year institutions and towards four-year attendance (Table 8). The effect of charter attendance on the probability of attending a

four-year public institution in Massachusetts is particularly large. Moreover, these schools seems to be highly effective for subgroups that are often difficult to serve, including boys, special education students, and students with low achievement at high school entry (Table 10).

In view of often-voiced concerns about the effect of charter schools on student attrition, we explore a possible explanation for these gains in the form of school switching and peer effects (Table 11). Charter attendance increases school switching outside of transitional grades, but this does not accentuate the effect of charter enrollment on peer composition. If anything, charter peers become more like peers at traditional public schools as students progress through high school. As a result, it seems unlikely that changes in peer composition are the primary driver of our findings.

Conclusions:

Studies of many educational interventions show promising short-run gains followed by discouragingly fast fadeout. This paper uses randomized admission lotteries to ask whether the substantial short-run test score effects of Boston's charter high schools translate into gains on longer-run outcomes like SAT scores, Advanced Placement test-taking and scores, college attendance, and college choice. These large college preparation and college choice gains for charter school students suggest that the short-run test score impacts reported in previous work on Boston's charter schools are not driven by gaming or teaching to the test; rather, they seem to represent increases in underlying human capital, with effects that generalize to a number of other contexts. The cohorts of lottery applicants in our sample are too young to generate reliable estimates of effects on college persistence or graduation. In future work, we plan to investigate the effects of Boston's charter schools on these outcomes, as well as longer-run labor market outcomes like employment and earnings.

Appendices

Not included in page count.

Appendix A. References

Abdulkadiroglu, A., Angrist, J., Dynarski, S., Kane, T. J., and Pathak, P. (2011). Accountability and flexibility in public schools: Evidence from Boston's charters and pilots. The Quarterly Journal of Economics, 126(2):699-748.

Angrist, J. D., Cohodes, S. R., Dynarski, S. M., Fullerton, J., Kane, T. J., Pathak, P. A., and Walters, C. R. (2011a). Student achievement in Massachusetts' charter schools. Center for Education Policy Research at Harvard University.

Angrist, J. D., Dynarski, S. M., Kane, T. J., Pathak, P. A., and Walters, C. R. (2010). Inputs and impacts in charter schools: KIPP Lynn. American Economic Review, 100(2):239-243.

Angrist, J. D., Dynarski, S. M., Kane, T. J., Pathak, P. A., and Walters, C. R. (2012). Who benefits from KIPP? Journal of Policy Analysis and Management, 31(4):837-860.

Booker, K., Sass, T. R., Gill, B., and Zimmer, R. (2008). Going beyond test scores: Evaluating charter school impact on educational attainment in Chicago and Florida. RAND, Santa Monica, CA.

Dobbie, W. and Fryer, R. G. (2012). Are high quality schools enough to reduce social disparities? Evidence from the Harlem Children's Zone. Unpublished manuscript, Harvard University.

Jacob, B. A. (2007). Test-based accountability and student achievement: An investigation of differential performance on NAEP and state assessment. Working Paper 12817, National Bureau of Economic Research, Cambridge, MA.

Neal, D. and Schanzenbach, D. W. (2010). Left behind by design: Proficiency counts and test-based accountability. Review of Economics and Statistics, 92(2):263-283.

Thernstrom, A. M. and Thernstrom, S. (2003). No excuses: Closing the racial gap in learning. Simon & Schuster, New York

Appendix B. Tables and Figures

Please see next page.

Table 4: Lottery Estimates of Effects on MCAS Performance Categories

	First Attempt		Ever	
	Mean	Effect	Mean	Effect
	(1)	(2)	(3)	(4)
Panel A: MCAS ELA				
Needs Improvement or Higher	0.964	-0.016	0.990	-0.014
		(0.023)		(0.011)
Proficient or Higher	0.659	0.170***	0.661	0.167***
		(0.061)		(0.061)
Advanced or Higher	0.091	0.187***	0.091	0.190***
		(0.036)		(0.036)
Meets Competency Determination	0.810	0.165***	0.828	0.151***
		(0.053)		(0.052)
N				3671
Panel B: MCAS Math				
Needs Improvement or Higher	0.914	0.082**	0.977	0.031**
		(0.033)		(0.015)
Proficient or Higher	0.642	0.172***	0.645	0.148**
		(0.065)		(0.065)
Advanced or Higher	0.319	0.267***	0.319	0.267***
		(0.061)		(0.061)
Meets Competency Determination	0.757	0.133**	0.799	0.101*
		(0.058)		(0.056)
N				3615
Panel C: ELA and Math Combined				
Needs Improvement or Higher	0.903	0.080**	0.975	0.019
		(0.035)		(0.016)
Proficient or Higher	0.542	0.173**	0.544	0.166**
		(0.071)		(0.073)
Advanced or Higher	0.075	0.161***	0.075	0.165***
		(0.035)		(0.035)
Meets Competency Determination	0.694	0.177***	0.741	0.144**
		(0.066)		(0.066)
Eligible for Adams Scholarship Using BPS Cutoffs	0.199	0.242***		
		(0.059)		
N				3594

Notes: This table reports 2SLS estimates of the effects of Boston charter attendance on 10th-grade MCAS performance categories and eligibility for the Adams Scholarship. The Competency Determination requires scaled scores of 220 in both ELA and math for the classes of 2006-2009, and scores of 240 in both subjects for the classes of 2010-2013. A student is eligible for the Adams Scholarship if he or she is proficient in both subjects, advanced in at least one subject, and scores among the top 25% of the Boston district on his or her first attempt. BPS cutoffs for projected graduation cohorts 2012 and 2013 are imputed with the 2011 cutoff. A student "needs improvement" if he or she scores at or above 220 on both tests; "is proficient" if he or she scores at or above 240 on both tests; and "is advanced" if he or she scores at or above 260 on both tests. See Table 3 notes for detailed regression specifications. Means are for non-charter attendees.

*significant at 10%; **significant at 5%; ***significant at 1%

Table 5: Lottery Estimates of Effects on SAT Test-taking and Scores

	Taking		Reasoning (1600)		Composite (2400)	
	Mean [s.d.] (1)	Effect (2)	Mean [s.d.] (3)	Effect (4)	Mean [s.d.] (5)	Effect (6)
Took SAT	0.634 [0.482]	0.033 (0.078)				
Score Above MA Bottom Quartile		-	0.253 [0.435]	0.135** (0.066)	0.253 [0.435]	0.116* (0.067)
Score Above MA Median		-	0.092 [0.289]	0.113** (0.049)	0.082 [0.275]	0.100** (0.040)
Score In MA Top Quartile		-	0.026 [0.160]	0.000 (0.016)	0.019 [0.137]	-0.009 (0.017)
N						2957
Average Score (For takers)		-	846.4 [166.6]	75.2*** (29.1)	1254.3 [240.1]	102.8** (42.9)
N						1897

	Math (800)		Verbal (800)		Writing (800)	
	Mean [s.d.] (1)	Effect (2)	Mean [s.d.] (3)	Effect (4)	Mean [s.d.] (5)	Effect (6)
Score Above MA Bottom Quartile	0.299 [0.458]	0.165** (0.080)	0.263 [0.440]	0.121** (0.061)	0.278 [0.448]	0.107 (0.067)
Score Above MA Median	0.116 [0.320]	0.144** (0.057)	0.102 [0.303]	0.064 (0.046)	0.096 [0.294]	0.054 (0.041)
Score In MA Top Quartile	0.032 [0.177]	0.047* (0.028)	0.025 [0.157]	-0.019 (0.021)	0.022 [0.147]	0.010 (0.021)
N						2957
Average Score (For takers)	434.1 [95.5]	51.7*** (16.9)	412.3 [87.4]	23.5 (15.7)	407.9 [86.7]	27.5* (16.2)
N						1897

Notes: This table reports 2SLS estimates of the effects of Boston charter attendance on SAT test-taking and scores. The sample includes students projected to graduate between 2007 and 2012. SAT outcomes are coded using the last test taken by each student. The average score outcomes restrict the sample to SAT takers. All other outcomes are equal to zero for non-SAT takers. Maximum possible scores are shown in parenthesis next to outcome labels. See Table 3 notes for detailed regression specifications. Means are for non-charter attendees.

*significant at 10%; **significant at 5%; ***significant at 1%

Table 6: Lottery Estimates of Effects on Advanced Placement Test-taking and Scores

	All AP Exams		Science		Calculus		US History		English	
	Mean	Effect	Mean	Effect	Mean	Effect	Mean	Effect	Mean	Effect
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Took Exam	0.266	0.287***	0.099	0.324***	0.062	0.212***	0.034	0.178*	0.147	0.076
		(0.073)		(0.061)		(0.070)		(0.093)		(0.078)
Number of Exams	0.512	0.963***	0.112	0.314***						
		(0.274)		(0.070)						
Score 2 or Higher	0.136	0.154**	0.028	0.044	0.018	0.087*	0.023	0.056	0.087	0.070
		(0.068)		(0.032)		(0.045)		(0.048)		(0.054)
Score 3 or Higher	0.070	0.096*	0.016	0.020	0.015	0.073*	0.014	0.028	0.023	0.034
		(0.052)		(0.015)		(0.040)		(0.019)		(0.027)
Score 4 or 5	0.039	0.008	0.009	-0.001	0.008	0.021	0.007	-0.010	0.009	0.003
		(0.033)		(0.012)		(0.019)		(0.011)		(0.012)
N										2957

Notes: This table reports 2SLS estimates of the effects of Boston charter attendance on AP test-taking and scores. The sample includes students projected to graduate between 2007 and 2012. Outcomes are equal to zero for students who never took AP exams. Science subjects include Biology, Chemistry, Physics B, Physics Mechanics, Physics Electricity/Magnetism, Computer Science A, Computer Science AB, and Environmental Science. Outcomes for Calculus combine Calculus AB and Calculus BC. Outcomes for English combine English Literature and English Language. See Table 3 notes for detailed regression specifications. Means are for non-charter attendees.

*significant at 10%; **significant at 5%; ***significant at 1%

Table 8: Lottery Estimates of Effects on College Enrollment

	Within 6 Months		Within 18 Months	
	Mean	Effect	Mean	Effect
	(1)	(2)	(3)	(4)
Panel A: Any NSC-Covered School				
Any	0.484	0.063	0.601	0.115
		(0.072)		(0.084)
Two-year	0.121	-0.106**	0.183	-0.058
		(0.051)		(0.064)
Four-year	0.363	0.170**	0.418	0.173**
		(0.070)		(0.079)
Four-year Public	0.135	0.154***	0.145	0.195***
		(0.059)		(0.070)
Four-year Private	0.228	0.016	0.273	-0.022
		(0.076)		(0.094)
Four-year Public In MA	0.116	0.116**	0.121	0.146**
		(0.054)		(0.063)
Panel B: Barron's-Ranked Schools				
Lowest Selectivity Tier Only	0.195	-0.025	0.284	0.032
		(0.056)		(0.071)
Second Lowest Selectivity Tier Only	0.197	0.076	0.206	0.067
		(0.062)		(0.068)
Top Three Selectivity Tiers	0.092	0.012	0.111	0.016
		(0.056)		(0.055)
N		2599		1887

Notes: This table reports 2SLS estimates of the effects of Boston charter school attendance on college enrollment. Within 6 months enrollment (columns 1 and 2) is defined as enrollment by the semester following a student's projected high school graduation, while within 18 months enrollment (columns 3 and 4) is defined as enrollment within two fall semesters after projected graduation. The within 6 months enrollment sample includes students projected to graduate in 2011 or earlier. The within 18 months sample is restricted to students projected to graduate in 2010 or earlier. See Table 3 notes for detailed regression specifications. Means are for non-charter attendees.

*significant at 10%; **significant at 5%; ***significant at 1%

Table 10: Lottery Estimates of Effects by Subgroup

		Gender		Special Education		Baseline Scores		Subsidized Lunch	
		Boy	Girl	Yes	No	Below Median	Above Median	Yes	No
Outcomes		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: 10th- Grade MCAS									
Standardized ELA		0.446***	0.372***	0.529*	0.379***	0.423***	0.358***	0.394***	0.558***
		(0.164)	(0.118)	(0.272)	(0.107)	(0.144)	(0.116)	(0.116)	(0.188)
		1682	1989	662	3009	1765	1762	2673	998
Standardized Math		0.498***	0.615***	0.676***	0.551***	0.570***	0.497***	0.526***	0.748***
		(0.186)	(0.145)	(0.256)	(0.128)	(0.148)	(0.132)	(0.136)	(0.223)
	N	1653	1962	645	2970	1723	1734	2630	985
Panel B: SAT Outcomes									
Took SAT		-0.079	0.119	-0.137	0.067	-0.021	0.144	0.032	0.036
		(0.127)	(0.090)	(0.176)	(0.080)	(0.115)	(0.091)	(0.089)	(0.135)
	N	1371	1586	531	2426	1343	1314	2181	776
SAT Composite (2400)		90.0	96.8*	166.4**	91.8**	138.2***	77.2	85.4**	216.7**
		(64.9)	(52.2)	(82.6)	(45.2)	(50.7)	(50.4)	(41.5)	(94.9)
	N	772	1125	249	1648	777	1034	1372	525
Panel C: AP Outcomes									
Took any AP		0.227**	0.320***	0.293***	0.278***	0.269***	0.333***	0.320***	0.135
		(0.106)	(0.096)	(0.108)	(0.083)	(0.086)	(0.111)	(0.075)	(0.130)
Score 3 or Higher, any AP		0.148**	0.051	0.014	0.108*	0.034	0.180*	0.089	0.135
		(0.068)	(0.072)	(0.059)	(0.065)	(0.030)	(0.103)	(0.061)	(0.088)
	N	1371	1586	531	2426	1343	1314	2181	776
Panel D: High School Graduation Outcomes									
Four-year Graduation		-0.225**	-0.042	-0.433***	-0.076	-0.309***	0.072	-0.109	-0.140
		(0.101)	(0.082)	(0.159)	(0.069)	(0.090)	(0.086)	(0.070)	(0.136)
	N	1474	1731	573	2632	1447	1429	2354	851
Five-year Graduation		-0.022	-0.013	-0.089	0.018	-0.104	0.100	-0.018	0.084
		(0.097)	(0.077)	(0.167)	(0.070)	(0.091)	(0.087)	(0.074)	(0.136)
	N	1190	1409	454	2145	1168	1158	1918	681
Panel E: College Enrollment Within 18 Months									
Any		0.162	0.054	-0.038	0.125	0.213*	-0.027	0.093	0.190
		(0.126)	(0.103)	(0.252)	(0.088)	(0.118)	(0.131)	(0.089)	(0.190)
Four-year		0.165	0.173	0.253	0.152*	0.291***	-0.046	0.217**	0.028
		(0.119)	(0.116)	(0.238)	(0.086)	(0.111)	(0.141)	(0.085)	(0.167)
	N	866	1021	319	1568	842	832	1393	494

Notes: This table reports 2SLS estimates of the effects of Boston charter attendance by subgroup. The above- and below-median samples are constructed by splitting the sample by the median of the sum of baseline ELA and math scores, computed in the MCAS ELA outcome sample. See Table 3 notes for detailed regression specifications. Means are for non-charter attendees.
*significant at 10%; **significant at 5%; ***significant at 1%

Table 11: Lottery Estimates of Effects on School Switching and Peer Quality

	Mean (1)	Effect (2)	Mean (3)	Effect (4)	Mean (5)	Effect (6)	Mean (7)	Effect (8)
Panel A: School Switching								
Any switch	0.362	0.116						
		(0.086)						
N		3072						
Switch excluding transitional grades	0.330	0.151*						
		(0.082)						
N		3063						
Ever attend an exam school	0.145	-0.096**						
		(0.042)						
N		3205						
Panel B: Peer Quality								
	First Post-lotto Year		Second Post-lotto Year		Third Post-lotto Year		Fourth Post-lotto Year	
Peer Baseline ELA	-0.403	0.127*	-0.357	0.065	-0.301	0.046	-0.277	0.022
		(0.068)		(0.073)		(0.075)		(0.070)
Peer Baseline Math	-0.407	0.131*	-0.361	0.078	-0.298	0.076	-0.273	0.042
		(0.075)		(0.079)		(0.073)		(0.075)
Peer Baseline Sum of ELA and Math	-0.794	0.245*	-0.703	0.151	-0.585	0.124	-0.537	0.066
		(0.140)		(0.148)		(0.143)		(0.140)
N		3147		3188		2898		2744

Notes: This table reports 2SLS estimates of the effects of Boston charter attendance on school switching and peer quality. The sample includes applicants projected to graduate between 2006 and 2012. The any switch outcome is one for students observed in two or more schools at any time after the lottery. The switch excluding transitional grades outcome is one for students who transition from one observed school to another at a grade other than the exit grade of the first school. Peer quality is measured as the average baseline score of other students in the same school and year. See Table 3 notes for detailed regression specifications. Means are for non-charter attendees.
*significant at 10%; **significant at 5%; ***significant at 1%