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ABSTRACT

Estimates using admissions lotteries suggest that urban charter schools boost student achievement, while charter schools in other settings do not. We explore student-level and school-level explanations for these differences using a large sample of Massachusetts charter schools. Our results show that urban charter schools boost achievement well beyond ambient non-charter levels (that is, the average achievement level for urban non-charter students), and beyond non-urban achievement in math. Student demographics explain some of these gains since urban charters are most effective for non-whites and low-baseline achievers. At the same time, non-urban charter schools are uniformly ineffective. Our estimates also reveal important school-level heterogeneity in the urban charter sample. A non-lottery analysis suggests that urban schools with binding, well-documented admissions lotteries generate larger score gains than under-subscribed urban charter schools with poor lottery records. We link the magnitude of charter impacts to distinctive pedagogical features of urban charters such as the length of the school day and school philosophy. The relative effectiveness of urban lottery-sample charters is accounted for by over-subscribed urban schools' embrace of the No Excuses approach to education.

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I Introduction

A growing body of evidence suggests that urban charter schools have the potential to generate impressive achievement gains, especially for minority students living in high-poverty areas. In a series of studies using admissions lotteries to identify causal effects, we looked at the impact of charter attendance in Boston and at a KIPP school in Lynn, Massachusetts (Abdulkadiroğlu et al., 2009, 2011; Angrist et al., 2010a, 2010b). Boston and Lynn charter middle schools increase student achievement by about 0.2 standard deviations (σ) per year in English Language Arts (ELA) and about 0.4σ per year in math, relative to traditional public schools. Among high school students, attendance at a Boston charter school increases student achievement by about 0.2σ per year in ELA and 0.3σ per year in math. Outside of Massachusetts, lottery studies of charter schools in the Harlem Children's Zone (Dobbie and Fryer, 2011) and a Washington DC charter boarding school (Curto and Fryer, 2011) document similarly large gains. Studies of Chicago and New York charter schools also report positive effects (Hoxby and Rockoff, 2004; Hoxby, Murarka and Kang, 2009).

While these results are encouraging, they come from schools operating in traditional (for charters) urban settings. Although interest in charter schools is growing in school districts outside central cities (see, e.g., the discussion of New York area charters in Hu, 2011), results for more diverse sets of charter schools are also more mixed. In a recent report evaluating roughly two dozen Massachusetts charter schools from around the state, we find little evidence of achievement gains at schools outside of high-poverty urban areas (Angrist et al., 2011). Some of the estimates for non-urban Massachusetts charters show significant negative effects. These results echo findings from a multi-state study of 36 charter middle schools using admissions lotteries (Gleason et al., 2010). Here too, charter schools outside of urban areas seem to do little for achievement, though, as in our earlier work, urban schools with high-minority, high-poverty enrollment generate some gains. Other studies using statistical controls rather than entrance lotteries also document heterogeneity in the effects of charter schools. Hoxby (2004) and Zimmer et al. (2009) find that newly opened schools are less effective than older schools. Imberman (2011) reports that among charter schools in a large urban school district in the Southwest, schools that began as charters have large effects on discipline and attendance, while converted schools do not.

Our analysis focuses on heterogeneity in the effects of charter schools across demographic groups and between urban and non-urban areas. This breakdown is motivated by our earlier findings for Massachusetts, and by research showing similarly heterogeneous effects for other education alternatives. Using data on cohorts of students graduating in the early 1980s, Evans and Schwab (1995) and Neal (1997) show that Catholic school attendance leads to increases in high school graduation and college attendance. Both studies find larger benefits for black students and for students in urban settings. Grogger et al. (2000) and Altonji et al. (2005) report similar results on Catholic schooling for more recent cohorts. For example, Grogger et al. report that Catholic high school attendance increases the probability of graduation by 18 to 24 percentage points for urban minority students; estimates for other groups are less than 10 percentage points and mostly insignificant.

The analysis here reveals similar heterogeneity for charter schools in Massachusetts and develops a framework for interpreting this heterogeneity using both student- and school-level explanatory variables. We begin with a semiparametric analysis of heterogeneous potential outcomes that assigns a role to variation in no-treatment counterfactuals and to charter applicants' demographic characteristics and baseline scores. This investigation also includes a Oaxaca-Blinder (1973) style decomposition of the urban charter advantage.

We then turn to an analysis that attempts to isolate school-level characteristics that might explain differences in charter school effectiveness. Our school-level investigation of charter effect heterogeneity is built on a set of observational (i.e., non-lottery) estimates that rely on statistical controls to eliminate selection bias. We show that in the sample of schools for which lotteries can be used to capture causal effects, the observational analysis does a good job of replicating lottery-based findings. At the same time, the observational analysis suggests that the sample of urban schools for which a lottery-based analysis is feasible, that is, over-subscribed schools with good historical lottery records, boost scores more than other urban charter schools. Our school-level analysis explains the difference in effectiveness between lottery and non-lottery schools as well as the urban charter advantage.

The next section details school participation, describes the data, and outlines our empirical strategy for the lottery analysis. Section III presents the findings that motivate our investigation of charter effect heterogeneity. Section IV outlines the econometric framework used to investigate this heterogeneity and reports the results of our investigation. These results show that students at urban charters in the lottery sample are typical of the urban student population, and that urban charter attendance boosts achievement well beyond ambient non-charter levels. Student demographics and baseline scores play a role in this – urban schools work best for minority students and students with low baseline scores – but non-urban charters are largely ineffective. Section V compares observational and lottery-based estimates in the subsample for which a lottery-based analysis is possible and discusses a school-level analysis of the observational estimates. Urban and lottery-sample charter effectiveness can be explained by adherence to a *No Excuses* approach to urban education that emphasizes instruction time, comportment, and focuses on traditional math and reading skills. Conditional on *No Excuses* status, factors such as time in school and teacher characteristics have little predictive value for school-specific effects. Finally, consistent with the *No Excuses* explanation of the urban charter advantage, we show that urban charter attendance boosts the likelihood that charter applicants are subject to disciplinary action, while non-urban charter attendance has no effect on this outcome.

II Lottery Analysis: Data and Empirical Strategy

We attempted to collect lottery data for the set of Massachusetts charter schools serving middle and high school grades and meeting a set of pre-specified eligibility criteria.[1] The school selection process is detailed in Table 1. To be eligible for our analysis, schools had to accept students in the relevant entry grades (4th-7th grade for middle school and 9th grade for high school). We excluded closed schools and alternative schools serving non-traditional populations (usually students at risk of dropping out). We also excluded schools that opened after the 2009-2010 school year. The resulting set of eligible schools includes 27 of the 54 charters serving middle school grades and eight of 37 schools serving high school grades.[2] Three eligible schools serve both middle and high school grades, so there are 32 eligible campuses.[3] Some eligible schools are not included in the lottery analysis; some were under-subscribed, while others failed to keep sufficient lottery records. The final sample of over-subscribed schools with usable records includes 16 middle schools and six high schools. These schools are listed in Table A1. Nine of the lottery-study participating middle schools are in urban areas, with seven of these in Boston, one inside Interstate Highway 495, and one near the Rhode Island border. The other seven are in non-urban areas, three in the center of the state, one on Cape Cod, two inside I-495, and one near the New Hampshire border. Four participating high schools are in Boston. One non-urban high school in the lottery sample is on Cape Cod, and the other is near Springfield.

Much of our analysis focuses on differences between charter schools in urban and non-urban areas. This distinction is motivated by the evidence, reported here and elsewhere, that charter schools serving heavily minority, high-poverty student populations in urban areas are more likely to boost achievement than are other sorts of charter schools, and by similar findings for Catholic schools (Gleason et al., 2010; Grogger et al., 2000).[4] To document differences in charter school

[1]We focus on middle and high schools because data for elementary school lotteries are much less widely available. Moreover, pre-lottery test scores – a key component of the observational analysis – are unavailable for elementary school applicants.

[2]Many charters extend through the high school grades but do not have entrance lotteries for high school.

[3]Schools are classified as both middle and high if they have entrance lotteries at both levels, or if lottery records at the middle school level were available early enough for participants to be observed in high school. Our universe includes 69 unique schools.

[4]We define urban areas to be those in which the local district superintendent participates in the Massachusetts Urban Superintendents Network. In our sample, the distinction between urban and non-urban charter schools is essentially identical to splits based on fraction eligible for free lunch or fraction minority.

practices across areas, we surveyed the full set of eligible charter schools, regardless of the quality of their lottery records. Among 32 eligible schools, 28 school administrators completed this survey; we received a total of 30 responses since two surveyed schools are eligible at both the middle and high school levels.

Our survey revealed important differences between urban and non-urban charter schools. Table 2 summarizes the survey responses. Urban schools are younger than non-urban schools; in Spring 2010, the average urban school had been open for 8.2 years, while the average non-urban school had been open for 11.4 years. Urban charter schools also run a longer school day and year than do non-urban schools. The average urban charter year lasts 189 days and has a school day of 464 minutes, compared to 183 days and 422 minutes at non-urban schools. The additional time appears to go to increased math and reading instruction; urban schools spend 35 extra minutes per day on math and 40 extra minutes per day on reading. Urban charter schools are also 38 percent more likely to have Saturday school.

Our survey also covers aspects of school philosophy and organization. Urban charter schools are more likely than non-urban charters to require parents to sign a contract (82 percent compared to 46 percent), to require students to sign a contract (71 percent compared to 55 percent), and to use uniforms (88 percent compared to 73 percent). Urban charter schools are also much more likely to use a formal reward and punishment system to shape student behavior; 65 percent of urban schools use such a system, while only 18 percent of non-urban schools do so. The survey results reveal a sharp division between urban and non-urban charters with respect to the *No Excuses* approach to education. As discussed by Thernstrom and Thernstrom (2003) and Carter (2000), *No Excuses* principles include a strict disciplinary environment, an emphasis on student behavior and comportment, extended time in school, and an intensive focus on traditional reading and math skills. Seventy-one percent of urban charter administrators identify somewhat or fully with *No Excuses*, while no non-urban charter identifies with this approach.

The bottom rows of Table 2 compare the inputs and resources used by urban and non-urban charter schools. Urban schools are more likely to be eligible for Title I status and have somewhat higher per-pupil expenditures than non-urban charter schools ($14,095 compared to $11,090). On the other hand, student/teacher ratios at urban and non-urban charters are similar. Urban schools have younger teachers as measured by proportions under age 32 and over age 49, and are more likely to hire paid tutors to work with their students. Teacher departures, requirements to take student calls after hours, and the use of unpaid tutors and volunteers are similar across the two types of schools.

Student Data

The student-level data used in our analysis comes from an administrative record-keeping system with complete coverage of the students enrolled in Massachusetts' public schools.[5] Our coverage period runs from the 2001-2002 school year through the 2009-2010 school year. The administrative records include information on student race/ethnicity, gender, special education status, limited English proficiency status, free/reduced-price lunch status, town of residence, and school(s) of attendance, as well as raw and scaled scores on Massachusetts Comprehensive Assessment System (MCAS) exams. The MCAS is a set of high-stakes standardized tests given to students in Massachusetts' public schools in grades 3 through 8 and 10. The primary outcomes analyzed in our study are MCAS scores in math and English Language Arts (ELA). Outcomes are post-lottery test scores in grades 4 through 8 for middle school and 10 for high school. The data appendix provides details on the availability of outcomes for each applicant cohort. For the purposes of this project, raw MCAS scores were standardized to have mean zero and standard deviation one by subject, grade level, and year.

Our data processing protocol assigns students to a single school for every year they appear in the data, even if they attended more than one school in a given year. Typically, students appearing on the roster of more than one school were assigned to the school they attended longest, though students with any time in a charter school in a given year are coded as having been a charter student for the year. If a student attended more than one charter, the student was assigned to the charter he or she attended the longest.

The analysis sample for the lottery study was constructed by matching applicant records from the 16 participating middle schools and six participating high schools to administrative records using applicants' name, year, and grade. Where available, information on date of birth, town of residence, race/ethnicity, and gender was used to break ties. Ninety-two percent of applicants were matched. Applicants were excluded from the lottery analysis if they were disqualified from the lottery they entered (this mostly affected applicants to the wrong grade level). We also dropped siblings of current students, late applicants, and out-of-area applicants.[6] Students missing baseline demographic information in the state database were dropped as well.

[5]This is known as the Student Information Management System, or SIMS. See the data appendix for details.

[6]Charter schools typically give priority to sibling applicants, as well as to students in the local school district (or sometimes region) in which they are located. Our applicant risk sets (discussed in the next section) distinguish between in-area and out-of-area applicants for schools that take substantial numbers of both. At schools with fewer than five out-of-area applicants, those out-of-area were dropped.

Descriptive Statistics

We begin with a statistical picture of the Massachusetts student population in traditional public and charter schools. Table 3 shows descriptive statistics for students enrolled in traditional public schools, students enrolled in eligible charter schools, and the sample of students who applied to oversubscribed charters participating in the lottery study, separately for urban and non-urban areas. Traditional schools are defined as those that are not charters, alternative, special education, exam, or magnet schools. For the six groups described in the table, we report average demographic characteristics, program participation rates, and average baseline test scores. Baseline scores are from 4th grade for middle school and 8th grade for high school.

Traditional urban students look very different from traditional students in the rest of the state. Specifically, urban students are more likely to be black or Hispanic, to be English language learners (or of limited English proficiency, LEP), to participate in special education, and to receive a subsidized lunch. Urban students also have much lower baseline test scores than other public school students: urban students score 0.43σ and 0.46σ below the state average on math and ELA tests at the middle school level, respectively, and they score 0.42σ and 0.39σ below the average at the high school level. In contrast, non-urban students score 0.21σ and 0.23σ above the average at the middle school level; the corresponding non-urban advantages in high school are 0.27σ and 0.28σ.

Eligible charter school students who live in urban and non-urban areas are more similar to their peers in regular public schools than to one another. There are, however, important differences by charter status as well. Urban charter schools serve a much higher fraction of black students than do urban public schools. Urban charter students are also less likely to be limited English proficient, to participate in special education, or to qualify for subsidized lunch. Charter school students in both urban and non-urban areas have slightly higher baseline test scores than their public school counterparts. Applicants to charter schools with observed entrance lotteries are similar to the population of enrolled charter students in both urban and non-urban areas.

Empirical Strategy

The lottery-based identification strategy captures causal effects for applicants to over-subscribed charters with high quality lottery records. The second-stage equation in this context is

$$y_{igt} = \alpha_{2t} + \beta_{2g} + \sum_j \delta_j d_{ij} + X_i'\theta + \tau s_{igt} + \epsilon_{igt}, \tag{1}$$

where y_{igt} is a test score for student i in grade g in year t, α_{2t} and β_{2g} are year and grade effects, X_i is a vector of pre-lottery demographic characteristics (race, special education, limited

English proficiency, subsidized lunch status, and a female-minority interaction), and ϵ_{igt} represents random fluctuations in test scores. The set of d_{ij} includes a separate dummy variable for every combination of observed charter school lotteries (indexed by j) entered by students in the lottery sample. In what follows, we refer to these as "risk sets." The variable of interest, s_{igt}, measures years spent in charter schools between application and test dates.[7] The parameter τ captures the causal effect of charter school attendance.

OLS estimates of equation (1) may be biased because students do not choose to attend charter schools randomly. We therefore use a dummy variable, Z_i, indicating lottery offers as an instrument for time spent in charter school. The first stage for our 2SLS procedure is

$$s_{igt} = \alpha_{1t} + \beta_{1g} + \sum_j \kappa_j d_{ij} + X_i'\mu + \pi Z_i + \eta_{igt}, \qquad (2)$$

where π is the effect of a lottery offer on charter attendance. As in the second stage equation, the first stage includes risk set controls and baseline demographic characteristics, as well as year and grade effects. Over-identified models introduce risk-set-specific first-stage effects ($\pi_j * d_{ij} Z_i$).

Because lottery offers are randomly assigned within risk sets, they are likely to be independent of family background, student ability or motivation, and any other unobserved characteristics of charter applicants. The appendix presents evidence in support of our lottery-based identification strategy. Specifically, Table A2 shows that conditional on risk set, winning the lottery is uncorrelated with student characteristics, which suggests that randomization was successful. Table A3 shows that we find followup scores for 91 percent of middle school applicants and 78 percent of high school applicants. In middle school, we are one percent more likely to find followup scores for lottery winners, but this imbalance is unlikely to explain the treatment effects discussed below.

Equations (1) and (2) describe a just-identified system with one endogenous variable and one instrument. The 2SLS estimate of τ can be obtained by taking the ratio of the reduced form effect of Z_i on y_{igt} and the first stage effect of Z_i on s_{igt}. The reduced form has the same data structure and regressors as equation (2), replacing s_{igt} with y_{igt} on the left-hand side. In an effort to increase the precision of our estimates, we also estimate overidentified models that allow the first stage effect to vary by risk set. In regressions for high school, only 10th grade test scores are included, and we cluster standard errors at the school-grade-year level. In regressions for middle school, we include the full set of non-repeat post-lottery test scores through 8th grade and add a second layer of clustering at the student level.

[7]Our definition of s_{igt} includes years spent at any charter school, including those without available lottery records. This specification is based on a simple benchmark with homogeneous treatment effects across charter schools. We investigate school-level heterogeneity in Section V.

Differences in effectiveness between urban and non-urban charter schools are a primary focus of our analysis, so we also generate separate estimates for these two groups. These estimates are produced using equations of the form

$$y_{igt} = \alpha_{2t} + \beta_{2g} + \sum_j \delta_j d_{ij} + X_i'\theta + \tau_u s_{igt}^u + \tau_n s_{igt}^n + \epsilon_{igt}, \tag{3}$$

where s_{igt}^u and s_{igt}^n are years in urban and non-urban charter schools. The first stage for urban attendance can be written

$$s_{igt}^u = \alpha_{1t} + \beta_{1g} + \sum_j \kappa_j d_{ij} + X_i'\mu + \pi_u Z_i^u + \pi_n Z_i^n + \eta_{igt}, \tag{4}$$

where Z_i^u and Z_i^n indicate offers from urban and non-urban lotteries, with a similar specification for non-urban attendance.

III Lottery Estimates

Charter school lottery offers increase the average duration of charter school attendance sharply. The first stage estimates reported in column (1) of Table 4 show that among applicants to charter middle schools, students who win a charter school lottery spend about 1 more year in a charter before being tested than do students not offered a seat. Applicants who win high school lotteries spend about half a year more attending a charter school than applicants who lose the lottery before taking MCAS tests. These first stage estimates are similar to those reported in Abdulkadiroğlu et al. (2011) for a smaller sample of charter schools in Boston.

Middle school lottery winners outscore lottery losers by 0.06σ in ELA and 0.21σ in math. These reduced form estimates can be seen in column (2) of Table 4. High school lottery winners outperform lottery losers by about 0.11σ in ELA, 0.16σ in math, 0.16σ in Writing Composition, and 0.14σ in Writing Topic Development. These estimates, like the middle school results, are precisely estimated and significantly different from zero.

Because the middle school first stage is close to one, middle school 2SLS estimates differ little from the corresponding reduced form estimates. The estimates shown in column (3) of Table 4 imply that a year of attendance at a lottery sample charter middle school increases ELA scores by 0.07σ and math scores by 0.21σ. Column (4) reports 2SLS estimates from overidentified models that include a full set of risk set and offer interactions as instruments. The results here are close to the just-identified estimates in column (3), though estimates from the overidentified models are slightly more precise.

The high school 2SLS estimates imply larger causal effects than those found for middle schools. The score gains generated by time spent in charter high schools are on the order of

0.22σ per year for ELA and 0.32σ per year for math. Writing gains also are estimated to be substantial; a year of charter attendance increases Writing Topic scores by 0.30σ and Writing Composition scores by 0.27σ. As in middle school, the high school results from overidentified models are close to the corresponding just identified estimates.

Estimates for the pooled state sample mask considerable heterogeneity by school type, a pattern documented in Table 5. Although first stage effects at urban and non-urban middle schools are similar, second stage middle school estimates differ sharply. The 2SLS estimates for urban middle schools, reported in column (3) of Table 5, suggest these schools generate gains of about 0.14σ in ELA and 0.34σ in math per year. By contrast, the estimates for non-urban charter middle schools are negative. In particular, as can be seen in column (6), charter students at non-urban middle schools are estimated to lose ground relative to their public school peers at a rate of 0.16σ per year in both ELA and math. Not surprisingly, the high school lottery results for urban schools are similar to the statewide results (since only two of the high schools in the state sample are non-urban). The 2SLS estimates for non-urban charter high schools are uniformly negative, but too imprecise to be conclusive.

Subgroup variation in charter effects is documented in Tables 6 and 7, separately for urban and non-urban schools. These tables report the results of estimating equation (3) by 2SLS in various subsamples of students. Urban charter schools boost scores for most subgroups, though not uniformly. Girls realize larger gains in math, while boys see larger ELA gains. Black and Hispanic students benefit considerably from urban charter attendance in middle school, but the estimated math gains for whites are smaller and there is no increase in whites' ELA scores. Urban charter middle schools appear to produce especially large achievement gains for students eligible for a subsidized lunch and for those with low baseline scores. Attendance at urban charter high schools increases math scores in every group and raises reading scores for everyone except whites, though positive high school effects on reading are not always significantly different from zero.

Non-urban charter attendance fails to raise scores for any of the subgroups examined in Table 7, apparently reducing achievement for girls, whites, and students with low baseline scores. Estimates for black and Hispanic students are not significantly different from zero. A year of non-urban charter middle school reduces free-lunch students' ELA scores by 0.20σ, and reduces math scores in this group by 0.17σ, though the latter estimate is not statistically significant. Most of the estimates for non-urban high school charters are negative, though effects here are imprecise (estimates for black and Hispanic students are omitted due to small samples sizes in these non-urban groups).

IV Differences in Students

We investigate student- and school-level explanations for the striking difference in causal effects at urban and non-urban charter schools. The student-level analysis is cast in a semiparametric framework with heterogeneous potential outcomes, indexed against a Bernoulli treatment, $D_i \in \{0, 1\}$, to indicate charter attendance. The Bernoulli setup focuses on heterogeneity while abstracting from nonlinearities that seem second-order in this context (since the first stage effects of lottery offers are similar in the two settings for middle school, yet the effects there differ most dramatically). Let Y_{1i} and Y_{0i} denote potential test scores for student i in and out of charter schools, and let $U_i \in \{u, n\}$ indicate residence in an urban or non-urban area. The observed outcome for student i is

$$Y_i = Y_{0i} + (Y_{1i} - Y_{0i})D_i.$$

In other words, we observe Y_{0i} for applicants who don't go to charter school and Y_{1i} for those who do.

Although our empirical work uses data from many lotteries, the analysis of heterogeneity is explained with reference to a single lottery. Offers in this lottery are indicated by Z_i, as before. Potential treatment assignments, denoted D_{1i} and D_{0i}, tell us whether student i attends a charter school if he wins or loses the lottery. Offers are randomly assigned and assumed to affect test scores only through charter attendance, so the potential outcome vector $(Y_{1i}, Y_{0i}, D_{1i}, D_{0i})$ is independent of Z_i. We also assume that winning an entrance lottery can only make charter attendance more likely, so that $D_{1i} \geq D_{0i}\ \forall i$, with strict inequality for some students.

Under these assumptions, instrumental variables estimation using Z_i as an instrument for D_i in the sample of lottery applicants produces a local average treatment effect (LATE; Imbens and Angrist, 1994). Here, LATE is the effect of charter attendance for students induced to enroll in a charter school by winning an admissions lottery (the compliers, who have $D_{1i} > D_{0i}$). When computed separately for urban and non-urban students, IV estimates identify

$$\begin{aligned} \tau_l \equiv & \frac{E_l[Y_i|Z_i = 1] - E_l[Y_i|Z_i = 0]}{E_l[D_i|Z_i = 1] - E_l[D_i|Z_i = 0]} \\ & = E_l[Y_{1i} - Y_{0i}|D_{1i} > D_{0i}], \quad l \in \{u, n\}; \end{aligned}$$

where l indexes location and E_l denotes an expectation over students in location l. This is LATE in each setting.

We analyze three sources of student-level heterogeneity that might account for the difference between τ_u and τ_n. The first is the urban/non-urban difference in treated and non-treated counterfactuals (that is, distinct differences in average Y_{1i} and Y_{0i}). This investigation tells us whether the urban charter advantage reflects high scores in the treated state, low non-treated outcomes, or both. The second is variation in Y_{0i} across charter and non-charter students *within* each area. This tells us whether lottery compliers are unusual in either setting. Finally, we decompose the difference in charter effectiveness across urban and non-urban areas into a component due to differences in student populations and a component due to differences in effectiveness conditional on characteristics.

The Urban Gap in Treatment and No-Treatment Counterfactuals

The urban charter advantage can be broken down into two parts, the first capturing differences in potential outcomes in the treated state (differences in Y_{1i}) and the second capturing differences in potential outcomes in the non-treated state (differences in Y_{0i}). Specifically, we have

$$\begin{aligned}\tau_u - \tau_n = & \underbrace{E_u[Y_{1i}|D_{1i} > D_{0i}] - E_n[Y_{1i}|D_{1i} > D_{0i}]}_{\gamma_1} \\ & - \underbrace{(E_u[Y_{0i}|D_{1i} > D_{0i}] - E_n[Y_{0i}|D_{1i} > D_{0i}])}_{\gamma_0}.\end{aligned} \tag{5}$$

Here, γ_1 measures the difference in treated outcomes for compliers at urban and non-urban charter schools, while γ_0 measures the difference in non-treated outcomes between these two groups.

Pooling urban and non-urban charter applicants, we estimate γ_0 using

$$Y_i(1 - D_i) = \psi(1 - D_i) + \gamma_0(1 - D_i) \cdot 1\{U_i = u\} + \sum_j \delta_j d_{ij} + \epsilon_i, \tag{6}$$

with first stage

$$1 - D_i = \sum_j \kappa_j d_{ij} + \sum_j \pi_j d_{ij} Z_i + \eta_i. \tag{7}$$

The first stage equation for the interaction between $1 - D_i$ and urban status uses the same specification as equation (7).[8] For a model without covariates, Abadie (2003) shows that 2SLS estimation of this type of system produces estimates of marginal mean counterfactuals for compliers; in this case, the 2SLS estimate is the mean of Y_{0i} for compliers. (We estimate γ_1 using a model that replaces $(1 - D_i)$ with D_i in equations (6) and (7).) Our parameterization differs from Abadie's in two ways. First, we are interested in the *difference* in marginal mean outcomes between urban and non-urban compliers: ψ equals the average of Y_{0i} for lottery compliers in non-urban areas, while $\psi + \gamma_0$ is the average of Y_{0i} for compliers in urban areas. Second, our estimating equation includes a saturated model for risk sets. In this case, the 2SLS estimands are weighted averages of mean Y_{0i} for compliers across risk sets, with weights proportional to

[8]Since applicants to urban and non-urban charter schools are disjoint sets, the main effect for urban status is collinear with the d_{ij} and therefore omitted.

the variance of the first-stage fitted values in the risk set (this is a consequence of Theorem 3 in Angrist and Imbens 1995).[9]

Our analysis of counterfactuals is limited to middle schools since the sample of non-urban high school charter students is too small to produce useful estimates of γ_1 and γ_0. Columns (1) and (2) of Table 8 show 2SLS estimates of urban and non-urban charter effects using scores one year after application. Column (3), which reports $\tau_u - \tau_n$, shows that the difference in charter effects by urban status is 0.37σ in ELA and 0.72σ in math. Columns (4) and (5) show that differences in non-charter fallback can account for the full urban charter advantage in math and most of the advantage in ELA. The estimates of γ_0 imply that in public schools, non-urban compliers outscore urban compliers by 0.71σ in ELA and 0.63σ in math. In charter schools, non-urban compliers outscore urban compliers by only 0.33σ in ELA, and urban compliers score 0.09σ higher in math (though this estimate is not statistically significant).

Figure 1 presents a schematic representation of the results in Table 8. Urban charter middle schools serve populations with very low non-charter achievement, well below that of students in non-urban charters. The charter treatment pulls these students up to a level close to that of non-urban students (beyond these students in math, below them in ELA). Thus, it seems fair to see urban gains as recovery from a low base, though as we show next, this level is typical of all urban students in the state. By contrast, while non-urban charter students start out well ahead of their urban counterparts, the non-urban charter treatment pulls them back.

Non-treated Gaps in Urban and Non-urban Areas

Instead of comparing the no-treatment outcomes of urban compliers to the corresponding outcomes of non-urban compliers, we can benchmark achievement in each area using the *local non-charter mean.* This tells us whether the urban charter advantage is driven by unusually low no-treatment outcomes for compliers, or whether urban lottery compliers are, in fact, typical of their milieu. Figure 2 illustrates the alternative scenarios we have in mind: the left panel describes a situation in which the achievement of untreated urban students is comparable to ambient non-charter achievement, while the right panel describes a situation in which the urban fallback is unusually low.

The econometric analysis of within-area counterfactuals begins with a decomposition of urban

[9] For example, the probability limit of the 2SLS estimate of ψ in equation (6) is

$$\psi = \sum_{j \in \mathcal{NU}} \left(\frac{N_j \cdot \pi_j^2 \cdot Var(Z_i|d_{ij}=1)}{\sum_k N_k \cdot \pi_k^2 \cdot Var(Z_i|d_{ik}=1)} \right) E[Y_{0i}|D_{1i} > D_{0i}, d_{ij} = 1]$$

where $\mathcal{NU}$ is the set of non-urban lotteries and N_j is the number of students in risk set j.

and non-urban LATE as follows:

$$\tau_l = \underbrace{E_l[Y_{1i}|D_{1i} > D_{0i}] - E_l[Y_{0i}|D_i = 0]}_{\lambda_1^l} \qquad (8)$$
$$- \underbrace{(E_l[Y_{0i}|D_{1i} > D_{0i}] - E_l[Y_{0i}|D_i = 0])}_{\lambda_0^l}, \quad l \in \{u, n\}.$$

The term λ_0^l is the difference in average Y_{0i} between lottery compliers and the general population of non-charter students in the relevant area. The term λ_1^l is the difference between the treated outcomes of compliers and ambient non-charter achievement. In urban schools, for example, large λ_1^u and small λ_0^u mean that urban charters push their students beyond typical non-charter achievement in cities.

The decomposition in (8) is estimated using equations of the form

$$Y_i(1 - D_i) = \Delta(1 - D_i) + \sum_j \delta_j d_{ij} + \epsilon_i, \qquad (9)$$

estimated separately for urban and non-urban students, with the same first stage specification as equation (7). Here, the 2SLS estimand is a weighted average of Y_{0i} for lottery compliers across risk sets. To estimate $E[Y_{0i}|D_i = 0, U_i = l]$, we omit risk set controls and estimate equation (9) by OLS in a sample of students that includes both applicants and non-applicants. The OLS estimand is thus a simple average of Y_i for non-charter students in location l. Assuming that mean Y_{0i} is constant across risk sets for compliers, λ_0^l is the difference between the 2SLS and OLS estimates of Δ. λ_1^l is estimated by replacing $(1 - D_i)$ with D_i in equation (9).[10]

Estimates of equation (9) for urban middle and high schools appear in Table 9. Columns (2)-(4) show results from regressions that include non-applicants. Specifically, column (2) reports the average Y_{0i} for non-charter students, while column (3) shows λ_0^u, the difference in average outcomes for compliers and non-charter students. Estimates of λ_1^u, the difference between the treated outcomes of urban compliers and the ambient level of urban achievement, appear in column 4.[11] The estimates of λ_0^u suggest that urban lottery compliers are positively selected from the urban middle school population, but the estimated gaps are small, and marginally

[10] Standard errors for the difference between the 2SLS and OLS estimates were constructed using a stacked data set that includes two copies of each observation. Let $h \in \{1, 2\}$ index halves of the data, and define $E_{ihk} = 1\{h = k\}$ for $k \in \{1, 2\}$. We estimate

$$Y_{ih}(1 - D_{ih}) = \Delta_{2SLS} \cdot (1 - D_{ih}) \cdot E_{ih1} + \Delta_{OLS} \cdot (1 - D_{ih}) \cdot E_{ih2} + \delta \cdot E_{ih2} + \sum_j \delta_j \cdot d_{ij} \cdot E_{ih1} + \epsilon_{ih},$$

instrumenting $((1 - D_{ih}) \cdot E_{ih1})$ with $(Z_{ih} \cdot E_{ih1})$, and clustering standard errors by i as well as school-grade-year.

[11] Middle school scores are from the year after the lottery for applicants and 6th grade for non-applicants; high school scores are from 10th grade as always.

significant only for middle school ELA (λ_0^u for high school is virtually zero in both subjects). Because urban charter compliers have non-charter achievement levels that are fairly typical of students in urban areas, the large score gains generated by urban charter schools can be attributed to high scores in the treated state.

The last three columns of Table 9 compare the scores of urban compliers with a non-charter benchmark computed excluding students who do not apply to charter schools. The resulting estimates of λ_0^u, reported in column (6), are even smaller than those in column (3). Thus, among applicants to charter schools, the non-charter achievement levels of compliers and non-attenders are virtually identical. Urban charter schools therefore push the scores of compliers well beyond the average non-charter achievement levels of all of their applicants.

Table 10 reports estimates of λ_0^n and λ_1^n for students at non-urban middle schools. As in urban areas, the non-charter achievement level of non-urban compliers is slightly higher than that of students in the surrounding public schools. The ELA scores of non-urban compliers in public schools exceed the ambient non-urban achievement level by a statistically significant 0.12σ, while the estimate of λ_1^n for ELA is a precisely estimated -0.08σ. This implies that non-urban charter middle schools move their students from atypically high ELA achievement levels *down* to levels that are slightly below those of non-charter non-urban students. Non-charter math achievement of non-urban compliers is statistically indistinguishable from the ambient non-charter level, while non-urban charter attendance pulls compliers 0.19σ below the non-charter mean. As with urban applicants, the average level of Y_{0i} for non-urban compliers is very close to that of applicants who do not attend charter schools; both estimates of λ_0^n reported in column (6) are statistically insignificant and small. Since positive selection on Y_{0i} is reduced when non-applicants are excluded, the estimates of λ_1^n in column 7 show sharper declines than the corresponding estimates including non-applicants in column 4.

Combined with the estimates of γ_0 and γ_1 in Table 8, these results paint a consistent picture of the urban charter advantage. Urban middle school charters push the scores of their students from a typically low level up to a level much closer to the average level of achievement among non-urban charter students (the scenario sketched in the left panel of Figure 2). Non-urban charter middle schools reduce the scores of their students, in some cases markedly so. The non-urban high school sample is too small for precise comparisons of the outcomes of urban and non-urban compliers, but the results for urban charter high schools look broadly similar to those for middle schools: charter high schools raise the scores of urban students by pushing them beyond the level of high school achievement typical of urban areas.

Accounting for Student Demographics

We explore the role of student demographics in generating the urban charter advantage with the help of a decomposition in the spirit of Blinder (1973) and Oaxaca (1973). The first step uses the methods of Abadie (2003) to identify a linear local average response function for lottery compliers conditional on a vector of observable demographic variables, X_i. Specifically, we have

$$E_l[Y_i|D_{1i} > D_{0i}, D_i, X_i, d_{ij}] = X_i'\theta_l + \omega_l D_i + D_i X_i'\rho_l + \sum_j \delta_j d_{ij}, \quad l \in \{u, n\}. \tag{10}$$

This equation has a causal interpretation because conditional on being a complier, treatment (charter enrollment) is ignorable. Abadie (2003) shows that 2SLS using Bernoulli instruments for a Bernoulli treatment consistently estimates this sort of linear model for local average causal response.

Equation (10) generates the following parameterization of the urban/non-urban difference in charter school attendance effects:

$$\tau_u - \tau_n = (\omega_u - \omega_n) + \bar{X}_n'(\rho_u - \rho_n) + (\bar{X}_u' - \bar{X}_n')\rho_u, \tag{11}$$

where

$$\bar{X}_l \equiv E_l[X_i|D_{1i} > D_{0i}].$$

The last term in equation (11) captures the part of the urban charter advantage explained by differences in demographics. In particular, this term tells us how much smaller the effects of urban charter schools would be if they served the same mix of students as do non-urban schools. The first two terms capture the component of the urban advantage attributable to differences in effects within demographic groups.

Here, as always, Blinder-Oaxaca decompositions can be presented in two ways. In this case, the urban/non-urban difference in charter school effects can be decomposed with differences in means weighted by non-urban charter impacts instead of urban. Specifically, we can write

$$\tau_u - \tau_n = (\omega_u - \omega_n) + \bar{X}_u'(\rho_u - \rho_n) + (\bar{X}_u' - \bar{X}_n')\rho_n. \tag{12}$$

Like equation (11), this expression includes components associated with differences in demographics and differences in effectiveness conditional on demographics. The last term measures how much more effective non-urban charter schools would be if their students were demographically similar to the urban charter population.

We construct these decompositions by estimating

$$Y_i = X_i'\theta_l + \omega_l D_i + D_i X_i'\rho_l + \sum_j \delta_j d_{ij} + \epsilon_i$$

by 2SLS, separately for urban and non-urban applicants, with first stage

$$D_i = X_i'\mu_l + \pi_l Z_i + Z_i X_i'\zeta_l + \sum_j \kappa_j d_{ij} + \eta_i \tag{13}$$

for D_i and similar first stages for interaction terms involving D_i. The covariate vector, X_i, includes sex, race, special education status, limited English proficiency status, free lunch status, and dummies for performance at the advanced, proficient, or needs improvement level on baseline math and ELA tests.[12] Complier means for each component of X_i are estimated using the kappa-weighting procedure described in Abadie (2003).

Blinder-Oaxaca decompositions suggest that favorable demographics enhance urban charter effectiveness, but differences in student populations do not fully account for the urban charter advantage. This can be seen in Table 11, which reports the components of equations (11) and (12) for middle schools. (The non-urban high school samples are too small to admit meaningful investigations of effect heterogeneity using this approach.) Column (1) shows the difference in charter middle school treatment effects by urban status.[13] Columns (2) and (3) report the components of decomposition (11), which multiplies the urban/non-urban difference in demographics by treatment effects for urban schools. Column (2) shows how urban effectiveness might change if urban schools were to serve the non-urban population. These results suggest that 63 percent of the urban advantage in ELA (0.25/0.40) can be explained by student demographics. The corresponding estimate for math is 49 percent. Urban schools are especially effective for poor and minority students, and they serve more of these students than do non-urban schools. On the other hand, column (3) shows that even with the same student mix as non-urban charter schools, urban charters would be more effective than non-urban charters, especially in math. The urban charter advantage can therefore be attributed to a combination of student demographics and larger treatment effects within demographic groups.

At the same time, columns (4) and (5), which report the results of estimating decomposition (12) using non-urban treatment effects to load covariate differences, show that the urban charter advantage would shrink little if non-urban schools served an urban demographic: student characteristics account for only 14 percent (0.06/0.40) and 37 percent (0.25/0.68) of the urban advantages in ELA and math, respectively. The results here are much less precise than those based on decomposition (11), reflecting the fact that ρ_n is estimated less precisely than ρ_u. Still, this juxtaposition provides a useful summary of the underlying finding that non-urban charter schools are largely ineffective across subgroups.

[12]These score categories are used to determine whether schools in Massachusetts meet the Adequate Yearly Progress (AYP) standard under No Child Left Behind (NCLB).

[13]These differences differ slightly from those reported in Table 8 because equation (13) imposes first stage coefficients that are constant across risk sets, while the earlier estimates allow the first stage coefficients to vary.

V Differences in Schools

Our exploration of school-level heterogeneity in achievement effects is founded on observational estimates. Specifically, controlling for observable student characteristics, we estimate nonexperimental treatment effects for every eligible charter school in the state (that is, for middle and high schools serving traditional students, open during the relevant time period, and meeting the entry grade restrictions described in Section II). This observational identification strategy is first validated by comparing observational and lottery-based estimates in the lottery sample.

The validated observational analysis serves two purposes. First, an observational identification strategy allows us to compare effects for eligible charter schools with and without lottery records. If oversubscribed schools with usable lottery records differ systematically from other schools, then our lottery analysis may give an incomplete picture of charter effectiveness. Second, since the observational analysis includes schools and cohorts without available lottery information, we can use this approach to generate more precise school-specific estimates.

Observational Framework

Our observational estimates use a combination of matching and regression to control for observed differences between students attending different types of schools. Specifically, students attending lottery-eligible charters are matched to a control sample with the same baseline school, baseline year, sex, race, limited English proficiency status, special education status, and subsidized lunch status. Charter students are matched if they fall into a cell that includes at least one regular public school student; likewise, regular public school students are matched if they fall into in a cell that includes at least one student in an eligible charter school. Therefore, every charter student in the matched sample is compared to at least one demographically similar student from the same cohort and sending school. This procedure yields matches for 77 percent of students in eligible charter schools.

Within the matched sample, causal effects (denoted τ_o) are estimated using the following model for student i from cell c, observed in grade g in year t:

$$y_{igtc} = \alpha_t + \beta_g + \iota_c + b_i'\theta + S_{igts}'\tau_o + \epsilon_{igts} \qquad (14)$$

S_{igtc} is a vector of years spent in schools of various types (eligible charter schools, ineligible charter schools, alternative schools, and exam schools in urban and non-urban areas) for student i from baseline through year t, and b_i is a vector of student i's baseline scores on math, ELA, and (in high school) Writing Topic and Writing Composition tests. Importantly, these models also include fixed effects for the cells constructed in the matching procedure (represented by ι_c), so that the observational regressions implicitly compare the outcomes of demographically similar students from the same sending schools and cohorts who later spend different amounts of time in charter schools. The middle school analysis looks at effects on test scores in grades 5 through 8, while the high school analysis looks at 10th grade scores. Standard errors are clustered as in the lottery analysis.

The comparison of observational and lottery-based estimates is encouraging. Table 12 reports estimates of a version of equation (14) that distinguish between eligible charter schools with and without usable lottery records. As shown in columns (1) and (2), observational estimates for schools in the urban lottery sample are strikingly similar to the lottery results. For example, the observational regressions suggest that a year in an urban lottery school increases middle school scores by 0.17σ and 0.28σ in ELA and math; the corresponding lottery-based estimates are 0.14σ and 0.34σ. The observational and lottery estimates for urban charter high schools are also close. These results suggest that the combination of matching and regression accounts for much of the selection into charter attendance in urban areas, an important finding in its own right. Interestingly, estimates for non-lottery urban schools are smaller than the corresponding estimates for lottery schools; for middle school, the estimates for non-lottery schools are negative and statistically significant. This is further evidence of the importance of school-level heterogeneity in charter attendance effects.

Among estimates of attendance effects at eligible non-urban charter schools, the match between lottery estimates and observational results for schools with lottery records is not as good as for urban schools, though the two research designs generate qualitatively similar conclusions. For middle schools, estimated effects are negative using both lottery-based and observational techniques, but the observational estimates are considerably smaller. Observational estimates for high schools suggest small positive effects of non-urban charter attendance, while the lottery estimates are negative (though imprecise). Observational estimates for non-urban middle schools are reasonably similar across the lottery and non-lottery samples. Since both of the eligible non-urban high schools are part of the lottery sample, estimates for non-urban non-lottery high schools are not reported.

Explaining School-Specific Effects

Lottery and observational identification strategies generate broadly similar estimates in the sample of schools where they can be compared. This finding motivates an analysis of school-specific treatment effects estimated with observational techniques. The school-specific estimates come from a version of equation (14) that includes separate variables measuring years spent in each eligible charter school. These estimates are then linked to school policies and characteristics using the following school-level regression:

$$\hat{\tau}_s = \phi_0 + \phi_1 U_s + \phi_2 L_s + \phi_3 H_s + P_s' \phi_4 + u_s, \tag{15}$$

where $\hat{\tau}_s$ is an observational estimate of the effect of charter school s, U_s is an urban dummy, L_s is a lottery sample dummy, H_s is a high school dummy, and P_s is a vector of school policies and characteristics measured in our survey. The estimates of this equation are weighted by the reciprocal of the standard error of the estimated treatment effect. Standard errors are clustered at the school level to account for the fact that some schools contribute both middle and high-school estimates to the sample.

Not surprisingly given our earlier findings, estimates of equation (14) show substantially larger treatment efffects at urban and lottery sample schools. These effects are reported in columns (1) and (5) of Table 13, which show estimates of equation (15) including only U_s, L_s, and H_s on the right-hand side. Eligible urban schools produce achievement gains that are 0.20σ and 0.12σ larger than the effects of non-urban schools in math and ELA; lottery-sample schools generate gains that are 0.15σ and 0.10σ larger than the effects of non-lottery schools. Columns (2) and (4) add instruction time (minutes per day and in the relevant subject) and per-pupil expenditures to the model. Increased time in the classroom is increasingly promoted as a means of increasing student achievement; in 2006, the Massachusetts state legislature approved a program to extend the school day by two hours in a small set of schools, motivated in part by the long days at successful charter schools (Pennington, 2007). The achievement effects of per-pupil expenditures are of longstanding interest to researchers and policy-makers; increasing per-pupil expenditures in regular public schools is often seen as an alternative to more structural reforms (Hanushek, 1997). School-environment variables can indeed account for a substantial fraction of the larger treatment effects produced by urban and lottery sample charter schools: urban and lottery coefficients fall substantially for both subjects, though the urban ELA and lottery math coefficients remain statistically significant. On the other hand, only the total time variable generates a marginally significant effect, while the expenditure coefficient is essentially zero.

Extended learning time is one of a number of features of the *No Excuses* approach. The estimates in columns (3) and (7) of Table 13 come from models that swap a dummy for schools that subscribe to *No Excuses* for the school environment variables used to construct the estimates reported in columns (2) and (6).[14] *No Excuses* status fully accounts for the urban and lottery advantages in both math and ELA, without controlling for other features of the school environment. *No Excuses* charter schools generate math and ELA gains that are 0.31σ and

[14] The *No Excuses* variable used for this exercise is coded as one for schools described by survey respondents as fully or somewhat *No Excuses*. Results using a dummy for full *No Excuses* status only are similar.

0.17σ larger than the effects of other charters. As shown in columns (4) and (8), the addition of school environment variables pulls the *No Excuses* effects down somewhat, but their inclusion does not change the basic story, and these variables do not themselves generate statistically significant effects conditional on *No Excuses* status.

Discipline

Comportment and discipline are often said to be defining features of *No Excuses* charter schools; if urban charter effectiveness is due to the *No Excuses* approach, we might therefore expect to see a marked impact on disciplinary outcomes. Table 14 reports 2SLS estimates of equation (1) for suspensions and truancy in the year following applicant lotteries. The results for urban schools, reported in columns (1) through (3), are striking. Urban charter attendance is estimated to increase suspensions by 0.88 days in middle school and more than a full day in high school. These treatment effects exceed mean suspension rates in the lottery sample (0.62 days for middle school and 0.44 days for high school). The estimates for both middle and high school show significant increases in out-of-school suspensions, and smaller (though still substantial) increases in in-school suspensions. Though less precise, the results for truancy suggest that attendance at an urban charter high school reduces days of unauthorized absence – the truancy effect is statistically significant in models that include baseline test score controls. The truancy estimates for middle school are not significantly different from zero.

In contrast with the estimated effects of urban charter attendance on discipline, the estimates for non-urban charter schools show little effect. Non-urban estimates, reported in columns (4) through (6) of Table 14, are small, and none are significantly different from zero. These results sharpen the distinction between urban and non-urban charters. Attendance at urban *No Excuses* charter schools produces large effects on discipline as well as achievement; attendance at other charter schools has little effect in either domain.[15]

VI Conclusions

Massachusetts' urban charter schools generate large achievement gains, while non-urban charters appear to be largely ineffective and appear to reduce achievement for some. Candidate explanations for this constellation of findings include the fact that urban charter schools serve larger shares of minority students in districts where the surrounding achievement level is gen-

[15] Observational estimates of effects on discipline closely match the lottery estimates for schools in the lottery sample. Observational estimates in the sample of all eligible schools suggest that urban non-lottery schools have much smaller effects on suspensions than do urban lottery schools, though discipline effects at non-lottery schools are also positive.

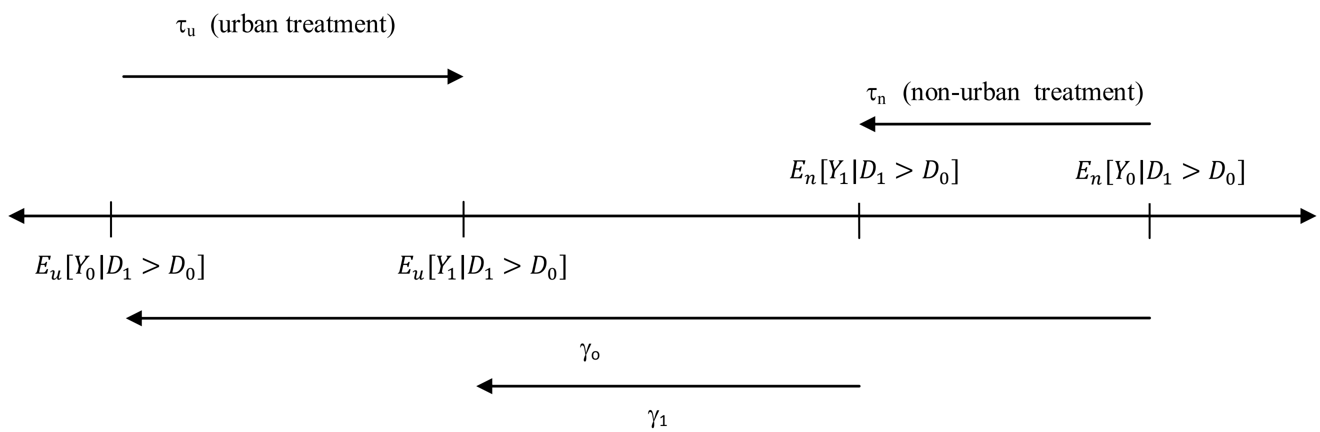
erally low, keep their students in school longer, spend more money per-pupil, and are much more likely to identify with the *No Excuses* instructional approach than are non-urban schools. Our analysis examines the contribution of these student- and school-level factors to the urban charter advantage.

Massachusetts' urban charter schools, including the over-subscribed schools at the heart of our lottery analysis, serve a typical urban population with non-charter achievement below the average in non-urban areas. On average, urban charters push their students well beyond ambient non-charter achievement in central cities, while non-urban charter schools leave their students' achievement essentially unchanged or diminished from a higher starting point. Urban charter schools are most effective for minorities, poor students, and low baseline achievers, so part of the urban charter advantage can be explained by student demographics. On the other hand, non-urban charter schools fail to boost achievement for any group.

Our analysis also reveals important heterogeneity within the set of urban schools. The over-subscribed schools with well-documented admissions processes that make up our lottery sample appear to be more effective than other urban charters. An analysis of school-specific treatment effects suggests that adherence to the *No Excuses* paradigm can account for both the urban and lottery-sample charter advantages. Learning time and per-pupil expenditures are not strongly correlated with school-specific impacts and do not explain differences in effectiveness after accounting for *No Excuses* status. Consistent with a *No Excuses* explanation of the urban charter advantage, the large achievement gains generated by urban charter schools are mirrored by substantial effects on disciplinary outcomes in the urban sample.

The large negative estimates of non-urban charter impacts reported here raise the question of why, despite their unimpressive achievement effects, many of these schools are over-subscribed. One possibility is that parents misjudge the consequences of non-urban charter attendance. In a related setting involving school choice, Rothstein (2006) argues that parental choice is driven primarily by peer characteristics rather than school effectiveness. Of course, it's also possible that non-urban charter schools generate gains that non-urban families value more than the skills measured by the MCAS, especially in view of the fact that most non-urban students do reasonably well in any case. Still, it seems unlikely that non-urban parents would see a deterioration in basic skills as desirable. In ongoing work, we're looking at a variety of post-secondary outcomes in an effort to determine whether the heterogeneous findings for achievement reported here have longer-term consequences. We also hope to investigate the effectiveness of *No Excuses* education for non-urban students by drawing new samples of students and schools from other states.

ELA:



Math:

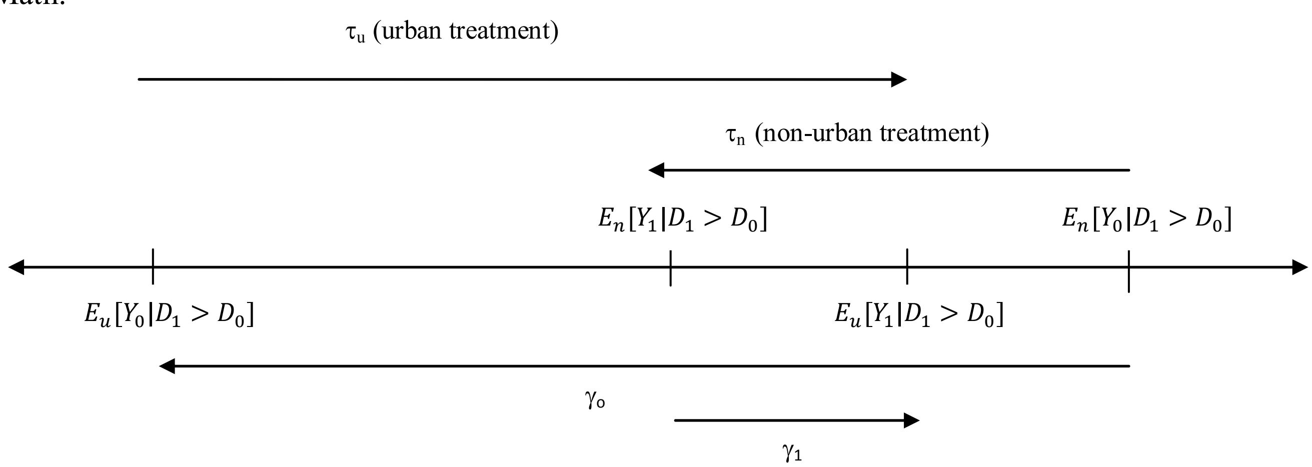


Figure 1: Gaps in Treatment and No-Treatment Counterfactuals, Urban vs. Non

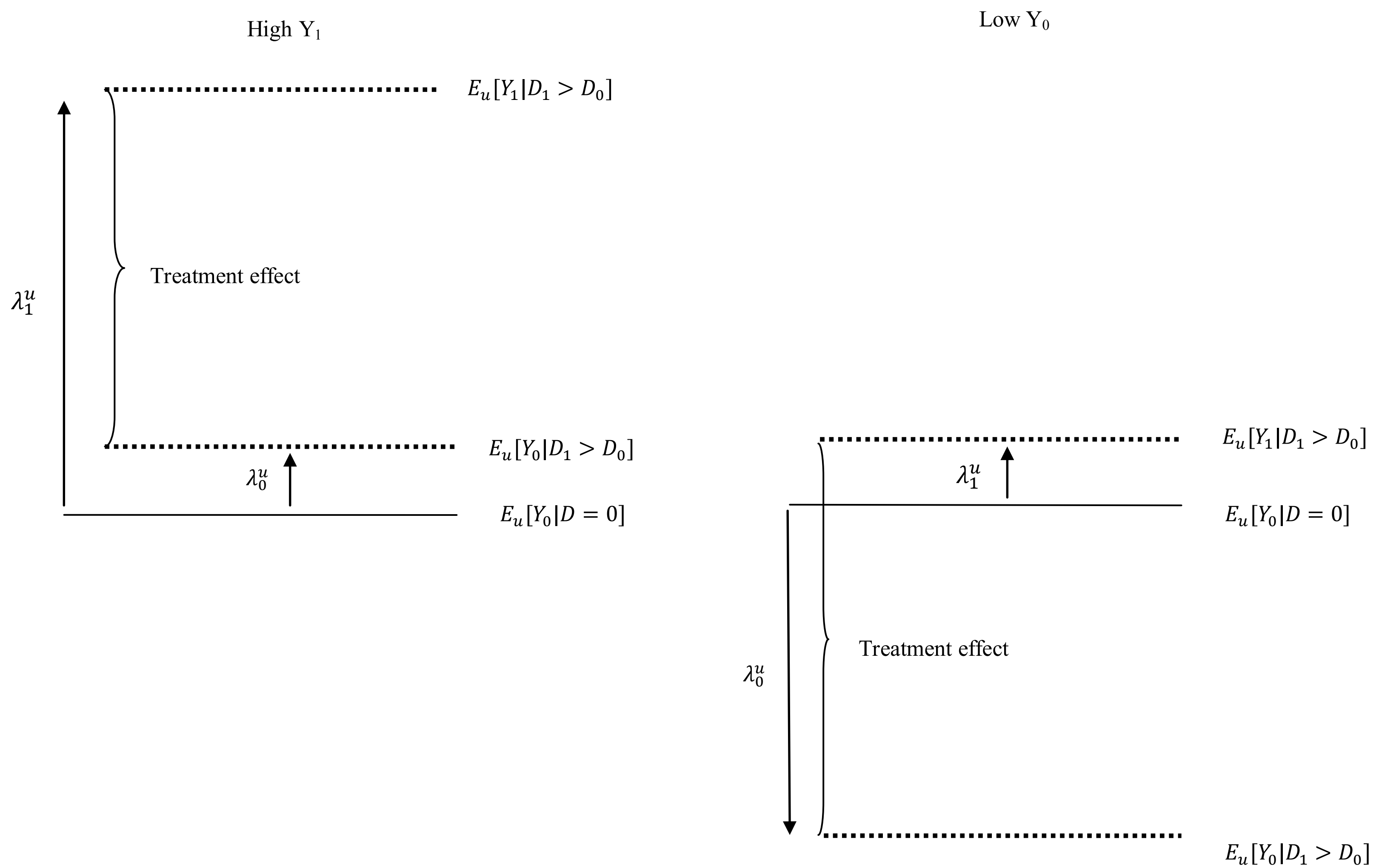


Figure 2: Treatment Effects in Urban Areas

Table 1: School Participation

School level	Urban status	Boston status	All charters (1)	Middle (entry in 4-7) and high (entry in 9) school charters* (2)	Charters eligible for lottery study (3)	Charters included in lottery study (4)
Middle						
	Urban		35	21	16	9
		Boston	13	10	8	7
		Non-Boston	22	11	8	2
	Nonurban		19	12	11	7
	Total (Urban and Nonurban)		54	33	27	16
High						
	Urban		25	10	6	4
		Boston	10	7	5	4
		Non-Boston	15	3	1	0
	Nonurban		12	3	2	2
	Total (Urban and Nonurban)		37	13	8	6

Notes: This table reports the number of middle and high charter schools in Massachusetts and their participation in the observational and lottery studies. The numbered notes below describe the schools included in each column. Columns (2)-(4) exclude middle schools that have their main admissions lottery in elementary school (e.g., K-8 schools) and high schools that hold their main admissions lotteries in elementary or middle school (e.g.,K-12 or 6-12 schools). MATCH Charter Public School, Boston Collegiate Charter School, and Four Rivers Charter Public School are counted twice: once as a middle school and once as a high school (lotteries from each level participate in the lottery study). Edward Brooke Charter School is counted as a middle school (it became K-8 in 2006, only lotteries from the middle grades participate in the lottery study). "Urban" towns are defined by the Massachusetts Department of Elementary and Secondary Education as the towns where the district superintendents participate in the Massachusetts Urban Superintendents Network. These towns include: Boston, Brockton, Cambridge, Chelsea, Chicopee, Everett, Fall River, Fitchburg, Framingham, Haverhill, Holyoke, Lawrence, Leominster, Lowell, Lynn, Malden, New Bedford, Pittsfield, Quincy, Revere, Somerville, Springfield, Taunton, and Worcester.

1. Middle and high charter schools in Massachusetts, including schools opened in 2010 (which is too recent to have MCAS outcomes), alternative charter schools,
2. Middle and high charter schools in Massachusetts with the designated entry grades (in 4-7 and 9)*, including schools opened in 2010, alternative schools, and
3. Middle and high charter schools in Massachusetts with the designated entry grades (in 4-7 and 9)*, excluding closed schools, alternative schools, and schools
4. Middle and high charter schools that are included in column (3), excluding schools that are undersubscribed or have insufficient lottery records.

* There is an exception to the 9th grade entry criteria for high school. Two schools with lotteries at the middle school entry point which also enroll students in the high school grades are included in the lottery analysis of 10th grade outcomes.

**Here we also exclude one school that opened in 2009, but has a 4th grade entry so did not reach our observational outcome grades (5-8) by Fall 2009.

Table 2: Characteristics of Charter Schools

Characteristic	Statewide (1)	Urban (2)	Non-urban (3)
Years open	9.43	8.18	11.36
Days per year	186.18	188.53	182.55
Average minutes per day	447.86	464.35	422.36
Have Saturday school	0.321	0.471	0.091
Avg. math instruction (min)	80.93	94.56	59.86
Avg. reading instruction (min)	84.00	99.62	59.86
CMO or Network Affiliation	0.357	0.294	0.455
Fully or somewhat "No excuses"	0.429	0.706	0.000
Parent contract	0.679	0.824	0.455
Student contract	0.643	0.706	0.545
Uniforms	0.821	0.882	0.727
Reward and punishment system	0.464	0.647	0.182
Avg. per-pupil expenditure	12824.19	14095.53	11090.55
Title I eligible	0.857	1.000	0.636
Number of teachers	25.736	22.735	30.373
Student/teacher ratio	11.614	11.565	11.691
Licensed teachers	51.146	51.853	50.055
Proportion 32 and younger	0.577	0.709	0.384
Proportion 49 and older	0.129	0.058	0.233
Left voluntarily	2.278	1.969	2.727
Left involuntarily	1.296	1.500	1.000
Require staff to take calls after hours	0.071	0.059	0.091
Unpaid tutors/volunteers	0.786	0.706	0.909
Paid tutors	0.143	0.235	0.000
N (schools)	28	17	11

Notes: This table reports results from a survey of Massachusetts charter schools with entry in middle (4th-7th) or high school (9th) grades. The survey sample excludes schools closed prior to 2010, schools that were not open before Fall 2010, and schools serving non-traditional student populations. Twenty-eight of 32 eligible schools responded to the survey.

Table 3: Descriptive Statistics

	Regular Public Schools		Charter schools (eligible)		Charter applicants (lottery)	
	Urban	Non-urban	Urban	Non-urban	Urban	Non-urban
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A. Middle Schools (5th-8th grade)					
Female	0.486	0.488	0.498	0.476	0.496	0.510
Black	0.185	0.027	0.407	0.036	0.479	0.022
Hispanic	0.314	0.036	0.237	0.048	0.234	0.025
Special education	0.190	0.163	0.166	0.160	0.176	0.184
Subsidized lunch	0.681	0.141	0.650	0.216	0.686	0.102
Limited English proficiency	0.150	0.016	0.077	0.025	0.086	0.008
Baseline Math score	-0.430	0.213	-0.339	0.239	-0.352	0.306
Baseline ELA score	-0.464	0.234	-0.330	0.261	-0.373	0.392
Years in charter	0.000	0.000	2.027	1.960	1.341	1.002
N (students)	153374	369866	6625	8316	4126	1693
N (schools)	262	390	16	11	9	7
	Panel B. High Schools (10th grade)					
Female	0.500	0.494	0.555	0.549	0.549	0.539
Black	0.190	0.028	0.535	0.020	0.615	0.029
Hispanic	0.272	0.032	0.176	0.010	0.256	0.017
Special education	0.169	0.155	0.160	0.105	0.174	0.115
Subsidized lunch	0.606	0.122	0.600	0.146	0.716	0.120
Limited English proficiency	0.093	0.009	0.022	0.005	0.035	0.003
Baseline Math score	-0.420	0.271	-0.413	0.322	-0.315	0.445
Baseline ELA score	-0.387	0.282	-0.325	0.413	-0.306	0.562
Years in charter	0.000	0.000	1.765	1.797	0.627	1.292
N (students)	116593	313366	2198	783	2973	349
N (schools)	101	304	8	2	4	2

Notes: This table reports descriptive statistics for the sample of public school students (columns 1 and 2), the sample of students in eligible charter schools (columns 3 and 4), and the sample of charter applicants (columns 5 and 6) from 2002-2010. The sample is restricted to students in Massachusetts public schools at baseline with at least one followup test score. The number of schools in columns (1) and (2) is counted in 6th grade for middle school and 10th grade for high school. Years in charter school is measured through 8th grade for middle school and 10th grade for high school.

Table 4: Lottery Results for Massachusetts Charter Schools

				2SLS	
		First Stage	Reduced Form	Just identified	Overidentified
School level	Subject	(1)	(2)	(3)	(4)
Middle	ELA	0.987***	0.065**	0.066**	0.062**
		(0.043)	(0.029)	(0.029)	(0.028)
	N			12126	
	Math	0.984***	0.211***	0.214***	0.175***
		(0.043)	(0.034)	(0.033)	(0.031)
	N			12346	
High	ELA	0.509***	0.113**	0.221***	0.190**
		(0.101)	(0.050)	(0.076)	(0.074)
	N			3303	
	Math	0.510***	0.164**	0.322***	0.269***
		(0.101)	(0.064)	(0.090)	(0.093)
	N			3255	
	Writing Topic	0.514***	0.156***	0.303***	0.290***
		(0.101)	(0.057)	(0.087)	(0.080)
	N			3268	
	Writing Composition	0.514***	0.140**	0.271***	0.227***
		(0.101)	(0.058)	(0.092)	(0.085)
	N			3268	

Notes: This table reports estimates of the effects of years in charter schools on test scores. The sample is restricted to students with baseline demographic characteristics who attended a Massachusetts public school when tested, and excludes students with sibling priority and late applicants. Columns (1)-(3) are produced by a 2SLS procedure using a lottery offer dummy as an instrument for years spent in charter schools. Column (4) uses risk set and offer interactions as instruments. All models control for race, sex, special education, limited English proficiency, subsidized lunch status, and a female by minority dummy. Year of birth, year of test, and risk set dummies are also included. Middle school regressions pool post-lottery outcomes from 4th through 8th grade and cluster by student identifier as well as school-grade-year. High school regressions include only scores for 10th grade and cluster by school-grade-year.

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

Table 5: Lottery Results for Urban and Non-urban Charter Schools

		Urban			Non-urban		
		First Stage	Reduced Form	2SLS	First Stage	Reduced Form	2SLS
School level	Subject	(1)	(2)	(3)	(4)	(5)	(6)
Middle	ELA	1.001***	0.141***	0.140***	0.978***	-0.155***	-0.156***
		(0.055)	(0.035)	(0.033)	(0.081)	(0.045)	(0.045)
	N		8762			3364	
	Math	0.990***	0.333***	0.336***	0.996***	-0.159***	-0.155***
		(0.054)	(0.038)	(0.036)	(0.081)	(0.050)	(0.051)
	N		9015			3331	
High	ELA	0.494***	0.117**	0.236***	1.082***	-0.014	-0.009
		(0.105)	(0.051)	(0.079)	(0.153)	(0.116)	(0.105)
	N		2954			349	
	Math	0.495***	0.178***	0.359***	1.088***	-0.274*	-0.246*
		(0.105)	(0.066)	(0.092)	(0.158)	(0.162)	(0.148)
	N		2910			345	
	Writing Topic	0.500***	0.166***	0.332***	1.082***	-0.157	-0.139
		(0.105)	(0.058)	(0.090)	(0.153)	(0.222)	(0.204)
	N		2920			348	
	Writing Composition	0.500***	0.149**	0.298***	1.082***	-0.155	-0.137
		(0.105)	(0.060)	(0.096)	(0.153)	(0.213)	(0.196)
	N		2920			348	

Notes: This table reports estimates of the effects of years in urban and non-urban charter schools on test scores. The sample is restricted to students with baseline demographic characteristics who attended a Massachusetts public school when tested, and excludes students with sibling priority and late applicants. Estimates are produced by a 2SLS procedure using urban and non-urban lottery offers as instruments for attendance at urban and non-urban charter schools. All models control for race, sex, special education, limited English proficiency, subsidized lunch status, and a female by minority dummy. Year of birth, year of test, and risk set dummies are also included. Middle school regressions pool post-lottery outcomes from 4th through 8th grade and cluster by student identifier as well as school-grade-year. High school regressions include only scores for 10th grade, and cluster by school-grade-year.

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

Table 6: 2SLS Estimates for Subgroups at Urban Charter Schools

		Sex		Race			Subsidized lunch	Lowest baseline quartile
		Female	Male	Black	Hispanic	White		
School level	Subject	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Middle	ELA	0.110**	0.171***	0.222***	0.218***	0.023	0.189***	0.307***
		(0.044)	(0.047)	(0.056)	(0.058)	(0.057)	(0.039)	(0.074)
	N	4405	4357	4152	1960	1982	5945	2082
	Math	0.394***	0.287***	0.502***	0.378***	0.109*	0.365***	0.420***
		(0.050)	(0.048)	(0.059)	(0.060)	(0.064)	(0.041)	(0.064)
	N	4535	4480	4312	2015	2014	6112	2148
High	ELA	0.172*	0.272**	0.222**	0.302*	0.047	0.191**	0.251
		(0.101)	(0.115)	(0.087)	(0.174)	(0.629)	(0.088)	(0.165)
	N	1625	1329	1817	756	227	2118	621
	Math	0.400***	0.306**	0.384***	0.189	0.641	0.298***	0.450***
		(0.122)	(0.132)	(0.101)	(0.210)	(0.495)	(0.104)	(0.132)
	N	1600	1310	1791	743	226	2086	679
	Writing Topic	0.320***	0.317***	0.452***	-0.188	0.116	0.271**	0.358**
		(0.119)	(0.110)	(0.097)	(0.239)	(0.665)	(0.106)	(0.150)
	N	1611	1309	1794	747	225	2093	679
	Writing Composition	0.231*	0.348***	0.369***	0.128	0.460	0.227**	0.347*
		(0.124)	(0.131)	(0.106)	(0.249)	(0.582)	(0.114)	(0.181)
	N	1611	1309	1794	747	225	2093	679

Notes: This table reports 2SLS estimates of the effects of time spent in charter schools for various subgroups of students. All regressions include year dummies, grade dummies, risk set dummies, and demographic controls. Middle school standard errors are clustered on student identifier as well as school-grade-year. High school standard errors are clustered by school-grade-year.

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

Table 7: 2SLS Estimates for Subgroups at Non-urban Charter Schools

			Sex		Race			Subsidized	Lowest baseline
			Female	Male	Black	Hispanic	White	lunch	quartile
School level	Subject		(1)	(2)	(3)	(4)	(5)	(6)	(7)
Middle	ELA		-0.202***	-0.117*	0.148	-0.151	-0.156***	-0.196*	-0.215***
			(0.058)	(0.068)	(0.587)	(0.273)	(0.046)	(0.118)	(0.077)
		N	1702	1662	67	94	3043	308	795
	Math		-0.220***	-0.105	0.153	-0.577	-0.138***	-0.168	-0.251***
			(0.067)	(0.073)	(0.626)	(0.381)	(0.049)	(0.157)	(0.075)
		N	1687	1644	65	96	3009	298	716
High	ELA		0.166	-0.267	-	-	-0.022	-0.579***	-0.031
			(0.122)	(0.183)			(0.109)	(0.208)	(0.135)
		N	188	161			325	42	76
	Math		-0.193	-0.347	-	-	-0.247	-0.052	-0.081
			(0.165)	(0.239)			(0.166)	(0.049)	(0.059)
		N	187	158			322	40	74
	Writing Topic		0.073	-0.480	-	-	-0.180	-2.713***	-0.396**
			(0.178)	(0.392)			(0.200)	(0.102)	(0.167)
		N	187	161			325	41	74
	Writing		0.022	-0.379	-	-	-0.161	-1.320***	-0.293
	Composition		(0.262)	(0.312)			(0.194)	(0.191)	(0.386)
		N	187	161			325	41	74

Notes: This table reports 2SLS estimates of the effects of time spent in non-urban charter schools for various subgroups of students. All regressions include year dummies, grade dummies, risk set dummies, and demographic controls. Middle school standard errors are clustered on student identifier as well as school-grade-year. High school standard errors are clustered by school-grade-year.

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

Table 8: Urban Gaps in Treatment and No-treatment Counterfactuals

					Differences in potential outcomes	
		Urban effect	Non-urban effect	Effect difference	Υ_0	Υ_1
School level	Subject	(1)	(2)	(3)	(4)	(5)
Middle	ELA	0.154**	-0.218***	0.371***	-0.705***	-0.333***
		(0.074)	(0.054)	(0.092)	(0.081)	(0.065)
	N	3817	1851	5668	5668	5668
	Math	0.468***	-0.252***	0.720***	-0.628***	0.092
		(0.084)	(0.072)	(0.111)	(0.085)	(0.071)
	N	4127	1768	5895	5895	5895

Notes: This table estimates the components of the difference in charter treatment effects by urban status due to differences in non-charter "fallback" and differences in treated outcomes. Outcomes are test scores the year after the lottery. Columns (1) and (2) display urban and non-urban charter treatment effects, respectively, and column (3) gives the difference. Column (4) shows an estimate of the difference in average non-treated outcomes between urban and non-urban compliers, computed as described in the text. Column (5) shows an estimate of the difference in treated outcomes between these two groups.

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

Table 9: Non-treated Gaps in Urban Areas

School level	Subject		Treatment Effect (1)	Non-applicants included: $E_u[Y_0\|D=0]$ (2)	λ_0^u (3)	λ_1^u (4)	Non-applicants excluded: $E_u[Y_0\|D=0]$ (5)	λ_0^u (6)	λ_1^u (7)
Middle	ELA		0.152**	-0.417***	0.116*	0.268***	-0.273***	-0.028	0.124**
			(0.074)	(0.013)	(0.062)	(0.058)	(0.037)	(0.047)	(0.061)
		N			102238			3752	
	Math		0.470***	-0.402***	0.071	0.540***	-0.318***	-0.014	0.456***
			(0.085)	(0.012)	(0.054)	(0.062)	(0.034)	(0.041)	(0.071)
		N			145925			4062	
High	ELA		0.346**	-0.368***	0.026	0.372***	-0.300***	-0.073	0.285**
			(0.166)	(0.020)	(0.104)	(0.139)	(0.034)	(0.100)	(0.142)
		N			129062			3256	
	Math		0.647***	-0.373***	0.002	0.639***	-0.277***	-0.117	0.533***
			(0.175)	(0.023)	(0.116)	(0.175)	(0.046)	(0.115)	(0.174)
		N			127196			3210	

Notes: This table compares non-treated potential outcomes for compliers vs. non-attenders in urban areas. For lottery applicants, outcomes are test scores in the year after the lottery for middle school and in 10th grade for high school. For non-applicants, outcomes are 6th grade scores in middle school, and 10th grade scores in high school. The treatment is a dummy for charter attendance. Column (1) presents 2SLS estimates of the effect of charter attendance on test scores, with the lottery offer dummy interacted with risk sets as instruments and risk sets as maintained controls. Column (2) shows average test scores for non-charter students, including non-applicants. Column (3) shows the difference between the average non-charter scores of compliers and non-charter students. Column (4) shows the difference between the treated outcomes of compliers and the scores of non-charter students. Columns (5)-(7) exclude non-applicants.

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

Table 10: Non-treated Gaps in Non-urban Areas

		Treatment Effect	Non-applicants included			Non-applicants excluded		
			$E_n[Y_0\|D=0]$	λ_0^n	λ_1^n	$E_n[Y_0\|D=0]$	λ_0^n	λ_1^n
School level	Subject	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Middle	ELA	-0.198***	0.265***	0.124**	-0.078**	0.392***	0.010	-0.207***
		(0.050)	(0.007)	(0.050)	(0.033)	(0.032)	(0.036)	(0.043)
	N			273443			1851	
	Math	-0.234***	0.238***	0.060	-0.192***	0.304***	-0.006	-0.258***
		(0.073)	(0.007)	(0.063)	(0.039)	(0.041)	(0.047)	(0.055)
	N			326982			1768	

Notes: This table compares non-treated potential outcomes for compliers vs. non-attenders in non-urban areas. For lottery applicants, outcomes are test scores in the year after the lottery for middle school and in 10th grade for high school. For non-applicants, outcomes are 6th grade math scores in middle school, and 10th grade scores in high school. The treatment is a dummy for charter attendance. Column (1) presents 2SLS estimates of the effect of charter attendance on test scores, with the lottery offer dummy interacted with risk sets as instruments and risk sets as maintained controls. Column (2) shows average test scores for non-charter students, including non-applicants. Column (3) shows the difference between the average non-charter scores of compliers and non-charter students. Column (4) shows the difference between the treated outcomes of compliers and the scores of non-charter students. Columns (5)-(7) exclude non-applicants.

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

Table 11: Decomposition of Urban Differences in Impact

				Decomposition 1 (urban loading)		Decomposition 2 (non-urban loading)	
			Urban vs. non-urban difference in TE	Due to diffs in cov. levels	Due to diffs in cov-specific TE	Due to diffs in cov. levels	Due to diffs in cov-specific TE
School level	Subject		(1)	(2)	(3)	(4)	(5)
Middle	ELA		0.403***	0.252***	0.151	0.057	0.345
			(0.079)	(0.086)	(0.104)	(0.399)	(0.436)
		N	4523				
	Math		0.675***	0.329***	0.346***	0.248	0.428
			(0.074)	(0.081)	(0.093)	(0.333)	(0.353)
		N	4521				

Notes: This table decomposes the difference between urban and non-urban charter treatment effects. Outcomes are test scores the year after the lottery. The treatment is a dummy for charter attendance. Column (1) shows the difference in urban vs. non-urban treatment effects, computed as described in the text. Columns (2) and (3) report the components of the urban/non-urban difference due to differences in covariate levels and differences in covariate-specific effects, respectively, weighting the difference in covariate levels by the urban treatment effects. Columns (4) and (5) report a decomposition that weights the difference in covariate levels by the non-urban treatment effects. The covariates used in the decompositions are race, sex, special education, limited English proficiency, free/reduced price lunch, and baseline score categories (advanced, proficient, needs improvement, warning) in math and ELA.
*significant at 10%; **significant at 5%; ***significant at 1%

Table 12: Comparison of Lottery and Observational Estimates for Eligible Charters

			Urban			Non-urban		
				Observational estimates			Observational estimates	
School level	Subject		Lottery estimate (1)	Lottery sample (2)	Non-lottery sample (3)	Lottery estimate (4)	Lottery sample (5)	Non-lottery sample (6)
Middle	ELA		0.140***	0.174***	-0.035***	-0.156***	-0.015**	-0.019
			(0.033)	(0.011)	(0.012)	(0.045)	(0.007)	(0.012)
		N	8762	64792	64792	3364	139101	139101
	Math		0.336***	0.277***	-0.035**	-0.155***	-0.032***	-0.013
			(0.036)	(0.013)	(0.015)	(0.051)	(0.007)	(0.010)
		N	9015	67926	67926	3331	145902	145902
High	ELA		0.236***	0.247***	0.082***	-0.009	0.050***	-
			(0.079)	(0.019)	(0.018)	(0.105)	(0.014)	
		N	2954	5011	5011	349	9441	
	Math		0.359***	0.305***	-0.019	-0.246*	0.039**	-
			(0.092)	(0.036)	(0.017)	(0.148)	(0.019)	
		N	2910	4916	4916	345	9401	
	Writing Topic		0.332***	0.272***	0.083***	-0.139	0.051***	-
			(0.090)	(0.029)	(0.028)	(0.204)	(0.020)	
		N	2920	4932	4932	348	9404	
	Writing Composition		0.298***	0.256***	0.072**	-0.137	0.038*	-
			(0.096)	(0.025)	(0.032)	(0.196)	(0.022)	
		N	2920	4932	4932	348	9404	

Notes: This table reports estimates of the effects of years in charter schools on test scores. Eligible charters are schools with entry grades 4-7 (middle) or 9 (high), and that meet the other restrictions from Table 1. The sample is produced by matching charter students to students in traditional public schools on cells defined by sending school, baseline year, and baseline demographics (race, sex, limited English proficiency, special education status, and free lunch status). All models control for cell fixed effects, year effects, grade effects, and baseline test scores. Middle school regressions pool outcomes from 5th through 8th grade and cluster by student identifier as well

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

Table 13: Effects of School Characteristics

Variable	Math (1)	Math (2)	Math (3)	Math (4)	ELA (5)	ELA (6)	ELA (7)	ELA (8)
Urban	0.198***	0.072	0.008	-0.041	0.120***	0.062*	0.011	0.014
	(0.057)	(0.082)	(0.062)	(0.053)	(0.036)	(0.037)	(0.033)	(0.042)
Total minutes per day/100	-	0.154*		0.095	-	0.080*	-	0.055
		(0.090)		(0.078)		(0.042)		(0.038)
Minutes in relevant subject/100	-	0.203	-	0.207	-	0.023	-	0.007
		(0.211)		(0.168)		(0.075)		(0.068)
Per-pupil expenditure/1000	-	-0.002	-	-0.009	-	0.004	-	-0.001
		(0.014)		(0.010)		(0.008)		(0.009)
School is No Excuses	-	-	0.306***	0.231***	-	-	0.169***	0.117**
			(0.082)	(0.060)			(0.045)	(0.048)
Lottery	0.154**	0.086*	0.051	0.038	0.101**	0.055	0.047	0.033
	(0.069)	(0.050)	(0.052)	(0.041)	(0.043)	(0.035)	(0.036)	(0.033)
High School	0.039	0.078	0.035	0.087	0.069*	0.076*	0.062*	0.078*
	(0.071)	(0.065)	(0.052)	(0.057)	(0.036)	(0.040)	(0.032)	(0.040)
Constant	-0.131*	-0.835**	-0.064	-0.490	-0.085*	-0.445**	-0.047	-0.267
	(0.067)	(0.375)	(0.043)	(0.299)	(0.045)	(0.176)	(0.033)	(0.183)
N	30	28	30	28	30	28	30	28

Notes: This table reports regressions of school-specific treatment effects on school characteristics. The sample includes only schools that completed the charter survey. Regressions weight by the inverse of the standard error of the coefficient estimates and cluster at the school level.

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

Table 14: Effects on Discipline

		Urban			Non-urban		
		Mean	2SLS	2SLS with baseline	Mean	2SLS	2SLS with baseline
School level	Outcome	(1)	(2)	(3)	(4)	(5)	(6)
Middle	Total days suspended	0.621	0.881***	0.861***	0.074	-0.015	-0.013
			(0.102)	(0.100)		(0.041)	(0.042)
	N		3919	3812		1701	1571
	Days of in-school suspension	0.085	0.194***	0.192***	0.010	-0.022	-0.026
			(0.044)	(0.045)		(0.016)	(0.016)
	N		3919	3812		1701	1571
	Days of out-of-school suspension	0.536	0.688***	0.669***	0.064	0.008	0.013
			(0.085)	(0.083)		(0.033)	(0.033)
	N		3919	3812		1701	1571
	Days truant	0.520	0.143	0.133	0.068	-0.039	-0.035
			(0.216)	(0.219)		(0.053)	(0.056)
	N		3919	3812		1701	1571
High	Total days suspended	0.436	1.215***	1.251***	0.133	-0.251	-0.250
			(0.212)	(0.215)		(0.269)	(0.309)
	N		3006	2868		377	327
	Days of in-school suspension	0.070	0.100*	0.104*	0.045	0.054	0.054
			(0.057)	(0.058)		(0.037)	(0.044)
	N		3006	2868		377	327
	Days of out-of-school suspension	0.366	1.115***	1.147***	0.088	-0.305	-0.304
			(0.196)	(0.198)		(0.262)	(0.322)
	N		3006	2868		377	327
	Days truant	0.621	-0.458	-0.808**	0.231	0.087	0.462
			(0.436)	(0.354)		(0.175)	(0.347)
	N		3006	2868		377	327

Notes: This table reports 2SLS regressions of disciplinary outcomes the year after the lottery on dummies for urban and non-urban charter attendance instrumented by the urban and non-urban offer dummies. All regressions include year dummies, year of birth dummies, grade dummies, and demographics; columns (3) and (6) add baseline score controls. Standard errors are clustered by school-grade-year.

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

Data Appendix

The data used for this study come from charter school lottery records, student demographic and school attendance information in the Massachusetts Student Information Management System (SIMS), and test scores from the Massachusetts Comprehensive Assessment System (MCAS) database. This appendix describes each data source and details the procedures used to clean and match them. The steps used here are an updated version of the methods described in the data appendix to Angrist et al. (2010).

A Data Sets

A.1 Charter School Entrance Lotteries

Data description and sample restrictions

Our sample of applicants is obtained from records of lotteries held at 19 Massachusetts charter schools between 2002 and 2008. The participating schools and lottery years are listed in Table A1, along with schools eligible for the lottery study that did not contribute records. A total of 84 school-specific entry cohorts are included in the analysis. Lotteries at three schools contribute observations to both the middle and high school samples.

The raw lottery records typically include applicants' names, dates of birth, contact information, and other information used to define lottery groups, such as sibling and out-of-area status. The first five rows in each panel of Table A4 show the sample restrictions we impose on the raw lottery records, separately by lottery cohort and school level. We exclude duplicate applicants and applicants listed as applying to the wrong entry grade. We also drop late applicants, out-of-area applicants, and sibling applicants, as these groups are typically not included in the standard lottery process. Imposing these restrictions reduces the number of middle school lottery records from 10,799 to 9,247 and reduces the number of high school records from 7,326 to 7,044.

Lottery offers

In addition to the data described above, the lottery records also include information regarding offered seats. We used this information to reconstruct indicator variables for whether lottery participants received randomized offers. At each school, records were sufficient to determine the students who were offered seats before the start of the school year following the lottery, including initially waitlisted students who received offers after others declined. Some of the records also indicate students who were initially offered seats on the day of the lottery; since this "initial offer" information is not available for all schools, we code our offer variable to include offers received by waitlisted students. The instrument Z_i used in our analyses is one for any student who received an offer from a school included in our lottery sample at any time during the year she applied. Offer rates were 66 percent and 64 percent in our middle and high school samples, respectively.

A.2 Student Information Management System Data

Data description

Our study uses SIMS data from the 2001-2002 school year through the 2009-2010 school year. Each year of data includes an October file and an end-of-year file. The SIMS records information on demographics and schools attended for all students in Massachusetts' public schools. An observation in the SIMS refers to a student in a school in a year, though there are some student-school-year duplicates for students that switch grades or programs within a school and year.

Coding of demographics and attendance

The SIMS variables used in our analysis include grade, year, name, town of residence, date of birth, sex, race, special education and limited English proficiency status, free or reduced price lunch, and school attended. We constructed a wide-format data set that captures demographic and attendance information for every student in each year in which he or she is present in Massachusetts' public schools. This file uses information from the longest-attended school in the first calendar year spent in each grade. Attendance ties were broken at random; this affects only 0.007 percent of records. Students classified as SPED, LEP, or free/reduced price lunch in any record within a school-year-grade retain that designation for the entire school-year-grade.

We measure charter school attendance in calendar years. A student is coded as attending a charter school in a particular year when there is any SIMS record reporting charter attendance in that year. Students who attend more than one charter school within a year are assigned to the charter they attended longest.

A.3 Massachusetts Comprehensive Assessment System Data

Data description and sample restrictions

We use MCAS data from the 2001-2002 school year through the 2009-2010 school year. Each observation in the MCAS database corresponds to a student's test results in a particular grade and year. We use math and English Language Arts (ELA) tests in grades 3 through 8 and 10, as well as Writing Topic and Writing Composition scores in grades 4, 7, and 10. The test score variables are standardized to have mean zero and standard deviation one within a subject-grade-year in Massachusetts. Repetitions of the same test subject and grade are dropped. In cases with multiple records within a year and grade, ties are broken at random; this affected 0.09 percent of MCAS records.

In the lottery-based middle school analysis, all post-lottery test scores through 8th grade are used as outcomes. High school outcomes are from 10th grade. The most recent pre-lottery score in a subject defines a student's baseline score. For the observational analysis, outcome grades are 5th through 8th for middle school 10th for high school; baseline scores are from 4th grade for middle school and 7th or 8th grade for high school.

B Matching Data Sets

B.1 Match from the MCAS to the SIMS

The processed SIMS and MCAS files were merged by grade, year, and a state student identifier known as the SASID. Scores that could not be matched to the SIMS were dropped. This restricted eliminated 0.7 percent of MCAS scores statewide.

B.2 Match from the Lottery Records to the State Database

Match procedure

Lottery records were matched to the state SIMS/MCAS database by name, application year, and application grade. In some cases, this procedure did not produce a unique match. We accepted some matches based on fewer criteria where the information on grade, year, and town of residence seemed to make sense.

Match success rate

Our matching procedure successfully located most applicants in the SIMS database. Table A5 reports cohort-specific match rates from the lottery records to the combined SIMS/MCAS file, separately for middle and high school. The overall match rates for middle and high school were 92.0 percent and 94.3 percent, respectively. Table A5 also reports separate match rates for offered and non-offered students. In middle school, offered students were slightly more likely to be matched (93.8 percent compared to 89.3 percent). Offered and non-offered applicants to charter high schools were matched to the SIMS at almost identical rates (94.1 percent compared to 94.8 percent).

C Construction of the Outcome Data Sets

C.1 Lottery Sample

Further sample restrictions

Once matched to the SIMS, each student is associated with a unique SASID; at this point, we can therefore determine which students applied to multiple schools in our lottery sample.

Following the match, we reshape the lottery data set to contain a single record for each student. If students applied in more than one year to lotteries at a particular school level (middle or high), we keep only the records associated with their first year of application. In our basic lottery analyses, we also exclude students without baseline demographics in the SIMS; in effect, this rule limits the sample to students in Massachusetts' public schools at baseline. Rows 6-9 in each panel of Table A4 report the impact of these restrictions on sample sizes for middle and high school. The set of matched first-time applicants with baseline demographics includes 6,214 middle school students and 4,207 high school students.

Final set of outcomes and students

To generate the middle school analysis file, the matched lottery/SIMS/MCAS file is reshaped to long format, with each observation referring to a test score outcome for a student in a particular year. The high school analysis file uses only 10th grade outcomes, so it includes a single observation for each student. Table A6 summarizes the analysis files for middle and high school. Columns (1) and (2) list the application and outcome grades for each cohort, and column (3) lists the number of applicants satisfying the sample restrictions from Table A4. In middle school, 5,773 of 6,214 students contribute at least one test score to the analysis. In high school, 3,322 of 4,207 students have at least one score. Middle school applicants contribute different numbers of scores to the analysis depending on their years and grades of application; math and ELA tests were not given in every middle school grade until 2006, and some cohorts are not observed through 8th grade. Table A7 lists the grades and years in which math and ELA subjects were administered. As shown in columns (5)-(8) of Table A6, we find 12,346 out of 14,180 expected scores for middle school math, 12,126 of 13,904 for middle school ELA, 3,255 of 4,206 for high school math, and 3,303 of 4,206 for high school ELA. These outcomes are used to produce the 2SLS estimates reported in Tables 4-7.

C.2 Observational Sample

To produce the analysis file used for the observational analysis, we begin with the matched SIMS/MCAS state database. As described in Section V, we define cells based on baseline school, baseline year, race, and sex, separately for middle school and high school. We then count the number of students in each cell who go on to spend time in eligible charter schools and regular public schools in the relevant range of grades (5th through 8th for middle school and 10th for high school). Observations in cells that do not include at least one student who attends eligible charter schools and one student who attends regular public schools are dropped. We then produce a long format data file containing the full set of test score outcomes for the remaining sample of matched students at the relevant school level, as well as variables counting years of attendance at each eligible charter school. This file is used to produce the observational estimates. Our matching procedure excludes 23 percent of students who attend eligible charter schools in middle or high school.

Table A1: Massachusetts Charter Schools Eligible for the Lottery Study

School (1)	Town (2)	Urban (3)	Grades (4)	Eligible middle (5)	Eligible high (6)	Years in lottery study (7)
Academy of the Pacific Rim Charter School	Boston	Yes	5-12	Yes		2005-2009
Advanced Math and Science Academy Charter School	Marlborough		6-12	Yes		
Barnstable Horace Mann Charter School	Marstons Mills		4-5	Yes		
Berkshire Arts and Technology Charter Public School	Adams		6-12	Yes		
Boston Collegiate Charter School	Boston	Yes	5-12	Yes	Yes	2002-2009
Boston Preparatory Charter Public School	Boston	Yes	6-11	Yes		2005-2009
Cape Cod Lighthouse Charter School	Orleans		6-8	Yes		2007-2009
Christa McAuliffe Regional Charter Public School	Framingham	Yes	6-8	Yes		
City on a Hill Charter Public School	Boston	Yes	9-12		Yes	2002, 2004-2008
Codman Academy Charter Public School	Boston	Yes	9-12		Yes	2004, 2008
Community Charter School of Cambridge	Cambridge	Yes	7-12	Yes		
Edward Brooke Charter School	Boston	Yes	K-8	Yes		2006-2009
Excel Academy Charter School	Boston	Yes	5-8	Yes		2008-2009
Four Rivers Charter Public School	Greenfield		7-12	Yes	Yes	2003-2009
Francis W Parker Charter Essential School	Devins		7-12	Yes		2006-2009
Global Learning Charter Public School	New Bedford	Yes	5-12	Yes		2006-2007, 2009
Hampden Charter School of Science	Chicopee	Yes	6-10	Yes		
Health Careers Academy Charter School	Boston	Yes	9-12		Yes	
Innovation Academy Charter School	Tyngsboro		5-11	Yes		2007-2009
KIPP Academy Lynn	Lynn	Yes	5-8	Yes		2005-2009
Marblehead Community Charter Public School	Marblehead		4-8	Yes		2005-2007
MATCH Charter Public School	Boston	Yes	6-12	Yes	Yes	2002-2009
New Leadership Charter School	Springfield	Yes	6-12	Yes		
North Central Charter Essential School	Fitchburg	Yes	7-12	Yes		
Phoenix Charter Academy	Chelsea	Yes	9-12		Yes	
Pioneer Charter School of Science	Everett	Yes	7-11	Yes		
Pioneer Valley Performing Arts Charter Public School	South Hadley		7-12	Yes		2006-2009
Rising Tide Charter Public School	Plymouth		5-8	Yes		2009
Roxbury Preparatory Charter School	Boston	Yes	6-8	Yes		2002-2009
Salem Academy Charter School	Salem		6-12	Yes		
Smith Leadership Academy	Boston	Yes	6-8	Yes		
Sturgis Charter Public School	Hyannis		9-12		Yes	2004, 2006, 2008

Notes: This table lists all charter schools in Massachusetts eligible for the lottery study. To be counted as eligible, a school must be open in the relevant years and meet the entry grade and student population restrictions required for inclusion in column (3) of Table 1.

Table A2: Covariate Balance

	Middle school		High school	
	All lotteries	Lotteries with baseline scores	All lotteries	Lotteries with baseline scores
	(1)	(2)	(3)	(4)
Hispanic	0.007	0.010	0.014	0.000
	(0.011)	(0.011)	(0.018)	(0.019)
Black	0.012	0.006	-0.002	0.011
	(0.012)	(0.013)	(0.020)	(0.022)
White	-0.010	-0.009	-0.008	-0.011
	(0.010)	(0.011)	(0.011)	(0.012)
Asian	0.002	0.003	0.001	-0.001
	(0.004)	(0.005)	(0.007)	(0.008)
Female	0.016	0.018	0.008	0.014
	(0.015)	(0.016)	(0.021)	(0.022)
Subsidized Lunch	0.007	0.008	0.020	0.001
	(0.013)	(0.013)	(0.018)	(0.020)
Special Education	-0.010	-0.011	-0.002	-0.003
	(0.012)	(0.012)	(0.016)	(0.017)
Limited English Proficiency	-0.008	-0.006	0.014*	0.011
	(0.008)	(0.008)	(0.007)	(0.007)
Baseline ELA Score	-	0.017	-	-0.072*
		(0.030)		(0.037)
Baseline Math Score	-	0.008	-	-0.057
		(0.030)		(0.040)
Baseline Writing Composition Score	-	-	-	0.023
				(0.039)
Baseline Writing Topic Score	-	-	-	-0.063
				(0.039)
p-value, from F-test	0.423	0.753	0.695	0.141
N	6214	5784	4207	3549

Notes: This table reports coefficients on regressions of the variable indicated in each row on an indicator variable equal to one if the student won the lottery. Regressions include risk set dummies and baseline grade dummies and exclude students with sibling priority and late applicants. Samples in columns (1) and (3) are restricted to students from cohorts where we should observe at least one test score. Samples in columns (2) and (4) are restricted to students who also have baseline test scores. F tests are for the null hypothesis that the coefficients on winning the lottery in all regressions are all equal to zero. These test statistics are calculated for the subsample that has non-missing values for all variables tested.
* significant at 10%; ** significant at 5%; *** significant at 1%

Table A3: Attrition

School level	Subject		Proportion of non-offered with MCAS (1)	Differential: Demographic controls (2)	Differential: Demographics and baseline scores (3)
Middle	ELA		0.911	0.016**	0.010
				(0.008)	(0.008)
		N	2348	6214	5873
	Math		0.916	0.011	0.007
				(0.008)	(0.008)
		N	2348	6214	5805
High	ELA		0.787	0.008	0.007
				(0.017)	(0.018)
		N	1332	4207	3687
	Math		0.773	0.010	0.004
				(0.018)	(0.018)
		N	1332	4207	4060
	Writing Topic and Writing Composition		0.773	0.010	0.003
				(0.018)	(0.019)
		N	1332	4207	3620

Notes: This table reports coefficients on regressions of an indicator variable equal to one if a student has a followup test score on an indicator variable equal to one if the student won the lottery. Regressions in columns (2) and (3) include risk set dummies as well as demographic variables, year of birth dummies, year of baseline dummies, and baseline grade dummies. Regressions in column (3) add baseline test scores. The sample is restricted to students who participated in an effective lottery from cohorts where we should observe follow-up scores. High school students who take Writing Topic exam must also take Writing Composition.

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

Table A4: Sample Restrictions for the Lottery Analysis

	Lottery cohort								
	2002-2003	2003-2004	2004-2005	2005-2006	2006-2007	2007-2008	2008-2009	2009-2010	All lotteries
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A. Middle School									
Total number of entry grade records	313	394	391	990	1578	2124	2132	2877	10799
Excluding disqualified applicants	313	394	391	990	1577	2106	2115	2873	10759
Excluding late applicants	313	391	390	972	1551	2046	2054	2829	10546
Excluding applicants from outside of area	313	387	388	963	1540	2028	2041	2741	10401
Excluding siblings	295	358	343	890	1378	1787	1801	2395	9247
Excluding records not matched to the SIMS	267	311	305	838	1311	1710	1669	2095	8506
Reshaping to one record per student	267	311	304	741	1115	1505	1424	1757	7424
Excluding repeat applications	267	308	302	728	1093	1470	1360	1705	7233
In Massachusetts public schools at baseline	201	228	222	600	919	1289	1187	1568	6214
Excluding students without a test score	187	208	208	569	875	1195	1104	1427	5773
Panel B. High School									
Total number of entry grade records	775	717	1094	955	1063	1330	1392	-	7326
Excluding disqualified applicants	775	717	1090	954	1061	1327	1391	-	7315
Excluding late applicants	765	710	1062	951	1053	1327	1372	-	7240
Excluding applicants from outside of area	765	706	1060	947	1050	1327	1372	-	7227
Excluding siblings	732	677	1029	925	1049	1298	1334	-	7044
Excluding students not matched to the SIMS	645	614	966	872	1029	1251	1255	-	6632
Reshaping to one record per student	573	614	740	650	772	881	863	-	5093
Excluding repeat applications	573	612	736	645	750	865	851	-	5032
In Massachusetts public schools at baseline	406	462	631	553	642	782	731	-	4207
Excluding students without a test score	328	357	513	437	528	630	529	-	3322

Notes: This table summarizes the sample restrictions imposed for the lottery analysis. Disqualified applications are defined as duplicate records and applications to the wrong grade.

Table A5: Match from Lottery Records to SIMS

Lottery cohort	Number of records (1)	Fraction with SIMS match: Total (2)	Offered (3)	Not offered (4)
		Panel A. Middle School		
2002-2003	295	0.908	0.934	0.859
2003-2004	358	0.869	0.882	0.820
2004-2005	343	0.889	0.924	0.849
2005-2006	890	0.942	0.967	0.886
2006-2007	1378	0.951	0.962	0.933
2007-2008	1787	0.957	0.978	0.917
2008-2009	1801	0.927	0.958	0.881
2009-2010	2395	0.875	0.865	0.884
All	9247	0.920	0.938	0.893
		Panel B. High School		
2002-2003	732	0.898	0.911	0.831
2003-2004	677	0.907	0.879	0.932
2004-2005	1029	0.941	0.939	0.945
2005-2006	925	0.943	0.943	0.942
2006-2007	1049	0.981	0.986	0.975
2007-2008	1298	0.964	0.974	0.959
2008-2009	1334	0.941	0.939	0.951
All	7044	0.943	0.941	0.948

Notes: This table summarizes the match from the lottery records to the SIMS data. The sample excludes disqualified applicants, late applicants, out-of-area applicants, and siblings.

Table A6: Outcome Data for the Lottery Analysis

Lottery cohort	Application grades (1)	Outcome grades (2)	Number of applicants (3)	Number with a test score (4)	Number of math scores expected (5)	Number of ELA scores expected (6)	Number of math scores observed (7)	Number of ELA scores observed (8)
Panel A. Middle School								
2002-2003	5-6	6-8	201	187	402	290	351	253
2003-2004	5-7	6-8	228	208	510	418	429	355
2004-2005	5-7	6-8	222	208	616	544	534	469
2005-2006	4-7	4-8	600	569	2104	2104	1874	1888
2006-2007	4-7	4-8	919	875	2944	2944	2547	2559
2007-2008	4-7	4-8	1289	1195	3662	3662	3101	3087
2008-2009	5-7	5-8	1187	1104	2374	2374	2093	2102
2009-2010	5-7	5-8	1568	1427	1568	1568	1417	1413
All	4-7	4-8	6214	5773	14180	13904	12346	12126
Panel B. High School								
2002-2003	5,9	10	406	328	406	406	327	328
2003-2004	5,7,9	10	462	357	462	462	351	355
2004-2005	7,9	10	631	513	631	631	500	510
2005-2006	7,9	10	553	437	553	553	426	433
2006-2007	9	10	642	528	642	642	522	523
2007-2008	9	10	782	630	782	782	609	628
2008-2009	9	10	730	529	730	730	520	526
All	5,7,9	10	4206	3322	4206	4206	3255	3303

Notes: This table summarizes observed test score outcomes for charter school lottery applicants. The sample is restricted to randomized applicants matched to baseline SIMS demographics. Expected test scores are post-lottery scores in grades 4-8 for middle school and grade 10 for high school that would be taken in Spring 2010 or earlier given normal academic progress after the lottery. Table A1 lists the schools participating in each cohort and their entry grades. Table A7 lists the availability of math and ELA tests by year.

Table A7: Availability of MCAS ELA and Math Tests by Year

Subject	School year	4th grade (1)	5th grade (2)	6th grade (3)	7th grade (4)	8th grade (5)	10th grade (6)
ELA	2001-2002	Yes			Yes		Yes
	2002-2003	Yes			Yes		Yes
	2003-2004	Yes			Yes		Yes
	2004-2005	Yes			Yes		Yes
	2005-2006	Yes	Yes	Yes	Yes	Yes	Yes
	2006-2007	Yes	Yes	Yes	Yes	Yes	Yes
	2007-2008	Yes	Yes	Yes	Yes	Yes	Yes
	2008-2009	Yes	Yes	Yes	Yes	Yes	Yes
	2009-2010	Yes	Yes	Yes	Yes	Yes	Yes
Math	2001-2002	Yes		Yes		Yes	Yes
	2002-2003	Yes		Yes		Yes	Yes
	2003-2004	Yes		Yes		Yes	Yes
	2004-2005	Yes		Yes		Yes	Yes
	2005-2006	Yes	Yes	Yes	Yes	Yes	Yes
	2006-2007	Yes	Yes	Yes	Yes	Yes	Yes
	2007-2008	Yes	Yes	Yes	Yes	Yes	Yes
	2008-2009	Yes	Yes	Yes	Yes	Yes	Yes
	2009-2010	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the years and grades in which MCAS ELA and math tests were administered between 2002 and 2010.

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