

Accountability and Flexibility in Public Schools: Evidence from Boston's Charters and Pilots

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Abstract

Charter schools are publicly funded but operate outside the regulatory framework and collective bargaining agreements characteristic of traditional public schools. In return for this freedom, charter schools are subject to heightened accountability. This paper estimates the impact of charter school attendance on student achievement using data from Boston, where charter schools enroll a growing share of students. We also evaluate an alternative to the charter model, Boston's pilot schools. These schools have some of the independence of charter schools, but operate within the school district, face little risk of closure, and are covered by many of same collective bargaining provisions as traditional public schools. Estimates using student assignment lotteries show large and significant test score gains for charter lottery winners in middle and high school. In contrast, lottery-based estimates for pilot schools are small and mostly insignificant. The large positive lottery-based estimates for charter schools are similar to estimates constructed using statistical controls in the same sample, but larger than those using statistical controls in a wider sample of

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schools. The latter are still substantial, however. The estimates for pilot schools are smaller and more variable than those for charters, with some significant negative effects.

I Introduction

Charter schools operate with considerably more independence than traditional public schools. They are free to structure their curriculum and school environment. Among other things, many charter schools fit more instructional hours into a year by running longer school days and providing instruction on weekends and during the summer (Matthews 2009, Wilson 2008, Hoxby et al, 2009). Because few charter schools are unionized, they can hire and fire teachers and administrative staff without regard to the collectively bargained seniority and tenure provisions that constrain such decisions in most public schools. Although charter students made up only 2.9 percent of U.S. public school enrollment in 2008-2009, charter enrollment has grown rapidly and seems likely to accelerate in the near future (NAPCS 2009). The growth of charter schools is an active component of the contemporary education reform movement's pursuit of accountability and flexibility in public education.

Proponents see charter schools' freedom from regulation as a source of educational innovation, with the added benefit of providing a source of competition that may prompt innovation and improvement in the rest of the public system. At the same time, charter schools are controversial because, after a transition period in which the state provides subsidies, they receive a tuition payment for each enrolled student paid by students' home (or "sending") districts. In Massachusetts, the site of our study, tuition payments are determined largely by average per-pupil expenditure in sending districts. Not surprisingly, therefore, public school districts are concerned about the revenue lost when their students enroll in charter schools.

The purpose of this paper is to assess the causal effects of charter school attendance and a closely related alternative, called pilot schools, on student achievement. Pilot schools arose in Boston as a union-supported alternative to charter schools.¹ Boston's charter schools are legally constituted by the state as individual school districts and therefore operate independently of the Boston Public Schools (BPS). In contrast, Boston's pilot schools are legally part of the BPS district, and the extent to which they operate outside collective bargaining provisions is spelled out in school-specific election-to-work agreements signed by pilot faculty. In addition to these negotiated exemptions, pilot schools have more flexibility and decision-making powers over school budgets, academic programs, and educational policies than do traditional BPS schools. This freedom includes the opportunity to set school policies

¹See Center for Collaborative Education (2006). Versions of the pilot school model are being tried in Los Angeles (Manzo, 2007).

related to student promotion, graduation, discipline, and attendance.²

In practice, pilot schools occupy a middle ground between charter schools and traditional public schools. Their teachers are part of the Boston Teachers Union (BTU), with their pay, benefits and working hours determined by the district-wide BTU contract. On the other hand, pilot schools can set their own policies with regard to curriculum, student promotion, and graduation. They also fit more instructional hours into a school year than traditional schools, though the pilot school day is typically shorter than that at the charter schools in our sample. Accountability standards appear to bind less strictly for pilot schools than for charter schools: while nine Massachusetts charters have been lost, no pilot school has been closed.

The charter and pilot school models fit into a broader movement towards choice and decentralization in public education. Related policy experiments that have also been the subject of impact evaluations include vouchers for private schools (Rouse 1998; Angrist, Bettinger, Bloom, King and Kremer 2002), magnet schools (Cullen, Jacob, and Levitt 2006), and intra-district choice plans (Abdulkadiroğlu and Sönmez 2003; Hastings, Kane and Staiger 2008; Abdulkadiroğlu, Pathak and Roth 2009; Deming 2009). Charter schools are one of the most widely replicated members of this reform family.

This study contributes to a growing literature that uses admissions lotteries to measure the effects of charter schools on student achievement. Studies of a large sample of schools in New York City and three schools in Chicago find modest effects (Hoxby and Murarka 2009; Hoxby and Rockoff 2005). On the other hand, two schools in the Harlem Children’s Zone appear to produce large gains, with math scores increasing by about half a standard deviation for each year spent in a charter school (Dobbie and Fryer 2009). A recent study by Mathematica Policy Research uses the lottery approach to evaluate over-subscribed charter schools in several states (Gleason, Clark, Tuttle and Dwoyer, 2010). The Mathematica study reports results for urban, high-poverty charter schools similar to those in Hoxby and Murarka (2009). Angrist, Dynarski, Kane, Pathak and Walters (2010) study a Knowledge is Power Program (KIPP) school in Lynn, Massachusetts. The KIPP results show large positive effects a lot like those reported here for Boston charter middle schools.³

²See the Boston Teachers Union website (http://www.btu.org/leftnavbar/HP_PilotSchools.htm), which also notes: “Pilot schools do not have to purchase a variety of services provided by the central office, such as substitute teachers, textbooks, SPED contracted services, and academic coaches. By not purchasing these services pilot schools ‘save’ , typically, \$300 to \$400 per year per student. They are allowed to retain these funds and purchase these services privately if they wish.”

³Farther afield, Clark (2009) uses a regression-discontinuity design to study the impact of attendance at Britain’s grant-maintained schools, a charter-like model. Grant-maintained schools appear to have produced large achievement gains. Charter evaluations that don’t use lotteries have generally produced more mixed results. See, for example, Booker, Sass, Gill, and Zimmer (2008) for Chicago and Florida; Eberts and Hollenbeck (2002) for Michigan; Bifulco and Ladd (2006) for North Carolina; Booker, Gilpatric, Gronberg, and Jansen (2007) for Texas; Berends, Mendiburo, Nicotera

The schools in our study are attended by students who would otherwise attend traditional Boston public schools. The Boston Public Schools (BPS) system serves a disproportionately Black and Hispanic student population. Like students in many urban schools, BPS students have lower test scores and lower rates of high school graduation and college attendance than students from nearby suburban districts. Boston’s charter schools also serve a high proportion of Black students, even relative to the majority non-white BPS district. The effects of charter schools in urban populations are of special interest since any gains in this context might help reduce the black-white achievement gap.

The primary empirical challenge in any study of alternative school models is selection bias. Students who attend charter and pilot schools differ in a number of ways from the general pool of public school students, a fact that biases naive comparisons. We can hope to eliminate some of this bias by controlling for student characteristics such as free lunch status, but the possibility of bias from unobserved variables such as motivation or family background remains. An important aspect of our study, therefore, is the use of student admissions lotteries to estimate causal effects. These lotteries, which admit applicants randomly at over-subscribed schools, are used to construct a quasi-experimental research design that should generate unbiased estimates of the causal effects of charter and pilot attendance on current applicants.

A charter or pilot school contributes application cohorts to our lottery estimates when the school is over-subscribed and therefore runs a lottery, has complete lottery records, and, in the case of pilots, uses a lottery to select students instead of tests or an audition.⁴ In addition, the charter schools in our lottery study were all operating at the time we collected lottery data (closed charter schools have often been under-subscribed). These selection criteria may have consequences for the external validity of our results. The over-subscription condition tilts our sample towards charter and pilot schools that parents find appealing, as does the requirement that schools still be open. From a policy perspective, however, this is an interesting set of schools. As it stands, Massachusetts currently limits both the number of charter schools and the proportion of a district’s budget that can be lost due to charter enrollment. Were the supply of alternative school models allowed to freely vary, it seems reasonable to expect currently operating over-subscribed schools to expand and imitators to open similar schools.⁵

The requirement that participating schools have complete lottery records also affects our selection of charter schools for the lottery study. Specifically, the records requirement tilts the charter lottery sample towards schools that have archived lottery records. Massachusetts law does not require charter schools to retain their lottery data. The net impact of the record-keeping constraint is unclear. On

(2008) for a Northwest Urban District; and CREDO (2009).

⁴More precisely, a given school-year-grade cell contributes to the lottery analysis if entry at that point is over-subscribed and the associated lottery records are available.

⁵Vaznis (2010) discusses proposals for expansion by many of the charter schools in our sample.

one hand, poor record-keeping may be a sign of disorganization that spills over into teaching. On the other hand, lottery record-keeping may be a distraction that takes time and energy away from instructional activity. In some cases, lost records are also a result of bad luck and the fact that the preservation of lottery data is not a priority once the school admissions process is complete.

Finally, on the pilot side, not all schools use the centralized lottery system that is embedded in the BPS school assignment mechanism. Some pilot schools opt out of the BPS assignment mechanism and chose students by a combination of admissions testing or audition. Non-lottery pilots share this feature with Boston’s elite exam schools (the most famous of which is the Boston Latin School). In contrast, over-subscribed charters are legally bound to use lotteries to select students.

In an effort to gauge the generality of our lottery-based findings, we complement the quasi-experimental lottery analysis with an observational analysis of the full set of charter and pilot schools. The observational analysis controls for demographic and background characteristics as well as students’ lagged test scores (for example, the elementary school scores of middle school students). This investigation produces estimates remarkably similar to the lottery-based estimates when carried out in the sample of charter schools that have lotteries, lending some credence to the observational analysis. At the same time, the observational analysis suggests that the charter schools in the lottery study are better than others in the sense of generating larger treatment effects. The schools in the Boston lottery study generally subscribe to a philosophy and pedagogical approach known as “No Excuses.” We therefore think of our (mostly positive) charter estimates as indicative of what the No Excuses charter model can accomplish, rather than an overall charter-school treatment effect.

For pilot middle schools, the observational analysis also generates results that are broadly consistent with the lottery-based analysis, but the observational results for pilot high schools are more positive than the corresponding lottery-based estimates. This leaves a somewhat muddled picture. Across all designs and samples, however, the estimated effects of attending a pilot school are smaller than the corresponding charter estimates. In the subsample of pilot middle schools that serve only grades 6-8, lottery-based estimates of effects on middle school math scores are negative and significantly different from zero.

The next section describes Boston’s charter and pilot schools in more detail and briefly discusses a few related studies and questions. Following that, Section III lays out our lottery-based estimation framework while Section IV discusses data and descriptive statistics. Section V presents the main lottery analysis. Section VI discusses attrition, school switching and reports results from models with ability interactions and possible peer effects. Section VIII reports observational results from a broader sample and compares these to the lottery estimates. The paper concludes in Section IX.

II Background

The 1993 Massachusetts Education Reform Act opened the door to charter schools in Massachusetts. Non-profit organizations, universities, teachers, and parents can apply to the state’s Board of Elementary and Secondary Education for a charter (there are no for-profit charter schools in Massachusetts). Massachusetts charter schools are generally managed by a board of trustees and are independent of local school committees. Like other public schools, charter schools charge no tuition and are funded mostly by sending districts according to formulas set by the state.

Massachusetts charter schools have a number of organizational features in common with charter schools in other states. They are typically outside local collective bargaining agreements and as a result have greater flexibility than traditional public schools in staffing, compensation, and scheduling. The five Massachusetts charter schools studied by Merseth (2009), four of which appear in our lottery study, have a longer school day and year than traditional public schools. Many charter schools offer extensive tutoring during and after school. Teachers in charter schools need not hold an active state license to begin teaching, though they must pass the Massachusetts Test for Educator Licensure within the first year of employment.

Wilson (2008) describes seven Boston charters, six of which are in our lottery study, as well as a charter school in Lynn, near Boston. Wilson identifies school practices prevalent at the schools in his sample. This collection of practices is sometimes said to characterize the “No Excuses” model, a term that probably originates with Thernstrom and Thernstrom (2003). No Excuses schools are characterized by small size, frequent testing, a long school day and year, selective teacher hiring, and a strong student work ethic. Other features include an emphasis on discipline and comportment, teacher-led whole-class instruction, and the absence of computer-aided instruction. Merseth’s (2009) detailed account of the workings of five Boston-area charters, which she calls “high-performing schools,” suggests they share these features.

The first two columns of Table 1 compare some of the statistically measurable differences between Boston charter schools and traditional (BPS) public schools. This table shows student-weighted averages of teacher characteristics and student-teacher ratios by school type. The student-teacher ratio is substantially lower in charter schools and charter teachers are less likely to be licensed or to be “highly qualified” as defined by NCLB. The latter is likely a consequence of the relative inexperience of many charter school teachers, who are substantially younger than teachers in the traditional public schools.⁶ As shown in column 7 of Table 1, the features that characterize the full roster of Boston

⁶The definition of highly qualified has varied over time, but typically this designation is awarded to teachers who have a bachelor’s degree, full state certification or licensure, and have shown that they know the subject they teach (usually this requires some additional certification). Note that in Table 1, the denominators for the proportion licensed and the

charter schools are shared by the schools in our lottery study.

Massachusetts charter schools appear to face more stringent accountability requirements than do traditional public schools. The state Charter School Office reviews and makes recommendations on charter applications, reviews the performance of existing charter schools, and decides whether charters should be renewed. Charter schools are held accountable via annual reports, financial audits, and site visits, and are required to file for renewal every five years. Renewal applications must show that a school's academic program is successful, that the school is a viable organization, and that it has been faithful to its charter. Since 1994, the state has received a total of 350 charter applications and has granted 76. Eight of the 76 Massachusetts charters ever granted were surrendered or revoked as of Fall 2009 (Massachusetts Department of Elementary and Secondary Education, 2009b). A ninth (Uphams Corner Charter School) was revoked later in 2009.⁷

In the 2009-2010 school year, 26,384 Massachusetts students attended 62 charter schools, including 16 in Boston (Massachusetts Department of Elementary and Secondary Education 2009a). State law caps the number of charter schools at 72, and total enrollment at 30,034, so the statewide charter cap is not currently a binding constraint. However, a provision that limits each district's spending on charter school students to nine percent of local school spending generates binding or near-binding caps in districts (including Boston) where charter enrollment is relatively high. The question of whether to lift local caps is currently the subject of intense debate, fueled in part by the availability of federal stimulus money for states that facilitate new charters (Vaznis 2009).

Pilot schools were developed jointly by BPS and the Boston Teachers Union (BTU) as an alternative to both charter schools and traditional public schools. Pilot schools are created as the result of a planning process currently funded by the Boston Foundation, a private charity, with technical assistance from the Center for Collaborative Education, a local nonprofit organization that runs the Boston Pilot Schools Network. New schools may be granted pilot status but most are conversions from traditional BPS schools. Pilot school conversions must be authorized by a two-thirds vote of the BTU membership employed at the school and authorized by the BTU Pilot School Steering Committee.⁸

Like charter schools, pilot schools are answerable to independent governing boards. Also like charters, pilot schools determine their own budgets, staffing, curricula, and scheduling. Unlike charter schools, however, they remain part of the Boston school district and their teachers are BTU members

proportion highly qualified differ.

⁷Four of the eight charter losses through Fall 2009 occurred before school operations began. Two of the remaining four were revocations and two were non-renewals.

⁸The pilot school model originated in Boston but other Massachusetts districts have begun to experiment with it. The Massachusetts Board of Elementary and Secondary Education recently adopted a Commonwealth Pilot School option for schools that otherwise would have been designated as underperforming under NCLB. Five Commonwealth Pilot Schools are now operating in Boston, Fitchburg, and Springfield.

covered by most contract provisions related to pay and seniority. Pilot school teachers have no job protection within schools but remain in the BPS system if they choose to leave the school or are removed by the pilot school principal. Many pilot schools also develop and advertise a curriculum with a distinctive emphasis or focus, such as technology or the arts. In this respect, pilot schools are something like magnet schools.

Pilot teachers sign an election-to-work agreement that spells out the extent to which union contract provisions apply. These agreements vary by school.⁹ Pilot schools are subject to external review, but the review process to date appears to be less extensive and structured than the external state charter reviews. No pilot school has been closed or converted back to a traditional public school.¹⁰ Pilot schools are open to all students in the BPS district and operate as part of the district. In the 2007-8 school year, 6,337 BPS students were enrolled in 20 pilot schools. Assignment to all elementary and middle pilot schools, and to two of the seven regular pilot high schools, is through the centralized BPS choice plan, which includes a lottery when schools are over-subscribed.

Pilot teachers have characteristics between those of traditional BPS schools and charter schools, as can be seen in columns 3 and 8 of Table 1. For example, pilot teachers are younger than traditional BPS teachers but not as young as charter teachers. Many pilot schools share with charter schools longer school days and years. But the BTU agreement covering pilot schools limits uncompensated overtime, as do school-specific election-to-work agreements. This is reflected in statistics on hours of instruction that we collected from the schools in the lottery study. The official BPS school year is 180 days, with a little over six hours of instruction per day, for a total of 1,110 annual school hours. Annual school hours at pilot middle and high school hours run a little longer, but still under 1,200 hours per year. In contrast, the average charter middle school in our sample provides 1,500 hours of instruction, while charter high schools provide about 1,400 hours.¹¹

III Empirical Framework

We're interested in the effects of charter or pilot school attendance on student achievement. Because the effects of attendance at different types of school seem likely to be an increasing function of the time spent in school, we model score effects as a function of years in pilot or years in charter. The causal relation of interest is captured using equations like this one for the scores, y_{igt} , of student i in

⁹See <http://www.ccebos.org/pilotschools/resources/index.html> for sample agreements.

¹⁰For more on pilot structure, see http://www.ccebos.org/pilotschools/pilot_qa.doc and <http://www.ccebos.org/pilotguides/>. The current BTU contract allows for the creation of up to seven additional pilot schools. In 2007, two pilot conversions were voted down, while the Boston School committee, approved three new pilots.

¹¹Data on hours of instruction at charter and pilot schools come from the individual schools' web sites.

grade g testing in year t :

$$y_{igt} = \alpha_t + \beta_g + \sum_j \delta_j d_{ij} + \gamma' X_i + \rho S_{igt} + \epsilon_{igt}. \quad (1)$$

The variable S_{igt} is the years spent in a charter or pilot school as of the test date, counting any repeated grades, and counting time in all charter and pilot schools, not just the ones in our lottery study. We define a year to be a charter or pilot year if any portion of that year is spent in a charter or pilot school. The causal effect of S_{igt} is ρ . The terms α_t and β_g are year-of-test and grade-of-test effects, while X_i is a vector of demographic controls with coefficient γ , and ϵ_{igt} is an error term that reflects random fluctuation in test scores. The dummies d_{ij} are indicators for lottery-specific risk sets (indexed by j), described below.

If S_{igt} were randomly assigned, ordinary least squares (OLS) estimates of (1) would capture an average causal effect of years spent at a charter or pilot school. Because students and parents selectively chose schools, however, OLS estimates may be biased by correlation between school choice and unobserved variables related to ability, motivation, or family background. We therefore use an instrumental variables (IV) strategy that exploits the partial random assignment of S_{igt} in school-specific lotteries. Assuming the applicant lotteries are fair, students who win and lose a given lottery should have similar characteristics.

The first stage equations for IV estimation take the form:

$$S_{igt} = \lambda_t + \kappa_g + \sum_j \mu_j d_{ij} + \Gamma' X_i + \pi Z_i + \eta_{igt}, \quad (2)$$

where λ_t and κ_g are year-of-test and grade effects in the first stage. The first-stage effect is the coefficient, π , on the instrumental variable, Z_i . The instruments are dummy variables indicating applicants who were offered seats in charter or pilot school lotteries. Equation (1) does not impose a linear effect of years in a charter school; rather, IV estimates of ρ can be interpreted as the weighted-average causal response to each year spent in a charter or pilot school, where the weights are proportional to effect of the instrument on the cumulative distribution function of the endogenous variable.¹²

In practice, the use of lottery instruments is complicated by the fact that the odds of being offered a seat at a charter or pilot school vary with the number of applications and the extent to which an applicant's chosen schools are over-subscribed. We therefore control for the number and identity of schools to which an applicant applied; this group of schools is called the applicant *risk set*. For a given charter applicant, the charter risk set is the list of all lotteries to which the student applied in

¹²For more details on this interpretation, see Angrist and Imbens (1995).

a given year and entry grade, among the lotteries included in the charter lottery analysis. Students who did not apply to any of the charter schools in the lottery study are not in any charter risk set and are therefore omitted. The relevant sample of pilot applicants includes only those students who listed a pilot school first on their BPS assignment form (few students who did not do so end up in a pilot school). The pilot risk set is defined by the identity of this first-choice school and the applicant's walk-zone status. Charter and pilot risk sets also vary by grade of entry and year of application (the entry cohort).¹³

IV Data and Descriptive Statistics

The Massachusetts Students Information Management System (SIMS) contains information on all Massachusetts public school students' race, ethnicity, sex, reduced-price lunch status, special education status, English-language learner status, town of residence and current school. These data are collected in October and again at the end of the school year. We worked with SIMS files for the 2001-2002 through 2008-2009 school years. The SIMS data were used to determine how many years students spent in a charter, pilot or traditional BPS school. A student observed at any time during a school year in a charter or pilot school was classified as a charter or pilot student for that year. To construct an analysis file, we used student identifiers to merge SIMS demographic and school history data with test scores from the Massachusetts Comprehensive Assessment System (MCAS) database, from spring 2002-2008. The MCAS database contains raw scores for math, English language arts (ELA), and writing. MCAS is administered each spring, typically in grades 3-8 and 10. For the purposes of our analysis, scores were standardized by subject, grade, and year to have mean zero and unit variance in the population of students attending Massachusetts public schools.

Lottery Procedures and Sample Coverage

We matched the MCAS and SIMS data to applicants records from charter school lotteries.¹⁴ Each charter school collects applications and holds its own lottery in years in which the school is over-

¹³The relevant risk set for students in the pilot lottery is based on the BPS assignment mechanism. Among first-choice applicants to a given pilot school, admission priority is randomly assigned, with lotteries run separately for students who live inside and outside the school's walk-zone. In the pilot analysis, the risk set is therefore specified as the interaction of the four variables indicating the student's first-choice pilot school, walk-zone status for that school, and the year and grade of application.

¹⁴We matched records using applicants' names as well as year and grade of application. Gender, race, town of residence, and date of birth were used to resolve ambiguities. We matched 93.0 percent of charter applicants at the middle school level (93.6 percent of those admitted and 92.4 percent of those not admitted) and 95.8 percent of applicants at the high school level (95.8 percent of those admitted and 95.9 percent of those not admitted). Additional information related to the construction of analysis files appears in the data appendix.

subscribed. Siblings of students already attending the school are guaranteed a seat, as are students continuing on from earlier grades, so siblings and continuing students are omitted from the lottery analysis.

We contacted all operating charter schools in Boston and asked for current and past lottery records. This resulted in a set of 5 middle schools and 3 high schools with usable records from over-subscribed lotteries. Appendix Table A1 details the universe of Boston charter schools and the applicant cohorts included in our study, along with notes explaining why schools or cohorts were omitted. Of the four charter schools with elementary grades, three had no usable records. A fourth K-8 school had records for a cohort of 6th grade applicants and is included in the middle school sample. Of 10 currently operating charter schools that enroll middle school students, five contribute to the lottery analysis. Two charter middle schools closed before or while our study was under way; one was under-subscribed. One of the excluded middle schools was too new to contribute outcome data and four had inadequate records. Two of the omitted middle schools admit primarily in elementary grades in any case.

Of four operating charter schools that admit students at the high school level, three contribute to the lottery analysis. Two charter high schools closed before or during our study (one was under-subscribed). We also omit charter high schools that focus on non-traditional, older, or working students. The fourth, Health Careers Academy (HCA), is omitted because the lotteries at this school appear to have been substantially non-random, with marked imbalances in baseline scores between winners and losers. A case could be made for excluding this school anyway: HCA is a Horace Mann Charter school that operates under somewhat different rules than regular Massachusetts charters (known as “Commonwealth charters”) and pilots. (Our working paper, Abdulkadiroglu et. al. (2009), reports results including HCA and discusses the difference between Horace Mann and Commonwealth charter schools.) Two schools with high school grades admit only in middle school and are therefore included in the middle school sample.

Students can apply to as many charter schools as they like; charter school lotteries are statistically and administratively independent. Applicants may therefore be accepted or wait-listed at more than one school. When admitted students decline, slots open up for additional offers farther down the lottery list. Thus, some students are offered spots immediately, while others may be offered seats closer to the beginning of the school year. This fact allows us to construct two charter instruments: *initial offer* and *ever offer*. Initial offer is a dummy set to one if a student is offered a seat at one of the schools in the applicants’ charter risk set, while ever offer also counts offers made later. Suppose, for example, that 200 applicants apply for 100 seats. All applicants are sequenced in the lottery and the first 100 receive an initial offer in March, the day of or the day after the lottery. Because some students decline offers or cannot be located, an additional 50 are offered seats in August. Thus, 150

are coded as ever receiving an offer.

The validity of the offer instruments turns in part on the completeness of school lottery records. We attempted to recover the original sequence numbers and initial offer data as well as ever offer data. However, the complete sequence was not always available and the initial offer instrument cannot be constructed for some cohorts in some schools; see appendix Table A1 for details. We cannot be sure that the lottery data is complete or accurate. But the offer variables are highly correlated with eventual enrollment, while the demographic characteristics and pre-application test scores of winners and losers are reasonably well balanced, as we show below. This suggests our lottery reconstruction was successful though for reasons discussed below, we prefer the more complete and possibly more reliable ever-offer instrument to initial.

Students apply to pilot schools as part of the regular BPS assignment mechanism. Most BPS schools are choice schools. BPS parents submit a rank order list of at least three schools in January to obtain a seat at a new school in September. At each school, admission priority is determined in part by whether the applicant is a continuing student who is guaranteed admission, currently has a sibling at the school, or lives in the school's walk zone. Within these priority groups, students are selected using an ordering determined by the BPS lottery number. The choice mechanism tries to assign as many students as possible to their top choice, using coarse priority rankings and lottery numbers when there are more applicants than capacity.¹⁵ This produces a system that induces random assignment with varying probabilities, conditional on priority groups such as sibling and walk-zone status.

Students were classified as pilot applicants if they listed a pilot school as their first choice. Because most pilot schools are oversubscribed, students who rank a pilot school as a second or lower choice are unlikely to be assigned to a pilot. The BPS assignment mechanism runs in multiple rounds but we use information only from the first round. Data on parents' choices and BPS lottery numbers came from the BPS applications data base. These data were matched to our merged MCAS-SIMS analysis file using state student identifiers.

All elementary and middle school pilots use the BPS assignment mechanism and lottery, but only two pilot high schools do so. Four others use school-specific admissions criteria, such as musical auditions, to select their students. One is a 6-12 school that was not over-subscribed. Of the seven pilot schools that enroll elementary school students, five were over-subscribed and contribute to the lottery study. Of seven pilot middle schools admitting 6th graders, six were over-subscribed and contribute to the lottery study. Of the four K-8 pilot schools, our lottery middle school sample includes kindergarten applicants from three (the kindergarten entry grade is known as K2, the year after preschool, K1). One K-8 pilot school opened too late to contribute middle school test scores by K2 applicants.

¹⁵For details, see Abdulkadiroğlu and Sönmez (2003) and Abdulkadiroğlu, Pathak, Roth and Sönmez (2006).

Student Characteristics and Covariate Balance

Table 2 reports descriptive statistics for students at Boston’s traditional schools, charter schools, and pilot schools, as well as separate tabulations for those included in the charter and pilot lottery samples. The racial and ethnic composition of the student bodies attending pilot elementary and middle schools is similar to that at traditional BPS schools: around 45 percent Black and 30 percent Hispanic. In contrast, charter schools have a higher proportion of Black students (about 70 percent) and a lower proportion of Hispanic students (about 20 percent). Differences in racial make-up across school types are similar at the high school level.

Roughly 85 percent of students at traditional Boston schools are eligible for a free or reduced-price lunch, a measure of poverty status. Charter students are not as poor; about 70 percent fall into this category. The pilot school student body occupies a middle ground, with more poor students than the charter schools but fewer than the traditional schools. Relatively few English language learners (also known as limited English proficiency or LEP students) attend charter schools. For example, just over seven percent of charter middle schools students are LEP, while the traditional Boston population is 20 percent LEP (pilot schools are also at 20 percent). Charter schools also enroll fewer special education students than traditional and pilot schools. Girls are over-represented at charter schools and, to a lesser extent, at pilot schools; this is particularly striking at the high school level, where 60 percent of charter school students are female, compared to 52 percent at the pilot schools and 50 percent at traditional schools. Importantly, however, the demographic make-up of the charter and pilot lottery samples, described in columns 6 and 7 of Table 2, is similar to that of the total charter and pilot samples.

Table 2 also reports pre-treatment test scores, which are measured in elementary school for the middle school sample and in middle school for the high school sample. For middle school students, baseline scores come from tests taken in fourth grade while for high school students baseline scores come from tests taken in eighth grade. There are no baseline scores for elementary school students, since MCAS testing starts in third grade. Baseline scores are normalized by year and subject to have zero mean and unit standard deviation among all test takers in Massachusetts.

At the middle school level, pilot school students have somewhat lower baseline scores than students at traditional schools, while the baseline scores of charter students are higher than those of students in traditional BPS schools. At the high school level, charter school students have higher baseline scores, averaging about 0.4 standard deviations above those of students in traditional schools and a tenth of a standard deviation above those of students attending pilot schools. Among charter school students applying to lotteried middle schools, there is a baseline advantage of about 0.2 standard deviations. This baseline difference motivates a brief analysis of ability interactions and peer effects, discussed

after presentation of the main results and robustness checks.

As a measure of lottery quality, Table 3 reports differences in demographic characteristics and baseline scores between lottery winners and losers. The numbers reported in the table are regression-adjusted differences by win/loss status, where a win means a student was offered a spot in a charter or pilot school in the relevant risk set (this is the ever-offer instrument). The regressions control only for risk sets (year of application and the set of schools applied to for charters; first-choice school, year of application, and walk zone status for pilots). Conditional on these covariates, offers should be randomly assigned.

With a few exceptions, the differences in Table 3 are small and statistically insignificant. There are no significant contrasts for middle school applicants. Among high school applicants, charter lottery winners are 5 percentage points less likely to be Hispanic and about 5 percentage points more likely to be black than losers. These differences are only marginally significant. Among elementary pilot school applicants, lottery winners are seven percentage points less likely to be eligible for a subsidized lunch; among high school applicants, this comparison has the opposite sign. These and the other scattered marginally significant contrasts in the table seem likely to be chance findings, a conclusion supported by the F statistics at the bottom of each column, which test the joint hypothesis that all differences in baseline test scores and background characteristics in the column are zero.¹⁶

V Lottery-Based Estimates

Charter School Effects

Charter middle school applicants offered a spot at one of the schools to which they applied spent about a year longer attending a charter school than applicants who were not offered a spot as of the MCAS test date. This can be seen in column 1 of Table 4A (labeled “first stage”). With perfect compliance, equal-sized cohorts, and no dropouts or loss to follow-up, the first stage for the middle school lotteries would be two years, since this is the average time spent in middle school as of MCAS exams in 6th, 7th and 8th grade. In practice, about a fifth of lottery winners never attend a charter school, while some lottery losers eventually end up in a charter school (by entering a future admissions lottery, gaining sibling preference when a sibling wins the lottery, or moving off a wait list after the offers coded by our instrument were made). The first stage is also affected by the fact that some

¹⁶We also estimated covariate balance models restricted to students who have follow-up data. These results, reported in Table A5A for ever-offer, are similar to those in Table 3. In a school-by-school covariate balance analysis for lottery applicants, none of the schools included in the study have imbalance significant at a 10% level using the ever offer instrument. As noted above, school-by-school covariate balance showed one high school with substantial and significant imbalance at a level that seems very unlikely to be a chance event. This school was therefore dropped from the analysis.

students who initially enroll in a charter school later switch, an issue we explore further below.

Middle-school students who won a charter lottery scored about 0.25σ higher on ELA and 0.40σ higher in math, a result shown in column 2 of Table 4A (labeled “reduced form”). The 2SLS estimate of the effect of an additional year in a charter school is the ratio of the reduced-form estimates to the first-stage coefficients. Since the first stage coefficients are close to one, the 2SLS estimates (reported in column 3) are similar to the reduced-form estimates, though their interpretation differs. When estimated without demographic controls, the 2SLS estimates imply that ELA scores increase by about 0.25σ for each year in a charter, while the per-year math effect is 0.42σ . These estimates are reasonably precise, with standard errors around 0.07, showing that our research design has the power to detect more modest effects as well. The addition of controls for demographic characteristics and baseline scores has little effect on the middle school estimates, as can be seen in columns 4 and 5.

Although the reduced-form effects for high school math scores are smaller than the corresponding reduced form effects in middle school, the high school first stage is also smaller. As a consequence, the math score gains generated by time spent in charter high schools are estimated to be similar to the corresponding 2SLS estimates for middle school. The 2SLS estimate for high school ELA without controls is smaller and not quite significant. With demographic controls the estimate is a marginally significant 0.18σ , and when baseline score controls are added, the high school ELA effect is a fairly precisely estimated 0.27σ . High school students also take a writing topic and composition test; here the 2SLS estimates show mostly significant gains ranging from $0.21\sigma - 0.35\sigma$.

Pilot School Effects

Our lottery-based analysis of pilot effects looks at elementary-grade outcomes as well as test scores from middle and high school. The impact of a pilot school offer on time spent in elementary school is almost three years, as can be seen at the top of column 6 in Table 4A. The relatively large elementary-level pilot first stage is driven by the fact that elementary school applicants apply to enter in kindergarten, while they are not tested until third or fourth grade. The reduced form effect of a pilot school offer on elementary school applicants is a about 0.21σ for ELA, but this translates into a much smaller per-year effect of 0.07σ , reported in column 8 for models without demographic controls. The reduced-form math result is smaller and not significantly different from zero.

The estimated effect of a pilot offer on time spent in high school is similar to the corresponding first stage for charter applicants, while the pilot middle school first stage is larger. On the other hand, the estimated effects on ELA and math scores with no controls or demographics—both reduced form and 2SLS—are small and not significantly different from zero. Here too, it’s worth pointing out that the standard errors are such that modest effects, say on the order of 0.1σ , would be detectable in

middle school, though the high school design has less power. Like the corresponding estimates for middle school, the high school estimates of effects on math and ELA scores are close to zero, though the high school pilot results show significant effects on writing.

With addition of controls for baseline scores, the middle school math effect is significant but negative, at -0.22σ . This is a puzzling result in view of the fact that there is little relation between the pilot lottery instruments and baseline scores, so the change in middle school math estimates cannot be attributed to omitted variables bias. Rather, this result stems from the loss of K-8 pilot schools in the lagged-score sample. We confirmed this by estimating middle school pilot effects with demographic controls in a sample that includes grade 6-to-8 middle schools only. These (unreported) results are similar to those with lagged score controls. Thus, grade 6-to-8 pilot schools appear to be weaker than K-8 schools, at least as measured by their impact on MCAS math scores.

Robustness

As a check on the main results using the ever-offer instrument, Table 4B reports a similar set of results using initial offer. This is a check on the consistency of our lottery reconstruction effort since both instruments should be valid, though the initial offer sample is smaller. When estimated without baseline scores, the middle and high school results in Tables 4A and 4B are remarkably similar. For example, middle school estimates using the initial offer instrument with demographic controls produces an estimate of 0.21σ for ELA and 0.37σ for math. The addition of baseline scores pulls the middle school math effect down to 0.25σ . The initial offer estimates for high school also come out broadly similar to the ever-offer estimates, though the ELA result without controls is not significantly different from zero. This sensitivity seems unsurprising given the smaller initial-offer sample and the fact that covariate balance is not as good for initial offer in the follow up sample. This result also accords with our impression that the reconstruction of ever offer was more successful than our attempt to determine when those offers were made.¹⁷

An alternative parameterization of the ever offer first stage uses potential years in charter for winners. Potential years counts the grades a student who wins the lottery would spend in a charter school if the offer is accepted and the student stays in school. A student who does not get an offer is coded as having zero potential years. The potential offer parameterization is useful because it generates a natural benchmark for charter and pilot student mobility. Specifically, these first-stage estimates

¹⁷Table A5B reports covariate balance results using the initial offer variable. Using all lottery applicants with initial offer data generates balance results similar to those in Table 3. Among charter applicants with followup scores, however, the overall p-values from the joint F-tests range from 0.008 to 0.08, driven by the fact that initial offer winners are about 4 percentage points less likely to be LEP. Consistent with this, in a school-by-school analysis, half the schools show initial offer imbalance at a 1% level or lower.

can be compared to the expected years spent in any new middle or high school for an arbitrary BPS student.

The potential offer first stage, reported in columns 1 and 6 of Table 4C, shows that a middle school lottery winner spends about 0.42 years in charter for every potential year in a charter school and 0.40 years in pilot for every potential year in a pilot school. In high school, a charter lottery winner spends about 0.25 years in charter for every potential year in a charter school while the potential-year pilot first stage is 0.32. These first stages are similar to the BPS pseudo first stage that links the time actually spent in any new middle or high school to potential years in that school. Specifically, the pseudo first stage for BPS middle school is about 0.4 while the pseudo first stage for BPS high schools is about 0.3. In addition, Table 4C shows that using potential years as an instrument yields charter and pilot effect estimates that are remarkably similar to those reported in Table 4A.

Figures 1 and 2 provide evidence on the question of whether time in charter has a cumulative effect, as is implicit in our years-in-charter/years-in-pilot 2SLS models. These figures plot middle school reduced-form estimates by cohort and grade. The plots start in fourth grade, the baseline comparison, where differences should be small.¹⁸ Not surprisingly, treatment effects estimated at this level of aggregation are fairly noisy and few are individually significant. On the other hand, the mostly increasing middle school math reduced form estimates suggest a cumulative effect, especially in the ever-offer reduced form. The initial-offer reduced forms are flatter, though it should be noted that even a flat reduced form implies an increasing second-stage estimate because the first stage falls over time.¹⁹

Consistent with the smaller pooled estimates for ELA, the cohort-by-grade ELA estimates in Figure 2 are mostly smaller and noisier than those Figure 1 for math. Here, too, however, the trend in cohort-specific reduced forms is mostly up or at least flat, implying increasing second-stage effects as charter exposure increases. Figure 3 is a pair of similar plots for pilot schools; these show no evidence of an effect in any cohort or grade.

Finally, we document the impact of individual risk sets on our estimates through a visual representation of IV estimates based on a version of equations (1) and (2). Averaging equation (1) conditional on treatment status and risk set (and dropping covariates), we have

$$E[y_{igt}|d_{ij} = 1, Z_i] = \alpha_t + \beta_g + \delta_j + \rho E[S_{igt}|d_{ij}, Z_i] \quad (3)$$

¹⁸The sample used to construct Figures 1 and 2 includes applicants with baseline scores. The reduced-form estimates plotted in the figure come from models that include risk set and demographic controls.

¹⁹As suggested by the potential years results, the grade-by-grade first stage declines since some of those offered a seat switch out while some losers end up in charters down the road. As a result the first stage for “charter in eighth grade” is only about half the size if the first stage for “any time in charter.” The same reasoning applies to a grade-by-grade analysis of pilot reduced forms.

Differencing (3) by offer status within risk sets, this becomes

$$E[y_{igt}|d_{ij} = 1, Z_i = 1] - E[y_{igt}|d_{ij}, Z_i = 0] = \rho(E[S_{igt}|d_{ij}, Z_i = 1] - E[S_{igt}|d_{ij}, Z_i = 0]). \quad (4)$$

In other words, the slope of the line linking offer-status differences in test scores within risk sets to the corresponding offer-status differences in average years at a charter or pilot school should be the causal effect of interest, ρ .

The empirical counterpart of equation (4) for charter applicants' middle school math scores appears in Panel A of Figure 4. The unit of observation here is a charter risk set (The plots exclude two charter and five pilot risk sets without at least five lottery winners and five lottery losers.) The regression line fits well and suggests that the charter school effect is not driven by a small number of risk sets. The slope of the line in the figure is 0.46. The corresponding 2SLS estimate of ρ using a full set of offer \times risk set dummies as instruments in a model without covariates is 0.45.²⁰ In contrast, the analogous plot for pilot schools, plotted in Panel B, shows a flatter line, with a slope of -0.009 . The pilot x-axis has a wider range than that for charters because applicants to pilot K-8 schools spend a longer time in a pilot school than applicants to grade 6-8 schools, the typical structure for charter middle schools.

Magnitudes

A natural benchmark for the size of the estimates reported here is the black-white test score gap. Among students attending regular BPS middle schools, blacks score about 0.7σ below whites in ELA and 0.8σ below whites in math. The charter middle school effect of 0.2σ on middle school ELA scores is therefore enough to close most of the black-white reading gap for students who stay in a charter middle school through grade 8. The even larger math gains of about 0.4σ are more than enough to eliminate the racial gap in mathematics achievement. The effects of roughly 0.2σ estimated for high school ELA and 0.35σ math are also large enough to close or eliminate the black-white high school gap of about 0.8σ in both subjects (assuming four years of charter high school enrollment).

These are extrapolations based on the per-year average effects estimated by 2SLS and are not direct estimates. On the other hand, Figures 1 and 2 are consistent with cumulative effects. These projections also presume equal effects for black and white students. Estimates by race (not reported here) are consistent with this interpretation.

²⁰The sample analog of equation (4) can be thought of as a strategy that combines Wald estimates based on Z_i across risk sets. Generalized Least Squares (GLS) estimation of the sample analog of equation (3) can be shown to be the same as 2SLS using a full set of offer \times risk set dummies as instruments (Angrist 1991). OLS estimation of (4) is not exactly 2SLS because OLS does not weight by the number of observations in each risk set. In practice, the results here are close to the corresponding 2SLS estimates reported in Table 4A (0.42σ).

VI Threats to Validity

Selective Attrition

Lottery winners and losers should be similar at the time the lotteries are held. Subsequent attrition may lead to differences in the follow-up sample, however, unless the attrition process itself is also random. In other words, we worry about differential and selective attrition by win/loss status. For example, losers may be less likely to be found than winners, since students who lose the opportunity to attend a charter or pilot school may be more likely to leave the public schools altogether. Differential attrition generates selection bias (although those who leave Boston for another Massachusetts public school district should turn up in our sample). A simple test for selection bias looks at the impact of lottery offers on the probability lottery participants contribute MCAS scores to our analysis sample. If differences in follow-up rates are small, selection bias from differential attrition is also likely to be modest.²¹

Table 5 reports the mean follow-up rate for lottery participants along with estimates of win-loss differentials. Roughly 80 percent of charter and pilot lottery losers contribute a post-randomization test score. These high follow-up rates are due to the fact that our extract is limited to those enrolled in BPS or a Boston-area charter school at baseline and to our use of a statewide MCAS data set. Follow-up differentials by win/loss status were estimated using regression models that parallel the reduced forms reported in Table 4A. Positive coefficients indicate that lottery winners are more likely to contribute an MCAS score.

The estimated follow-up differentials for charter high school applicants are virtually zero. The follow-up differentials for charter middle school outcomes are a little larger, on the order of 3-4 percentage points. Selective attrition of this magnitude is unlikely to be a factor driving the charter results reported in Table 4.

There are virtually no attrition differentials for pilot middle school applicants. The largest differentials turn up for participants in pilot high school lotteries, as can be seen in columns 5 and 6 of Table 5. For example, controlling for demographic characteristics, high school winners are roughly 6 percentage points more likely to have an ELA test score than losers, a significant effect with an estimated standard error of 2.4. But this too seems unlikely to explain our results, which show no effect on pilot lottery winners in high school. First, the most likely scenario for selective attrition has relatively high achieving losers dropping out. Second, the attrition differentials in this case are still

²¹More formally, if attrition can be described by a latent-index model of the sort commonly used to model discrete choice in econometrics, then selection bias in lottery comparisons arises only if winning the lottery affects the probability of MCAS participation. See, e.g., Angrist (1997).

fairly small.²² Nevertheless, as a check on the main findings, we discarded the most imbalanced cohorts to construct a sample of charter middle school and pilot high school applicants with close-to-balanced attrition. We then re-estimated treatment effects using this balanced sample. Attrition differentials for balanced cohorts are reported in appendix Table A3, while the corresponding lottery-based estimates of treatment effects are reported in Table A4. These results are similar to those reported in Table 4A.

School switching

Charter critics have that argued that large achievement gains at No Excuses charters are driven in part by efforts to encourage weaker or less committed students to leave. For example, Ravitch (p. 156, 2010) writes: “Schools of choice may improve their test scores by counseling disruptive students to transfer to another school or flunking low-performing students, who may then decide to leave.” A report on charter schools in the San Francisco Bay area is widely cited as evidence in support for this concern (Woodworth, *et al.*, 2008).

The estimates reported in Table 4 are not directly affected by excess withdrawals since these estimates are driven by win/loss comparisons (the 2SLS reduced forms), without regard to whether students enroll or stay in the charter schools where they received an offer. Thus, the winner group includes students who switch as well as those who stay. Likewise, the loser group includes a few highly motivated students who succeed in enrolling in a charter at a later date. The reduced form effects of charter offers are large and mostly significant, while the pilot reduced forms are mostly zero.

At the same time, excess withdrawals by weak or unmotivated students potentially boost our lottery based estimates if those who leave would have been disruptive or generate negative peer effects. It therefore makes sense to look for evidence of excess withdrawals. Some of the relevant evidence appears in Table 4C, which shows that the ratio of actual to potential time-in-charter is similar to the ratio of actual to potential time in school for an average BPS student (about .4 for middle school and .3 for high school). We add to this here with a direct look at school switching.

About 47 percent of charter middle school applicants and 31 percent of pilot middle school applicants switch schools at some point after the lottery to which they applied. This can be seen in the row labeled “mean switch rate” in Table 6. This table shows that charter lottery winners are about 15 percentage points less likely to switch than losers. This estimate comes from regression models that parallel the reduced forms reported in Table 4A, where the dependent variable is an indicator variable equal to one if a student switched schools and the instrument is ever offer.

²²In a school-by-school attrition analysis using the ever offer instrument, two schools have marginally significant follow-up differentials of 5-7%, though only one is significant at the 0.05 level. Three out of eight schools have initial offer follow-up differentials at the 0.05 level, and one of these is significant at the 0.01 level.

This reduced switch rate is partly mechanical since many charter middle schools start in grade 6, while most regular BPS students switch between grades 5 and 6 when they start middle school. Moreover, switched are driven by some of the applicant population entering one of Boston’s three exam schools in grade 7. Omitting any grade 5-6 and exam school transitions, charter lottery winners and losers experience roughly the same switch rate. At pilot middle schools, winners are less likely to switch on average, but this difference is not significantly different from zero.

Among high school applicants, charter lottery winners are more likely to switch schools than losers, a marginally significant difference of 5-6 percentage points. Excess switching comes from a single charter high school; without applicants to this schooling the sample, the differential falls to 1-2 percentage points, while the estimated charter high school effects are essentially unchanged. On balance, therefore, we find little evidence to suggest high mobility out of charter schools drives the main findings.

VII Ability Interactions and Peer Effects

The fact that charter applicants have baseline scores somewhat higher than the BPS average motivates an analysis of treatment effect heterogeneity. Specifically, we explore treatment effect interactions with an applicants’ own ability and interactions with the ability of peers. The interaction with own ability addresses the question of whether charter schools do well by serving a relatively high-ability group (since charter applicants have higher baseline scores). The interaction with peer ability provides evidence on the extent to which peer effects might explain our findings. The analysis here and in the rest of the paper focuses on middle and high school students since elementary school applicants have no baseline scores.

The equation used to estimate models with own ability interaction terms looks like this:

$$y_{igt} = \alpha_t + \beta_g + \sum_j \delta_j d_{ij} + \gamma' X_i + \rho_0 S_{igt} + \rho_1 S_{igt} (B_i - b_g) + \epsilon_{igt}, \quad (5)$$

where B_i is student i ’s baseline score and b_g is the average B_i in the sample, so that the main effect of S_{igt} , ρ_0 , is evaluated at the mean. The vector of covariates, X_i , includes baseline scores. The interaction term of interest is ρ_1 ; this term tells us whether effects are larger or smaller as baseline

ability increases. The corresponding first stage equations are

$$S_{igt} = \lambda_{1t} + \kappa_{1g} + \sum_j \mu_{1j} d_{ij} + \Gamma'_1 X_i + \pi_{10} Z_i + \pi_{11} B_i Z_i + \eta_{1igt} \quad (6)$$

$$S_{igt}(B_i - b_g) = \lambda_{2t} + \kappa_{2g} + \sum_j \mu_{2j} d_{ij} + \Gamma'_2 X_i + \pi_{20} Z_i + \pi_{21} B_i Z_i + \eta_{2igt}, \quad (7)$$

so that equation (5) is identified by adding an interaction between B_i and Z_i to the instrument list.

The effects of charter middle school attendance are larger for students with *lower* baseline scores, though the estimated interaction terms are small. This can be seen in the second column of Table 7, which reports 2SLS estimate of ρ_0 and ρ_1 in equation (5). For example, a lottery applicant with a baseline score 0.2σ below the mean is estimated to have an ELA score gain that is 0.025σ higher ($0.123 * 0.2 = 0.025$) and a math score gain that is 0.029σ higher ($0.146 * 0.2$) than an applicant with a baseline score at the mean. None of the estimated own-ability interaction terms for applicants to charter high school are significantly different from zero. These results, which are similar to our estimates of own-ability interactions in a KIPP middle school (Angrist, et al, 2010), weigh against the view that charter schools focus on high achieving applicants. There are no significant own-ability interactions from the analysis of treatment effects in pilot schools.

Estimates of models with peer-ability interactions were constructed from the following second-stage equation

$$y_{igt} = \alpha_t + \beta_g + \sum_j \delta_j d_{ij} + \gamma' X_i + \rho_0 S_{igt} + \rho_1 S_{igt}(\bar{b}_{(i)} - \bar{b}_g) + \epsilon_{igt}, \quad (8)$$

where $\bar{b}_{(i)}$ is the mean baseline score (without i) in i's risk set and \bar{b}_g is the mean of this variable in the sample. Applicants in risk sets with higher scoring peers are likely to end up in charter or pilot schools with higher scoring peers if they win the lottery. This model therefore allows treatment effects to vary as a function of peer quality. The corresponding first-stage equations are

$$S_{igt} = \lambda_{1t} + \kappa_{1g} + \sum_j \mu_{1j} d_{ij} + \Gamma'_1 X_i + \pi_{10} Z_i + \pi_{11} \bar{b}_{(i)} Z_i + \eta_{1igt} \quad (9)$$

$$S_{igt}(\bar{b}_{(i)} - \bar{b}_g) = \lambda_{2t} + \kappa_{2g} + \sum_j \mu_{2j} d_{ij} + \Gamma'_2 X_i + \pi_{20} Z_i + \pi_{21} \bar{b}_{(i)} Z_i + \eta_{2igt}. \quad (10)$$

Note that the covariate vector, X_i , includes main effects for applicant risk sets.

Contrary to the usual view of high achieving peers, Table 7 shows that the score gain from charter middle school attendance varies inversely with peer means. For example, students who apply to

charter schools in a risk set with peer means 0.1σ below the sample mean are estimated to have an ELA gain that roughly 0.08σ higher, and a math gain about 0.1σ higher, for each year spent in a charter. None of the other peer interactions reported in Table 7 are significantly different from zero, though it should be noted that the estimate peer interactions for high school students are not very precise. It’s also worth noting that the strong negative peer interactions for middle schools do not imply that low-achieving peers raise other students scores. Rather, this result tells us something about the type of charter *school* that generates the largest gains. The most successful charter middle schools in our sample serve the most disadvantaged applicants.

VIII Observational Estimates

The lottery analysis uses a sample of applicants and schools for which lotteries are both relevant and documented. To gauge the generality of the lottery findings, we also estimated charter and pilot school attendance effects in a larger sample that includes all Boston schools. In this case, identification of causal effects is based on statistical controls for family background and earlier achievement. We refer to this analysis as an “observational study,” to distinguish it from the quasi-experimental lottery-based identification strategy. The observational results consist of OLS estimates of equations like (1), controlling for student demographics and baseline scores.

The data structure for the observational analysis is similar to that for the quasi-experimental study. Baseline scores and demographics for middle school come from 4th grade data, while baseline scores and demographics for high school come from 8th grade data. The regressors of interest count years spent attending a charter or pilot school at the relevant level (e.g., years in a charter middle school), as well as time spent in an exam or alternative school. Specifically, the observational estimates were constructed by fitting

$$y_{igt} = \alpha_t + \beta_g + \gamma'X_i + \rho_c C_{igt} + \rho_p P_{igt} + \rho_e E_{igt} + \rho_{alt} A_{igt} + \epsilon_{igt}, \quad (11)$$

where C_{igt} , P_{igt} , E_{igt} , and A_{igt} denote years in a charter, pilot, exam, or alternative school, with corresponding effects, ρ_c , ρ_p , ρ_e , and ρ_a . The sample includes students with complete demographic information, who attended Boston schools at the time they took baseline and follow-up tests.²³ The observational analysis looks at middle and high schools only as there is no baseline score data for elementary school students.

²³To parallel the exclusion of K-8 pilot school applicants in the lottery analysis with baseline scores, the observational analysis that controls for baseline scores is estimated on samples that omit students who attended elementary grades in a K-8 pilot.

The full-sample observational charter results, reported in the first two columns of Table 8, show significant positive effects for each subject. Controlling for baseline scores has little effect on the charter estimates. In models with baseline scores, each year spent at a charter middle school is estimated to increase ELA scores by about 0.12σ and to increase math scores by about 0.20σ . In contrast, the corresponding estimates for pilot middle schools are significant and negative, on the order of -0.08σ for ELA and -0.09σ for math. Charter and pilot high school effects estimated in the full sample are both significant and positive, though the charter effects are larger.

The charter middle school effects in cols 1-2 of Table 8 are smaller than those reported in Table 4. To see whether this is due to the characteristics of students in the lottery study, Table 8 reports charter estimates using lottery applicants only in columns 3-4. The charter middle school results for lottery applicants, reported in columns 3 and 4, are similar to the full-sample estimates. The middle school estimates in the smaller sample are similar to those in the full sample, though the high school estimates are smaller. Thus, the fact that the lottery analysis uses lottery applicants only does not explain the smaller observational estimates for charter schools in Table 8. It's also interesting to note that that charter estimates in this table are insensitive to baseline score controls.

The observational results for pilot schools are more sensitive to both the lottery sample restriction and the inclusion of baseline scores. Controlling for baseline scores, the pilot middle school estimates in the lottery applicant sample are strongly negative, while the high school estimates go from significant and positive in the full sample to zero when the sample is restricted to pilot lottery applicants. The latter change may be due to the fact that our sample of pilot high schools with lotteries includes only two out of six schools.

Table 9 looks at differences in effects across *schools*. Specifically, we compare the effects of time spent in lottery-study schools with the effects of time spent in other pilots and charters (the analysis sample here includes all students and not just lottery applicants). The estimates in Table 9 were constructed by replacing the terms $\rho_c C_{igt}$ and $\rho_p P_{igt}$ in equation (11) with terms that allow for separate effects for time spent in schools in and out of the set of schools in the lottery study. For example, we replace $\rho_c C_{igt}$ with $\rho_{lc} C_{ligt} + \rho_{nc} C_{nigt}$, where C_{ligt} counts years spent in charter schools included in the lottery study and C_{nigt} counts non-lottery charter years (note that $C_{igt} = C_{ligt} + C_{nigt}$). The observational estimates reported in Table 9 control for baseline scores.

A model with separate effects for time spent in lottery-study schools goes a long way towards reconciling the observational and lottery estimates, especially for charters. For example, the observational estimates of the effects of time attending a charter middle school in the lottery study are 0.17σ for ELA and 0.32σ for math. These estimates are not too far from the corresponding lottery estimates with baseline scores (0.20σ for ELA and 0.36σ for math). The high school estimates are also a good

match: compare, for example, ELA effects of about 0.26σ using both designs.

The results in Table 9 support the notion that the observational study design does a good job of controlling for selection bias in the evaluation of charter effects (or, perhaps, that there is not much selection bias in the first place). On the other hand, the table also suggests that the charter schools in our lottery study are among the best in the city. Observational estimates of the effects of time spent in charter schools not included in the lottery study, while still economically and statistically significant, are about half as large as the corresponding effects of time spent in lottery-sample schools.

The observational and lottery-based analyses of pilot middle schools both produce negative effects in the sample that includes lagged scores. The observational results for ELA are more negative than the corresponding lottery estimates, while the opposite is true for math. The match across designs is not as good for the pilot high school results, where the observational analysis for lottery schools produces substantial and significant positive effects. In contrast, the lottery results for high school ELA and math are small and not significantly different from zero (though the match for writing is good). The variance in pilot results across designs may be due to the fact that the lottery estimates for pilot high schools are not very precise. It is noteworthy, however, that estimates of pilot high school treatment effects are larger for schools used in the lottery study than for other pilot schools.

IX Summary and Conclusions

Lottery-based estimates of the impact of charter attendance on student achievement in Boston show impressive score gains in middle and high school. We also evaluate an alternative to the charter model, Boston's pilot schools. Lottery-based estimates for pilot schools are small and mostly insignificant. In an effort to gauge the generality of our lottery-based findings, we complement the quasi-experimental lottery analysis with an observational study of the full set of charter and pilot schools. This approach produces estimates similar to the lottery-based estimates for the sample of charter schools that have lotteries, lending credence to the observational analysis.

Across study designs and samples, the estimated effects of attending a pilot school are smaller than the corresponding charter estimates. Although we cannot pinpoint the source of differential impacts, a number of factors seem likely to be important. The student-teacher ratio is smaller in charter high schools and the charter school day and year are longer in both high school and middle school. Pilot schools are more like traditional Boston public schools than charter schools. These differences may originate in collective bargaining constraints that make it more expensive for pilot schools to expand instructional hours and to staff their schools with more teachers per student. In addition, most of the charter schools in our sample embrace major elements of the No Excuses model, an instructional

paradigm that is not common in public schools, pilot or otherwise.

A natural benchmark for our estimates is the black-white test score gap. Among students attending regular BPS middle schools, blacks score about 0.7 standard deviations below whites in language arts and 0.8 standard deviations below whites in math. Our estimated charter school effects are large enough to reduce the black-white reading gap in middle school by two-thirds. The even larger estimated math gains (about 0.4 standard deviations) are more than enough to eliminate the racial gap in math while students are in middle school. The effects of roughly 0.2 standard deviations estimated for high school ELA and math are also large enough to close the black-white high school gap of about 0.8 standard deviations in both subjects (assuming four years of charter high school enrollment).

The results reported here raise a number of questions. One is whether charter schools increase educational attainment as well as short-term achievement. We briefly explored charter and pilot effects on high school graduation and grade repetition, reported in Table A6. These results show no clear evidence of graduation or repetition effects, but the estimates are limited to one high school cohort. A short horizon for high school graduation is also problematic since charter schools are said to be more likely to opt for grade retention when students are struggling. In subsequent work, we hope to follow more cohorts for a longer period in an effort to track post-secondary outcomes like college matriculation and completion.

Second, the larger observational estimates of charter schools that contribute to the lottery study raise the question of which organizational or pedagogical features produce the largest achievement gains. There are too few schools in the lottery study to generate an informative comparison of specific charter models or practices. It's worth noting, however, that most of the schools in the lottery study fall under the umbrella of the "No Excuses" model. We therefore see the results reported here as particularly informative for this charter model (see also our study of a single No Excuses-style KIPP school, Angrist, *et al.*, 2010).

In conclusion, it's worth emphasizing that our lottery results reported here do not capture overall charter and pilot school effects, even for Boston. Rather, the lottery results are generated by schools that parents find most appealing and, in the case of charter schools, those that have well-documented lotteries and that continue to operate. Perhaps not surprisingly, our observational analysis suggests that the schools in the charter lottery study are among the most effective of Boston's charter schools. On the other hand, results for this relatively effective set of schools seem likely to be of considerable policy interest. Current policy discussions in Massachusetts involve proposals to expand many of the charter schools in our sample (Vaznis 2010). It seems likely that over-subscribed and well-run schools will be the first to expand if caps are lifted. State regulators might also reasonably choose to encourage replication of schools that have documented effectiveness and high demand.

Table 1: Teacher Characteristics by School Type

	Traditional BPS	Pilot, Charter, Exam or Alternative School			Lottery Sample		
	Schools (1)	Charter (2)	Pilot (3)	Exam (4)	Alternative (5)	Charter (7)	Pilot (8)
<i>I. Elementary School (3rd and 4th grades)</i>							
Teachers licensed to teach assignment	96.0%	67.3%	93.3%	-	86.6%	-	92.3%
Core academic teachers identified as highly qualified	95.9%	81.4%	91.7%	-	74.9%	-	90.6%
Total number of teachers in core academic areas	25.8	77.1	29.6	-	46.5	-	30.0
Student/Teacher ratio	13.3	11.8	12.2	-	5.2	-	12.2
Proportion of teachers 32 and younger	26.4%	63.9%	53.9%	-	27.2%	-	50.7%
Proportion of teachers 49 and older	39.9%	8.2%	12.2%	-	30.8%	-	11.0%
Number of teachers	32.2	87.3	25.5	-	50.8	-	50.8
Number of schools	73	3	7	-	2	-	2
<i>II. Middle School (6th, 7th, and 8th grades)</i>							
Teachers licensed to teach assignment	88.1%	50.9%	78.5%	96.0%	82.6%	52.2%	78.5%
Core academic teachers identified as highly qualified	88.0%	79.2%	79.1%	94.8%	75.2%	86.2%	79.1%
Total number of teachers in core academic areas	39.0	34.3	31.2	75.7	40.7	20.3	31.2
Student/Teacher ratio	12.3	11.7	14.7	21.3	5.9	11.6	14.7
Proportion of teachers 32 and younger	27.8%	76.1%	56.2%	30.0%	28.0%	82.2%	56.2%
Proportion of teachers 49 and older	35.8%	4.3%	13.1%	43.1%	28.9%	1.3%	13.1%
Number of teachers	47.6	37.7	38.0	88.4	56.8	24.9	38.0
Number of schools	29	12	7	3	3	5	7
<i>III. High School (10th grade)</i>							
Teachers licensed to teach assignment	88.8%	56.7%	84.7%	96.2%	86.5%	57.8%	90.5%
Core academic teachers identified as highly qualified	87.6%	85.5%	83.2%	94.7%	75.1%	87.5%	89.6%
Total number of teachers in core academic areas	58.9	19.3	24.9	75.8	36.9	16.5	17.9
Student/Teacher ratio	14.6	12.0	15.8	21.2	6.7	12.8	14.5
Proportion of teachers 32 and younger	32.4%	67.7%	44.3%	30.0%	31.4%	71.6%	41.4%
Proportion of teachers 49 and older	39.5%	6.2%	15.1%	44.1%	22.8%	4.3%	7.7%
Number of teachers	75.1	21.3	27.5	88.4	46.9	16.7	20.3
Number of schools	22	8	7	3	4	3	2

Notes: This table reports student weighted average characteristics of teachers and school using data posted 2004-2009 posted on the Mass DOE website at http://profiles.doe.mass.edu/state_report/teacherdata.aspx. Age data is only available for 2008 and 2009. Teachers licensed in teaching assignment is the percent of teachers who are licensed with Provisional, Initial, or Professional licensure to teach in the area(s) in which they are teaching. Core classes taught by highly qualified teachers is the percent of core academic classes (defined as English, reading or language arts, mathematics, science, foreign languages, civics and government, economics, arts, history, and geography) taught by highly qualified teachers (defined as teachers not only holding a Massachusetts teaching license, but also demonstrating subject matter competency in the areas they teach). For more information on the definition and requirements of highly qualified teachers, see http://www.doe.mass.edu/nclb/hq/hq_memo.html.

Table 2: Descriptive Statistics

	Traditional BPS Schools (1)	Enrolled in Pilot or Charter		Applicants in Lottery Sample		Applicants in Lottery Sample with Baseline Scores	
		Charter (2)	Pilot (3)	Charter (4)	Pilot (5)	Charter (6)	Pilot (7)
<i>I. Elementary School (3rd and 4th grades)</i>							
Female	48.5%	52.9%	48.1%	-	50.2%	-	-
Black	41.5%	70.7%	40.8%	-	54.0%	-	-
Hispanic	35.7%	17.2%	35.0%	-	22.8%	-	-
Special education	10.2%	5.5%	10.7%	-	9.9%	-	-
Free or reduced price lunch	82.1%	69.8%	69.4%	-	65.9%	-	-
Limited English proficiency	27.4%	4.5%	19.2%	-	6.9%	-	-
Years in charter	0.017	4.598	0.011	-	0.225	-	-
Years in pilot	0.036	0.027	4.026	-	1.982	-	-
Number of students	13211	892	1091	-	596	-	-
Number of schools	74	4	7	-	5	-	-
<i>II. Middle School (6th, 7th, and 8th grades)</i>							
Female	47.0%	49.2%	49.7%	48.3%	52.4%	48.3%	55.3%
Black	46.3%	67.4%	49.4%	58.9%	49.2%	58.8%	50.1%
Hispanic	37.6%	20.7%	29.2%	19.3%	30.8%	19.4%	34.8%
Special education	24.7%	18.1%	22.0%	19.0%	17.9%	18.9%	18.2%
Free or reduced price lunch	88.4%	73.2%	84.9%	68.0%	78.0%	68.2%	87.1%
Limited English proficiency	20.0%	7.7%	20.4%	6.8%	13.0%	6.8%	15.8%
4th Grade ELA Score	-0.863	-0.620	-0.889	-0.418	-0.726	-0.418	-0.726
4th Grade Math Score	-0.761	-0.689	-0.845	-0.407	-0.691	-0.407	-0.691
Years in charter	0.022	2.535	0.018	1.791	0.268	1.785	0.272
Years in pilot	0.025	0.040	2.251	0.130	1.073	0.128	1.223
Number of students	14082	2865	3163	1386	1973	1363	1324
Number of schools	34	12	7	5	7	5	6
<i>III. High School (10th grade)</i>							
Female	49.7%	59.9%	51.6%	54.4%	44.8%	54.3%	44.9%
Black	50.6%	66.0%	53.4%	65.6%	58.0%	65.6%	57.8%
Hispanic	36.1%	15.2%	26.6%	24.1%	24.7%	24.0%	24.8%
Special education	22.9%	15.0%	17.4%	15.8%	11.9%	15.5%	11.9%
Free or reduced price lunch	84.0%	65.7%	75.8%	74.3%	77.6%	74.6%	78.1%
Limited English proficiency	15.3%	2.7%	4.6%	2.6%	4.3%	2.6%	4.4%
8th Grade ELA Score	-0.728	-0.255	-0.349	-0.293	-0.283	-0.293	-0.283
8th Grade Math Score	-0.722	-0.286	-0.367	-0.303	-0.270	-0.303	-0.270
Years in charter	0.015	2.017	0.023	0.657	0.262	0.655	0.264
Years in pilot	0.006	0.010	1.937	0.546	0.957	0.547	0.954
Number of students	9489	1149	1984	1484	1038	1474	1032
Number of schools	22	8	7	3	2	3	2

Notes: This table reports sample means in baseline years by school type in each column. The numbered notes below describe each sample. Demographic characteristics are taken from grade K for elementary school students, grade 4 for middle school students, and grade 8 for high school students. All students reside in Boston and must be enrolled in BPS or a charter school in the baseline year. Students must have at least one MCAS score to be included in the table. The test scores counted include ELA and Math for elementary and middle school students, and ELA, Math, Writing Topic, and Writing Composition for high school students.

1. BPS students excluding exam, alternative, charter and pilot students from 2004-2009.
2. Students enrolled in charter schools from 2004-2009.
3. Students enrolled in pilot schools from 2004-2009.
4. Charter applicant cohorts in randomized lotteries: middle school students in 2002-2007, and high school students in 2003-2006.
5. Pilot applicant cohorts: elementary school students in 2002-2004, middle school students in 2002-2007, and high school students in 2003-2006.

Table 3: Covariate Balance at Charter and Pilot Schools

	Charter Schools				Pilot Schools				
	Middle School		High School		School	Middle School		High School	
	All Lotteries	Lotteries with Baseline Scores	All Lotteries	Lotteries with Baseline Scores	All Lotteries	All Lotteries	Lotteries with Baseline Scores	All Lotteries	Lotteries with Baseline Scores
	(1)	(2)	(3)	(4)	(5)	(7)	(8)	(9)	(10)
Hispanic	0.005 (0.024)	0.000 (0.024)	-0.046* (0.026)	-0.047* (0.027)	-0.032 (0.038)	-0.026 (0.026)	-0.027 (0.038)	0.025 (0.028)	0.012 (0.029)
Black	-0.022 (0.030)	-0.018 (0.031)	0.058** (0.029)	0.057* (0.030)	0.016 (0.042)	-0.001 (0.028)	0.007 (0.040)	-0.009 (0.031)	0.005 (0.032)
White	0.012 (0.024)	0.013 (0.024)	-0.021 (0.014)	-0.020 (0.015)	0.028 (0.036)	0.029 (0.020)	0.024 (0.021)	-0.015 (0.017)	-0.010 (0.018)
Asian	0.003 (0.008)	0.004 (0.008)	0.000 (0.011)	-0.002 (0.011)	-0.031* (0.018)	0.004 (0.015)	-0.001 (0.020)	-0.003 (0.015)	-0.009 (0.016)
Female	0.024 (0.032)	0.027 (0.032)	-0.005 (0.030)	-0.013 (0.032)	0.013 (0.049)	0.025 (0.032)	0.025 (0.042)	0.015 (0.031)	0.010 (0.033)
Free or Reduced Price Lunch	0.001 (0.029)	0.000 (0.030)	0.011 (0.026)	0.006 (0.027)	-0.072* (0.042)	-0.009 (0.024)	-0.015 (0.029)	0.058** (0.026)	0.068** (0.027)
Special Education	-0.029 (0.025)	-0.028 (0.026)	-0.002 (0.023)	0.001 (0.024)	-0.025 (0.026)	-0.003 (0.021)	0.010 (0.034)	-0.031 (0.022)	-0.019 (0.024)
Limited English Proficiency	0.019 (0.015)	0.019 (0.015)	0.009 (0.010)	0.009 (0.009)	-0.026 (0.024)	-0.031** (0.016)	-0.043* (0.026)	0.020 (0.014)	0.000 (0.011)
Baseline ELA Test Score	-	0.052 (0.063)	-	-0.042 (0.050)	-	-	0.044 (0.089)	-	0.013 (0.057)
Baseline Math Test Score	-	0.086 (0.061)	-	-0.009 (0.056)	-	-	0.081 (0.084)	-	-0.078 (0.058)
Baseline Writing Composition Test Score	-	-	-	0.031 (0.052)	-	-	-	-	0.034 (0.057)
Baseline Writing Topic Test Score	-	-	-	-0.062 (0.053)	-	-	-	-	0.010 (0.057)
p-value, from F-test	0.853	0.824	0.400	0.270	0.182	0.459	0.735	0.301	0.580
N	1521	1485	1849	1723	670	2172	1377	1301	1188

Notes: This table reports coefficients on regressions of the variable indicated in each row on an indicator variable equal to one if the student ever received an offer. Charter regressions include dummies for (combination of schools applied to)*(year of application) and exclude students with sibling priority. Pilot regressions include dummies for (first choice)*(year of application)*(walk zone) and exclude students with sibling priority or guaranteed admission. Samples are restricted to students from cohorts where we should observe at least one test score. Samples in columns (2), (4), (6), (8), and (10) are restricted to students who also have baseline test scores. Robust F-tests are for the null hypothesis that the coefficients on winning the lottery in all regressions are all equal to zero. These tests 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 4A: Lottery Results Using Ever Offer

Level	Subject	Charter Schools					Pilot Schools				
		Basic controls		2SLS w/demographics (4)	2SLS w/demographics and baseline (5)	Basic controls		2SLS w/demographics (9)	2SLS w/demographics and baseline (10)		
		First Stage (1)	Reduced Form (2)			First Stage (6)	Reduced Form (7)			2SLS (8)	
Elementary School	ELA	-	-	-	-	-	2.945*** (0.189)	0.209** (0.084)	0.071** (0.028)	0.062** (0.026)	-
	N							1141	1141		
	Math	-	-	-	-	-	2.950*** (0.190)	0.110 (0.085)	0.037 (0.029)	0.033 (0.028)	-
	N							1139	1139		
Middle School	ELA	1.000*** (0.099)	0.253*** (0.066)	0.253*** (0.067)	0.203*** (0.056)	0.198*** (0.047)	1.526*** (0.172)	0.022 (0.065)	0.014 (0.042)	0.010 (0.040)	-0.041 (0.103)
	N		3157		3157			4314		4314	3024
	Math	0.967*** (0.094)	0.401*** (0.065)	0.415*** (0.067)	0.376*** (0.059)	0.359*** (0.048)	1.450*** (0.167)	-0.065 (0.064)	-0.045 (0.044)	-0.041 (0.041)	-0.223** (0.090)
	N		3317		3317			4777		4777	3348
High School	ELA	0.550*** (0.154)	0.089 (0.072)	0.162 (0.110)	0.178* (0.098)	0.265*** (0.076)	0.683*** (0.107)	0.005 (0.055)	0.007 (0.079)	-0.011 (0.075)	-0.058 (0.062)
	N		1473		1473			1034		1034	978
	Math	0.543*** (0.152)	0.190** (0.084)	0.350*** (0.118)	0.368*** (0.114)	0.364*** (0.085)	0.680*** (0.107)	0.007 (0.069)	0.010 (0.099)	-0.033 (0.101)	0.021 (0.067)
	N		1455		1455			1022		1022	1009
	Writing Topic	0.551*** (0.155)	0.167* (0.089)	0.303** (0.128)	0.319*** (0.120)	0.349*** (0.120)	0.674*** (0.108)	0.126** (0.058)	0.186** (0.086)	0.170** (0.085)	0.158* (0.081)
	N		1461		1461			1023		1023	961
	Writing Composition	0.551*** (0.155)	0.106 (0.075)	0.192 (0.116)	0.214* (0.115)	0.212** (0.104)	0.674*** (0.108)	0.108* (0.063)	0.161* (0.094)	0.150 (0.093)	0.138 (0.085)
	N		1461		1461			1023		1023	961

Notes: This table reports the coefficients from regressions using years spent in charter or pilot schools. The instrument is an indicator for ever receiving an offer. The sample is restricted to students with baseline demographic characteristics. Regressions include year of test and year of birth dummies. Middle school and elementary school regressions pool grade outcomes and include dummies for grade level. Charter regressions include dummies for (combination of schools applied to)*(year of application) and exclude students with sibling priority. Pilot regressions include dummies for (first choice)*(year of application)*(walk zone status) and exclude students with sibling priority or guaranteed admission. Columns (4) and (9) report 2SLS coefficients from specifications that add demographic controls, which include dummies for female, black, hispanic, asian, other race, special education, limited english proficiency, free/reduced price lunch, and a female*minority interaction. Columns (5) and (10) add controls for baseline test scores. Tests are given in 3rd and 4th grade for elementary school, 6th, 7th, and 8th grade for middle school, and 10th grade for high school. Regressions use robust standard errors and are clustered on year by 10th grade school for high school and student identifier as well as school by grade by year for pooled regressions.

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

Table 4B: Charter Lottery Results Using Initial Offer

		Charter Schools				
		Basic controls				
Level	Subject	First Stage (1)	Reduced Form (2)	2SLS (3)	2SLS w/demographics (4)	2SLS w/demographics + baseline (5)
Middle School	ELA	0.690*** (0.084)	0.209*** (0.060)	0.302*** (0.088)	0.205*** (0.074)	0.208*** (0.066)
		N	2660		2660	2612
	Math	0.684*** (0.083)	0.298*** (0.061)	0.436*** (0.090)	0.367*** (0.079)	0.250*** (0.059)
		N	2736		2736	2689
High School	ELA	0.399*** (0.111)	0.046 (0.055)	0.116 (0.129)	0.179 (0.121)	0.321*** (0.113)
		N	1473		1473	1401
	Math	0.395*** (0.111)	0.106 (0.067)	0.268* (0.160)	0.341** (0.153)	0.436*** (0.137)
		N	1455		1455	1432
	Writing Topic	0.398*** (0.111)	0.090 (0.063)	0.227 (0.146)	0.268* (0.145)	0.329** (0.162)
		N	1461		1461	1386
	Writing Composition	0.398*** (0.111)	-0.004 (0.061)	-0.009 (0.151)	0.040 (0.148)	0.024 (0.148)
		N	1461		1461	1386

Notes: This table reports 2SLS regressions using years spent in charter schools with initial offer as the instrument. All other notes are the same as Table 4A.

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

Table 4C: Lottery Results with Potential Years Instrument

Level	Subject	Charter Schools					Pilot Schools				
		Basic controls		2SLS w/demographics (3)	2SLS w/demographics and baseline (5)	Basic controls		2SLS w/demographics (9)	2SLS w/demographics and baselines (10)		
		First Stage (1)	Reduced Form (2)			First Stage (6)	Reduced Form (7)				
Elementary School	ELA	-	-	-	-	0.632*** (0.041)	0.043** (0.018)	0.068** (0.028)	0.058** (0.026)	-	
	N						1141		1141		
	Math	-	-	-	-	0.633*** (0.041)	0.019 (0.019)	0.031 (0.029)	0.025 (0.029)	-	
	N						1139		1139		
Middle School	ELA	0.415*** (0.041)	0.090*** (0.025)	0.217*** (0.058)	0.171*** (0.048)	0.181*** (0.042)	0.408*** (0.038)	0.012 (0.012)	0.028 (0.029)	0.030 (0.026)	-0.033 (0.074)
	N		3157		3157		3101		4314		4314
	Math	0.415*** (0.041)	0.142*** (0.025)	0.342*** (0.058)	0.309*** (0.051)	0.312*** (0.043)	0.400*** (0.039)	0.002 (0.011)	0.004 (0.028)	0.010 (0.025)	-0.127** (0.057)
	N		3317		3317		3258		4777		4777
High School	ELA	0.257*** (0.071)	0.038 (0.033)	0.146 (0.109)	0.160 (0.102)	0.264*** (0.081)	0.325*** (0.051)	0.000 (0.027)	0.001 (0.080)	-0.013 (0.078)	-0.061 (0.065)
	N		1473		1473		1401		1034		1034
	Math	0.253*** (0.071)	0.088** (0.038)	0.349*** (0.120)	0.366*** (0.118)	0.354*** (0.086)	0.327*** (0.051)	0.003 (0.033)	0.011 (0.100)	-0.029 (0.102)	0.016 (0.069)
	N		1455		1455		1432		1022		1022
	Writing Topic	0.258*** (0.072)	0.083** (0.042)	0.321** (0.131)	0.331*** (0.125)	0.370*** (0.125)	0.320*** (0.051)	0.061** (0.027)	0.190** (0.087)	0.176** (0.086)	0.161** (0.081)
	N		1461		1461		1386		1023		1023
	Writing Composition	0.258*** (0.072)	0.050 (0.035)	0.193 (0.119)	0.209* (0.120)	0.216** (0.108)	0.320*** (0.051)	0.056* (0.030)	0.175* (0.096)	0.167* (0.096)	0.157* (0.087)
	N		1461		1461		1386		1023		1023

Notes: This table reports the coefficients on regressions using years spent in charter or pilot schools. The instrument is potential years in the relevant school type interacted with the lottery ever offer dummy. Potential years is calculated as the number of years a student could have spent at school after the lottery and prior to the relevant test. All other notes are the same as Table 4A.

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

Table 5: Attrition

Level	Subject	Charter Schools			Pilot Schools		
		Prop of non-offered with MCAS	Differential		Prop of non-offered with MCAS	Differential	
			Demographics	Demographics + Baseline Scores		Demographics	Demographics + Baseline Scores
(1)	(2)	(3)	(4)	(5)	(6)		
Elementary School							
	ELA	-	-	-	0.834	0.058* (0.033)	-
		N				1340	-
	Math	-	-	-	0.834	0.062* (0.034)	-
		N				1340	
Middle School							
	ELA	0.814	0.035* (0.020)	0.030 (0.020)	0.801	0.014 (0.021)	0.010 (0.023)
		N	3766	3693		4778	3084
	Math	0.816	0.036* (0.020)	0.034* (0.020)	0.790	0.012 (0.020)	0.006 (0.024)
		N	3934	3852		5846	3788
High School							
	ELA	0.785	0.001 (0.026)	-0.002 (0.026)	0.775	0.039 (0.025)	0.061** (0.024)
		N	1849	1752		1301	1215
	Math	0.778	0.000 (0.026)	-0.001 (0.026)	0.765	0.034 (0.025)	0.046* (0.025)
		N	1849	1813		1301	1273
	Writing Topic and Writing Composition	0.778	0.000 (0.026)	-0.001 (0.027)	0.765	0.034 (0.025)	0.054** (0.026)
		N	1849	1744		1301	1205

Notes: This table reports coefficients on regressions of an indicator variable equal to one if the outcome test score is non-missing on an indicator variable equal to one if the student ever received an offer. Regressions in column (2) and (3) include dummies for (combination of schools applied to)*(year of application) as well as demographic variables, year of birth dummies, and year of baseline dummies. Columns (5) and (6) control for (first choice)*(year of application)*(walk zone) dummies, demographics, year of birth dummies and year of baseline dummies. Regressions in columns (3) and (6) also add baseline test scores. Middle school and elementary school regressions pool grades and include grade dummies. Standard errors are clustered at the student level. 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 must also take Writing Composition.

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

Table 6: School Switching Regressions

Level	Outcome	Charter Schools		Pilot Schools	
		Demographics (1)	Demographics and baseline scores (2)	Demographics (3)	Demographics and baseline scores (4)
Middle school	Any switch	-0.146*** (0.032) 1378	-0.145*** (0.032) 1347	-0.056 (0.040) 1347	-0.045 (0.040) 1300
	Any switch excluding 5-6 transition and exam schools	-0.018 (0.029) 1378	-0.011 (0.029) 1347	-0.056 (0.040) 1347	-0.045 (0.040) 1300
Mean switch rate		0.471	0.472	0.313	0.314
High school	Any switch	0.063** (0.028) 1561	0.052* (0.028) 1466	-0.032 (0.025) 1119	-0.042* (0.025) 1042
	Mean switch rate	0.231	0.231	0.164	0.161

Notes: This table reports coefficients from regressions of an indicator variable equal to one if a student switched schools on an indicator variable equal to one if the student won the lottery. The "any switch" variable is equal to one if a student ever switched from one observed school to another after the lottery, either within a school year or between school years. The second middle school row excludes switches between 5th and 6th grade for 5th grade charter applicants, as well as switches to exam schools in 7th grade for all applicants (these schools start in 7th). Robust standard errors are reported in parentheses.

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

Table 7: Interaction Models

Level	Subject	Own baseline score				Peer mean baseline score			
		Charter Schools		Pilot Schools		Charter Schools		Pilot Schools	
		main effect	interaction	main effect	interaction	main effect	interaction	main effect	interaction
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Middle School									
	ELA	0.203*** (0.046)	-0.123*** (0.046)	-0.042 (0.102)	-0.035 (0.044)	0.226*** (0.047)	-0.810*** (0.241)	-0.031 (0.116)	-0.107 (0.307)
		N	3,101		3,024		3,090		3,024
	Math	0.368*** (0.046)	-0.146*** (0.051)	-0.220** (0.091)	-0.023 (0.034)	0.383*** (0.048)	-1.035*** (0.213)	-0.284** (0.111)	0.358 (0.229)
		N	3,258		3,348		3,247		3,348
High School									
	ELA	0.365*** (0.084)	0.021 (0.086)	0.014 (0.066)	-0.081 (0.049)	0.355*** (0.088)	0.155 (0.322)	0.032 (0.068)	0.571 (0.633)
		N	1,432		1,009		1,432		1,009
	Math	0.268*** (0.077)	0.039 (0.116)	-0.059 (0.063)	-0.009 (0.089)	0.322*** (0.107)	-0.826 (0.809)	-0.044 (0.065)	0.628 (0.620)
		N	1,401		978		1,401		978
	Writing Topic	0.351*** (0.120)	0.023 (0.084)	0.148* (0.084)	-0.068 (0.079)	0.301* (0.154)	0.690 (0.931)	0.162* (0.084)	0.094 (0.612)
		N	1,386		961		1,386		961
	Writing Composition	0.218** (0.103)	0.034 (0.083)	0.136 (0.086)	-0.043 (0.090)	0.214* (0.127)	-0.040 (0.825)	0.153* (0.091)	0.562 (0.605)
		N	1,386		961		1,386		961

Notes: This table shows results analogous to those reported in the 2SLS lottery results in Table 4A, but specifications now include interaction terms. Columns (1)-(4) interact years in charter or pilot schools with a student's own baseline test score, and add baseline score interacted with the offer dummy to the instruments. Columns (5)-(8) interact years in charter or pilot schools with the baseline mean of the members of a student's risk set, and add baseline score in the risk set interacted with the offer dummy to the instruments. The interacting variables are de-meant so that the main effects are evaluated at the mean, and all specifications include main effects of the interacting variable. Controls include year of birth dummies, year of test dummies, demographic controls, and baseline test scores. Middle school regressions pool grade outcomes and include dummies for grade level. Regressions use robust standard errors and are clustered on year by 10th grade school for high school and student identifier as well as school by grade by year for middle school.

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

Table 8: Observational Analysis for Charter and Pilot Schools

Level	Subject	Charter Schools				Pilot Schools			
		All students		Lottery applicants		All students		Lottery applicants	
		Demographics (1)	Demographics and baseline (2)	Demographics (3)	Demographics and baseline (4)	Demographics (5)	Demographics and baseline (6)	Demographics (7)	Demographics and baseline (8)
Middle School									
	ELA ¹	0.139*** (0.014)	0.122*** (0.012)	0.123*** (0.025)	0.153*** (0.022)	-0.076*** (0.015)	-0.086*** (0.012)	-0.049*** (0.014)	-0.129*** (0.020)
	N	44077	40852	2920	2876	44077	40852	3821	2835
	Math ²	0.199*** (0.018)	0.201*** (0.016)	0.215*** (0.028)	0.241*** (0.023)	-0.093*** (0.016)	-0.093*** (0.013)	-0.075*** (0.014)	-0.191*** (0.023)
	N	48357	45035	3076	3028	48357	45035	4261	3142
High School									
	ELA ³	0.285*** (0.022)	0.202*** (0.020)	0.102*** (0.034)	0.139*** (0.030)	0.178*** (0.019)	0.110*** (0.016)	-0.012 (0.029)	-0.016 (0.025)
	N	20111	15610	1352	1286	20111	15610	949	898
	Math ⁴	0.309*** (0.041)	0.178*** (0.033)	0.107* (0.060)	0.166*** (0.049)	0.154*** (0.027)	0.070*** (0.023)	-0.031 (0.042)	-0.001 (0.028)
	N	19800	19254	1336	1315	19800	19254	937	926
	Writing Topic ⁵	0.254*** (0.026)	0.225*** (0.028)	0.147*** (0.041)	0.164*** (0.042)	0.158*** (0.021)	0.142*** (0.020)	0.023 (0.033)	0.018 (0.034)
	N	19673	15328	1341	1273	19673	15328	938	882
	Writing Composition ⁶	0.278*** (0.025)	0.236*** (0.025)	0.124*** (0.035)	0.148*** (0.033)	0.177*** (0.021)	0.152*** (0.020)	0.045 (0.038)	0.032 (0.035)
	N	19673	15328	1341	1273	19673	15328	938	882

Notes: This table reports coefficients from regressions using years spent in different types of schools. The excluded category is traditional BPS schools. Coefficients are estimated for years spent in pilot schools, charter schools, exam schools, and alternative schools. For a given school level and test, column pairs (1) and (5), (2) and (6), (3) and (7), and (4) and (8) report coefficients from the same regressions. The samples are restricted to students with baseline demographic characteristics in Boston at the time of the test. Demographics include female, black, hispanic, asian, other race, special education, limited english proficiency, free/reduced price lunch, and a female*minority dummy. Regressions also include year of test and year of birth dummies. Middle school and elementary school regressions pool grade outcomes and include dummies for grade level. Regressions use robust standard errors and are clustered on year by 10th grade school for high school and student identifier as well as school by grade by year for pooled regressions.

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

1. Middle school ELA is for Grade 6 (2005-09), Grade 7 (2005-09), and Grade 8 (2005-09).
2. Middle school Math is for Grade 6 (2004-09), Grade 7 (2005-09), and Grade 8 (2006-09).
3. High school ELA is for Grade 10 (2004-09).
4. High school Math is for Grade 10 (2004-09).
5. High school Writing Topic is for Grade 10 (2004-09).
6. High school Writing Composition is for Grade 10 (2004-09).

Table 9: Estimates in and Out of Lottery Schools

Level	Subject	Charter Schools				Pilot Schools			
		Lottery		Observational		Lottery		Observational	
		With Demographics (1)	With Baseline Scores (2)	Lottery Schools (3)	Non-lottery Schools (4)	With Demographics (5)	With Baseline Scores (6)	Lottery Schools (7)	Non-lottery Schools (8)
Middle School									
	ELA	0.203*** (0.056)	0.198*** (0.047)	0.174*** (0.020)	0.098*** (0.014)	0.010 (0.040)	-0.041 (0.103)	-0.089*** (0.015)	-0.082*** (0.016)
		N 3157	3101		40852	4314	3024		40852
	Math	0.376*** (0.059)	0.359*** (0.048)	0.316*** (0.024)	0.148*** (0.018)	-0.041 (0.041)	-0.223** (0.090)	-0.108*** (0.014)	-0.071*** (0.019)
		N 3317	3258		45035	4777	3348		45035
High School									
	ELA	0.178* (0.098)	0.265*** (0.076)	0.258*** (0.025)	0.169*** (0.023)	-0.011 (0.075)	-0.058 (0.062)	0.167*** (0.016)	0.089*** (0.016)
		N 1473	1401		15610	1034	978		15610
	Math	0.368*** (0.114)	0.364*** (0.085)	0.269*** (0.055)	0.122*** (0.033)	-0.033 (0.101)	0.021 (0.067)	0.167*** (0.033)	0.038 (0.024)
		N 1455	1432		19254	1022	1009		19254
	Writing Topic	0.319*** (0.120)	0.349*** (0.120)	0.311*** (0.044)	0.174*** (0.028)	0.170** (0.085)	0.158* (0.081)	0.235*** (0.017)	0.108*** (0.021)
		N 1461	1386		15328	1023	961		15328
	Writing Composition	0.214* (0.115)	0.212** (0.104)	0.309*** (0.034)	0.193*** (0.027)	0.150 (0.093)	0.138 (0.085)	0.218*** (0.028)	0.128*** (0.021)
		N 1461	1386		15328	1023	961		15328

Notes: Columns (1), (2), (5) and (6) report 2SLS coefficients from Table 4A. Observational models are estimated by OLS and include separate variables for years in lottery sample pilot schools, lottery sample charter schools, non-lottery sample pilot schools, and non-lottery sample charter schools. For a given school level and test, columns (3), (4), (7), and (8) report coefficient estimates from the same regression. Demographics include female, black, hispanic, asian, other race, special education, limited english proficiency, free/reduced price lunch, and a female*minority dummy. Regressions also include year of test and year of birth dummies as well as baseline scores. Observational models restrict the sample to students who were in Boston in the year of the relevant test. Middle school regressions pool grade outcomes and include dummies for grade level. Regressions use robust standard errors and are clustered on year by 10th grade school for high school and student identifier as well as school by grade by year for middle school.

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

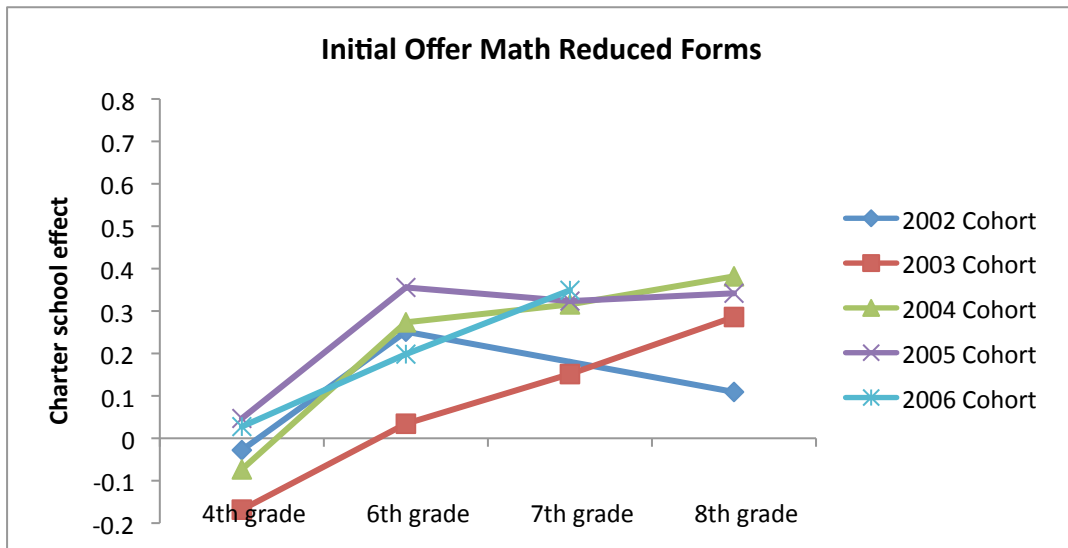
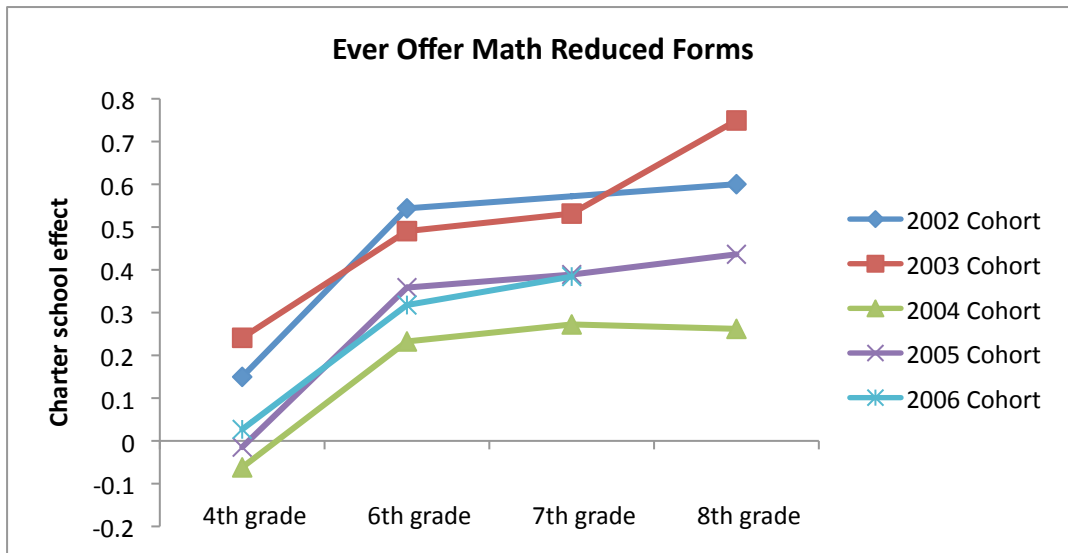


Figure 1. Charter Math Reduced Forms

These figures plot reduced form initial and ever offer math coefficients by grade and 4th grade cohort for charter applicants. All coefficients for a given plot come from a single regression that uses interactions of cohort, grade and the offer variable. Regressions include risk set and demographic controls. The points for 7th grade in the 2002 cohort are interpolated, as no 7th grade math test was given for this cohort.

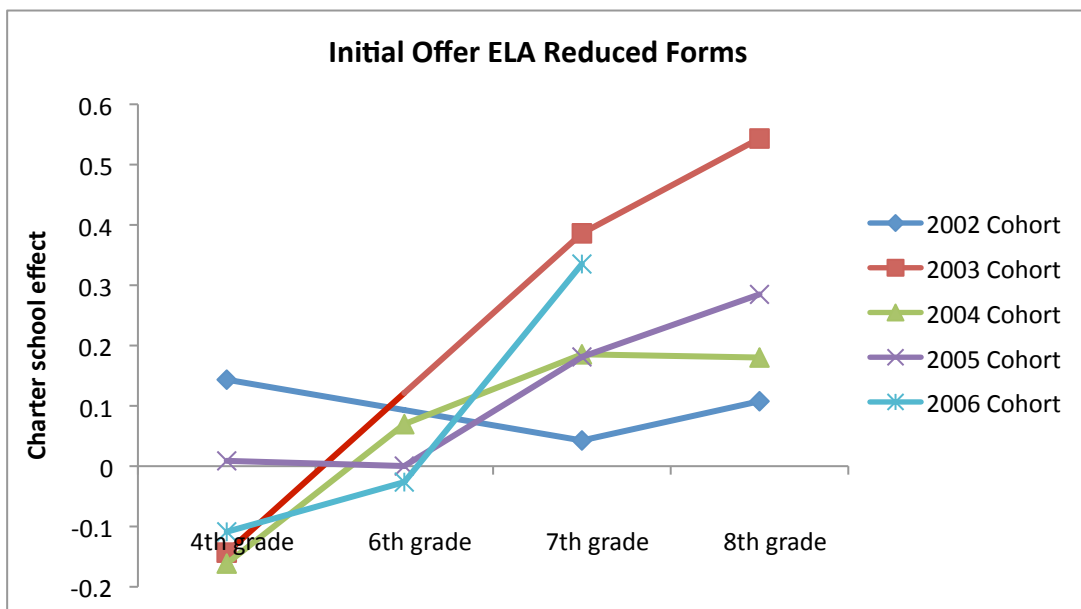
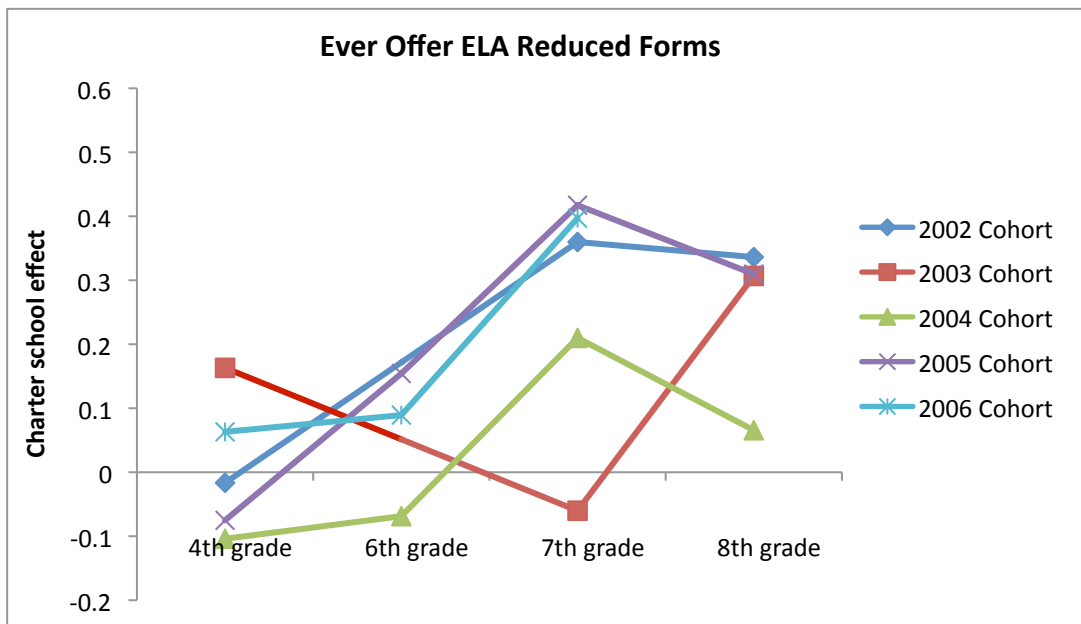


Figure 2. Charter ELA Reduced Forms

These figures plot reduced form initial and ever offer ELA coefficients by grade and 4th grade cohort for charter applicants. All coefficients for a given plot come from a single regression that uses interactions of cohort, grade and the offer variable. Regressions include risk set and demographic controls. The points for 6th grade in the 2002 and 2003 cohorts are interpolated, as 6th grade ELA tests were not given for these cohorts.

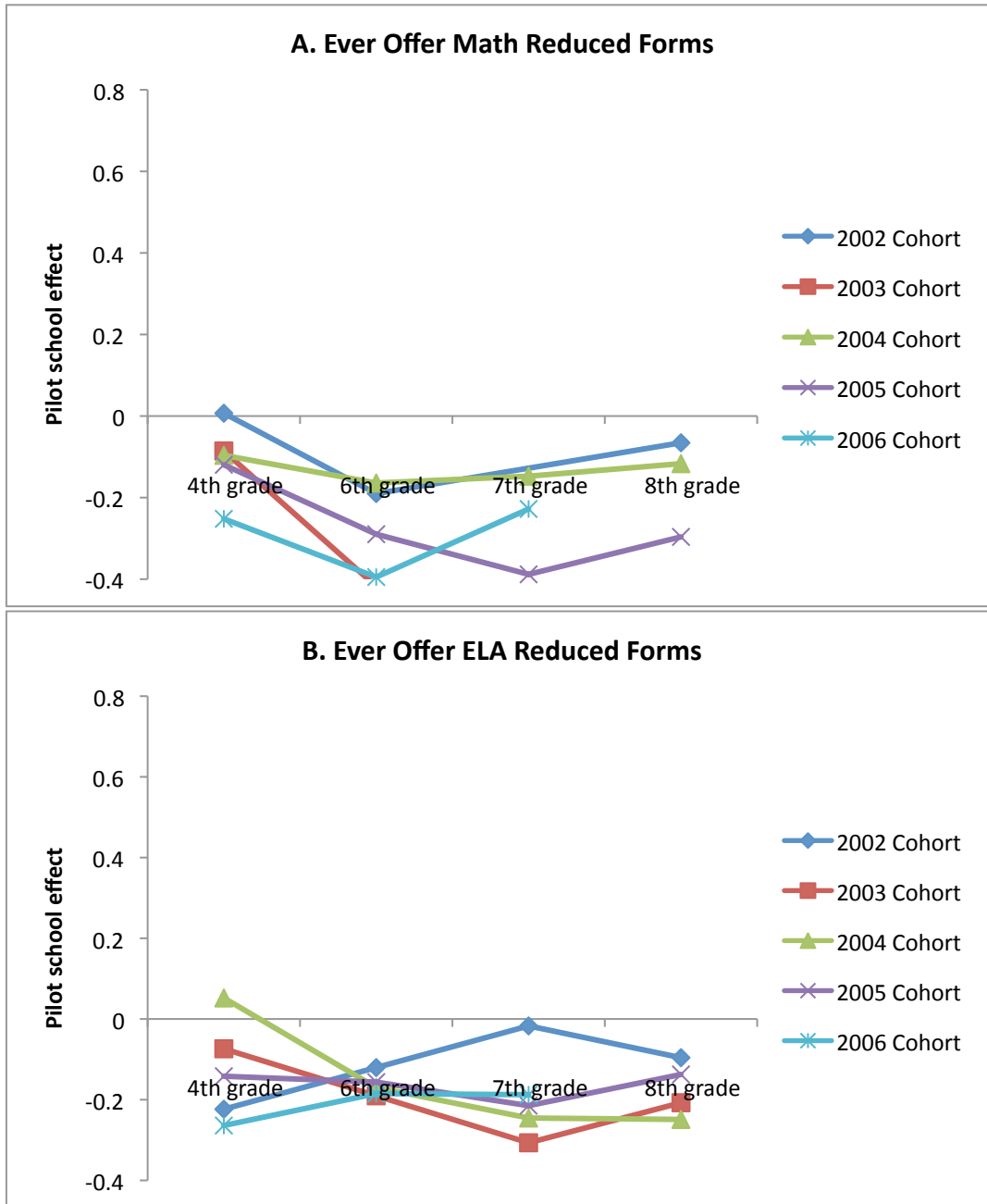
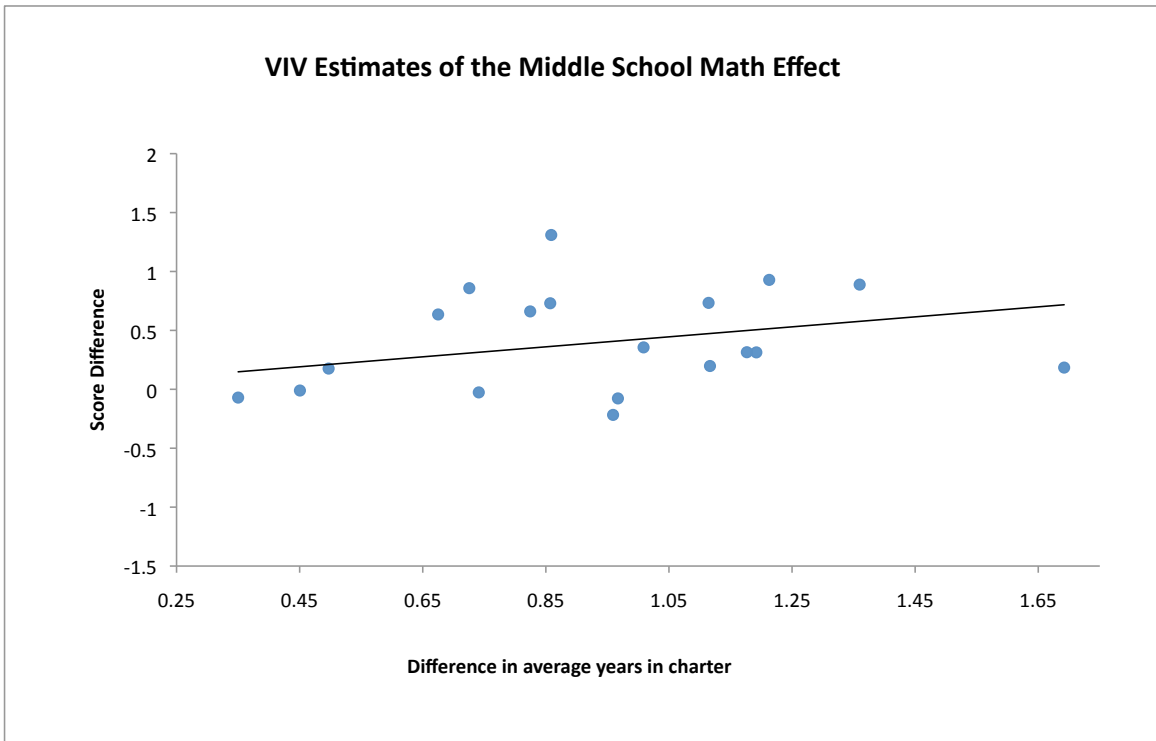
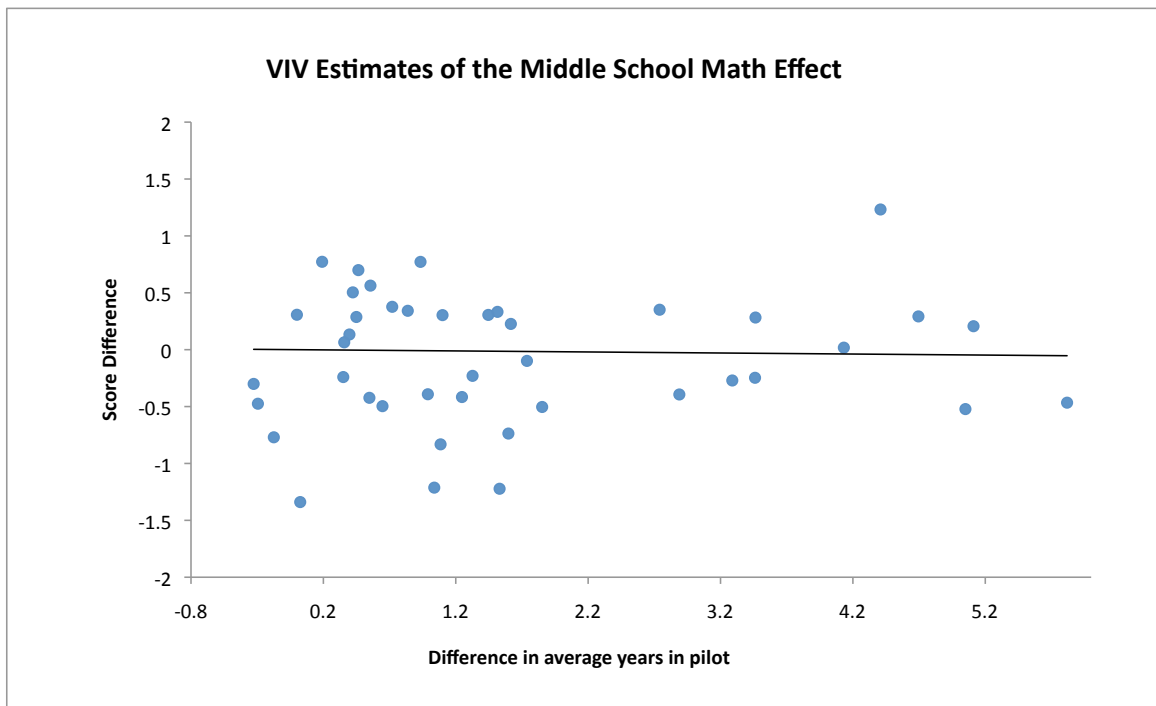


Figure 3. Pilot Reduced Forms

These figures plot reduced form math and ELA coefficients by grade and 4th grade cohort for pilot 6th grade applicants. All coefficients for a given plot come from a single regression that uses interactions of cohort, grade and the offer variable. Regressions include risk set and demographic controls. The point for 7th grade in the 2002 cohort is interpolated, as no 7th grade math test was given for this cohort.



A. Charter Schools



B. Pilot Schools

Figure 4. VIV Estimates

This figure plots treatment-control differences in test score means against treatment-control differences in years in charter (Panel A) or pilot (Panel B). The unit of observation is a charter or pilot application risk set (N=19 for charters and N=42 for pilots). The charter slope (unweighted) is .458, and the corresponding 2SLS estimate is .446. The pilot slope (unweighted) is -.009, while the corresponding 2SLS estimate is -.007. The charter graph is produced after dropping two risk sets with less than five students in either treatment or control; the sample size-weighted average of these risk sets' values is shown. The pilot graph is produced after dropping ten such risk sets.

Data Appendix

Data Sources

The data for this study come from four sources: the BPS assignment system, lottery records from individual charter schools, and two state-wide datasets, the SIMS and MCAS files. The BPS assignment data are documented in Abdulkadiroğlu, Pathak, Roth and Sönmez (2006). Lottery coverage is documented below and in Table A1. Following the discussion of lottery data, we describe the state data sets and our matching process in detail.

Lottery Coverage

Charter School Lotteries

- Of the four charters that enroll elementary school students, three had no usable lottery data. One K-8 school contributes to the middle school sample but is not used in the elementary school analysis because it added elementary grades only in 2006.
- Of the 12 charters that enroll middle school students, five contribute to the lottery analysis: Academy of the Pacific Rim, Boston Collegiate, Boston Preparatory, Edward Brooke and Roxbury Preparatory. Two non-included schools closed before or while the study was under way. Lottery data from four schools were unavailable, either because they were not archived or lost. The final omitted school, MATCH Charter Public Middle School, did not enroll 6th grade students until 2008.
- Of the eight (non-alternative) charter schools that enroll high school students, three contribute to the lottery analysis: City on a Hill, Codman Academy, and MATCH. Two non-included schools closed before or while the study was under way. Two schools do not admit via lottery in grade nine but rather admit students in grade five; they contribute to the middle school analysis. One did not have adequate lottery records to be part of the sample.

Pilot School Lotteries

- Of the eight pilot schools that enroll elementary school students, five were over-subscribed in our sample window and contribute to the lottery study: Lyndon, Mason Elementary, Mission Hill, Orchard Gardens, and Young Achievers. Two other Pilot schools, Dennis Haley and Mary Lyon, were converted to pilot status in Fall 2008, which was too late to contribute to the study.

- Of eight pilot middle schools admitting 6th graders, six were over-subscribed and contribute to the lottery study: Harbor School, Josiah Quincy, Lilla G. Frederick, Lyndon, Mission Hill, Orchard Gardens. The Boston Teachers Union School opened too late to contribute to the study. We also have middle school scores from three K-8 pilot schools: Lyndon, Mission Hill, and Young Achievers. A fourth K-8 pilot began enrolling kindergartners too late to contribute middle school outcomes.
- Of the six (non-alternative) 9-12 pilot high schools, two use lotteries and contribute to the lottery study: Another Course to College, and TechBoston Academy. One 6-12 pilot was not over-subscribed. A single alternative high school that serves an older population was omitted from the study.

The applicant cohorts contributed by each school are detailed in Appendix Table A1.

State Data

The first state dataset is the Massachusetts Students Information Management System (SIMS). This contains snapshots of all students in public school in Massachusetts in October and at the end of the school year. The SIMS file includes demographic information on students, their current school, residence, and days of attendance. We work with the SIMS files for the 2001-2002 through the 2008-2009 school years. Schools are classified as pilot schools, charter schools, public schools, exam schools, and alternative/special education schools using a grade-specific algorithm described below. The second state-wide dataset is the Massachusetts Comprehensive Assessment System (MCAS) database. The MCAS files have raw test scores for Math, English Language Arts (ELA), and Writing for grades 3-8 and grade 10 from spring 2002-2008. We standardized MCAS scores by subject, grade, and year to a combined BPS and Boston charter schools reference population. MCAS scores were merged to the SIMS file using unique student identifiers (the SASID). Students in the MCAS file but not in the SIMS file are excluded because they cannot be assigned to schools.

Cohorts and Test Years Analyzed

The cohorts and test years analyzed in this study are summarized in the table below. Baseline years are defined as the year a student attended 8th grade for the high school sample, the year a student attended 4th grade for the middle school sample, and the year a student attended kindergarten for the elementary school and K2-applicants-to-K-8-middle-school samples. There are no baseline test score for this last group of students, but we pulled demographics from the baseline years listed in the table.

	Elementary school	Middle (non-K2 applicants)	Middle (K2 lottery applicants)	High school
Outcome years	2006-2009	2003-2009	2004-2009	2005-2008
Lottery application years	2002-2004	2002-2007	1997-2001	2003-2006
Baseline years	2003-2005	2002-2006	1998-2002	2003-2006

Notes: Lottery application years refer to Fall; outcome and baseline years refer to Spring. Baseline grades are kindergarten for elementary school and K2 middle school applicants, 4th grade for all other middle school students and 8th grade for high school students. The 2003 middle school outcome year is used only for 2002 pilot middle school 6th grade applicants, as we use BPS demographics for these students from 2001 and we do not have state demographics for 2001.

Analysis Samples

Three matched files constitute the extracts used for statistical analysis. The first (the charter lottery extract) was constructed by linking charter applicants to SIMS and MCAS data; the second (the pilot lottery extract) was constructed by linking students with a first-choice pilot in the BPS assignment mechanism to SIMS and MCAS records; and the third and largest (the observational extract) uses the SIMS and MCAS matched file without regard to charter and pilot application. Table A2 provides more details on how these three extracts were constructed. In the charter lottery sample, since students may apply to more than one charter school, we transform applications into one observation per student per each year. If, for example, a student applies to a charter entry grade in 2002 and later in 2003, the student contributes two observations.

All three extracts are limited to students enrolled in BPS or a Boston charter school as of the relevant baseline date.

Classification of Schools

We define pilot schools as those listed on the pilot school network website at <http://www.ccebos.org/pilotschools/schools.html>. We define charter schools as those schools identified as such by the Massachusetts Department of Elementary and Second Education website (<http://www.profiles.doe.mass.edu>). As discussed in the text, Health Careers Academy is a Horace Mann charter high school. Boston Day and Evening Charter, also a Horace Mann charter, serves

over-age students. This school, along with Greater Egleston Community High School, a pilot school that also serves older students, was designated as an alternative/special education school rather than a charter or pilot and therefore omitted from our lottery analysis.

Placing Students at Schools

In the Massachusetts State SIMS file, school designation is grade-specific. When a student repeats grades, we kept the first school in grade. Students attending multiple schools in a given school year were assigned as follows:

- A student was assigned to a Boston charter or pilot school if any record for the grade shows attendance at a Boston charter or pilot.
- If a student attends multiple Boston charters or pilots, or both a Boston charter and pilot, the student was assigned to school for which the attendance duration is longest. Ties are broken randomly.
- Students attending multiple traditional BPS schools were assigned to the school of the longest attendance duration. Ties were broken randomly.

Table A.1: Charter and Pilot School Study Participation

	Years in Lottery Study (1)	Years with initial offer data (2)	Grade Coverage (3)	Year Opened or Converted (4)	Notes (5)
<i>I. Elementary School (3rd and 4th grades)</i>					
Charter Schools					
Boston Renaissance Charter Public School	x	x	K-6	1995	School lottery data are unavailable for elementary school.
Conservatory Lab Charter School	x	x	PK-5	1999	School lottery data are unavailable for elementary school.
Edward Brooke Charter School	x	x	K-8	2002	Became K-8 in 2006, so test scores are not used in elementary analyses.
Neighborhood House Charter School	x	x	K-8	1995	School lottery data are unavailable for elementary school.
Pilot Schools					
Boston Teachers Union School	x	x	PK-2, 6	2009	New school in 2009. Does not contribute to observational or lottery analysis.
Gardner Elementary School	x	x	K-5	2007	All applicants in 2002-2004 are accepted. Converted from regular public school.
Haley Elementary School	x	x	PK-5	2008	Converted from regular public school.
Lee Academy	x	x	K-5	2004	New school in 2004, all applicants in 2004 are accepted.
Lyndon K-8 School	2002-2004	x	K-8	1995	
Mary Lyon School	x	x	K-9	2008	Converted from regular public school
Mason Elementary School	2003-2004	x	PK-5	2003	All applicants in 2002 are accepted. Converted from regular public school.
Mission Hill School	2002-2004	x	K-8	1997	
Orchard Gardens K-8 School	2003-2004	x	K-8	2003	No Kindergarten class in 2002 (new school).
Young Achievers K-8 School	2002-2004	x	K-8	1995	
<i>II. Middle School (6th, 7th, and 8th grades)</i>					
Charter Schools					
Academy of the Pacific Rim Charter Public School	2005-2007	2005-2007	5-12	1997	Insufficient lottery records from 2002-2004. Entry grade was 6th until 2007.
Boston Collegiate Charter School	2002-2006	2002-2006	5-12	1998	
Boston Preparatory Charter Public School	2005-2007	2005-2007	6-10	2004	All applicants in 2004 accepted. Initial offer only 2005 (all applicants eventually accepted).
Boston Renaissance Charter Public School	x	x	K-6	1995	School lottery data are unavailable.
Edward Brooke Charter School	2006	2006	K-8	2002	Entry grade was 5th until 2006 when school become K-8. Initial offer data only for 2006 (all applicants eventually accepted)
Excel Academy Charter School	x	x	5-8	2003	Insufficient admissions lottery records.
Frederick Douglass Charter School	x	x	5-12	2000	Closed.
MATCH Charter Public Middle School	x	x	6-7	2008	New school.
Neighborhood House Charter School	x	x	K-8	1995	School lottery data are unavailable for elementary school.
Roxbury Preparatory Charter School	2003-2007	2006-2007	6-8	1999	Ever offer only 2003-2005.
Smith Leadership Academy Charter Public School	x	x	6-8	2003	Insufficient admissions lottery records.
Uphams Corner Charter School	x	x	5-8	2002	Insufficient admissions lottery records. Closed.
Pilot Schools - 6th Grade Applicants					
Boston Teachers Union School	x	x	PK-2, 6	2009	New school in 2009. Does not contribute to observational or lottery analysis.
Harbor School	2002, 2006-2007	x	6-8	1997	All applicants in 2003-2005 are accepted.
Josiah Quincy Upper School	2004-2007	x	6-12	1999	No Grade 6 class in 2002 and 2003 (new school).
Lilla G. Frederick Pilot Middle School	2004-2007	x	6-8	2003	No Grade 6 class in 2002 and all Grade 6 applicants in 2003 are accepted.
Lyndon K-8 School	2004-2007	x	K-8	1995	All Grade 6 applicants in 2002 and 2003 guaranteed admission.
Mission Hill School	2002-2003, 2007	x	K-8	1997	All Grade 6 applicants in 2004-2006 guaranteed admission.
Orchard Gardens K-8 School	2003-2007	x	K-8	2003	No Grade 6 class in 2002 (new school).
Young Achievers K-8 School	x	x	K-8	1995	All Grade 6 applicants in 2002-2007 guaranteed admission.
Pilot Schools - K2 applicants					
Lyndon K-8 School	1997-1998, 2000-2001	x	K-8	1995	All K2 applicants in 1999 were admitted.
Mary Lyon School	x	x	K-9	2008	Converted from regular public school.
Mission Hill School	1998-2001	x	K-8	1997	
Orchard Gardens K-8 School	x	x	K-8	2003	
Young Achievers K-8 School	1999-2001	x	K-8	1995	Error in records 1998, all K2 applicants in 1997 guaranteed admission.
<i>III. High School (10th grade)</i>					
Charter Schools					
Academy of the Pacific Rim Charter Public School	x	x	5-12	1997	No admissions lottery for students in Grade 9.
Boston Collegiate Charter School	x	x	5-12	1998	No admissions lottery for students in Grade 9.
City On A Hill Charter Public School	2004-2006	2004-2006	9-12	1995	Initial offer data only for 2002 and 2004 (all applicants eventually accepted), records unclear in 2003.
Codman Academy Charter Public School	2004	2004	9-12	2001	Initial offer data only for 2004 (all applicants eventually accepted)
Frederick Douglass Charter School	x	x	5-12	2000	Closed.
MATCH Charter Public High School	2003-2006	2003-2006	9-12	2000	
Roxbury Charter High Public School	x	x	9-12	2003	Closed.
Horace Mann Charter Schools					
Boston Day and Evening Academy Charter School	x	x	9-12, Alt	1998	Included in observational study as alternative school.
Health Careers Academy Charter School	x	x	9-12	1998	Insufficient lottery records.
Pilot Schools					
Greater Egleston Community High	x	x	9-12, Alt	1996	Included in observational study as alternative school. Converted from regular public school.
Another Course to College	2003-2006	x	9-12	2003	No Grade 9 class in 2002 (new school). Converted from regular public school.
Boston Arts Academy	x	x	9-12	1998	Selective admissions.
Boston Community Leadership Academy	x	x	9-12	2002	Selective admissions. Converted from regular public school.
Fenway High School	x	x	9-12	1995	Selective admissions. Converted from regular public school.
Josiah Quincy Upper School	x	x	6-12	1999	All students guaranteed admission.
New Mission High School	x	x	9-12	1996	Selective admissions.
TechBoston Academy	2003-2006	x	9-12	2002	No Grade 9 class in 2002 (new school).

Notes: Schools may not participate in the lottery study (those marked with x in column (1)) if they are undersubscribed, only have seats for students with guaranteed admissions, did not keep sufficient records, or if they do not have an entry lottery at the school level start grade as indicated in the notes. Some schools are excluded because they opened too late to be relevant for the study. Fredrick Douglass Charter School and Roxbury Charter High Public School were closed by the State Board of Education in the 2004-2005 school year. A school that spans K-8 is listed as both elementary and middle. These schools are part of the elementary pilot school lottery study, and also for the middle school pilot school lottery student with entry at K and 6.

Table A.2: Sample Selection

	Elementary (1)	Middle School (2)	High School (3)
<i>A. Charter Lottery Sample</i>			
Application cohorts	-	2002-2007	2003-2006
Applications to charter in entry grades	-	3,161	3,199
Excluding applications not matched to state dataset	-	3,078	3,066
Excluding applications with sibling priority	-	2,832	3,024
Excluding late applicants	-	2,831	3,024
Transforming applications into one observation per student	-	2,335	2,304
Excluding students with no follow-up test score	-	2,106	1,826
Excluding students not in BPS or in a charter school in Boston at baseline (this also drops those without baseline demographics)	-	1,386	1,484
Excluding students with no baseline test scores	-	1,363	1,474
Number of follow-up ELA scores for students in BPS or a Boston charter at baseline	-	3,157	1,473
Number of follow-up Math scores for students in BPS or a Boston charter at baseline	-	3,317	1,455
<i>B. Pilot Lottery Sample</i>			
		<i>6th grade applicants</i> 2002-2007	<i>K2 applicants</i> 1997-2001
Application cohorts	2002-2004	2002-2007	2003-2006
Applied to pilot school as first choice	1,259	4,865	1,273
Excluding guarantees and students with sibling priority	1,051	3,545	1,007
Excluding students that applied to a pilot not subject to randomization	934	1,722	752
Excluding students with no follow-up test score	730	1,605	482
Excluding students not in BPS or in a charter school in Boston at baseline (this also drops those without baseline demographics)	596	1,492	481
Excluding students with no baseline test scores	-	1,324	-
Number of follow-up ELA scores for students in BPS or a Boston charter at baseline	1,141	3,254	1,060
Number of follow-up Math scores for students in BPS or a Boston charter at baseline	1,139	3,684	1,093
<i>C. Regression Sample</i>			
Baseline years	2002-2005	2002-2007	2002-2007
Students in BPS or in a charter school in Boston and demographics at baseline	21,589	28,005	30,443
Students in BPS or in a charter school in Boston and demographics at outcome	15,952	23,482	22,231
Excluding students with no follow-up test score	15,465	22,657	20,236
Excluding students with no baseline test scores	-	22,115	19,675
Number of follow-up ELA scores for students in BPS or a Boston charter at baseline and outcome	26,383	44,077	20,110
Number of follow-up Math scores for students in BPS or a Boston charter at baseline and outcome	23,695	48,357	19,799

Table A.3: Differential Attrition Omitting Unbalanced Cohorts

		Charter Schools			Pilot Schools		
Level	Subject	Prop of non-offered with MCAS (1)	Differential		Prop of non-offered with MCAS (4)	Differential	
			Demographics (2)	Demographics + Baseline Scores (3)		Demographics (5)	Demographics + Baseline Scores (6)
Elementary School							
	ELA	-	-	-	0.853	0.036 (0.035)	-
		N				1085	-
	Math	-	-	-	0.853	0.035 (0.035)	-
		N				1085	
Middle School							
	ELA	0.814	0.031 (0.021)	0.027 (0.021)	0.803	0.020 (0.021)	0.003 (0.024)
		N	3594	3525		4277	2799
	Math	0.815	0.030 (0.021)	0.029 (0.021)	0.800	0.022 (0.021)	0.007 (0.024)
		N	3676	3600		4795	3147
High School							
	ELA	0.785	0.001 (0.026)	-0.002 (0.026)	0.756	0.020 (0.032)	0.038 (0.032)
		N	1849	1752		887	822
	Math	0.778	0.000 (0.026)	-0.001 (0.026)	0.752	0.013 (0.033)	0.033 (0.032)
		N	1849	1813		887	865
	Writing Topic and Writing Composition	0.778	0.000 (0.026)	-0.001 (0.027)	0.752	0.013 (0.033)	0.032 (0.033)
		N	1849	1744		887	816

Notes: This table has the same notes as Table 5, and is based on a sample which excludes the following cohorts: Pilot 4th grade ELA and Math (2004 applicants), charter 7th grade ELA and Math (2004 6th grade applicants), and pilot 10th grade ELA, Math and Writing (2006 applicants).

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

Table A.4: Charter and Pilot Lottery Results Omitting Unbalanced Cohorts

Level	Subject	Charter Schools					Pilot Schools				
		Basic controls					Basic controls				
		First Stage (1)	Reduced Form (2)	2SLS (3)	2SLS w/demographics (4)	2SLS w/demographics and baseline scores (5)	First Stage (6)	Reduced Form (7)	2SLS (8)	2SLS w/demographics (9)	2SLS w/demographics and baseline scores (10)
Elementary School											
	ELA	-	-	-	-		2.945*** (0.189)	0.209** (0.084)	0.071** (0.028)	0.062** (0.026)	-
		N						1141	1141		
	Math	-	-	-	-		2.950*** (0.190)	0.110 (0.085)	0.037 (0.029)	0.033 (0.028)	-
		N						1139	1139		
Middle School											
	ELA	0.988*** (0.101)	0.258*** (0.068)	0.261*** (0.070)	0.212*** (0.058)	0.204*** (0.048)	1.526*** (0.172)	0.022 (0.065)	0.014 (0.042)	0.010 (0.040)	-0.041 (0.103)
		N	3082		3082	3027		4314	4314	3024	
	Math	0.956*** (0.096)	0.391*** (0.065)	0.409*** (0.068)	0.372*** (0.060)	0.351*** (0.049)	1.450*** (0.167)	-0.065 (0.064)	-0.045 (0.044)	-0.041 (0.041)	-0.223** (0.090)
		N	3243		3243	3185		4777	4777	3348	
High School											
	ELA	0.550*** (0.154)	0.089 (0.072)	0.162 (0.110)	0.178* (0.098)	0.265*** (0.076)	0.603*** (0.101)	0.013 (0.060)	0.021 (0.098)	-0.002 (0.100)	-0.085 (0.084)
		N	1473		1473	1401		688	688	648	
	Math	0.543*** (0.152)	0.190** (0.084)	0.350*** (0.118)	0.368*** (0.114)	0.364*** (0.085)	0.599*** (0.102)	-0.083 (0.082)	-0.138 (0.135)	-0.195 (0.139)	-0.116 (0.097)
		N	1455		1455	1432		684	684	672	
	Writing Topic	0.551*** (0.155)	0.167* (0.089)	0.303** (0.128)	0.319*** (0.120)	0.349*** (0.120)	0.594*** (0.102)	0.090 (0.071)	0.151 (0.116)	0.136 (0.124)	0.144 (0.121)
		N	1461		1461	1386		679	679	637	
	Writing Composition	0.551*** (0.155)	0.106 (0.075)	0.192 (0.116)	0.214* (0.115)	0.212** (0.104)	0.594*** (0.102)	0.087 (0.077)	0.147 (0.125)	0.130 (0.136)	0.132 (0.121)
		N	1461		1461	1386		679	679	637	

Notes: This table has the same notes as Table 4A, and is based on a sample which excludes the following cohorts: Pilot 4th grade ELA and Math (2004 applicants), charter 7th grade ELA and Math (2004 6th grade applicants), and pilot 10th grade ELA, Math and Writing (2006 applicants).

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

Table A.5.A: Ever Offer Covariate Balance with Followup

	Charter Schools				Pilot Schools				
	Middle School		High School		Elementary School	Middle School		High School	
	All Lotteries	Lotteries with Baseline Scores	All Lotteries	Lotteries with Baseline Scores	All Lotteries	All Lotteries	Lotteries with Baseline Scores	All Lotteries	Lotteries with Baseline Scores
	(1)	(2)	(3)	(4)	(5)	(7)	(8)	(9)	(10)
Hispanic	-0.003 (0.026)	-0.008 (0.026)	-0.056* (0.029)	-0.054* (0.029)	-0.026 (0.041)	-0.025 (0.028)	-0.041 (0.039)	0.009 (0.031)	-0.005 (0.032)
Black	-0.024 (0.032)	-0.021 (0.032)	0.064** (0.032)	0.061* (0.033)	0.009 (0.045)	-0.001 (0.030)	0.011 (0.041)	-0.013 (0.034)	0.004 (0.035)
White	0.021 (0.025)	0.023 (0.025)	-0.010 (0.016)	-0.009 (0.017)	0.030 (0.038)	0.033 (0.022)	0.032 (0.023)	-0.008 (0.019)	-0.002 (0.020)
Asian	0.005 (0.009)	0.005 (0.009)	0.000 (0.013)	-0.002 (0.013)	-0.034* (0.019)	0.000 (0.016)	-0.001 (0.021)	0.012 (0.017)	0.002 (0.017)
Female	0.035 (0.033)	0.035 (0.034)	-0.022 (0.034)	-0.031 (0.035)	0.010 (0.052)	0.000 (0.034)	-0.001 (0.044)	0.002 (0.034)	-0.018 (0.036)
Free or Reduced Price Lunch	-0.007 (0.031)	-0.010 (0.031)	0.027 (0.029)	0.022 (0.030)	-0.100** (0.045)	-0.007 (0.026)	-0.024 (0.031)	0.041 (0.029)	0.042 (0.029)
Special Education	-0.018 (0.027)	-0.020 (0.027)	-0.007 (0.025)	-0.005 (0.026)	-0.038 (0.027)	0.005 (0.023)	0.017 (0.036)	-0.031 (0.024)	-0.027 (0.026)
Limited English Proficiency	0.013 (0.016)	0.014 (0.016)	0.007 (0.011)	0.009 (0.011)	-0.022 (0.026)	-0.035** (0.017)	-0.054** (0.027)	0.009 (0.015)	0.001 (0.012)
Baseline ELA Test Score	-	0.038 (0.065)	-	-0.073 (0.055)	-	-	-	-	0.064 (0.058)
Baseline Math Test Score	-	0.083 (0.063)	-	-0.022 (0.060)	-	-	-	-	-0.035 (0.062)
Baseline Writing Composition Test Score	-	-	-	0.010 (0.057)	-	-	-	-	0.050 (0.061)
Baseline Writing Topic Test Score	-	-	-	-0.069 (0.058)	-	-	-	-	0.007 (0.059)
p-value, from F-test	0.903	0.834	0.549	0.510	0.073*	0.489	0.559	0.848	0.823
N	1386	1356	1484	1390	596	1973	1302	1038	966

Notes: This table has the same notes as Table 3. The sample is restricted to students that have at least one followup test score.

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

Table A.5.B: Initial Offer Covariate Balance

	All charter applicants				Charter applicants with followup scores			
	Middle School		High School		Middle School		High School	
	All Lotteries	Lotteries with Baseline Scores	All Lotteries	Lotteries with Baseline Scores	All Lotteries	Lotteries with Baseline Scores	All Lotteries	Lotteries with Baseline Scores
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Hispanic	0.009 (0.025)	0.005 (0.025)	-0.036 (0.023)	-0.037 (0.024)	0.011 (0.026)	0.008 (0.026)	-0.053** (0.025)	-0.048* (0.026)
Black	-0.006 (0.031)	-0.012 (0.031)	0.054** (0.026)	0.056** (0.026)	-0.013 (0.032)	-0.018 (0.033)	0.059** (0.029)	0.057* (0.029)
White	0.001 (0.024)	0.010 (0.024)	-0.021* (0.012)	-0.022* (0.012)	0.008 (0.025)	0.016 (0.025)	-0.007 (0.014)	-0.008 (0.014)
Asian	-0.002 (0.009)	-0.001 (0.009)	-0.006 (0.009)	-0.008 (0.009)	-0.003 (0.009)	-0.003 (0.010)	-0.006 (0.010)	-0.009 (0.010)
Female	0.020 (0.033)	0.017 (0.033)	-0.017 (0.028)	-0.021 (0.029)	0.017 (0.034)	0.013 (0.034)	-0.008 (0.032)	-0.010 (0.033)
Free or Reduced Price Lunch	0.041 (0.030)	0.033 (0.030)	0.001 (0.025)	-0.006 (0.025)	0.043 (0.031)	0.037 (0.031)	0.014 (0.028)	0.011 (0.028)
Special Education	-0.038 (0.025)	-0.034 (0.026)	0.015 (0.021)	0.010 (0.021)	-0.039 (0.026)	-0.037 (0.027)	0.005 (0.023)	-0.003 (0.023)
Limited English Proficiency	-0.025* (0.014)	-0.026* (0.014)	0.007 (0.010)	0.006 (0.009)	-0.039*** (0.015)	-0.040*** (0.015)	0.008 (0.010)	0.010 (0.010)
Baseline ELA Test Score	-	-0.016 (0.069)	-	-0.050 (0.048)	-	-0.003 (0.071)	-	-0.076 (0.052)
Baseline Math Test Score	-	0.090 (0.066)	-	-0.053 (0.050)	-	0.130* (0.068)	-	-0.058 (0.055)
Baseline Writing Composition Test Score	-	-	-	0.020 (0.050)	-	-	-	0.026 (0.056)
Baseline Writing Topic Test Score	-	-	-	-0.062 (0.050)	-	-	-	-0.057 (0.053)
p-value, from F-test	0.382	0.162	0.240	0.218	0.084*	0.008***	0.446	0.354
N	1277	1248	1849	1723	1156	1133	1484	1390

Notes: This table reports results from regressions like those in Table 3, except initial offer is used rather than ever offer. Columns (1)-(4) use all lottery participants. Columns (5)-(8) restrict the sample to students with at least one followup test score.

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

Table A.6: Graduation and Repetition for High School Students

Outcome	Charter Schools		Pilot Schools	
	Demographics (1)	Demographics and baseline scores (2)	Demographics (3)	Demographics and baseline scores (4)
Graduation (includes attriters)	-0.051 (0.089)	-0.052 (0.090)	-0.021 (0.056)	-0.067 (0.058)
	1130	1055	761	702
Graduation (excludes attriters)	-0.016 (0.079)	-0.026 (0.080)	0.010 (0.048)	-0.017 (0.049)
	990	926	655	607
Ever repeat	0.068 (0.045)	0.064 (0.045)	-0.002 (0.035)	-0.001 (0.035)
	1561	1466	1119	1042
Mean graduation rate in sample (including attriters)	0.717	0.714	0.714	0.721

Notes: This table reports coefficients from 2SLS regressions using years in charter and pilot schools as the endogenous variable with the ever offer dummy as the instrument. The dependent variable in the first row is equal to one if a student graduated high school. The sample is restricted to students who should have graduated by Spring 2009. The sample in the next row excludes students who have not graduated and are not observed in Massachusetts public schools in the 2008-2009 school year. The "ever repeat" variable is one if a student ever repeats a grade. Robust standard errors are reported in parentheses.

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

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