MIT Libraries | DSpace@MIT

MIT Open Access Articles

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

The MIT Faculty has made this article openly available. ***Please share*** how this access benefits you. Your story matters.

Citation: Abdulkadiroglu, Atila et al. "Accountability and Flexibility in Public Schools: Evidence from Boston's Charters and Pilots." Quarterly Journal of Economics, (2011) 126(2): 699-748.

As Published: http://dx.doi.org/10.1093/qje/qjr017

Publisher: President and Fellows of Harvard College

Persistent URL: http://hdl.handle.net/1721.1/61945

Version: Author's final manuscript: final author's manuscript post peer review, without publisher's formatting or copy editing

Terms of use: Creative Commons Attribution-Noncommercial-Share Alike 3.0

Massachusetts Institute of Technology

ACCOUNTABILITY AND FLEXIBILITY IN PUBLIC SCHOOLS: EVIDENCE FROM BOSTON'S CHARTERS AND PILOTS*

Atila Abdulkadiroğlu
Joshua D. Angrist
Susan M. Dynarski
Thomas J. Kane
Parag A. Pathak

This draft: September 2010

Abstract

We use student assignment lotteries to estimate the effect of charter school attendance on student achievement in Boston. We also evaluate a related alternative, Boston's pilot schools. Pilot schools have some of the independence of charter schools, but operate within the Boston Public School district and are covered by some collective bargaining provisions. Lottery estimates show large and significant score gains for charter students in middle and high school. In contrast, lottery estimates for pilot school students are mostly small and insignificant, with some significant negative effects. Charter schools with binding assignment lotteries appear to generate larger gains than other charters.

*The authors are grateful to the Boston Foundation for financial support and to Boston's charter schools, the Boston Public Schools, and Carrie Conaway, Mary Street, and the staff of the Massachusetts Department of Elementary and Secondary Education for data and assistance. Sarah Cohodes, Jon Fullerton and the staff of the Center for Education Policy Research at Harvard University played a critical role in this project. We also thank Bruno Ferman, Yuqiao Huang, Simone Schaner, Nathaniel Schwartz, Chris Walters, and Victor Vazquez for expert research assistance. We benefited from comments by David Figlio, Derek Neal and seminar participants at the Bank of Italy, the LSE, McGill, the New York Fed, NBER, Simon Fraser, SOLE, UBC, UC Dublin, UCLA, UC Riverside, UIUC, University of Pennsylvania, University of Tennessee, ZEW, the November 2009 New Directions in the Economic Analysis of Education Conference at the University of Chicago and the June 2010 Frontiers in Economics of Education Workshop at Tel Aviv University. Abdulkadiroğlu acknowledges an NSF-CAREER award. Pathak thanks the Rappaport Institute for Greater Boston for research support.

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

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

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 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.

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). 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 much like those reported here for Boston charter middle schools.[3]

[2]See 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."

[3]Farther 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,

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 remains of bias from unobserved variables such as motivation or family background. 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

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

[4]More 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.

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 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 choose 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.

Lottery-based estimation shows large score gains for students who spend time at a charter school, but zero or even effects of time in a pilot school. In an effort to gauge the generality of these 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 of charter effects 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

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

and pedagogical approach know 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.

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 Thernstom (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 I 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 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 I, the features that characterize the full roster of Boston 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

[6]The 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 proportion highly qualified differ.

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 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.

[7]Four 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.

[8]The 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.

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 I. 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

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

[10]For 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.

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

causal relation of interest is captured using equations like this one for the scores, y_{igt}, of student i in 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. The estimation pools grades within levels (Tests are given in 3rd and 4th grade in elementary school; 6-8th grade in middle school; and 10th grade in high school). 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 (students in elementary school and middle school are tested more than once), 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 (charter and pilot lottery procedures are described in detail in the next section). 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 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

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

[13]The 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.

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

The main data source for this study is matched sample linking MCAS and SIMS data to applicant records from charter school lotteries.[14] Each charter school collects applications and holds its own lottery in years in which the school is over-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 A.1 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.[15] 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.

[14]Records were matched 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 online data appendix.

[15]Along with the data appendix, all appendix tables and figures are available on-line.

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 undersubscribed). 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 applicant's 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 A.1 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.

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.[16] 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).

[16]For details, see Abdulkadiroğlu and Sönmez (2003) and Abdulkadiroğlu, Pathak, Roth and Sönmez (2006).

One K-8 pilot school opened too late to contribute middle school test scores by K2 applicants.

Student Characteristics and Covariate Balance

Table II 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 II, is similar to that of the total charter and pilot samples.

Table II 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 0.5 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 III 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 III are small and statistically insignificant. There are no significant contrasts for middle school charter applicants. Among charter high school applicants, lottery winners are 5 percentage points less likely to be Hispanic and about 6 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.[17]

[17] We 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

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 IVA (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 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 0.25 standard deviations (hereafter, σ) higher on ELA and 0.40σ higher in math, a result shown in column 2 of Table IVA (labeled "reduced form").[18] 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.[19]

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.

[18]The results reported in Table IV and later tables pool grade outcomes within the relevant level (e.g., grades 6-8 in middle school).

[19]Students contribute multiple scores (from tests in different grades) in elementary and middle school, so these standard errors are two-way clustered on student identifier and on school-by-year. Standard errors for high school estimates are

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 $0.19\sigma - 0.36\sigma$.

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 IVA. 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.

clustered on school by year only.

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 IVB 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 and 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.[20]

An alternative parameterization of the ever-offer first stage uses potential years in charter as the instrument. Potential years counts the grades a student who wins the lottery would spend in a

[20]Table 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 follow-up 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.

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-years parameterization is useful because it generates a natural benchmark for charter and pilot student mobility. Specifically, these first-stage estimates can be compared to the expected years spent in any new middle or high school for an arbitrary BPS student.

The potential-years first stage, reported in columns 1 and 6 of Table IVC, 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 IVC shows that using potential years as an instrument yields charter and pilot effect estimates that are remarkably similar to those reported in Table IVA.

Figure I provides 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 (using ever offer) by cohort and grade. The plot starts in fourth grade, the baseline comparison, where differences should be small.[21] 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 forms in Panel A suggest a cumulative effect. It should also be noted that even a flat reduced form implies an increasing second-stage estimate because the first stage falls over time.[22]

Consistent with the smaller pooled estimates for ELA, the cohort-by-grade ELA estimates in Panel B of Figure I are smaller and noisier than those in Panel A for math. Here, too, however, the trend in

[21] The sample used to construct Figure I includes applicants with baseline scores. The reduced-form estimates plotted in the figure come from models that include risk set and demographic controls.

[22] As 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.

cohort-specific reduced forms is mostly up or at least flat, implying increasing second-stage effects as charter exposure increases. Figure II plots the corresponding cohort-by-grade reduced form estimates for pilot schools; these show no evidence of an effect in any cohort or grade.

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

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

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 sample analog of equation (4) for charter applicants' middle school math scores appears in Panel A of Figure III. 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.[23] 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.

The strong achievement reported for charter schools raise the question of whether charter atten-

[23] 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).

dance increases educational attainment as well as test scores. An appendix table (A.6) reports charter and pilot effects on high school graduation rates and the probability of grade repetition. There is no clear evidence of graduation or repetition effects, but these estimates are limited to one year's follow-up data for a single high school cohort. A short horizon for high school graduation is problematic if charter schools are more likely than traditional public schools to opt for grade retention when students are struggling. In future work, we hope to follow more cohorts for a longer period, tracking post-secondary outcomes like college matriculation and completion.

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 that 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.[24]

Table V 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-

[24]More 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).

up differentials by win/loss status were estimated using regression models that parallel the reduced forms reported in Table IVA. 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 IV.

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 V. 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 fairly small.[25] 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 A.3, while the corresponding lottery-based estimates of treatment effects are reported in Table A.4. These results are similar to those reported in Table IVA.

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

[25] In 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.

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 IV 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 generated negative peer effects. It therefore makes sense to look for evidence of excess withdrawals. Some of the relevant evidence appears in Table IVC, 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 VI. This table shows that charter lottery winners are about 15 percentage points less likely to switch than losers. This estimate comes from a regression model that parallels the reduced forms reported in Table IVA, where the dependent variable is an indicator variable equal to one if a student switched schools and the instrument is ever offer.

This lower 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. Some switches are driven by charter applicants who enter 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 school in 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 because they serve 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:

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

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 coefficient of interest is ρ_1; this term tells us whether effects are larger or smaller as baseline scores increase. The corresponding first-stage equations are

$$S_{igt} = \lambda_{1t} + \kappa_{1g} + \sum_j \mu_{1j} d_{ij} + \Gamma_1' X_i + \pi_{10} Z_i + \pi_{11} B_i Z_i + \eta_{1igt} \tag{6}$$

$$S_{igt}(B_i - b_g) = \lambda_{2t} + \kappa_{2g} + \sum_j \mu_{2j} d_{ij} + \Gamma_2' X_i + \pi_{20} Z_i + \pi_{21} B_i Z_i + \eta_{2igt}, \tag{7}$$

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

The effect of attending a charter middle school is larger for students with *lower* baseline scores, though the estimated interaction terms are small. This can be seen in the second column of Table VII, 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

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

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

$$S_{igt} = \lambda_{1t} + \kappa_{1g} + \sum_j \mu_{1j} d_{ij} + \Gamma_1' X_i + \pi_{10} Z_i + \pi_{11} \bar{b}_{(i)} Z_i + \eta_{1igt} \tag{9}$$

$$S_{igt}(\bar{b}_{(i)} - \bar{b}_g) = \lambda_{2t} + \kappa_{2g} + \sum_j \mu_{2j} d_{ij} + \Gamma_2' X_i + \pi_{20} Z_i + \pi_{21} \bar{b}_{(i)} Z_i + \eta_{2igt}. \tag{10}$$

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

Contrary to the usual view of high achieving peers, Table VII 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 VII are significantly different from zero, though it should be noted that the estimated 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 were relevant and well documented. We'd like to gauge the external validity of the findings this generates: are the effects at other Boston charters and pilots similar? To get a handle on external validity, we computed OLS estimates controlling for student demographics and baseline scores. Although statistical controls do not necessarily eliminate selection bias, we validate the observational strategy by comparing observational and lottery estimates in the sample where both can be computed. Where observational and lottery estimates are close, the observational estimates seem likely to be informative for non-lottery-sample schools as well.

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. Time in charter and pilot schools has different effects for schools in and out of the lottery study. Specifically, the observational estimates were constructed by fitting

$$(11) \qquad y_{igt} = \alpha_t + \beta_g + \gamma' X_i + \rho_{lc} C_{ligt} + \rho_{lp} P_{ligt} + \rho_{nc} C_{nigt} + \rho_{np} P_{nigt} + \rho_e E_{igt} + \rho_a A_{igt} + \epsilon_{igt},$$

where C_{ligt} and P_{ligt} measure time in lottery-study charter and pilot schools (with effects ρ_{lc} and ρ_{lp}), C_{nigt} and P_{nigt} measure time in non-lottery charter and pilot schools (with effects ρ_{nc} and ρ_{np}), and E_{igt} and A_{igt} denote years in an exam or alternative school, with effects ρ_e and ρ_a. The sample used to estimate this equation includes students with complete demographic information, who attended Boston schools at the time they took baseline and follow-up tests.[26]

Observational estimates of the effect of time spent in lottery-study schools are similar to the corresponding lottery estimates, especially for charter schools. This can be seen in Table VIII, which reports estimates of charter and pilot effects using the two designs. 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 VIII 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

[26]To 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 observational analysis looks at middle and high schools only because there is no baseline score data for elementary school students.

much selection bias in the first place). On the other hand, assuming this is the case, the table also suggests that the charter schools in our lottery study are among the best in Boston. Observational estimates of the effect of time spent in charter schools that were not included in the lottery study are economically and statistically significant, but 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 pilot ELA effects are more negative than the corresponding lottery estimates, while the opposite is true for math. The match across designs is not as good for pilot high schools, where the observational analysis for lottery schools produces substantial and significant positive effects, while the lottery results for ELA and math are small and not significantly different from zero (though the match for writing is good). The variation in pilot results across designs may be due to the fact that the lottery estimates for pilot high schools are not very precise. It's 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 for students in middle and high school. In contrast, lottery-based estimates for pilot school students are small and mostly insignificant, sometimes even negative. Although we cannot say for sure why charter and pilot school effects are so different, a number of factors seem likely to be important. For one thing, the student-teacher ratio is smaller in charter high schools while the charter school day and year are longer in both high school and middle school. Charter teaching staff are also unusually young. These differences may originate in collective bargaining agreements that make it relatively expensive for pilot schools to expand instructional hours and staffing and favor teacher seniority over classroom effectiveness. In addition, most of the charter schools in our lottery sample embrace elements of the No Excuses model, an instructional paradigm that is not common in public schools, pilot or otherwise.

Many of the charter schools in this study aspire to boost minority achievement, so a natural benchmark for charter effects is the black-white test score gap. Among students attending regular BPS middle schools, blacks score about 0.7σ below whites in language arts and 0.8σ below whites in math. The charter school effects reported here are therefore large enough to reduce the black-white reading gap in middle school by two-thirds. The even larger estimated math gains (about 0.4σ) are more than enough to eliminate the racial gap in math while students are in middle school. The effects of roughly 0.2σ estimated for high school ELA and math are large enough to close the black-white high school gap of about 0.8σ in both subjects (assuming four years of charter high school enrollment).

It's worth emphasizing that the large gains reported here are generated by charter schools with over-subscribed and well-documented admissions lotteries. Charter schools with good records that parents find attractive may be relatively effective. In an effort to gauge the external validity of the lottery estimates, we computed observational estimates that rely solely on statistical controls, with separate effects for schools in and out of the lottery sample. The lottery estimates of charter effects are similar to the observational estimates when the latter are estimated using the same set of schools. On the other hand, the observational estimates for charter schools that contribute to the lottery study are larger than the observational estimates for other charter schools (though the latter are still positive and significantly different from zero).

There are too few schools in the lottery study to generate an informative comparison of specific charter models or practices. Because most of the schools in the lottery study fall under the umbrella of the No Excuses model, however, the lottery results can be seen as particularly informative for this charter model. In line with this finding, our study of a single No Excuses-style KIPP school also generates evidence of large gains (Angrist, *et al.*, 2010). Likewise, in ongoing work using a larger sample of schools from around the state, preliminary results point to larger gains in urban schools, most of which embrace key elements of the No Excuses paradigm. Other charter schools seem to generate insignificant or even negative effects (see also Gleason, *et. al.*, 2010 for evidence of heterogeneous charter effects.) In future work, we hope to provide additional evidence on the relative effectiveness of alternative charter models.

Abdulkadiroğlu: Department of Economics, Duke University, email: atila.abdulkadiroglu@duke.edu. Angrist: Department of Economics, Massachusetts Institute of Technology and NBER, email: angrist@mit.edu. Dynarski: Gerald R. Ford School of Public Policy and School of Education, University of Michigan and NBER, email: dynarski@umich.edu. Kane: Graduate School of Education, Harvard University and NBER, email: kaneto@gse.harvard.edu. Pathak: Department of Economics, Massachusetts Institute of Technology and NBER, email: ppathak@mit.edu.

X References

Abdulkadiroglu, Atila, Joshua D. Angrist, Susan M. Dynarski, Thomas J. Kane, and Parag A. Pathak, "Accountability and Flexibility in Public Schools: Evidence from Boston's Charters and Pilots," NBER Working Paper No. w15549, 2009.

Abdulkadiroglu, Atila, and Tayfun Sönmez, "School Choice: A Mechanism Design Approach," *American Economic Review*, 93 (2003), 729-747.

Abdulkadiroglu, Atila, Parag A. Pathak, Alvin E. Roth, and Tayfun Sönmez, "Changing the Boston School Choice Mechanism," NBER Working Paper No. w11965, 2006.

Abdulkadiroglu, Atila, Parag A. Pathak, and Alvin E. Roth, "Strategy-Proofness versus Efficiency in Matching with Indifferences: Redesigning the NYC High School Match," *American Economic Review*, 99 (2009), 1954-1978.

Angrist, Joshua D., "Group-data Estimation and Testing in Simple Labor Supply Models," *Journal of Econometrics*, 47 (1991), 243-266.

Angrist, Joshua D., "Conditional Independence in Sample Selection Models," *Economics Letters*, 54 (1997), 103-112.

Angrist, Joshua D., Susan M. Dynarski, Thomas J. Kane, Parag A. Pathak, and Christopher Walters, "Who Benefits from KIPP?" NBER Working Paper No. w15740, 2010.

Angrist, Joshua D., and Guido W. Imbens, "Two-Stage Least Squares Estimation of Average Causal Effects in Models with Variable Treatment Intensity," *Journal of the American Statistical Association: Applications and Case Studies*, 90 (1995), 431-442.

Berends, Mark, Maria Mendiburo, and Anna Nicotera, "Charter Schools Effects in an Urban School District: An Analysis of Student Achievement Growth," Vanderbilt University Working Paper, 2008.

Bifulco, Robert, and Helen F. Ladd, “The Impact of Charter Schools on Student Achievement: Evidence from North Carolina,” *American Education Finance Association*, 1 (2006), 50-90.

Booker, Kevin, Tim R. Sass, Brian Gill, and Ron Zimmer, “Going Beyond Test Scores: Evaluating Charter School Impact on Educational Attainment in Chicago and Florida,” RAND Working Paper No. WR-610-BMG, 2008.

Center for Collaborative Education, “Description of the Boston Pilot Schools Network,” 2006. Retrieved online on November 25, 2009 from http://www.ccebos.org/pilotschools/pilot_qa.doc.

Clark, Damon, “The Performance and Competitive Effects of School Autonomy,” *Journal of Political Economy*, 117(2009), 745-783.

Center for Research on Education Outcomes, *Multiple Choice: Charter Performance in 16 States* (Stanford, CA: CREDO, 2009).

Dobbie, Will, and Roland G. Fryer, “Are High Quality Schools Enough to Close the Achievement Gap? Evidence from a Social Experiment in Harlem,” NBER Working Paper No. w15473, 2009.

Eberts, Randall W., and Kevin M. Hollenbeck, “Impact of Charter School Attendance on Student Achievement in Michigan,” Upjohn Institute Staff Working Paper No. 02-080, 2002.

Gleason, Philip, Melissa Clark, Christina C. Tuttle, and Emily Dwoyer, *The Evaluation of Charter School Impacts: Final Report* (Washington, DC: National Center for Education Evaluation and Regional Assistance, 2010).

Hoxby, Caroline M., and Sonali Murarka, ”Charter Schools in New York City: Who Enrolls and How They Affect Student Achievement,” NBER Working Paper No. w14852, 2009.

Hoxby, Caroline M., Sonali Murarka, and Jenny Kang, *How New York City’s Charter Schools Affect Achievement* (Cambridge, MA: New York City Charter Schools Evaluation Project, 2009)

Hoxby, Caroline M., and Jonah E. Rockoff, “The Impact of Charter Schools on Student Achievement,” Harvard Institute of Economic Research Working Paper Series Paper, 2005.

Manzo, Kathleen K., "Arts Education Building Steam in L.A. Area Schools," *Education Week*, June 13, 2007.

Massachusetts Department of Elementary and Secondary Education, *Massachusetts Charter Schools Fact Sheet*, 2009a. Retrieved online on September 30, 2009 from http://www.doe.mass.edu/charter/factsheet.pdf.

Massachusetts Department of Elementary and Secondary Education, *2008-2009 Enrollment by Grade Report*, 2009b. Retrieved online on September 30, 2009 from http://profiles.doe.mass.edu/state_report/enrollmentbygrade.aspx?mode=school&orderBy=.

Mathews, Jay, *Work Hard. Be Nice. How Two Inspired Teachers Created the Most Promising Schools in America* (Chapel Hill: Alongquin Press, 2009).

Merseth, Katherine K., John Roberts, Kristy Cooper, Mara Casey Tieken, and Jon Valant, *Inside Urban Charter Schools: Promising Practices and Strategies in Five High-performing Charter Schools* (Cambridge, MA: Harvard Education Publishing, 2009).

National Alliance for Public Charter Schools, *Public Charter School Dashboard*, 2009. Retrieved on September 21, 2009 from http://www.publiccharters.org/files/publications/DataDashboard.pdf.

Ravitch, Diane, *The Death and Life of the Great American School System* (New York, NY: Basic Books, 2010).

Thernstrom, Abigail, and Stephan Thernstrom, *No Excuses: Closing the Racial Gap in Learning* (New York, NY: Simon & Schuster, 2003).

Vaznis, James, "Backers Seek End to Charter Cap," *The Boston Globe*, August 5, 2009.

Vaznis, James, "Charter Schools Seeking to Expand," *The Boston Globe*, June 29, 2010

Wilson, Steven F., "Success at Scale in Charter Schooling," American Enterprise Institute Future of American Education Project Working Paper No. 2008-02, 2008.

Woodworth, Katrina R., Jane L. David, Roneeta Guha, Haiwen Wang, and Alejandra Lopez-Torkos, *San Francisco Bay Area KIPP Schools: A Study of Early Implementation and Achievement, Final Report* (Menlo Park, CA: SRI International, 2008).

Table I: Teacher Characteristics by School Type

	Traditional	Pilot, Charter, Exam or Alternative				Lottery Sample	
	BPS Schools	Charter	Pilot	Exam	Alternative	Charter	Pilot
	(1)	(2)	(3)	(4)	(5)	(7)	(8)
I. Elementary School (3rd and 4th grades)							
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
II. Middle School (6th, 7th, and 8th grades)							
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
III. High School (10th grade)							
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 II: Descriptive Statistics

	Traditional BPS Schools	Enrolled in Pilot or Charter		Applicants in Lottery Sample		Applicants in Lottery Sample w/ Baseline Scores	
		Charter	Pilot	Charter	Pilot	Charter	Pilot
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
			I. Elementary School (3rd and 4th grades)				
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	-	-
			II. Middle School (6th, 7th, and 8th grades)				
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
			III. High School (10th grade)				
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. Demographic characteristics are 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. Columns report descriptive statistics for the following samples: BPS students excluding exam, alternative, charter and pilot students from 2004-2009 (1); students enrolled in charter schools from 2004-2009 (2); students enrolled in pilot schools from 2004-2009 (3); charter applicant cohorts in randomized lotteries: middle school students in 2002-2007 and high school students in 2003-2006 (4); pilot applicant cohorts: elementary school students in 2002-2004, middle school students in 2002-2007, and high school students in 2003-2006 (5).

Table III: Covariate Balance at Charter and Pilot Schools

	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	0.005	0.000	-0.046*	-0.047*	-0.032	-0.026	-0.027	0.025	0.012
	(0.024)	(0.024)	(0.026)	(0.027)	(0.038)	(0.026)	(0.038)	(0.028)	(0.029)
Black	-0.022	-0.018	0.058**	0.057*	0.016	-0.001	0.007	-0.009	0.005
	(0.030)	(0.031)	(0.029)	(0.030)	(0.042)	(0.028)	(0.040)	(0.031)	(0.032)
White	0.012	0.013	-0.021	-0.020	0.028	0.029	0.024	-0.015	-0.010
	(0.024)	(0.024)	(0.014)	(0.015)	(0.036)	(0.020)	(0.021)	(0.017)	(0.018)
Asian	0.003	0.004	0.000	-0.002	-0.031*	0.004	-0.001	-0.003	-0.009
	(0.008)	(0.008)	(0.011)	(0.011)	(0.018)	(0.015)	(0.020)	(0.015)	(0.016)
Female	0.024	0.027	-0.005	-0.013	0.013	0.025	0.025	0.015	0.010
	(0.032)	(0.032)	(0.030)	(0.032)	(0.049)	(0.032)	(0.042)	(0.031)	(0.033)
Free or Reduced	0.001	0.000	0.011	0.006	-0.072*	-0.009	-0.015	0.058**	0.068**
Price Lunch	(0.029)	(0.030)	(0.026)	(0.027)	(0.042)	(0.024)	(0.029)	(0.026)	(0.027)
Special Education	-0.029	-0.028	-0.002	0.001	-0.025	-0.003	0.010	-0.031	-0.019
	(0.025)	(0.026)	(0.023)	(0.024)	(0.026)	(0.021)	(0.034)	(0.022)	(0.024)
Limited English	0.019	0.019	0.009	0.009	-0.026	-0.031**	-0.043*	0.020	0.000
Proficiency	(0.015)	(0.015)	(0.010)	(0.009)	(0.024)	(0.016)	(0.026)	(0.014)	(0.011)
Baseline ELA Test	-	0.052	-	-0.042	-	-	0.044	-	0.013
Score		(0.063)		(0.050)			(0.089)		(0.057)
Baseline Math Test	-	0.086	-	-0.009	-	-	0.081	-	-0.078
Score		(0.061)		(0.056)			(0.084)		(0.058)
Baseline Writing	-	-	-	0.031	-	-	-	-	0.034
Composition Test Score				(0.052)					(0.057)
Baseline Writing	-	-	-	-0.062	-	-	-	-	0.010
Topic Test Score				(0.053)					(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 recieved 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 IVA: Lottery Results Using Ever Offer

			Charter Schools					Pilot Schools				
			Basic controls			2SLS w/ controls		Basic controls			2SLS w/ controls	
			First Stage	Reduced Form	2SLS	demographics	demographics + baseline	First Stage	Reduced Form	2SLS	demographics	demographics + baseline
Level	Subject		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Elementary School												
	ELA		-	-	-	-	-	2.945***	0.209**	0.071**	0.062**	-
								(0.189)	(0.084)	(0.028)	(0.026)	
		N							1141		1141	
	Math		-	-	-	-	-	2.950***	0.110	0.037	0.033	-
								(0.190)	(0.085)	(0.029)	(0.028)	
		N							1139		1139	
Middle School												
	ELA		1.000***	0.253***	0.253***	0.203***	0.198***	1.526***	0.022	0.014	0.010	-0.041
			(0.099)	(0.066)	(0.067)	(0.056)	(0.047)	(0.172)	(0.065)	(0.042)	(0.040)	(0.103)
		N		3157		3157	3101		4314		4314	3024
	Math		0.967***	0.401***	0.415***	0.376***	0.359***	1.450***	-0.065	-0.045	-0.041	-0.223**
			(0.094)	(0.065)	(0.067)	(0.059)	(0.048)	(0.167)	(0.064)	(0.044)	(0.041)	(0.090)
		N		3317		3317	3258		4777		4777	3348
High School												
	ELA		0.550***	0.089	0.162	0.178*	0.265***	0.683***	0.005	0.007	-0.011	-0.058
			(0.154)	(0.072)	(0.110)	(0.098)	(0.076)	(0.107)	(0.055)	(0.079)	(0.075)	(0.062)
		N		1473		1473	1401		1034		1034	978
	Math		0.543***	0.190**	0.350***	0.368***	0.364***	0.680***	0.007	0.010	-0.033	0.021
			(0.152)	(0.084)	(0.118)	(0.114)	(0.085)	(0.107)	(0.069)	(0.099)	(0.101)	(0.067)
		N		1455		1455	1432		1022		1022	1009
	Writing Topic		0.551***	0.167*	0.303**	0.319***	0.349***	0.674***	0.126**	0.186**	0.170**	0.158*
			(0.155)	(0.089)	(0.128)	(0.120)	(0.120)	(0.108)	(0.058)	(0.086)	(0.085)	(0.081)
		N		1461		1461	1386		1023		1023	961
	Writing		0.551***	0.106	0.192	0.214*	0.212**	0.674***	0.108*	0.161*	0.150	0.138
	Composition		(0.155)	(0.075)	(0.116)	(0.115)	(0.104)	(0.108)	(0.063)	(0.094)	(0.093)	(0.085)
		N		1461		1461	1386		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 recieving 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 IVB: Charter Lottery Results Using Initial Offer

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

			Charter Schools				Pilot Schools				
			Basic controls			2SLS w/ controls		Basic controls			2SLS w/ controls	
			First Stage	Reduced Form	2SLS	demographics	demographics + baseline	First Stage	Reduced Form	2SLS	demographics	demographics + baseline
Level	Subject		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Elementary School												
	ELA		-	-	-	-	-	0.632***	0.043**	0.068**	0.058**	-
								(0.041)	(0.018)	(0.028)	(0.026)	
		N							1141		1141	
	Math		-	-	-	-	-	0.633***	0.019	0.031	0.025	-
								(0.041)	(0.019)	(0.029)	(0.029)	
		N							1139		1139	
Middle School												
	ELA		0.415***	0.090***	0.217***	0.171***	0.181***	0.408***	0.012	0.028	0.030	-0.033
			(0.041)	(0.025)	(0.058)	(0.048)	(0.042)	(0.038)	(0.012)	(0.029)	(0.026)	(0.074)
		N		3157		3157	3101		4314		4314	3024
	Math		0.415***	0.142***	0.342***	0.309***	0.312***	0.400***	0.002	0.004	0.010	-0.127**
			(0.041)	(0.025)	(0.058)	(0.051)	(0.043)	(0.039)	(0.011)	(0.028)	(0.025)	(0.057)
		N		3317		3317	3258		4777		4777	3348
High School												
	ELA		0.257***	0.038	0.146	0.160	0.264***	0.325***	0.000	0.001	-0.013	-0.061
			(0.071)	(0.033)	(0.109)	(0.102)	(0.081)	(0.051)	(0.027)	(0.080)	(0.078)	(0.065)
		N		1473		1473	1401		1034		1034	978
	Math		0.253***	0.088**	0.349***	0.366***	0.354***	0.327***	0.003	0.011	-0.029	0.016
			(0.071)	(0.038)	(0.120)	(0.118)	(0.086)	(0.051)	(0.033)	(0.100)	(0.102)	(0.069)
		N		1455		1455	1432		1022		1022	1009
	Writing Topic		0.258***	0.083**	0.321**	0.331***	0.370***	0.320***	0.061**	0.190**	0.176**	0.161**
			(0.072)	(0.042)	(0.131)	(0.125)	(0.125)	(0.051)	(0.027)	(0.087)	(0.086)	(0.081)
		N		1461		1461	1386		1023		1023	961
	Writing Composition		0.258***	0.050	0.193	0.209*	0.216**	0.320***	0.056*	0.175*	0.167*	0.157*
			(0.072)	(0.035)	(0.119)	(0.120)	(0.108)	(0.051)	(0.030)	(0.096)	(0.096)	(0.087)
		N		1461		1461	1386		1023		1023	961

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 V: Attrition

			Charter Schools		Pilot Schools		
		Prop of non-offered with MCAS	Attrition Differential		Prop of non-offered with MCAS	Attrition Differential	
			Demographics	Demographics + Baseline Scores		Demographics	Demographics + Baseline Scores
Level	Subject	(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*	0.030	0.801	0.014	0.010
			(0.020)	(0.020)		(0.021)	(0.023)
	N		3766	3693		4778	3084
	Math	0.816	0.036*	0.034*	0.790	0.012	0.006
			(0.020)	(0.020)		(0.020)	(0.024)
	N		3934	3852		5846	3788
High School							
	ELA	0.785	0.001	-0.002	0.775	0.039	0.061**
			(0.026)	(0.026)		(0.025)	(0.024)
	N		1849	1752		1301	1215
	Math	0.778	0.000	-0.001	0.765	0.034	0.046*
			(0.026)	(0.026)		(0.025)	(0.025)
	N		1849	1813		1301	1273
	Writing Topic and Writing Composition	0.778	0.000	-0.001	0.765	0.034	0.054**
			(0.026)	(0.027)		(0.025)	(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 recieved 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 VI: School Switching Regressions

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

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

Notes: This table shows results 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-meaned 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 VIII: Effects of Lottery and non-Lottery Schools

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

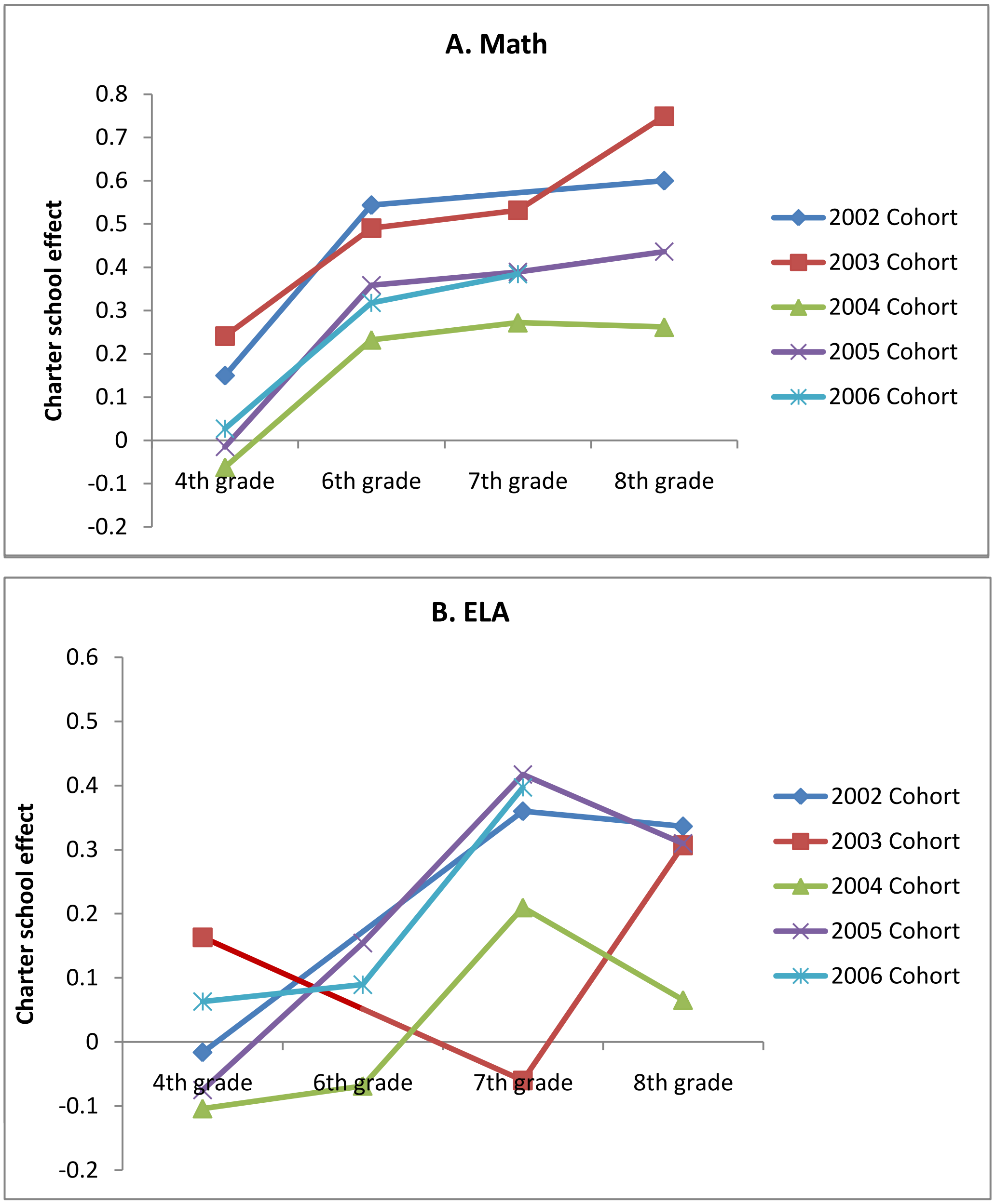


Figure I. Charter Ever Offer Reduced Forms

These figures plot reduced form ever offer math and 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 7th grade in the 2002 cohort are interpolated, as no 7th grade math test was given for this cohort.

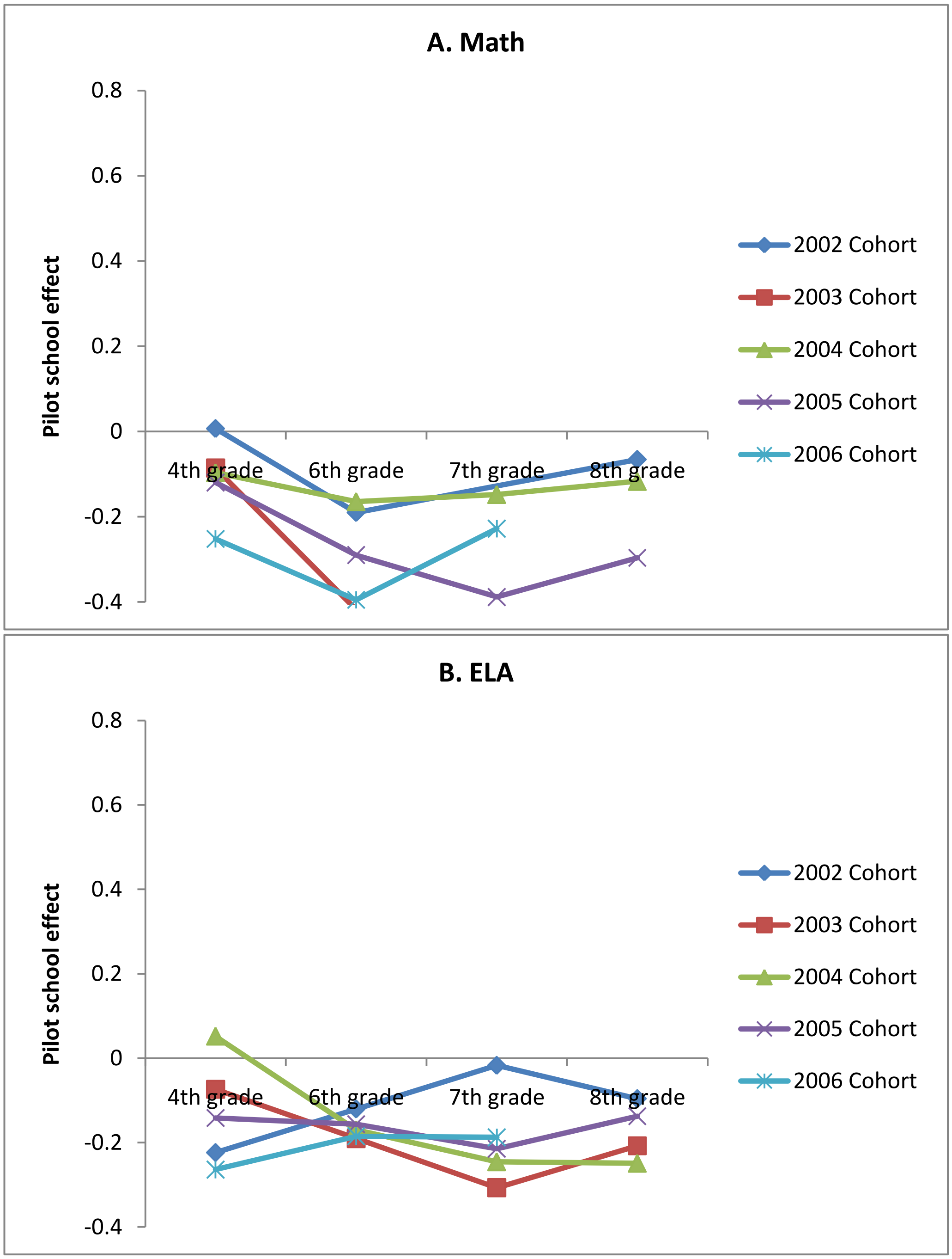
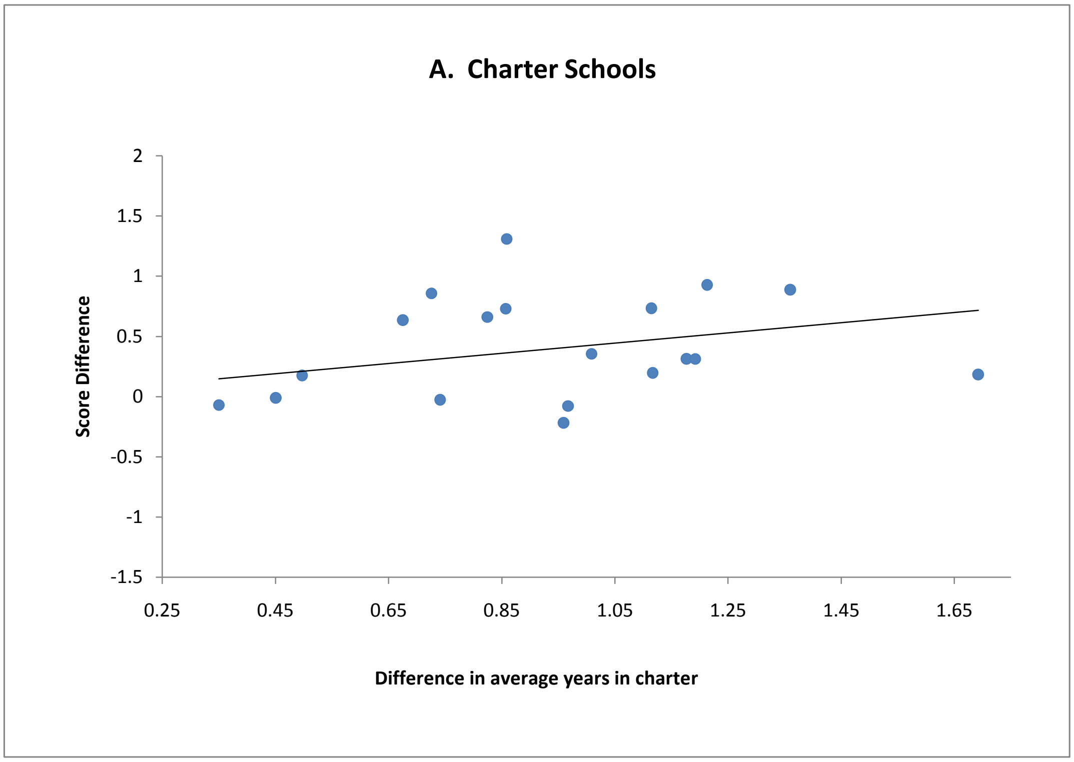


Figure II. Pilot Reduced Forms

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.



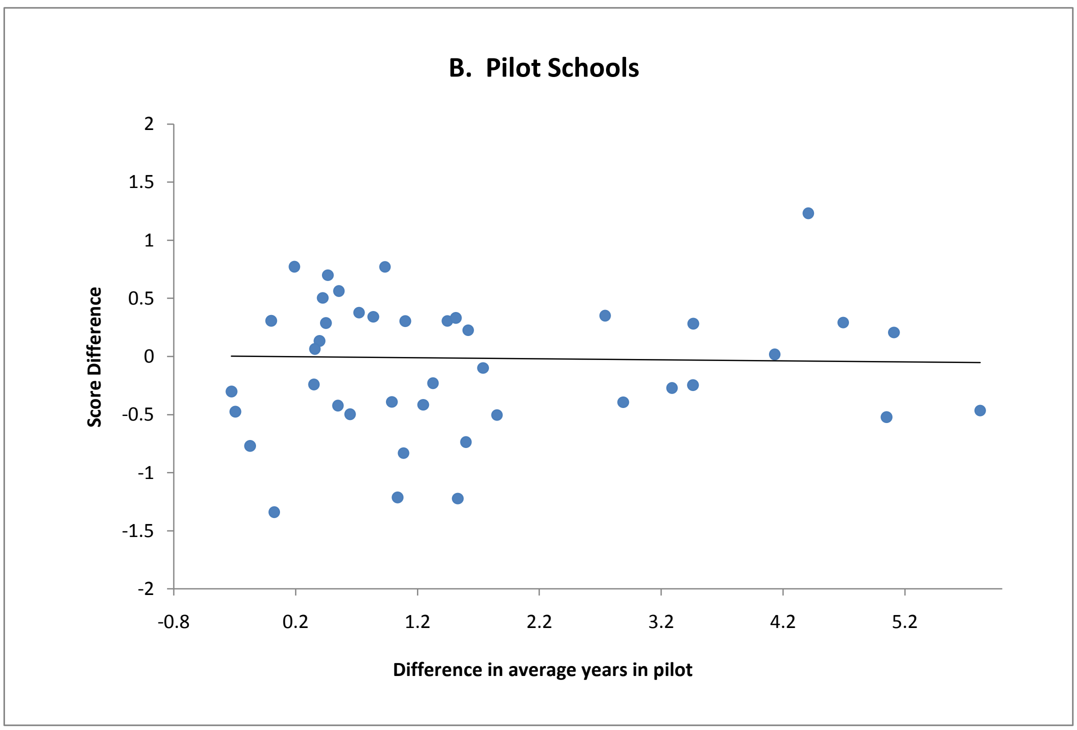


Figure III. VIV Estimates of Middle School Math Effects

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.