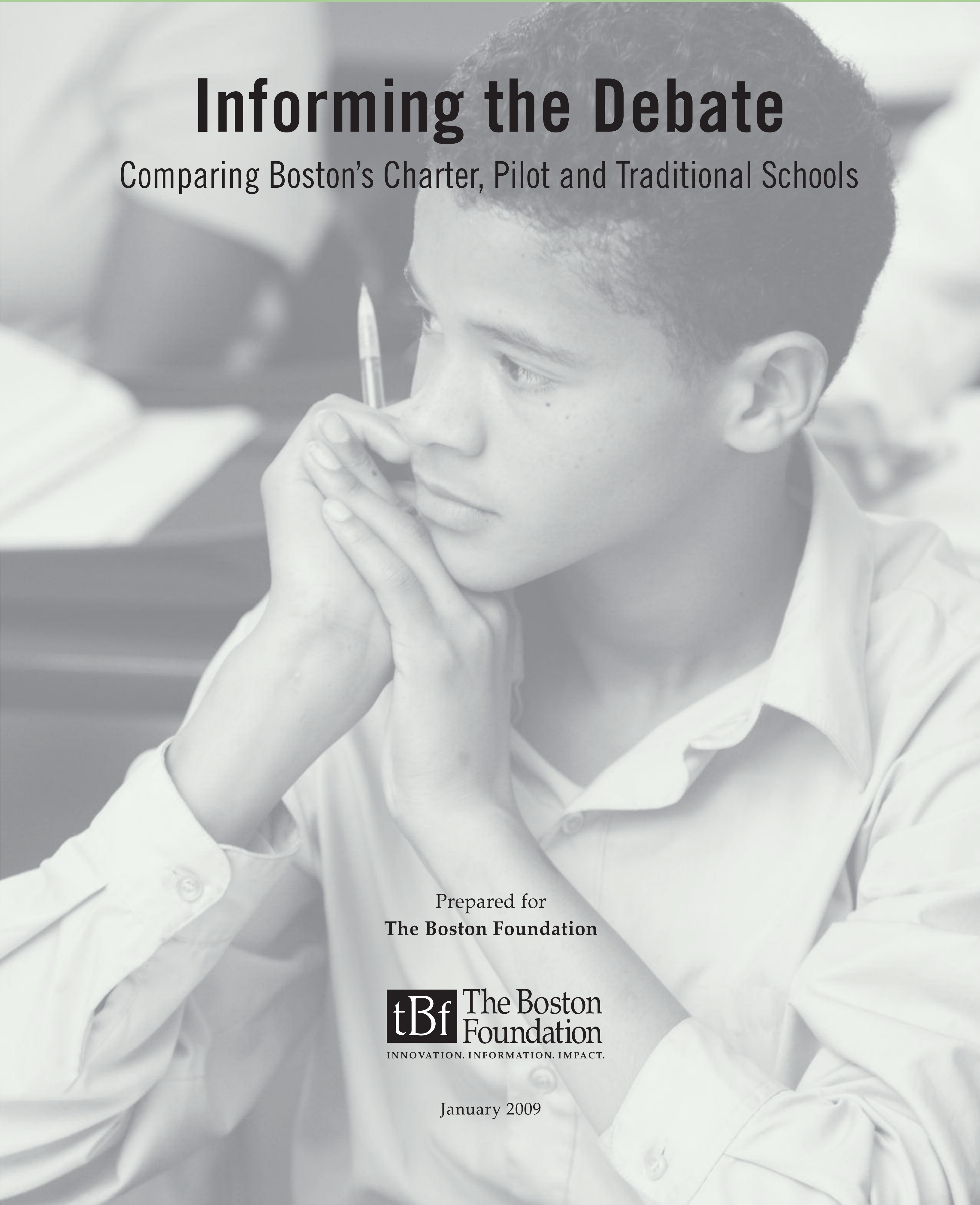
UNDERSTANDING BOSTON



Informing the Debate

Comparing Boston's Charter, Pilot and Traditional Schools

Prepared for
The Boston Foundation

tBf The Boston Foundation
INNOVATION. INFORMATION. IMPACT.

January 2009

The Boston Foundation

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UNDERSTANDING BOSTON is a series of forums, educational events, and research sponsored by the Boston Foundation to provide information and insight into issues affecting Boston, its neighborhoods, and the region. By working in collaboration with a wide range of partners, the Boston Foundation provides opportunities for people to come together to explore challenges facing our constantly changing community and to develop an informed civic agenda.

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Informing the Debate

Comparing Boston's Charter, Pilot and Traditional Schools

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Mitchell D. Chester, Ed.D.
Commissioner

Dear Colleagues:

Our goal as a Commonwealth is to increase student performance to a higher level than ever before, and to do that we need to develop strategies to reconfigure the business of public education.

The promise of Charter and Pilot Schools is that their increased flexibility and autonomy may represent the structural breakthrough we need to achieve our ambitious goal. This study helps us to examine whether these structural differences matter for student outcomes, and to quantify that impact for both Charter and Pilot schools.

The results of this study are both statistically significant and educationally important. But they also open many further questions. What is causing the differences in performance we see between Charters and Pilots? What is it about Charter Schools that allows them to achieve such strong results, and how can their effective practices be more widely disseminated? How can Pilot Schools take better advantage of the autonomy they already have to produce improved outcomes? When is more autonomy a good solution for improving student performance, and when might other strategies make more sense?

With Governor Patrick's recent proposal for Readiness Schools on the table, this is an opportune time for the state to try to answer these questions, and the results of this study are a first step on this path. I look forward to working with educational stakeholders statewide to learn more about how we can use the elements of school redesign to increase student performance.

Mitchell D. Chester, Ed.D.
Commissioner of Elementary and Secondary Education
Commonwealth of Massachusetts

The Boston Foundation

INNOVATION. INFORMATION. IMPACT.

Dear Community Members:

The Boston Foundation is proud to partner with the Massachusetts Department of Elementary and Secondary Education to present this comprehensive, in-depth analysis comparing the results of "like" students in Boston's Charter Schools, Pilot Schools and traditional schools. It uses data from the state, allowing the authors to follow individual students over time—and it controls for a dizzying number of factors.

Over the years, the Foundation has supported numerous programs that benefit the Boston Public Schools as a whole, particularly those reforms that encourage innovation toward improved student performance. In recent years, our funding and civic leadership have focused heavily on promoting Charter Schools, which operate independently of the Boston Public Schools and receive their charter from the state, and especially on Pilot Schools, which have many of the same autonomies as Charters, but remain part of the Boston Public Schools. Both Charter and Pilots are freed of many of the regulations that apply to other public schools, and have the flexibility to determine their own budgets, staffing and curricula.

We are delighted that these results show that Charter Schools—at both the middle and high school levels—have a very positive impact on student achievement. The results in math achievement for middle-school students are nothing short of remarkable. We are disappointed that the results for Pilot Schools are both less encouraging—revealing no great advantage for students—and more ambiguous, due to the small sample size available to the authors.

There is no doubt that Pilot Schools deserve further study, especially as more of them open in the coming years. Anecdotal evidence reveals that Pilots, like Charters, are particularly good at unleashing the creativity of both educators and students. And we know that both types of schools are popular with families, who report that they create the kinds of learning environments that benefit students. As this report reveals, it is not an exaggeration to say that Charter Schools in Boston are making real progress in breaking the persistent connection between poverty and poor results. The Boston Foundation joins with the city and the state in continuing in seeking innovative approaches to education in all schools so that we can break that connection for all of Boston's students.

Paul S. Grogan
President and CEO
The Boston Foundation

Acknowledgments

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Contents

Caveats

This report consists of two distinct research designs: an observational study and a lottery study. Together they provide a comprehensive analysis of student achievement in Boston's public schools, including Pilots and Charters. Each design is described in detail on page 8. This study is limited by the constraints of our two research designs. The observational study includes all schools but does not control for unobserved differences in background characteristics. The lottery study controls for all differences in students' background, including unobserved differences, but does not include all schools.

A second caveat relates to the observed control variables used in our study. These include indicators for participation in special education and limited English proficiency. These broad categories may disguise large differences in student groups. Special education students range from those needing intensive all day services to students needing a little extra time in a resource room. English learners may know no English at all or have some proficiency. It is possible that Pilot and Charter Schools serve different proportions of these subgroups. Unfortunately, our state data set does not provide finely detailed breakdowns for these two variables in a manner consistent or comprehensive enough to be useful for this study.

Finally, it also bears emphasizing that our study is not designed to uncover *why or how* Charter Schools and Pilot Schools might change test scores. Rather we focus on the narrower though still important question of *whether* different types of schools produce significant achievement gains. For the moment, we cannot say which educational strategies or characteristics are most valuable in each school setting, though that is a question we hope to address in future work. Thus, it's important to keep in mind the fact that there might be many reasons for a school's success: instructional focus, student/teacher ratios, staff qualifications or background, use of tutors, and length of school day, to name a few.

1. Introduction

Fifteen years ago, lawmakers in Massachusetts sponsored a bold experiment designed to answer this question: If public schools were granted more autonomy to staff their own classrooms, choose their own curricula and manage their own budgets, could they deliver improved student achievement? The first Charter School opened in Boston shortly after the landmark Massachusetts Education Reform Act in 1993.[1] In 1995, the Boston Public Schools (BPS) and the Boston Teacher's Union (BTU) responded by creating their own version of the autonomous school model, known as the "Pilot School." Since then, enrollment has grown rapidly in both types of autonomous schools. Of the public school students in Boston, roughly 17 percent of 10th grade students and 21 percent of those in 7th grade enrolled in Charters and Pilots in the fall of 2007. Fifteen years later, our goal is to assess the impact of these new school models on student achievement.

Charter Schools are public schools that have been freed from many of the regulations and statutes that apply to other public schools. Because their charters are granted by the state, Charter Schools are not subject to the supervision of local school committees or superintendents and their personnel are typically not included in local district collective bargaining agreements. In exchange for the added flexibility, Charter Schools are accountable for producing certain results, which are set forth in the school's charter. Over the past 15 years, of the 75 charters granted, eight (approximately 10 percent) have been surrendered or revoked (Massachusetts Department of Elementary and Secondary Education, 2008b).

Pilot Schools are also public schools that have been granted the flexibility to determine their own budgets, staffing, curricula, and scheduling, but they remain part of the local school district. Although they are generally freed from work-rule provisions, they are subject to the collectively-bargained pay scales and seniority protections. Many perceive Pilot Schools as a middle-ground between traditional public schools and Charter Schools, preserving some of the protections of collective bargaining and local district supervision, while still allowing considerable autonomy on budgets, staffing, and curriculum.

Although Pilots and Charters are increasingly popular with students and parents, there is still no consensus on whether they are producing better results. Proponents claim that the freedom to innovate has led to more personalized schooling options and improved student outcomes (Tung & Ouimette, 2007; Peyser, 2008). Opponents argue that Charters and Pilots skim the best students from the traditional public schools (for instance, see comments in Jan, 2007), drive weaker students back into the traditional public system (or out of school altogether) (Vaznis, 2008), and drain the resource base of traditional schools (Massachusetts Association of Superintendents, 2005). (Although Charter Schools are not supervised by local school committees, their funding is drawn from the revenues of the districts where their students reside, using a formula established by the state.)

The debate over Charter and Pilot Schools has intensified recently. At least seven new Pilot Schools are scheduled to open by September of 2009, and Charter supporters have begun advocating for an increase to the state's current charter cap (see, for instance, Peyser, 2008). In addition, the Pilot School concept is gaining traction outside of Boston as an alternative to Charters. Recently, the Massachusetts Board of Elementary and Secondary Education adopted a Commonwealth Pilot School option for schools that otherwise would have been designated as chronically underperforming. Four schools have become Commonwealth Pilot Schools in Boston, Fitchburg, and Springfield. In addition, the Pilot School model is being tried in other cities such as Los Angeles, California (Manzo, 2007).

Why is there no consensus regarding the success or failure of the Pilot/Charter experiments? How can the jury still be out, given the deluge of MCAS results over the past decade? Indeed, when the MCAS results were recently released, four of the top 10 scoring public middle schools in 8th grade math and three of the top 10 scoring public high schools in 10th grade math were Charters (The Boston Globe, 2008). What is the source of the lingering skepticism?

There are two main reasons that widely published MCAS scores fail to persuade: first, whether admitted by lottery or by an application process, parents and students *volunteer* to attend Charter and Pilot Schools. Precisely because of this volunteer status, there are strong reasons to believe that Pilot and Charter students are not representative of the typical public school students. One possibility is that the volunteers are refugees, those least well-served by the traditional public school system and the most desperate to escape. On the other hand, those parents who are willing to volunteer could be the most engaged in their children's education, willing to drive long distances to help their children achieve. Predictably, Charter/Pilot advocates tend to emphasize the former: to the extent that volunteers are desperate and drawn from the lowest income neighborhoods where many Charters are located, we might expect them to have had even lower achievement than students in the traditional public schools. Just as predictably, critics tend to emphasize the latter: to the extent that Charter/Pilot Schools skim off the most engaged families, we might expect their students to outperform—even if the schools themselves were subpar. Although there is almost surely some truth to both assertions, it is difficult to know which is the predominant one and whether the net effect is to privilege or handicap Charter/Pilot Schools relative to the traditional public schools.

A second reason that the published MCAS results fail to persuade is the suspicion that Charter and Pilot Schools are shedding students who do not perform well. Although all Charter Schools and many Pilot Schools are prevented from hand-picking students at admission time, some students do subsequently drop-out and return to traditional public schools. This dropping-out has led to the charge that Charter and Pilot Schools are retaining only the highest achieving of their students, leaving the traditional schools to deal with the low-achieving students who could not perform up to standards.

In this report, we take two different approaches to resolving the doubts surrounding the impacts of Charter and Pilot Schools. First, we use newly-available data from the Commonwealth of Massachusetts' data system to follow individual students over time, and use those data to control for each student's achievement, demographics, and program participation *prior* to attending a Charter or Pilot School. In other words, we compare Charter and Pilot students to traditional public school students who had similar academic achievement and other traits during an earlier school year (8th grade for high school students and 4th grade for middle school students). We refer to these results as the "observational" results since they rely on using *observed* student traits for all Boston-area students, whether or not they applied to Pilot or Charter Schools, and then controlling for these traits.

As we discuss below, one weakness of this approach is that there may be important differences between the Pilot/Charter students and traditional school students that are *not captured* by prior academic achievement or demographics. For instance, it is hard to imagine that one could measure the many dimensions of family background sufficiently well to satisfy a skeptical audience. Simple indicators of race or ethnicity or participation in federal lunch programs capture very few of the nuances that differentiate families. As a result, one cannot control for all the variables that critics and advocates are likely to cite for generating misleading results.

Therefore, in a second approach, we use the fact that the school assignment process is based on lotteries. This research design compares the outcomes of those who were offered a slot in a Charter or Pilot school to those who applied to the same schools and were not offered a slot. As long as these lotteries were fair (and we see little evidence to suggest that they were not), we can expect the two groups to be similar in all ways, measured and unmeasured. Within the pool of applicants subject to the lotteries, any subsequent difference in performance between those "lotteried" into and out of a given school might plausibly be attributed to the effect of having been offered a chance to attend. We refer to these results as "experimental" or "lottery-based" results since they rely on comparisons of those lotteried into and out of Pilots and Charters.

This lottery-based approach is a very strong research design. When evaluating these results, however, it's important to keep in mind that our lottery study necessarily includes only schools and years for which the applicant lotteries were both oversubscribed and carefully documented. If oversubscription and good record keeping are signs of school quality, the results from our lottery study may be somewhat better than we should expect from a broader sample of schools. In ongoing research, we are investigating this possibility by collecting additional data and looking at the determinants of individual schools' MCAS success.

The benefits and costs of each study design are summarized in the accompanying text box.

Summary of Findings

Whether using the randomized lotteries or statistical controls for measured background characteristics, we generally find large positive effects for Charter Schools, at both the middle school and high school levels. For each year of attendance in middle school, we estimate that Charter Schools raise student achievement .09 to .17 standard deviations in English Language Arts and .18 to .54 standard deviations in math relative to those attending traditional schools in the Boston Public Schools. The estimated impact on math achievement for Charter middle schools is extraordinarily large. Increasing performance by .5 standard deviations is the same as moving from the 50th to the 69th percentile in student performance. This is roughly half the size of the black-white achievement gap. In high school, the estimated gains are somewhat smaller than in middle school: .16 to .19 standard deviations in English Language Arts; .16 to .19 in mathematics; .2 to .28 in writing topic development; and .13 to .17 in writing composition with the lottery-based results. The estimated impacts of middle schools and high school Charters are similar in both the "observational" and "lottery-based" results.

Unfortunately, the results for Pilot Schools are more ambiguous and deserve further study. In the elementary grades, the estimated impact of Pilots was positive in English language arts (.09), but not statistically different from zero in mathematics. In the middle school grades, the observational results suggest that Pilot School students *may* actually *lose ground* relative to traditional public school students, with point estimates of -.05 standard deviations per year in English Language Arts and -.07 in math. However, our lottery-based results suggest that the performance of Pilot School students is not statistically distinguishable from zero. At the high school level, Pilot impacts are somewhat more encouraging but still ambiguous. The estimates based on statistical controls are positive and generally similar in magnitude to those of Charter Schools. However, the estimated impacts of Pilot high schools using the lotteries are not statistically significantly different from zero.

In the sections below, we first describe the policy environment in which Charters and Pilots operate and later describe the data and our results.

Benefits and Costs of The Two Study Designs

When weighing the evidence in this report, it's important to keep in mind the costs and benefits of the two types of research design used to construct our statistical estimates.

	Benefits	Costs
Observational Study	• Large and representative sample including all schools in operation • Controls for a wide range of *observed* differences between students, including past test scores	• This study design does not control for *unobserved* background differences between students who attend different types of schools (e.g., differences in family background or student motivation)
Lottery Study	• Eliminates *both observed and unobserved* background differences between the students who attend different types of schools	• This study design includes only schools and years in which the demand for seats exceeds the supply and for which historical lottery records are available and complete

2.
Charter and Pilot Schools

Charter Schools are public schools that operate under a charter granted by the state's Board of Elementary and Secondary Education. Each school is managed by a board of trustees and is independent of any local school committee (603 CMR 1.02).

There are two types of Charter Schools: Commonwealth Charters and Horace Mann Charters. There are two key differences between them. First, in addition to being approved by the state board, Horace Mann Charter Schools must be approved by the local school committee and the local teachers union (603 CMR 1.04 (2)). (In contrast, Commonwealth Charters apply directly to the state board and do not require local school committee or teacher union approval.) Second, the employees of Horace Mann Charter Schools typically continue to be members of the collective bargaining unit, to accrue seniority, and to receive, at minimum, the salary and benefits established by the local collective bargaining unit (603 CMR 1.02). In contrast, employees of Commonwealth Charter Schools are not required to be members of the local collective bargaining unit.

State law caps the number of Charter Schools at 120 (48 Horace Mann Charter Schools and 72 Commonwealth Charter Schools). There are far fewer Horace Mann Charters than allowed by law (seven rather than 48). However, although the number of Commonwealth Charters is also somewhat below the cap (54 rather than 72), their numbers are effectively capped in many regions of the state. The state board is required to grant at least three charters in school districts with below average MCAS scores before any additional charters can be granted. Moreover, tuition paid by school districts to Charters cannot exceed nine percent of that district's net school spending. As a result, as of fall 2008, no new Charter Schools are being approved for Boston (Massachusetts Department of Elementary and Secondary Education, 2008a, Appendix B). **Table 1** presents basic information about the number of Charter Schools in Massachusetts.

Funding for Charter Schools comes largely from the school districts where their students reside. (Some Charter Schools also raise funds from individuals and foundations, but they are prohibited from charging students an application fee or tuition (603 CMR 1.03 3)).

TABLE 1

Charter School Facts, 2007-2008

Number of Operating Commonwealth Charter Schools	54
Number of Operating Horace Mann Charter Schools	7
Total Number of Operating Charter Schools	61
Maximum Enrollment Allowed by Charters Operating, 2007-2008	30,034
Total Students Attending Charter Schools, 2007-2008	25,034
Total Students on Charter School Waiting Lists for 2008-2009, March 2008	21,312
Percent of PK-12 Public School Population in Charter Schools, 2007-2008	2.60%
School Type	
Elementary	6
Elementary-Middle	13
Middle	10
Middle-High	17
High	7
K-12	8
Location	
Boston	16
Urban, Non-Boston	25
Other	20
School Size, Maximum	
Less than 100	1
100-300	18
301-500	25
501-1000	12
More than 1,000	5
Regional	
Yes	22
No	39

Note: Charter School waitlists include duplicate students who applied to more than one Charter School.

Two schools are classified as both Horace Mann Charter Schools and Pilot Schools.

Source: MA DOE (2008) Massachusetts Charter Schools Fact Sheet, www.doe.mass.edu/charter/factsheet.pdf

Commonwealth Charter Schools are funded on a per pupil rate based on a formula set by the state.[2] Horace Mann Charter Schools must submit annual budget requests to the local superintendent and school committee. Budget allocations for Horace Mann Charter Schools must be consistent with the allocations for other public schools in the district, and any reductions to a Horace Mann Charter School's budget must be proportionate to reductions for other schools in the district (603 CMR 1.08). For students at either type of Charter School, school districts must provide transportation options similar to those provided for regular public school students. These funding requirements have proven to be controversial. Sending districts claim that the revenue lost to Charter Schools is much greater than the amount they save by serving fewer students (Massachusetts Association of School Superintendents, March 2005).

A Charter School is prohibited from administering admission tests to applicants or basing their admission decisions on prior academic achievement (603 CMR 1.06 2). When oversubscribed, a Charter School must hold a lottery. All lotteries must be conducted in public with a "disinterested party" selecting the names at random. The names of the applicants not selected for enrollment are placed on a waiting list in the order drawn, and these applicants may be offered admission as spaces become available due to lottery winners declining admission. Applicants with siblings already enrolled in the Charter School have preference in the lottery. Waitlisted applicants are offered a space based upon their lottery number, except when the enrollment of a non-sibling applicant would exceed the sending district's charter tuition cap, in which case the student will be skipped over but kept on the waiting list (603 CMR 1.06 4d).[3]

Charter Schools must renew their charter every five years. During the intervening years Charters are held accountable via annual reports, financial audits, and site visits. **Table 2** presents data on the Charter School application and accountability process.

In the renewal application, schools must show that the their academic program is a success, the school is a viable organization, and the school is faithful to its charter (MA DOE, 2006). Applications for renewal of Horace Mann Charter Schools are similar to those for Commonwealth Charter Schools, with the additional stipulation that they are approved by the local teachers union and local school committee.

TABLE 2

Charter School Accountability Information

Total Charter Applications Received Since 1994	336
Total Number of Charters Granted Since 1994	75
Charter Schools Operating on 5-Year Only Renewals	16
Charter Schools Operating on 5- or 10-Year Renewals	30
Charter Schools Less than 5 Years Old	17
Charter Schools Closed or Never Opened since 1994	12
Reason for Closing	
Pre-Opening Surrender	4
Post-Opening Surrender	4
Revocation During 1st Charter	2
Non-Renewal	2

Note: Total number of charters granted includes 2 charters approved but not yet opened.

Source: MA DOE (2008) Massachusetts Charter Schools Fact Sheet, www.doe.mass.edu/charter/factsheet.pdf

Since 2000, all Charter School teachers have had to pass the Massachusetts Tests for Educator Licensure (MTEL) within one year of employment, or be certified to teach in Massachusetts prior to employment (603 CMR 1.07).

Pilot Schools were jointly created by the Boston Public Schools (BPS) and the Boston Teachers Union in 1995 to serve as an alternative to both Charters and traditional public schools. Pilot Schools are in many ways similar to Horace Mann Charter Schools. While Pilot Schools have a high degree of autonomy over their budgets, staffing, governance, curriculum/assessment, and the school calendar, they are subject to a review every five years (not unlike a charter renewal review). In addition, each new Pilot School must be approved by both the school district and the teachers union (CCE, 2006 March). Moreover, as with Horace Mann Schools, funding for Pilot Schools must be approved by the BPS, their employees retain their seniority, and the BPS pay scale serves as a minimum for each teacher's pay.

As of the 2007-2008 school year, there were 20 operating BPS Pilot Schools. At least seven new Pilot Schools are planned to open by September of 2009. **Table 3** displays general information about the 20 operating Pilot Schools in Boston.

TABLE 3

Boston Pilot School Characteristics, 2005-2006

Number of Pilot Schools	20
Total Students Attending Pilot Schools	6,337
Percent of PK-12 Boston Public School Population in Pilot Schools	11%
School Type	
Elementary	4
Elementary-Middle	4
Middle	2
Middle-High	1
High	9
K-12	0
School Size, Maximum	
Less than 100	0
100-300	11
301-500	7
501-1000	2
More than 1,000	0
Pilot Status	
Start-Up	13
Conversion	7

Note: Two schools are classified as both Horace Mann Charter Schools and Pilot Schools.

Source: Center for Collaborative Education (2006, September). The Essential Guide to Pilot Schools: Overview. p. 88. Boston, MA. www.ccebos.org/publinks.html

In contrast to Charter Schools, Pilot Schools are, in principle, not required to run lotteries when oversubscribed. This is a key distinction between Pilot and Charter Schools. However, in practice, the enrollment process differs by the school's grade span. Elementary and middle Pilot Schools do participate in the regular BPS school choice process. Under that process, students can be guaranteed admission if they live within a certain distance of the school. As long as a school's enrollment is not filled by these guaranteed students, elementary and middle Pilot Schools are subject to lotteries as part of the BPS choice system. However, at the high school level only two of the Pilot Schools admit via lottery. Another one admits only students who are overage for high school (for instance, over the age of 16 when entering 9th grade)[4]. The remaining five Pilot high schools admit selectively, requiring students to complete applications or, in the case of a school dedicated to the performing arts, complete an audition. These applications and auditions are not supposed to include prior academic performance.

Previous Studies of Massachusetts Pilots and Charters

Until very recently, we could find little rigorous research looking at the efficacy of Pilot and Charter Schools in Massachusetts. For instance, the Center for Collaborative Education, a nonprofit organization that provides technical support for Pilot Schools, has done a number of reports looking at Pilot School results. The most recent of these reports found that students in Pilot high schools have higher MCAS scores, higher attendance rates, and lower drop-out rates than other BPS schools (excluding the BPS exam schools). However, none of the analyses controlled for more than one student characteristic at a time. Thus, it is difficult to tell whether the better outcomes found in the Pilot Schools are due to the schools themselves or to inadequate controls for student background differences (Tung & Ouimette, 2007).

A 2006 study commissioned by the Massachusetts Department of Elementary and Secondary Education examined the relative impact of Charter Schools on student achievement as measured by the MCAS (National Center for the Improvement of Educational Assessment, 2006). This analysis used more sophisticated statistical methods (including individual student growth as a measure of effectiveness) and compared Charter Schools to the districts their students would have attended. Some of the results in that report suggested that there were positive Charter impacts. However, the study was also limited in several ways. First, in analyzing school impact on student growth, individual Charter School outcomes were compared to the district as a whole. We might suspect that elementary, middle, and high school performance differs substantially and should not be lumped together—especially when comparing district performance to a particular middle or high school. Second, student growth was only calculated for students who remained within the Charter School, meaning that if Charter Schools do systematically encourage low-performing students to leave before state testing, the estimated impacts would be too large. Finally, though a number of background characteristics were controlled for in the analysis, there still remains the possibility that there are unobserved characteristics of those who choose Charter

Schools that cause Charter students to perform better or worse than others.

Outside of Massachusetts, more work has been done on the impact of Charter Schools. One study is particularly relevant. In 2007 the New York City Charter Schools Evaluation Project examined charter school outcomes for grades 3 through 8. They used a lottery based approach for an "apples-to-apples" comparison of charter lottery winners and Charter lottery losers and found that the average effect of attending a Charter School was .09 standard deviations to students' math score per year of attendance and .04 standard deviations on students' English score per year of attendance (Hoxby and Murarka, 2007). We use a similar research design in the lottery study portion of our investigation.

3.
Data Collection and Sample

The Massachusetts Department of Elementary and Secondary Education provided us with individual student-level data for all Massachusetts students for the school years 2001-02 through 2006-07. The state student information system contains unique student identifiers, which allow one to track the test performance of individual students across time. In addition to detailed test performance data, these files contain demographic and program participation information (e.g., race/ethnicity, gender, free/reduced lunch status, special education status, and English learner status).

As discussed above, the study team used two strategies to investigate the causal effect of Pilots and Charters. The first relies on statistical controls (each student's performance and demographics prior to the grades studied), to estimate impacts of individual schools. The second exploits the applicant lotteries at Pilots and Charters in a quasi-experimental research design. In the latter study, those not offered the opportunity to attend a Charter and or Pilot provide a comparison group. The data collection and sample construction for these two approaches differ in a number of important respects.

The observational analysis using statistical controls relies on the data provided by the Massachusetts Department of Elementary and Secondary Education. The observational study includes all students in Boston, not just those students applying to Charters and Pilots. Specifically, to be included in the study, students must have: (a) been present in Massachusetts' student information system; (b) have resided in the Boston school district prior to the beginning of the relevant grade span studied (6-8 and 9-12) or at the beginning of elementary school; and (c) attended either a Boston public school (including a Pilot) or a local Charter School. They must also have baseline grade and demographic information including gender, race/ethnicity, date of birth, and program participation information. In addition, we use as outcomes the MCAS scores from 2004 through 2007 in English Language Arts, math, writing topic and writing composition.[5] Students must have had an MCAS test score during the elementary school grades (3, 4), the middle school grades (6, 7, and 8) or in the high school grade (10th grade) when the MCAS is administered. In those statistical models that include prior MCAS scores as a control variable, the sample is limited to those with non-missing prior scores.[6]

In order to account for the possibility that certain types of schools push out students that are poor performers, we assign students to Charter or Pilot Schools no matter how many days or months during the year they attended that school.[7] Thus, a student who attends a Charter School (or Pilot School) for one month but transfers to a traditional BPS school for the remaining school year is assigned to the Charter School (or Pilot School) for the year.[8]

Raw MCAS test scores of Boston students are standardized by grade, subject, and year to have a mean of zero and a standard deviation of one. In order to increase the statistical power of the study, observations are also pooled across years. This means that rather than estimating separate effects for each school year, we use all the information from all years to generate an average effect of each school type.[9]

For the experimental study, the analysis sample includes only Charter and Pilot School applicants. Moreover, their chosen school must have had an *oversubscribed admissions lottery* in the year of application. For Pilot Schools, this means that the school had to participate in the BPS choice plan *and* that more families chose the school as their first choice in the relevant year than there were available slots. Moreover, the schools could not be filled up by those who had been guaranteed a slot.

While state law requires Charter Schools to have an admission lottery, not all Charter Schools are oversubscribed. In other words, some Charter Schools go through their entire initial waiting list by the opening of school in September.[10] In such cases, there was essentially no random assignment and the lottery from a given school in a given year could not be used. In addition, we only included Charter Schools within the City of Boston itself. Although a few Charter Schools outside Boston also accept Boston students, we did not use those lotteries due to the relatively small numbers of such students. However, Boston students who attend nearby Charter Schools outside of the city limits are included in the results with statistical controls.

Under state law, siblings of current Charter students are guaranteed admission to that school and therefore were not subject to the lottery. These students are excluded. Pilot Schools that span school levels guarantee admission to students who enroll in the school at lower grades. We also exclude these non-randomly assigned students.

In addition, we exclude all applicants whose town of residence in the baseline year was not Boston (in order to ensure that the comparison group was the Boston Public Schools). However, in order to minimize any bias due to attrition, we retain all applicants with outcomes in the final estimate of the Charter School effect—whether or not they remained in Boston at the time of testing.

Moreover, we limit the sample to those attending a Boston Public School or a Charter School in the baseline year (4th grade for the middle school result, 8th grade for the high school results). In other words, we exclude those who may have been applying to a lotteried school from a private school. If a student had been attending a private school at the time of application, we would observe her outcome in the state's public school database only if she won the lottery and subsequently enrolled in a Charter School. We would lose track of any of her counterparts who lost the lottery and subsequently went back to the private school. Therefore, to ensure a balanced sample of lottery winners and losers to study, we dropped applicants who we do not observe enrolled in BPS or Charter Schools in the baseline year.

Four of the seven Pilots that cover middle school grades are K-8 schools. In order to ensure that our lottery estimates of Pilot middle school impacts are not distorted due to focusing only on the minority of Pilot Schools that cover only the middle grades, we include in the middle school Pilot lottery sample winners and losers of K-2 lotteries from the years 1997 to 2000 as well. Because these years are outside the range of the data provided by the Massachusetts Department of Elementary and Secondary Education, we relied on choice and enrollment data provided by BPS to track these applicants.[11]

Finally, we did not attempt to evaluate the effects of Charter Schools at the elementary level. Because of the time between the lotteries and state testing (four or five years, depending on whether the Charter admits in pre-K or K), not all Charters maintained adequate historical records for the study, and we had some additional difficulty gathering existing data. As a result, in conjunction with Massachusetts Department of Elementary and Secondary Education, we decided not to pursue Charters at the elementary level.

A more detailed discussion of the lottery procedures and how they were used is contained in Section 6 below. Also, see Table A.1 in the appendix for details on students included in the samples.

Table 4 contains descriptive statistics of our sample, including demographic information and raw standardized baseline MCAS scores. The first column contains information about the students in "traditional" Boston public schools. This excludes students in Pilot Schools, Charter Schools, exam schools, and alternative schools.[12] Information about students in these schools is presented in columns two through five.

Pilot School students are less likely to be Hispanic and, in middle and high school, less likely to be special education students than students in traditional BPS schools. Pilot Schools also tend to serve a lower percentage of English learners in the elementary and high school grades. Students going to Pilot high schools enter with substantially higher test scores than their peers in traditional schools (.341 standard deviations higher on average in math and .328 standard deviations higher in ELA).

Charter School students are more likely to be African American and somewhat less likely to be Hispanic than traditional BPS students. In high school, Charter Schools serve substantially more girls than traditional schools (60% vs. 51%). Charter Schools also serve a smaller proportion of special education students, free- and reduced-price lunch students, and English learners than do the traditional BPS schools. In addition, high school Charter students tend to come in with substantially better math and ELA performance on the MCAS than those in traditional BPS schools (.412 standard deviations higher in math and .412 standard deviations higher in ELA).

In column (6) of Table 4, we examine whether those who apply to Pilot Schools as their first choice are different from the overall population in traditional BPS schools. It is notable that applicants look quite different across

TABLE 4

Descriptive Statistics

	Traditional BPS Schools	Enrolled in Pilot, Charter, Exam or Alternative School				All Applicants	Applicants in Lottery Study	
		Pilot	Charter	Exam	Alternative	Pilot	Pilot	Charter
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Elementary School (3rd and 4th grades)								
Female	47.9%	46.9%	52.8%	-	14.3%	53.6%	52.9%	-
Black	44.1%	41.8%	75.4%	-	50.0%	49.6%	55.0%	-
Hispanic	34.9%	31.1%	13.7%	-	21.4%	19.7%	20.1%	-
Special education	10.1%	9.7%	4.8%	-	60.7%	5.8%	6.3%	-
Free or reduced price lunch	83.1%	66.8%	66.0%	-	82.1%	58.5%	65.2%	-
Limited English proficiency	33.4%	20.9%	3.9%	-	17.9%	7.3%	7.5%	-
Number of students	5,651	373	415	-	28	468	333	-
Middle School (6th, 7th, and 8th grades)								
Proportion of K2 applicants	-	-	-	-	-	15.1%	20.8%	-
Female	47.0%	49.5%	49.5%	55.2%	16.8%	51.2%	53.5%	48.8%
Black	47.1%	51.2%	70.4%	27.3%	67.3%	48.6%	49.9%	60.1%
Hispanic	36.7%	26.9%	18.1%	16.1%	20.2%	23.6%	30.4%	17.1%
Special education	23.9%	20.5%	18.2%	2.6%	78.4%	13.6%	14.9%	18.3%
Free or reduced price lunch	89.1%	85.3%	73.0%	66.0%	90.4%	76.6%	76.8%	68.2%
Limited English proficiency	20.4%	19.1%	6.1%	8.3%	8.7%	11.8%	12.9%	5.1%
4th Grade Math Score*	-0.110	-0.156	-0.084	1.259	-0.581	0.111	-0.039	0.173
4th Grade ELA Score*	-0.100	-0.100	0.084	1.080	-0.781	0.147	0.000	0.277
Number of students	9,768	2,070	2,034	1,599	208	3,828	1,320	953
High School (10th grade)								
Female	50.8%	51.2%	59.9%	59.3%	29.1%	-	43.4%	59.3%
Black	51.4%	53.4%	67.7%	24.5%	68.5%	-	57.7%	68.8%
Hispanic	35.4%	25.6%	14.3%	11.6%	17.3%	-	23.1%	21.6%
Special education	23.1%	17.6%	15.7%	0.8%	70.1%	-	13.5%	15.5%
Free or reduced price lunch	84.3%	75.4%	65.7%	49.4%	71.3%	-	75.7%	75.3%
Limited English proficiency	19.9%	8.2%	3.8%	2.8%	7.9%	-	5.6%	4.3%
8th Grade Math Score	-0.278	0.063	0.134	1.273	-0.573	-	0.181	0.089
8th Grade ELA Score	-0.177	0.151	0.235	1.049	-0.675	-	0.207	0.179
Number of students	7,443	1,546	915	3,288	254	-	679	1,480

Notes: The table reports sample means in baseline years by school type for the following groups corresponding to column numbers:

Students must have at least one MCAS score to be included in the table.

1. BPS students excluding exam, alternative, Charter and Pilot students from 2004 to 2007
2. Students enrolled in Pilot Schools from 2004 to 2007
3. Students enrolled in Charter Schools from 2004 to 200
4. Students enrolled in exam schools from 2004-2007
5. Students enrolled in alternative schools from 2004-2007
6. Pilot applicant cohorts: elementary school applicants in 2002 and 2003; applicants to middle school in 2002-2006, 6th grade applicants to K-8 schools 2003-2006, K2 applicants to K-8 schools 1997-2000. We do not include "all applicants" for high schools, as we did not have access to application lists for Pilots that accepted students by application, not the Boston Choice Program.
7. Pilot applicant cohorts in randomized lotteries: elementary school applicants in 2002 and 2003; applicants to middle school in 2002-2006, 6th grade applicants to K-8 schools 2003-2006, K2 applicants to K-8 schools 1997-2000; and high school applicants in 2003-2005
8. Charter applicant cohorts in randomized lotteries: middle school students in 2002-2006, and high school students in 2002-2005

Demographic characteristics are taken from grade K for elementary school students, grade 4 for middle school students, and grade 8 for high school students. All students reside in Boston and be enrolled in BPS or a Charter School in the baseline year.

*only for 6th grade applicants

Demographic information comes from the state data, except for K2 applicants to K-8 schools. For these students, demographic information comes from BPS

TABLE 5

Score Differences Relative to BPS without Charters and Pilots

Level				Difference						
			Traditional BPS Schools	Enrolled in Pilot, Charter, Exam or Alternative School				All Applicants	Applicants in Lottery Study	
Subject	Grade	Test Years	(1)	Pilot (2)	Charter (3)	Exam (4)	Alternative (5)	Pilot (6)	Pilot (7)	Charter (8)
Elementary School (3rd and 4th grades)										
ELA										
	3rd	2005-2007	0.026	0.164	0.435	-	-0.332	0.401	0.302	-
	4th	2005-2007	0.110	0.066	0.123	-	-0.976	0.461	0.338	-
Math										
	3rd	2005-2007	-0.032	0.329	0.359	-	-0.968	0.422	0.295	-
	4th	2005-2007	0.137	-0.022	-0.103	-	-1.050	0.276	0.199	-
Middle School (6th, 7th, and 8th grades)										
ELA										
	6th	2005-2007	0.035	-0.067	0.231	-	-0.976	-0.035	-0.035	0.340
	7th	2005-2007	-0.163	-0.088	0.520	1.093	-1.076	0.163	0.163	0.604
	8th	2006-2007	-0.167	-0.110	0.622	1.138	-0.609	0.167	0.167	0.609
Math										
	6th	2004-2007	0.009	-0.006	0.360	-	-0.646	-0.009	-0.009	0.545
	7th	2005-2007	-0.272	-0.134	0.740	1.440	-0.672	0.272	0.272	0.798
	8th	2006-2007	-0.236	-0.155	0.818	1.398	-0.620	0.236	0.236	0.754
High School (10th grade)										
ELA										
	10th	2004-2007	-0.280	0.463	0.629	1.269	-0.535	-	0.519	0.482
Math										
	10th	2004-2007	-0.358	0.359	0.575	1.513	-0.400	-	0.515	0.405
Writing Topic										
	10th	2004-2007	-0.252	0.400	0.564	1.023	-0.470	-	0.490	0.456
Writing Composition										
	10th	2004-2007	-0.217	0.414	0.585	0.932	-0.354	-	0.486	0.479

Notes: This table reports the raw test scores standardized by grade, subject, and year for the same groups as identifed in Table 4.
All students have demographic characteristics and reside in Boston and are enrolled in BPS or a Charter School in the baseline year.

almost every measure from those in traditional BPS schools.[13]

Finally, columns (7) and (8) look at applicants to schools in our experimental sample (that is, oversubscribed Pilot and Charter Schools that hold lotteries and are located within the City of Boston). These columns suggest that the applicants in our sample are indeed different from students in traditional schools. In high schools, both Pilot and Charter applicants have substantially higher 8th grade MCAS scores. Middle school Charter and Pilot applicants also have higher test scores prior to middle school entry.

In **Table 5** we examine student achievement outcomes for students in the various types of schools. The first column shows the average scores for students in traditional BPS schools. Columns (2) through (8) show performance of students in different schools and samples *relative to the performance of BPS students*. In other words, high school Pilot students perform .46 standard deviations better on the ELA MCAS than those in traditional public schools.

From this table we can see that students in elementary Pilots and Charters generally perform better than those in "traditional schools." In middle schools, Charter students far outperform students in traditional schools, while Pilot students seem to perform at slightly lower levels than other BPS students. In high schools, both Pilot and Charter students substantially outperform students in traditional BPS schools.

We should not, however, take this alone as evidence that Pilot and Charter Schools are either effective or ineffective. As noted in the discussion of Table 4, students who go to Pilot and Charter Schools are different in important ways from those that do not. We need to take account of these differences before judging the relative effectiveness of these different school models. In the rest of this report we discuss using statistical controls and lotteries to better understand the true impact of Pilot and Charter Schools.

4.
Results Using Statistical Controls

We begin with estimates using regression methods to adjust for measurable differences among the students attending Pilots, Charters, and traditional Boston public schools. Specifically, we estimate the effect of years spent in each school type relative to years spent in a traditional public school. For middle school students, we pool outcomes across 6th, 7th, and 8th grades, estimating the differences in achievement associated with years spent in each school type since the start of middle school. For high school students, we focus on the 10th grade outcomes (since this was the only grade tested at the high school level), estimating the coefficient on years spent in each school type in 9th and 10th grade. If a student was ever enrolled in a Charter School or Pilot School over the course of a school year, we count her as having attended that school type. For instance, if a student enrolled in 9th grade for a month in a Charter or Pilot School, we count her as spending her freshman year of high school in that type of school. (We did so to account for any selective attrition that might be occurring at those schools.)

In all the results reported in **Table 6**, we include controls for baseline demographic characteristics, including indicators for gender, four race/ethnicity categories, free/reduced price lunch status, limited English proficiency, special education, year of test, year of birth, and an interaction term for female minority students. Although we do not control for baseline MCAS scores in columns (1) through (4), we add such controls in columns (5) through (8) (controlling for 4th grade scores for the middle school results and 8th grade for the high school results).

In columns (1) through (4) (the results which do not control for prior academic achievement), the coefficient on years spent in exam schools is positive (.3 to .58 standard deviations) and much larger than either Pilot or Charter Schools. However, after we control for prior academic achievement in columns (5) through (8), the estimated effect of a year spent in a Charter School remains positive and often quite similar to the estimated effect of a year spent in an exam school. For instance, in middle school mathematics, the increment in performance associated with spending a year in a Charter or exam school rather than one of the traditional BPS middle schools, is about .18 for Charter Schools and .2 standard deviations for exam schools. In high school math, the estimated effect of a year in a Charter and exam school is .16 and .22 standard deviations respectively, a statistically insignificant difference.

In **Table 6**, the estimated effects of Pilot Schools are more ambiguous and, occasionally, disconcerting. Indeed, in the middle school grades, Pilot School students modestly *underperform* relative to similar students attending traditional BPS schools. After controlling for baseline performance in column (5), the estimated effect of a year in a Pilot School (rather than a traditional BPS school) is -.05 standard deviations in English language arts and -.07 in mathematics. In the high school grades, the estimated effects of a year in a Pilot School is generally positive, ranging from .06 standard deviations in math to .15 standard deviations in writing topic development. Yet, in all cases, the estimates are smaller in magnitude than the estimated impacts of Charter Schools.

We find similar Charter and Pilot School effects in models that include controls for peer test score performance and peer demographic characteristics. Peers are defined as students in the school and grade where outcome tests are measured. In particular, although the estimated school type effects become slightly smaller, they remain significant, with Charter Schools again producing the strongest results.

The discussion of the exam school results shows the critical importance of controlling for at least baseline scores when attempting to estimate school impacts. Thus, we do not focus on the results for elementary schools in the statistical control study because we have no baseline tests for these elementary schools. There is simply no way to tell whether students in elementary Charters and Pilots come in academically “ahead” compared to students in regular BPS schools.

TABLE 6

Regression Results for Pilot and Charter Schools

	Demographics					Demographics + Baseline Scores				
	Pilot	Charter	Exam	Alternative		Pilot	Charter	Exam	Alternative	
Subject	(1)	(2)	(3)	(4)	N	(5)	(6)	(7)	(8)	N
Elementary School										
ELA	0.033	0.056***	-	-0.116*	11,600	-	-	-	-	-
	(0.026)	(0.022)	-	(0.065)		-	-	-	-	
Math	0.036	0.018	-	-0.224***	8,886	-	-	-	-	-
	(0.034)	(0.027)	-	(0.080)		-	-	-	-	
Middle School										
ELA	-0.075***	0.106***	0.424***	-0.229***	24,524	-0.053***	0.093***	0.159***	-0.192***	23,579
	(0.018)	(0.017)	(0.035)	(0.061)		(0.015)	(0.014)	(0.018)	(0.048)	
Math	-0.096***	0.171***	0.506***	-0.134***	28,880	-0.072***	0.176***	0.195***	-0.085**	27,868
	(0.022)	(0.025)	(0.056)	(0.047)		(0.019)	(0.022)	(0.036)	(0.040)	
High School										
ELA	0.175***	0.238***	0.446***	-0.096*	13,379	0.115***	0.170***	0.238***	0.014	9,308
	(0.019)	(0.024)	(0.024)	(0.053)		(0.017)	(0.022)	(0.023)	(0.045)	
Math	0.134***	0.258***	0.578***	-0.032	13,181	0.059**	0.158***	0.224***	-0.024	12,747
	(0.029)	(0.049)	(0.029)	(0.051)		(0.026)	(0.040)	(0.024)	(0.055)	
Writing Topic	0.160***	0.228***	0.372***	-0.111*	13,115	0.147***	0.196***	0.316***	-0.088	9,223
	(0.027)	(0.029)	(0.029)	(0.059)		(0.028)	(0.033)	(0.031)	(0.059)	
Writing Composition	0.146***	0.207***	0.303***	-0.053	13,115	0.128***	0.173***	0.233***	-0.085	9,223
	(0.022)	(0.026)	(0.019)	(0.065)		(0.021)	(0.027)	(0.018)	(0.070)	

Notes: This table reports the coefficients on regressions using years spent in types of schools. The excluded group are students in traditional BPS schools. Coefficients are estimated for years spent in Pilot Schools, Charter Schools, exam schools, and alternative schools. Sample restricted to students with baseline demographic characteristics. Demographics include female, black, hispanic, asian, other race, special education, limited english proficiency, free/reduced price lunch, and a female*minority dummy. Regressions also include year of test and year of birth dummies. Middle school and elementary school regressions pool grade outcomes and include dummies for grade level. Regressions use robust standard errors and are clustered on year by 10th grade school for high school and student identifier and school by year for the pooled middle school and elementary school regressions.

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

5.
Results Using Pilot and Charter Applicant Lotteries

The regression results in Table 6 adjust for the student characteristics we observe, such as students' race, free lunch status, and an earlier test score. At the same time, students who enroll in Pilots and Charters may differ in ways we cannot observe. For example, Charter high schools may attract highly motivated students who would have had high MCAS scores anyway, while Pilot middle schools may attract students who have run into trouble in the regular system and are looking for alternatives. Because we cannot observe students' motivation, and we do not have a complete record of students' past performance, self-selection based on these characteristics may bias our regression estimates.

The lotteries used by many Pilots and Charters to select applicants lead to a simple empirical strategy that eliminates selection bias from unobserved variables. Assuming the applicant lotteries are fair (a proposition we examine below), students who apply and are not admitted to a Charter or Pilot should be similar in both measured and unmeasured characteristics to those who apply and are admitted. As a result, comparisons of outcomes between those who were and were not randomly offered a place in the school to which they applied can be used to construct and unbiased estimate of the school's impact. The first step in our lottery-based analysis is a description of how the lotteries worked in the years studied.

Pilot Lotteries

The Pilot lotteries are integrated into the overall BPS centralized assignment mechanism. This mechanism assigns students to schools on the basis of walk-zone and sibling priorities, student preferences, and a random number. The process begins each January, when students (and their parents) who want to change schools or who have outgrown their current school are asked to rank order of their school choices for the following year on a form submitted to the school registration office. Elementary and middle school students may only register for a school in their assignment zone (East, West, or North). High school students may rank any high school in the district-wide student assignment plan. BPS provided us with its student assignment records detailing these choices.

For the purposes of our lottery study of Pilot Schools, we defined a student as a *Pilot applicant* if he or she picked a Pilot School as their top choice. Once student choices are made, each applicant in the BPS district receives a school-specific priority based on two criteria. First, students are granted sibling priority if they have a sibling already enrolled. Second, students are granted priority if they live in the school's walk-zone. The elementary school walk-zone covers students who live within one mile of the school, while the walk-zone radius for middle school is 1.5 miles, and the walk-zone radius for high school is two miles.

Based on these criteria, students are placed in four school-specific priority groups as follows: 1) students with sibling and walk-zone priority; 2) students with sibling priority; 3) students with walk-zone priority; and 4) students with neither sibling nor walk-zone priority. Within these four broad categories, students are ranked by a randomly-assigned lottery number.

BPS school assignments are made by a computer program that considers both school priorities and student preferences. Importantly, all Pilot elementary and middle schools in our study period were assigned through this centralized process. On the other hand, only two Pilot high schools, Another Course to College and TechBoston Academy, were assigned via lottery. The remaining Pilot high schools use school-specific admissions criteria to pick their students and are therefore omitted from our lottery-based analysis.

A number of other features of the student assignment system have an impact on our empirical strategy. First, at schools where there are more slots than first-choice applicants, lottery numbers have no effect on applicants' prospects of admission. We therefore focus on schools with more top-choice applicants than slots. We call these "over-subscribed" schools. Lottery numbers are also irrelevant for most students with sibling priority (since there are rarely more sibling priority applicants than slots). Consequently, we also exclude students with sibling priority from the Pilot lottery sample.

Second, some students who do not pick a Pilot School as their first choice rank a Pilot school as their second or lower choice. But because most Pilot Schools are over-subscribed, students who do not receive their non-Pilot top choice and who have ranked a Pilot School as their second or lower choice are unlikely to be assigned to a Pilot. We therefore include only students who picked a Pilot School as their top choice in the analysis sample.

Third, Pilot Schools covering grades that span school levels (i.e., K-8 schools and 6-12 schools) guarantee admission to students who attend the school at lower grades. Sometimes all available slots at a given school in an entry grade will be filled with these guaranteed students. For middle grades in K-8 Pilot Schools we therefore expand the lottery sample to include K-2 applicants in the years from 1997 to 2000.[14]

A final complication in the BPS system is the division of each physical school into two equal-sized virtual schools. Students are admitted to a physical school if they are admitted to either virtual school. In one virtual school, students are ordered as described above. In the other, walk-zone priority is ignored. This feature allows some students outside the walk-zone to gain admission to a school even if there are enough walk-zone applicants to fill every slot.

Our quasi-experimental research design exploits the fact that in the sample of first choice applicants to a given Pilot School, admission priority at that school is randomly assigned by lottery number (conditional on whether a student lives inside or outside the school's walk-zone). Specifically, within the group of (non-sibling and non-guaranteed) students applying to the same Pilot School, in the same year, and with the same walk-zone status, students with a smaller random number are more likely to receive an offer to attend that school. We use this feature of the BPS assignment mechanism to implement an instrumental variables strategy where the endogenous variable is the number of years a student attended a Pilot School, and the instrument is a dummy variable for whether a student had a lottery number below the highest number offered a spot at the student's top choice Pilot. By virtue of random assignment, students with lottery numbers above and below the highest number should differ only in their likelihood of attending a Pilot.

Charter Lotteries

Over-subscribed Charter Schools also use a lottery process to select from among their applicants, but the process is simpler than the Pilot lottery and completely decentralized. Each Charter School collects applications and then, on a specified day, holds its own lottery. Within this lottery, siblings have priority. Once siblings have been given slots, other applicants are offered slots depending on their lottery number. If there are more applicants than slots, a waiting list is created with order determined by lottery number. Those offered slots have a set amount of time (at least partially determined by the school) to accept or decline. When students decline, slots open up and schools make offers down the list.

Although simple in concept, several complications do arise in practice. First, each Charter lottery is independent of all other Charter lotteries. Thus applicants can apply to one, two, or all of the Charter Schools in the area. The applicant may then be admitted to none, one, two, or all the schools that he or she applied to. This can result in a situation in which an applicant "accepts" one school, is then admitted off the waiting list at a second school, and then goes back and declines the first school (with a new slot thereby opened).

Second, data management procedures and data quality vary from school to school. Some undersubscribed Charter Schools had no records of those who declined admission in earlier lotteries and two Charter Schools had closed their doors entirely with no applicant records available. Thus, we do not have complete application lists for all who applied to undersubscribed Charter Schools.

Individual Charter Schools also collect varying amounts of detail from their applicants. More detail makes it easier to match Charter applicants to state MCAS scores.

The study team collected historical lottery records from each of the currently operating middle and high schools in Boston. Once the lottery data was gathered, the study team matched the individual applicant information to the demographic and test score files provided by the Massachusetts Department of Elementary and Secondary Education. Matches were based on name, year of application, and grade of application, as well as gender, race, town of residence, and date of birth.[15] Our matching procedure located state records for 96.5% of Charter applicants at the middle school level (97.3% of those admitted and 94.8% of those not admitted) and 92.2%

TABLE 7

Attrition

	Pilot			Charter		
	Proportion of Non-offered with MCAS	Differential between Offered and Not Offered		Proportion of Non-offered with MCAS	Differential between Offered and Not Offered	
		No Controls	Demographics + Baseline Scores		No Controls	Demographics + Baseline Score
Subject	(1)	(2)	(3)	(4)	(5)	(6)
	Elementary School					
ELA	0.766	-0.012	-0.014	-	-	-
		(0.047)	(0.048)	-	-	-
N	381	574	574	-	-	-
Math	0.772	-0.023	-0.026	-	-	-
		(0.046)	(0.048)	-	-	-
N	381	574	574	-	-	-
	Middle School					
ELA	0.714	0.028	0.041	0.807	0.051*	0.048*
		(0.029)	(0.029)		(0.027)	(0.027)
N	1509	2674	2602	533	1720	1671
Math	0.722	0.043	0.052*	0.819	0.047*	0.045*
		(0.028)	(0.029)		(0.026)	(0.026)
N	1,711	3,082	3,019	579	1,887	1,829
	High School					
ELA	0.739	0.040	0.061*	0.773	0.026	0.023
		(0.033)	(0.033)		(0.026)	(0.029)
N	506	889	822	607	1856	1468
Math	0.735	0.032	0.053	0.769	0.020	0.021
		(0.033)	(0.033)		(0.026)	(0.026)
N	506	889	866	607	1856	1801
Writing Topic and Writing Composition	0.731	0.037	0.057*	0.764	0.023	0.029
		(0.033)	(0.034)		(0.027)	(0.029)
N	506	889	816	607	1,856	1,465

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 won the lottery. Regressions in column (2) include (walk zone)*(school choice)*(year of application) fixed effects, year of birth dummies and year of baseline dummies. Regressions in column (5) include (school choice)*(year of application) fixed effects, year of birth dummies and year of baseline dummies. Regressions in columns (3) and (6) add baseline covariates for all three school levels and baseline test scores for middle and and high schools. Middle school and elementary school regressions pool grades and include grade dummies. Middle school Pilot regressions stack K2 applicants (to K-8 schools) and 6th grade applicants. All demographics variables are interacted with grade of application. In column (3), baseline scores are set equal to zero for all K2 applicants.

Standard errors are clustered at student level

Sample is restricted to students who participated in an effective lottery from cohorts where we should observe their test scores.

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

of applicants at the high school level (92% of those admitted and 92.9% of those not admitted).

Many students applied to more than one oversubscribed Charter School. These students had greater odds of being offered a slot in a Charter School (by essentially buying more than one lottery ticket). Moreover, those who applied to more than one school (or to a particular set of schools) may well have been different. As a result, our Charter analysis focuses on the difference in scores between lottery winners and losers in samples of applicants who applied to the same sets of schools. In other words, imagine that one group of applicants applied only to school A, another group of applicants applied only to school B, and a third group of applicants applied to both schools, A and B. We compare outcomes for lottery winners and losers in the first group, as well as the second group and the third group, and then averaged the winner-loser difference across the three groups. The impact of being offered a Charter School slot is estimated solely within groups of applicants who have made all the same choices.

Threats to Validity: Attrition and Covariate Balance

Our analysis focuses on students who participate in a lottery and for whom we have post-lottery test scores. At the time of the lottery, those who win and lose should be very similar, because a coin flip is all that distinguishes them. However, even if the lotteries were truly random, subsequent attrition from the sample may create differences between winners and losers that could threaten the validity of our analysis. For instance, the members of the control group who were most dissatisfied with the traditional public schools may move to private schools (and thereby differentially leave the analysis sample) or the members of the treatment group who were most desperate to attend the Charter Schools may stay (and thereby differentially remain in the sample).

About a quarter of students disappear from the MCAS data between the time of the lottery and the time of the test in the study. **Table 7** reports differences in the proportion of lottery winners and losers subsequently missing MCAS data for Pilots and Charters at each grade level. For most groups, we found no statistically significant difference in the proportion of lottery winners and losers with MCAS data in the follow-up year. However, there are some exceptions. For instance, high school lottery winners among Pilot School applicants and middle school lottery winners among Pilot and Charter School applicants are somewhat more likely to have MCAS outcomes. In view of this imbalance an appendix table reports the results of an analysis using a sample of high school applicants who were about equally likely to have follow-up data in the winning and losing group. This sample was constructed by dropping subjects and years with the greatest imbalance (Tables A2, A3, and A4 in Appendix).

Table 8 and **Table 9** report differences in baseline characteristics for all lottery applicants as well as for those with matched MCAS data (that is, those who remained in the sample after the initial application). A zero in these tables indicates no difference between the lottery winners and losers. Starred values indicate statistically significant differences. For both Pilot and Charter Schools, most of the differences in average characteristics are small and not statistically different from zero. However, there were a few exceptions. In the elementary Pilot lotteries, those who remained in the sample were somewhat less likely to be on the Free or Reduced Price Lunch Program. In middle school Pilot lotteries, the lottery winners were somewhat less likely to have Limited English Proficiency; in the high school Pilot lotteries, the lottery winners were more likely to be participating in the Free or Reduced Price Lunch Program. In the Charter middle school lottery, the lottery winners are somewhat more likely to have Limited English Proficiency and to be female. On the other hand, there are only two comparisons which exhibit a statistically significant difference in baseline test scores between treatment and control groups—in the high school Pilot and Charter lottery, lottery winners had somewhat lower test scores in math and writing topic scores respectively.

Tables 8 and 9 should be interpreted in light of the fact that they report many statistical tests. Consequently, we might expect to find some statistically significant differences by chance alone. We can control for the increased likelihood of finding significant differences in multiple tests by using a joint test. Therefore, the bottom of each column reports the results of tests of the joint hypothesis that all differences in baseline test scores and background characteristics are zero. In the full sample of applicants as well as for those with non-missing MCAS scores, we cannot reject the hypothesis that the lottery winners and losers had similar baseline characteristics following the lottery. This is encouraging, as it suggests

TABLE 8

Pilot Lottery – Covariate Balance Lottery Winners Minus Lottery Losers

	Elementary		Middle School		High School	
	All (1)	Remained in Sample (2)	All (3)	Remained in Sample (4)	All (5)	Remained in Sample (6)
Hispanic	-0.052	-0.052	-0.018	-0.038	0.029	0.025
	(0.044)	(0.051)	(0.031)	(0.036)	(0.034)	(0.038)
Black	0.037	0.020	0.032	0.026	-0.016	-0.021
	(0.049)	(0.055)	(0.033)	(0.038)	(0.038)	(0.042)
White	0.019	0.031	-0.006	0.025	-0.012	-0.008
	(0.042)	(0.046)	(0.023)	(0.025)	(0.022)	(0.026)
Asian	-0.030	-0.031	-0.010	-0.016	0.000	0.007
	(0.022)	(0.023)	(0.017)	(0.019)	(0.018)	(0.021)
Female	0.027	-0.001	0.008	-0.015	-0.007	0.002
	(0.058)	(0.064)	(0.037)	(0.042)	(0.037)	(0.042)
Free or Reduced Price Lunch	-0.064	-0.117**	-0.003	-0.018	0.082**	0.063*
	(0.051)	(0.057)	(0.027)	(0.031)	(0.033)	(0.037)
Special Education	-0.013	-0.018	-0.008	0.003	-0.020	-0.025
	(0.028)	(0.030)	(0.025)	(0.031)	(0.029)	(0.033)
Limited English Proficiency	0.008	0.003	-0.056***	-0.076***	0.003	-0.006
	(0.033)	(0.038)	(0.019)	(0.023)	(0.019)	(0.020)
N	416	333	1,640	1,320	889	679
Baseline ELA Test Score+	-	-	0.033	0.027	0.008	0.066
	-	-	(0.085)	(0.089)	(0.068)	(0.070)
N	-	-	1084	1008	822	639
Baseline Math Test Score+	-	-	0.039	0.014	-0.148**	-0.094
	-	-	(0.086)	(0.090)	(0.071)	(0.078)
N	-	-	1,095	1,018	866	666
Baseline Writing Composition Test Score	-	-	-	-	0.033	0.035
	-	-	-	-	(0.062)	(0.065)
N	-	-	-	-	816	637
Baseline Writing Topic Test Score	-	-	-	-	0.000	0.002
	-	-	-	-	(0.065)	(0.067)
N	-	-	-	-	816	637
F(df,df_r)	1.515	1.628	0.901	1.194	1.053	0.888
p-value	0.150	0.116	0.514	0.299	0.398	0.559
df	8	8	8	8	12	12
df_r	401	318	1,593	1,273	789	616

Notes: This table reports coefficients on regressions of the variable indicated in each row on an indicator variable equal to one if the student won the lottery. Regressions also include (walk zone)*(school choice)*(year of application) fixed effects. Samples in columns (1), (3) and (5) are restricted to students from cohorts where we should observe at least one test scores. Samples in columns (2), (4) and (6) are restricted to students with at least one non-missing test score.

Robust standard errors. + Only for 6th grade applicants. F test 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. For middle school, only demographics variables are jointly tested.

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

TABLE 9

Charter Lottery – Covariate Balance Lottery Winners Minus Lottery Losers

	Middle School		High School	
	All (1)	Remained in Sample (2)	All (3)	Remained in Sample (4)
Hispanic	0.015	0.023	-0.009	-0.024
	(0.028)	(0.030)	(0.027)	(0.030)
Black	-0.033	-0.045	0.002	0.002
	(0.036)	(0.038)	(0.030)	(0.033)
White	0.012	0.018	-0.003	0.007
	(0.029)	(0.030)	(0.014)	(0.014)
Asian	0.005	0.003	0.017	0.023
	(0.010)	(0.011)	(0.013)	(0.016)
Female	0.064*	0.092**	-0.027	-0.029
	(0.038)	(0.040)	(0.031)	(0.034)
Free or Reduced Price Lunch	-0.002	-0.020	0.003	0.020
	(0.035)	(0.037)	(0.028)	(0.031)
Special Education	-0.022	-0.018	0.021	0.014
	(0.030)	(0.032)	(0.024)	(0.026)
Limited English Proficiency	0.028*	0.023	0.022	0.022
	(0.015)	(0.016)	(0.014)	(0.015)
N	1,052	953	1,856	1,480
Baseline ELA Test Score	0.012	0.014	-0.018	0.008
	(0.065)	(0.068)	(0.051)	(0.055)
N	1,027	931	1,468	1,178
Baseline Math Test Score	0.092	0.079	0.023	0.032
	(0.070)	(0.073)	(0.057)	(0.062)
N	1,025	931	1,801	1,445
Baseline Writing Composition Test Score			-0.046	-0.010
			(0.050)	(0.055)
N			1,465	1,176
Baseline Writing Topic Test Score			-0.136**	-0.122**
			(0.056)	(0.060)
N			1,465	1,176
F(df,df_r)	0.897	0.991	0.972	1.017
p-value	0.559	0.449	0.474	0.430
df	10	10	12	12
df_r	993	899	1,402	1,124

Notes: This table reports coefficients on regressions of the variable indicated in each row on an indicator variable equal to one if the student won the lottery. Regressions also include (school choice)*(year of application) fixed effects. Samples in columns (1) and (3) are restricted to students from cohorts where we should observe at least one test scores. Samples in columns (2) and (4) are restricted to students with at least one non-missing test score.

Robust standard errors.

F test 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%

the Pilot and Charter lotteries were indeed fair and therefore provide a valuable research tool.

Method of Analysis

Two methodological issues arise in the empirical strategy based on the Pilot and Charter lotteries. First, we must place students in the relevant risk set, that is the group for which an offer of admission is determined solely by random assignment (as opposed to, say, a student's walk-zone status for a Pilot School or the number of Charter School applications the student completed). Second, we must convert estimates of the causal effect of being *offered* a place at a Pilot or Charter to the causal effect of *spending time* as a student at a Charter or Pilot (since some students who are offered a place do not go, while others not offered apply later or to another school and are then admitted). This is called the compliance problem. The tool we use both for risk adjustment and to solve the compliance problem is two-stage least squares (2SLS), a particularly convenient type of instrumental variables (IV) estimator.

Here, we explain how 2SLS solves the compliance problem. Especially important is the terminology associated with this procedure, which we use to describe our estimates and label them in tables. The explanation of how 2SLS places students in the relevant risk set is more technical and less substantive and therefore appears in an appendix.

The starting point for 2SLS is the notion of potential outcomes capturing the causal effect of spending time in a Pilot or Charter. To make the discussion concrete, we describe procedures to estimate the effect of time spent in a Charter middle school on 8th grade math scores. For each middle school Charter applicant, we imagine two potential 8th grade math scores:

Y_{0i} = the student's score if he never enrolls in a Charter

Y_{ti} = the student's score if he spends t years in a Charter

The average time spent in a Charter School by middle school students enrolled in a Charter School is between one and two years.

The causal effect of spending t years in a Charter School is

$$Y_{ti} - Y_{0i}.$$

We never get to see this difference for any one student—we only get to see Y_{0i} if the student spends no time in a Charter or Y_{ti} if the student spends t years in a Charter. Nevertheless, the 2SLS method allows us to use the Charter lotteries to estimate the average $Y_{ti} - Y_{0i}$ for a group of affected students.

The simplest way to explain 2SLS is with a linear model linking Y_{ti} and Y_{0i}. In particular, suppose

$$Y_{ti} = Y_{0i} + \rho t. \quad (1)$$

Here, the causal effect of interest is denoted by the Greek letter ρ. This number—sometimes called a "parameter"—tells us how much more (or less) a given student would score on the MCAS as a result of having spent years in a Charter School.[16]

Equation (1) helps us understand why simply comparing the MCAS scores of students who do and do not go to Charter Schools is likely to be misleading. Because students who go to Charter Schools may be more motivated than those who do not, they probably have higher Y_{0i}, on average. Comparisons of the average scores of those who did and did not attend Charters therefore reflect differences in average Y_{0i} (a student's intrinsic MCAS potential) as well as the causal effect of interest, ρ. Social scientists call this the problem of "selection bias."

The Charter lotteries solve the problem of selection bias because they generate variation in students' time in Charter while at the same time they are unrelated to potential test scores (Y_{0i}) by virtue of random assignment. To see how this works in practice, suppose there is a single Charter School, with a Charter lottery held every year. If all of the applicants who are offered a place in this school were to enroll and attend in every subsequent grade the school offers, we need only compare the winners and losers to estimate the causal effects of attendance. In practice, however, some students who are offered a place will decide not to go or leave quickly, while others not offered a place will apply next year and attend later, probably for a shorter time. We must therefore adjust estimates of the effects of being offered a place in the Charter for differences in time spent actually attending the Charter.

This necessary adjustment works like this:

$$\rho = \frac{\text{(Average score of winners – Average score of losers)}}{\text{(Average Charter years for winners – Average Charter years for losers)}} \quad (2)$$

This formula can be derived by averaging both sides of equation (1) in the groups of winners and losers and re-arranging the averages. The difference in the average Y_{0i} drops out of the formula because randomly-chosen winners and randomly-chosen losers have the same *potential* test scores. Intuitively, the formula takes the effect of winning the lottery in the numerator and rescales it by the difference in time spent in Charter. Since the only reason for a difference in scores between winners and losers is the difference in time spent in Charter Schools, the ratio in equation (2) is the causal effect of a year spent in a Charter School.

Equation (2) is an important formula that is widely used in research designs involving random assignment. The two parts of the formula—top and bottom—have names that describe the role played by these key components in the analysis. In a clinical randomized trial such as used in medical research, the numerator is called an "*intention-to-treat-effect*" (ITT) because it measures the causal effect of an offer of treatment, without worrying about whether subjects act on the offer. In more general settings, not necessarily involving lotteries or random assignment, econometricians call ITT the "*reduced form effect*" or just the "reduced form" (RF). The reduced form captures the causal effect of an instrument. In our setting, the instrument is the random offer of a Charter slot in the population of applicants subject to random assignment. Our basic 2SLS procedure uses a simple binary instrument indicating whether applicants ever got an offer to attend a Charter prior to the start of school—whether or not they were initially placed on a wait list. In a modification of this approach, described briefly below and in greater detail in the technical appendix, we also report 2SLS estimates using two instruments, one indicating applicants who *ever* got an offer and another indicating applicants who got an offer on the day of the Charter School lottery.

The bottom of equation (2) is called the *first stage*. This captures the relation between the instrument and time spent in a Charter School. In other words, the first stage tells us how much of an experiment we really have. If, for example, winning the lottery has no effect on whether or how long a student attends a Charter School, our empirical strategy literally blows up because the denominator is zero. Luckily, in our data, middle school lottery winners spent roughly one more year in a Charter School than middle school lottery losers. Hence, we have a strong and precisely measured first stage. On the other hand, equation (2) tells us why we should omit siblings and guaranteed students from lottery-based analyses: the lottery has no effect on these students' time in Charter and hence, for them, there is no first stage.

The technical appendix provides a more complete explanation of our 2SLS estimation procedure.

Lottery Results

The reduced form, first stage, and 2SLS estimates for the effect of Charters are reported in **Table 10** and those for Pilots in **Table 12**. The results are reported with and without controls for demographics and baseline scores.

The reduced form or intent-to-treat results for Charter Schools show the differential in test outcomes for students who win the lottery, whether or not they actually attend a Charter School. Columns (1) and (4) suggest that lottery winners do in fact have higher achievement outcomes in both middle and high in everything but writing composition.

The first stage results for Charters (shown in columns (2) and (5) of Table 10) give the difference in years spent in a Charter School associated with winning a lottery. Thus we can see from Table 10 that among 6th, 7th, and 8th grade students, the lottery winners spent between .86 and .94 more years in a Charter School than lottery losers. Between 8th and 10th grade, lottery winners had spent roughly half a year more in Charter Schools than lottery losers. Note that the difference is considerably less than two years.

Why is this? We illustrate with the high schools with effective Charter lotteries: first, 53.0 percent of lottery winners never attend a Charter School at all. Second, among the lottery winners who started in a Charter School in 9th grade, 86.5 percent remained through 10th grade. Third, a non-trivial number of the lottery losers in schools with effective lotteries attended a Charter School—in fact, 22.2 percent of lottery losers in the entry year eventually attend a Charter, of whom 79.9 percent continued to 10th grade. All three of these factors are probably non-random and subject to selection: For instance, only the most devoted or most desperate students among the lottery winners may have attended the Charter Schools, and only the most satisfied students stayed. The lottery losers who were most unhappy with the traditional public schools probably found a way into a Charter School.

TABLE 10

Charter Lottery Results

	No Controls			Demographics + Baseline Scores		
	Reduced Form	First Stage	2SLS	Reduced Form	First Stage	2SLS
Subject	(1)	(2)	(3)	(4)	(5)	(6)
			Middle School			
ELA	0.200***	0.943***	0.212***	0.161***	0.943***	0.171***
	(0.072)	(0.135)	(0.072)	(0.057)	(0.129)	(0.052)
N		1,461			1,426	
Math	0.503***	0.859***	0.585***	0.468***	0.869***	0.539***
	(0.080)	(0.123)	(0.103)	(0.068)	(0.118)	(0.078)
N		1,627			1,587	
			High School			
ELA	0.088*	0.573***	0.153*	0.094**	0.573***	0.164**
	(0.053)	(0.133)	(0.089)	(0.041)	(0.138)	(0.073)
N		1,472			1,170	
Math	0.142**	0.574***	0.247**	0.109**	0.573***	0.187**
	(0.061)	(0.132)	(0.102)	(0.052)	(0.138)	(0.083)
N		1,462			1,428	
Writing Topic	0.141**	0.575***	0.245**	0.161**	0.567***	0.283**
	(0.064)	(0.133)	(0.120)	(0.063)	(0.138)	(0.126)
N		1,458			1,159	
Writing Composition	0.092	0.575***	0.160*	0.075	0.577***	0.129
	(0.056)	(0.133)	(0.093)	(0.056)	(0.137)	(0.091)
N		1,458			1,159	

Notes: This table reports the coefficients on regressions using years spent in Charter Schools. Sample restricted to students with baseline demographic characteristics. Demographics include female, black, Hispanic, Asian, other race, special education, Limited English Proficiency, free/reduced price lunch, and a female* minority dummy. Regressions also include year of test and year of birth dummies. Middle school and elementary school regressions pool grade outcomes and include dummies for grade level. All Charter regressions also include for combination of schools applied to* year of application and exclude students with sibling priority. 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 year for pooled regressions.

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

As shown in column (3) of Table 10, 2SLS estimates of the effect of one year spent in a Charter middle school are .21 for ELA and .59 for math. The latter estimate is extraordinarily large—equal to roughly half the black-white achievement gap. For English language arts, the results are similar in high school, with .15 standard deviations per year. However, for math in high school, the results are still positive and significant, but smaller in size—.25 standard deviations per year. The estimated effect on writing is .25 standard deviations per year for topic development and .16 for writing composition.

The results reported in columns (1), (2), and (3) are constructing using models that omit controls for demographic characteristics or baseline test scores, though the models do include controls for each risk set as described in equation (3). The addition of demographic and lagged score controls leaves the results generally unchanged (except for writing composition), as can be seen in columns (4) through (6). This is reassuring, since the lotteries were supposed to be randomized, and reflects the fact that there was no correlation between these baseline characteristics and whether or not a student won the lottery.

TABLE 11

Charter Lottery Results, Alternative Identification Schema

	No Controls			Demographics + Baseline Scores		
	Overid model	Initial offer	Eventual offer	Overid model	Initial offer	Eventual offer
Subject	(1)	(2)	(3)	(4)	(5)	(6)
			Middle School			
ELA	0.202***	0.167*	0.212***	0.179***	0.206**	0.171***
	(0.064)	(0.092)	(0.072)	(0.048)	(0.093)	(0.051)
N		1,461			1,426	
Math	0.508***	0.275**	0.585***	0.488***	0.320***	0.539***
	(0.090)	(0.114)	(0.103)	(0.070)	(0.096)	(0.078)
N		1,630			1,587	
			High School			
ELA	0.090	0.053	0.153*	0.138**	0.123*	0.164**
	(0.073)	(0.081)	(0.089)	(0.064)	(0.068)	(0.073)
N		1,472			1,170	
Math	0.192**	0.161*	0.247**	0.139**	0.112	0.187**
	(0.084)	(0.092)	(0.102)	(0.069)	(0.070)	(0.083)
N		1,462			1,428	
Writing Topic	0.195**	0.165**	0.245**	0.230**	0.197**	0.283**
	(0.080)	(0.077)	(0.120)	(0.093)	(0.092)	(0.126)
N		1,458			1,159	
Writing Composition	0.101	0.066	0.160*	0.074	0.039	0.129
	(0.064)	(0.066)	(0.093)	(0.065)	(0.067)	(0.091
N		1,458			1,159	

The notes for this table are the same as Table 10. Columns (1) and (4) are a 2SLS model where the instrumental variables are initial offer and ever offer. Columns (2) and (5) are a 2SLS model where the instrumental variable is initial offer. Columns (3) and (6) are a 2SLS model where the instrumental variable is ever offer.

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

In a modification of our basic 2SLS procedure, we also computed estimates using two instrumental variables for applicants' time spent in a Charter School instead of one. The two-instrument model adds a variable for whether an applicant received an offer in the initial lottery draw. For some applicants, the receipt of an initial offer is correlated with Charter attendance in spite of the fact that all applicants were eventually offered a spot (since applicants are more likely to matriculate in response to an earlier offer). The addition of an initial offer instrument therefore allows us to include a number of additional schools and application cohorts, making the Charter lottery sample more representative. A second and somewhat less important consideration is that 2SLS estimate using two instrumental variables should be more precise.

Table 11 compares 2SLS estimates using alternative instrument sets for the models. Specifically, this table shows the same estimates as reported in Table 10 (columns 3 and 6), along with estimates using the initial offer instrument only (columns 2 and 5) and

TABLE 12

Pilot Lottery Results

	No Controls			Demographics + Baseline Scores*		
	Reduced Form	First Stage	2SLS	Reduced Form	First Stage	2SLS
Subject	(1)	(2)	(3)	(4)	(5)	(6)
			Elementary School			
ELA	0.275***	2.697***	0.102***	0.252***	2.685***	0.094***
	(0.101)	(0.259)	(0.037)	(0.088)	(0.259)	(0.032)
N		442			442	
Math	0.158	2.712***	0.058	0.139	2.702***	0.051
	(0.100)	(0.257)	(0.036)	(0.096)	(0.255)	(0.034)
N		443			433	
			Middle School			
ELA	0.020	1.006***	0.020	0.022	0.996***	0.021
	(0.081)	(0.178)	(0.080)	(0.072)	(0.174)	(0.070)
N		2,025			1,974	
Math	-0.119	0.899***	-0.133	-0.102	0.994***	-0.109
	(0.089)	(0.177)	(0.096)	(0.068)	(0.173)	(0.071)
N		2,368			2,321	
			High School			
ELA	-0.002	0.619***	-0.004	-0.036	0.617***	-0.058
	(0.052)	(0.104)	(0.083)	(0.046)	(0.110)	(0.074)
N		676			636	
Math	-0.087	0.604***	-0.145	-0.055	0.583***	-0.094
	(0.075)	(0.104)	(0.123)	(0.051)	(0.110)	(0.091)
N		671			659	
Writing Topic	0.082	0.612***	0.135	0.090	0.583***	0.152
	(0.071)	(0.104)	(0.112)	(0.073)	(0.110)	(0.118)
N		669			628	
Writing Composition	0.058	0.612***	0.095	0.058	0.596***	0.097
	(0.066)	(0.104)	(0.103)	(0.063)	(0.110)	(0.101)
N		669			628	

Notes: This table reports the coefficients on regressions using years spent in Pilot Schools. Sample restricted to students with baseline demographic characteristics. Demographics include female, black, hispanic, asian, other race, special education, Limited English Proficiency, free/reduced price lunch, and a female* minority dummy. Regressions also include year of test and year of birth dummies. Middle school and elementary school regressions pool grade outcomes and include dummies for grade level. All Pilot regressions also include dummies for walk zone*first choice* year of application and exclude students with sibling priority. 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 year for pooled regressions.

Middle school Pilot regressions stack K2 applicants (to K-8 schools) and 6th grade applicants. All demographics variables are interacted with grade of application. In column (3), baseline scores are set equal to zero for all K2 applicants.

+ Elementary school regressions don't use information on baseline scores.

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

estimates using both initial and ever-offered instruments (columns 1 and 3). By and large the results are similar across instrument sets, though somewhat smaller for middle school math scores

Table 12 reports 2SLS results from the Pilot School lotteries. Although we were unable to identify sufficient Charter elementary schools with lotteries usable for research purposes, there are five Pilot Schools with usable lotteries in elementary grades. The reduced form estimates in column (2) shows that lottery winners who applied to these schools spent almost three more years in elementary Pilot Schools than lottery losers. As reported in column (3), the estimated effect of a year in a Pilot elementary school was .102 standard deviations in English language arts. There was no statistically significant effect on math in elementary school of a year in a Pilot School.

As can be seen in column (3), the impact of Pilot middle schools on both English Language Arts and math is small and not statistically significantly different from zero.

The Pilot estimates for high school applicants are also not significantly different from zero. It is important to note here, however, that these estimates are not very precise. We see the high school lottery results for the Pilot Schools as being essentially inconclusive.

To make this clear, **Figures 1 through 4** plot the relative scores of lottery winners (the lines) to lottery losers (represented by zero on the y-axis) over grades in middle and high school Pilot and Charter Schools.[17] The relatively steep upward slopes of the lines suggest that Charter School impacts increase over the course of school.

Reconciling the Randomized and Non-Randomized Results

The lottery-based estimates are only available in the schools where a random number played a role in determining enrollment. Not all Pilot and Charter Schools were subject to lotteries in every year. Some Charter and Pilot Schools were unable to fill all of their open slots. To the extent that over-subscription is a sign of quality, the lottery-based estimates may systematically exclude some of the less *desirable* schools. Thus the lottery results may be excluding the lowest-impact Charter and Pilot Schools. In addition, we noted above

FIGURE 1:

Results for Pilot High Schools by Grade: Lottery Winners vs. Lottery Losers

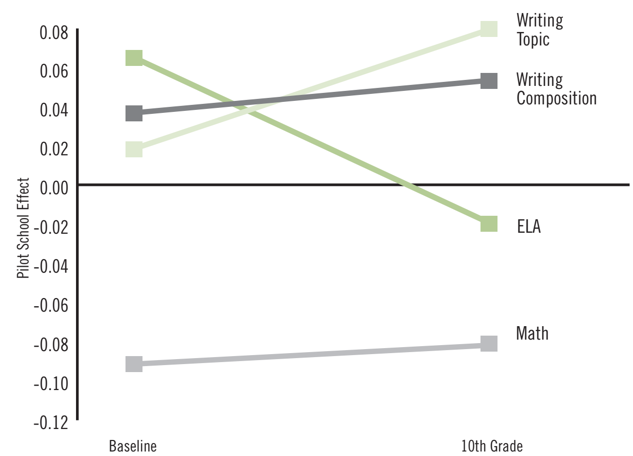


FIGURE 2:

Results for Charter High Schools by Grade: Lottery Winners vs. Lottery Losers

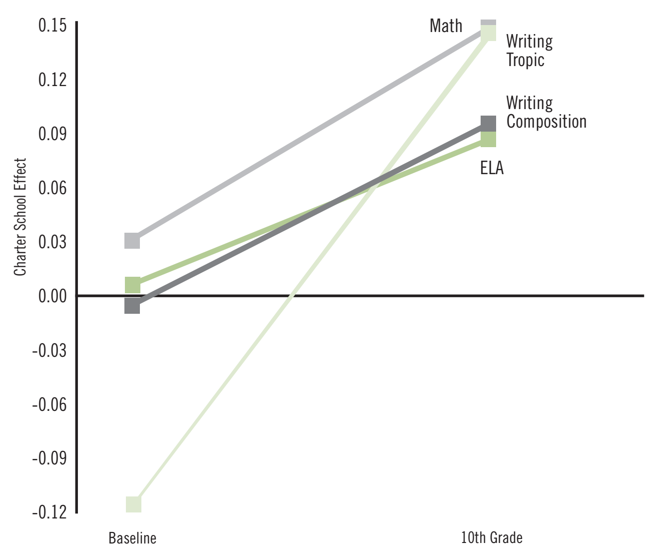


FIGURE 3:

Results for Pilot Middle Schools by Grade: Lottery Winners vs. Lottery Losers

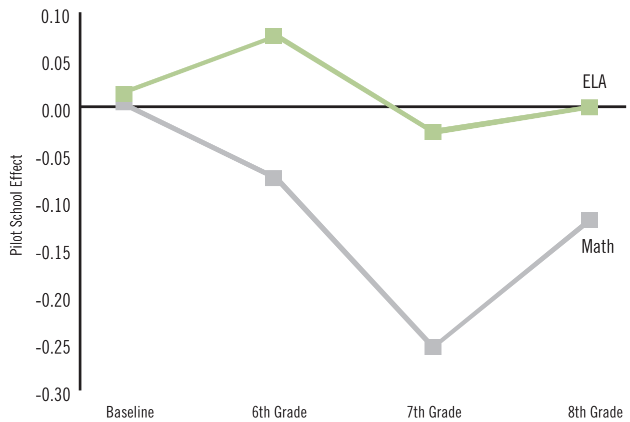
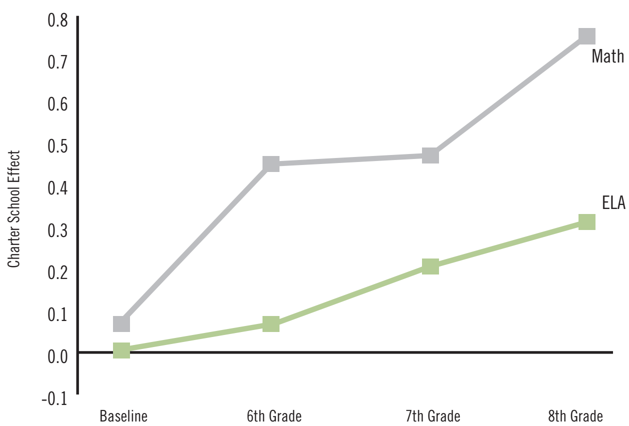


FIGURE 4:

Results for Charter Middle Schools by Grade: Lottery Winners vs. Lottery Losers



that numerous high school Pilots do not use lotteries for admission. We do not know how their effectiveness compares to the lotteried schools.

Obviously, it is not possible to compare the lottery-based estimates for schools with and without lotteries, since there are no lottery data on the latter. Fortunately, we can generate separate estimates—at least for the observational estimates based on statistical controls—for those Pilot and Charter Schools that did and did not have lotteries. The Charters and Pilots that went through random assignment are referred to as "in lottery sample." The Charter and Pilot Schools that did not have a randomized lottery are "not in lottery sample" and are included in this category if they are undersubscribed, only have slots for guaranteed students, span school levels and have their lottery at the initial entry year, or were closed. (Schools do not have to be oversubscribed to be part of the statistical control sample, since we only needed to be able to "control for" baseline test scores and demographic characteristics to generate an impact estimate.) **Table 13** reports the estimates based on statistical controls separately for schools that were subject to lotteries and those that were not. Columns (1) and (2) compare the statistically controlled estimates of the impact of a year spent in Pilots with and without lotteries, while columns (3) and (4) reports similar comparisons for Charters.

Table 13 has several important implications: First, for the middle school Pilots that were subject to lotteries, the non-experimental estimates of impact per year are a -.05 standard deviations in ELA and -.1 standard deviations in math, while the non-experimental estimates for those Pilots not subject to lotteries is not statistically different from zero.

Interestingly, the opposite is true for Charter middle schools: the non-experimental estimates for those schools that were subject to lotteries are substantially larger than similar estimates for the non-lotteried schools—.16 versus .08 in middle school ELA and .36 versus .13 for middle school math. This suggests that in middle school the highly demanded Charters do tend to outperform those in lower demand.

The bottom panel of Table 13 reports similar results for high school. For both Charter and Pilot Schools, the schools in our lottery sample are estimated to have had larger impacts than the schools that outside the lottery sample.

TABLE 13

Regression Results for Pilots and Charters by School Lottery Status

	Demographics + Baseline Scores			
	Pilot		Charter	
Subject	Not in Lottery Sample (1)	In Lottery Sample (2)	Not in Lottery Sample (3)	In Lottery Sample (4)
	Middle School			
ELA	-0.014	-0.050***	0.075***	0.164***
	(0.025)	(0.020)	(0.015)	(0.017)
Math	0.023	-0.101***	0.130***	0.356***
	(0.033)	(0.020)	(0.024)	(0.042)
	High School			
ELA	0.099***	0.156***	0.153***	0.187***
	(0.016)	(0.023)	(0.026)	(0.033)
Math	0.034	0.142***	0.147***	0.168***
	(0.025)	(0.042)	(0.039)	(0.065)
Writing Topic	0.110***	0.244***	0.168***	0.223***
	(0.031)	(0.024)	(0.042)	(0.045)
Writing Composition	0.100***	0.199***	0.170***	0.175***
	(0.021)	(0.025)	(0.037)	(0.035)

Notes: This table reports similar regressions to those in Table 6. The observation counts are the same as is Table 6, as are the coefficients for non Charter non Pilot Schools. The only difference is that they include separate dummies for Pilot and Charter Schools with and without randomized lotteries. Only schools with randomized lotteries are included in the lottery regressions. Lotteries may not occur if a school accepts students by application (Pilot high schools), if a school is undersubscribed, or if a school admits only guaranteed students.

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

6.
Summary of Outcomes

Summary of Student Achievement Impacts

Table 14 summarizes our estimates of impacts on student achievement. Columns (1) and (2) show estimates using statistical controls in the full sample, while columns (3) and (4) show estimates using statistical controls for the subsample with effective lotteries. These are compared with lottery-based estimates in columns (5) and (6). If the estimates using statistical controls were biased—that is, if there were some important difference between Charter and Pilot students and other students that affects student achievement and is not adequately captured by controlling for baseline performance and demographics—we might expect different results using the two methods.

TABLE 14

Summary of Impacts

	Observational Results				Lottery Results	
Subject	Pilot (1)	Charter (2)	Pilot in Lottery Sample (3)	Charter in Lottery Sample (4)	Pilot (5)	Charter (6)
			Elementary School			
ELA	-	-	-	-	0.094***	-
	-	-	-	-	(0.032)	-
Math	-	-	-	-	0.051	-
	-	-	-	-	(0.034)	-
			Middle School			
ELA	-0.053***	0.093***	-0.050***	0.164***	0.021	0.171***
	(0.015)	(0.014)	(0.020)	(0.017)	(0.070)	(0.052)
Math	-0.072***	0.176***	-0.101***	0.356***	-0.109	0.539***
	(0.019)	(0.022)	(0.020)	(0.042)	(0.071)	(0.078)
			High School			
ELA	0.115***	0.170***	0.156***	0.187***	-0.058	0.164**
	(0.017)	(0.022)	(0.023)	(0.033)	(0.074)	(0.073)
Math	0.059**	0.158***	0.142***	0.168***	-0.094	0.187**
	(0.026)	(0.040)	(0.042)	(0.065)	(0.091)	(0.083)
Writing Topic	0.147***	0.196***	0.244***	0.223***	0.152	0.283**
	(0.028)	(0.033)	(0.024)	(0.045)	(0.118)	(0.126)
Writing Composition	0.128***	0.173***	0.199***	0.175***	0.097	0.129
	(0.021)	(0.027)	(0.025)	(0.035)	(0.101)	(0.091)

Notes: This table summarizes the results from Tables 6, 10, 12, and 13. See those tables for sample sizes and notes. High school and middle school models in this table have demographic and baseline controls; elementary school regressions have demographic controls. Lottery results are the 2SLS coefficients.

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

TABLE 15

Regression and Lottery Results for Additional Outcomes

	Means of Dependent Variables			Observational Impacts		Lottery Impacts	
	All Students (1)	Pilot Applicants (2)	Charter Applicants (3)	Pilot (4)	Charter (5)	Pilot (6)	Charter (7)
			Middle School				
ELA Homework	0.220	0.215	0.204	0.007	0.014	-0.059	0.012
				(0.010)	(0.009)	(0.075)	(0.038)
N				6,220		276	222
Math Homework	0.224	0.225	0.238	-0.007	0.016*	-0.134	-0.037
				(0.008)	(0.008)	(0.084)	(0.051)
N				6,256		276	219
Repeat a Grade	0.064	0.071	0.077	-0.008	0.021	-0.079	-0.016
				(0.006)	(0.015)	(0.070)	(0.038)
N				8,917		432	305
Switch Schools	0.186	0.212	0.229	-0.037***	0.050**	0.167	-0.052
				(0.012)	(0.019)	(0.180)	(0.067)
N				8,917		432	305
			High School				
ELA Homework	0.274	0.229	0.238	-0.013	0.013	-0.052	0.125*
				(0.010)	(0.017)	(0.135)	(0.069)
N				7,255		400	744
Math Homework	0.304	0.267	0.258	0.007	0.020	-0.012	0.133**
				(0.009)	(0.014)	(0.175)	(0.058)
N				10,395		415	887
Repeat a Grade	0.206	0.172	0.190	-0.052***	-0.025	-0.004	-0.018
				(0.013)	(0.015)	(0.031)	(0.039)
N				15,600		731	1307
Switch Schools	0.214	0.173	0.240	-0.051***	0.026	-0.088*	0.018
				(0.015)	(0.022)	(0.053)	(0.061)
N				15,469		719	1294
Graduate	0.834	0.884	0.901	0.021***	0.014*	0.039	0.020
				(0.007)	(0.008)	(0.028)	(0.028)
N				15,600		731	1307

Notes: Columns 4 and 5 of this table reports the coefficients on regressions using years spent in types of schools. The excluded group are students in traditional BPS schools. Sample restricted to students with baseline demographic characteristics and baseline scores. Regressions also include year of test and year of birth dummies. Columns 6 and 7 are the 2SLS lottery coefficients on years spent in Pilot or Charter Schools. Sample restricted to students with baseline demographic characteristics and baseline scores. All Pilot regressions also include dummies for walk zone* first choice* year of application and exclude students with sibling priority. All Charter regressions also include for combination of schools applied to* year of application and exclude students with sibling priority. Columns 4, 5, 6, and 7 use regressions with robust standard errors and are clustered on year by 10th grade school for high school and year by 8th grade school for middle school. Columns 1, 2, and 3 report the means for the outcome variables for each of the regression samples, the N's for these columns are same as their respective regressions.

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

The impacts estimated by lottery and using statistical controls are similar for Charter Schools. For instance, the Charter School impact on math achievement in middle school is .36 when estimated with statistical controls in the experimental sample and .54 when estimated with lotteries. The Charter impact on ELA is .164 with statistical controls in the lottery sample and .171 respectively when estimated using lotteries.

The results are more mixed for Pilot Schools. In middle school, the observational estimate suggesting a negative impact and the lottery results suggesting no impact on student achievement.

The results are somewhat more encouraging for Pilot high schools. While none of the lottery-based estimates for Pilot high schools are statistically significant, the estimates based on statistical controls are generally positive for the Pilot high schools—and often comparable in magnitude to those estimated for the Charter high schools.

Impacts on Other Student Outcomes

Test scores are a limited measure of achievement and not the only impact measure of policy interest. We therefore estimated impacts on other outcomes using both statistical controls and lotteries. Specifically, we examined the following:

- The probability of reporting doing more than three hours of math or ELA homework in week;[18]
- The probability of repeating a grade;
- The probability of switching schools;
- The probability of disappearing from the sample (unfortunately, we cannot tell whether this is the result of students leaving the state, switching to a private school, or dropping out);
- The probability of graduating on time (for cohorts that we have sufficient years for).

The coefficients on years spent in a Charter or Pilot on each of the above outcomes are reported in **Table 15**. In the lottery study, very few of the differences are large or statistically significant.

The lottery results suggest that Pilot School students in high school are about 9% less likely to switch schools than regular BPS students. In contrast, Charter School students are no more likely to switch schools than students in non-charter schools. This is surprising since some observers have argued that Charter Schools are disproportionately likely to retain high achievers. If Charters are especially likely to let struggling students go, we might expect to see lower retention rates among Charter applicants who won the lottery. Those who switch out of Charters might also be lower achievers than those who stay, with the achievement difference between switchers and stayers especially high for Charter Schools.

To explore the composition of school switchers, we looked at differences between lottery winners who switch schools and lottery losers who switch schools. In middle school, Charter School applicants and Pilot School applicants who switched schools had higher baseline test scores than non-switching peers while there is little difference in demographic characteristics between the two groups. This weighs against a differential selection explanation of middle school charter effects

In high school, Charter School and Pilot School lottery winners who switch schools tend to have lower baseline test scores than those who remain. Selective out-migration of low achievers might generate peer effects that contribute to the overall impact of attendance at these schools. At the same time, this effect—which appears to be similar at Pilots and Charters—is not important enough to generate a significant positive treatment effects at Pilot Schools. It also bears emphasizing that pure composition effects (as opposed to peer effects) do not impart a bias in our lottery-based research design since we identify winners and losers as having attended a Charter or Pilot School even when attendance is for a fraction of a year.

A detailed comparison of lottery winners and losers by switcher status can be found in appendix Table A.6.

There are sizeable differences in Table 15 for the proportion of students reporting doing more than three hours of math and ELA homework in a week. At least at the high school level, the Charter School students were 13 percentage points more likely to report doing more than three hours of math homework per week and 13 percentage points more likely to report doing more than three hours of ELA homework per week.

7.
Conclusion

Charter Schools appear to have a consistently positive impact on student achievement in all MCAS subjects in both middle school and high school. The Charter results are broadly similar whether estimated using statistical controls or by exploiting the random assignment in applicant lotteries.

In contrast, the results for Pilot Schools are less conclusive. At the high school level, the estimates based on statistical controls point to a positive effect, similar in magnitude to Charter Schools. Unfortunately, the estimates based on the lotteries fail to confirm the positive impact, possibly due to the small sample size and relative imprecision.

At the middle school level, the estimated impacts of Pilot Schools using statistical controls suggested negative effects while the impacts using the lotteries suggested no effects. This contrasts sharply with the large positive effects on math we find for Charters. Although we cannot know whether new Pilots and Charters will have similar effects, it seems fair to say that the impact of Charters has been more consistently positive.

The similarity between the Charter findings in the observational and lottery-based studies is important because the observational study covers all Charter Schools. This consistency makes the overall Charter findings stronger (while inconsistency across study designs makes the pilot results less conclusive). At the same time, it's important to keep in mind that while the lottery study uses a stronger research design than the observational study, both the Charter and Pilot lottery results come only from schools and years in which the demand for seats exceeds the number of seats. Our Charter lottery results also omit schools and years for which lottery records are missing or incomplete. These considerations have the largest impact on the sample of Charter middle schools in the lottery study, where the estimated test score effects are largest.

On balance, our lottery-based findings provide strong evidence that the charter model has generated substantial test score gains in high-demand Charter Schools with complete records. On the other hand, these results should not be interpreted as showing that Boston Charters always produce test score gains. In Charter Schools with lower demand and incomplete lottery records, we have to rely on non-experimental results. (This is also true for the four Pilot high schools that do not use lotteries for student assignment.) The observational results tend to reinforce the lottery findings for charters because they also show substantial positive effects. Taken in isolation, however, the observational results are inherently less conclusive because they are potentially affected by selection bias.

One of the challenges in completing this study was the variety of ways in which Charter Schools maintain their data on applicants, lottery results, attendance offers, and waiting lists. There is no uniform record-keeping for Charter admission lotteries, and there is no central repository for such data. A standardized or central repository for lottery and admission data would allow for more regular reporting of impacts, such as those reported here.

In the same spirit, it's also worth noting that while most Pilot Schools already operate within Boston Public Schools' standardized and centralized student assignment system, a number of Pilot high schools are not subject to lottery-based assignment when they are oversubscribed. We believe that lottery-based student assignment is essential for credible evaluation of new school models—lotteries complement and reinforce simpler evaluation strategies based on statistical controls.

Appendix

Two-Stage Least Squares and Risk Sets

Equation (2) is the simplest possible 2SLS estimator. In practice, however, our lottery-based estimates were computed using a somewhat more general method that allows for the fact that the students in our sample can participate in many different lotteries, each with its own probability of winning. Among Charter applicants, the odds of winning (being offered a Charter slot) are determined by the schools an applicant applies to. For example, an applicant might apply to every Charter School, dramatically increasing the odds of getting a slot somewhere. Likewise, among Pilot applicants, the odds of getting a slot are determined by which school the student picks first and by whether the student receives walk zone priority at that school.

To make this idea precise, suppose there are two Charter Schools. Charter applicants can therefore find themselves in three possible risk sets: the set of students who applied to school 1 only; the set of students who applied to school 2 only; and the set of students who applied to both schools. Let the risk set be denoted by R_i where R_i is a risk-set index, in this case equal to 1, 2, or 3. Because students in different risk sets may differ (e.g., those who apply to both Charters may be the most ambitious) and they have different probabilities of winning a Charter lottery (those who apply to both are most likely to win), we must control for a student's risk set when estimating the causal effect of Charter attendance.

The general 2SLS estimator solves the problem of varying risk sets by including a dummy for each possible risk set as a control or covariate. Specifically, 2SLS estimates were constructed using the following linear model

$$y_i = \alpha + \Sigma j\, D_{ij}\beta^j + \rho t_i + \varepsilon_i \qquad (3)$$

where y_i is student i's test score, Dij is a dummy variable that indicates when student i is in risk set j (i.e., when R_i=j) with corresponding risk-set effect β_j, and t_i is a variable measuring student i's time spent in Charter. The causal effect of interest is again ρ, while ε_i is an error term that captures the random part of potential scores in the absence of Charter attendance (in other words, randomness in Y_{0i}). The risk sets for Charter applicants are all possible sets of Charter Schools to which a student might apply. The risk sets for Pilot Schools are all possible Pilots the student might pick as a first choice, entered separately according to whether or not the student is in each school's walk-zone.

The first stage that goes with the more general 2SLS procedure based on equation (3) comes from a regression of time spent in Charter (t_i) on the dummies D_{ij} and a dummy for whether student i was offered a Charter slot. This is the risk-adjusted equivalent of the denominator of (2). The reduced form effect that goes with the more general 2SLS procedure comes from a regression of y_i on the dummies D_{ij} and a dummy for whether student i was offered a Charter slot. This is the risk-adjusted equivalent of the numerator of (2). The 2SLS estimate of the causal effect interest is again the ratio of the RF effect to the first-stage. As a specification check, we also constructed estimates adding demographic controls and lagged test scores as covariates to the 2SLS model in equation (3). Although not strictly necessary, the addition of these controls makes the 2SLS estimates more directly comparable to the regression estimates in Table 6. Finally, for Charter Schools, we also report estimates combining two instrumental variables, one indicating applicants who received an offer in a school's initial lottery draw, the other the same ever-offer instrument used to construct the 2SLS estimates in Table 10. (The instrument used for Pilot lottery estimation can also be thought of as indicating an initial offer since this instrument indicates applicants who received an offer in the first round of the BPS assignment mechanism). The first stage for the two-instrument model comes from a regression of time spent in Charter (t_i) on the dummies D_{ij} and two dummies, one indicating whether for whether applicant i was ever offered a Charter slot And one indicating whether the applicant received an initial offer. In practice, initial offer information is missing for some schools, in which case the initial offer instrument is coded as zero (this is innocuous since we control for application risk sets).

A useful idea in this context is *visual instrumental variables* (VIV). A VIV plot provides a visual representation of the 2SLS estimator in action. The points plotted for VIV are constructed by taking the average of both sides of equation (3) across risk groups and by win/loss status in each lottery. Specifically, we compute conditional expectations or averages as follows

$$E[y_i \mid R_i, \text{i won}] = \alpha + \Sigma_j D_{ij}\beta_j + \rho E[t_i \mid R_i, \text{i won}] + E[\varepsilon_i \mid R_i, \text{i won}] \qquad (4a)$$

$$E[y_i \mid R_i, \text{i lost}] = \alpha + \Sigma_j D_{ij}\beta_j + \rho E[t_i \mid R_i, \text{i lost}] + E[\varepsilon_i \mid R_i, \text{i lost}] \qquad (4b)$$

The terms $E[y_i \mid R_i, \text{i won}]$ and $E[y_i \mid R_i, \text{i lost}]$ denote the average test score by risk group and win/loss status, while the terms $E[t_i \mid R_i, \text{i won}]$ and $E[t_i \mid R_i, \text{i lost}]$ denote the average time in Charter by risk group and win/loss status. Subtracting (4b) from (4a), we have

$$E[y_i \mid R_i, \text{i won}] - E[y_i \mid R_i, \text{i lost}] \approx \rho \bullet \{E[t_i \mid R_i, \text{i won}] - E[t_i \mid R_i, \text{i lost}]\}. \qquad (5)$$

This relation is a consequence of the fact that the average error term for winners, $E[\varepsilon_i \mid R_i, \text{i won}]$, should be close to the average error term for losers, $E[\varepsilon_i \mid R_i, \text{i lost}]$. Therefore a scatter plot of the difference in average scores between winners and losers on the difference in average Charter time between winners and losers should have a slope approximately equal to the causal effect of interest, ρ. The unit of observation in this scatter plot is the risk set, i.e., groups of students categorized by the unique sets of Charter (or Pilot) schools to which they applied.

Figure A1 shows the VIV plot for middle school math scores, separately for Charters and Pilots in Panels A and B. Panel A shows that students in risk sets with a bigger win/loss gap in time in Charter also have a bigger average score differential. The slope of the line in the scatter plot is .270, suggesting that time spent in a Charter School increases math scores. The corresponding 2SLS estimate is even larger and highly significant, as discussed above (formal 2SLS differs from the VIV estimate because 2SLS implicitly weights risk set averages by their size). In contrast, Panel B shows a slope of -.217, suggesting students who spent more time in a Pilot middle school because they won the lottery ended up with lower scores as a result. The corresponding 2SLS estimate for Pilots is also large and negative, but only marginally significantly different from zero.

FIGURE A1:

Visual Representation of 2SLS Estimates of Effects on Middle School Math Scores

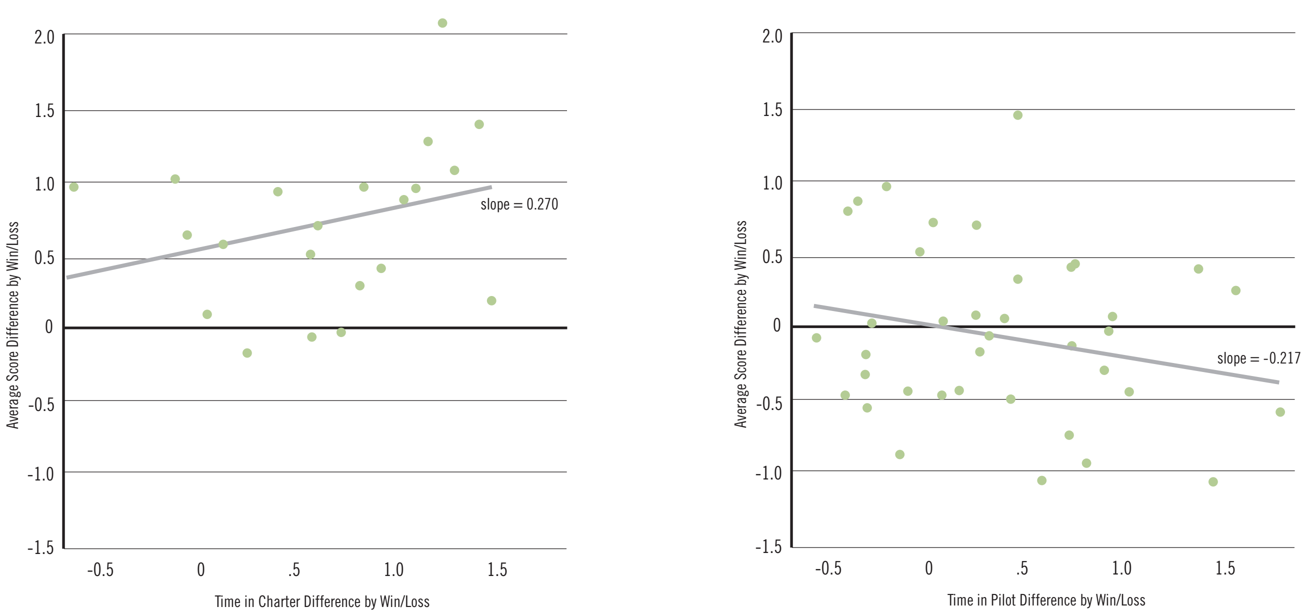


TABLE A.1

Pilot Lottery – Covariate Balance Lottery Winners Minus Lottery Losers

	Elementary	Middle School		High School
	(1)	(2)		(3)
	A. Regression Sample			
Baseline years	2002-2003	2002-2005		2002-2005
Students in BPS or in a Charter School in Boston and demos at baseline	8,844	19,856		20,748
Students in BPS or in a Charter School in Boston and demos at outcome	6,649	16,246		15,117
Excluding students with no follow-up test score	6,467	15,679		13,446
Excluding students with no baseline test scores	-	15,166		13,152
	B. Pilot Sample			
		6th grade applicants*	K2 applicants	
Application cohorts	2002-2003	2003-2006	1997-2000	2003-2005
Applied to Pilot School as first choice	720	3,553	991	978
Excluding guarantees and students with sibling priority	609	2,524	772	955
Excluding students that applied to a Pilot not subject to randomization	597	1,376	517	955
Excluding students with no baseline demos	464	1,153	516	894
Excluding students not in BPS or in a Charter School in Boston at baseline	416	1,124	516	889
Excluding students with no follow-up test score	333	1,045	275	679
	C. Charter Sample			
Application cohorts	-	2002-2006		2002-2005
Applications to Charter	-	3,022		3,570
Excluding applications not matched to state dataset	-	2,924		3,300
Excluding applications with sibling priority	-	2,752		3,191
Excluding applications that applied to a Charter not subject to randomization	-	1,927		2,991
Transforming applications into one observation per student	-	1,627		2,292
Excluding students with no baseline demos	-	1,155		1,943
Excluding students not in BPS or in a Charter School in Boston at baseline	-	1,052		1,856
Excluding students with no follow-up test score	-	953		1,480

* Excludes students who also applied to a K-8 school at K2.

TABLE A.2

Differential Attrition Omitting Unbalanced Cohorts

	Pilot			Charter		
	Proportion of Non-offered with MCAS	Differential between Offered and Not Offered		Proportion of Non-offered with MCAS	Differential between Offered and Not Offered	
		No Controls	Demographics + Baseline Scores		No Controls	Demographics + Baseline Score
Subject	(1)	(2)	(3)	(4)	(5)	(6)
			Middle School			
ELA	0.714	0.028	0.041	0.813	0.040	0.038
		(0.029)	(0.029)		(0.027)	(0.027)
N	1,509	2,674	2,602	475	1,555	1,514
Math	0.719	0.034	0.043	0.823	0.038	0.038
		(0.029)	(0.029)		(0.026)	(0.026)
N	1,547	2,750	2,698	542	1,801	1,745
			High School			
ELA	0.747	0.022	0.051	0.773	0.026	0.023
		(0.041)	(0.042)		(0.026)	(0.029)
N	312	554	507	607	1,856	1,468
Math	0.735	0.032	0.053	0.769	0.020	0.021
		(0.033)	(0.033)		(0.026)	(0.026)
N	506	889	866	607	1,856	1,801
Writing Topic and Writing Composition	0.734	0.017	0.044	0.764	0.023	0.029
		(0.042)	(0.044)		(0.027)	(0.029)
N	312	554	504	607	1,856	1,465

Sample excludes the following cohorts:
- Pilot high school, ELA and writing: 2005
- Pilot high school, writing: 2005
- Pilot middle school, 8th grade math: 2004
- Charter middle school, math: 7th grade outcome, 6th grade applicants in 2004
- Charter middle school, ELA: 7th grade outcome, 6th grade applicants in 2004 and 8th grade outcome, 5th grade applicants in 2002

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

TABLE A.3

Charter Lottery Results Omitting Unbalanced Cohorts

	No Controls			Demographics + Baseline Scores		
	Reduced Form	First Stage	2SLS	Reduced Form	First Stage	2SLS
Subject	(1)	(2)	(3)	(6)	(7)	(8)
			Middle School			
ELA	0.207***	0.929***	0.223***	0.165***	0.932***	0.177***
	(0.075)	(0.143)	(0.076)	(0.060)	(0.136)	(0.054)
N		1,299			1,274	
Math	0.490***	0.837***	0.585***	0.454***	0.849***	0.534***
	(0.081)	(0.126)	(0.109)	(0.071)	(0.121)	(0.082)
N		1,448			1,415	

Notes: This table reports the coefficients on regressions using years spent in Charter Schools. Sample restricted to students with baseline demographic characteristics. Demographics include female, black, Hispanic, Asian, other race, special education, Limited English Proficiency, free/reduced price lunch, and a female* minority dummy. Regressions also include year of test and year of birth dummies. Middle school and elementary school regressions pool grade outcomes and include dummies for grade level. All Charter regressions also include for combination of schools applied to* year of application and exclude students with sibling priority. 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 year for pooled regressions.

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

Sample excludes the following cohorts:

- Pilot high school, ELA and writing: 2005
- Pilot high school, writing: 2005
- Pilot middle school, 8th grade math: 2004
- Charter middle school, math: 7th grade outcome, 6th grade applicants in 2004
- Charter middle school, ELA: 7th grade outcome, 6th grade applicants in 2004 and 8th grade outcome, 5th grade applicants in 2002

TABLE A.4

Pilot Lottery Results Omitting Unbalanced Cohorts

	No Controls			Demographics + Baseline Scores		
	Reduced Form	First Stage	2SLS	Reduced Form	First Stage	2SLS
Subject	(1)	(2)	(3)	(4)	(5)	(6)
			Middle School			
ELA	0.012	0.408***	0.029	0.032	0.443***	0.072
	(0.085)	(0.093)	(0.206)	(0.073)	(0.095)	(0.166)
N		1,928			1,848	
Math	-0.113	0.353***	-0.320	-0.088	0.391***	-0.225
	(0.094)	(0.087)	(0.253)	(0.068)	(0.087)	(0.167)
N		2,276			2,210	
			High School			
ELA	0.001	0.625***	0.001	-0.017	0.632***	-0.027
	(0.067)	(0.124)	(0.105)	(0.058)	(0.136)	(0.091)
N		418			389	
Math	-0.087	0.604***	-0.145	-0.055	0.583***	-0.094
	(0.075)	(0.104)	(0.123)	(0.051)	(0.110)	(0.091)
N		671			659	
Writing Topic	0.132	0.615***	0.215	0.148	0.583***	0.245
	(0.088)	(0.126)	(0.134)	(0.095)	(0.110)	(0.150)
N		411			383	
Writing Composition	0.094	0.615***	0.153	0.100	0.605***	0.166
	(0.070)	(0.126)	(0.103)	(0.077)	(0.136)	(0.115)
N		411			383	

Notes: This table reports the coefficients on regressions using years spent in Pilot Schools. Sample restricted to students with baseline demographic characteristics. Demographics include female, black, Hispanic, Asian, other race, special education, Limited English Proficiency, free/reduced price lunch, and a female* minority dummy. Regressions also include year of test and year of birth dummies. Middle school and elementary school regressions pool grade outcomes and include dummies for grade level. All Pilot regressions also include dummies for walk zone* first choice* year of application and exclude students with sibling priority. 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 year for pooled regressions.

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

Sample excludes the following cohorts:

- Pilot high school, ELA and writing: 2005
- Pilot high school, writing: 2005
- Pilot middle school, 8th grade math: 2004
- Charter middle school, math: 7th grade outcome, 6th grade applicants in 2004
- Charter middle school, ELA: 7th grade outcome, 6th grade applicants in 2004 and 8th grade outcome, 5th grade applicants in 2002

TABLE A.5

Charter and Pilot School Study Participation

	Application cohorts (1)	In Observational Study (2)	Notes (3)
I. Elementary School (3rd and 4th grades)			
CHARTER SCHOOLS			
Benjamin Bannaker Charter Public School		x	Located out of Boston, only Boston residents in study
Boston Renaissance Charter Public School		x	
Conservatory Lab Charter School		x	
Neighborhood House Charter School		x	
Prospect Hill Academy Charter School		x	Located out of Boston, only Boston residents in study
PILOT SCHOOLS			
Gardner Elementary School		x	
Lyndon K-8 School	2002-2003	x	K-8 school
Mason Elementary School	2002-2003	x	
Mission Hill School	2002-2003	x	K-8 school
Orchard Gardens K-8 School	2003	x	K-8 school
Young Achievers K-8 School	2002-2003	x	K-8 school
II. Middle School (6th, 7th, and 8th grades)			
CHARTER SCHOOLS			
Academy of the Pacific Rim Charter Public School	2005-2006	x	
Benjamin Bannaker Charter Public School		x	Located out of Boston, only Boston residents in study
Boston Collegiate Charter School	2002-2005	x	
Boston Preparatory Charter Public School	2005-2006	x	
Boston Renaissance Charter Public School		x	K-8 school
Edward Brooke Charter School		x	
Excel Academy Charter School		x	
Frederick Douglass Charter School		x	closed
Neighborhood House Charter School		x	K-8 school
Prospect Hill Academy Charter School		x	Located out of Boston, only Boston residents in study
Roxbury Preparatory Charter School	2002-2006	x	
Smith Leadership Academy Charter Public School		x	
Uphams Corner Charter School		x	
PILOT SCHOOLS – 6th grade applicants			
Harbor School	2002, 2006	x	
Josiah Quincy Upper School	2004-2006	x	
Lilla G. Frederick Pilot Middle School	2004-2006	x	
Lyndon K-8 School	2004-2006	x	K-8 school
Mission Hill School	2002, 2003	x	K-8 school
Orchard Gardens K-8 School	2003-2006	x	K-8 school
Young Achievers K-8 School		x	K-8 school

continued on next page

TABLE A.5

Charter and Pilot School Study Participation *continued*

	Application cohorts (1)	In Observational Study (2)	Notes (3)
PILOT SCHOOLS – K2 applicants			
Lyndon K-8 School	1997-1998, 2000	x	K-8 school
Mission Hill School	1997-2000	x	K-8 school
Orchard Gardens K-8 School	1997-2000	x	K-8 school
Young Achievers K-8 School	1999-2000	x	K-8 school
III. High School (10th grade)			
CHARTER SCHOOLS			
Academy of the Pacific Rim Charter Public School		x	6-12 school
Boston Collegiate Charter School		x	5-12 school
City On A Hill Charter Public School	2002-2003, 2005	x	
Codman Academy Charter Public School	2004	x	
Community Charter School of Cambridge		x	Located out of Boston, only Boston residents in study
Frederick Douglass Charter School		x	closed, 6-9 school
MATCH Charter Public High School	2002-2005	x	
Prospect Hill Academy Charter School		x	Located out of Boston, only Boston residents in study
Roxbury Charter High Public School		x	closed
Horace Mann Charter Schools			
Boston Day and Even Academy Charter School			Included in observational study as alternative school
Health Careers Academy Charter School	2002-2005	x	
PILOT SCHOOLS			
Greater Egleston Community High			Included in observational study as alternative school
Another Course to College	2003-2005	x	
Boston Arts Academy		x	Entry by application
Boston Community Leadership Academy		x	Entry by application
Fenway High School		x	Entry by application
Josiah Quincy Upper School		x	6-12 school
New Mission High School		x	Entry by application
TechBoston Academy	2003-2005	x	

Notes: Schools may not participate in the lottery study if they are undersubscribed, only have seats for students with guaranteed priority, did not keep sufficient records, or if they do not have an entry lottery at the school level start grade. Fredrick Douglass Charter School and Roxbury Charter High Public School were closed by the State Board of Education in the 2004-2005 school year.

TABLE A.6

Descriptive Statistics of Lottery Winners and Lottery Losers by School Switching Status

	Charter School Applicants						Pilot School Applicants					
	Lottery Winners			Lottery Losers			Lottery Winners			Lottery Losers		
	Same School	Switch Schools	Disappear from Sample	Same School	Switch Schools	Disappear from Sample	Same School	Switch Schools	Disappear from Sample	Same School	Switch Schools	Disappear from Sample
Middle School												
Female	58.5%	32.9%	42.6%	40.5%	47.2%	50.0%	60.7%	54.5%	34.7%	53.2%	50.8%	48.0%
Black	61.5%	56.1%	36.2%	67.6%	69.4%	37.5%	60.7%	47.7%	56.9%	34.9%	38.1%	43.3%
Hispanic	18.5%	17.1%	12.8%	21.6%	19.4%	16.7%	33.3%	26.1%	27.8%	44.0%	31.4%	24.4%
Special Education	17.7%	17.1%	14.9%	21.6%	16.7%	16.7%	20.0%	9.1%	13.9%	11.9%	13.6%	11.0%
Limited English Proficiency	8.5%	7.3%	4.3%	2.7%	5.6%	8.3%	17.0%	11.4%	12.5%	24.8%	20.3%	9.4%
Free or reduced price lunch	69.2%	68.3%	55.3%	78.4%	72.2%	70.8%	83.0%	71.6%	68.1%	84.4%	63.6%	60.6%
N	130	82	47	37	36	24	135	88	72	109	118	127
*4th Grade Math MCAS**	0.178	0.238	0.382	-0.004	0.237	-0.075	-0.174	0.215	-0.525	-0.076	0.438	-0.181
*4th Grade ELA MCAS**	0.271	0.185	0.435	0.103	0.443	-0.032	-0.072	0.300	-0.197	0.020	0.500	0.074
High School												
Female	58.9%	54.7%	53.5%	59.6%	56.8%	57.4%	35.2%	41.3%	44.0%	48.9%	50.6%	52.1%
Black	65.6%	78.5%	67.0%	66.9%	68.2%	70.2%	51.6%	60.9%	52.0%	59.3%	69.4%	59.6%
Hispanic	22.7%	17.7%	23.8%	23.1%	21.2%	18.1%	26.5%	17.4%	26.0%	22.6%	21.2%	22.3%
Special Education	15.6%	18.5%	21.1%	15.5%	15.2%	19.1%	13.6%	10.9%	22.0%	14.4%	11.8%	11.7%
Limited English Proficiency	4.8%	4.2%	7.0%	3.9%	4.5%	2.1%	6.3%	2.2%	10.0%	5.5%	5.9%	2.1%
Free or reduced price lunch	75.3%	75.1%	71.9%	75.9%	79.5%	67.0%	81.9%	87.0%	74.0%	72.5%	69.4%	60.6%
N	799	265	185	381	132	94	287	46	50	327	85	94
8th Grade Math MCAS	0.133	-0.164	-0.006	0.089	0.033	0.118	0.135	-0.283	-0.050	0.204	0.112	0.129
7th Grade ELA MCAS	0.188	0.058	0.049	0.186	0.171	0.185	0.199	0.047	-0.186	0.186	0.210	0.245

This table reports descriptive statistics for the lottery population, applicants to Pilot Schools and Charter Schools in middle school and high school, by win/loss in the lottery and by whether or not students switched schools as defined by having more than one school per grade or by changing schools during the middle or high school period.

*Only for 6th grade applicants

Endnotes

[1] The first two purposes described in the Massachusetts Charter School legislation were "to stimulate the development of innovative programs within public education" and "to provide opportunities for innovative learning and assessments" (Massachusetts General Law, Chapter 71 § 89).

[2] For Commonwealth Charter Schools, some of the costs to districts are offset by a state reimbursement formula.

[3] A district's total Charter School tuition payment may not exceed 9% of their net school spending. If a waiting list applicant is a sibling and her enrollment would exceed the sending district's Charter tuition cap, the Commonwealth will provide the tuition, subject to appropriation (603 CMR 1.06 4d). If a Charter School has open seats after the initial lottery procedure the school may repeat the process until space is filled. Public notice must be given at least one week prior to any additional application deadlines.

[4] This school, Greater Egleston Community High School, is included in the results with statistical controls as an alternative school.

[5] We do not use science outcomes. The baseline test for science occurs in the fifth grade – a grade included in some middle schools and not in others. As a result, we do not have a baseline science test that cleanly occurs before treatment.

[6] For the 10th grade MCAS results, the test score control variable was 8th grade math score when math was the outcome and 7th grade English language arts score or writing score for those subjects. For the middle school results requiring a prior test score as a control variable, students were required to have a 4th grade math or English Language Arts score. No baseline scores are available for the elementary school model. If a student repeated a grade, we include only the first observation in each grade. We include students who were in 8th grade for the high school models and 4th grade in the middle school models in 2002 to 2005, and students who were in kindergarten in 2002 and 2003 for the elementary school model.

[7] About 95 percent of Charter and Pilot students attend their Charter or Pilot school for more than 90 days of the school year.

[8] If a student attended more than one traditional school in a school year, he or she was assigned to the most attended school. If he or she attended more than one school and at least one of those schools was a specialized school, he or she was assigned to the most attended of the specialized schools. Ties in days of attendance were broken randomly.

[9] We did run results year by year and saw no trend of either improvement or decline in the results. This suggests that pooling is a reasonable strategy.

[10] Some Charters do accept applications after the date of their lottery, and thus may still have a waiting list in September. We exclude late applicants.

[11] We do not do this procedure in the lottery study for middle-high schools for two reasons. First, the data required for Charter Schools would extend to years prior to the data received from the Massachusetts Department of Elementary and Secondary Education. Thus we could not execute this for Charters. Unlike K-8 schools, there is only one middle-high Pilot (Quincy Upper School). In order to keep the high school methodology similar across Charters and Pilots, we made the decision not to include this Pilot in the high school lottery results (it is included in the middle school lottery results, as are the middle-high Charters).

[12] Note that the alternative school category includes all alternative BPS schools – even alternative Pilot and Charter Schools. To make the comparison between types of schools fair, we needed to remove schools that specifically serve overage or struggling students.

[13] Note that we are unable to say anything about all applicants to Pilot Schools in high school as not all Pilot high schools were part of the Boston Choice process during the study years. Thus we are unable to know the characteristics of all applicants. Similarly, we do not have information on all Charter applicants. Several undersubscribed schools did not keep good historical records of all past applicants. In addition, two area Charter Schools were closed during the study period and we do not have records from these schools.

[14] The school assignment algorithm changed in 2005, but retains the feature that students are ranked by lottery number within broad priority groups (see Abdulkadiroğlu, Pathak, Roth, & Sönmez (2005) for details).

[15] Students were not matched on school of attendance in grades after the application grade, since this would bias the match rate towards students accepted by Charter Schools.

[16] In practice, we do not need such strong modeling assumptions, but they streamline the illustration; for a more general derivation, see Angrist & Imbens (1995).

[17] Figures 1 through 4 represent the "intent to treat" or reduced form estimates for the difference between winners and losers. We do not control for baseline demographics or scores in the figures.

[18] This question is asked as part of the MCAS student survey given to middle school and high school students when they take the MCAS.

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