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Can Successful Schools Replicate? Scaling Up Boston's Charter School Sector†

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Can schools that boost student outcomes reproduce their success at new campuses? We study a policy reform that allowed effective charter schools in Boston, Massachusetts to replicate their school models at new locations. Estimates based on randomized admission lotteries show that replication charter schools generate large achievement gains on par with those produced by their parent campuses. The average effectiveness of Boston's charter middle school sector increased after the reform despite a doubling of charter market share. An exploration of mechanisms shows that Boston charter schools compress the distribution of teacher effectiveness and may reduce the returns to teacher experience, suggesting the highly standardized practices in place at charter schools may facilitate replicability. (*JEL* H75, I21, I28)

The feasibility of scaling up effective programs is a perennial problem in social policy. Successful demonstration projects often fail to reproduce their effects at scale. In the education sphere, for example, recent large-scale studies of early childhood programs, class size reductions, and the Success For All curriculum show effects that fall short of the impressive gains seen in smaller-scale evaluations of similar interventions (Heckman et al. 2010; Heckman, Pinto, and Savelyev 2013; Puma et al. 2012; Krueger 1999; Jepsen and Rivkin 2009; Borman et al. 2007; Quint et al. 2015). This suggests that in some cases the success of programs may be driven by unique inputs or population characteristics such as special teachers, school leaders, peer environments, or other factors that cannot be easily replicated (see Banerjee et al. 2017 on the challenges of scaling up demonstration programs, including

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general equilibrium and spillover effects; see Davis et al. 2017 on the role of labor supply in scale-up efforts).

The potential for sustained success at scale is of particular interest for "No Excuses" charter schools, a recent educational innovation that has demonstrated promise for low-income urban students. These schools share a set of practices that includes high expectations, strict discipline, increased time in school, frequent teacher feedback, high-intensity tutoring, and data-driven instruction. Evidence based on randomized admission lotteries shows that No Excuses charter schools generate test score gains large enough to close racial and socioeconomic achievement gaps in a short time, as well as improvements in longer-run outcomes like teen pregnancy and four-year college attendance (Abdulkadiroğlu et al. 2011, 2017; Angrist, Pathak, and Walters 2013; Angrist et al. 2012, 2016; Dobbie and Fryer 2011, 2013, 2015; Tuttle et al. 2013; Walters 2018). Other recent studies demonstrate positive effects of No Excuses policies when implemented in traditional public schools or in low-performing schools converted to charter status (Fryer 2014, Abdulkadiroğlu et al. 2016). No school district has adopted these policies on a wide scale, however, and No Excuses charters serve small shares of students in many of the cities where they operate. It therefore remains an open question whether the effects documented in previous research can be replicated at a larger scale. Replicability is also a core issue for charter schools more generally, since by design charters are intended to serve as laboratories of innovation and spread successful educational practices.[1]

We address this question using a recent policy change that expanded the charter school sector in Boston, Massachusetts, a city where most charter schools operate according to No Excuses principles. In 2010, Massachusetts passed a comprehensive education reform law that raised the state's cap on the fraction of funding dedicated to charter school tuition payments in low-performing districts. Charter operators that the state deemed "proven providers" with track records of success were permitted to expand existing campuses or open new schools in these districts. As a result, the number of charter schools in Boston increased from 16 to 32 between 2010 and 2014, with most of these new campuses linked to existing No Excuses charter schools. This expansion led to dramatic growth in charter market share in Boston: the fraction of sixth-grade students attending charter schools increased from 15 to 31 percent between 2010 and 2015.

This increase is equal to the difference in charter attendance rates between the fifteenth-ranked and one hundredth-ranked school districts in the United States (charter share rankings from 2016–2017; see David, Hesla, and Pendergrass 2017).[2] Boston's charter expansion is therefore a large, policy-relevant change in charter share, and it occurs in a single education market. Among charter expansions studied in the previous literature, the closest analog is the growth of the Knowledge is Power Program (KIPP) network of charter schools (Tuttle et al. 2015). Between 2010 and 2015, the KIPP network doubled its student population, from about 27,000

[1] Massachusetts charters are required by law to disseminate their "best practices." For details on the Massachusetts policy, see http://www.doe.mass.edu/charter/bestpractices/.

[2] School district rankings are for all grade levels, and the charter share we focus on here is for middle schools. The leap for all grade levels is a move from approximately 208th to 63rd.

to 55,000 students. Nonexperimental estimates comparing KIPP students to observably similar non-KIPP students showed that the network continued to boost student achievement over this period of expansion, but that these gains were smaller in the years of greater expansion. However, the KIPP expansion differs on important dimensions from the Boston expansion studied here, as the growth of KIPP schools was diffuse, over many cities, rather than concentrated in a particular locality. The policy we study is also distinct from the turnaround strategies studied in Fryer (2014) and Abdulkadiroğlu et al. (2016), which involved transformations of extant schools rather than new entrants to a market.

Other localities face policy choices regarding charter expansions similar to the policy change we study here. New York City reached its cap on the number of charter schools in the city in winter 2019 with 10 percent of students enrolled in charter schools. An increase in the charter cap could result in an influx of charter schools, and would follow similar cap increases in 2007 and 2010 (New York State Department of Education 2019). Massachusetts voters faced a decision about a referendum for another similar charter cap increase in 2016, which did not pass. Boston has again reached the cap on charter schools and thus the state may face future legislation about changing the cap. Several other states have reached their overall caps on charter schools or have limited remaining growth, including Connecticut, Maine, and Rhode Island, setting the stage for policy decisions regarding further growth (Ziebarth and Palmer 2018). The federal government also supports charter school replication, with several charter school networks receiving very large grants to replicate their models, including 2019 awards of over $100 million to IDEA Public Schools and over $85 million to KIPP.[3]

We use records from randomized charter school admission lotteries to study changes in the effectiveness of Boston's charter middle school sector during this period of rapid expansion. By comparing the outcomes of students who randomly receive lottery offers to those who do not, we eliminate selection bias that plagues observational comparisons and generate reliable estimates of the causal effects of charter school attendance. The lottery records used here cover 14 of the 15 charter schools admitting students in fifth or sixth grade during the time period of our study. This is important in light of evidence that schools with more readily available lottery records tend to be more effective (Abdulkadiroğlu et al. 2011). Unlike previous studies that focus on subsets of oversubscribed charter schools, our estimates provide a representative picture of the effectiveness of the Boston charter middle school sector before and after expansion.

Consistent with past work, our estimates for cohorts applying before 2010 show large positive impacts of charter attendance on test scores. Specifically, a year of attendance at a Boston charter middle school boosted math achievement by between 0.18 and 0.32 standard deviations (σ) and increased English Language Arts (ELA) achievement by about 0.1σ during this period. Our results also indicate that policymakers selected more effective schools for expansion: proven providers produced larger effects than other charter schools before the reform.

[3] For details on these awards, see https://oese.ed.gov/offices/office-of-discretionary-grants-support-services/charter-school-programs/state-charter-school-facilities-incentive-grants/awards/.

We make two main contributions to the literature. Our first contribution is to show that a swift, within-market scale-up of a "proven" policy can be successful. Estimates for the post-reform period reveal that Boston's charter sector remained effective while doubling in size. Proven providers and other existing charters maintained their effectiveness after the reform, while expansion charters generate achievement gains comparable to those of their parent schools. Moreover, expansion charters produce these large impacts while enrolling students that appear more representative of the general Boston population than students at other charters. Together, the estimates for new and existing schools imply an increase in overall charter effectiveness despite the substantial growth in charter market share after the 2010 reform. This is the first evidence on the effects of a large scale-up of an effective charter school sector within a single market.

Our second contribution is a detailed investigation of the mechanisms that made this successful within-market expansion possible. The Boston context benefits from a large number of recent college graduates in the teacher labor market, a long track record with charters, and a geographically desirable location, which may limit the generalizability of our findings. However, by exploring which mechanisms make possible successful replication at scale, we provide more general evidence on what contributes to successful schools. We explore the roles of student composition, public school alternatives, and school practices in mediating the effectiveness of expansion charter schools. Though changes in demographic composition contributed modestly to the positive impacts of new charters, neither changes in the student body nor the quality of applicants' fallback traditional public schools explain the pattern of results. Instead, it appears that proven providers successfully transmitted hiring and pedagogical practices to new campuses. An analysis of teacher value-added indicates that charter schools compress the distribution of teacher effectiveness and may reduce returns to experience while also employing a large share of new and inexperienced teachers. These findings are consistent with the possibility that Boston charter schools' use of highly standardized school models that limit teacher discretion may facilitate replicability in new contexts.

The next section provides background on charter schools in Boston and the charter expansion reform. Section II describes the data and Section III details the empirical framework used to analyze it. Section IV presents lottery-based estimates of charter school effects before and after the reform. Section V explores the role of student composition and fallback schools, and Section VI discusses charter management practices and teacher productivity. Section VII offers concluding thoughts.

I. Background

A. *Charter Schools in Boston*

The first charter schools in Boston opened in 1994. Boston charters offer a different educational experience than traditional public schools operating in the Boston Public Schools (BPS) district. Table 1 compares inputs and practices of BPS schools and the 14 charter middle schools in our analysis sample (described in more detail later on). Columns 1 and 5 of panel A show that charter students spend more days

Table 1—School Characteristics

	All charters (1)	Proven providers (2)	Expansion charters (3)	Other charters (4)	Boston Public Schools (5)
Panel A. Comparison with traditional public schools					
Days per year	185.9	183.8	186.6	187.3	180.0
Hours per day	8.1	8.1	8.0	8.0	7.3
Percent of teachers licensed in teaching assignment	47.2	45.7	42.8	59.6	95.1
Percent of core academic classes taught by highly qualified teachers	78.7	88.9	68.7	88.4	93.2
Average years of teaching experience in Massachusetts for teachers	2.6	2.9	1.6	3.3	12.3
Student/teacher ratio	11.2	12.5	10.2	11.7	11.7
Average per-pupil expenditure	$17,041	$17,900	$17,831	$14,052	$18,766
Title 1 eligible	1.0	1.0	1.0	1.0	1.0
Panel B. Charter school characteristics					
Years open through 2012–2013	7.4	11.0	2.4	14.3	
Tutoring	1.0	1.0	1.0	1.0	
Homework help program	0.4	0.3	0.3	1.0	
Saturday programming	0.6	0.5	0.6	0.7	
School break programming	0.5	0.5	0.3	1.0	
No Excuses index	0.9	0.9	0.9	0.8	
Contact parents at least monthly	0.5	0.5	0.4	0.7	
Distance from parent campus (miles)	—	—	3.1	—	
Observations (schools)	14	4	7	3	5

Notes: This table displays characteristics for charter schools in the analysis sample along with Boston Public Schools (BPS) district schools serving middle school grades. Characteristics are measured in the 2012–2013 school year. Per-pupil expenditure is CPI-adjusted to 2015 dollars. The No Excuses index is an equally weighted average of No Excuses characteristics mentioned in charter school annual reports, described in detail in footnote 5.

Sources: Data sources include charter school annual reports, school websites, Massachusetts Department of Elementary and Secondary Education (MA DESE) School District Profiles, and MA DESE Education Personnel Information Management System (EPIMS) data.

per year and hours per day in school than BPS students. Charter teachers tend to be younger and less experienced than BPS teachers; as a result, they are much less likely to be licensed or designated highly qualified.[4] BPS and charter schools have similar student/teacher ratios, but charters spend somewhat less money per pupil ($18,766 versus $17,041), a difference driven by lower salaries and retirement costs for their less experienced teachers (Setren, forthcoming).

Boston charter schools commonly subscribe to No Excuses pedagogy, an approach that utilizes strict discipline, extended instructional time, selective teacher hiring, frequent testing, high expectations, teacher feedback, data-driven instruction, and tutoring (Carter 2000, Thernstrom and Thernstrom 2004). Panel B of Table 1 reports the mean of an index of No Excuses policies, constructed as an equally weighted average of features typically associated with the No Excuses model.[5] On average, Boston

[4] In the time period of our study, teachers were designated highly qualified if they possessed a Massachusetts teaching license and a bachelor's degree, and passed a state examination or held a degree in their subject area. The highly qualified label was discontinued with the passage of the federal Every Student Succeeds Act (ESSA) in 2015.

[5] The No Excuses index is an average of indicators equal to one if the following items are mentioned in a school's annual report: high expectations for academics, high expectations for behavior, strict behavior code,

charter schools implement 90 percent of these policies. Charters also commonly offer Saturday school and school break programming for homework help and tutoring. These practices differ markedly from practices at BPS schools and at nonurban charter schools in Massachusetts (Angrist, Pathak, and Walters 2013).

Previous research has documented that Boston charters boost math and English standardized test scores (Abdulkadiroğlu et al. 2011, Cohodes et al. 2013, Walters 2018). Recent evidence shows that Boston charter high schools also increase longer-term outcomes, including SAT scores, Advanced Placement (AP) credit, and enrollment in four-year college (Angrist et al. 2016). These findings are consistent with studies showing positive effects for urban No Excuses charters elsewhere (Dobbie and Fryer 2011, 2013; Angrist et al. 2010, 2012; Chabrier, Cohodes, and Oreopoulos 2016; Abdulkadiroğlu et al. 2017; Davis and Heller 2019; Winters 2020).

Funding for Massachusetts public school students follows their school enrollment. Specifically, charter schools receive tuition payments from their students' home districts equal to district per-pupil expenditure. The state partially reimburses districts for charter school payments during a transition period, but these reimbursements have not been fully funded in recent years. Prior to 2010, Massachusetts law capped the overall number of charter schools at 120 and limited total charter school tuition to 9 percent of a district's spending. Charter expenditure in Boston reached this cap in fall 2009 (Boston Municipal Research Bureau 2008). As a result, the charter cap limited the expansion of charter schools in Boston before 2010.

B. *Charter Expansion*

In January 2010, Governor Deval Patrick signed An Act Relative to the Achievement Gap into law.[6] This reform relaxed Massachusetts' charter cap to allow the charter sector to double for districts in the lowest decile of performance according to a measure derived from test score levels and growth. The law also included provisions for school turnarounds and the creation of "innovation" schools (Massachusetts State Legislature 2010).

For Boston and other affected districts, the 2010 reform increased the limit on charter spending from 9 percent to 18 percent of district funds between 2010 and 2017. "Proven providers"—existing schools or school models the Massachusetts Board of Elementary and Secondary Education deemed effective—could apply to open new schools or expand enrollment. The law also allowed school districts to create up to 14 "in-district" charter schools without prior approval from the local teachers' union or proven provider status. Concurrent with the increased supply of charter seats, the law required charters to increase recruitment and retention

college preparatory curriculum, core values in school culture, selective teacher hiring or incentive pay, emphasis on math and reading, uniforms, hires Teach For America teachers, Teaching Fellows, or AmeriCorps members, affiliated with Teach For America alumni, data-driven instruction, and regular teacher feedback.

[6] See https://malegislature.gov/Laws/SessionLaws/Acts/2010/Chapter12.

efforts for high-need students and allowed charters to send advertising mailers to all students in the district.[7]

The state received 71 initial applications (some of which it solicited) for new charter schools or expansions from August 2010 to August 2012, and invited 60 percent of applicants to submit final round proposals. To determine whether a school model qualified for proven provider status, the Massachusetts Board of Elementary and Secondary Education compared existing schools using the model to other charters and traditional public schools. Criteria for this evaluation included enrollment of high-need students, attrition, grade retention, dropout, graduation, attendance, suspensions, and performance on state achievement tests (Massachusetts Department of Elementary and Secondary Education 2015). The state granted proven provider status to four of seven Boston charter middle schools, as well as to the KIPP organization, which operated a charter school in Lynn, Massachusetts, but had not yet entered Boston. Together, the provisions of the 2010 reform led to the establishment of 27 new charter campuses between 2011 and 2013, as well as expansions of 17 existing charter schools, typically to new grade levels (Massachusetts Department of Elementary and Secondary Education 2016).

Charter enrollment in Boston expanded rapidly after 2010. This can be seen in Figure 1, which plots shares of kindergarten, sixth-grade, and ninth-grade students attending charter schools. These statistics are calculated using the administrative enrollment data described below. Sixth-grade charter enrollment doubled after the reform, expanding from 15 to 31 percent between 2010 and 2015. Charter enrollment also grew substantially in elementary and high school, though not as dramatically as in middle school. The share of Boston students in charter schools increased from 5 percent to 13 percent in kindergarten and from 9 percent to 15 percent in ninth grade over the same time period.

The characteristics and practices of Boston's new expansion charter schools are broadly similar to those of their proven provider parent schools. This is shown in columns 2 through 4 of Table 1, which describe proven providers, other charters operating before 2010, and new expansions. Like proven providers, expansion schools have longer school days and years than BPS schools, and rate highly on the index of No Excuses practices. Per-pupil expenditure is similar at proven provider and expansion schools, and lower at other charters. New campuses located an average of 3.1 miles from their parent campuses, often expanding into different Boston neighborhoods (see Figure 2).

Expansion charter schools are primarily staffed by young teachers with little teaching experience. Table 2 reports that 78 percent of teachers at proven providers in the year before expansion were less than 32 years old, while 87 percent of expansion charter teachers were below this threshold in the year after expansion. These and other teacher characteristics come from an administrative database of Massachusetts public school employees (see the online Data Appendix). Columns 4 and 7 show that proven providers transferred some teachers from parent campuses to help staff

[7] The state's definition of high-need students includes those with special education or English language learner status, eligibility for subsidized lunch, or low scores on state achievement tests, as well as students deemed to be at risk of dropping out of school.

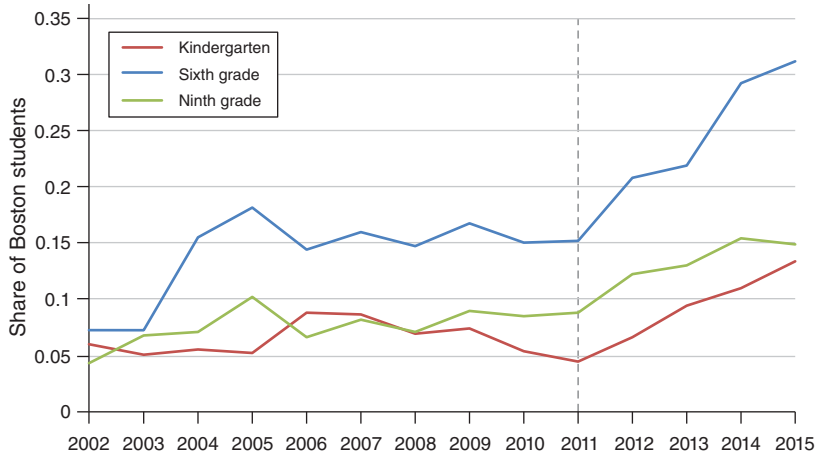

FIGURE 1. CHARTER SCHOOL ENROLLMENT IN BOSTON

Notes: This figure plots the share of Boston fourth-, sixth-, and ninth-grade students enrolled in charter schools between the 2001–2002 and 2014–2015 school years. The gray dashed line denotes the last school year before the charter expansion policy went into effect.

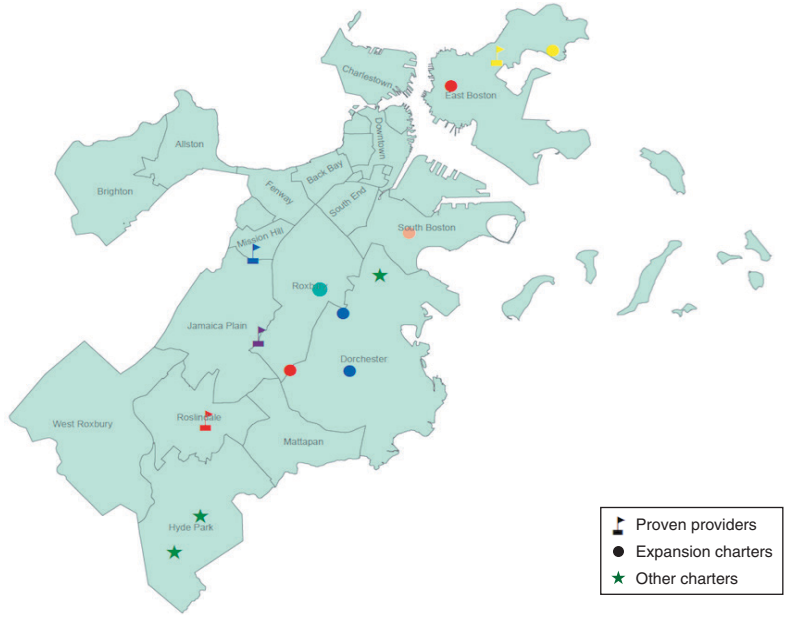

FIGURE 2. LOCATIONS OF BOSTON CHARTER SCHOOLS

Notes: This figure maps the location of the middle school charters in Boston, including schools that expanded (proven providers), new charter schools (expansion charters), and other charters. Each color denotes a different charter network.

their expansions: 12 percent of parent campus teachers moved to expansion campuses, accounting for 25 percent of the teaching workforce at these new schools.

Table 2—Staffing at Proven Provider and Expansion Charter Schools

		Teachers at proven providers in 2010–2011				Teachers at expansion charters in first year		
	BPS overall (1)	All (2)	Stay at parent campus (3)	Move to expansion (4)	Leave network (5)	All (6)	Came from parent campus (7)	New teacher (8)
Fraction in category	—	1.00	0.62	0.12	0.26	1.00	0.25	0.66
<32 years old	0.30	0.78	0.73	0.95	0.85	0.87	0.86	0.89
>49 years old	0.26	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Unlicensed	0.04	0.28	0.24	0.29	0.38	0.53	0.07	0.76
Years working in Massachusetts public schools	11.47	2.89	3.26	2.20	2.25	1.44	3.41	0.45
Observations (full time equivalent teachers)	4,261	88	54	11	22	55	14	36

Notes: This table describes characteristics of teachers at Boston charter schools before and after expansion. Column 1 summarizes Boston Public Schools (BPS) teacher characteristics in 2011–2012. Columns 2–5 display statistics for teachers working at proven provider charters in the 2010–2011 school year. Columns 6–8 show statistics for teachers working at expansion charters during the 2011–2012 school year. New teacher status in column 8 is defined as having less than one year of experience teaching in Massachusetts public schools. A small number of expansion charter teachers came from schools other than the parent campus and their characteristics are similar to teachers in column 7.

Transferred teachers were less experienced than teachers who remained at parent campuses (2.2 years versus 3.3 years). Most of the remaining expansion teachers had not previously taught in a Massachusetts school (66 percent), though a few transferred from other schools (9 percent). As a result, the average teacher at an expansion charter had only 1.4 years of teaching experience, compared to 2.9 years for teachers at parent campuses and 11.5 years for BPS teachers.

II. Data

A. *Data Sources and Sample Construction*

We study the effectiveness of Boston charter middle schools using records from randomized admission lotteries conducted between 2004 and 2013. Some charters serving middle school grades (fifth through eighth) accept students prior to fifth grade, mostly in kindergarten; we focus on schools with fifth- or sixth-grade entry because their lotteried students are old enough to take achievement tests within our data window. Our sample includes 14 of the 15 Boston charter schools with fifth- or sixth-grade entry, accounting for 94 percent of enrollment for schools in this category during the 2013–2014 school year.[8]

Lottery records typically list applicant names along with application grades, dates of birth, towns of residence, and sibling statuses. Our analysis excludes sibling applicants, out-of-area applicants, and students who applied to nonentry grades (siblings are guaranteed admission, while out-of-area applicants are typically ineligible). The lottery records also indicate which students received admission

[8] Two charter middle schools that closed before 2010 are excluded from this calculation. The one missing school declined to provide lottery records.

offers. We distinguish between immediate offers received on the day of the lottery and later offers received from the waitlist; in some lotteries, all students eventually receive waitlist offers. All offers are coded as waitlist offers in a few lotteries where we cannot distinguish between immediate and waitlist offers. Further information on school coverage and lottery records appears in online Appendix Tables A1 and A2. We use the "proven provider" label to refer to the four middle school charters in Boston that were granted permission to expand. The seven new campuses opened in the 2011–2012 and 2012–2013 school years are labeled "expansion charters," and the three remaining charter middle schools are "other charters."[9]

We match the lottery records to state administrative data based on name, date of birth, town of residence, and application cohort. The administrative data cover all students enrolled in Massachusetts public schools between 2002 and 2014. As shown in online Appendix Table A3, we find matches for 95 percent of lottery applicants in this database. Administrative records include school enrollment, gender, race, special education status, English language learner status, subsidized lunch status, and test scores on Massachusetts Comprehensive Assessment System (MCAS) achievement tests. We standardize MCAS scores to have mean zero and standard deviation one for Boston students by subject, grade, and year. In addition to information on charter lottery applicants, we use administrative data on other Boston students to describe changes in charter application and enrollment patterns after the 2010 reform. The online Data Appendix provides more details regarding data processing and sample construction.

B. *Descriptive Statistics*

Charter application and enrollment patterns in our analysis sample mirror the large increases in charter market share displayed in Figure 1. As shown in Table 3, 15 percent of eligible Boston students applied to charter schools with fifth- or sixth-grade entry before the 2010 reform, 12 percent received offers from these schools, and 10 percent enrolled. This implies roughly 1.5 applicants for each available charter seat. The application rate increased to 35 percent in 2013, and attendance reached 17 percent.[10] The increase in applications therefore outpaced enrollment growth, boosting the number of applicants per seat to 2. This increase in demand was particularly pronounced at other charter schools (neither proven providers nor expansions), which saw their applications per seat rise from 1.9 to 4.[11] After

[9] We categorize MATCH Middle School as a proven provider, as MATCH obtained that categorization from the state. MATCH's expansion campus opened at the elementary level, however. We categorize KIPP: Boston as an expansion campus, but this school does not have a direct parent campus in Boston as KIPP's only previous Massachusetts campus was in Lynn. We classify UP Academy as an expansion charter even though it opened under a different provision of the charter school law. To check whether our results are sensitive to these classification decisions, online Appendix Table A4 reports an alternative version of our main results with these three schools categorized as "other charters." The findings here remain generally the same, with other charters demonstrating slightly larger effects and proven providers and expansion schools showing smaller gains than in our preferred specification.

[10] These attendance percentages are lower than the percentages in Figure 1, since they exclude charter schools that enroll students at earlier entry grade levels.

[11] The number of applicants per seat is larger for each individual charter type than for the sector as a whole because some students apply to more than one school.

Table 3—Charter Middle School Applications and Enrollment

	Before charter expansion			After charter expansion			
	Any charter (1)	Proven providers (2)	Other charters (3)	Any charter (4)	Proven providers (5)	Expansion charters (6)	Other charters (7)
Percent of Boston students applying	15	9	8	35	19	19	18
Percent of Boston students with lottery offers	4	2	3	10	4	7	3
Percent of Boston students with lottery or waitlist offers	12	7	6	23	10	15	6
Percent of Boston students enrolling in charters	10	5	4	17	5	9	4
Applicants per seat	1.5	1.8	1.9	2.0	3.4	2.2	4.0

Notes: This table summarizes applications and enrollment for Boston charter middle schools in the analysis sample before and after the 2010–2011 charter sector expansion. The sample of charters excludes schools serving middle school grades with primary entry points prior to fifth grade. Students are included if they enrolled in Boston schools in both fourth and sixth grade. Columns 1–3 show statistics for cohorts of students entering fifth grade in fall 2008 or 2009. Columns 4–7 display statistics for cohorts entering fifth grade in fall 2011–2013.

Table 4—Characteristics of Boston Middle School Students

	Before charter expansion			After charter expansion			
	Enrolled	Randomized applicants		Enrolled	Randomized applicants		
	BPS (1)	All charters (2)	Proven providers (3)	BPS (4)	All charters (5)	Proven providers (6)	Expansion charters (7)
Female	0.478	0.487	0.485	0.476	0.493	0.484	0.486
Black	0.418	0.561	0.638	0.313	0.443	0.450	0.453
Latino/a	0.353	0.237	0.295	0.435	0.406	0.453	0.432
Asian	0.093	0.018	0.012	0.096	0.033	0.025	0.034
White	0.122	0.171	0.040	0.130	0.092	0.047	0.053
Subsidized lunch	0.839	0.687	0.742	0.792	0.802	0.835	0.831
English language learners	0.223	0.117	0.160	0.410	0.363	0.412	0.395
Special education	0.248	0.191	0.181	0.236	0.202	0.197	0.209
Attended charter in 4th grade	0.002	0.120	0.093	0.001	0.040	0.028	0.016
4th grade math score	—	0.220	0.043	—	0.021	0.020	−0.061
4th grade English score	—	0.303	0.156	—	0.023	−0.014	−0.090
Observations	18,934	2,724	1,263	8,330	4,478	2,250	2,414

Notes: This table shows descriptive statistics for Boston middle school students before and after the 2010–2011 charter school sector expansion. The sample includes all students who attended Boston schools in fourth grade and fifth or sixth grade between 2004 and 2013. Columns 1 and 4 show statistics for students who did not enroll in a charter school in fifth or sixth grade. Columns 2, 3, 5, 6, and 7 report statistics for randomized charter school applicants. Randomized applicants exclude siblings, disqualified students, and out-of-area applicants. Test scores are standardized to have mean zero and standard deviation one in Boston by subject, grade, and year.

the expansion, half of charter school sixth-grade students attended new expansion campuses.

Table 4 describes the characteristics of Boston middle school students in BPS and our randomized lottery applicant sample. Charter applicants are consistently more likely to be Black than BPS students. Both before and after 2010, students attending proven providers were less disadvantaged than other Boston students as measured by special education status, English learner status, and fourth-grade test scores.

As shown in Table 4, the characteristics of applicants to expansion charters differ markedly from those of other charter students. Special education and English learner rates are similar among expansion charter applicants and the BPS population. Expansion charter applicants also score below the BPS average on fourth-grade math and English tests, and are more likely than BPS students to be eligible for subsidized lunches. These facts indicate that expansion charters attract a more disadvantaged, lower-achieving population than their proven provider parent schools. This pattern may reflect the changes in recruitment practices resulting from the 2010 Achievement Gap Act, which mandated that charter schools take steps to enroll higher-need students and allowed charters to advertise directly to all students in the district by mail.

III. Empirical Framework

We use charter lottery offers as instruments for charter school attendance in a causal model with multiple endogenous variables, each representing enrollment in a type of charter school. The structural equation links charter attendance with outcomes as follows:

$$(1) \qquad Y_{ig} = \alpha_g + \sum_{k=1}^{K} \beta_k C_{ig}^k + \sum_{j=1}^{J} \delta_j R_{ij} + X_i'\gamma + \varepsilon_{ig},$$

where Y_{ig} is a test score for charter applicant i in grade g and C_{ig}^k measures years of enrollment in charter school type k through grade g.[12] Charter types include parent campuses, expansion campuses, and other charters; we also distinguish between enrollment before and after the charter expansion law. The parameters of interest, β_k, represent causal effects of an additional year of attendance at each charter type relative to traditional public schools.[13] The key control variables in equation (1) are a set of indicators, R_{ij}, for all combinations of charter lottery applications present in the data. Lottery offers are randomly assigned within these "risk sets." A vector of baseline demographic characteristics, X_i, is also included to increase precision. These characteristics, which are measured in the year prior to a student's lottery application, include gender, race, a female-minority interaction, subsidized lunch status, English language learner status, and special education status.

The first-stage equations for each charter enrollment type are given by

$$(2) \quad C_{ig}^k = \mu_g^k + \sum_{\ell=1}^{K} \left(\pi_{\ell 1}^k Z_{i1}^\ell + \pi_{\ell 2}^k Z_{i2}^\ell \right) + \sum_{j=1}^{J} \lambda_j^k R_{ij} + X_i'\theta^k + \eta_{ig}^k; \quad k = 1, \ldots, K.$$

Here, Z_{i1}^k denotes a dummy variable equal to one if applicant i received an immediate offer to attend charter type k on the day of a lottery, and Z_{i2}^k equals one if the applicant later received an offer from the waitlist. Like equation (1), the first stage also controls for lottery risk set indicators and baseline student characteristics.

[12] Test scores are the first instance that a student takes the MCAS in that grade. Years of enrollment includes repeated grades.

[13] If treatment effects vary across students or years of attendance these coefficients can be interpreted as average causal responses (ACRs), weighted averages of causal effects for individuals whose attendance is shifted by lottery offers (Angrist and Imbens 1995).

Two-stage least squares (2SLS) estimates are obtained by ordinary least squares (OLS) estimation of equation (1) after substituting predicted values from (2) for the charter attendance variables. The estimation sample stacks all post-lottery test scores in grades five through eight for randomized charter applicants, and standard errors are clustered by student to account for correlation in outcomes across grades.

Our empirical strategy is motivated by the fact that charter lottery offers are randomly assigned within lottery risk sets and are therefore independent of ability, family background, and all other predetermined student attributes. Online Appendix Table A5 presents a check on this by comparing baseline characteristics for offered and nonoffered applicants within lottery risk sets. These comparisons show that lottery winners and losers are similar for all charter school types and time periods, indicating that random assignment was successful.[14]

IV. Lottery Estimates

Proven provider charter schools generated larger achievement gains than other charter schools in Boston prior to the 2010 expansion. This can be seen in Table 5, which reports two-stage least squares estimates of equations (1) and (2).[15] The first-stage estimates in panel A show that charter offers boosted years enrolled by about one year before expansion, and around half a year after expansion. This reflects the fact that less time has elapsed in our data for cohorts applying after 2010, resulting in fewer years of potential charter enrollment between lottery and test dates. Columns 2 and 3 of panel B demonstrate that a year of charter attendance at a proven provider increased math and English scores by 0.32σ and 0.12σ prior to the reform, estimates that are highly statistically significant. Corresponding math and English effects for other Boston charters equal 0.18σ and 0.08σ. The difference in effects for proven providers and other charters is statistically significant in math ($p = 0.00$), though not in English. This finding indicates that policymakers selected more effective charter schools to be eligible for expansion.

Proven providers and other charters maintained their effectiveness after the charter expansion reform. As shown in columns 5 and 7 of Table 5, proven providers boost math and English scores by 0.37σ and 0.19σ per year of attendance after 2010, while other charters increase scores by 0.19σ and 0.13σ in this period. These estimates are slightly larger than estimates for earlier cohorts, though the differences between pre- and post-reform effects are not statistically significant for either group. As in the pre-reform period, the difference in effects between proven providers and other charters is significant in math ($p = 0.03$). These results indicate that

[14] Even with random assignment, selective attrition may lead to bias in comparisons of lottery winners and losers. Online Appendix Tables A3 and A6 show that the attrition rate from our sample is low: we match 95 percent of applicants to the administrative data, and find roughly 85 percent of post-lottery test scores that should be observed in our sample window for matched students. The match rate is 4 percent higher for students offered charter seats, and we are 3 percent more likely to find scores for students with lottery offers at non-proven-provider charters before 2010. This modest differential attrition seems unlikely to meaningfully affect the results reported below.

[15] Online Appendix Table A7 reports a pooled set of 2SLS estimates combining charter types and time periods. Reduced-form estimates are reported in online Appendix Table A8 and OLS estimates of charter school effects that control for prior test scores and baseline characteristics appear in online Appendix Table A9.

Table 5—Charter Effects on Test Scores before and after Charter Expansion

	Before charter expansion			After charter expansion			
		Estimates			Estimates		
	Non-charter mean (1)	Proven providers (2)	Other charters (3)	Non-charter mean (4)	Proven providers (5)	Expansion charters (6)	Other charters (7)
Panel A. First-stage estimates							
Immediate offer		1.304	1.554		0.795	0.659	0.930
		(0.067)	(0.047)		(0.054)	(0.046)	(0.052)
Waitlist offer		1.027	0.984		0.400	0.348	0.853
		(0.050)	(0.061)		(0.048)	(0.041)	(0.071)
Panel B. 2SLS estimates							
Math	0.117	0.320	0.183	−0.074	0.365	0.326	0.193
		(0.037)	(0.026)		(0.070)	(0.074)	(0.055)
p-value: Equals proven provider			0.000			0.632	0.030
p-value: Equals other charters						0.070	
Observations (applicants)	1,093	1,279	1,909	2,443	2,303	2,416	2,405
Observations (total scores)				17,395			
English	0.201	0.122	0.084	−0.032	0.186	0.229	0.126
		(0.037)	(0.025)		(0.074)	(0.076)	(0.054)
p-value: Equals proven provider			0.324			0.619	0.470
p-value: Equals other charters						0.162	
Observations (applicants)	1,087	1,277	1,911	2,441	2,307	2,420	2,412
Observations (total scores)				17,316			

Notes: Panel A reports first-stage effects of charter lottery offers on years of enrollment in charter schools. Panel B displays 2SLS estimates of the effects of charter school attendance on test scores. The sample stacks post-lottery test scores in grades five through eight. The endogenous variables are counts of years spent in the different charter types (pre-expansion proven providers, pre-expansion other charters, post-expansion proven providers, expansion schools, and post-expansion other charters). The instruments are immediate and waitlist lottery offer dummies for each school type. Immediate offer equals one for applicants offered seats on the day of the lottery. Waitlist offer equals one for applicants offered seats from the waitlist. Controls include lottery risk sets, as well as gender, race, ethnicity, a female-minority interaction, special education, English language learner, subsidized lunch status, and grade and year indicators. Standard errors are clustered by student.

expanding to operate new campuses did not dilute the effectiveness of proven provider charters at their original campuses.

Proven providers also successfully replicated their impacts at expansion schools. Column 6 of Table 5 demonstrates that a year of attendance at an expansion charter school increases math and English test scores by 0.32σ and 0.23σ. These estimates are comparable to estimates for parent campuses, and the hypothesis that expansion and proven provider effects are equal cannot be rejected at conventional levels ($p = 0.63$ and 0.62 in math and English). Estimated effects for expansion charters are larger than corresponding estimates for other charters during the same time period, though these differences are only marginally statistically significant for math and not statistically significant for English. Combined with the consistent effects for proven providers and other charters over time, these results indicate that Boston's charter middle school sector slightly increased its average effectiveness despite the growth in charter market share over this period.

These findings are robust to a number of specification checks. The results are generally similar when test scores are limited to the first year after entrance to a charter, though standard errors increase due to the reduction in power (online Appendix Table A10). Focusing on the first year addresses the concern that charter students might spend more time in grade before their exams due to grade retention.[16] Additionally, if charter impacts varied a great deal by grade level, the cross-period findings could be influenced by a different mix of grade levels, since there is a longer time horizon in the pre-expansion period. By limiting the findings to the year after the lottery, both the pre- and post-expansion groups outcomes are limited to fifth- and sixth-grade scores. In the truncated sample in math, proven providers outperform expansion campuses, though expansion campuses still have very large impacts and the difference is marginally significant. In English, the opposite is true. Both proven providers and expansion campuses retain their edge over other charter campuses.

Another specification check addresses issues raised by de Chaisemartin and Behaghel (2020), who argue that in cases with small lottery samples, the "waitlist offer" instrument can generate bias because the student receiving the final "waitlist offer" is different than the average waitlist member (as they respond affirmatively to the offer). Waitlists in our context are generally large, but we still address this concern by showing estimates that only use the initial offer on the day of the lottery as an instrument in online Appendix Table A11. As predicted, there are few differences in our findings, though we need to exclude a few lotteries where only waitlist offer information was retained. If anything, the charter effects are slightly larger in a specification with the initial offer-only instrument.

Finally, we also consider whether changes in peer quality or school switching are driving the charter findings. Panel A of online Appendix Table A12 shows 2SLS estimates of impacts on the baseline test scores of school-level peers; panel B shows 2SLS estimates of the likelihood of switching schools. Charter attendance boosts the likelihood that students attend school with higher-achieving peers. However, peer gains by charter type do not align with achievement gains. Other charters produce the largest gains in peer quality but the smallest impacts on test scores. Expansion campuses produce small changes in peer quality but large test score effects. The charter school peer advantage also diminishes over time; by the third year after the lottery, we find only small effects on peer quality in the pre-expansion period and minimal effects in the post-expansion period. Angrist et al. (2016) document a similar pattern for charter high schools. Furthermore, even in the first year, the peer effect would have to be *larger* than the peer differential to account for the full magnitude of the charter effects. The peer effects literature typically finds that peer effects transmit at a 10 to 30 percent rate (Sacerdote 2011). These results suggest that changes in peer quality are not the channel mediating charter gains.

Charter schools' effectiveness may be driven in part by decisions to push disruptive students out of schools. Our impact estimates account for this by assigning a student a full year of charter attendance even if he or she only attends a charter

[16] We also restrict test scores to the first in-grade exam and count years of charter attendance to include repeaters in all specifications.

for one day during the year. However, a direct investigation of effects on school switching is also of interest in view of concerns about selective push-out at charter schools. Panel B of online Appendix Table A12 reveals that winning a charter lottery reduces the likelihood that a student subsequently switches schools. This effect is partially due to differences in transition grades, as BPS students typically transition from elementary school to middle school in sixth grade whereas some charter middle schools start in fifth grade. When we examine school switching at nontransition grades, however, charter students remain 6 to 9 percentage points less likely to switch schools (though these differences are generally not statistically significant in the post-expansion period).

V. Exploring Effect Heterogeneity

Massachusetts' charter expansion reform led to a larger increase in Boston's charter market share than expansions evaluated in previous studies, which typically look at diffuse growth of charter networks across many markets (e.g., Tuttle et al. 2015). This suggests that mechanisms related to Boston's uniquely large within-market expansion may be important for understanding the effects of the reform. Charter skeptics commonly argue that charters succeed by "cream skimming" small numbers of unusually motivated students (Rothstein 2004). A large within-market expansion necessarily requires charters to enroll a new population of students, which may limit the scope for such cream skimming and change the mix of students that selects into the charter sector more generally. The role of student selection is especially policy relevant here since Massachusetts' expansion law encouraged charters to recruit and retain students with higher needs, as measured by criteria including English proficiency, special education status, and past achievement. Relatedly, a large literature also argues that school choice programs may affect the performance of traditional public schools, either through cream skimming and negative peer effects (e.g., Ladd 2002) or through competition that pressures public schools to improve (Hoxby 2003). We next investigate these potential mechanisms by exploring effect heterogeneity across students and fallback traditional public schools.

A. *Student Characteristics*

As a starting point for our investigation of student selection, online Appendix Table A13 summarizes effect heterogeneity as a function of observed student characteristics. The estimates show consistent positive impacts across most subgroups, charter school types, time periods, and subjects. Effects are similar for English language learners and students without this designation, though estimates for the former group are often imprecise due to small sample sizes. All estimates are positive for students with and without special education status; effects for special education students appear to be somewhat smaller at proven providers and larger at expansion charters, but these differences may be a chance finding due to the many splits examined. As in previous studies (e.g., Walters 2018), we find that effects tend to be larger for students with lower previous test scores. The large estimated effects for high-need subgroups at expansion charters are noteworthy: expansion schools

continue to generate substantial gains for these groups despite serving larger shares of such students than other Boston charters.

We analyze the consequences of this heterogeneity for the effectiveness of charter expansion via a Oaxaca-Blinder-style decomposition, which splits charter school treatment effects into components explained and unexplained by student characteristics (Oaxaca 1973, Blinder 1973). This decomposition is based on 2SLS estimates of the equation

$$Y_{ig} = \alpha_g + \sum_{k=1}^{K} \left(\beta_k^0 + X_i'\beta^x\right) C_{ig}^k + \sum_{j=1}^{J} \delta_j R_{ij} + X_i'\gamma + \varepsilon_{ig}. \tag{3}$$

Equation (3) allows a separate main effect for attendance at each charter type (β_k^0) as well as an interaction with student characteristics common across charter types (β^x). Charter exposure C_{ig}^k and its interactions with X_i are treated as endogenous. The immediate and waitlist offer variables for each charter type Z_{i1}^k, Z_{i2}^k, and their interactions with X_i are the excluded instruments.

Let $\bar{X}_k$ denote the average characteristics of students attending charter k, and let $\mu^x \equiv E[X_i]$ denote the mean of X_i for the Boston population. Assuming that all treatment effect heterogeneity is captured by observed characteristics, the effect of charter type k for students enrolled at k (the effect of treatment on the treated, TOT_k) can be represented as

$$\begin{aligned} TOT_k &= \beta_k^0 + \bar{X}_k'\beta^x \\ &= \underbrace{\left(\beta_k^0 + \mu^{x\prime}\beta^x\right)}_{ATE_k} + \underbrace{\left(\bar{X}_k - \mu^x\right)'\beta^x}_{Match_k}. \end{aligned} \tag{4}$$

This expression decomposes the TOT for charter type k into an average treatment effect for the Boston population, ATE_k, and a deviation from the average treatment effect due to the characteristics of type k's students, $Match_k$. If $Match_k > 0$, students with atypically high benefits select into the charter sector, while $Match_k < 0$ would imply that charter students benefit less than the average Boston student. We might expect a large charter expansion to reduce $Match_k$ by drawing in new students who, at the margin, benefit less from charter attendance than more eager students who attended when the sector was small. On the other hand, Walters (2018) argues that in earlier periods Boston's charter sector attracted students with lower than average gains, perhaps because the intensive charter treatment is more helpful for those with less motivated parents who are also less likely to seek alternative schooling options. We assess these ideas by studying estimates of ATE_k and $Match_k$ for each school type and time period.

Table 6 reports estimates of the components of the decomposition in equation (4) using gender, race, ethnicity, English language learner status, subsidized lunch, special education, and baseline test scores as interaction variables. Two-stage least squares estimates appear in panel A, and panel B displays results based on OLS estimates of equation (3) for comparison. As with the treatment effect estimates in online Appendix Table A9, the OLS decomposition results tend to be qualitatively similar and more precise than the 2SLS results. Estimated match components are

Table 6—Decomposition of Charter School Effects

	Before charter expansion		After charter expansion		
	Proven providers (1)	Other charters (2)	Proven providers (3)	Expansion charters (4)	Other charters (5)
Panel A. 2SLS results					
Math					
TOT	0.333	0.185	0.319	0.359	0.197
	(0.029)	(0.020)	(0.050)	(0.052)	(0.037)
ATE	0.320	0.198	0.321	0.345	0.208
	(0.030)	(0.022)	(0.051)	(0.053)	(0.038)
Match	0.013	−0.013	−0.002	0.014	−0.011
	(0.009)	(0.009)	(0.008)	(0.005)	(0.005)
Observation (scores)			15,924		
English					
TOT	0.185	0.100	0.156	0.207	0.096
	(0.030)	(0.020)	(0.053)	(0.051)	(0.039)
ATE	0.180	0.119	0.144	0.190	0.105
	(0.031)	(0.022)	(0.054)	(0.052)	(0.040)
Match	0.004	−0.019	0.013	0.016	−0.009
	(0.009)	(0.009)	(0.008)	(0.005)	(0.005)
Observations (scores)			15,932		
Panel B. OLS results					
Math					
TOT	0.365	0.234	0.307	0.326	0.228
	(0.009)	(0.009)	(0.011)	(0.014)	(0.011)
ATE	0.361	0.258	0.306	0.313	0.243
	(0.010)	(0.010)	(0.011)	(0.014)	(0.011)
Match	0.003	−0.023	0.001	0.013	−0.015
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)
Observations (scores)			84,246		
English					
TOT	0.275	0.094	0.203	0.164	0.200
	(0.010)	(0.010)	(0.011)	(0.013)	(0.010)
ATE	0.280	0.125	0.191	0.149	0.215
	(0.010)	(0.011)	(0.012)	(0.013)	(0.010)
Match	−0.004	−0.031	0.012	0.015	−0.015
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)
Observations (scores)			84,290		

Notes: This table decomposes estimates of charter school treatment effects into components explained and unexplained by student characteristics. These characteristics are female, Black, Latino/a, subsidized lunch, English language learner, special education, and baseline test scores. Estimates in panel A come from 2SLS models treating years of enrollment in each charter type and years in any charter interacted with student characteristics as endogenous, instrumenting with charter lottery offers and their interactions with student characteristics. These models control for main effects of student characteristics and lottery risk sets, and are estimated in the sample of randomized applicants. Estimates in panel B come from corresponding OLS models estimated in the full sample of Boston students. These models exclude lottery risk sets and include controls for Asian, non-white other race, baseline charter attendance, and a female-minority interaction.

close to zero for proven providers in both time periods, while match components for other charters are negative in both periods. This indicates that the demographic composition of other charters reduces their effectiveness, a result that is consistent

with Walters' (2018) finding that disadvantaged students were less likely to apply to Boston charter schools despite experiencing larger achievement benefits in data prior to the reform.

In contrast, column 4 of Table 6 reveals positive match effects for expansion charter schools. This pattern is due to the fact that expansion charters enroll a lower-achieving set of students compared to other charters (see Table 4). Since achievement gains are larger for this group, the match effect reinforces the effectiveness of expansion charters. The magnitudes of these match effects are relatively small, however, accounting for roughly 4 percent and 8 percent of the TOT in math and ELA. Changes in student characteristics increased the effectiveness of new charter campuses but were not the primary driver of the effectiveness of expansion schools.

B. *Fallback Schools*

One potential explanation for the success of Boston charter school expansion, where other efforts at program replication have been less successful, is that students in expansion campuses face particularly poor alternatives if they do not attend a charter school. Chabrier, Cohodes, and Oreopolous (2016) find that poor fallback school options are one of the strongest predictors of charter school effectiveness. It is also possible that charter schools influence the counterfactual by diverting resources from district schools (Arsen and Ni 2011, Bifulco and Reback 2014, Cook 2018, Ladd and Singleton 2020). However, Ridley and Terrier (2018) find small gains in district school finances (and test scores) in Massachusetts using the same charter expansion law.[17] Charter operators may have intentionally opened expansion campuses in areas of Boston with lower-performing traditional public schools. To see if low-quality fallback schools explain the success of expansion campuses, we compare fallback school conditions across charter school types, both before and after charter school expansion.

Table 7 shows average school-level value-added estimates for traditional public schools attended by students that enroll in district schools as a result of losing a charter lottery (untreated compliers).[18] Value-added estimates are OLS coefficients from regressions of test scores on school indicators, with controls for lagged test scores and demographics. Specification tests reported by Angrist et al. (2017) indicate that estimates from models of this type provide a reasonable proxy for school effectiveness. In both math and English, estimated value-added of the traditional public school fallback alternatives attended by charter applicants does not differ by charter school type, and these fallback schools appear to be of roughly average quality among schools in BPS. Students' fallback options

[17] Other studies of competitive effects of charter schools on nearby district schools' test scores generally find zero or small positive impacts (Booker et al. 2008; Cordes 2018; Jinnai 2014; Davis 2013; Sass 2006; Shin, Fuller, and Dauter 2017; Winters 2012; Zimmer et al. 2009; Zimmer and Buddin 2009). One exception is Imberman (2011), which found a mix of neutral and negative effects. For reviews of this literature, see Betts (2009); Gill and Booker (2008); Gill (2016); and Epple, Romano, and Zimmer (2016).

[18] We estimate untreated complier outcomes using methods from Abadie (2002).

Table 7—Value-Added of Fallback Schools for Charter Applicants

	Before charter expansion		After charter expansion		
	Proven providers (1)	Other charters (2)	Proven providers (3)	Expansion charters (4)	Other charters (5)
Untreated complier mean: math	0.008	0.015	0.028	0.017	0.027
	(0.010)	(0.009)	(0.015)	(0.017)	(0.013)
Observations	7,194				
Untreated complier mean: English	−0.015	−0.012	0.000	−0.007	−0.007
	(0.008)	(0.008)	(0.013)	(0.015)	(0.012)
Observations	7,194				

Notes: This table summarizes OLS value-added estimates for schools attended by untreated charter lottery compliers. Untreated complier means are estimates from 2SLS regressions of school value-added interacted with a traditional public school indicator on a set of variables equal to one minus attendance at each charter type, instrumented with charter lottery offers and controlling for demographics and lottery risk sets. School value-added estimates come from OLS regressions of test scores on a set of school indicator variables, controlling for lagged test scores and student demographics.

therefore do not seem to be an important component of variation in effects across charter types or time periods.

VI. School Practices

Our results so far show that changes in student characteristics and the quality of applicants' fallback schools do not explain the effectiveness of expansion charters. This suggests that successful replication of the Boston charter model may be driven by attributes of the expansion schools themselves. We explore this hypothesis by providing a more detailed account of organizational practices at parent and expansion charter schools in Boston. This portion of our analysis includes a qualitative overview of the mechanics of charter expansion based on interviews with school leaders,[19] as well as a quantitative assessment of teacher value-added that gives an indication of how heterogeneity in teacher quality is managed in traditional public and charter schools.

A. *Standardized School Models and Leadership*

Proven provider charter schools sought to maintain fidelity of their school models during expansion by emphasizing adherence to the same educational practices at new campuses. Table 1 shows a comparison of practices at parent and expansion charters based on information drawn from charter school annual reports.[20]

[19] S. Dunn, J. Clark, W. Austin, A. Hall, and D. Lehman, personal communication, May 2017.

[20] The Massachusetts Department of Elementary and Secondary Education provided the 2012–2013 annual reports for each of the Boston charter middle schools at our request. The state requires charter schools to submit annual reports and uses the reports when considering schools' charter renewal applications.

Expansion schools typically have the same amount of instructional time as their parent campuses, including identical length of the school day, time devoted to math and reading instruction, and days in the school year.[21] Expansion schools similarly implemented their parent campuses' No Excuses practices, tutoring, homework help, and Saturday school programs.

Expanding charter networks also tried to maintain similar pedagogical practices at old and new campuses. Teachers co-planned curricula, and teachers judged to be effective were encouraged to share their lesson plans across the network. This model of shared teaching resources was aimed at supporting new, inexperienced teachers, who comprised two-thirds of the new schools' staff. Survey evidence from Boston charters indicates that such collaboration is common within the sector, with 59 percent of new teachers reporting co-planning the curriculum with their peers (The New Teacher Project 2014). Recent evidence from other contexts shows that such collaboration can increase student achievement (Jackson and Bruegmann 2009; Ronfeldt et al. 2015; Papay et al. 2020; Sun, Loeb, and Grissom 2017) and that access to high-quality lesson plans also boosts student achievement (Jackson and Makarin 2018).

High teacher turnover rates are the norm at Boston charter schools. This is shown in Table 8, which summarizes teacher mobility patterns at charter and traditional public schools. As a result, some practices aimed at quickly training new teachers were in place prior to the 2010 reform. This may have aided schools' efforts to bring inexperienced teachers at new campuses up to speed on key practices. Two charter networks run their own teacher training programs and hired some of the graduates as full-time teachers. Charter networks also centralized teacher recruitment and professional development, potentially saving on search costs and resulting in similar types of teachers hired at new and old schools. Each network reported conducting some share of professional development at the network level.

Growing charter networks had stable leadership throughout the scaling-up process. Principals in the new and original campuses did not change throughout the expansion period in this study.[22] Furthermore, principals were trained internally: all of the principals at expansion campuses were former teachers from the original campus. School leaders who oversaw their networks' expansions stressed the value of selecting principals from within the network because of their familiarity with core school practices. Columns 3 and 6 of Table 8 show that roughly 4 percent of charter school teachers were promoted to a leadership position from 2011 to 2014, compared to less than 1 percent of BPS teachers.

B. *Evidence on Teacher Productivity*

The qualitative evidence above suggests that Boston charter schools limit teacher discretion by emphasizing a standard set of pedagogical practices, which may

[21] Edward Brooke's replication campus in East Boston is an exception, with six more days in its school year than its parent campus.

[22] We verified this in Education Personnel Information Management Systems (EPIMS), the educator database available from the Department of Elementary and Secondary Education, which contains yearly staff-level data for all employees in Massachusetts public schools.

Table 8—Teacher Movement

	BPS			Charters		
	New teachers (<5 years of experience) (1)	Experienced teachers (2)	All (3)	New teachers (<5 years of experience) (4)	Experienced teachers (5)	All (6)
Panel A. Year-to-year teacher mobility						
Percent remain teachers at school	70.9	78.9	76.4	68.5	76.1	69.7
Percent stop teaching at school	29.1	21.1	23.6	31.5	23.9	30.3
Observations	1,732	3,916	5,629	952	176	1,127
Panel B. Destinations for teachers who leave						
Percent teach at another school	13.4	10.1	11.2	7.6	9.7	7.9
Percent leave teaching	18.4	13.2	14.8	24.5	14.2	22.8
Percent become school leader	0.4	0.7	0.6	3.8	6.3	4.2
Observations	558	940	1,495	341	53	393

Note: This table summarizes the year-to-year changes in employment of teachers who taught in Boston Public Schools (BPS) or charter schools between 2010 and 2013. Panel A displays work status in the following year for each teacher-year observation. Panel B displays destinations for teachers who switched schools from one year to the next.

facilitate efforts to implement similar school models at new campuses. We assess this quantitatively by studying variation in teacher value-added at charter and district schools. Teacher value-added estimates come from the following model for achievement of student i in grade g in calendar year t:

$$(5) \qquad Y_{igt} = \alpha_g + \lambda_t + X'_{igt}\gamma + \beta_{s(i,g)} + \theta_{j(i,g)t} + \delta_{c(i,g,t)} + \xi_i + \varepsilon_{igt}.$$

The control vector X_{igt} includes student demographic characteristics and lagged test scores, as well as classroom-level averages of these variables. We also include grade (α_g) and calendar year (λ_t) fixed effects. The function $s(i,g)$ labels the school that student i attends for grade g, $j(i,g)$ describes the identity of her grade g teacher, and $c(i,g,t)$ denotes a specific classroom. Because classroom-level averages of the observables are included as controls, equation (5) describes a "correlated random effects" model in which the mean of the teacher effect distribution may depend on the characteristics of students in the classroom (Mundlak 1978, Chamberlain 1982). In other words, we are not imposing independence of teacher quality from student observables.

We also allow school and teacher effects to depend on observed school and teacher characteristics. The mean of the distribution of school effects β_s differs for charter and traditional public schools. The teacher effects (which measure variation in teacher effectiveness within school) are in turn written

$$\theta_{jt} = \theta_j^0 + W'_{jt}\theta^w,$$

where W_{jt} includes teacher j's experience as of year t in one of three experience groups (novice, one to four years of experience, and greater than five years of experience) as well as interactions of charter status with experience. Given the small number of charter teachers in the sample, we do not separate teachers at proven

Table 9—Teacher Value-Added Estimates

	Math			English		
	Charter (1)	BPS (2)	*p*-value: Charter = BPS (3)	Charter (4)	BPS (5)	*p*-value: Charter = BPS (6)
Experience profile						
One to four years of experience	0.085	0.094	0.835	0.044	0.091	0.261
	(0.024)	(0.036)		(0.023)	(0.034)	
Five or more years of experience	0.053	0.080	0.600	0.047	0.096	0.307
	(0.035)	(0.038)		(0.033)	(0.034)	
Random effect parameters: SD						
School	0.135	0.102	0.315	0.091	0.111	0.449
	(0.028)	(0.016)		(0.021)	(0.015)	
Teacher	0.118	0.185	0.000	0.104	0.178	0.000
	(0.012)	(0.010)		(0.012)	(0.008)	
Class	0.076	0.152	0.000	0.081	0.116	0.001
	(0.008)	(0.005)		(0.009)	(0.005)	
Observations	48,416			54,075		

Notes: This table shows teacher value-added across years of experience and variance in teacher value-added at the school, teacher, and classroom level. These results come from maximum likelihood estimation of a random effects model with normally distributed teacher, school, and classroom effects. The model controls for student demographics and lagged test scores as well as class averages of these variables. The mean of the school effect varies with charter status, and the mean of the teacher effect varies by experience and experience interacted with charter status.

providers, expansions, and other charters for the purposes of the value-added model, nor do we estimate experience premia for each year.[23] We model the school effects β_s, within-school teacher effects θ_j^0, and classroom effects δ_c as normally distributed conditional on X_{igt}, with variances that differ in charter and traditional public schools. The student random effect ξ_i and idiosyncratic error ε_{igt} are also modeled as normally distributed. Random effects specifications of this sort are common in the literature on teacher value-added, and previous studies have argued that such models generate estimates of teacher effectiveness that exhibit little selection bias (Kane, Rockoff, and Staiger 2008; Chetty, Friedman, and Rockoff 2014).[24]

As can be seen in Table 9, maximum likelihood estimation of model (5) reveals two notable patterns. The first is revealed by comparing variation in school, teacher, and class effects across the charter and traditional sectors. Both charter and district schools have similar variation in school-level effectiveness. At the teacher and classroom levels, we find less variation in effectiveness in the charter sector. In math, the standard deviation of the teacher random effect θ_j^0 is 0.12σ compared to 0.19σ in BPS, while the standard deviation of the class effect δ_c is 0.08σ compared to 0.15σ. The distribution of teacher and class effects are similarly compressed for English

[23] Data for the value-added model are from 2011–2014, the years in which it is possible to link students, teachers, and classrooms in our data.

[24] Our findings are robust to other approaches of measuring value-added. We estimated alternative specifications using teacher random effects, teacher and school random effects, teacher fixed effects, and school and teacher fixed effects. We also estimated models excluding students who attended both charter and BPS during middle school, and used finer measures of teacher experience. These alternative approaches yielded similar patterns of results.

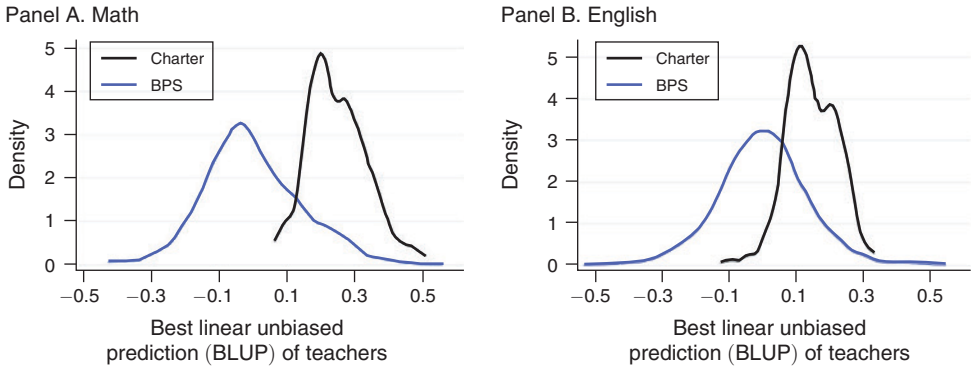

FIGURE 3. DISTRIBUTION OF TEACHER-SPECIFIC POSTERIOR MEAN PREDICTIONS OF VALUE-ADDED

Note: This figure plots the distribution of teacher-specific posterior mean predictions of value-added for charter and Boston Public School (BPS) teachers.

scores: the standard deviation of θ_j^0 is 0.11σ in charters versus 0.18σ in BPS, and the standard deviation of δ_c is 0.08σ versus 0.12σ in BPS.[25]

Figure 3 displays distributions of posterior mean predictions of individual teacher value-added based on estimates of equation (5) separately for charter and traditional public schools. Note that the charter school teacher impacts are centered at the mean of the charter school effects.[26] These distributions are visibly less diffuse than that of their traditional public school counterparts, and appear to show a compressed distribution of effects rather than a truncated tail on either end.[27]

Overall, the evidence in Table 9 and Figure 3 suggests that the charter sector reduces variation in teacher effectiveness within schools, which may be due to charters' centralized management of teachers and standardized instructional practices.[28] Charter schools might also hire a population of teachers with less variation in practices. The reduction in variation at the classroom level (which is typically attributed to random events like construction noise on test day) suggests some of this variation is systematic and can be reduced through standardized practices as well.

A second pattern revealed by the value-added analysis is that returns to teacher experience seem less pronounced in charter schools than in BPS. Comparing

[25] The results found here—that charter value-added standard deviations are around 0.11 and district about 0.18—indicate that charter schools in Boston are toward the minimum known range of teacher value-added estimates, whereas Boston district schools are in the middle of the distribution. Hanushek and Rivkin (2010) review the dispersion of teacher value-added in 10 localities, and find that the standard deviation of teacher effects ranges between 0.11 and 0.36σ in math and 0.10 and 0.26σ in reading.

[26] This implies that comparisons between teachers are made within but not between sectors (BPS and charter). Cross-sector comparisons would require an analysis based on teachers switches between BPS and charter schools, which occur infrequently in our sample.

[27] This result is somewhat speculative due to the noisy charter estimates. Note in addition that Figure 3 analyzes the distribution of posterior means rather than the true underlying distribution of latent teacher effects.

[28] Taylor (2018) and Jackson and Makarin (2018) also show compression of teacher value-added distributions in settings with standardized instructional practices. In Taylor (2018), standardization comes from the use of computer-aided instruction; in Jackson and Makarin (2018), standardization comes from access to high-quality instructional materials.

teachers with one to four years of experience and teachers with five or more years of experience to novices shows that more experienced teachers generally outperform new teachers. However, the experience premium is larger in BPS (though the differences are not statistically significant), with teachers with one to four years of experience outperforming novice teachers in BPS by about 0.09σ in both math and English. The corresponding experience premium for teachers in charter schools is similar in math but about half the size in English, at 0.04σ. For teachers with more than five years of experience, BPS teachers maintain their edge relative to novices, but any premium for charter school teachers is small and not statistically significant (though again the experience profiles in charter schools and BPS are not statistically distinguishable). In short, either through selection of teachers or through training, charter schools appear to dampen one of the most persistent findings in the literature on teacher effectiveness (Harris and Sass 2011; Papay and Kraft 2015; Clotfelter, Ladd, and Vigdor 2007; Rockoff 2004)—that teachers make significant gains in their first few years of teaching. Teachers at charter schools deliver effective education despite the high proportion of novice teachers and substantial teacher turnover. Taken together, the conclusions from the value-added analysis are consistent with the hypothesis that highly standardized management practices may contribute to the successful replication of charter school effects.

VII. Conclusion

The replication and expansion of successful schools is one strategy to address persistent achievement gaps in the United States. The efficacy of this strategy requires schools selected for expansion to maintain their success at new locations and with new student populations. Previous research has shown that urban No Excuses charter schools boost test scores markedly for small groups of applicants, suggesting the potential for transformational effects on urban achievement if these gains can be maintained at larger scales. We examine a recent policy change in Massachusetts that doubled Boston's charter sector over a short time period, allowing us to evaluate changes in the effects of No Excuses charters as these schools expanded to serve a larger share of the population within a single school market.

Our results demonstrate that Boston's No Excuses charters reproduced their effectiveness at new campuses. Lottery-based estimates show that schools selected for expansion produced larger gains than other charters in the pre-reform period, indicating that Massachusetts' accountability regime successfully identified more successful schools. New expansion campuses generate test score gains similar to those of their parent campuses despite a doubling of charter market share in middle school.

The demographics of students served by expansion charters are similar to those of the Boston population as a whole, suggesting that charter effectiveness is not driven by unique peer environments. We find that changes in student populations and the quality of fallback traditional public schools play only a small role in the effectiveness of charter expansion. Both a qualitative analysis of organizational practices during expansion and a quantitative analysis of variation in teacher value-added indicate that charter schools use a highly standardized model that

limits variation in practices across schools and classrooms. This standardized approach may facilitate the portability of charter effectiveness to new campuses. More broadly, the role of these and other organizational practices in explaining successful replication of social programs is an important area for future work.

This paper also provides evidence on the efficacy of different organizational forms for replicating social programs. When a program is successful, policymakers face the decision of whether to have the original implementer continue to provide the program, or whether governments or other agencies should take over the program at a larger scale. This paper shows that in the charter school context, replicating existing charters is a viable strategy for charter expansion. This is consistent with the findings of Bold et al. (2018), who show that the successful Kenyan contract teacher program evaluated in Duflo, Dupas, and Kremer (2011, 2015) was replicated with provision by the original provider but not by the government (despite an identical contract). The "proven provider" design of the Massachusetts 2010 charter law is unique among the states with charter school laws, and it remains to be seen if other states or charter authorizers adopt such policies.[29] However, the share of charter schools managed by charter school management organizations (independent, nonprofit organizations that manage two or more charter schools) has grown from 16 percent in 2009 (Furgeson et al. 2011) to 23 percent in 2017 (David 2018), indicating that the market may institute a replication strategy even if authorizers do not.

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[29] Denver Public Schools also has a separate process allowing both existing traditional public and charter schools to apply to open a replication campus. Similarly, the federal government supports charter replication through grant competitions; the Massachusetts law change is the only replication program we are aware of that has been codified into state law.

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Online Appendix

Can Successful Schools Replicate? Scaling Up Boston's Charter School Sector

Sarah R. Cohodes, Elizabeth M. Setren, and Christopher R. Walters

1 Data Appendix

We use lottery records, student demographic and enrollment data, state standardized test scores, and school personnel files in this article. Lottery records collected from individual schools contain the list of applicants, offer status, and factors that affect an applicant's lottery odds, including sibling status, disqualifications, late applications, and applying from outside of Boston. The Student Information Management Systems (SIMS) dataset contains enrollment and demographic data for all public school students in Massachusetts. Student standardized test scores come from the state database for the Massachusetts Comprehensive Assessment System (MCAS). The Massachusetts Education Personnel Information Management Systems (EPIMS) database provides school staff information. Next we describe these datasets, the matching process, and sample construction.

1.1 Lottery records

Massachusetts legally requires charters to admit students via lottery when more students apply to a charter school than the number of available seats for a given grade. Our paper uses records from charter lotteries conducted between spring 2004 to spring 2013 for 14 charter schools accepting students in 5th or 6th grade. Each of the 14 schools contributes oversubscribed lottery data.[1] Schools vary in the grades they serve and in years of operation. Table A1 lists this information and the years each school contributes to the analysis. We exclude one school that did not provide lottery records (Smith Leadership Academy) and two schools that closed before the charter expansion (Uphams Corner Charter School in 2009 and Frederick Douglass Charter School in 2005). Lottery data typically includes applicants' names, dates of birth, and lottery and waitlist offer status. Offers to attend charter schools either occur on the day of the lottery (referred to as $immediate\ offer$) or after the day of the lottery when students receive offers from the randomly sequenced waitlist as seats become available. In three out of the 65 lotteries in the study, the schools gave all applicants offers or did not give waitlist offers to non-siblings. Four lotteries did not distinguish the timing of the offers so we code the immediate offer variable to equal zero for these cohorts.

The Uncommon Schools/Roxbury Preparatory charter network held a single lottery for its three campuses in the Spring 2012 and Spring 2013 lotteries. When the school called a student's lottery number, the student could pick from the campuses that still had open seats. Our lottery records show which campus they picked at the time of the lottery. We find the last lottery number for each campus and code all students with better lottery numbers as having offers from that campus.

Uncommon Schools offered seats from the waitlist as they became available for individual campuses. Parents chose to accept or decline waitlist offers for single schools. If they declined, they were taken off the waitlist and would not be considered for seats at the other campuses.

1.2 Enrollment and demographics

The SIMS data contains individual level data for students enrolled in public schools in Massachusetts from 2003-2004 through 2013-2014. The data contains snapshots from October and the end of the school year. Each student has only one observation in each time period, except when

[1] We do not have Spring 2004 lottery records for Brooke Roslindale, Boston Prep, and Academy of the Pacific Rim or Spring 2005 records for Brooke Roslindale. Brooke Roslindale does not have lotteries in after charter expansion because their elementary school students filled the middle school seat. All other schools and years have oversubscribed lottery data.

students switch grades or schools within year. Fields include a unique student identifier, grade level, year, name, date of birth, gender, ethnicity, special education status, limited English proficiency status, free or reduced price lunch status, school attended, suspensions, attendance rates, and days truant.

We code students as charter attendees in a school year if they attended a charter at any point during a year. Students who attend more than one charter school in a year are assigned to the charter they attended the longest. Students who attend more than one traditional public school and no charter schools in a year are assigned to the school they attended the longest. We randomly choose between schools if students have attendance ties between the most attended schools.

1.3 Test scores

This paper uses individual student math and English Language Arts (ELA) Massachusetts Comprehensive Assessment System (MCAS) test scores from 2003-2004 through 2013-2014. Massachusetts public school students take the exam each year in grades grades 5 through 8. Data includes the unique student identifier. We standardize the raw scores to to have a mean of zero within subject-grade-year in Boston.

1.4 Staff records

The Education Personnel Information Management Systems (EPIMS) contains yearly staff level data for all employees in Massachusetts public schools. We use data collected in October of the 2007-08 through the 2013-14 school years. Data includes job position, school, full time equivalency, date of birth, date of hire for first public school job in Massachusetts, license status, and highly qualified status. We use the full time equivalency of all staff and teachers. If one school has two half time teachers, they are counted as one full time equivalent teacher. A teacher who teaches at multiple schools counts towards the staff statistics at each school.

1.5 Matching data

We use applicants' names, date of birth, grade, and year to match their lottery records to the state enrollment data. The applicants who uniquely and exactly match the grade, year, name, and date of birth (if available) in the state records are assigned to the matched unique student id. After this initial match, we strip names in the lottery and enrollment data of spaces, surnames, hyphens, and apostrophes. Unique matches after this cleaning are assigned to the matched unique student id. Then, we use reclink, a fuzzy matching STATA program, to suggest potential matches for the remaining students. This matches students with slight spelling differences and those who appear in one grade older or younger than the charter application grade. We hand check these suggested matches for accuracy. We search for the remaining unmatched students by hand in the data. Typically this last group contains name truncations, name misspellings, or first and last names in the wrong field.

The matching process assigns 95 percent of applicants to the state administrative records (see Table A3). Students who do not match either enroll in private, parochial, or out-of-state schools, have names and birthdates too common to match, or have spelling errors too extreme to match with confidence. Receiving a charter offer makes students 3.8 more likely to match to the data,

as shown in Table A3. As a result, our findings show causal estimates for the set of students who enroll in Massachusetts Public Schools.

We match the enrollment and demographic data to the student test scores using the unique student identifier. Students who attend out of state, private, or parochial schools do not have test score outcomes for their years outside of Massachusetts public schools.

1.6 Sample restrictions

We exclude applicants who receive higher or lower preference in the lottery. Late applicants, those who apply to the wrong grade, out-of-area applicants, and siblings fall into these categories and typically have no variation in offer status. When students have duplicate applications within an individual school's lottery, we keep only one application. If students apply to charter schools in different years, we use only the first application year. We restrict the sample to students with baseline demographics data, excluding students applying from outside of Massachusetts public schools. With these restrictions imposed, the original raw sample of applications narrows from 20,981 to 8,473.

References

Massachusetts Department of Elementary and Secondary Education. 2011-2014. Educator Personnel Information Management System (EPIMS). Commonwealth of Massachusetts. Multiple electronic files.

Massachusetts Department of Elementary and Secondary Education. 2001-2014. Massachusetts Comprehensive Assessment System (MCAS). Commonwealth of Massachusetts. Multiple electronic files.

Massachusetts Department of Elementary and Secondary Education. 2011-2014. Student Course System (SCS). Commonwealth of Massachusetts. Multiple electronic files.

Massachusetts Department of Elementary and Secondary Education. 2001-2014. Student Information Management System (SIMS). Commonwealth of Massachusetts. Multiple electronic files.

2 Additional Tables

Table A1: Charter Middle Schools in Boston

	Year Opened (1)	Grades (2)	Outcome Years In Analysis (3)
Proven Providers			
Roxbury Preparatory: Mission Hill Campus	1999 - 2000	5 - 8 (12)	2004-05 - 2013-14
Brooke Roslindale	2002 - 03	5 - 8	2006-07 - 2009-10
Excel East Boston	2003 - 04	5 - 9 (12)	2008-09 - 2013-14
MATCH Middle School	2008 - 09	6 - 8	2008-09 - 2013-14
Expansion Charters			
Roxbury Preparatory: Lucy Stone Campus	2011 - 12	5 - 8	2011-12 - 2013-14
Roxbury Preparatory: Dorchester Campus	2012 - 13	5 - 7 (8)	2012-13 - 2013-14
Brooke Mattapan	2011 - 12	5 - 8	2011-12 - 2013-14
Brooke East Boston	2012 - 13	5 - 7 (8)	2012-13 - 2013-14
Excel Orient Heights	2012 - 13	5 - 7 (8)	2012-13 - 2013-14
KIPP	2012 - 13	5 - 7 (8)	2012-13 - 2013-14
UP Academy Boston	2011 - 12	6 - 8	2011-12 - 2013-14
Other Charters			
Academy of the Pacific Rim	1997 - 98	5 - 12	2005-06 - 2013-14
Boston Collegiate	1998 - 99	5 - 12	2004-05 - 2013-14
Boston Prep	2004 - 05	6 - 12	2005-06 - 2013-14
Not Included in Study			
Helen Davis Leadership Academy	2003 - 04	6 - 8	declined to participate
Frederick Douglas Charter	2000 - 01	6 - 10	closed in 2004-05
Uphams Corner Charter	2002 - 03	5 - 8	closed in 2008-09

Notes: This table lists Boston middle school charter schools by school type, opening year, grade levels, and outcome years included in the analysis. Grade levels shown in parentheses indicate planned enrollment grades which were not present at the time of analysis.

Table A2: Lottery Records

Year of application	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	All
Total number of records	341	739	913	1143	1422	1595	1467	4283	4312	4766	20981
Excluding disqualifed applications	341	738	911	1135	1404	1594	1444	4273	4305	4760	20905
Excluding late applications	340	738	909	1135	1363	1566	1397	4163	4196	4583	20390
Excluding out of area applications	340	733	900	1123	1353	1548	1379	4094	4071	4513	20054
Excluding siblings	300	677	836	1021	1223	1408	1249	3758	3760	4320	18552
Excluding records not matched to SIMS	266	634	801	1000	1181	1378	1179	3627	3573	4016	17655
Keep only first year of charter application	266	617	770	962	1093	1282	1038	3308	2962	3469	15767
Excluding repeat applications	266	617	770	962	1093	1282	1038	3308	2962	3458	15756
Reshaping to one record per student	265	523	586	760	868	963	812	2055	1715	1900	10447
Has baseline demographics and in Boston at baseline	176	382	437	571	679	722	623	1790	1499	1594	8473

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

Table A3: Match from Lottery Data to Administrative Data

			Regression of Match on Offer	
Lottery Year	Number of Applications (1)	Proportion Matched (2)	Immediate Offer (3)	Any Offer (4)
2004	268	0.989	-0.006	-0.007
			(0.026)	(0.013)
2005	616	0.987	-	0.002
			-	(0.013)
2006	742	0.991	-	0.004
			-	(0.016)
2007	924	0.984	0.019**	0.034***
			(0.008)	(0.013)
2008	1018	0.957	0.042***	0.061***
			(0.013)	(0.019)
2009	1106	0.977	0.004	0.011
			(0.011)	(0.010)
2010	1041	0.924	0.065***	0.071***
			(0.016)	(0.017)
2011	2614	0.954	0.018***	0.025***
			(0.007)	(0.007)
2012	2503	0.939	0.001	0.033***
			(0.011)	(0.011)
2013	2712	0.902	0.045***	0.078***
			(0.012)	(0.015)
All Cohorts	15482	0.949	0.023***	0.038***
			(0.003)	(0.004)

Notes: This table summarizes the match from the lottery records to administrative student data. The sample excludes late applicants, siblings, disqualified applicants, duplicate names, and out-of-area applicants. Columns (3) and (4) report coefficients from regressions on a dummy for a successful match on immediate and any charter offer dummies. All regressions control for school-by-year dummies.
*significant at 10%; **significant at 5%; ***significant at 1%

Table A4: Alternative Definition of Proven Provider & Replicate

	Before Charter Expansion			After Charter Expansion			
		2SLS			2SLS		
	Non-Charter Mean	Proven Providers	Other Charters	Non-Charter Mean	Proven Providers	Expansion Charters	Other Charters
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Math	0.117	0.337***	0.201***	-0.074	0.314***	0.218**	0.207***
		(0.043)	(0.025)		(0.087)	(0.092)	(0.044)
N				17395			
English	0.201	0.162***	0.091***	-0.032	0.155*	0.202**	0.105**
		(0.043)	(0.024)		(0.089)	(0.094)	(0.044)
N				17316			

Notes: This table reports the main 2SLS results from Table 6, but using alternative charter school type categorizations. In this robustness check, MATCH Middle School, UP Academy Boston, and KIPP Boston are considered other charters.
*significant at 10%; **significant at 5%; ***significant at 1%

Table A5: Covariate Balance

	Before Charter Expansion		After Charter Expansion		
	Proven Providers	Other Charters	Proven Providers	Expansion Schools	Other Charters
	(1)	(2)	(3)	(4)	(5)
Female	0.000	-0.004	-0.005	0.011	0.020
	(0.034)	(0.028)	(0.027)	(0.027)	(0.028)
Black	-0.026	0.007	-0.027	-0.025	-0.015
	(0.032)	(0.027)	(0.027)	(0.026)	(0.028)
Latino/a	0.027	0.000	-0.001	0.005	-0.010
	(0.031)	(0.022)	(0.027)	(0.026)	(0.027)
Asian	-0.014	0.007	0.008	0.010	0.000
	(0.009)	(0.008)	(0.010)	(0.011)	(0.009)
White	0.016	-0.003	0.007	0.001	0.018
	(0.011)	(0.024)	(0.010)	(0.012)	(0.017)
Subsidized Lunch	0.015	0.010	-0.011	-0.016	-0.016
	(0.029)	(0.027)	(0.020)	(0.019)	(0.023)
English Language Learners	-0.005	-0.001	-0.004	-0.039	-0.027
	(0.023)	(0.014)	(0.027)	(0.026)	(0.025)
Special Education	-0.005	0.005	0.002	0.013	0.018
	(0.027)	(0.022)	(0.021)	(0.022)	(0.022)
Attended charter before applying	0.010	-0.008	-0.015	-0.015*	-0.003
	(0.019)	(0.020)	(0.010)	(0.008)	(0.014)
Baseline math score	-0.024	-0.022	0.058	-0.033	-0.004
	(0.071)	(0.053)	(0.050)	(0.051)	(0.055)
Baseline English score	-0.036	0.000	0.048	0.037	0.011
	(0.071)	(0.052)	(0.053)	(0.051)	(0.055)
N (offered)	1009	1309	1466	1825	1142
P-value	0.594	0.891	0.526	0.136	0.979

Notes: This table reports coefficients from regressions of baseline characteristics on charter offers, controlling for lottery risk set indicators. P-values are from tests of the hypothesis that all coefficients are zero.
*significant at 10%; **significant at 5%; ***significant at 1%

Table A6: Attrition

	Before Charter Expansion			After Charter Expansion			
		Offer Differential			Offer Differential		
	Non-offered Followup Rate	Proven Providers	Other Charters	Non-offered Followup Rate	Proven Providers	Expansion Charters	Other Charters
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Math	0.834	0.018	0.032**	0.869	0.000	0.013	-0.023
		(0.018)	(0.015)		(0.015)	(0.016)	(0.018)
N				20102			
English	0.825	0.018	0.034**	0.869	0.001	0.011	-0.025
		(0.017)	(0.015)		(0.015)	(0.016)	(0.018)
N				20102			

Notes: This table investigates attrition for randomized charter school lottery applicants. Columns (1) and (4) report fractions of follow-up test scores in grades five through eight that are observed for students not offered seats. Columns (2)-(3) and (5)-(7) report coefficients from regressions of a follow-up indicator on a lottery offer indicator (immediate or waitlist) and students not offered seats. Regressions control for lottery risk sets, as well as gender, ethnicity, a female-minority interaction, special education, English language learner, subsidized lunch status, and grade and year indicators. Standard errors are clustered by student.

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

Table A7: Overall Charter Effects

		First Stage	Reduced Form	2SLS
		(1)	(2)	(3)
Math		0.978***	0.218***	0.223***
		(0.025)	(0.023)	(0.023)
	N		17395	
English		0.977***	0.120***	0.123***
		(0.025)	(0.022)	(0.022)
	N		17316	

Notes: This table reports first stage, reduced form, and 2SLS estimates for the full sample of lotteried charter middle schools across all years and schools. The endogenous variable is years in any charter school and the instrument is any charter offer. The sample stacks post-lottery test scores in grades five through eight. Models control for baseline covariates and lottery risk sets. Standard errors are clustered by student.

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

Table A8: Reduced Form Charter Effects on Test Scores

	Before Charter Expansion		After Charter Expansion		
	Proven Providers (1)	Other Charters (2)	Proven Providers (3)	Expansion Charters (4)	Other Charters (5)
Math	0.386***	0.146***	0.157***	0.119**	0.102**
	(0.047)	(0.039)	(0.046)	(0.046)	(0.051)
English	0.157***	0.095***	0.069	0.093**	0.056
	(0.044)	(0.036)	(0.043)	(0.045)	(0.048)

Notes: This table shows reduced form estimates of the effects of charter offers on math and English scores. Charter offer equals one if the student receives an immediate or a waitlist offer. See Table 5 for detailed regression specification notes.

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

Table A9: Ordinary Least Squares Estimates of Charter Effects

	Before Charter Expansion			After Charter Expansion			
		OLS			OLS		
	Non-Charter Mean	Proven Providers	Other Charters	Non-Charter Mean	Proven Providers	Expansion Charters	Other Charters
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Math	0.004	0.355***	0.228***	-0.059	0.299***	0.322***	0.212***
		(0.010)	(0.010)		(0.011)	(0.014)	(0.010)
N (students)				31218			
N (scores)				84246			
English	0.009	0.268***	0.088***	-0.032	0.185***	0.161***	0.181***
		(0.010)	(0.011)		(0.012)	(0.013)	(0.010)
N (students)				31242			
N (scores)				84290			

Notes: This table reports ordinary least squares (OLS) estimates of the effects of time spent in charter schools on math and English scores for students who attend a school in Boston in the fourth grade. The sample stacks scores in grades five through eight for all Boston students. All regressions control for fourth grade math and English scores, as well as gender, ethnicity, a female-minority interaction, special education, English language learner, subsidized lunch status and grade and year indicators.

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

Table A10: Charter Effects on Test Scores One Year After the Lottery

	Before Charter Expansion			After Charter Expansion			
		Estimates			Estimates		
	Non-Charter Mean	Proven Providers	Other Charters	Non-Charter Mean	Proven Providers	Expansion Charters	Other Charters
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: First Stage Estimates							
Immediate Offer		0.748***	0.768***		0.572***	0.431***	0.620***
		(0.031)	(0.021)		(0.031)	(0.028)	(0.031)
Waitlist Offer		0.585***	0.471***		0.274***	0.258***	0.491***
		(0.025)	(0.029)		(0.026)	(0.026)	(0.039)
Panel B: 2SLS Estimates							
Math	0.112	0.482***	0.201***	-0.054	0.521***	0.305**	0.076
		(0.075)	(0.063)		(0.106)	(0.121)	(0.086)
P -value: Equals proven provider			0.001			0.081	0.000
P -value: Equals other charters						0.058	
N (Applicants)	1172	1245	1769	2694	2281	2387	2375
N (Total scores)			7087				
English	0.221	0.095	-0.045	-0.051	0.231**	0.284**	0.034
		(0.078)	(0.061)		(0.112)	(0.129)	(0.089)
P -value: Equals proven provider			0.112			0.698	0.121
P -value: Equals other charters						0.045	
N (Applicants)	1138	1159	1762	2697	2286	2395	2382
N (Total scores)			7006				

Notes: Panel A reports the first stage effects of charter lottery offers on enrollment in a charter school in the first year after the lottery. Panel B displays the 2SLS estimates of the effects of charter school enrollment in the first year after the lottery on test scores. The endogenous variables are indicators of charter enrollment for the different charter types (pre-expansion proven providers, pre-expansion other charters, post-expansion proven providers, expansion schools, and post-expansion other charters). The instruments are immediate and waitlist lottery offer dummies for each school type. Immediate offer equals one for applicants offered seats on the day of the lottery. Waitlist offer equals one for applicants offered seats from the waitlist. Controls include lottery risk sets, as well as gender, race, ethnicity, a female-minority interaction, special education, English language learner, subsidized lunch status, and grade and year indicators.

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

Table A11: Charter Effects on Test Scores with Immediate Offer Instruments Only

	Before Charter Expansion			After Charter Expansion			
		Estimates			Estimates		
	Non-Charter Mean	Proven Providers	Other Charters	Non-Charter Mean	Proven Providers	Expansion Charters	Other Charters
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
		Panel A: First Stage Estimates					
Immediate Offer		0.855***	1.185***		0.710***	0.484***	0.854***
		(0.070)	(0.048)		(0.054)	(0.045)	(0.054)
		Panel B: 2SLS Estimates					
Math	0.117	0.294***	0.201***	-0.074	0.412***	0.408***	0.218***
		(0.061)	(0.031)		(0.082)	(0.085)	(0.068)
P -value: Equals proven provider			0.108			0.960	0.041
P -value: Equals other charters						0.036	
N (Applicants)	1093	1279	1909	2443	2303	2416	2405
N (Total scores)			17395				
English	0.201	0.165***	0.083***	-0.032	0.222**	0.267***	0.151**
		(0.061)	(0.030)		(0.090)	(0.086)	(0.067)
P -value: Equals proven provider			0.153			0.638	0.479
P -value: Equals other charters						0.197	
N (Applicants)	1087	1277	1911	2441	2307	2420	2412
N (Total scores)			17316				

Notes: Panel A reports the first stage effects of charter lottery offers on years of enrollment in charter schools. Panel B displays the 2SLS estimates of the effects of charter school attendance on test scores. The sample stacks post-lottery test scores in grades five through eight. The endogenous variables are counts of years spent in the different charter types (pre-expansion proven providers, pre-expansion other charters, post-expansion proven providers, expansion schools, and post-expansion other charters). The instruments are immediate offer dummies for each school type. Immediate offer equals one for applicants offered seats on the day of the lottery. Controls include lottery risk sets, as well as gender, race, ethnicity, a female-minority interaction, special education, English language learner, subsidized lunch status, and grade and year indicators. Standard errors are clustered by student.

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

A12: Lottery Estimates of Effects on Peer Quality and School Switching

	Before Charter Expansion			After Charter Expansion			
	Non-Charter Mean	2SLS		Non-Charter Mean	2SLS		
		Proven Providers	Other Charters		Proven Providers	Expansion Charters	Other Charters
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Peer Quality: Peer Baseline Sum of Math and English							
First Post-lotto Year	-0.979	0.234***	0.405***	-1.016	0.212***	0.174*	0.334***
		(0.059)	(0.042)		(0.070)	(0.090)	(0.059)
N (applicants)	1163	1102	1728	2803	2360	2474	2456
N (total)				7089			
Second Post-lotto Year	-0.824	0.168***	0.148***	-0.989	0.221***	0.102	0.197***
		(0.059)	(0.039)		(0.064)	(0.084)	(0.057)
N (applicants)	960	1050	1648	2295	2223	2327	2311
N (total)				6753			
Third Post-lotto Year	-0.699	0.078*	0.054*	-0.944	0.031	0.023	0.060
		(0.044)	(0.031)		(0.059)	(0.079)	(0.052)
N (applicants)	926	1043	1644	1486	1375	1560	1355
N (total)				5123			
Panel B: School Switching After One Year							
Any Switch	0.499	-0.257***	-0.521***	0.500	-0.337***	-0.263***	-0.489***
		(0.047)	(0.043)		(0.077)	(0.096)	(0.072)
Switch Excluding Transition Grades	0.176	-0.064*	-0.140***	0.178	-0.057	-0.072	-0.088*
		(0.035)	(0.031)		(0.056)	(0.070)	(0.053)
Ever Attend Exam School	0.082	-0.053**	-0.033*	0.053	-0.021	-0.014	0.013
		(0.021)	(0.019)		(0.034)	(0.043)	(0.032)

Notes: This table displays 2SLS effects of charter enrollment in different types of charter schools on peer quality and switching schools on year after the lottery. Peer quality is defined as the average baseline test score math and English total for the other students in the student's school and grade. Students who do not appear in Massachusetts public schools in October following the charter application are not counted as school switchers. The switch excluding transitional grades equals one for students who switch schools in grades other than the exit grade of their first school. It does not equal one if the school closed the year the student switched. See Table 5 for detailed regression specification notes.

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

Table A13: Charter School Effects for Subgroups

	Math scores					English scores				
	Before expansion		After expansion			Before expansion		After expansion		
	Proven Providers	Other Charters	Proven Providers	Expansion Charters	Other Charters	Proven Providers	Other Charters	Proven Providers	Expansion Charters	Other Charters
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
English Language Learner	0.288***	-0.194	0.505***	0.284*	0.329***	0.165	-0.254*	0.334***	0.220	0.233*
	(0.088)	(0.157)	(0.101)	(0.148)	(0.118)	(0.101)	(0.138)	(0.108)	(0.146)	(0.120)
N (scores)	468	455	1729	1804	1275	468	454	1733	1807	1279
Not English Language Learner	0.330***	0.192***	0.252***	0.334***	0.145**	0.127***	0.097***	0.090	0.242***	0.092
	(0.040)	(0.027)	(0.093)	(0.082)	(0.067)	(0.038)	(0.025)	(0.097)	(0.084)	(0.064)
N (scores)	3368	5640	2567	2955	3077	3286	5630	2565	2962	3084
Special Education	0.217**	0.156**	0.242	0.628***	0.180	0.039	0.119*	0.130	0.301	0.165
	(0.103)	(0.064)	(0.189)	(0.177)	(0.212)	(0.117)	(0.062)	(0.204)	(0.202)	(0.227)
N (scores)	693	1178	823	930	758	683	1171	818	936	763
Not Special Education	0.346***	0.184***	0.407***	0.270***	0.190***	0.158***	0.091***	0.232***	0.221***	0.109*
	(0.039)	(0.029)	(0.073)	(0.082)	(0.060)	(0.036)	(0.027)	(0.076)	(0.080)	(0.058)
	3143	4917	3473	3829	3594	3071	4913	3480	3833	3600
Below-mean baseline score	0.356***	0.239***	0.484***	0.481***	0.181**	0.148**	0.112**	0.324***	0.321***	0.165*
	(0.057)	(0.043)	(0.094)	(0.104)	(0.073)	(0.068)	(0.050)	(0.108)	(0.098)	(0.087)
N (scores)	1488	2072	2150	2265	1901	1320	1865	1964	2211	1727
Above-mean baseline score	0.343***	0.161***	0.207***	0.280***	0.240***	0.181***	0.080***	0.017	0.180**	0.132**
	(0.035)	(0.026)	(0.079)	(0.072)	(0.057)	(0.032)	(0.023)	(0.083)	(0.077)	(0.060)
N (scores)	2348	4023	2146	2494	2451	2434	4219	2334	2558	2636

Notes: This table reports 2SLS estimates of the effects of charter school attendance on test scores for subgroups of students. The sample stacks post-lottery test scores in grades five through eight. The endogenous variables are counts of years spent in the different charter types. The instruments are immediate and waitlist lottery offer dummies for each school type. Controls include lottery risk sets, as well as gender, ethnicity, a female-minority interaction, special education, English language learner, subsidized lunch status, and grade and year indicators.

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