# Optional ERIC Coversheet — Only for Use with U.S. Department of Education Grantee Submissions

This coversheet should be completed by grantees and added to the PDF of your submission if the information required in this form **is not included on the PDF to be submitted**.

**INSTRUCTIONS**

- Before beginning submission process, download this PDF coversheet if you will need to provide information not on the PDF.
- Fill in all fields—information in this form **must match** the information on the submitted PDF and add missing information.
- Attach completed coversheet to the PDF you will upload to ERIC [use Adobe Acrobat or other program to combine PDF files]—do not upload the coversheet as a separate document.
- Begin completing submission form at https://eric.ed.gov/submit/ and upload the full-text PDF with attached coversheet when indicated. Your full-text PDF will display in ERIC after the 12-month embargo period.

**GRANTEE SUBMISSION REQUIRED FIELDS**

**Title of article, paper, or other content**

**All author name(s) and affiliations on PDF. If more than 6 names, ERIC will complete the list from the submitted PDF.**

| Last Name, First Name | Academic/Organizational Affiliation | ORCID ID |
|---|---|---|
| | | |
| | | |
| | | |
| | | |
| | | |
| | | |

**Publication/Completion Date—**(if *In Press,* enter year accepted or completed)

**Check type of content being submitted and complete one of the following in the box below:**

- ○ If article: Name of journal, volume, and issue number if available
- ○ If paper: Name of conference, date of conference, and place of conference
- ○ If book chapter: Title of book, page range, publisher name and location
- ○ If book: Publisher name and location
- ○ If dissertation: Name of institution, type of degree, and department granting degree

**DOI or URL to published work** (if available)

**Acknowledgement of Funding**— Grantees should check with their grant officer for the preferred wording to acknowledge funding. If the grant officer does not have a preference, grantees can use this suggested wording (adjust wording if multiple grants are to be acknowledged). Fill in Department of Education funding office, grant number, and name of grant recipient institution or organization.

"This work was supported by U.S. Department of Education **[Office name]** through **[Grant number]** to **Institution]** .The opinions expressed are those of the authors and do not represent views of the **[Office name]** or the U.S. Department of Education.

# The Effect of Charter School Openings on Traditional Public Schools in Massachusetts and North Carolina

*By* Kirsten Slungaard Mumma*

*The rapid expansion of charter schools has fueled concerns about their impact on traditional public schools. I estimate the effect of charter openings on traditional public schools in Massachusetts and North Carolina by comparing schools near actual charter sites to those near proposed sites that were never occupied. I find charter openings reduced public school enrollment by around 5 percent and reduced white enrollment in North Carolina. I find no impact on student achievement and my 95 % confidence intervals rule out effects larger than +/- 0.05 standard deviations. I find no effects on attendance or suspensions.*

In 2016, over three million children attended one of approximately 7,000 charter schools in the United States (National Center for Education Statistics (NCES), 2018). The share of all public school students enrolled in charters has grown from 1% in school year (SY) 2000-01 to 7% in SY 2016-17, with much larger shares in many urban areas (Ibid). Charter schools directly impact the students they serve, and may also indirectly affect students who remain in the traditional public schools through the effect of charter expansion on the resources, efficiency, and demographics of the traditional public sector. As charter enrollments have grown, concerns about these indirect effects have taken center stage in debates about charter policy.

Theory offers competing predictions as to the effects of charters on traditional public schools. On the one hand, school choice initiatives such as charters have been described as a "tide that lifts all boats," introducing competitive pressure that pushes all schools to improve (Hoxby, 2003; Friedman, 1962). On the other hand, charters may harm traditional public schools by draining financial resources, "cream-skimming" the best and brightest students, and contributing to segregation (Fiske & Ladd, 2001; Lacireno-Paquet, Holyoke, Moser, & Henig, 2002; Bi-

* Slungaard Mumma: Harvard University, 6 Appian Way, Cambridge, MA 02138, kslung@g.harvard.edu. Many thanks to the Massachusetts Department of Elementary and Secondary Education (MA DESE), especially Carrie Conaway, Matt Deninger, and Kendra Winner, the North Carolina Education Research Data Center (NCERDC) at Duke University, especially Kara Bonneau and Clara Muschkin, and the North Carolina Department of Public Instruction (NCDPI). I am grateful to David Deming, Martin West, Thomas Kane, Joshua Goodman, Eric Taylor and Jonah Rockoff for their helpful comments. Thanks to Jonathan Kolstad and the four anonymous reviewers for their insightful suggestions. The research reported here was supported in part by a training grant from the Institute of Education Sciences (R305B150010). Opinions expressed do not represent the views of the U.S. Department of Education. I gratefully acknowledge data and administrative support from the Center for Education Policy Research at Harvard. IRB approval granted under registration number IRB17-1292. All errors are my own.

fulco & Ladd, 2006; Ladd, Clotfelter, & Holbein, 2017). The empirical literature on the effects of charter competition is expansive, but has not yielded a consensus on the magnitude or direction of the effect. While a number of studies have found positive effects of charter competition on student test scores (e.g. Ridley & Terrier, 2018; Jinnai, 2014; Booker et al., 2008; Sass, 2006), several others have found null/negligible impacts (e.g. Winters, 2012; Bettinger 2005; Bifulco & Ladd, 2006), and a few have found negative effects (e.g. Ni, 2009; Imberman, 2011). Differences in findings likely reflect both differences in the charter sectors under consideration and differences in identification strategies and their underlying assumptions.

A key challenge to identifying the effects of charters on traditional public schools is the endogeneity of charter school locations (Betts, 2009). Charter schools do not locate at random, but instead target specific kinds of communities amenable to new school options (Glomm, Harris, & Lo, 2004; Henig & MacDonald, 2002; Singleton, 2019; Bifulco & Buerger, 2015). Most of the prior literature identifies charter effects by using OLS regressions that include school and student fixed-effects (e.g. Betts, 2009; Sass, 2006; Arsen & Ni, 2012; Booker et al., 2008; Bifulco & Ladd, 2006; Jinnai, 2014; Gilraine, Petronijevic, & Singleton, 2019).[1] Including school fixed-effects in regression models addresses bias due to stable differences between the traditional public schools that charters locate near and other traditional public schools, as would be the case if charters sought to open near persistently low-performing schools. School fixed-effects do not, however, address time-varying differences across schools exposed to higher or lower levels of charter competition, as would be the case if charters opened near schools on a downward performance trajectory or in areas experiencing population growth.

This paper uses a novel identification strategy adapted from Greenstone and Moretti's (2003) "runner-up" methodology to identify the effect of charter schools on traditional public schools in Massachusetts and North Carolina. Greenstone & Moretti compare "winner" and "runner-up" counties in a competitive bidding process for a large industrial plant to identify the local economic effects of plant openings. My difference-in-differences approach compares traditional public schools near charters to those near sites that were proposed by charter operators in their applications to the state but never ultimately occupied. The key innovation of this identification strategy is the use of a theoretically-motivated counterfactual to address bias due to time-varying differences between traditional public schools near charters and those farther away. Using this strategy, I estimate the grade-level effects of charter school openings on the demographics of traditional public schools and on student test scores, attendance, and suspensions.

I find that treated grades in traditional public schools experience meaningful decreases in enrollment when a charter operates nearby, losing about 4.7% of

[1] Exceptions to this are Bettinger (2005) and Imberman (2011), who use instrumental variable approaches to overcome the endogeneity of charter school locations. The IV strategies these authors employ rely on political/geographic features that are specific to the charter sectors they study and cannot be applied here.

grade-level enrollment on average. I find that charters decrease the proportion of white students and increase the proportion of Hispanic students in traditional public schools in North Carolina, but find no effect of charters on the proportion of Black, white, or Hispanic students in traditional public schools in Massachusetts in my preferred specification. I find no effect of charters on the achievement of students in the traditional public schools in either state, with a 95% confidence interval covering approximately -0.014 to 0.036 $\sigma$ in ELA and -0.014 to 0.052 $\sigma$ in math, on average. I find no effect on attendance or suspensions. My main results are robust to the use of alternative radii and treatment specifications. I find some evidence of positive effects on test scores for academically-focused charters.

My results augment and sometimes contradict the growing literature on charter schools in these states. Research on the direct effects of charters in Massachusetts, including lottery-based studies, find positive effects for students attending urban charters and null or negative effects for students in non-urban charters (Abdulkadiroglu et al., 2011; Angrist et al, 2016; Angrist, Pathak, & Walters, 2013). Estimates of the effect of attending a charter in North Carolina are negative or null (Carruthers, 2012; Bifulco & Ladd, 2006). Several studies have considered the indirect effects of charter schools in North Carolina.[2] Using a fixed-effects model, Jinnai (2014) finds positive effects of charter competition on test scores for traditional public school students in overlapping grades but no effect on students in grades not also served by the charter. Although I also define treatment at the grade-level, I fail to find a significant effect. In Table 5, I show that the schools that charters locate near differ significantly from those slightly farther away in terms of levels and trends in key characteristics. Taken together, these results suggest that estimates from fixed-effects models may be biased by time-varying differences between schools that face higher/lower levels of charter exposure.

Using a sample of North Carolina charters that opened in 2012 or 2013, Gilraine, Petronijevic, and Singleton (2019) (hereafter, GPS) compare students who live near new charter schools to those who live farther away to identify the combined direct and indirect effects of charter expansion.[3] The authors find positive effects for students near charters that focus on academic achievement but no effect for students near specialized ("horizontally-differentiated") charters. In contrast, the parameter I estimate reflects only the indirect effects of charter expansion on the traditional public schools, including any effects on resources, school/teacher productivity, and peer group composition. My aggregate measure of indirect effects could reflect both positive and negative effects operating through these channels if, for instance, negative effects on peer groups were paired with positive effects

[2]In addition to the papers discussed here, Bifulco & Ladd (2006) find no effect of charter competition on student achievement using a fixed-effects model for an earlier sample of North Carolina charter schools. Also in North Carolina, Jackson (2012) finds small decreases in teacher quality at low-income, high-minority schools after charter openings.

[3]Although the authors base their identification strategy on student home addresses instead of school addresses, given that school assignment is based on student addresses, estimates from this approach may also be subject to the same bias as those from a conventional fixed-effects models.

on productivity. Nevertheless, my point estimates for academically-focused and horizontally-differentiated charters are consistent with GPS's theory of heterogeneous effects, though somewhat imprecise.

Studying Massachusetts charter schools, Ridley & Terrier (2018) use an instrumental variable strategy based on a change in charter law combined with synthetic controls to identify the effect of district-level charter expansion on spending and student achievement. The authors find that charter competition shifted expenditure from support services to salary/instruction and had small positive effects on student test scores. Unlike my paper and the others highlighted here, Ridley and Terrier model charter competition at the district (as opposed to school) level. Our different conclusions raise questions about which entities within the public school system are most likely to respond to growth.

A growing body of research considers the effects of charter schools on segregation, including in North Carolina. Examining the movement of students who transfer into charters, Bifulco and Ladd (2007) find that students gravitate to charters with higher own-group representation than the schools they left. Ladd, Clotfelter, and Holbein (2017) find that North Carolina charters have shifted from serving a disproportionately minority population to serving a disproportionately white population and that charters are increasingly likely to be racially imbalanced, serving predominantly white or predominantly non-white students. While these papers focus on the demographics of charter schools, my study addresses the effect of movement into charters on the representation of students remaining in the traditional public schools, and is the first I am aware of to tackle this question in a causal framework. I find evidence that seats vacated by students entering the charter sector are filled by Hispanic students in North Carolina, increasing the concentration of Hispanic students and reducing white enrollment in treated schools.

The remainder of this paper is organized as follows: in section II, I describe the charter school sectors in both states. In section III, I describe my data sources, discuss my original dataset of proposed school sites, and compare the baseline characteristics of actual and proposed charter locations. In section IV, I discuss my econometric models. In section V, I present my results. I conclude in section VI.

## I. Charter Schools in Massachusetts and North Carolina

Charter schools are public schools that are managed by private organizations. Massachusetts passed its charter law in 1993 and North Carolina followed suit in 1996. In both states, a single statewide charter school authorizer is responsible for approving new charters and overseeing existing schools. Charter schools are open to any student in the state and seats in oversubscribed charter schools must be allocated by random lottery.[4] Figure 1 shows the growth of the charter sectors in

[4]In Massachusetts, priority is given to students who live in the district in which the charter is located.

these states over time. The number of charter schools grew rapidly after legislation that revised or eliminated caps on charter growth in 2010 (Massachusetts) and 2011 (North Carolina). In 2016, 4.5% of public school students in Massachusetts and 6.0% in North Carolina attended charter schools (NCES, 2018).

Charter schools compete with traditional public schools for students and resources. Traditional public school districts pay charters a sum roughly equal to their own per-pupil expenditure for every child who lives in the district's catchment area but attends a charter school. In Massachusetts, the costs of charter expansion are partially offset by a state tuition reimbursement policy that ostensibly covers 100% of annual increases in charter tuition for year one and 25% of increased payments for six years thereafter.[5] Because of this policy, Terrier and Ridley (2018) find that charter expansion in Massachusetts after 2011 increased per-pupil expenditure in traditional public school districts in the short term. North Carolina does not have a tuition reimbursement scheme, meaning the financial impact is more immediate. Ladd and Singleton (2020) find that districts in North Carolina lose an average of $3,600 per charter student after accounting for the district's ability to reduce expenses given reduced enrollment.

Table 1 compares the student and school-level characteristics of charter schools and traditional public schools in each state. While charter schools in Massachusetts serve higher proportions of Black, Hispanic, and economically disadvantaged students than traditional public schools, charter students in North Carolina are more likely to be white and less likely to be economically disadvantaged than traditional public school students overall.

## II. Data and Descriptive Statistics

### *A. School- and student-level data*

I obtained student-level education records for public school students in the state of Massachusetts from the Massachusetts Department of Elementary and Secondary Education for SY 2001-02 to SY 2014-15. I obtained similar records for students in North Carolina for SY 1997-98 to SY 2014-15 from the North Carolina Education Research Data Center (NCERDC) at Duke University. Student records include enrollment and attendance data, demographic information, and standardized test scores in math and reading/English language arts (hereafter, ELA).[6] I standardize test scores by year, grade, and subject within state. Data on attendance rates and out-of-school suspensions are available for a subset

[5]In practice, districts received less than this. In FY 2019, state funding covered only 66% of total costs of this initiative. In addition, funding is provided directly to the municipality (instead of the school district) and may or may not reach district budgets (Massachusetts Department of Elementary and Secondary Education, 2018).

[6]Test scores for Massachusetts are from the Massachusetts Comprehensive Assessment System (MCAS) and generally cover grades 3-8 and 10. Test scores in North Carolina come from end-of-grade exams and cover grades 3-8. The annual assessment used in Massachusetts changed substantially in spring 2014. See Data Appendix B in the Online Appendices for detail.

of years.[7] School location data and school- and grade-level demographics come from the National Center for Education Statistics (NCES) Common Core of Data (CCD) for SY 1995-96 to SY 2015-16. I cleaned addresses and coordinates for consistency and to fix obvious errors. (See Appendix A in the Online Appendices for detail).

### B. *Applications to open new charter schools*

My difference-in-differences identification strategy compares traditional public schools near charter schools to those near sites that charter schools considered but never occupied. I identify proposed sites from applications submitted by the charter school operators to the state charter authorizer before being granted permission to open a new school, typically 9-18 months before the school opens.[8]

Securing a facility is one of the biggest hurdles to charter expansion nationwide (Gill & Maas, 2013). Unlike traditional public schools, charters are typically responsible for finding and paying for their own facilities.[9] The search for a facility is complicated by the fact that charters need to begin looking for a site before they have approval to open. In addition, since charters tend to grow by adding grades over time, some schools may need to locate a temporary (smaller) space while simultaneously planning for a longer-term location in the area. Prospective charter operators are not required to sign a lease or purchase land before applying, but they are asked to describe their intended location or plans to secure one in their application. A sample response for a charter school in Springfield, MA is as follows:

> "We have identified 3 top choices... The three buildings are (1) the former Wesson Hospital on High Street, (2) the former Springfield Cinemas and attached stores at Wilbraham Road and Breckwood Boulevard, and (3) the former Ring Nursing home and a neighboring building at Ridgewood Terrace and Mulberry Street." *Martin Luther King, Jr. School of Excellence Charter Application, 2005*

This school opened in rented space at a local church near the hospital mentioned here, where it operated for four years before moving to a new facility. I compiled final applications for independent[10] charter schools that opened in North Carolina

[7]In Massachusetts, attendance data cover SY 2003-04 to SY 2014-15 and suspension data cover SY 2002-03 to SY 2014-15. In North Carolina, attendance data cover SY 2005-06 to SY 2013-14 and suspension data cover SY 2009-10 to SY 2014-15.

[8]During this time, applications for schools in North Carolina were generally due in April, about 16 months before the school was set to open in August of the following year. Charter schools in Massachusetts must open within 18 months of their application being approved. See Appendix Tables A.1 and A.2 for additional detail on the current charter application processes in these states.

[9]In Massachusetts, charters receive a relatively generous $893 per student in facilities funding, though many report paying more (Charter School Facilities Initiatives, 2013).

[10]Massachusetts permits two kinds of charters schools: Commonwealth charter schools, which operate

and Massachusetts from fall 2000 to fall 2013 and created an original dataset of proposed charter locations based on details in these applications. I checked the addresses of proposed sites against the actual locations of charters reported in the CCD to determine whether proposed sites were ever occupied.

### *C. Charter school sample*

Table 2 summarizes the breakdown of charter openings included/excluded from my initial sample. To be included, the following conditions applied: (1) the charter's application needed to provide sufficient detail for me to identify at least one proposed site (some applications did not specify a location at all or were too vague in their description to identify a specific location), and (2) at least one proposed site needed to differ from the actual site(s) the charter school occupied after opening. Of the 45 independent charter schools that opened in Massachusetts from 2000 to 2013, I was able to identify a proposed site for 31, and was able to include 26 in my initial sample.[11] Of the 67 charters that opened in North Carolina, I was able to identify a proposed site for 30, and was able to include 19. In Appendix C, I present a list of each individual opening included in my initial sample.

Charter schools are motivated to identify a location as soon as possible in order to secure commitments from local families and community members to show support for the application and help recruit students once the application is approved. While charters identify particular neighborhoods to serve when planning a school, idiosyncracies of the local real estate market largely determine where within an intended community they are able to operate. The intuition behind my identification strategy is that the sites proposed by charters in these applications are honestly reported and reflect their true locational priorities, regardless of whether or not a specific building is ultimately selected. Consistent with this, I find that charters tend to target a relatively narrow geographic area, with the average distance between proposed (but never occupied) and actual sites being 2.3 miles (SD=2.9 miles). (See Figure 2 for a map or proposed and actual sites.) About 38% (23/60) of charters that included sufficient information on their application to identify a proposed site ultimately occupied one of these sites.[12] Anecdotal evidence provides a variety of explanations as to why a proposed building in an otherwise desirable location was not ultimately occupied, from a building becoming unavailable by the time a charter is prepared to execute a lease, to the discovery of costly environmental or accessibility retrofitting requirements, to

independently of local school districts, and Horace Mann charter schools, which are approved by the local school board and teachers' union. I include only Commonwealth charters in my sample. Similarly, I exclude conversion charter schools in North Carolina that previously operated as private schools.

[11]The number of charter openings in each estimation sample varies, as noted below, because I limit my sample school-by-grade cases with both treatment and control observations at a given radius and also because student-level outcome data is available for only a subset of years and grades, as discussed. At the 2-mile radius, this first condition limits my sample to 23 schools in Massachusetts and 13 in North Carolina.

[12]One charter school occupied two proposed sites as a first and secondary location.

unforeseeable parking or zoning conflicts.

My sample represents a subset of all charters that opened during this period. In Table 3, I conduct a cross-sectional comparison of the characteristics of charters included in my initial sample and other charters that opened over this time. Charters included in my sample serve a higher proportion of Hispanic students than excluded charters but are otherwise comparable, including in terms of average test scores and a school value-added measure.[13]

In Appendix Table A3, I compare charters that listed multiple sites on their applications to those that listed just one. Charters that listed multiple sites serve a higher proportion of Hispanic students and operate in neighborhoods with a larger Black population than charters that listed one site, though these differences are not significant. In Appendix Table A4, I compare charters that operated in a proposed site to those that did not, finding no notable differences.

### *D. Balance Tests*

Table 4 compares the baseline characteristics of traditional public schools located within two miles of an actual (treatment) or proposed-only (control) site of a charter school in my sample. The school-level characteristics in panels A and B are defined the year before the charter school opened. The neighborhood-level characteristics in panel D come from the 2000 Census. Columns (1) and (2) present the mean of each characteristic for the treatment and control schools, respectively, weighted to give each charter opening equal weight.[14] Column (3) presents results from a t-test of equivalence for the (weighted) means, with standard errors clustered at the school level. There are no statistically significant differences between treatment and control schools for any individual characteristic, including average test scores and changes in test scores over three years. At the bottom of Table 4, I report results from an F-test of the joint significance for all variables in Panels A-C, plus indicators for missing test scores. I fail to reject the null hypothesis.

I present results for balance tests separately for each state in appendix Tables A5 and A6. I find no significant differences for any individual characteristic. I fail to reject the null for the F-test in Massachusetts, but reject the null in the North Carolina sample. This seems to be due to the fact that tracts near actual sites of charters in North Carolina report higher median incomes than those near proposed sites. While the comparability of treatment/control sites is reassur-

[13]Following GPS (2019), I define value-added as the school-year fixed effect in a regression of math scores on cubic controls for prior scores in both subjects, demographic controls, grade-by-year fixed effects, and indicators for missing scores or demographic variables. Value-added estimates are calculated separately for each state using a sample of all students in grades 4-8 and 10 (Massachusetts only) with a prior year's test score in the same subject, or a prior same subject score from two years prior in the case of 10th graders. Value-added measures are normalized to sum to zero across all schools in a state in a given year.

[14]Specifically, I weight each observation by the inverse of the number of schools near actual or proposed-only sites for each charter opening.

ing, my identification strategy does not require equivalence of treatment/control schools at baseline and is instead based on an assumption of parallel trends that I investigate graphically later.

## III. Empirical Strategy

### A. *Endogeneity of charter school locations*

If charters located at random, we could identify the effect of charters on traditional public schools by comparing schools near charters to those farther away. In this section, I consider what kinds of schools/communities charters tend to target in Massachusetts and North Carolina and whether the endogeneity of charter locations is likely to bias estimates from conventional fixed-effects models. Using a longitudinal dataset of school-level observations for all traditional public schools in Massachusetts and North Carolina, I regress the number of charters serving an overlapping grade within two miles of the school on the characteristics listed in the first column and dummies for each year. I report the coefficient on the number of nearby charters in columns (1) and (2) from separate regressions estimated in each state subsample. I limit my sample to traditional public schools with at least one charter within five miles to approximate the comparison groups used in other studies.

I find strong associations between the number of nearby charters and school- and neighborhood-level characteristics. There is a positive association between the number of nearby charters and the proportion of economically disadvantaged and minority students, and a strong negative association with average test scores, suggesting that charters tend to open in disadvantaged communities with relatively low-performing schools. I also find significant associations between the number of nearby charters and trends in population growth (negative in Massachusetts, positive in North Carolina) and changes in test scores (positive in Massachusetts). These differences in time-varying characteristics imply that estimates from conventional fixed-effects models may be biased by time-varying differences in school effectiveness and/or peer group composition.

### B. *Econometric Models*

The models I use are adapted from Greenstone and Moretti (2003).[15] I begin by estimating the effect of charter openings on grade-level enrollment and demographics using a longitudinal dataset of school-by-grade observations. I restrict my sample to schools ($j$) that are located within two miles of an actual or a proposed-only site of a charter ($k$) included in my sample, and to grades ($g$) that are also served by the charter at some point. I require that there be both a "treatment" and "control" observation for each charter-by-grade combination

[15] See also Greenstone, Hornbeck, and Moretti (2010).

within the designated radius. In addition, I require that the traditional public school exist for at least three years before the charter opens. I drop observations for Kindergarten and Pre-K because of inconsistencies in reporting over time. I use a 2-mile radius in my preferred specification because it is within the range of radii used in the prior literature and maximizes my sample of charter openings.[16] I present results using a 1.5-mile and 2.5-mile radius as robustness checks.

I estimate the effect of charter openings on grade-level demographics as follows:

(1)

$$DEMO_{gjkt} = \pi SERVES_{gkt} + \lambda ACTUAL_{gjk}xSERVES_{gkt} + \theta_{gt} + \omega_{gjk} + \epsilon_{gjkt},$$

where $DEMO$ is a grade-level demographic characteristic, such as total enrollment or proportion of students who are Black, for grade $g$ in year $t$ at traditional public school $j$ paired to – i.e, within two miles of a proposed-only or actual site of – charter school $k$. $SERVES$ is an indicator variable that is equal to 1 when charter school $k$ has started serving grade $g$. $ACTUALxSERVES$ is an interaction between $SERVES$ and $ACTUAL$, an indicator variable that is equal to 1 if school $j$ is located within two miles of any actual site of charter $k$ and grade $g$ is an ever-treated grade.[17] The model includes school-by-grade-by-charter fixed effects ($\omega_{gjk}$) to control for time-invariant differences across treated/untreated grades within a charter "case" and state-by-grade-by-year fixed effects ($\theta_{gt}$) to account for differences across cohorts. $\lambda$ is the parameter of interest, representing the incremental difference in outcomes for grades at treated schools relative to those at control schools. Standard errors are clustered at the school-by-grade level, the level at which treatment is defined.[18]

A few points bear clarification. First, including the school-by-grade-by-charter "case" effect means that $\lambda$ is calculated from "within-case" variation, comparing changes in outcomes at schools near actual and proposed-only sites of the same charter school. Second, I define treatment at the grade-level. Since charters tend to open a few grades at a time, the first year of treatment does not necessarily correspond to the year the charter opens. Third, while traditional public schools rarely move, I observe some secondary mobility of charter schools.[19] Since I define $ACTUAL$ so that it is equal to 1 if traditional public school $j$ is located within two miles of *any* actual site of charter $k$, it is sometimes the case that an observation is identified as treated when charter $k$ is not currently located within a 2-mile radius.[20]

[16]At smaller radii, there are fewer nearby schools. At larger radii, the "circles" around actual and proposed-only sites tend overlap more substantially, meaning there are less likely to be "control" sites.

[17]An "ever-treated" grade is a grade that is served by both charter k and traditional public school $j$ at some time when charter $k$ is located within two miles of school $j$.

[18]Some demographic effects become imprecise when clustering at the school-level. Clustering at the school-level does not affect inference for my main effects on student outcome; standard errors are practically indistinguishable clustering at either level.

[19]Of the 45 schools in my initial sample, I observe 24 in a single site, 18 in two sites, 2 in three sites, and 1 in four sites.

[20]This is true for 5% of observations in the estimation sample at the 2-mile radius. Half of these (2.5%) represent observations that are identified as treated after charter $k$ has moved out of the 2-mile

Fourth, since charters target relatively small geographic areas, proposed-only sites are often also in the general vicinity of an actual charter location. As such, although control schools may experience more pressure from charter openings – consistent with the marginal enrollment losses I show in the following section – control schools may also be "treated" to a lesser extent when a charter opens, contributing to downward bias.[21] Finally, following Greenstone and Moretti, I re-use observations if the same school/grade is treated by multiple charters, modelling shocks within each charter-by-grade case.[22]

To evaluate the assumption of parallel trends, I adapt equation (1) by regressing the outcome on charter-by-grade-by-school fixed-effects, state-by-grade-by-year fixed effects, and event-time indicators for each year relative to the year the charter starts serving that grade interacted with indicators for being a treatment/control observation, as follows:

(2)
$$DEMO_{gjkt} = \alpha + \sum_{p=-10}^{10} \delta_{Ap} ACTUAL_{gjk} + \sum_{p=-10}^{10} \delta_{Pp} PROPOSED_{gjk} + \theta_{gt} + \omega_{gjk} + \epsilon_{igjkt},$$

where $p$ indexes each event period from 10 years before to 10 years after charter school $k$ starts serving grade $g$ (at $p = 0$), $ACTUAL$ is defined as before, and $PROPOSED$ is an indicator that is equal to 1 if school $j$ is located within two miles of a proposed but never-occupied site.[23] The double subscripts $Ap$ and $Pp$ are used to distinguish between the period ($p$) specific coefficients that pertain to actual ($A$) and proposed-only ($P$) sites. I focus on event periods $p$ that fall between $p = -10$ and $p = 10$ because values outside this range are particularly noisy given my unbalanced panel.[24] The vectors $\delta_{Ap}$ and $\delta_{Pp}$ are the parameters of interest, representing the (controlled) mean values of each outcome for treatment and control observations at each period. Using the *nlcom* command in Stata, I estimate the differences between the $\delta_{Ap}$ and $\delta_{Pp}$ at each period $p$ and the corresponding $\delta_{Ap}/\delta_{Pp}$ defined at period $p = -1$. These transformed coefficients can be interpreted as the difference in the outcome relative to levels in the last

radius of school $j$, and the other half represent observations at schools within the 2-mile radius of a future site of charter $k$. I find similar results if I set observations as missing if a school moves from treated to untreated status, or if I identify the 2.5% of charters near future sites as untreated until charter $k$ starts operating nearby.

[21] It is also sometimes the case that schools treated by charter openings in my sample are near other charters that open during this time. Thus, my measure of charter competition should be understood as a measure of incremental charter competition.

[22] In my preferred specification, 67% of unique school-by-grade observations are used in one charter case, 22.5% are used in two charter cases, 8.22% are part of three charter cases, and the remaining $< 2.5\%$ are part of more than three cases.

[23] Since the sample is limited to schools within two miles of an actual or proposed-only site of charter $k$, $ACTUAL$ and $PROPOSED$ are mutually exclusive indicators.

[24] Observations for $p > 10$ and $p < -10$ contribute to estimating the intercept ($\alpha$) in equation (2).

pre-period. I plot these transformed coefficients to examine the assumption of parallel trends.

To estimate the effect of charter openings on student outcomes, I match student-level observations to the school-by-grade-by-year dataset to estimate equations (1) and (2). I estimate the effect of charter openings on student outcomes as follows:

(3)
$$Y_{igjtk} = \delta SERVES_{gkt} + \lambda ACTUAL_{gjk}xSERVES_{gjkt} + \beta X_{igt} + \theta_{gt} + \omega_{gjk} + \epsilon_{igjkt},$$

where $Y_{igjtk}$ is the outcome (e.g. test score) of student $i$ in year $t$ in grade $g$ at school $j$ paired to charter $k$. $SERVES$ and $ACTUALxSERVES$ are defined as before. As in equation (1), grade-by-year-by-state and school-by-grade-by-charter fixed-effects are included. $X_{igt}$ is a vector of student-level characteristics including race/ethnicity gender, economically disadvantaged status, limited English proficient status, and lagged outcomes.[25] The test score sample is limited to students in grade 4-8 and 10 with a non-missing test score in at least one subject in the prior year.[26] The test score models include cubics of once-lagged and twice-lagged test scores in both subjects as well as indicators for missing test scores. The attendance/suspension sample is limited to students in grades 1-12 with a non-missing annual attendance observation from the prior year and indicators for missing suspension data. The attendance/suspension models include prior year's attendance and an indicator for having an out-of-school suspension in the prior year.

There are three additional details about equation (3) to consider. First, I address student selection of traditional public schools after charter openings by controlling for student covariates and lagged outcomes instead of including student fixed-effects, as in some prior studies. Student fixed-effects are extremely data intensive, requiring that a student be observed twice at the same school in a tested grade both before and after a charter opens nearby, or under varying degrees of competition, to contribute to identification. Student fixed-effects are ill-suited to my "paired" difference-in-differences approach with a binary treatment indicator because my estimation sample includes only grades that are served by both the charter and traditional public school and it is relatively uncommon to observe a student in a tested grade before a charter serves that grade and then also observe that student in a tested grade currently served by the charter given the charters tend to add grades sequentially.[27] Second, I define treatment at the grade-level. Under this definition, a student is treated if he/she attends a school located within two miles of an actual site of the charter and the charter currently

[25] In North Carolina, controls also include gifted status, special education status, and parental education levels for some years. In Massachusetts, controls include special education status and an indicator for being an immigrant. All regressions include indicators for missing values of demographic variables.

[26] Observations for students in grade 10 are from Massachusetts only. For 10th graders I define once-lagged scores as 8th grade scores since students are not tested in grade 9.

[27] For example, if a student is observed in grade 3 before a charter school begins serving grade 3, it would be unusual for the charter to begin serving grade 4 in the following year.

serves the student's grade. This approach is consistent with Jinnai (2014), who found competitive effects only in overlapping grades. In addition, defining treatment at the grade-level ensures that $\lambda$ captures any effects of charters that stem from changes in the composition of peer groups (Hoxby, 2000).[28] I consider an alternative specification that defines treatment at the school-level as a robustness check. Third, I re-use student-level observations if they are paired to more than one charter-by-grade case, as previously described.

To evaluate the assumption of parallel trends, I adapt equation (3) by regressing the student outcome on all the fixed effects, student covariates, and lagged outcomes included in equation (3) as well as on the interacted event-time indicators included in equation (2), as follows:

(4)

$$Y_{igjtk} = \alpha + \sum_{p=-10}^{10} \delta_{Ap} ACTUAL_{gjk} + \sum_{p=-10}^{10} \delta_{Pp} PROPOSED_{gjk} + \beta X_{igt} + \theta_{gt} + \omega_{gjk} + \epsilon_{igjkt},$$

where all variables are defined as before. I plot the transformed estimates of $\delta Ap$ and $\delta Pp$ to assess parallel trends.

## IV. Results

### A. Demographics

ENROLLMENT. — The first row of Table 6 presents estimates of the effect of charter openings on grade-level enrollment. Relative to schools near proposed-only sites, grades at schools near actual sites of charters lose 4.7 students after a charter starts serving that grade, or about 4.7% of enrollment in a typical 100-student grade. Enrollment losses of this magnitude translate into significant financial losses; based on the average per-pupil revenue and teacher salaries, each treated grade translates to a loss of about $70,000 in Massachusetts (0.87 FTE) or $80,000 in North Carolina (1.56 FTE).[29] Figure 3 plots the transformed coefficients estimated using equation (2) with grade-level enrollment as the outcome. Trends in grade-level enrollment are similar and fairly flat in the pre-period and decline sharply at treated schools after the charter serves that grade, as expected. (Appendix Figure A.1 plots trends for each state separately.)

[28] Since the effect of charter schools on demographics is mediated by the (grade-dependent) movement into the traditional public schools, and charters tend to grow with a single cohort of students, we do not expect to find changes in the composition of students in untreated grades.

[29] In Massachusetts in SY 2016-17, average per-pupil revenue was $19,235 and average teacher salaries were $80,357. In North Carolina, average per-pupil revenue was $10,570 and average teacher salaries were $51,231. Source: National Education Association (2019).

Racial/Ethnic Composition. — The remaining rows of Table 6 present estimates of the effect on the proportion of white, Black, and Hispanic students in a grade. At the 2-mile radius, I find no significant effects on the racial/ethnic composition of traditional public schools in Massachusetts. In North Carolina, charter openings decrease the proportion of white students in a grade by 5.7 percentage points and increase the proportion of Hispanic students by 5.2 percentage points. During the period of this study, the Hispanic population grew remarkably in North Carolina, increasing from 4.4% of all public school students in 2000 to 17.5% in 2017 and driving overall enrollment gains (NCES, 2020).[30] In Appendix Table A.7, I present results using the number of students in a grade by race/ethnicity as the outcome of interest. The effects on the proportion of white and Hispanic students appear to reflect both lost white enrollment and relative increases in the number of Hispanic students. I interpret this as evidence that seats vacated by students entering the traditional public schools were filled with students from the growing Hispanic population. Figure 4 presents estimates from equation (2) for the proportion white, Hispanic, and Black students in the pooled sample. Pre-trends are similar across outcomes. (See Appendix Figures A.2 and A.3 for figures plotted for each state separately).

Robustness Checks. — In Table 7, I present results using the 1.5-mile and 2.5-mile radii as robustness checks. In North Carolina, estimated effects on the proportion of Hispanic and white students are similar across specifications. Estimates on the proportion of Black students are negative and significant at the 1.5 and 2.5-mile radius. The effect on Black students seems to reflect both the movement of Black students into charters and increased Hispanic enrollment. In Massachusetts, I find small significant estimates at the 2.5-mile radius, indicating a small positive effect on the proportion of white and Hispanic students and a small negative effect on the proportion of Black students. I am hesitant about these results, however, because of issues with balance in Massachusetts at the 2.5-mile radius, discussed below.

### B. *Student Outcomes*

Test Scores. — Table 8 presents the estimated effect of charter openings on student test scores. I find no significant effects of charter schools on student achievement in either state in any subject using the 2-mile radius. For the pooled sample, the 95% confidence interval covers -0.014-0.052$\sigma$ in math and -0.014-0.036$\sigma$ in ELA. I note positive (but imprecise) point estimates in the Massachusetts sample. Point estimates in North Carolina are smaller in magnitude, negative, and

[30]Total enrollment in public schools in North Carolina increased by 18.9% from 2000 to 2015. In Massachusetts, total enrollment fell by 4.4% over that same period (NCES, 2018).

very imprecise.[31] In Figure 5, I plot point estimates generated by equation (4) to consider pre-trends for treatment/control observations. I find no evidence of a violation of parallel trends. (See Appendix Figures A4 and A5 for figures plotted separately for each state).

Attendance and Suspensions. — Effects on student behavior could be a leading indicator for effects on test scores if charters push traditional public schools to improve attendance and minimize behavioral disruptions.[32] Table 9 presents estimates of the effect of charter openings on annual attendance and an indicator variable for having any out-of-school suspensions reported in that year. I find no significant effects on attendance or suspensions. I assess the assumption of parallel trends in Figure 6. I find no evidence of a violation of parallel trends, though I note these are particularly noisy given uneven coverage of outcome data across year/states. (See Appendix Figures A6 and A7 for figures plotted separately for each state).

Robustness Checks. — Table 10 presents results using the 1.5-mile and 2.5-mile radii as robustness checks. All results are insignificant at the 1.5-mile radius. At the 2.5-mile radius, I find significant positive effects on math and significant negative effects on attendance in the Massachusetts and pooled samples. (Estimates on ELA and suspensions are not significant). I interpret these results with caution, however, because the sample of schools at the 2.5-mile radius for Massachusetts is substantially imbalanced.[33]

If untreated grades are affected by charter openings before a charter serves that grade, my approach to assigning treatment when a grade is served by the charter could underestimate charter effects. In Table 11, I present results using an alternative specification that defines treatment at the school-level, coinciding with the year a charter opens.[34] My results are insignificant and statistically

[31] In results not reported here, I consider the effect of charter openings on student achievement in models that do not include student controls. I find negative point estimates that are imprecise, consistent with small negative effects on average performance in North Carolina. Point estimates for Massachusetts are close to zero.

[32] As Imberman (2011) notes, an effect disciplinary responses could indicate positive behavioral changes or changes in school responses to behavior.

[33] At the 2.5-mile radius, schools near actual sites in Massachusetts are significantly lower-performing in math/ELA and serve a more economically disadvantaged population than schools near proposed-only sites. This is due, in part, to the fact that treatment and control sites for charters are closer to each other in Massachusetts than in North Carolina (1.69 miles vs. 4.67 miles, respectively), and Massachusetts is much denser than North Carolina. As such, there is more overlap in the radii around actual and proposed-only sites, leaving a narrower slice for defining control schools at this radius.

[34] This can be expressed as:

$$Y_{igjtk} = \mu OPEN_{kt} + \nu ACTUAL_{gjk} x OPEN_{jkt} + \beta X_{igt} + \theta_{gt} + \omega_{gjk} + \epsilon_{igjkt}, \tag{5}$$

where OPEN is an indicator that is equal to 1 after charter $k$ begins operating, ACTUAL is defined as before, and $\nu$ is the parameter of interest. The sample is limited to grades that are ever-served by both the charter and traditional public school and the data are structured in the same way as previously described for estimating equation (3). Standard errors are clustered a the school-level, the level at which treatment is assigned.

indistinguishable from those in Table 8.

Finally, I consider the possibility that traditional public schools respond to charter pressure even before the charter opens.[35] The charter application process is largely public, with applications posted online in recent years. As such, it is possible that a school near a proposed site would know that a charter intended to locate nearby at the time the application was submitted. To consider this, I plot mean test scores for schools near sites proposed by charters in my sample over the period before and just after the application was submitted. Using a dataset of student-by-year observations for students at traditional public schools within two miles of a proposed site, I regress student test scores on event-period dummies for each period from $p = -5$ to $p = 2$, where $p = 0$ the year the application was submitted, controlling for state-by-grade-by-year fixed effects.[36] I plot these in Figure A8. I look for breaks in school performance between $p = 0$ and $p = 2$, preceding charter-induced changes in student composition, as an indication of anticipatory effects.[37] I find no evidence of anticipatory behavior.

Heterogeneity. — In Table 12, I present results considering heterogeneity of effects for different types of charter schools: urban/non-urban schools and academically-focused/horizontally-differentiated schools. Research on charter schools in Massachusetts and a national study of charter quality finds that urban charters tend to outperform their traditional public school counterparts, though the same may not be true for non-urban charters (Abdulkadiroglu et al. 2011; Angrist, Pathak, & Walters, 2013; Center for Research on Educational Outcomes, 2015). If the effect of charter openings is correlated with relative charter quality, we might expect stronger effects in urban areas. I also consider heterogeneity of effects for academically-focused and specialized (horizontally-differentiated) charter schools, following GPS (2019). GPS propose that traditional public schools will respond to competition from schools that emphasize traditional academics, such as "No Excuses" charter schools, but will not respond to competition from charters that cater to special interests, such as Montessori programs or Chinese immersion. I identify schools as horizontally-differentiated based on the description of their program in their applications. (See Appendix D for detail). I find insignificant effects for urban and non-urban charters. Consistent with GPS (2019), I find in-

[35]See Figlio & Hart (2014) for evidence of anticipatory effects of a school choice program before student transfers take place.

[36]Here, "proposed sites" include sites that were proposed and occupied and sites that were proposed but never occupied. As before, I re-use student observations if a traditional public school is near proposed sites for two different charters in my initial sample. I adjust the point estimates plotted in Figure A8 by subtracting the value at $p = -1$ to center the graph at zero the year before the application was submitted.

[37]Most of the 43 charters in this estimation sample opened at $p = 1$ (23/43) or $p = 2$ (19/43). One school opened at $p = 5$. All of the North Carolina charter schools included in the initial sample submitted their final applications in the spring the year before the charter opened (16 months prior). In the Massachusetts sample, 19 schools submitted their applications in the fall two years before the charter opened (22 prior), one school submitted an application in the fall the year before it opened, and one school submitted its application in the fall 5 years before opening.

significant effects for "horizontally-differentiated" charter openings and evidence of positive effects for academically-focused charters, with positive point estimates in both subjects that are statistically significant in ELA.

Comparison to Alternative Estimation Approach. — My identification strategy uses data on proposed charter locations that may not be available for researchers studying charter effects in other contexts. In this section, I consider whether I would find the same effects of charter openings without information on proposed locations by comparing traditional public schools near charters to those slightly farther away. To do this, I construct a simple differences-in-differences estimator comparing traditional public schools located less than two miles from a charter school to those slightly farther away (between 2-5 miles). While not identical, this approach is more comparable to the strategies used elsewhere in the literature. My sample consists of students with non-missing test scares at traditional public schools that are ever located within 5 miles of a charter.[38] The model predicting the test score of student $i$ in grade $g$ at traditional public school $j$ in year $t$ is as follows:

$$Y_{igjt} = \beta_1 AFTER_{jt} + \beta_2 TREATxAFTER_{jt} + \alpha X_{igt} + \theta_{gt} + \kappa_{gj} + \epsilon_{igjt}, \quad (6)$$

where TREAT is an indicator variable that is equal to 1 if a charter school ever operates within two miles of traditional public school $j$, AFTER is an indicator that is equal to 1 if a charter has started operating within five miles of school $j$, and the interaction term TREATxAFTER is the treatment variable. $\beta_2$ is the coefficient of interest, indicating the incremental change in scores for students in schools less than two miles from a charter as compared to students in schools 2-5 miles from a charter. The model includes all student-level covariates and lagged scores included in my main estimates in addition to school by grade fixed effects ($\kappa_{gj}$), which subsume the main effect of $TREAT$, and state-specific grade by year fixed effects ($\theta_{gt}$). Standard errors are clustered at the school level.

Estimates from this approach (Table 13) would not lead to the same conclusions as estimates from my main specification. While I find no effects in North Carolina, in Massachusetts I find significant positive effects in both subjects.[39] Although the 95% confidence intervals of these point estimates overlap those from my main model, these results suggest that the choice of counterfactual, in addition to other modelling choices, can be consequential for inference estimating charter effects.

[38]This includes charters that opened after 2002 in Massachusetts and charters that opened after 1997 in North Carolina.

[39]When I limit my sample to charters also included in my sample, I also find positive and significant estimates for Massachusetts in math. Estimates in ELA become insignificant.

## V. Discussion

I adapt Greenstone and Moretti's "runner-up" methodology to identify the effect of charter openings on the demographics and outcomes of students in traditional public schools in Massachusetts and North Carolina. I find that charter openings decrease enrollment in overlapping grades at nearby traditional public schools by about 4.7%. The effect of charter openings on the racial/ethnic composition of the traditional public schools varies by sector, with no effect in Massachusetts but significant (5.7 percentage point) reductions in the proportion of white students and increases of a similar magnitude in the proportion of Hispanic students in North Carolina. I find no effect of charter openings on the achievement of traditional public school students in either state, in contrast with some of the prior literature, and no effects on attendance rates or the probability of being suspended. I find evidence that academically-focused charters may generate positive effects on student achievement. My results are robust to the use of alternative radii and treatment specifications.

While the absence of a negative effect on student outcomes is reassuring, it is worth considering why I fail to find a positive effect. First, it may be that charter openings affect students in the traditional public sector in multiple, opposing ways; for example, generating negative impacts on resources and peer group composition but positively impacting the productivity of teachers and school leaders. Second, schools may not face strong incentives to respond to charter competition. Overcrowded schools in areas with growing populations may be relieved by charter openings. In Massachusetts, the tuition reimbursement plan generates short-term increases in per-pupil revenue when charters expand, blunting the financial consequences of charter growth. If districts, not schools, are the entities most affected by charter expansion, approaches to measuring charter effects at the school-level may suffer downward bias. Future work in this area could consider how institutional features of the state charter sector affect incentives and consequences for traditional public schools.

Given the diversity of charter schools across the 43 states with charter laws, it is unlikely that any single study will provide a convincing and complete account of the effects of charter expansion on the traditional public schools. This study highlights how empirical approaches to addressing the endogeneity of charter locations can be consequential for inference when estimating the effects of charters on traditional public schools, emphasizing the value of alternative identification strategies.

## REFERENCES

[1] Abdulkadiroğlu, Atila, Joshua D. Angrist, Susan M. Dynarski, Thomas J. Kane, and Parag A. Pathak. 2011. "Accountability and flexibility in public schools: Evidence from Boston's charters and pilots." *The Quarterly Journal of Economics* 126, no. 2: 699-748.

[2] Angrist, Joshua D., Sarah R. Cohodes, Susan M. Dynarski, Parag A. Pathak, and Christopher R. Walters. 2016. "Stand and deliver: Effects of Boston's charter high schools on college preparation, entry, and choice." *Journal of Labor Economics* 34, no. 2: 275-318.

[3] Angrist, Joshua D., Parag A. Pathak, and Christopher R. Walters. 2013. "Explaining charter school effectiveness." '*American Economic Journal: Applied Economics* 5, no. 4: 1-27.

[4] Arsen, David, and Yongmei Ni. 2012. "The effects of charter school competition on school district resource allocation." *Educational Administration Quarterly* 48, no. 1: 3-38.

[5] Bettinger, Eric P. 2005. "The effect of charter schools on charter students and public schools." *Economics of Education Review* 24, no. 2: 133-147.

[6] Betts, Julian R. (2009). "The competitive effects of charter schools on traditional public schools." *Handbook of Research on School Choice* (2009): 195-208.

[7] Bifulco, Robert, and Christian Buerger. 2015. "The influence of finance and accountability policies on location of New York state charter schools." *Journal of Education Finance* 40, no. 3: 193-221.

[8] Bifulco, Robert, and Ladd, Helen F. 2006. "The impacts of charter schools on student achievement: Evidence from North Carolina." *Education Finance and Policy* 1, no. 1: 50-90.

[9] Bifulco, Robert, and Helen F. Ladd. 2007. "School choice, racial segregation, and test-score gaps: Evidence from North Carolina's charter school program." *Journal of Policy Analysis and Management* 26, no. 1: 31-56.

[10] Booker, Kevin, Scott M. Gilpatric, Timothy Gronberg, and Dennis Jansen. 2008. "The effect of charter schools on traditional public school students in Texas: Are children who stay behind left behind?." *Journal of Urban Economics* 64, no. 1: 123-145.

[11] Carruthers, Celeste K. 2012. "New schools, new students, new teachers: Evaluating the effectiveness of charter schools." *Economics of Education Review* 31, no. 2: 280-292.

[12] Center for Research on Educational Outcomes. 2015. "Urban charter school study: report on 41 regions." Technical report. http://credo.stanford.edu/documents/NCSS%202013%20Final%20Draft.pdf.

[13] Charter School Facilities Initiative. 2013. "An analysis of the charter school facility landscape in Massachusetts." http://facilitiesinitiative.org/media/1235/.

[14] Figlio, David, and Cassandra Hart. "Competitive effects of means-tested school vouchers." *American Economic Journal: Applied Economics* 6, no. 1: 133-56.

[15] Fiske, Edward B., and Helen F. Ladd. (2001). *When schools compete: A cautionary tale.* Washington, D.C.: Brookings Institution Press.

[16] Friedman, Milton. 1962. *Capitalism and Freedom.* Chicago: University of Chicago Press.

[17] Gill, Sean, and Tricia Maas. (2017) "Opening the Schoolhouse Door: Helping Charter Schools Access Space in District-Owned Facilities." Center on Reinventing Public Education.

[18] Gilraine, Michael, Uros Petronijevic, and John D. Singleton. (2019) "Horizontal differentiation and the policy effect of charter schools." Annenberg Institute EdWorking Paper No. 19-80.

[19] Glomm, Gerhard, Douglas Harris, and Te-Fen Lo. 2005. "Charter school location." *Economics of Education Review* 24, no. 4: 451-457.

[20] Greenstone, Michael & Moretti, Enrico. 2003. "Bidding for industrial plants: Does winning a 'Million Dollar Plant' increase welfare." National Bureau of Economic Research Working Paper No. w9844.

[21] Greenstone, Michael, Richard Hornbeck, and Enrico Moretti. 2010. "Identifying agglomeration spillovers: Evidence from winners and losers of large plant openings." 'emphJournal of Political Economy 118, no. 3: 536-598.

[22] Henig, Jeffrey R., and Jason A. MacDonald. 2002. "Locational decisions of charter schools: Probing the market metaphor." Social Science Quarterly 83, no. 4: 962-980.

[23] Hoxby, Caroline. 2000. "Peer effects in the classroom: Learning from gender and race variation." National Bureau of Economic Research Working Paper No. w7867.

[24] Hoxby, C. M. 2003. "School choice and school productivity. Could school choice be a tide that lifts all boats?." In *The Economics of School Choice* (pp. 287-342). University of Chicago Press.

[25] Imberman, Scott A. 2011. "The effect of charter schools on achievement and behavior of public school students." *Journal of Public Economics* 95, no. 7-8 (2011): 850-863.

[26] Jackson, C. Kirabo. 2012. "School competition and teacher labor markets: evidence from charter school entry in North Carolina." *Journal of Public Economics* 96, no. 5-6: 431-448.

[27] Jinnai, Yusuke. 2014."Direct and indirect impact of charter schools' entry on traditional public schools: New evidence from North Carolina." *Economic Letters* 124, no. 3: 452-456.

[28] Lacireno-Paquet, Natalie, Thomas T. Holyoke, Michele Moser, and Jeffrey R. Henig. 2002. "Creaming versus cropping: Charter school enrollment practices in response to market incentives." *Educational Evaluation and Policy Analysis* 24, no. 2: 145-158.

[29] Ladd, Helen F., Charles T. Clotfelter, and John B. Holbein. 2017. "The growing segmentation of the charter school sector in North Carolina." *Education Finance and Policy* 12, no. 4: 536-563.

[30] Ladd, Helen F., and John D. Singleton. 2020. "The fiscal externalities of charter schools: Evidence from North Carolina." *Education Finance and Policy* 15, no. 1: 191-208.

[31] Massachusetts Department of Elementary and Secondary Education (MA DESE). 2018. "March 2018. Understanding charter school tuition reimbursements." http://www.doe.mass.edu/charter/finance/tuition/reimbursements.html.

[32] National Center for Education Statistics (NCES). 2018. "Table 216.20: Number and enrollment of public elementary and secondary schools, by school level, type, and charter and magnet status: Selected years, 1990-91 through 2016-17." *Digest of Education Statistics* 2018 ed: https://nces.ed.gov/programs/digest/d18/tables/dt18_216.20.asp?current=yes.

[33] National Center for Education Statistics. 2020. "Table 203.70: Percentage distribution of enrollment in public elementary and secondary schools, by race/ethnicity and state or jurisdiction: Fall 2000 to fall 2017." *Digest of Education Statistics* 2020 ed: https://nces.ed.gov/programs/digest/d19/tables/dt19_203.70.asp.

[34] National Education Association (NEA). 2019. "Rankings of the states 2018 and estimates of school statistics 2019." NEA Research. https://www.nea.org/assets/docs/2019%20Rankings%20and%20Estimates%20Report.pdf.

[35] Ni, Yongmei. 2009. "The impact of charter schools on the efficiency of traditional public schools: Evidence from Michigan." *Economics of Education Review* 28, no. 5: 571-584.

[36] Ridley, Matthew, and Camille Terrier. 2018. "Fiscal and education spillovers from charter school expansion." National Bureau of Economic Research Working Paper No. w25070.

[37] Sass, Tim R. 2006. "Charter schools and student achievement in Florida." *Education Finance and Policy*, 1(1), 91-122.

[38] Singleton, John D. 2019. “Incentives and the supply of effective charter schools.” *American Economic Review* 109, no. 7: 2568-2612.

[39] Slungaard Mumma, Kirsten. 2021. “Data and code for The Effect of Charter School Openings on Traditional Public Schools in Massachusetts and North Carolina.” Inter-university Consoritum for Political and Social Research [distributor]. https://doi.org/E134341V1.

[40] Winters, Marcus A. 2012. “Measuring the effect of charter schools on public school student achievement in an urban environment: Evidence from New York City.” *Economics of Education Review* 31, no. 2: 293-301.

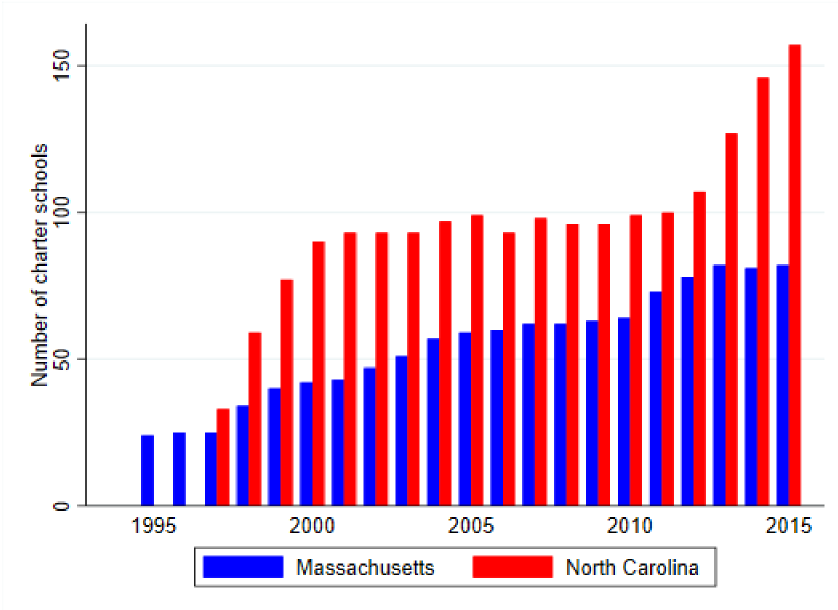



Figure 1. Number of Charter Schools by Year

*Source:* CCD

(a) Massachusetts (b) North Carolina

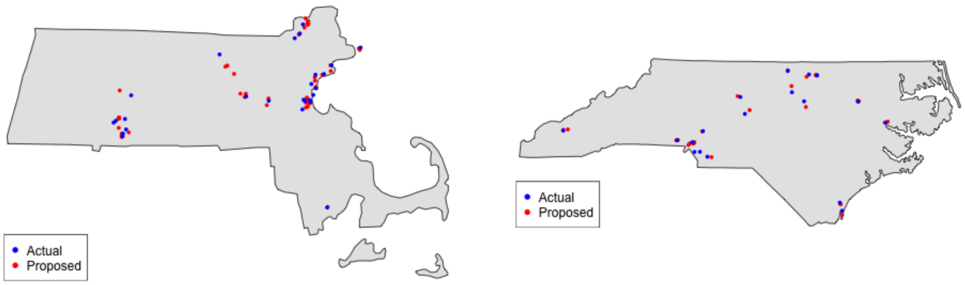



Figure 2. Actual and Proposed Sites of Charter Schools

*Note:* Actual locations reflect first occupied location reported in the CCD.

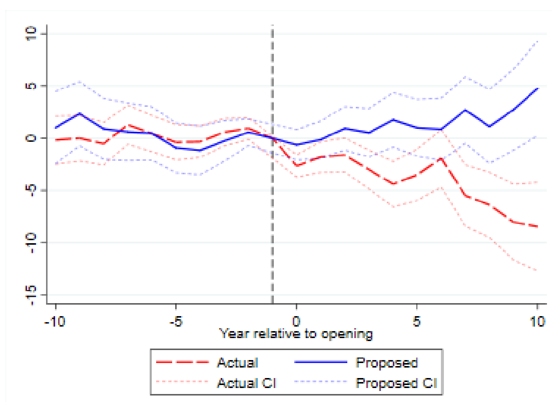
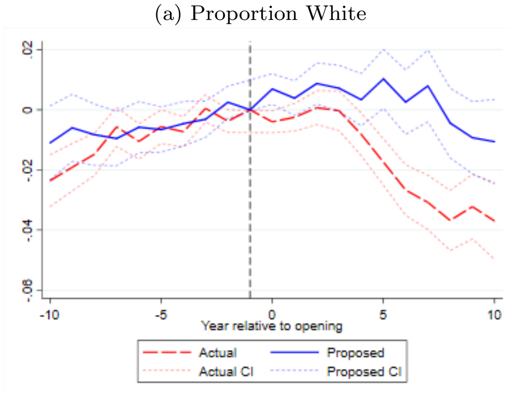


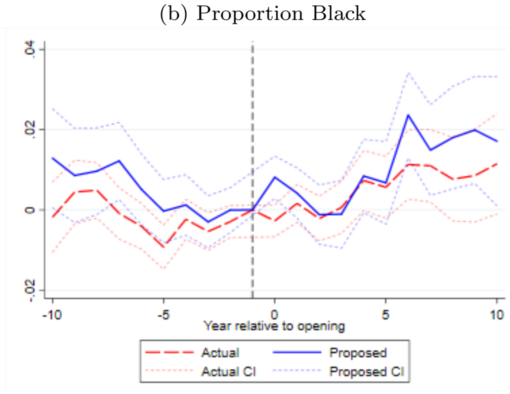

Figure 3. Grade-Level Enrollment (Pooled)

*Note:* This figure plots the coefficients $\delta_{Ap}$ and $\delta_{Pp}$ estimated by equation (2) with grade-level enrollment as the outcome for the pooled (multi-state) sample. Coefficients are transformed to express the difference in $\delta_{Ap}$ and $\delta_{Pp}$ relative to $\delta_{A-1}$ and $\delta_{P-1}$, respectively. Transformed values of $\delta_{A0}$ and $\delta_{P0}$ are reported in Table A8. Dotted lines represent 95% confidence intervals. Source: CCD.

(a) Proportion White




(b) Proportion Black




(c) Proportion Hispanic

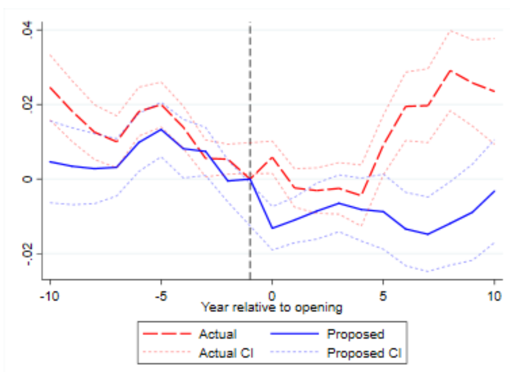



Figure 4. Effect of Charter Openings on Proportion Students by Race/Ethnicity (Pooled)

*Note:* This figure plots the coefficients $\delta_{Ap}$ and $\delta_{Pp}$ estimated by equation (2) for the proportion of students in a grade for each race/ethnicity in the pooled (multi-state) sample. Coefficients are transformed to express the difference in $\delta_{Ap}$ and $\delta_{Pp}$ relative to $\delta_{A-1}$ and $\delta_{P-1}$, respectively. Transformed values of $\delta_{Ap}$ and $\delta_{Pp}$ are reported in Table A9-A11. Dotted lines represent 95% confidence intervals. Source: CCD.

(a) Math (b) ELA

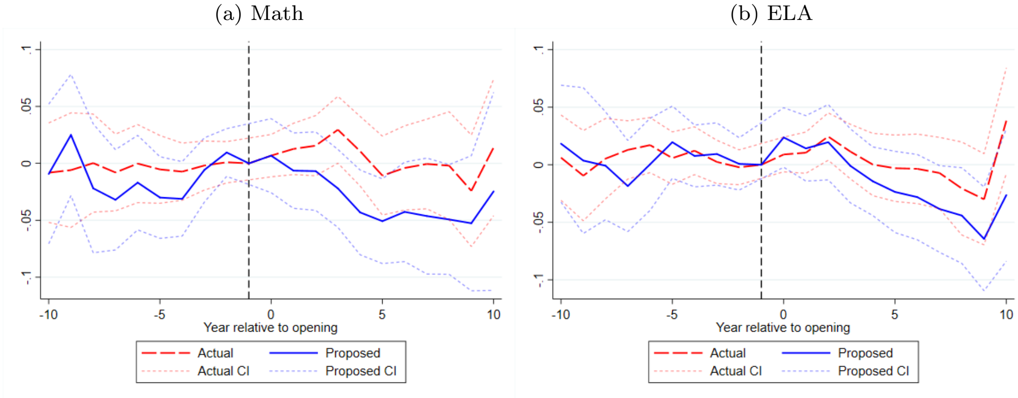



Figure 5. Effect of Charter Openings on Achievement (Pooled)

*Note:* This figure plots the coefficients $\delta_{Ap}$ and $\delta_{Pp}$ estimated by equation (4) for test scores using the pooled (multi-state) sample. Regressions include student covariates and cubics of once- and twice-lagged test scores. Coefficients are transformed to express the difference in $\delta_{Ap}$ and $\delta_{Pp}$ relative to $\delta_{A-1}$ and $\delta_{P-1}$, respectively. Transformed values of $\delta_{Ap}$ and $\delta_{Pp}$ are reported in Table A12 and A13. Dotted lines represent 95% confidence intervals.

(a) Attendance Suspensions

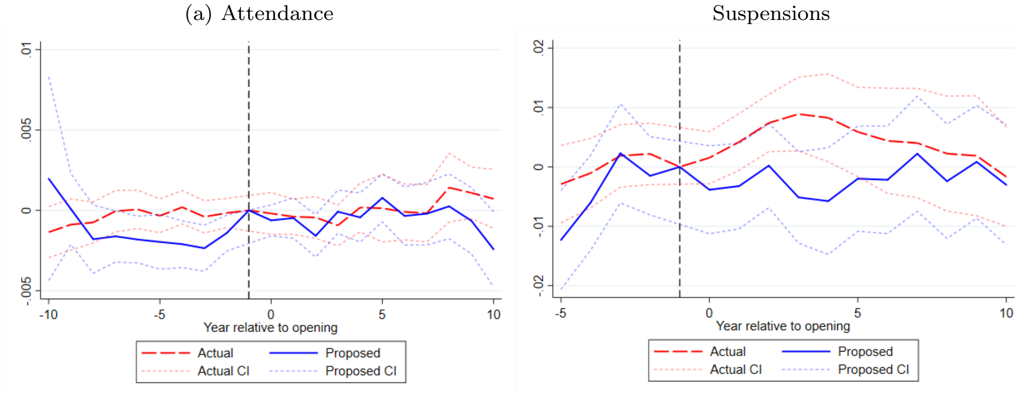



Figure 6. Effect of Charter Openings on Attendance and Suspensions (Pooled)

*Note:* This figure plots the coefficients $\delta_{Ap}$ and $\delta_{Pp}$ estimated by equation (4) for attendance and suspensions using the pooled (multi-state) sample. Regressions include student covariates, once- and twice-lagged attendance, and indicators for having an out-of-school suspension reported one- and two-years prior. I require one non-missing prior attendance observation for all observations included in the sample. Coefficients are transformed to express the difference in $\delta_{Ap}$ and $\delta_{Pp}$ relative to $\delta_{A-1}$ and $\delta_{P-1}$, respectively. Transformed values of $\delta_{Ap}$ and $\delta_{Pp}$ are reported in Table A14 and A15. Dotted lines represent 95% confidence intervals.

Table 1—Charter and Traditional Public Schools in Massachusetts and North Carolina

| | Massachusetts | | North Carolina | |
|---|---|---|---|---|
| | Traditional | Charter | Traditional | Charter |
| | (1) | (2) | (3) | (4) |
| *A. Schools* | | | | |
| Number of Schools | 1703 | 82 | 2333 | 157 |
| Urban Schools | 287 | 40 | 573 | 60 |
| Non-Urban Schools | 1416 | 42 | 1760 | 97 |
| *B. Students* | | | | |
| Number of Students | 873,751 | 40,297 | 1,453,516 | 82,260 |
| Proportion White | 0.65 | 0.33 | 0.50 | 0.53 |
| Proportion Black | 0.08 | 0.28 | 0.26 | 0.30 |
| Proportion Hispanic | 0.18 | 0.31 | 0.16 | 0.08 |
| Proportion Asian | 0.06 | 0.04 | 0.02 | 0.03 |
| Proportion Disadvantaged | 0.40 | 0.53 | 0.64 | 0.35 |
| *C. Test Scores* | | | | |
| Math | 0.02 | 0.09 | -0.00 | 0.11 |
| | (0.99) | (0.90) | (1.00) | (0.90) |
| | [439,451] | [16,381] | [646,385] | [42,524] |
| ELA | 0.02 | 0.08 | -0.01 | 0.23 |
| | (0.99) | (0.88) | (1.00) | (0.88) |
| | [437,934] | [16,368] | [643,365] | [42,494] |

Data in panels A and B are from the CCD for fall 2015, except the proportion economically disadvantaged students in Massachusetts, which was last available in fall 2014. In all tables, disadvantaged refers to economically disadvantaged students. Test scores are standardized by grade, year, and subject within state. Standard deviation for test scores are shown in parentheses; observations are in brackets. Test scores for North Carolina are end-of-grade scores from spring 2016 for students in grades 3-8 and 10. Test scores for Massachusetts are MCAS test scores for students in grades 3-8 in spring 2014, the last year this test was offered.

Table 2—Charter Openings Included/Excluded from Initial Sample

| | | Actual site was proposed | | Number of proposed sites | | | |
|---|---|---|---|---|---|---|---|
| | N | Yes | No | 1 | 2 | 3 | 4+ |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| *A. Massachusetts* | | | | | | | |
| Included | 26 | 5 | 21 | 7 | 11 | 7 | 1 |
| Excluded | | | | | | | |
| Opened in only proposed site | 4 | 4 | 0 | 4 | 0 | 0 | 0 |
| Insufficient info | 13 | | | | | | |
| Missing application | 1 | | | | | | |
| Other | 1 | | | | | | |
| *B. North Carolina* | | | | | | | |
| Included | 19 | 3 | 16 | 11 | 4 | 3 | 1 |
| Excluded | | | | | | | |
| Opened in only proposed site(s) | 11 | 11 | 0 | 10 | 1 | 0 | 0 |
| Insufficient info | 33 | | | | | | |
| Missing application | 3 | | | | | | |
| Other | 1 | | | | | | |

Data reflect Commonwealth charter school openings in Massachusetts and non-conversion charter schools in North Carolina. "Other" schools include one school that focuses on dropout engagement for over-age students in Massachusetts and one school in North Carolina whose only proposed site was very far from the addresses reported in the CCD (>17-133 miles) and whose full address history could not be verified online. I observe a few schools that operated <0.10 mile from a proposed site (possibly within the same building complex) and count these as operating in actual sites.

Table 3—Charter Openings Included and Excluded from Initial Sample

| | Excluded (1) | Included (2) | t-stat (p-value) (3) |
|---|---|---|---|
| *A. Number of Schools* | | | |
| All | 72 | 45 | |
| Urban | 33 | 22 | |
| Non-Urban | 39 | 23 | |
| Massachusetts | 19 | 26 | |
| North Carolina | 53 | 19 | |
| *B. Students* | | | |
| Average Grade Size | 58.36 | 66.27 | 1.33 |
| | | | (0.19) |
| Proportion White | 0.43 | 0.39 | -0.69 |
| | | | (0.49) |
| Proportion Black | 0.35 | 0.28 | -1.10 |
| | | | (0.27) |
| Proportion Hispanic | 0.15 | 0.26 | 2.34 |
| | | | (0.02) |
| Proportion Disadvantaged | 0.41 | 0.29 | -1.60 |
| | | | (0.11) |
| *C. Test Scores* | | | |
| Average ELA | 0.13 | 0.11 | -0.31 |
| | | | (0.76) |
| Average Math | 0.06 | 0.03 | -0.29 |
| | | | (0.77) |
| School Value-Added | -0.02 | -0.01 | 0.21 |
| | | | (0.84) |
| *D. Neighborhood Characteristics* | | | |
| Proportion White | 0.59 | 0.59 | -0.08 |
| | | | (0.94) |
| Proportion Black | 0.27 | 0.23 | -0.64 |
| | | | (0.52) |
| Proportion Hispanic | 0.09 | 0.13 | 1.38 |
| | | | (0.17) |
| Median Income | $26,182.00 | $26,986 | 0.45 |
| | | | (0.65) |
| *E. Year Opened* | | | |
| 2000-2002 | 26 | 7 | |
| 2003-2005 | 12 | 8 | |
| 2006-2008 | 8 | 6 | |
| 2009-2011 | 6 | 7 | |
| 2012-2013 | 20 | 17 | |

All data are defined in SY 2015-16, except for MA test score which are from SY 2013-14. Value-added measures are calculated as the school-by-year fixed effect in a regression of math scores on cubic controls for prior scores in both subjects, demographic controls, grade-by-year fixed effects, and indicators for missing scores or demographic variables. Value-added measures are normalized to sum to zero across all schools in a state in a given year. (See footnote 13 for detail). Column (3) reports the t-statistic and p-value of a test of equivalence of means for columns (1) and (2). All data come from the CCD except test scores and value-added measures.

Table 4—Balance Tests: Pooled

| | Actual (1) | Proposed (2) | t-stat (p-value) (3) | Obs (4) |
|---|---|---|---|---|
| *A. Student Demographics* | | | | |
| Average Grade Size | 149.14 | 140.57 | 0.55 | 587 |
| | | | (0.58) | |
| Change in Grade Size (5 years) | 0.44 | 1.83 | -0.33 | 577 |
| | | | (0.74) | |
| Proportion White | 0.39 | 0.39 | -0.15 | 598 |
| | | | (0.88) | |
| Proportion Black | 0.28 | 0.30 | -0.56 | 598 |
| | | | (0.58) | |
| Proportion Hispanic | 0.27 | 0.25 | 0.50 | 598 |
| | | | (0.61) | |
| Proportion Disadvantaged | 0.59 | 0.58 | 0.10 | 587 |
| | | | (0.92) | |
| *B. Test Scores* | | | | |
| Average Math | -0.22 | -0.26 | 0.24 | 558 |
| | | | (0.81) | |
| Average ELA | -0.24 | -0.28 | 0.28 | 559 |
| | | | (0.78) | |
| Change in Average Math (3 years) | 0.00 | 0.01 | -0.24 | 462 |
| | | | (0.81) | |
| Change in Average ELA (3 years) | -0.00 | -0.01 | 0.27 | 461 |
| | | | (0.79) | |
| *C. Neighborhood Characteristics* | | | | |
| Proportion White | 0.59 | 0.62 | -0.80 | 598 |
| | | | (0.43) | |
| Proportion Black | 0.21 | 0.20 | 0.54 | 598 |
| | | | (0.59) | |
| Proportion Hispanic | 0.14 | 0.14 | 0.17 | 598 |
| | | | (0.87) | |
| Median Household Income | $41,742 | $42,260 | -0.19 | 598 |
| | | | (0.85) | |
| Change in Population (1990-2000) | 425.05 | 441.12 | -0.06 | 598 |
| | | | (0.95) | |
| F-stat for joint probability test | | | | 1.34 |
| P-value for F-test | | | | (0.17) |
| Observations | | | | 566 |

Sample is limited to charters with at least one treatment and control school at the 2-mile radius. Schools near actual sites are within two miles of any actual site of a charter. Schools near proposed sites are within two miles of any proposed-only site of a charter (and are not also within two miles of an actual site). Observations are weighted to give each charter "case" equal weight, as described. Characteristics are defined in the year before the charter school opens. Column (3) reports results from a t-test for equivalence of (weighted) means in columns (1) and (2). F-test results are for a regression predicting being at an actual site with all covariates listed here and indicators for missing test score values. Standard errors are clustered at the school-level.

Table 5—Relationship Between Number of Nearby Charters and School Characteristics

| | Massachusetts (1) | North Carolina (2) |
|---|---|---|
| *A. Student Demographics* | | |
| Average Grade Size | -7.6 | -16.6 |
| | (2.72) | (4.4) |
| | [12,603] | [11,300] |
| Change in Grade Size (5 Years) | -0.65 | -0.65 |
| | (0.312) | (0.904) |
| | [11,772] | [9,925] |
| Proportion White | -0.132 | -0.079 |
| | (0.007) | (0.010) |
| | [12,785] | [11,305] |
| Proportion Disadvantaged | 0.107 | 0.053 |
| | (0.008) | (0.011) |
| | [11,937] | [10,653] |
| *B. Test Scores* | | |
| Average Math | -0.091 | -0.086 |
| | (0.013) | (0.018) |
| | [12,785] | [11,305] |
| Average ELA | -0.107 | -0.081 |
| | (0.013) | (0.019) |
| | [12,785] | [11,305] |
| Change in Average Math (3 years) | 0.011 | 0.002 |
| | (0.004) | (0.004) |
| | [7,508] | [9,159] |
| Change in Average ELA (3 years) | 0.007 | 0.000 |
| | (0.003) | (0.004) |
| | [7,508] | [9,158] |
| *C. Neighborhood Characteristics* | | |
| Median Household Income | -5206.71 | -5157.64 |
| | (490.960) | (956.635) |
| | [12,776] | [11,305] |
| Change in Population (1990-2000) | 152.942 | -166.077 |
| | (32.454) | (70.069) |
| | [12,766] | [11,305] |

Columns (1) and (2) report the coefficient on the number of charters within a 2-mile radius in separate regressions predicting each school- or neighborhood-level characteristics listed in the left-hand column using a panel of traditional public schools located within 5 miles of at least one charter. Regressions include year fixed effects and are estimated separately for each state subsample. Data cover SY 1997-98 to 2015-16 for North Carolina and SY 2000-01 to 2015-16 for Massachusett. (Test scores run until after SY 2013-14 in Massachusetts). Robust standard errors (in parentheses) are clustered at the school level.

Table 6—Effect of Charter Openings on Enrollment/Demographics

| | Pooled | Massachusetts | North Carolina |
|---|---|---|---|
| | (1) | (2) | (3) |
| Enrollment | -4.709 | -3.621 | -7.521 |
| | (1.363) | (1.603) | (2.632) |
| Mean | 95.8 | 96.8 | 91.7 |
| Proportion White | -0.009 | 0.006 | -0.057 |
| | (0.004) | (0.004) | (0.011) |
| Mean | 0.276 | 0.276 | 0.275 |
| Proportion Black | -0.002 | -0.003 | -0.002 |
| | (0.004) | (0.004) | (0.012) |
| Mean | 0.310 | 0.251 | 0.565 |
| Proportion Hispanic | 0.010 | -0.001 | 0.052 |
| | (0.003) | (0.004) | (0.007) |
| Mean | 0.352 | 0.406 | 0.112 |
| Observations | 32,539 | 25,379 | 7,160 |
| Charters | 36 | 23 | 13 |

Regression coefficients estimated using equation (1) with a dataset of school-by-grade-by-year observations for grades that are ever-served by a charter in the estimation sample using a 2-mile radius. All outcomes defined at the grade-level. Data come from the CCD from SY 1995-96 to SY 2015-16. Grade-level enrollment is available from SY 1995-96 to SY 2015-16; number and proportion of students are available from SY 1998-99 to SY 2015-16. Robust standard errors (in parentheses) are clustered at the school-by-grade level. Mean refers to the mean of schools near proposed-only schools.

Table 7—Alternative Radii, Effect of Charter Openings on Enrollment/Demographics

| | Pooled (1) | Massachusetts (2) | North Carolina (3) |
|---|---|---|---|
| *Panel A. 1.5 Mile Radius* | | | |
| Enrollment (Grade) | -2.637 | -1.353 | -7.083 |
| | (1.692) | (1.958) | (3.310) |
| Proportion White | 0.000 | 0.003 | -0.012 |
| | (0.005) | (0.005) | (0.012) |
| Proportion Black | -0.008 | -0.003 | -0.035 |
| | (0.004) | (0.004) | (0.013) |
| Proportion Hispanic | 0.007 | -0.001 | 0.044 |
| | (0.004) | (0.005) | (0.009) |
| Observations | 21,815 | 17,469 | 4,346 |
| Charters | 35 | 23 | 12 |
| *Panel B. 2.5 Mile Radius* | | | |
| Enrollment (Grade) | -2.737 | -0.465 | -6.754 |
| | (1.149) | (1.477) | (1.941) |
| Proportion White | 0.002 | 0.011 | -0.017 |
| | (0.004) | (0.004) | (0.007) |
| Proportion Black | -0.016 | -0.014 | -0.030 |
| | (0.004) | (0.004) | (0.008) |
| Proportion Hispanic | 0.017 | 0.010 | 0.043 |
| | (0.003) | (0.004) | (0.006) |
| Observations | 41,472 | 30,189 | 11,283 |
| Charters | 35 | 21 | 14 |

Regression coefficients estimated using equation (1) with a dataset of school-by-grade-by-year observations for grades that are ever-served by a charter in the estimation sample using the indicated radius. All outcomes defined at the grade-level. Data come from the CCD from SY 1995-96 to SY 2015-16. Grade-level enrollment is available from SY 1995-96 to SY 2015-16; number and proportion of students are available from SY 1998-99 to SY 2015-16. Robust standard errors (in parentheses) are clustered at the school-by-grade level.

Table 8—Effect of Charter Openings on Student Achievement

| | Pooled | Massachusetts | North Carolina |
|---|---|---|---|
| | (1) | (2) | (3) |
| Math | 0.019 | 0.030 | -0.001 |
| | (0.017) | (0.022) | (0.024) |
| | [1,092,499] | [758,083] | [334,416] |
| Mean | -0.277 | -0.333 | -0.106 |
| | | | |
| ELA | 0.011 | 0.018 | -0.006 |
| | (0.013) | (0.019) | (0.013) |
| | [1,091,735] | [758,505] | [333,230] |
| Mean | -0.311 | -0.376 | -0.112 |
| | | | |
| Charters | 34 | 22 | 12 |
| Years | 1997-2015 | 2002-2013 | 1997-2015 |

This table reports estimates of $\lambda$ from equation (3). The estimation sample consists of student-by-year observations for students in grades served by both the charter and traditional public school at traditional public schools within two miles of an actual or proposed-only site, as described. Estimates include controls for student gender, economically disadvantaged status, race/ethnicity, special education status (Massachusetts), an indicator for being an immigrant (Massachusetts), limited English proficiency status, parental education levels (North Carolina), disabled and gifted student status (North Carolina) and once- and twice-lagged test scores in both subjects. All regressions include indicators for missing values of demographic variables. Once-lagged scores for 10th graders are defined in grade 8. The test score sample is limited to students in grade 4-8 and grade 10 (Massachusetts only) with at least one non-missing once-lagged test score.Test score coverage varies by state; see "Years" row. Test scores are standardized by year, grade, and subject within state. Standard errors (in parentheses) are clustered at the grade-by-school level. Number of observations in brackets. Control mean reflects the mean for schools near proposed-only sites.

Table 9—Effect of Charter Openings on Annual Attendance and Suspensions

| | Pooled (1) | Massachusetts (2) | North Carolina (3) |
|---|---|---|---|
| Attendance | -0.001 | -0.001 | -0.001 |
| | (0.001) | (0.001) | (0.001) |
| | [1,940,027] | [1,594,079] | [345,948] |
| Mean | 0.946 | 0.943 | 0.961 |
| Charters | 36 | 23 | 13 |
| Years | 2003-2015 | 2003-2015 | 2006-2014 |
| | | | |
| Suspensions | 0.003 | 0.002 | 0.017 |
| | (0.003) | (0.003) | (0.009) |
| | [1,847,917] | [1,603,735] | [244,182] |
| Mean | 0.080 | 0.077 | 0.109 |
| Charters | 36 | 23 | 13 |
| Years | 2003-2015 | 2003-2015 | 2010-2015 |

This table reports estimates of $\lambda$ from equation (3). The estimation sample consists of student-by-year observations for students in grades served by both the charter and traditional public school at traditional public schools within two miles of an actual or proposed-only site, as described. Estimates include student demographic controls as well as once-lagged annual attendance and an indicator for having an out-of-school suspension in the previous year. The sample is limited to students in grades 1-12 with a non-missing attendance observation in the prior year. Outcome data coverage varies by state; see "Years" row. Standard errors (in parentheses) are clustered at the grade-by-school level. Number of observations in brackets. Control mean reflects the mean for schools near proposed-only sites.

Table 10—Robustness Checks: Alternative Radii

| | Pooled (1) | Massachusetts (2) | North Carolina (3) |
|---|---|---|---|
| *Panel A. 1.5 Mile Radius* | | | |
| Math | 0.031 | 0.055 | -0.017 |
| | (0.022) | (0.029) | (0.027) |
| | [758,425] | [531,886] | [226,539] |
| ELA | 0.022 | 0.042 | -0.015 |
| | (0.016) | (0.022) | (0.014) |
| | [756,761] | [530,885] | [225,876] |
| Attendance | -0.001 | -0.001 | -0.000 |
| | (0.001) | (0.001) | (0.001) |
| | [1,354,310] | [1,136,285] | [218,025] |
| Suspensions | -0.001 | -0.003 | 0.016 |
| | (0.004) | (0.004) | (0.013) |
| | [1,295,313] | [1,142,941] | [152,372] |
| *Panel B. 2.5 Mile Radius* | | | |
| Math | 0.052 | 0.076 | 0.023 |
| | (0.014) | (0.021) | (0.017) |
| | [1,417,296] | [857,414] | [5,598,82] |
| ELA | 0.019 | 0.032 | 0.004 |
| | (0.011) | (0.019) | (0.010) |
| | [1,412,880] | [854,717] | [5,581,63] |
| Attendance | -0.002 | -0.002 | -0.001 |
| | (0.000) | (0.001) | (0.001) |
| | [2,428,287] | [1,869,517] | [558,770] |
| Suspensions | -0.003 | -0.003 | 0.001 |
| | (0.003) | (0.003) | (0.006) |
| | [2,272,891] | [1,882,340] | [390,551] |

This table reports estimates of $\lambda$ from equation (3) The estimation sample consists of student-by-year observations for students in grades served by both the charter and traditional public school at traditional public schools within the specified radius of an actual or proposed-only site, as described. See notes for Tables 8 and 9 for more detail.

Table 11—Robustness Checks: School-Level Definition of Treatment

| | Pooled<br>(1) | Massachusetts<br>(2) | North Carolina<br>(3) |
|---|---|---|---|
| Math | -0.001 | 0.003 | -0.012 |
| | (0.018) | (0.026) | (0.024) |
| | [1,232,185] | [805,286] | [426,899] |
| | 35 | 22 | 13 |
| ELA | -0.002 | 0.001 | -0.008 |
| | (0.014) | (0.021) | (0.012) |
| | [1,231,652] | [806,096] | [425,556] |
| Charters | 35 | 22 | 13 |
| Years | 1997-2015 | 2002-2013 | 1997-2015 |

This table reports estimates of $\nu$ from equation (5). Treatment is defined in the first year the charter opens. The sample is defined and structured in the same way as for Tables 8 and 9 and limited to observations in grades served by both the charter and traditional public school. Standard errors are clustered at the school-level, the level at which treatment is assigned.

Table 12—Heterogeneity of Effects on Student Achievement

| | Urban<br>(1) | Non-Urban<br>(2) | Horizontally-Differentiated<br>(3) | Academically-Focused<br>(4) |
|---|---|---|---|---|
| Math | 0.008 | 0.015 | -0.026 | 0.040 |
| | (0.022) | (0.033) | (0.025) | (0.021) |
| | [847,536] | [244,961] | [267,818] | [824,681] |
| Mean | -0.291 | -0.229 | -0.120 | -0.328 |
| Charters | 25 | 9 | 8 | 26 |
| ELA | 0.011 | 0.021 | -0.012 | 0.034 |
| | (0.015) | (0.024) | (0.018) | (0.017) |
| | [845,767] | [245,967] | [268,287] | [823,447] |
| Mean | -0.341 | -0.208 | -0.129 | -0.371 |
| Charters | 25 | 9 | 8 | 26 |

See notes for Tables 8 and 9. Pooled (multi-state) sample. See Appendix D for detail on designations of “horizontally-differentiated” and “academically-focused” charter schools. Urban schools refer to schools with a ”city” locale code in the CCD.

Table 13—Alternative Estimation Approach

| | Pooled (1) | Massachusetts (2) | North Carolina (3) |
|---|---|---|---|
| *Panel A. Math* | | | |
| TreatxAfter | 0.008 | 0.143 | -0.025 |
| | (0.020) | (0.048) | (0.020) |
| After | -0.006 | -0.122 | 0.015 |
| | (0.020) | (0.053) | (0.019) |
| | [1,861,127] | [654,977] | [1,206,150] |
| *Panel B. ELA* | | | |
| TreatxAfter | 0.019 | 0.119 | -0.001 |
| | (0.014) | (0.036) | (0.011) |
| After | -0.012 | -0.102 | 0.005 |
| | (0.013) | (0.039) | (0.011) |
| | [1,859,996] | [657,090] | [1,202,906] |
| Charters | 92 | 47 | 45 |

Estimates from alternative specification strategy (see description of equation 6). Includes students with non-missing scores at schools located within 5 miles of a charter school.