Horizontal Differentiation and the Policy Effect of Charter Schools*

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Abstract

While school choice may enhance competition for students, incentives for public schools to raise productivity may be muted if public education is viewed as imperfectly substitutable with alternatives. In this paper, we estimate the policy-relevant effect of charter school expansion on education quality while accounting for the horizontal differentiation of charter school programs. To do so, we combine student-level administrative data with novel information about the educational programs of charter schools that opened in North Carolina following the removal of the statewide cap in 2011. The dataset contains students' standardized test scores as well as geocoded residential addresses, which allow us to compare the test score changes of students who lived near the new charters prior to the policy change with those for students who lived farther away. We apply this research design to estimate separate effects for students exposed to entry by horizontally differentiated and non-horizontally differentiated charter schools. The results indicate that learning gains for treated students are driven entirely by non-horizontally differentiated charter schools: we find that the causal effect of non-horizontally differentiated charter school expansion is 0.05σ while the effect of horizontally differentiated charter schools is not different from zero.

Keywords: Horizontal Differentiation, Charter Schools, Competition.

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1 Introduction

School choice policies provide parents and students with schooling options other than government-run public schools. For example, charter schools – the primary vehicle for school choice in the United States – are privately-operated, but publicly-funded and tuition-free. A significant literature, relying on lottery-based designs that account for student selection, establishes that specific charter schools improve student learning and later-life outcomes (Hoxby and Murarka, 2009; Abdulkadirolu et al., 2011; Angrist et al., 2013a; Dobbie and Fryer, 2013b). These findings have helped spur recent policy momentum behind charter school expansion.

Theoretically, there are two main channels through which greater school choice, such as charter schools, may affect student outcomes: First, the opening a charter school will cause some students who would otherwise attend traditional public schools to enroll. For these students, the causal effect of charter expansion is measured by how effective the new charter school is at improving outcomes relative to the alternative. Second, the expansion of charter schools can have indirect effects on the students who remain in public schools. Specifically, greater choice may put competitive pressure on government-run schools (Friedman, 1962; Hoxby, 2000). Public funding for charter schools, for instance, is tied to student enrollment. As a result, expansion of school choice creates incentives on the margin for public schools to be productive in order to retain students. For policy, this potential effect is first-order, as these incentives may raise the quality of education across the board, creating "a tide that lifts all boats" (Hoxby, 2002).

A key premise underlying this indirect channel is that competition between schools is largely along vertical lines: Parents and students views schools as homogeneous, save for productivity differences, and choose among alternatives accordingly. However, evidence from a variety of contexts indicates that parents and students view schools as differentiated products (Bayer et al., 2007; Burgess et al., 2015; Arcidiacono et al., 2017) and select schools based on idiosyncratic match (Hastings et al., 2006; Walters, 2014). School sorting

on learning impacts or effectiveness, such as captured by measures of school value-added, is accordingly limited (e.g., Hsieh and Urquiola (2003); Rothstein (2006); Abdulkadiroglu et al. (2017)). One reflection of this horizontal differentiation of schools is wide variation in educational programs among charter schools. Programs and instruction offered at charters include Montessori, experiential and project-based learning, as well as language immersion, arts and sports-based curricula. To the degree that traditional public school education is viewed by households as imperfectly substitutable with such programs, competitive incentives for public schools to increase productivity may in turn be muted (MacLeod and Urquiola, 2013).

In this paper, we examine the policy-relevant effect of charter school expansion on education quality while accounting for the horizontal differentiation of charter programs. To do so, we propose a novel strategy that leverages variation following North Carolina's removal of the statewide cap on charter schools in 2011. Our approach does *not* require separately estimating the effects of charter expansion on charter and traditional public school students. Our analysis is facilitated by a unique dataset that combines student-level administrative records from North Carolina with information about charter schools' educational programs.

Our dataset links measures of student learning in North Carolina with exposure to charter school entry following the removal of the charter school cap. From the North Carolina Education Research Center (NCERDC), we obtain longitudinal student-level records that include performance on standardized exams as well as the geocoded residence of the student, which is key for defining treatment status. These data are then merged with information about the educational program of each entering charter school. Using applications to the State Board of Education to open, we classify charter schools as horizontally differentiated from public education if learning is experiential or project-based as opposed to focused on core skills through traditional instruction. This classification allows us to account for the role of horizontal differentiation of charter programs in estimating the effect of charter expansion.

With these data in hand, our research design combines the timing of the policy change with information on the distances between students' *pre-policy-change* residences and new

charter schools that opened following the removal of the cap. Our difference-in-differences approach then identifies the policy-relevant effect of expansion by comparing test score changes for students who lived near the new charter schools prior to the policy change (treatment) with test score changes for students who lived farther away (control). We estimate separate effects for students exposed to entry by horizontally differentiated charter schools and for students exposed to entry by non-horizontally differentiated charter schools irrespective of whether the students switched into a charter school or remained in traditional public schools. By remaining agnostic about students' ex-post schooling choices in our approach, our method relies on weaker assumptions about student sorting than strategies used in prior work.

The results indicate that students ultimately exposed to charter school entry following the policy change experienced an average improvement in standardized math test scores of 0.02 standard deviations relative to untreated students. However, this combined effect masks substantial heterogeneity by charter school type: we find that the causal effect of non-horizontally differentiated charter school expansion is 0.05 standard deviations while the expansion of horizontally differentiated charter schools instead has no effect on student test scores. We subject these findings to several robustness and validity checks, which demonstrate that our results are not driven by either students sorting across neighborhoods in response to (or in anticipation of) the policy change or strategic charter school location decisions based on unobservable neighborhood trends. Further, our results are robust to alternative definitions of exposure to charter school expansion.

Our results are consistent with the demand for horizontally differentiated charter schools being unresponsive to adjustments in traditional public school quality. Public schools therefore face weak incentives to improve their quality when faced with competition from a horizontally differentiated charter school because parents who seek differentiated programs likely desire non-traditional or alternative learning experiences. To better understand the role of the direct and competitive channels, we calculate value-added (in terms of student test scores)

for individual charter schools. The results indicate that not only is the aggregate effect of horizontally differentiated charter schools not different from zero but that, consistent with the differences in curricula and academic expectations, horizontally differentiated charters have lower value-added impacts on average.

Our findings are important for evaluating the expansion of school choice policies and of charter schools in particular. When considering whether to allow expansion in a particular neighborhood, policymakers will want to know how all students in that area are likely to be affected, regardless of whether students remain in public schools or switch to a new charter school. For students exposed to charter entry, we find gains that are driven entirely by charter schools that are non-horizontally differentiated in their educational program. In identifying the importance of heterogeneity among charter schools for this aggregate effect, our findings thus complement prior work that has emphasized the effectiveness of "No Excuses" charter operators in particular (e.g. Angrist et al. 2012, 2013b; Dobbie and Fryer 2013a) and the equilibrium implications of behavioral differences across types of charter schools (Singleton, 2017).

We find that the positive impacts operate primarily through the indirect channel, connecting with a large empirical literature that examines quality competition in education markets (e.g. Hoxby 2000; Neilson 2013). Figlio and Hart (2014), for example, find increases in learning for students attending public schools disproportionately exposed to competition by Florida's means-tested voucher program. In contrast, previous findings regarding the competitive effects of charter schools have been generally mixed (see Epple et al. (2015)); our results suggest that this ambiguity stems in part from prior neglect of horizontal differentiation of educational programs among charters, which biases estimates towards zero.

The remainder of the paper proceeds as follows. In the next section, we sketch a stylized model of school competition that motivates our focus on horizontal differentiation and describe the construction of the dataset. We then detail our research design, based around the combination of North Carolina's lifting of the charter school cap in 2011 and geocoded

student addresses, in Section 3. We present the results, including robustness checks and investigation of the mechanisms, in Section 4 before concluding.

2 Background and Data

North Carolina lifted its statewide cap on the number of charter schools in the state on June 6th, 2011. The removal of the cap, previously set at 100 since the initial 1996 legislation that authorized charter schools in the state (starting for the 1997-98 school year), led to considerable entry of new charter schools in North Carolina. Nine charter schools were approved by the newly-created Charter Schools Advisory Board to open for the 2012-13 school year, with another twenty-three approvals following in 2013-14.

Figure 1 displays the number of charters in North Carolina for school years 1996-97 through 2016-17. The figure clearly shows the lifting of the charter school cap in the 1997-98 school year and the rapid expansion in the charter sector thereafter: North Carolina went from no charter schools in 1996-97 to just shy of 100 charter schools in 2000-01. The number of charter schools in the state then remained near one hundred for the next decade, with only minor fluctuations due to closures of some charter schools. Another rapid expansion then occurs in 2012-13 when the charter school cap is lifted, with the number of charter schools operating in North Carolina reaching 176 in 2016-17.

In this paper, we use this policy variation to estimate the policy-relevant effect of charter school expansion. This effect represents the combined influence of two channels: On the one hand, the opening of a charter school causes some students who would otherwise attend traditional public schools to enroll. For these students, the effect of expansion is measured by the relative effectiveness of the new charter school. In this regard, lottery-based designs provide compelling evidence of student learning gains from charter school attendance (Hoxby and Murarka, 2009; Abdulkadirolu et al., 2011; Angrist et al., 2013a; Dobbie and Fryer, 2013b).[1] Moreover, these impacts are heterogeneous across types of charter schools. Gains

[1]Other work uses longitudinal variation in administrative datasets, finding more mixed results (Sass,

are pronounced for "No Excuses" charter schools (Angrist et al., 2013b; Dobbie and Fryer, 2013a), so-named for an educational program emphasizing high-expectations, comportment, and core math and reading skills (Carter, 2000; Thernstrom and Thernstrom, 2004)

Charter expansion may also cause spillover effects on students who choose to remain in public schools. Specifically, choice may stimulate competition for students, potentially raising the quality of education across the board (Hoxby, 2002). Nonetheless, prior findings regarding the effects of charter schools on public school students tend to be mixed or even contradictory. For instance, while Sass (2006), Booker et al. (2008), and Winters (2012) report positive effects, Bettinger (2005), Bifulco and Ladd (2006), and Zimmer and Buddin (2009) do not find evidence of competitive gains. Imberman (2011), which uses an IV strategy to overcome endogenous charter location, meanwhile finds mixed to negative effects from charter competition. However, this ambiguity of results may be in part because heterogeneity among charter schools – neglected by the prior work – is equally important for the competitive channel. In particular, the effect of charter expansion via this mechanism depends on the nature of school competition. Below, we formalize this intuition in a simple model that serves to motivate our subsequent empirical analysis.

We describe the unique dataset we assemble to pursue the analysis in the second subsection. We combine detailed, student-level administrative records from North Carolina for the 2008-09 to 2016-17 school years with novel information that summarizes the educational programs of charter schools. This information allows us to account for the role of horizontal differentiation in estimating the policy effect of charter expansion on student learning. We also present summaries of the data.

2006; Hanushek et al., 2007; Booker et al., 2007). Similarly, CREDO (2009) uses matching techniques with student level-data from fifteen states and D.C., finding notable heterogeneity in average charter quality. Beyond school outcomes, papers using panel and lottery-based approaches have also examined medium and longer term impacts. See Epple et al. (2015) for a recent review.

2.1 School Competition and Horizontal Differentiation

School choice may have competitive impacts that raise the quality of education even for students who remain in public schools. In this subsection, we develop a model that highlights how this theoretical expectation depends on the character of school competition. The model motivates our empirical analysis, which leverages data on the educational programs of charter schools in North Carolina to account for horizontal differentiation.

The model considers the quality choice facing a local public school that is exposed to an entering charter school. We make the simplifying assumption that, absent the charter school's presence, the public school would capture the entire enrollment, given by N. The key primitive of the model is the semi-elasticity of demand for the charter school with respect to the public school's quality, represented by $-\sigma$. This parameter fully characterizes the nature of competition: progressively larger values of σ imply increasingly vertical competition, as greater public school quality draws additional students away from the charter school. In contrast, $\sigma = 0$ reflects entirely horizontal differentiation, in which case demand for the charter school is unresponsive to public school quality.

The public school chooses quality q in order to maximize a utility function given by:

$$U = \mu(N - D_c(q; \sigma)) - \frac{1}{2}q^2$$

where μ is the public school's constant per-pupil markup. $D_c(q; \sigma)$ represents the charter school's demand function, which is bounded above by N and depends on σ, the parameter characterizing competition. There is also a convex cost of supplying quality, which we normalize to 1.[2] An immediate implication of this setup is that the public school would set quality at 0 absent competition from the charter school.

[2]This rent-seeking objective of public schools parallels the setup in McMillan (2004), though with choice of quality instead choice of effort. McMillan (2004) also models effort as instead raising per-unit costs.

The first-order condition of the maximization problem is given by:

$$-\mu \frac{\partial D_c}{\partial q} = q$$

Multiplying both sides by q and re-arranging, the solution is given by:

$$q^* = \sqrt{\mu\sigma}$$

From this expression, it is easy to see that the equilibrium quality of the public school is increasing in the per-pupil markup, μ, and decreasing in the semi-elasticity of demand, $-\sigma$.

This result highlights how the competitive effect of charter expansion is likely to depend on the degree of substitutability between the public school and the entering charter school: For charter schools in which $\sigma > 0$, the public school will raise its quality in response to competition. However, as horizontal differentiation increases, decreasing σ, the competitive response of the public school becomes more muted. In the extreme case of a charter school that is perfectly differentiated horizontally (i.e. $\sigma = 0$), the effect of competition on public school quality is zero. This has an important implication for empirical analyses that neglect horizontal differentiation of charter programs: treating all charter exposure equally is likely to bias estimates of competitive effects towards zero.

While this model is highly stylized, it motivates us to examine the role of horizontal differentiation among charter schools in estimating the policy effect of charter expansion. To do so, we assemble a unique dataset described in detail in the next subsection.

2.2 Data Sources and Summaries

For our analysis, we assemble a dataset that links annual measures of North Carolina students' learning to their exposure to charter school entry following the 2011 removal of the statewide cap on charter schools. Importantly, the data include novel information about each entering charter school's educational program gathered from applications to the State

Board of Education. This section describes the primary data sources and includes summaries drawn from the data.

2.2.1 Data Sources

We use detailed, student-level administrative records from the North Carolina Education Research Center (NCERDC). The records include information about all North Carolina public school students (charter and traditional public) for the 2008-09 to 2016-17 school years. The data contain test scores for each student in mathematics and reading on standardized, end-of-grade exams in grades three through eight, which we use to measure students' learning. Test scores are reported on a developmental scale, designed such that each additional point represents the same knowledge gain, regardless of the student's grade or baseline ability. To create comparability of test scores across grades, we standardize this scale at the student level to have a mean of zero and a variance of one for each grade-year. In addition to test scores, the student data contain information regarding each student's grade, socioeconomic status, race and ethnicity, and gifted or special education status.[3]

We also obtain information regarding students' residential location in each school-year from the NCERDC. As we detail in the next section, this information is necessary for our research design, which defines exposure to charter entry by a student's residence in the school year in which the cap on charter schools was lifted.[4] For confidentiality reasons, student location in the NCERDC data is reported at the Census block level. We therefore define each student's location as the centroid of the block in which they reside.[5]

We combine the student-level data with information about the educational program of each charter school. Following the lifting of the statewide cap in 2011, a number of charter schools submitted applications to open to the Charter Schools Advisory Board. Each application to open a charter school contains detailed, mandatory information about the

[3]We also gather data for whether a student is repeating or skipping a grade.
[4]Residential information for students in charter schools is not contained in NCERDC.
[5]The median area of a Census block in North Carolina is 2.26 acres.

prospective school, including its intended grade levels, projected enrollment, leadership and governance, mission, instructional program, and statements of goals and educational focus. We use the information contained in the applications, which are posted publicly online, to manually classify each approved charter school as either "horizontally differentiated" or "not horizontally differentiated" from public schools in their educational program. In particular, we classify as horizontally differentiated charter schools that emphasize project-based or experiential learning (including Montessori) in their application. Charters are otherwise classified as not horizontally differentiated. Non-horizontally differentiated schools therefore include those focused on core skills and/or using traditional instruction, including charter schools that following "No Excuses" practices (Angrist et al., 2013b). We present our classification of the individual charter schools in the Appendix.

2.2.2 Data Summaries

Our data of newly-opened charter schools consist of twenty-three elementary charter schools that opened in 2012-13 and 2013-14. Of these schools, six opened in 2012-13 and seventeen opened in 2013-14. We divide these newly-opened charters by their horizontal differentiation to traditional public schools: we designate thirteen schools as horizontally differentiated and ten schools as non-horizontally differentiated. Appendix .2 indicates each newly-opened charter designation and opening year.

Figure 2 indicates where exactly in North Carolina these newly-opened charters locate. For each newly-opened charter, we draw a circle with a 5-mile radius around the opening location as students residing within these five mile radii will be considered 'nearby' the newly-opened charter in our main specifications later. We can see that the majority of our charters open in urban/suburban areas and that there is some clustering by differentiation: there is a cluster of five horizontally differentiated charters in the Durham-Raleigh area and a cluster of four non-horizontally differentiated charters in the Greensboro region.

Table 1 reports summary statistics for students living within five miles of these newly-

opened charters. Column (2) clearly indicates that these newly-opened charter schools open in areas with much higher proportion of black students, with a corresponding lower proportion of white students than in North Carolina at large. When we further subdivide by charter type, we see that the non-horizontally differentiated charters appear to locate in these higher proportion black (and lower proportion white) regions than their horizontally differentiated counterparts. Furthermore, we also see that these horizontally differentiated charters locate in regions with higher quality public schools (as measured by value-added) than their non-horizontally differentiated counterparts.

3 Research Design

Credibly estimating the effect of charter school expansion requires addressing three main empirical challenges. First, because students choose between attending a traditional public school or charter school, one must account for student selection into schools. Second, charter schools do not locate randomly within school districts, but rather choose where to operate strategically (Singleton, 2017). Estimating the effects of charter school expansion on student outcomes therefore requires accounting for systematic differences between areas with and without charter schools. Third, as highlighted by our stylized model of school competition, charter schools offer incredibly heterogeneous curricula, so competitive effects on traditional public schools are likely to vary greatly by charter type. Failing to account for thid heterogeneity may bias estimates towards zero.

Prior studies have approached these challenges in a number ways, depending on whether the estimate of interest is the direct effect of charter schools on the students who attend or the indirect (competitive) effect on students who stay in traditional public schools. In this section, we provide a detailed description of our strategy for estimating the aggregate effect of charter school expansion using variation following the lifting of North Carolina's statewide cap, followed by a discussion of our identifying assumptions and how they compare to those

in prior work.

3.1 Overview

We propose an estimation approach for credibly estimating the *aggregate* or *policy-relevant* effect of charter school expansion, consisting of both the effect on charter school and traditional public school students. By not attempting to separately identify the two effects, our method relies on weaker assumptions about student selection into charter or traditional public schools than estimation strategies used in prior work (with observational data). We also differentiate charter schools into different types using information on school characteristics obtained from school applications and websites, allowing us to uniquely account for the role of horizontal differentiation and relax the assumption of common competitive effects across all charter school types.

The key innovation in our approach is that it is agnostic about which students switch into charter schools and which students do not. We avoid having to distinguish between such students by relying on the 2011 policy change in North Carolina that lifted the cap on the number of charter schools allowed to operate in the state. Combining this change in the institutional environment with information on the distance between students' *pre-policy-change* residences and the new charter schools that opened following the removal of the cap, we identify students who are (likely) exposed to charter school expansion (treatment) and students who are not (control).

Here, our research design leverages the plausibly exogenous *timing* of the policy change, which makes it unlikely that students could sort across neighborhoods in anticipation of the new policy or that the first waves of charter school entrants had full discretion over when to enter the market.[6] We then identify the policy-relevant effect of charter school expansion by comparing test score changes for students who lived near the new charter schools prior to the policy change with test score changes for students who live farther away, *regardless* of

[6]We present evidence to this effect below.

whether any of these students switched into the charter schools or remained in the traditional public school system. We now provide a detailed description of our estimation strategy along with a more complete discussion of the identifying assumptions.

3.2 Details

Our strategy for estimating the aggregate effect of charter school expansion combines the removal of the charter school cap in North Carolina with information on the location of students' residences at the time of the cap's lifting.[7]

North Carolina removed the cap in June 2011, with the 2012-13 academic year being the first year new charter schools could begin operating after the policy change. A key institutional feature for our research design is that the first two waves of new charter school entrants had to declare an intent to open *prior* to any new schools beginning operations. Schools opening during the first two academic years following the policy change therefore could not make decisions about when and where to locate based on market responses to new entrants. As such, we treat the timing of their market entry as plausibly exogenous (along with the timing of the policy change)[8] and restrict our analyses to focus on only the first two waves of charter school entrants.[9]

To be more specific about the application timing for the first two waves of charter schools, the first wave of schools hoping to open during the 2012-13 academic year applied through a special 'fast track' application process designed to approve schools quickly after the lifting of the cap. Schools were required to submit an application to the Charter Schools Advisory Board by November 10, 2011 and the board made its final decision about the fast-tracked applications on February 2, 2012, at which point approved schools began preparations for

[7]Note that, in contrast to the literature that estimates competitive effects, we do not rely on the distance between traditional public schools and the newly-opened charter schools.

[8]We discuss our approach for accounting for remaining concerns about endogenous charter school location decisions below.

[9]It is also the case that charter schools entering in later waves are mainly high schools and none are horizontally differentiated, making it very difficult to execute our research design.

opening in August 2012.[10] Schools hoping to open as part of the second wave in the 2013-14 academic year had to initiate the application process by May 2012 and successful applicants were granted official approval in March of 2013, at which point they began preparing to open in August of 2013.[11] Therefore, schools opening in both the first and second academic years after the policy change had to submit their applications *prior* to any new charter school opening.[12] In total, nine charter schools were approved by the Charter Schools Advisory Board to open in the 2012-13 school year, while twenty-three schools were approved to open 2013-14 school year.

Using the timing of the application and approval process for schools opening in the first two years after the policy change, we define a student as being exposed to (or 'treated' by) charter school expansion if his or her residence in the 2011-12 school year (the year before new charter schools opened in the post-policy-change period) is within r miles of one of the immediate entrants of charter schools that were slated to open in either the 2012-13 or 2013-14 academic year. We pool openings in both academic years because traditional public schools may have been able to anticipate new charter school openings in 2013-14 and preemptively respond to an approaching change in competitive pressure in 2012-13.[13]

To formalize our measure of exposure to charter school expansion, suppose that there are a total of C immediate entrant charter schools and index these charter schools by $c = 1, 2, \ldots, C$. Define d_{ic} as the distance between student i's residence in the 2011-12 academic

[10]To see a detailed breakdown of this timeline, see, for example, the application of Cornerstone Charter Academy: `http://www.ncpublicschools.org/docs/charterschools/resources/application/2011fasttrack/cornerstone.pdf`

[11]See the timeline under the Application Process Data for 2013-2014 (`http://www.ncpublicschools.org/charterschools/applications/process/`) and the date of submission of the, for example, Howard and Lillian Lee Scholar charter school application (`http://www.ncpublicschools.org/docs/charterschools/resources/application/2014apps/leescholarsrubric.pdf`).

[12]Future charter school applications followed a similar structure, which we highlight with charter schools attempting to open for the 2015-16 school year. Applicants were first required to submit an application to the Charter Schools Advisory Board by early December 2013. Applications were then reviewed by the board and successful applicants were invited to an in person interview with the board in February or March of 2014, at which point the Board would vote, either granting the school a charter or rejecting their application. Successful applicants then would have a 'planning year' during the 2014-15 school year and be able to open for the 2015-16 school year.

[13]Although we are low on statistical power when treating the two waves separately, all of our results are quantitatively and qualitatively similar.

year and charter school c. Further, let $g = g(i,t)$ denote the grade of student i in year t. Student i is treated by charter expansion when his or her 2011-12 residence is within r miles of at least one immediate entrant charter school that serves the same grade as that in which the student is enrolled at the time of opening (as long the student is in eighth grade or below).[14] Specifically, student i's treatment status is given by

$$treat_i^r = \max\{treat_{i,2012-13}^r, treat_{i,2013-14}^r\}, \tag{3.1}$$

where, for $t = 2012-13$ or $2013-14$, we have

$$treat_{it}^r = \begin{cases} 1, & \text{if } \min\{d_{ic}, d_{i2}, \ldots, d_{iC}|\ c \text{ serves grade } g(i,t)\ \&\ g(i,t) \leq 8\} \leq r \\ 0, & \text{otherwise.} \end{cases} \tag{3.2}$$

Our main treatment variable ($treat_i^r$) is best viewed as measuring an intention to treat, as students who move from their 2011-12 residence after the policy change may not necessarily be treated by the charter school expansion that came after the policy change.[15]

Defining treatment this way, we estimate the following difference-in-differences regression to recover the effect of charter school expansion while allowing for potentially differential effects across horizontally and non-horizontally differentiated charter schools:

$$\begin{aligned} y_{isgt} = \alpha + \delta_g + \lambda_t + \zeta X_{isgt} + \mu_h treat_i^r + \phi Post_t + \beta_h Post_t * treat_i^r + \cdots \\ \mathbb{1}_{\{c \in NH\}}\Big(\alpha_{nh-h} + \delta_{g,nh-h} + \lambda_{t,nh-h} + \zeta_{nh-h} X_{isgt} + \mu_{nh-h} treat_i^r + \cdots \\ \phi_{nh-h} Post_t + \beta_{nh-h} Post_t * treat_i^r\Big) + \epsilon_{isgt}. \end{aligned} \tag{3.3}$$

Here, $Post_t$ indicates that the observation is from the academic year 2012-13 or later, while

[14] A newly-opened charter is considered to serve a given grade if its charter school application indicates that they plan to serve that grade within three years of opening.

[15] Below, we provide a detailed analysis of moving rates before and after the policy change, showing that they are not differential across students who are treated and untreated by charter school expansion.

$\mathbb{1}_{\{c \in NH\}}$ is a binary variable indicating that student i is either a member of the control or treatment group for non-horizontally differentiated charter schools. In practice, we define two control groups, one that is used as the comparison group for students treated by non-horizontally differentiated charter schools and one that is used as the comparison group for students treated by horizontally differentiated charter schools. The binary variable $\mathbb{1}_{\{c \in NH\}}$ turns on for (i) students for whom the closest charter school is both non-horizontally differentiated and within r miles of their residences (i.e., those treated by non-horizontally differentiated schools) and (ii) for students for whom the closest charter school is non-horizontally differentiated but greater than r miles away from their residences (i.e., the control group for students who are treated by non-horizontally differentiated schools). The variable turns off for (i) students for whom the closest charter school is both horizontally differentiated and within r miles of their residences (i.e., those treated by horizontally differentiated schools) and for students for whom the closest charter school is horizontally differentiated but greater than r miles from their residences (i.e., the control group for students who are treated by horizontally differentiated schools).

This regression includes both charter school and traditional public school students between the 2007-08 and 2016-17 academic years.[16] The dependent variable (y_{isgt}) is the standardized (at the grade-year level) test score of student i in school s in grade g at time t. The vector X_{isgt} is a set of covariates including student race, gender, gifted status, English learner status, disability status, free or reduced price lunch status, grade skipping or repeating status, while δ_g is a set of grade fixed effects, and λ_t is a set of year fixed effects. To allow for treated and untreated students to be as comparable as possible, we further restrict the analysis sample to students whose 2011-12 residence is within $2r$ miles of a charter school that would open in either the 2012-13 or 2013-14 academic year. Here, treated students are those who lived within r miles of such a school and untreated students are those who lived

[16]Our sample ends in the 2016-17 academic year and, although data on student residence locations becomes available in the 2006-07 academic year, we do not include this year in our main analyses to keep our analysis sample consistent across specifications with and without prior student tests scores. We therefore have five years of data both prior to and after the policy change.

between r and $2r$ miles away.[17]

The parameters β_h and β_{nh-h} are the main parameters of interest in equation (3.3), representing, respectively, the effect of being treated by horizontally differentiated charter school expansion and the additional (or differential) effect of being treated by non-horizontally differentiated charter school expansion. The parameter β_h captures the change in the difference between the average performance of students treated by horizontally differentiated charter schools and untreated students before and after the policy change (conditional on the other control variables). The parameter β_{nh-h} captures the differential effect of this change (that is, the effect relative to β_h) when students are treated by non-horizontally differentiated charter schools. The sum $\beta_h + \beta_{h-nh}$ is therefore the total effect of non-horizontally differentiated charter school expansion.

The OLS estimates of β_h and β_{nh-h} recover causal effects of charter school expansion under the assumption that trends in unobservable characteristics that affect test scores are the same across treated and untreated students. It is instructive to think about the validity of this assumption in the context of the main threats to identification.

Student Selection. Much of the prior literature (that uses observational data) relies on student fixed-effects methods to account for student selection into school types when estimating either the direct (see, for example, Bifulco and Ladd 2006; Imberman 2011a) or competitive effects of charter schools (see, for example, Bifulco and Ladd 2006; Imberman 2011b: Jinnai 2014). Although these methods credibly account for selection into charter schools or traditional public schools that is based on time-invariant unobserved student characteristics, they remain vulnerable to student selection into schools based on time-varying characteristics, such as anticipated performance trends.[18]

[17] We discuss our choice of r (the distance between students' residence and the newly-opened charter schools) below and we also show that our main results are robust to many different choices of r in subsection 4.2.

[18] For example, parents may make decisions about whether to exit the traditional public school system based on *trends* in their students' test scores, in which case the estimated effect of charter school attendance or competitive pressure could reflect the continuation of a trend rather than the unbiased effect of attending a charter school or being in a traditional public school that faces competition.

By defining treatment using the distance between immediate charter school entrants after the policy change and students' residences *prior* to these openings, our strategy circumvents such selection issues because it is agnostic as to whether a student remained in their traditional public school or switched into a charter school. Students are treated (i.e., exposed to charter school expansion) simply if their 2011-12 residence is sufficiently close to a charter school that opens in the post-policy change period.

Although we are able to address the issue of student selection into schools, one may worry that our strategy is potentially vulnerable to students moving across (i.e., selecting into) neighborhoods in response to the policy change. It is possible, for example, that students whom we define as untreated (according to their 2011-12 residences) later move into an area with a newly-opened charter school nearby. Such students would contribute to the average change in test scores for the control group despite being exposed to treatment. On a related note, it is possible that students anticipated the new charter school openings and moved across neighborhoods prior to the 2012-13 academic year in order to move into or out of areas where new charters would locate. In this case, our estimation strategy could also reflect a pre-existing performance trend rather than the (pure) effect of charter school expansion. To address these potential issues, we directly explore moving rates before and after the policy change as well as estimate specifications with student fixed effects.

Endogenous Charter School Location Decisions. By fixing treatment status according to students' residences in 2011-12 and then comparing test score gains before and after the policy change, we investigate how test scores change among students living within given neighborhoods. As such, our strategy accounts for the possibility that there are differences in time-invariant unobservable characteristics across treated and untreated neighborhoods and that charter schools make location decisions based on these characteristics.

Our strategy is not robust, however, to charter schools making location decisions based on differential trends across treated and untreated neighborhoods. For example, if charter schools locate in areas where average test scores are falling relative to the other areas,

then our estimated effects of charter expansion would be downward biased by pre-existing neighborhood trends. After presenting our main results below, we conduct event studies and estimate specifications with neighborhood-specific trends to demonstrate that our results are not driven by differential trends across areas with and without newly-opened charter schools.

Horizontal Differentiation of Charter School Programs. Most prior studies that estimate either the direct or competitive effects of charter schools constrain these effects to be the same for all charter schools. This constraint potentially imposes a strong restriction on the data, as charter schools offer heterogeneous programs and are therefore likely to attract different student types and create differential incentives to respond across traditional public schools. In particular, as outlined by our stylized model, we expect that the charter schools that are not horizontally differentiated with traditional public schools are likely to create the strongest competitive incentives.

As discussed above, we embrace this heterogeneity in our analysis, using the data available on charter schools' applications and websites to categorize each charter school as either horizontally differentiated from traditional public schools or not. With this information in hand, we separately identify the effects of horizontally and non-horizontally differentiated charter school expansion in specification (3.3) above.

The Choice of Distance Cutoff to Define Treatment

Prior to presenting our results, we first discuss the distance cutoff we use to define a student as treated by charter school expansion. Most studies that estimate competitive effects of charter schools on traditional public schools use radii ranging from 1 to 10 miles as the distance cutoff in which competitive forces are strongest. We use the midpoint of this range for our main specifications, taking $r = 5$ miles to construct our treatment variable in equation (3.1). As Table 2 demonstrates, however, non-trivial proportions of students transfer from traditional public schools to charter schools when their place of residence (in 2011-12 academic year) is both closer to and farther away from newly-opened charter schools.

Among students observed attending a public school in 2011-12 and living in a residence that is within 2 miles of any newly-opened charter school, 2.68 percent transferred to a charter school by the 2013-14 academic year.[19] As the distance between student residence and charter schools increases, the proportion of students transferring monotonically declines, with only 0.23 percent of students living between 10 and 15 miles of charter school eventually transferring. We therefore present several specification checks below, showing that our main results are very similar for a wide range of distance cutoffs that define treatment.

4 Results

4.1 Main Results

Columns (1) and (2) in Table 3 report our main results obtained from estimating equation (3.3). When constraining the policy-relevant effect of charter school expansion to be the same across horizontally and non-horizontally differentiated charter schools, the estimated effect on student math scores is 3 percent of a standard deviation. As expected, allowing for differential effects across charter school types indicates that the aggregate effect masks important (and statistically significant) heterogeneity. In particular, students affected by the expansion of non-horizontally differentiated schools realize an improvement in math performance of 0.05 standard deviations, while those affected by the expansion of horizontally differentiated schools do not realize any improvement. The point estimates are very similar across specifications with (column 2) and without (column 1) student demographic variables as additional controls.

These results are consistent with our discussion and stylized model above. The demand for horizontally differentiated charter schools (that is, those not focused on core skills or using traditional instruction) is unlikely to be responsive to adjustments in traditional public school

[19] Among students who lived within 2 miles of non-horizontally and horizontally differentiated charter schools, respectively, 2.76 and 2.63 percent transferred to a charter school of each type by the 2013-14 academic year.

quality. Parents and students in these schools expect a differentiated learning experience, one not focused on the core skills measured by the standardized tests we use as the outcome variable. Being aware of parental preferences, traditional public schools face little incentive to improve their own quality. In addition, because of the differentiated curricula and academic expectations that are prevalent in horizontally differentiated schools, such schools are likely to have low value-added in terms of student test scores.[20] Together, these forces mute the both competitive and direct effects of horizontally differentiated charter schools, implying that the test score effects of charter school expansion are only seen among charter schools that are not horizontally differentiated from traditional public schools.

We are unable to precisely disentangle the direct and competitive effects of non-horizontally differentiated charter schools because very few students switch into the charter sector during our sample period. Restricting our sample to student who stayed in traditional public schools throughout our entire sample period leaves our results virtually unchanged, as less than 2 percent students exited the traditional public school system.[21] To better understand the direct effects of different charter types, we estimate charter school value-added below and show that it is indeed lower for horizontally differentiated charter schools, suggesting a weaker direct effect among these schools on student test score outcomes.

4.2 Robustness Checks

In this subsection, we conduct a series of robustness checks, demonstrating that our results are robust to concerns about students sorting across neighborhoods in response to the policy change and charter schools making location decisions based on differential trends in student performance across neighborhoods. We also perform a series of specification checks, considering several alternative ways of defining treatment status.

[20] We show that this is indeed the case below.

[21] Among 117,210 treated students, 2,090 students switch into a charter school by the end of our sample period. Among the 139,237 control students, only 1,363 students switch into charter schools.

4.2.1 Student Selection Across Neighborhoods

Because treatment is determined by place of residence *prior* to new charter school openings, students who move into neighborhoods with new charter schools in response to the policy change are not treated according to our definition. Nonetheless, these students may affect our estimated treatment effect. Suppose, for example, that charter school expansion has a positive competitive effect on traditional public schools and that students move in order to attend these now improved schools. In our specification, these students would remain in the control group but would have higher test scores because they are attending the same (now improved) schools as the treated students, thus biasing our exposure effect downward.

On a related note, because the charter school cap was officially lifted in June 2011 and the first 'fast track' charter school applications were submitted in November 2011, it is possible that families anticipated the new charter school openings in August 2012 and responded by moving into different neighborhoods prior. If so, our estimated effect could reflect the continuation of a performance trend that started prior to the policy change.

We now explore these potential sources of bias directly. Figure 3 plots differential moving rates across treated and control students for both horizontally and non-horizontally differentiated charter schools. We construct the figure by first estimating the following equation

$$\begin{aligned} m_{isgt} = \alpha + \delta_g + \lambda_t + \zeta X_{isgt} + \mu_h treat_i^r + \sum_{t=2008-09}^{2010-11} \beta_h^t \mathbb{1}_{\{year=t\}} * treat_i^r + \cdots \\ \sum_{j=2012-13}^{2016-17} \beta_h^t \mathbb{1}_{\{year=t\}} * treat_i^r + \mathbb{1}_{\{c \in NH\}} \Bigg(\alpha_{nh-h} + \delta_{g,nh-h} + \lambda_{t,nh-h} + \cdots \\ \zeta_{nh-h} X_{isgt} + \mu_{nh-h} treat_i^r \sum_{t=2008-09}^{2010-11} \beta_{nh-h}^t \mathbb{1}_{\{year=t\}} * treat_i^r + \cdots \\ \sum_{j=2012-13}^{2016-17} \beta_{nh-h}^t \mathbb{1}_{\{year=t\}} * treat_i^r \Bigg) + \epsilon_{isgt}, \end{aligned} \tag{4.1}$$

where we regress an indicator for student i changing residences between year $t-1$ and t on grade fixed effects, year fixed effects, demographic control variables, year fixed effects

interacted with treatment status, and year fixed interacted with treatment status and an indicator for treatment being by a non-horizontally differentiated charter school. We then plot the estimated β_h^t and $\beta_h^t + \beta_{nh-h}^t$ terms (in separate panels), which represent the degree to which moving rates are differential between untreated students and students treated by horizontally and non-horizontally differentiated charter schools, respectively. In each year, we also plot the 90-percent confidence interval associated with the estimated coefficients.

As can be seen in Figure 3, there is no evidence that treated students move across neighborhoods at a differential rate than untreated students in both the pre- and post-policy change period. This is true in both the specifications where we consider non-horizontally and horizontally differentiated charter schools.[22] The evidence in Figure 3 suggests that it is unlikely that the treatment effect we estimate of non-horizontally differentiated charter school expansion is driven by differential sorting of treated and untreated students *either* before *or* after the new charter schools began operating.

Although we do not find any evidence for either pre-existing or policy-induced differential moving rates, we further assess the robustness of our results to threats stemming from student selection by estimating specifications in which we augment equation (3.3) to include student fixed effects. The effect of charter school expansion in these specifications is estimated by using within-student changes in test scores, thereby mitigating any potential biases stemming from students sorting across treated and non-treated areas, as the effect of charter school expansion is identified by within-student gains in treated areas relative to within-student gains in non-treated areas (instead of simply differential average test score changes across the two areas).

The corresponding estimates are reported in column (3) in Table 3 and are very similar to the main estimates presented in columns (1) and (2). Here, we note that different samples of students contribute identifying variation in the specifications with and without student

[22]We do not perform the analyses here with the 2009-10 academic year. Student residence is defined using Census blocks and the definitions of Census blocks change in the 2009-10 academic year (with the 2010 Census), making it difficult to determine whether students moved relative to the previous year.

fixed effects. When accounting for student fixed effects, only students for whom we observe a test score both before and after 2011-12 (the final year before new charter school entrances) contribute identifying variation to our estimate of the policy relevant effect of charter school expansion, whereas we do not face that constraint in specifications without student fixed effects.[23] Despite approximately only 60 percent of our main sample contributing identifying variation when we make within-student comparisons, our results remain very similar to those from our baseline specifications.

In summary, we do not find any evidence that treated and non-treated students sorted across neighborhoods differentially either prior to the policy change or in response to it. This is perhaps not surprising, as the policy change happened quickly and families would have had imperfect information about where new charter schools would eventually locate.

4.2.2 Endogenous Charter School Location Decisions

As discussed above, our identification strategy is potentially vulnerable to charter schools choosing to locate in neighborhoods based on pre-existing trends in student performance. If, for example, charter schools locate in areas where average test scores are rising relative to other areas, then our estimated effects of charter expansion would be upward biased by pre-existing neighborhood trends. The opposite would be true if they locate in areas where average scores are differentially decreasing.

We evaluate the extent to which differential trends across treated and non-treated areas

[23]Residential address data is available for students starting in kindergarten, implying that we are able to observe which schools students attend prior to the time we observe their first standardized test score in third grade.

are likely to play a role in our analysis with the following event-study design

$$y_{isgt} = \alpha + \delta_g + \lambda_t + \zeta X_{isgt} + \mu_h treat_i^r + \sum_{t=2008-09}^{2010-11} \beta_h^t \mathbb{1}_{\{year=t\}} * treat_i^r + \cdots$$
$$\sum_{j=2012-13}^{2016-17} \beta_h^t \mathbb{1}_{\{year=t\}} * treat_i^r + \mathbb{1}_{\{c \in NH\}} \Bigg(\alpha_{nh-h} + \delta_{g,nh-h} + \lambda_{t,nh-h} + \cdots$$
$$\zeta_{nh-h} X_{isgt} + \mu_{nh-h} treat_i^r \sum_{t=2008-09}^{2010-11} \beta_{nh-h}^t \mathbb{1}_{\{year=t\}} * treat_i^r + \cdots$$
$$\sum_{j=2012-13}^{2016-17} \beta_{nh-h}^t \mathbb{1}_{\{year=t\}} * treat_i^r \Bigg) + \epsilon_{isgt}, \tag{4.2}$$

where the estimated β_h^t and $\beta_h^t + \beta_{nh-h}^t$ terms in the pre-reform period capture potentially differential trends in outcomes across untreated and untreated areas (by horizontally and non-horizontally differentiated schools, respectively). Figure 4 below shows the estimated coefficients in each year along with the associated 90-percent confidence intervals. There is no evidence of differential trends in test scores prior to the policy change between untreated students and students treated by either horizontally and non-horizontally differentiated charter schools. The test scores for students who are treated by non-horizontally-differentiated charter schools only start to clearly increase following the policy change.

The results in Figure 4 indicate that there is no evidence for differential trends in test scores across untreated and treated students prior to the policy change. To further rule out differential trends as driving our results, we re-estimate equation (3.3) but additionally include neighborhood fixed effects and neighborhood-specific linear time trends in the specification.

Because we use the distance between each student's 2011-12 residence and the newly-opened charter schools to define treatment, we record the Census tract in which each resided in the 2011-12 school year as his or her neighborhood. If the new charter schools located near treated students because the neighborhoods in which these students lived were experiencing differential trends relative to the neighborhoods of untreated students, then we should not

continue to estimate a positive and statistically significant effect of charter school expansion after accounting for such potentially differential test-score trends prior to the policy change.

Columns (4) in Table 3 presents the estimated effects from specifications that control for neighborhood-specific trends in test scores, while column (5) presents estimates from a specification that additionally accounts for student fixed effects. The estimates in columns (4) and (5) are very similar to those from our main specifications in columns (1) and (2), suggesting that endogenous charter school entry based on pre-existing neighborhood-specific trends in test scores is unlikely to be driving our results.

4.2.3 Specification Checks

In this subsection, we explore the sensitivity of our results to the specification of our estimating equation. In particular, we consider varying the radius we use to define treatment and defining treatment using a continuous measure of distance.

Varying the Treatment Radius

Figure 5 shows how our main treatment effect estimates from column (1) of Table 3 change as we change the radius used to define treatment. As a point of reference, recall that our main specification uses a radius of 5 miles and our main treatment effect estimate for the expansion of non-horizontally differentiated charter schools is 0.05 student-level standard deviations of the mathematics score. The profile in Figure 5 shows that the estimated treatment effect is stable for many choices of radii, ranging in integer values from 2 miles to 8 miles. In all cases but one (4 miles) the estimated treatment effect is not statistically differentiable from our main estimate of 0.04 standard deviations.

Measuring Treatment Using Continuous Distance

We also re-estimate our main specification (using our main sample of students living within 10 miles of a newly-opened charter school) but measure treatment using a continuous

measure of distance between student residence and charter school location. If exposure to charter school expansion becomes weaker with distance, then we would expect the treatment effect to be decreasing in the distance between students' residences and charter schools. This is exactly what we find in Table 4, which reproduces all of the results from Table 3 while measuring treatment using continuous distance instead of binary cutoff. The estimate in column (1) implies that a one-mile increase in students' 2011-12 residences from the nearest non-horizontally differentiated charter school decreases the estimated treatment effect by 0.006 standard deviations. A back-of-the-envelope calculation implies that this estimate is remarkably close to our main estimate using a binary cutoff at 5 miles to define treatment.

Among treated students in our main specification (those living within 5 miles of a newly-opened charter school), the average distance between their residences and the nearest non-horizontally differentiated charter school 2.5 miles. Among untreated students, the average distance is 7.5 miles, implying an average difference in distance between treated and untreated students of 5 miles. Using the estimate of -0.006 standard deviations per mile in column (2) of Table 4 implies a test score difference between treated and untreated groups of 0.03 standard deviations (-0.006SD$\times$5 miles), which is very similar to the estimate of 0.05 standard deviations in column (1) of Table 3.

In sum, our estimates are remarkably stable, across both various radii and the alternative methods for measuring treatment intensity.

4.3 Direct Effects and the Role of Vertical Differentiation

In this subsection, we explore the direct effects of charter schools and the role of vertical differentiation in our analysis. In doing so, we address an important concern that our results may be driven by vertical differentiation of charter schools along the quality dimension rather than horizontal differential of charter school programs.

To start, we estimate school-level value-added using standard methods. In particular, we regress student test scores on a flexible function of prior-year test scores, student demographic

controls, and school fixed effects in a pooled sample of traditional public school and charter school students. We then take each school's fixed effect as its value-added estimate.

Depicting average school-level value-added in each post-policy-change year, Figure 6 shows our main finding with respect to the direct effects of charter schools. As expected, the average non-horizontally differentiated charter school has much higher test score value-added than the average horizontally differentiated school. As a point of reference, value-added estimates are normalized such that the average value-added among traditional public schools is zero. In the first post-policy-change year, both types of charter school have substantially lower value-added than traditional public schools, on average. By the second year, however, non-horizontally differentiated charter schools have higher average value-added than traditional public schools,[24] while horizontally differentiated charter schools continue to lag behind for the duration of our sample period.

Given these descriptive results, we now explore whether our main results are explained by non-horizontally differentiated schools also being vertically differentiated along the quality dimension (as measured by value-added). To do so, we modify our main estimating equation by including the value-added of the nearest charter school for each student in the regression, along with the appropriate interaction terms:

$$
\begin{aligned}
y_{isgt} = {} & \alpha + \delta_g + \lambda_t + \zeta X_{isgt} + \mu_h treat_i^r + \phi Post_t + \beta_h Post_t * treat_i^r + \cdots \\
& \mathbb{1}_{\{c \in NH\}} \Big(\alpha_{nh-h} + \delta_{g,nh-h} + \lambda_{t,nh-h} + \zeta_{nh-h} X_{isgt} + \mu_{nh-h} treat_i^r + \cdots \\
& \phi_{nh-h} Post_t + \beta_{nh-h} * Post_t * treat_i^r \Big) + \cdots \\
& \nu_h^1 \mathrm{VA}_{ic} + \nu_h^2 \mathrm{VA}_{ic} * treat_i^r + \nu_h^3 \mathrm{VA}_{ic} * Post_t + \nu_h^4 \mathrm{VA}_{ic} * treat_i^r * Post_t + \cdots \\
& \mathbb{1}_{\{c \in NH\}} \Big(\nu_{nh-h}^1 \mathrm{VA}_{ic} + \nu_{nh-n}^2 \mathrm{VA}_{ic} * treat_i^r + \cdots \\
& \nu_{nh-h}^3 \mathrm{VA}_{ic} * Post_t + \nu_{nh-h}^4 \mathrm{VA}_{ic} * treat_i^r * Post_t \Big) + \epsilon_{isgt}. \qquad (4.3)
\end{aligned}
$$

If traditional public schools respond to non-horizontally differentiated schools also being

[24] Average value-added among charter schools changes over time because of new charter school openings.

vertically differentiated, then we would expect to find a positive and significant estimate for the sum $\nu_h^4 + \nu_{nh-h}^4$, the total effect of charter school value-added in the post-policy-change period for students who are treated by the expansion of non-horizontally differentiated charter schools. Further, if vertical differentiation fully explains our results above, we would expect our main estimate of the impact of non-horizontally differentiated charter school expansion ($\beta_h + \beta_{h-nh}$) to fall to zero.

Table 5 reports the results from estimating equation (4.3). In column (1), we reproduce our main estimates from column (2) of Table 3. In column (2), we add to the specification the value-added of the charter school a student is exposed to in order to test whether vertical differences between charter schools explain our main findings. As the results reveal, the coefficient measuring the effect of charter school value-added on treated students in the post-policy-change period is small and statistically insignificant, while our main effect of non-horizontally differentiated charter schools is unchanged.

In column (3), we allow for differential effects of school value-added by charter type, investigating whether competitive responses by public schools to a given charter type vary with charter school value-added – that is, we directly test whether the public school response to non-horizontally differentiated charter schools is increasing in the value-added of those schools. We find that the value-added of non-horizontally differentiated charter schools is positively associated with student outcomes, but our main effect of non-horizontally differentiated charter schools remains unchanged.

In columns (3) to (6), we demonstrate similar patterns using specifications that are estimated with student fixed effects and neighborhood-specific trends. Our main results are again unchanged, while the estimated association between charter school value-added and student outcomes is now not statistically different from zero. In sum, although non-horizontally differentiated charter schools are better along the test score quality dimension than horizontally differentiated schools, we find no evidence that vertical differentiation drives our findings.

5 Conclusion

School choice policies, such as charter schools, aim to expand educational opportunity and raise the quality of education even for students who remain in public schools. In particular, by enhancing competition, school choice creates incentives on the margin for public schools to be productive in order to retain students. However, as we highlight with a stylized model, this theoretical expectation depends crucially on the nature of school competition. To the degree that traditional public school education is viewed as imperfectly substitutable with alternative educational programs, such as those offered by many charter schools, competitive incentives for public schools may in turn be muted. This insight thus has an important implication for empirical analyses of charter school expansion: treating all charter exposures equally is likely to bias estimates of effects towards zero.

With this motivation, this paper estimates the policy-relevant or aggregate effect of charter expansion using variation following North Carolina's removal of the statewide cap on charter schools in 2011. We assemble a unique dataset that combines student-level administrative data with novel information about the educational programs of entering charter schools. The student-level records contains students' performance on end-of-grade standardized exams as well as geocoded residential addresses, which are important for our research design. We use the educational program information, collected from the schools' applications to the State Board of Education, to categorize each charter school as either horizontally or non-horizontally differentiated from public education. We classify as horizontally differentiated charter schools that emphasize project-based or experiential learning (including Montessori) in their application, whereas non-horizontally differentiated schools include those focused on core skills and/or using traditional instruction (including charter schools that following "No Excuses" practices).

The difference-in-differences research design that we implement combines the timing of the policy change with the distances between students' *pre-policy-change* residence and the new charter schools that opened following the removal of the cap. This information allows

us to compare the test score changes of students who lived near the new charters prior to the policy change with those for students who lived further away to identify the aggregate effect of charter expansion. Importantly, we apply this approach to estimate separate effects for students exposed to entry by horizontally differentiated charter and for those students exposed to entry by non-horizontally differentiated charter schools irrespective of the students' ex-post schooling choices.

The results indicate that students ultimately exposed to charter school entry following the policy change experienced an average improvement in standardized math test scores of 0.03 standard deviations, which is driven entirely by non-horizontally differentiated charter schools: we find that the causal effect of non-horizontally differentiated charter school expansion is 0.05 standard deviations while the expansion of horizontally differentiated charter schools instead has no effect on student test scores. These findings are robust to several robustness and validity checks, such as student fixed effects and neighborhood-level trends designed to rule out student sorting and strategic charter school location as confounders. Our results are also robust to alternative definitions of exposure to charter school expansion.

Our findings are important for evaluating the expansion of school choice policies and of charter schools in particular. When considering whether to allow expansion in a particular neighborhood, policymakers will want to know how all students in that area are likely to be affected, regardless of whether students remain in public schools or switch to a new charter school. For students exposed to charter entry, we find gains that are driven entirely by charter schools that are non-horizontally differentiated in their educational program. Notably, we identify charter schools' type from information contained on their application, suggesting that an applicant's likelihood of creating competitive externalities on educational quality can potentially be screened by authorizers and policymakers.

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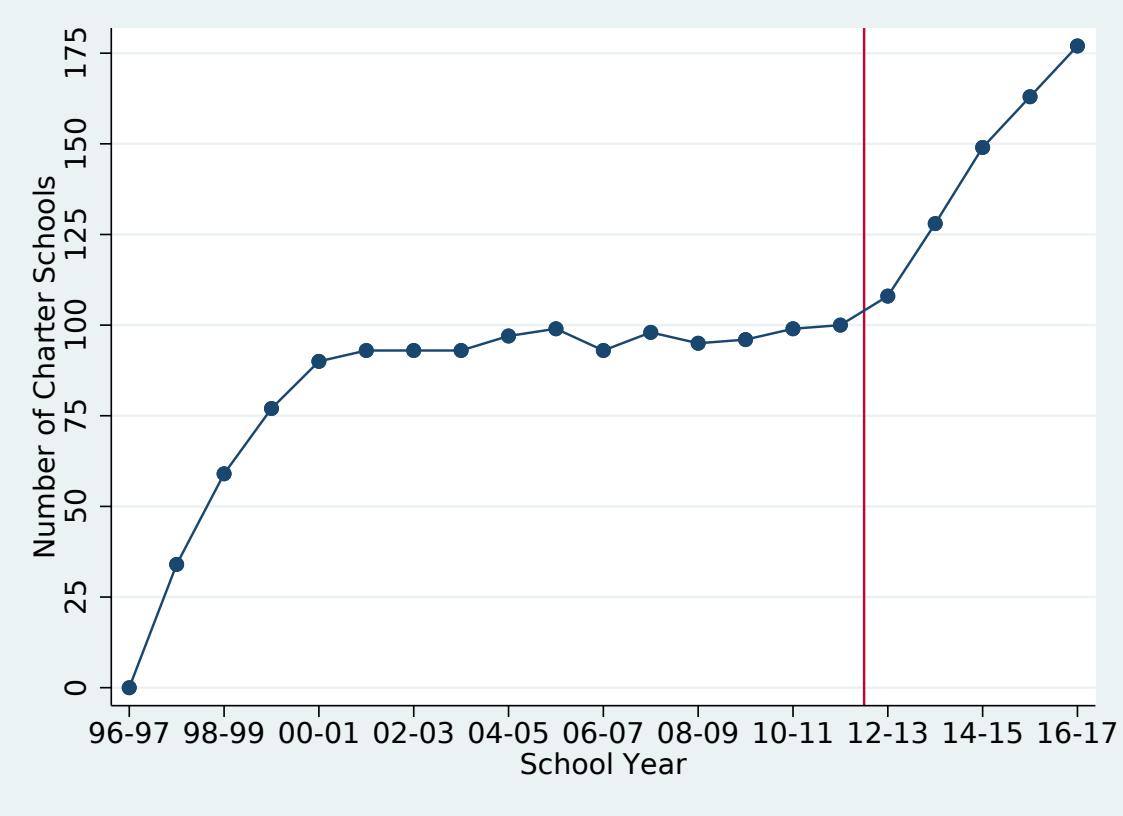
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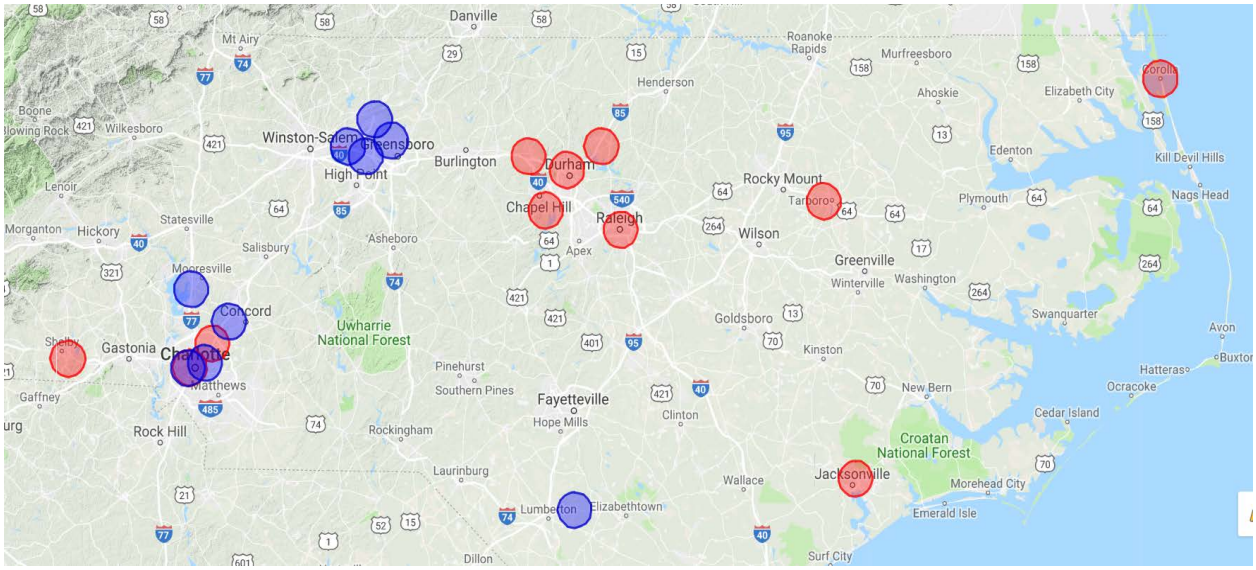
Figures

Figure 1: Number of Charter Schools in North Carolina by Year



Notes: This figure displays the number of charter schools by year in North Carolina from 1996-97 to 2016-17, excluding two virtual charter schools that opened in 2015-16. The vertical line represents the lifting of the 100 school charter cap for the 2012-13 school year.

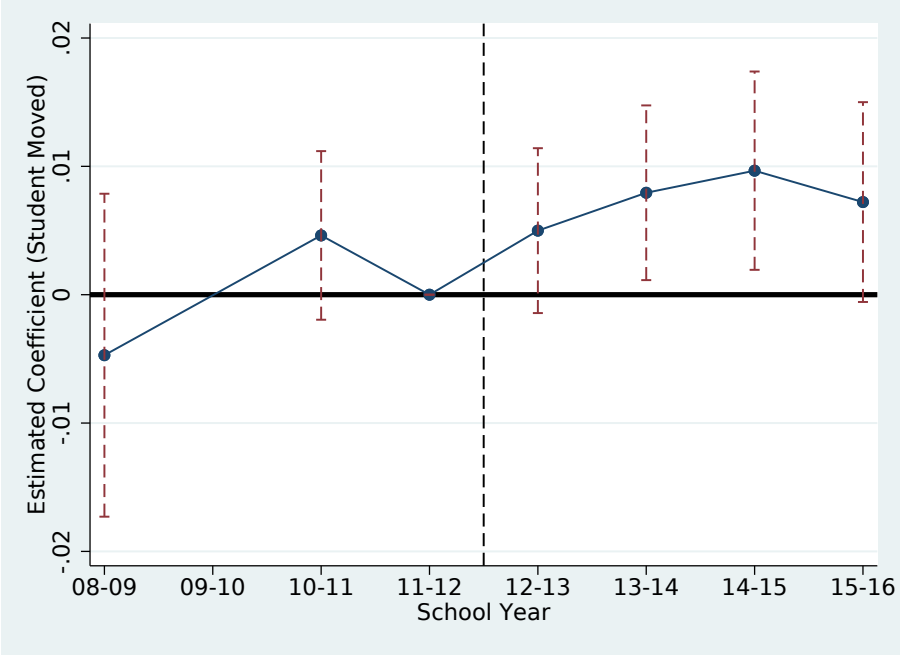
Figure 2: Locations of Charter Schools Opening in 2012-13 or 2013-14



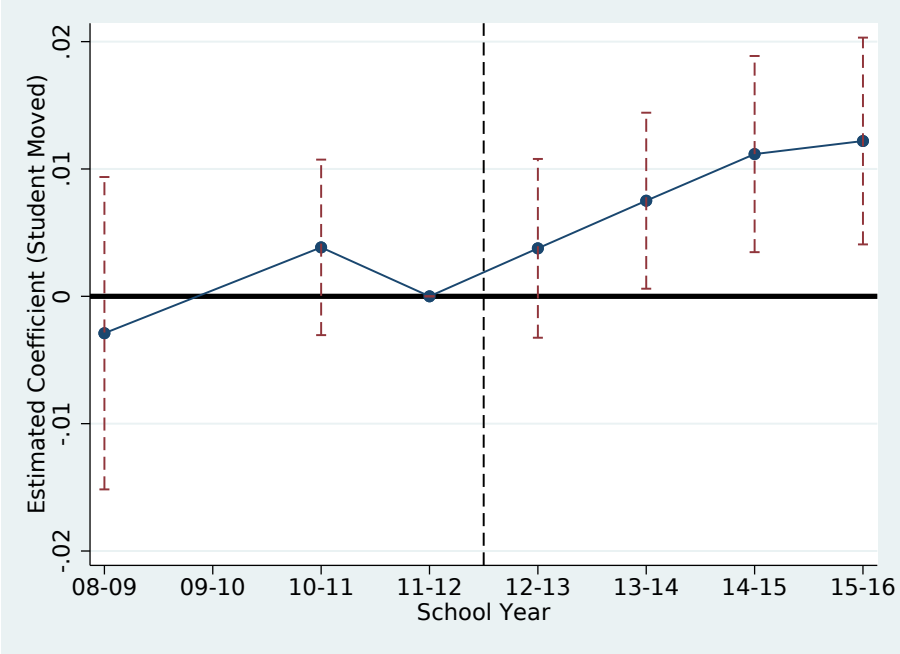
Notes: This figure draws a 5 mile circle around the 23 charter schools in our data that opened in the 2012-13 or 2013-14 school year. Blue circles represent that the charter is non-horizontally differentiated from the local public school while red circles represent that the charter is horizontally differentiated from the local public school (as described in Section 2.2). Only students residing within these circles will be included in our main specifications. For students residing in regions where the circles intersect, the student will be assigned to the nearest charter school so that no student is double counted in our regressions. Our main results are robust to dropping observations where these circles intersect.

Figure 3: Robustness: Difference-in-Differences Results for Moving

((a)) Non-Horizontally Differentiated



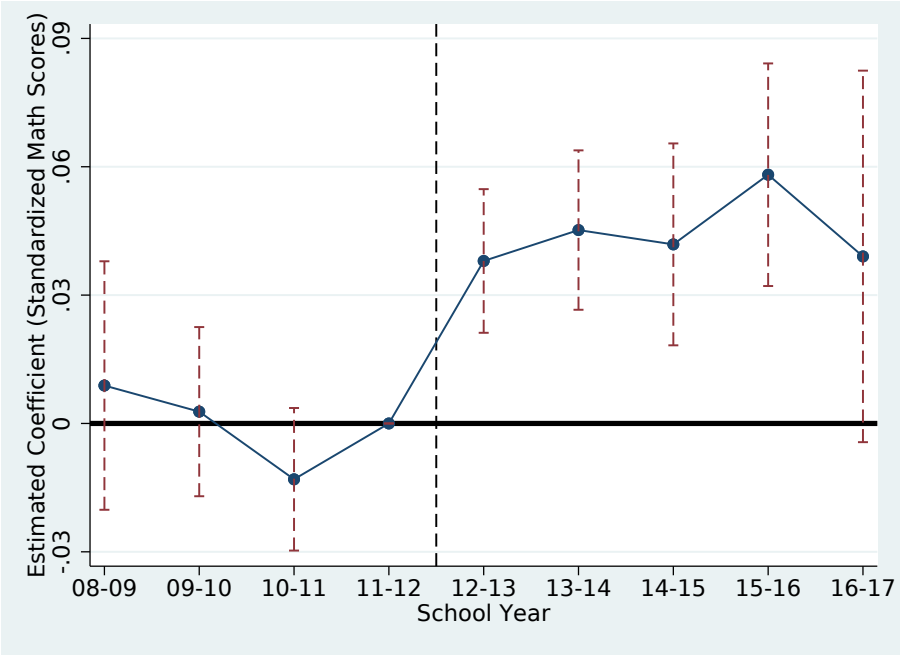
((b)) Horizontally Differentiated



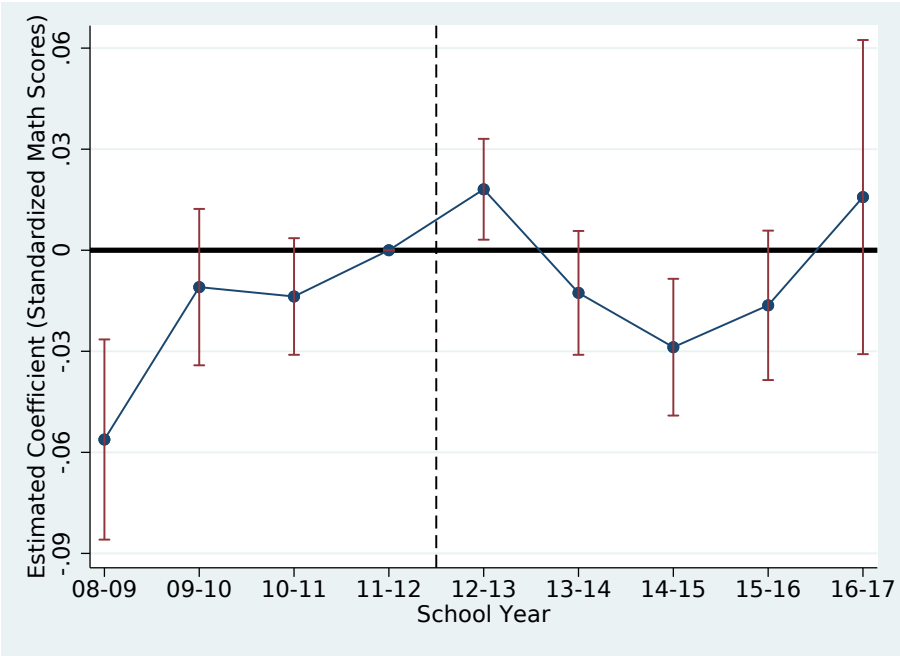
Notes: This figure shows the estimated difference in moving rates for students in a region that is 'treated' by a newly-opened charter relative to a 'control' region as described in equation 4.1. A student is defined as having moved if they: (i) move a distance greater than 1 mile, and (ii) exit their previous treated/control region. Treated areas are defined as neighborhoods within 5 miles of a charter school that opened in 2012-13 or 2013-14. Control areas are defined as neighborhoods between 5 and 10 miles of a charter schools that opened in 2012-13 or 2013-14. Results are subdivided by whether the nearby charter was horizontally differentiated or not from the local public school as described in Section 2.2. School year 2009-10 is omitted due to the change from the 2000 to 2010 census subdivisions created artificially high moving rates that year. Note that 2012-13 is considered the first 'treated' year because although the charters themselves opened in either the 2012-13 or 2013-14 school year, public schools would have known by the start of 2012-13 whether or not a charter was opening nearby or would open nearby in 2013-14. The dashed vertical line therefore separates the 'pre-years' from the 'post-years'. The horizontal line represents a point estimate of zero. The dashed 'whiskers' represent 95 percent confidence intervals with standard errors clustered at the census block group level.

Figure 4: Difference-in-Differences Results by Year and Charter Type

((a)) Non-Horizontally Differentiated



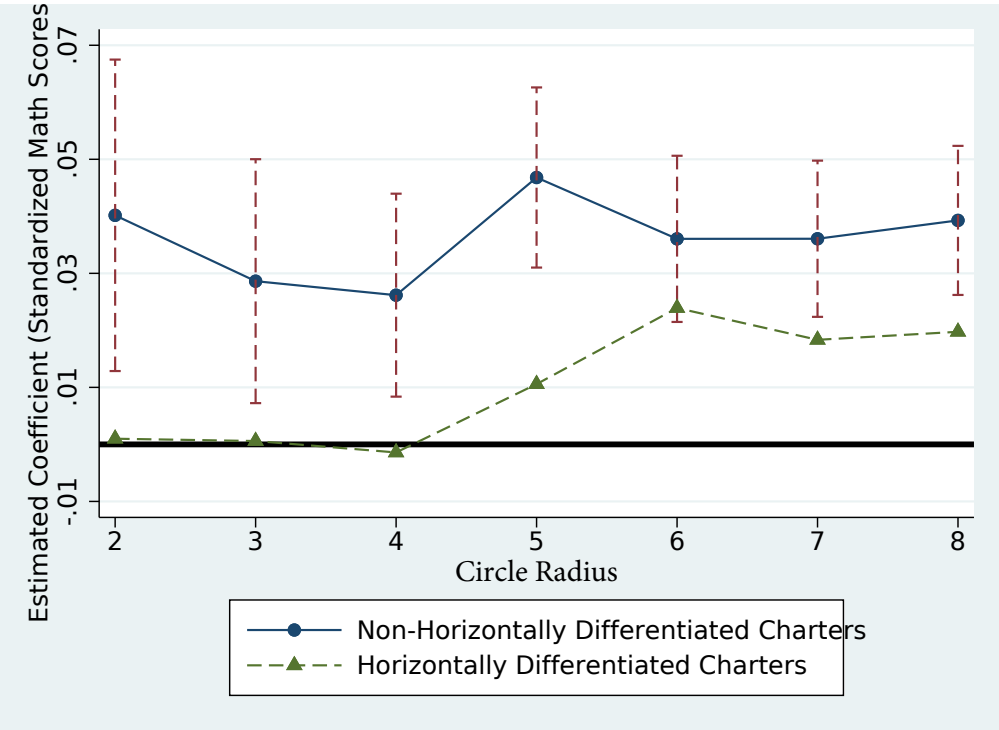
((b)) Horizontally Differentiated



Notes: This figure shows the estimated difference between student 'treated' by a newly-opened charter relative to 'control' students by year as described in equation (4.2). Treated students are defined as students living within 5 miles of a charter school that opened in the 2012-13 or 2013-14. Control students are defined as students living between 5 and 10 miles of a charter schools that opened in the 2012-13 or 2013-14. Results are subdivided by whether the nearby charter was horizontally differentiated or not from the local public school as described in Section 2.2. Note that 2012-13 is considered the first 'treated' year because although the charters themselves opened in either the 2012-13 or 2013-14 school year, public schools would have known by the start of 2012-13 whether or not a charter was opening nearby or would open nearby in 2013-14. The dashed vertical line therefore separates the 'pre-years' from the 'post-years'. The horizontal line represents a point estimate of zero. Demographic controls along with grade and year fixed effects are included. The dashed 'whiskers' represent 95 percent confidence intervals with standard errors clustered at the census block group level.

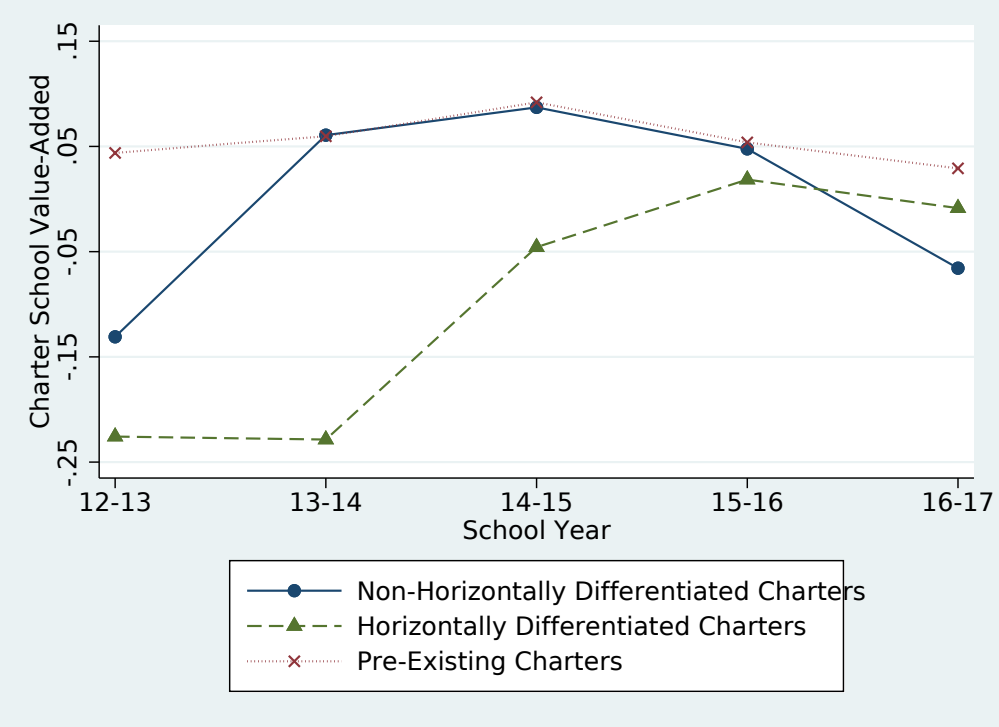
Figure 5: Robustness: Difference-in-Differences Results by Charter Type for Different Treatment Definitions

((a)) Non-Horizontally Differentiated



Notes: This figure shows sensitivity of our main result in equation (3.3) to the definition of the 'treated' and 'control' students by showing estimated effect for horizontally and non-horizontally differentiated charter for various 'circle' sizes. Specifically, a circle a diameter d will consider all student whose residential distance to the newly-opened charter in 2011-12 is between 0 and d miles as treated, while considering all students who live between d miles and $2d$ miles as control. The horizontal line represents a point estimate of zero. Demographic controls along with grade and year fixed effects are included. The dashed 'whiskers' on the point estimates for the non-horizontally differentiated charters represent 90 percent confidence intervals with standard errors clustered at the school level.

Figure 6: Vertical Differentiation by Charter Type



Notes: This figure shows value-added of charters by whether the charter was horizontally or non-horizontally differentiated. It is also shown for charters that were present in North Carolina before 2012-13. Value-added is defined as the school-year fixed effect in a regression of (grade-year) standardized math test scores on cubic controls for prior year math and English test scores as well as demographic controls and grade and year fixed effects. The regression includes all North Carolina grade 4-8 students with prior test scores. Demographic controls include ethnicity, gender, limited English proficiency status, free and reduced price lunch status, gifted status, disability designation and an indicator if the student is repeating or skipping a grade.

Tables

Table 1: Summary Statistics

		Students within 5 miles of:		
	All Students	Newly-Opened Charters	Non-Horizontally Differentiated	Horizontally Differentiated
	(1)	(2)	(3)	(4)
Percent White	51.8	35.9	40.0	31.7
Percent Black	26.2	39.4	35.3	43.7
Percent Hispanic	13.9	16.7	16.2	17.4
Percent Disadvantaged	51.9	55.8	53.7	58.0
Value-Added (charters)	-0.027	0.081	0.088	0.074
Value-Added (assigned public)	0.012	0.002	-0.013	0.018
Observations (# students)	6,962,804	649,573	334,097	315,476
Observations (# schools)	168	23	10	13

Notes: The value-added of the charters is weighted by the number of students within five miles of the charter.

Table 2: Proportion of Public-Charter Switchers Within Distance Bands to Newly-Opened Charters

Proportion of Public-Charter Switchers Between:

Charter Type	0-2 miles	2-4 miles	4-6 miles	6-8 miles	8-10 miles	10-15 miles
	(1)	(2)	(3)	(4)	(5)	(6)
All Newly-Opened Charters	2.68	1.85	1.39	1.00	0.86	0.23
Non-Horizontally Differentiated	2.76	1.91	1.46	0.96	0.94	0.26
Horizontally Differentiated	2.63	1.82	1.35	1.02	0.80	0.21
Observations (Non-Horizontally)	4,513	10,011	12,267	9,761	9,583	18,820
Observations (Horizontally)	6,948	20,477	20,762	18,046	14,008	30,662

Notes: This table shows the proportion of students in the 2013-14 school year whose 2011-12 residence is within a given distance band of charter schools that opened in the 2012-13 and 2013-14 school years and who switched from a public school to a newly-opened charter school. The data is then further subdivided into students within the distance band of non-horizontally and horizontally differentiated charter schools. Due to data constraints (see Section 2.2), we do not observe residential addresses for students that attend charter schools. Therefore, the sample in this table is restricted to charter school attendees in the 2013-14 school year who attended a *public school* in the 2011-12 school year. This data may therefore not be representative of the general population of charter school attendees.

Table 3: Difference-in-Differences Results

Mathematics Test Scores

	'Treated' (0-5 miles) vs. 'Control' (5-10 miles)				
	(1)	(2)	(3)	(4)	(5)
All Newly-Opened Charters	0.034***	0.031***	0.038***	0.022***	0.021***
	(0.011)	(0.007)	(0.008)	(0.006)	(0.005)
Non-Horizontally Differentiated	0.063***	0.047***	0.065***	0.035***	0.031***
$(\beta_h + \beta_{nh-h})$	(0.015)	(0.010)	(0.012)	(0.009)	(0.008)
Horizontally Differentiated	-0.003	0.011	0.004	0.007	0.010
(β_h)	(0.015)	(0.010)	(0.011)	(0.008)	(0.007)
Demographic Controls	No	Yes	Yes	Yes	Yes
Student Fixed Effects	No	No	Yes	No	Yes
Census Tract Time Trends (linear)	No	No	No	Yes	Yes
Observations (student-year)	1,110,883	1,036,066	1,036,066	1,036,066	1,036,066

Notes: This table shows difference-in-differences estimates from equation (3.3), whereby students living within 5 miles of a newly-opened charter school are considered 'treated' while those living 5-10 miles from a newly-opened charter are considered 'control' and the effect is allow to differ by whether the newly-opened charter school is horizontally differentiated or not from the local public school as described by Section 2.2. About 55 percent of total observations come from non-horizontally differentiated charters with the remaining 45 percent of observations coming from horizontally differentiated charters. Each column represents a different regression and all regressions include grade and year fixed effects. Demographic controls include ethnicity, gender, limited English proficiency status, free and reduced price lunch status, gifted status, disability designation and an indicator if the student is repeating or skipping a grade. Standard errors are clustered at the 2011-12 census block group level. ***,** and * denote significance at the 1%, 5% and 10% levels, respectively.

Table 4: Difference-in-Differences Results

Mathematics Test Scores

	Continuous Treatment (restricted to $\leq$ 10 miles)				
	(1)	(2)	(3)	(4)	(5)
All Newly-Opened Charters	-0.0040***	-0.0036***	-0.0043***	-0.0019**	-0.0022***
	(0.0013)	(0.0008)	(0.0010)	(0.0008)	(0.0007)
Non-Horizontally Differentiated	-0.0093***	-0.0058***	-0.0084***	-0.0045***	-0.0039***
$(\beta_h + \beta_{nh-h})$	(0.0018)	(0.0012)	(0.0016)	(0.0012)	(0.0010)
Horizontally Differentiated	0.0009	-0.0014	0.0000	0.0006	-0.0004
(β_h)	(0.0017)	(0.0011)	(0.0013)	(0.0011)	(0.0008)
Demographic Controls	No	Yes	Yes	Yes	Yes
Student Fixed Effects	No	No	Yes	No	Yes
Census Tract Time Trends (linear)	No	No	No	Yes	Yes
Observations (All Charters)	1,110,883	1,036,066	1,036,066	1,036,066	1,036,066

Notes: This table shows difference-in-differences estimates using distance to newly-opened charter as a continuous differencing variable. The data is restricted to less than 10 miles for comparability to Table 3. The results are further subdivided by whether the newly-opened charter school is horizontally differentiated or not from the local public school as described by Section 2.2. About 55 percent of total observations come from non-horizontally differentiated charters with the remaining 45 percent of observations coming from horizontally differentiated charters. Each column represents a different regression and all regressions include grade and year fixed effects. Demographic controls include ethnicity, gender, limited English proficiency status, free and reduced price lunch status, gifted status, disability designation and an indicator if the student is repeating or skipping a grade. Test score controls are cubic controls for 2011-12 math and English test scores. Standard errors are clustered at the 2011-12 census block group level. ***,** and * denote significance at the 1%, 5% and 10% levels, respectively.

Table 5: Difference-in-Differences Results with Vertical Differentiation

Mathematics Test Scores

	'Treated' (0-5 miles) vs. 'Control' (5-10 miles)					
	(1)	(2)	(3)	(4)	(5)	(6)
Horizontally Differentiated	0.011	0.014	0.022**	0.010	0.010	0.011
(β_h)	(0.010)	(0.010)	(0.010)	(0.007)	(0.007)	(0.007)
Non-Horizontally Differentiated	0.047***	0.042***	0.044***	0.031***	0.031***	0.031***
$(\beta_h + \beta_{nh-h})$	(0.010)	(0.010)	(0.010)	(0.008)	(0.008)	(0.008)
Charter VA	-	0.096	-0.234*	-	-0.023	-0.076
(ν_h^4)		(0.094)	(0.139)		(0.045)	(0.059)
Charter VA*Non-Horizontally Differentiated	-	-	0.503***	-	-	0.042
$(\nu_h^4 + \nu_{nh-h}^4)$			(0.119)			(0.069)
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Student FEs & Census Tract Trends	No	No	No	Yes	Yes	Yes
Observations (student-year)	1,036,066	1,036,066	1,036,066	1,036,066	1,036,066	1,036,066

Notes: This table shows difference-in-differences estimates controlling for vertical differentiation as described by (4.3).'Charter VA' refers to the value-added of the newly-opened charter school. Value-added is defined as the school fixed effect in a regression of (grade-year) standardized math test scores on cubic controls for prior year math and English test scores as well as demographic controls and grade and year fixed effects. The regression includes all North Carolina grade 4-8 students with prior test scores. Each column represents a separate regression. Columns (1) and (4) are provided for reference and are identical to columns (2) and (5) in Table 3, respectively. Demographic controls include ethnicity, gender, limited English proficiency status, free and reduced price lunch status, gifted status, disability designation and an indicator if the student is repeating or skipping a grade. Standard errors are clustered at the 2011-12 census block group level. ***,** and * denote significance at the 1%, 5% and 10% levels, respectively.

.1 Assigning Treatment Status

A key variable necessary to our analysis is the distance of each student's residence to a given newly-opened charter in the 2011-12 school year. To construct this variable, we first start with the course membership files, which gives us school and grade of enrollment for every student in grade K-7 attending a North Carolina public school (including charter schools) in the 2011-12 school year. We then merge that data to student residential location data from the North Carolina Education Data Research Center (NCERDC), giving us the census block group of residence for every student in a North Carolina public school in 2011-12. Unfortunately, the residential data is not available for students attending charter schools in 2011-12, and so they will not be included in our sample.[25] Our match rate is 87 percent.[26]

In the next step, we use the cartographic boundary shapefiles for U.S. Census block groups according to both the 2000 and 2010 boundary definitions[27] and get the longitude and latitude of the centroid of that block group. That centroid is then assigned as the residential location for each student. To give a sense of the sparsity of the residential data, North Carolina is divided into 6,183 Census block groups with an average population of 1,546 individuals (usually ranging between 600 to 3,000 individuals) and an average size of about 8 square miles (usually ranging between 0.5 to 300 square miles).

From there, we use STATA to calculate the distance from the centroid of each student's census block group to the latitude and longitude of the nearest newly-opened charter (see .2 for list of newly-opened charters and their latitudes and longitudes). We drop about 2,700 students (representing about 0.2 percent of the sample) with multiple locations per year as it is unclear to which location they should be assigned. From here, the main treatment status of each student in our analysis is easily defined: a student is assigned a value of one if the

[25]This is a general data limitations we face: residential location data is never reported for students attending charter schools.

[26]In 2010, the reported match rate for public school students for the NCERDC was 96 percent, however about six percent were only matched to higher levels of census divisions (e.g., census tract). Given that about 3 percent of students attend charter schools in 2010, our match rate of 87 is exactly the rate we would expect from NCERDC data.

[27]Available at `https://www.census.gov/geo/maps-data/data/cbf/cbf_blkgrp.html`.

student's residential census block group centroid is within 5 miles of a newly-opened charter and a value of zero if the student's residential census block group centroid is between 5 ad 10 miles of a newly-opened charter. The student is never double-counted, with treatment assignment always done relative to the *closest* newly-opened charter school them. Given that only X newly-opened charters have their treatment determining circles cross, [XX:describe circle crossings. Say results are similar if these crossing regions are omitted.].

.2 Assigning Horizontal Differentiation

Every charter school in North Carolina is given its own Local Education Area (LEA) code which uniquely identifies it. The first two characters of the code are numbers, which link it to the public school LEA wherein it locates. The last character of the code is a letter, which allows the charter school to be uniquely identified.

There were 9 'fast track' applications to open for the 2012-13 school year. Of those, we drop 3 schools (2 for being high schools, one for never opening)[28] leaving us with a sample of 6 schools opening in 2012-13.

While X schools opened in 2012-13, X of those school were high schools which we do not have clearly defined test score data for and are thus dropped. Similarly, X schools in 2013-14 were approved with, X of those schools being high schools, leaving us with X newly-opened 2013-14 charter schools that we analyze.

School Name (lea code)	Opening School Year	(Latitude, Longitude)	Horizontal Differentiation	Reas
Cabarrus Charter Academy (13B)	2013-14	(35.4104, -80.6691)	Non-horizontally differentiated	
Willow Oak Montessori (19C)	2013-14	(35.855, -79.0253)	Horizontally differentiated	'Montessori' in nar
Pinnacle Classical Academy (23A)	2013-14	(35.2611, -81.5043)	Horizontally differentiated	'Classical' in name
STEM Education for a Global Society Academy (24C)[1]	2013-14	(34.3127, -78.2063)	Horizontally differentiated	
Waters Edge Village School (27A)	2012-13	(36.37826, -75.832041)	Horizontally differentiated	
The Institute for the Development of Young Leaders (32P)	2013-14	(36.0163, -78.9139)	Horizontally differentiated	

[28]The two high schools were Bear Grass Charter and Research Triangle High, while the approved charter of The Howard and Lillian Lee School never opened.

North East Carolina Preparatory School (33A)	2012-13	(35.891794, -77.58057)	Horizontally differentiated	
The North Carolina Leadership Academy (34H)	2013-14	(36.1099, -80.0515)	Non-horizontally differentiated	
Falls Lake Academy (39A)	2013-14	(36.1104, -78.7351)	Horizontally differentiated	
Cornerstone Academy (41G)	2012-13	(36.13432, -79.827041)	Non-horizontally differentiated	
The College Prep and Leadership Academy of High Point (41H)	2012-13	(36.070916, -79.959375)	Non-horizontally differentiated	
Summerfield Charter Academy (41J)	2013-14	(36.2179, -79.9124)	Non-horizontally differentiated	
Langtree Charter Academy (49F)	2013-14	(35.5413, -80.8652)	Non-horizontally differentiated	
Corvian Community School (60M)	2012-13	(35.32301, -80.756351)	Horizontally differentiated	
Aristotle Preparatory Academy (60N)	2013-14	(35.2246, -80.8819)	Non-horizontally differentiated	
Charlotte Choice Charter (60P)	2013-14	(35.2441, -80.7949)	Non-horizontally differentiated	
Invest Collegiate Transform (60Q)	2013-14	(35.2254, -80.8732)	Horizontally differentiated	
Douglass Academy (65C)[2]	2013-14	(34.242, -77.9434)	Non-horizontally differentiated	
Island Montessori Charter (65D)	2013-14	(34.1079, -77.8985)	Horizontally differentiated	'Montessori' in nar
ZECA School of Arts and Technology (67B)	2013-14	(34.7791, -77.4152)	Horizontally differentiated	
The Expedition School (68C)[2]	2013-14	(36.07067, -79.113701)	Horizontally differentiated	
Southeastern Academy (78B)	2013-14	(34.6517, -78.8738)	Non-horizontally differentiated	
Triangle Math and Science Academy (92T)	2012-13	(35.77853, -78.635361)	Horizontally differentiated	

[1]This school closed at the end of the 2014-15 school year.
[2]This school did not seem to open until 2014-15.