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Horizontal Differentiation and the Policy Effect of Charter Schools

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While school choice may enhance competition, incentives for public schools to raise productivity may be muted if public education is viewed as imperfectly substitutable with alternatives. This paper estimates the aggregate 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 treatment effects for exposure to charter schools that are and are not differentiated horizontally from public school instruction. The results indicate learning gains for treated students that are driven entirely by non-horizontally differentiated charter schools: we find that non-horizontally differentiated charter school expansion causes a 0.05 SD increase in math scores. These learning gains are driven by public schools responding to increased competition.

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Horizontal Differentiation and the Policy Effect of Charter Schools*

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Abstract

While school choice may enhance competition, incentives for public schools to raise productivity may be muted if public education is imperfectly substitutable with alternatives. This paper estimates the aggregate effect of charter school expansion on education quality while accounting for the horizontal differentiation of charter programs. Our research design leverages variation following the 2011 removal of North Carolina's statewide cap to compare test score changes for students who lived near entering charters with the changes of students who lived farther away. We find learning gains that are driven by public schools responding to increased competition from non-horizontally differentiated charter schools.

Keywords: Charter schools, school choice, competitive effects, horizontal differentiation, strategic differentiation

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1 Introduction

School choice policies provide parents and students with schooling alternatives 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 charter schools can improve student learning and later-life outcomes.[1] 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, 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). Funding for public 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 view schools as homogeneous, save for productivity differences, and choose among alternatives accordingly. However, schools may strategically differentiate through product choice (MacLeod and Urquiola, 2013). Evidence

[1] This literature has focused on oversubscribed charters – often located in urban areas – as this is a necessary condition for the lottery design. Angrist, Pathak, and Walters (2013) and Place and Gleason (2019) find that charter schools in non-urban areas do not improve student achievement and suggest that there is substantial heterogeneity in the effect of attending a charter school. See Chabrier, Cohodes, and Oreopoulos (2016) for a more detailed up-to-date review and contextualization of the results from charter school lotteries.

from a variety of contexts indicates that parents and students view schools as differentiated products[2] and select schools based on idiosyncratic match (Hastings, Kane, and Staiger, 2006; Walters, 2018).[3] An important feature of charter schools is their autonomy to develop and implement alternative learning programs, such as Montessori, experiential and project-based learning, as well as language immersion, arts and sports-based curricula. To the degree that households view traditional public school education as imperfectly substitutable with such programs, competitive incentives for public schools to increase productivity may in turn be muted.

In this paper, we examine the role of curriculum choice by charter schools for evaluating the effects of charter school expansion on student achievement. To do so, we propose and implement an empirical strategy that leverages variation following North Carolina's Race to the Top-initiated removal of the statewide cap on charter schools in 2011. Our approach, a difference-in-differences design, does *not* require separately estimating the effects of expansion on charter and traditional public school students and is facilitated by a unique dataset that combines student-level administrative records from North Carolina with novel information about charter schools' educational programs.

The dataset that we assemble links measures of student learning in North Carolina with exposure to charter school entry following the cap removal. 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 each 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

[2]See, for instance, Bayer, Ferreira, and McMillan (2007); Burgess, Greaves, Vignoles, and Wilson (2015); Arcidiacono, Muralidharan, Shim, and Singleton (2017).

[3]Evidence for school sorting on learning impacts or effectiveness, such as captured by measures of school value-added, is also limited (e.g., Hsieh and Urquiola 2006; Rothstein 2006; Abdulkadiroglu, Pathak, Schellenberg, and Walters 2017).

skills through traditional instruction. This classification allows us to account for 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' residences *pre-policy-change* and new charter schools that opened following the removal of the cap. Our difference-in-differences approach then identifies the aggregate or 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 those exposed to entry by non-horizontally differentiated charter schools irrespective of whether the students switched into a charter school or remained in public schools. By remaining agnostic about students' ex-post schooling choices, our research design relies on weaker assumptions about student sorting than strategies used in prior work. In this vein, our approach is similar in spirit to that in Hsieh and Urquiola (2006), who estimate the aggregate effect of voucher-driven private school expansion in Chile.

We find that students 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 important heterogeneity by charter school type: while the causal effect of non-horizontally differentiated charter school expansion is 0.05 standard deviations, the expansion of charter schools that are horizontally differentiated in their curricula has no effect on student test scores. We subject these findings to several robustness checks, which demonstrate that our results are not driven by either student sorting across neighborhoods in response to (or in anticipation of) the policy change or by strategic charter school location decisions based on neighborhood trends. Further, these findings 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 public school quality and the gains caused by non-horizontally differentiated charter school expansion accruing via the competitive channel. We quantify the importance of the direct effect of charter expansion for the overall impact in two ways. First, to isolate the influence of the indirect channel on the aggregate impact, we counterfactually assign zero test score growth to students who switch to charters and re-estimate the effect. This exercise produces an effect of expansion in line with our main estimate. Second, we calculate value-added (in terms of student test scores) for individual charter schools. We show that, while non-horizontally differentiated charter schools have higher value-added on average, their aggregate impact comes almost entirely through the competitive channel as few students in our sample switch to charter schools. In addition, we show that vertical quality differentiation across charter school entrants, as captured by differences in value-added, is unable to account for the results. We also rule out changing peer quality at traditional public schools as a driver of their improvement.[4]

This paper connects with a growing empirical literature that examines competition in education markets (e.g., Hoxby 2000). 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, while Neilson (2017) identifies quality adjustment as the primary source of gains from a targeted voucher in Chile. In contrast, whether charter schools in the U.S. induce competitive test score responses from traditional public schools remains an unsettled question, with mixed findings in the prior literature (see Epple et al. 2016 for a recent review). Our results suggest that this ambiguity stems in part from neglecting important differences among charter schools. Moreover, the literature has largely focused on the quality and location dimensions of schools.[5] Our findings, however,

[4]Students switching into non-horizontally and horizontally differentiated charter schools are positively selected relative to students who stay in traditional public schools. Therefore, charter entry *worsens* peer quality in public schools, which would act to attenuate student learning gains (Hsieh and Urquiola, 2006), although we expect this attenuation to be limited given the small number of students who leave traditional public schools (see Section 5 for more details).

[5]A notable exception is Bau (2017), who examines how schools in Pakistan competitively tailor material to different student populations.

underscore that strategic differentiation of educational programs by schools is equally a key empirical feature of education markets.

Our findings are thus important for evaluating the expansion of school choice policies and of charter schools in particular. When considering whether to allow expansion of choice, policymakers will want to know how all students 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 exposure to charter schools that are not horizontally differentiated in their educational program. In identifying the importance of heterogeneity among charter schools, our findings thus complement prior work that has emphasized the effectiveness of "No Excuses" charter operators (e.g., Angrist et al. 2012, 2013; Dobbie and Fryer 2013) and the equilibrium implications of behavioral differences across charter school types (Singleton, forthcoming). An important finding, therefore, is that the direct and competitive channels of charter school expansion appear to be complementary: the schools we identify as non-horizontally differentiated, a number of which follow No Excuses-type practices, are also higher value-added, on average.

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 main results including robustness checks in Section 4 before examining the interpretation of our findings in Section 5. Section 6 concludes.

2 Background and Data

North Carolina lifted its statewide cap on the number of charter schools in the state on June 6th, 2011. Figure 1 displays the number of charters in North Carolina for school years 1996-97 through 2016-17. As shown in the figure, North Carolina went from no charter schools to just shy of 100 total – the limit since the 1996 legislation that authorized charter schools in the state – by 2000-01. The number of charter schools in the state then remained stable for the next decade (with only minor fluctuations due to a few closures). Rapid expansion came in 2012-13 when the charter school cap was removed: Nine charter schools opened for the 2012-13 school year, with another twenty-three approvals following in 2013-14. By 2016-17, 176 charter schools were in operation in North Carolina. Unlike similar policy changes spurred by Race to the Top, North Carolina's expansion applied to all school districts statewide and did not explicitly favor "high-performing" charter operators.[6]

In this paper, we use the policy variation from the removal of the cap to estimate the aggregate effect of charter school expansion. This represents the combined influence of two channels: First, 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; Abdulkadiroglu et al., 2011; Angrist et al., 2016; Dobbie and Fryer Jr, 2015).[7] These gains are pronounced at "No Excuses" charter schools (Angrist et al., 2013; Dobbie and Fryer, 2013), 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

[6]By contrast, the 2011 Massachusetts charter school expansion, analyzed by Cohodes et al. (2019) and Ridley and Terrier (2018), was restricted to under-performing districts, including Boston, and "proven" – frequently "No Excuses" – charter school providers. In addition, North Carolina features a relatively small presence of charter management organizations, especially compared to widely-studied states such as New York or Massachusetts.

[7]Other work uses longitudinal variation in administrative datasets, finding more mixed results (Sass, 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. (2016) for a recent review.

the quality of education across the board (Hoxby, 2002). This expectation, which motivates a large empirical literature, may be confounded by frictions in education markets, however. For example, MacLeod and Urquiola (2015) present a model in which, consistent with empirical findings (e.g., Rothstein 2006; Abdulkadiroglu et al. 2017), parents and students choose schools based on reputation (a function of selectivity and peer quality), weakening incentives for schools to compete on quality. Similarly, McMillan (2004) shows that, in the presence of household heterogeneity and demand spillovers, competition can perversely lead public schools to lower productivity. Suboptimal outcomes theoretically may also arise because schools strategically differentiate through product choice (Hotelling, 1929; Dixit and Stiglitz, 1977). As MacLeod and Urquiola (2013) discuss, "some schools [...] emphasize sports, while others focus on academics or music."[8] Often an explicit policy motivation for school choice – e.g., the North Carolina General Statutes currently list encouraging "different and innovative teaching methods" as one purpose of charters – such horizontal differentiation is also likely to soften competitive incentives.

Prior findings regarding the effects of charter schools on public school students tend to be mixed or contradictory: Sass (2006), Booker et al. (2008), Winters (2012), Cordes (2018) and Ridley and Terrier (2018) report positive effects; Bettinger (2005), Bifulco and Ladd (2006), and Zimmer and Buddin (2009) do not find any evidence of competitive gains; and Imberman (2011b), who uses an IV strategy to overcome endogenous charter location, finds mixed or even negative effects. As highlighted above, the curricular heterogeneity that results from charter schools strategically differentiating from traditional public education – an aspect neglected by the prior work – may be equally as important for the competitive channel as it is for the direct one and may partly explain the ambiguous conclusions in the prior literature.[9] Below, we formalize this intuition in a simple model that serves to motivate

[8] In a similar vein, Harris and Larsen (2019) use family rankings of schools in New Orleans to show that families prefer not just schools with higher school value-added, but also those with more extracurricular activities.

[9] Ferreyra and Kosenok (2018) consider charter schools' program focus as a determinant of household demand, but do not analyze the implications for public school responses to competitive pressure from charters, the focus of our work.

our subsequent empirical analysis.

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 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, which is fundamentally determined by the beliefs and preferences of parents, 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 one.[10] An immediate implication of this setup is that the public school would set quality at zero absent competition from the charter school.

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

The first-order condition of the public school's 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 – as perceived by parents and households – 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 entry 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 miss important heterogeneity in competitive responses.

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 that we gather 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 2009-10 to 2014-15 school years. The data contain test scores for each student in mathematics and reading on standardized end-of-grade exams in grades three through five, 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. We standardize this scale at the student level to have a mean of zero and a variance of one for each grade-year to ensure comparability of test scores across grades. In addition to test scores, the student data contain information regarding each student's grade, socioeconomic status, ethnicity, and gifted or special education status.[11]

In addition, we obtain information regarding students' residential locations in each school-year from the NCERDC. As we detail in the next section, this information is necessary for implementing 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.[12] For confidentiality reasons, student location in the NCERDC data is reported at the Census block group level. We therefore define each student's location as the centroid of the block group in which he or she resides.[13] We restrict our dataset to students for whom we observe a valid test score both before and after the 2012-13 school year so that we observe at least one pre- and post-reform observation for each student. We are left with a sample of 1,117,142 student-year observations which tracks 285,601 students from 2009-10 through 2014-15.

[11]We also gather data for whether a student is repeating or skipping a grade.
[12]Residential information for students in charter schools is not contained in the NCERDC data.
[13]The median area of a Census block group in North Carolina is 2.2 square miles.

We combine the student-level records with information about the educational program of each charter school. Following the lifting of the statewide cap in 2011, prospective charter schools submitted applications to the Charter Schools Advisory Board. Each application contains detailed, mandatory information about the 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 charter schools that emphasize project-based or experiential learning (including Montessori) in their application as horizontally differentiated. Charters are otherwise classified as not horizontally differentiated. Non-horizontally differentiated schools therefore include those focused on core skills and/or using traditional instruction.[14] We examine differences between horizontally and non-horizontally differentiated charters in the next subsection and present a more detailed description of our classifying methodology in Appendix A, with the classification of individual charter schools provided in Table A.1.

2.2.2 Data Summaries

Our data consist of twenty-three elementary charter schools that opened in either 2012-13 and 2013-14, the two years immediately following the lifting of the statewide cap. We focus on the elementary-level as most of the new entrants served kindergarten through fifth grade.[15] We divide these newly-opened charters by their horizontal differentiation to traditional public

[14]One concern with classifying charter schools in this way is that they may not follow through with (all of) their expressed intentions after opening, e.g. potentially offering a different curriculum than the one which we originally categorized as horizontally or non-horizontally differentiated. To address this, one could classify schools based on the content subsequently contained on their websites (after they commence operations). As we document below, however, there is strong signal content in the applications: while 17 of the 23 charter schools in our sample did not open until the 2013-14 academic year, the policy effect we estimate emerges in the 2012-13 academic year at which point parents and traditional public schools (and members of the Charter Schools Advisory Board) only had access to the information in the applications.

[15]In total, thirty-two charter schools opened following the cap removal, nine of which were non-elementary schools.

schools: we designate thirteen schools as horizontally differentiated and ten schools as non-horizontally differentiated.

Figure 2 indicates the exact location in North Carolina of each newly-opened charter school. For each entrant, we draw a circle with a 2.5-mile radius around the opening location as we will treat students residing within these circles as living 'nearby' the newly-opened charter in our main specifications (see below). We can see that the majority of charter schools open in urban/suburban areas and that there is some clustering by differentiation: there is a cluster of five horizontally differentiated charters in the Raleigh-Durham (i.e., 'Triangle') area and a cluster of four non-horizontally differentiated charters in the Greensboro region. As we outline below, such clustering does not pose a problem for our identification strategy, which compares students living at varying proximities of charter schools within a given region instead of comparing students across regions.[16]

Table 1 reports summary statistics for all students in our sample along with all students living within five miles of the newly-opened charters. Column (2) clearly indicates that these newly-opened charter schools open in areas with lower test scores and in regions with a much higher proportion of black students and 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 locate in regions with higher test scores and a lower proportion of black students (and a correspondingly higher proportion of white students) than their horizontally differentiated counterparts.

We explore descriptive differences between non-horizontally and horizontally differentiated charter schools in Appendix Table C.1, where we document the average characteristics of students who attend each school type. Contrasting column (2) with column (2) of Table 1 indicates that students who attend charter schools are more likely to be white and are less likely to be economically disadvantaged than the population of students residing within a

[16]Specifically, in our main specification, we impose sample restrictions and define our treatment and control groups in a way that minimizes the influence of across-region comparisons in our estimates. Further, in subsequent robustness checks, we make the within-region comparison explicit by including neighborhood fixed effects and neighborhood-specific time trends in the analysis.

5-mile neighborhood of the charters. They also achieve higher scores on statewide exams. These comparisons hold for each type of charter, as well: for example, compared to the average student within 5 miles of a non-horizontally differentiated charter school, the average student who attends such a school is 38 percentage points less likely to be economically disadvantaged and has much higher math and English test scores (approximately 0.2 and 0.3 standard deviations, respectively). In addition, Appendix Table C.1 reveals that students in non-horizontally differentiated charter schools appear more positively selected than students who attend horizontally differentiated charters.

Our classification of charters aims to capture differentiation from public school instruction in horizontal terms. To better understand how these program choices are correlated with specific characteristics of the schools, Appendix Table A.2 draws upon supplemental information gathered from the applications to show, consistent with a focus on traditional instruction and core skills, that non-horizontally differentiated charter schools place greater focus on skill development (including college preparation), place more emphasis on student comportment, and are less likely to have a curriculum focused on social or physical student well-being than charter schools we classify as horizontally differentiated. Moreover, the applications of non-horizontally differentiated schools reveal greater alignment with "No Excuses" philosophy and practices (Angrist et al., 2013).[17] One caveat these differences raise for interpreting our findings, detailed in the next section, is that our results do not represent the effect of educational program differentiation *per se*, but also embed the effects of correlated attributes.

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

[17] While none of the charters in our sample are operated by national educational management organizations typically considered "No Excuses," such as KIPP, they may nonetheless adopt aligned focuses and practices.

school or charter school, one must account for student selection into schools. Second, charter schools do not locate randomly within school districts, but rather select where to operate strategically (Singleton, forthcoming). Estimating the effects of charter school expansion on student outcomes therefore requires accounting for systematic differences between areas with and without charter schools. Finally, as highlighted by our stylized model of school competition, charter schools offer incredibly heterogeneous curricula, so competitive effects on public schools are likely to vary by charter type.

Prior studies have approached these challenges in a number of ways. In this section, we provide a detailed description of our strategy for estimating the aggregate effect of charter school expansion, which relies on 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 estimating the aggregate effect of charter school expansion, combining both the effect on charter and on traditional public school students. By not attempting to identify the two effects separately, our approach relies on weaker assumptions about student selection than strategies used in prior work. We also relax the assumption of common effects across all charter school types to account for the role of horizontal differentiation.

North Carolina lifted the cap on the number of charter schools allowed to operate in the state in 2011. We combine this policy change with information on the distance between students' residences *prior* to the change and the new charter schools that subsequently opened to identify students who are differentially exposed to charter school expansion (treatment) and students who are not (control). In this way, our research design leverages the timing of the policy change, which makes it unlikely that students would 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. We then estimate the aggregate effect of charter school expansion by comparing test score changes for students who lived near the new charter schools with test score changes for students who live farther away, irrespective of their ex-post schooling choices. We now provide a detailed description of our estimation strategy along with a more complete discussion of the identifying assumptions.

3.2 Details

Our empirical analysis focuses on charter schools that opened in the immediate two years, 2012-13 and 2013-14, following North Carolina's removal of the statewide cap. Six elementary charter schools were approved by the Charter Schools Advisory Board to open in the first year, while seventeen schools were approved to open the following year. This focus is an important feature of our research design: Because of the timeline for charter school applications and approval in the wake of the policy change, entrants in both years had to declare an intent to open prior to any new schools beginning operations.[18] As a result, charter schools opening in the two years after the policy change could not make decisions about when or where to locate based on market responses to new entrants. Moreover, by the start of the 2012-13 academic year, public schools knew whether a charter school intended to open nearby within the next two years.

Our research design thus examines changes beginning with the 2012-13 school year, the first post-policy change year, regardless of when each charter opened (2012-13 or 2013-14). Leveraging the timing and application process, we then define a student as being exposed to (or 'treated' by) charter school expansion if his or her residence during the 2011-12 school year – the academic year before any new charter schools opened – is within r miles of one of

[18] To be more specific about the timing for the first two waves of charter schools, schools hoping to open for the 2012-13 academic year (the first wave) applied through a special 'fast track' application process designed to generate approval quickly after the cap's lifting. Schools submitted an application to the Charter Schools Advisory Board by November 2011 and the board made its final decision about the fast-tracked applications in February 2012, at which point approved schools began preparations for opening in August 2012. Charter schools hoping to open for the 2013-14 academic year had to submit their application by April 2012 and were shortlisted in June 2012. Twenty-three of the thirty shortlisted schools were then approved March 2013, at which point they began preparing to open in August 2013.

the charter school entrants that opened in either 2012-13 or 2013-14. Our setup then allows the treatment effects to vary by whether the charter school is horizontally differentiated in its educational program or not. We present an overview of our measure of exposure to charter school expansion in this section, with Appendix B providing a more detailed description of the construction of students' residential locations, distances between those locations and the new waves of charter schools, and the sample restrictions we make.

To formalize our measure of treatment by charter school expansion, we define $d(i,c)$ as the distance between student i's residence in the 2011-12 academic year and our twenty-three entering charter schools indexed by c.[19] Letting c_i^* indicate the closest such charter school to student i's 2011-12 residence, we define student i as treated by charter expansion when his or her 2011-12 residence is within r miles of c_i^*:

$$treat_i^r = \begin{cases} 1, & \text{if } d(i, c_i^*) \leq r \\ 0, & \text{otherwise.} \end{cases} \tag{3.1}$$

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.[20] To further distinguish between students who live in areas affected by horizontally and non-horizontally differentiated charter schools, we define NH_i as a binary variable that is equal to one when c_i^* (the closest immediate entrant to student i) is a non-horizontally differentiated charter school and zero when it is horizontally differentiated.

With this notation in hand, we estimate the following difference-in-differences regression

[19]As mentioned, we restrict our sample to students who are in third to fifth grade for whom we observe at least one test score before and after 2012-13. Given this restriction, nearly all charter schools in our sample serve the grades in which students are enrolled and so are a feasible option for the students to attend. Specifically, while many charter schools open with a subset of their planned grades, in their first year of operation all charter schools in our data included at least third grade and all but four (out of twenty-three) covered fifth grade (with the remaining four planning to cover fifth grade after one or two years of operation).

[20]Later, we examine 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.

to recover the effect of charter school expansion while allowing for potentially differential effects across horizontally and non-horizontally differentiated schools:

$$\begin{aligned} y_{isgt} = \alpha + \delta_g + \lambda_t + \zeta X_{isgt} + \mu_h treat_i^r + \phi Post_t + \beta_h Post_t * treat_i^r + \cdots \\ NH_i \Big(\alpha_{nh-h} + \delta_{g,nh-h} + \lambda_{t,nh-h} + \zeta_{nh-h} X_{isgt} + \mu_{nh-h} treat_i^r + \cdots \\ \phi_{nh-h} Post_t + \beta_{nh-h} Post_t * treat_i^r \Big) + \epsilon_{isgt}. \end{aligned} \tag{3.2}$$

The dependent variable is the standardized (at the grade-year level) test score of student i in school s in grade g at time t, while δ_g is a set of grade fixed effects, λ_t is a set of year fixed effects, and 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, and grade skipping or repeating status. The variable $Post_t$ indicates that the observation is from the academic year 2012-13 or later. To ensure that treated and untreated students are as comparable as possible, we restrict the analysis sample to students whose 2011-12 residence is within $2r$ miles of their nearest immediate entrant charter school. Treated students are therefore those who lived within r miles of their nearest school and untreated students are those who lived between r and $2r$ miles away.[21] We cluster the standard errors at the Census block level.[22]

The parameters β_h and β_{nh-h} are the main parameters of interest in equation (3.2), 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 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

[21]We discuss our choice of r below and we also show that our main results are robust to alternative choices of r in subsection 4.2.

[22]Alternatively, we have clustered our standard errors at the student and school district level. Standard errors clustered at the Census block level are the most conservative of these options.

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 Sorting. 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.[23]

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.[24] 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.

Nonetheless, one worry is that our strategy is potentially vulnerable to students moving across (i.e., selecting into) neighborhoods in response to the policy change. Despite the

[23]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.

[24]Our method is similar in spirit to that of Cordes (2018) who defines treatment by distance from student's local public school (rather than residence) to nearby charter. Table C.8 mimics Cordes (2018) by assigning treatment based on pre-charter expansion public school rather than residence.

sudden timing, 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 effect of charter school expansion. Relatedly, students whom we define as untreated (according to their 2011-12 residences) might 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. 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.

Charter School Location Choice. 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.[25]

A potential weakness of our empirical approach, however, is the possibility that charter schools select where to open 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 that also include neighborhood-specific trends to demonstrate that our results are not driven by differences in trends across areas with and without newly-opened charter schools.

Horizontal Differentiation of Charter School Programs. As outlined by our stylized model, we expect that the charter schools that are not horizontally differentiated with

[25] Arcidiacono et al. (forthcoming), for example, find that Walmart selects locations near low-priced supermarkets, a decision rule that leads to overestimates of the competitive effects of Supercenters on retail prices if unaccounted for.

traditional public schools are likely to create the strongest competitive incentives. In contrast, numerous prior studies constrain direct and competitive 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 create differential incentives to respond across traditional public schools. As detailed above, our primary specification (equation (3.2)) allows for this heterogeneity by uniquely drawing on information from entrants' applications to open.

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 take $r = 2.5$ 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.[26] As the distance between student residence and the charter school 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 sensitivity checks, showing that our main results are very similar for a wide range of distance cutoffs that define treatment, as well as estimate results that define treatment continuously.

[26] 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.

4 Results

4.1 Main Results

Before presenting our main difference-in-differences estimates from equation (3.2), we present the patterns in the raw test score data that our identification strategy leverages. Figure 3 plots average standardized test scores by year for students whose 2011-12 residences are between 0-2.5 miles (i.e., 'treated') and between 2.5-5 miles (i.e., 'control') away from newly-opened charter schools. These trends are further subdivided by exposure to non-horizontally and horizontally differentiated charter schools. The key assumption behind our empirical strategy is that the test scores trend for treated students would have been the same as the trend for control students absent exposure to charter school expansion. The pre-policy-change trends in Figure 3 are consistent with this: the test scores of treated and control students appear to follow the same trends in areas that were affected by both non-horizontally differentiated (Figure 3(a)) and horizontally differentiated (Figure 3(b)) charter schools.

Two additional points about Figure 3 are worth noting. First, in areas where non-horizontally differentiated schools opened, test score trends are relatively flat for both treated and control students until 2012-13, when there is a sharp increase in the test scores of treated students but no corresponding increase for control students. Second, in areas where horizontally differentiated schools opened, test scores were trending upward for both treated and control students prior to the 2012-13 academic year, at which point they flatten out for *both* groups. These raw data patterns suggest that students treated by non-horizontally differentiated charter school expansion experienced positive test score gains as a result, while students treated by horizontally differentiated charter schools realized no change in test scores (relative to students in the control group). As we now discuss, our main difference-in-differences results are consistent with these patterns.

Columns (1) and (2) in Table 3 report the results obtained from estimating equation

(3.2). The top panel presents results that constrain the aggregate 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 1.9 percent of a standard deviation and statistically insignificant. The lower panel reveals that allowing for differential effects across charter school types masks important (and statistically significant) heterogeneity. In particular, students treated by the expansion of non-horizontally differentiated schools realize an improvement in math performance of 0.04 standard deviations relative to control students. In contrast, students treated by the expansion of horizontally differentiated schools do not realize any improvement. The effects of non-horizontally differentiated and horizontally differentiated charter school expansion are statistically different, as indicated by the p-values for the (two-sided) test of the null hypothesis that β_{nh} is equal to zero in columns (1) and (2). The point estimates are very similar across specifications with (column 2) and without (column 1) student demographic variables as additional control variables.[27]

These results are consistent with our initial discussion and stylized model: The demand for horizontally differentiated charter schools is unlikely to be responsive to adjustments in traditional public school quality. We consider several robustness checks for these results below before further examining the mechanisms in the next section.

4.2 Robustness

In this subsection, we highlight that our results are robust to concerns about students sorting across neighborhoods in response to (or in anticipation of) the policy change and to charter schools making location decisions based on differential trends in student performance across neighborhoods. We also consider several alternative ways of defining treatment status.

[27]In Appendix Table C.2, we examine treatment effects overall and by charter type on English language test scores and find no effects across the board.

4.2.1 Student Sorting Across Neighborhoods

We first consider the role of differential student sorting across neighborhoods for our results. For instance, 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.[28] In addition, because treatment is determined by residence prior to new charter school openings, students who move into neighborhoods with new charter schools in response to the policy change are untreated according to our definition. 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.

We examine student sorting across neighborhoods directly by examining differential moving rates across treated and control students for both horizontally and non-horizontally differentiated charter schools. Figure C.1 in Appendix C, which plots the results, is constructed by estimating the following equation

$$\begin{aligned} m_{isgt} = {} & \alpha + \delta_g + \lambda_t + \zeta X_{isgt} + \mu_h treat_i^r + \beta_h^{2010-11}\ \mathbb{1}_{\{year=2010-11\}} * treat_i^r + \cdots \\ & \sum_{j=2012-13}^{2014-15} \beta_h^t \mathbb{1}_{\{year=t\}} * treat_i^r + NH_i \Big(\alpha_{nh-h} + \delta_{g,nh-h} + \lambda_{t,nh-h} + \cdots \\ & \zeta_{nh-h} X_{isgt} + \mu_{nh-h} treat_i^r + \beta_{nh-h}^{2010-11}\ \mathbb{1}_{\{year=2010-11\}} * treat_i^r + \cdots \\ & \sum_{j=2012-13}^{2014-15} \beta_{nh-h}^t \mathbb{1}_{\{year=t\}} * treat_i^r \Big) + \epsilon_{isgt}, \end{aligned} \tag{4.1}$$

[28]Although students could have moved to new neighborhoods in anticipation of the charter schools that would eventually locate there, the student residential location data that we use from the NCERDC files to define student residences in the 2011-12 academic year is recorded at the start of the academic year, which is *before* charter school applications were submitted in November 2011. If some families did move to areas in anticipation of new charter schools, they therefore would have likely had to make those location decisions based on guesses about where the new charter schools would locate.

where we regress an indicator for student i changing residences between year $t-1$ and t, m_{isgt}, on grade fixed effects, year fixed effects, demographic control variables, year fixed effects interacted with treatment status, and year fixed effects interacted with treatment status and an indicator for treatment being by a non-horizontally differentiated charter school.[29] 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 95-percent confidence interval associated with the estimated coefficients.

As can be seen in Figure C.1, there is no evidence that treated students move across neighborhoods at a differential rate than untreated students in either the pre- or post-policy change period. This is true for both non-horizontally and horizontally differentiated charter schools. As a result, the evidence in Figure C.1 suggests that it is unlikely that the treatment effects we estimate are influenced by differential sorting of treated and untreated students either before or after the new charter schools began operating.

We further assess the robustness of our results to threats stemming from student selection across neighborhoods by estimating specifications in which we augment equation (3.2) to also include student fixed effects. The effect of charter school expansion in these specifications is estimated from within-student changes in test scores, thereby mitigating potential biases stemming from students sorting across treated and non-treated areas; the effect of charter school expansion is identified by within-student gains in treated areas relative to non-treated areas (instead of simply differential average test score changes across the two areas). The corresponding estimates are presented in column (3) in Table 3. The results are very similar to the main estimates presented in columns (1) and (2), again implying that students ex-

[29] The last academic year before the policy change (2011-12) is the omitted year. Because the dependent variable depends on whether students changed residences across adjacent years, we cannot include observations from the first year of our sample period (the 2009-10 academic year) in this regression. Although it is possible to calculate a value for the dependent variable in 2009-10, we opt not to because the 2008-09 residence data is reported according to 2000 Census block groups which do not perfectly overlap with the 2010 Census block groups that are used throughout our analyses.

posed to non-horizontally differentiated charter schools realized an average increase in math test scores of 0.05 standard deviations while students exposed to horizontally differentiated charter schools saw no improvement.

In summary, we do not find any evidence that treated and non-treated students sorted across neighborhoods differentially 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. Moreover, we find no evidence that any such sorting affects our estimated treatment effects.

4.2.2 Charter School Location Choice

Another concern is that 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 nearby areas, our estimated effects of charter expansion may be upward biased. The opposite would be true if charter schools locate in areas where average scores are differentially decreasing. In either case, the effects we estimate would not represent treatment effects of charter expansion, but rather strategic location choice by charter schools.

The raw test score trends that we present in Figure 3 already provide evidence against our estimates being biased by differential trends. However, we further evaluate the extent to which differential trends across treated and non-treated locations are likely to play a role in our analysis with the following event-study design

$$
\begin{aligned}
y_{isgt} = \alpha + \delta_g + \lambda_t + \zeta X_{isgt} + \mu_h treat_i^r + \sum_{j=2009-10}^{2010-11} \beta_h^t \mathbb{1}_{\{year=t\}} * treat_i^r + \cdots \\
\sum_{j=2012-13}^{2014-15} \beta_h^t \mathbb{1}_{\{year=t\}} * treat_i^r + NH_i \Bigg(\alpha_{nh-h} + \delta_{g,_{nh-h}} + \lambda_{t,nh-h} + \cdots \\
\zeta_{nh-h} X_{isgt} + \mu_{nh-h} treat_i^r + \sum_{j=2009-10}^{2010-11} \beta_{nh-h}^t \mathbb{1}_{\{year=t\}} * treat_i^r + \cdots \\
\sum_{j=2012-13}^{2014-15} \beta_{nh-h}^t \mathbb{1}_{\{year=t\}} * treat_i^r \Bigg) + \epsilon_{isgt},
\end{aligned}
\tag{4.2}
$$

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 treated and untreated areas (by horizontally and non-horizontally differentiated schools, respectively).

Figure 4 plots the estimated coefficients from equation (4.2) for each year prior to and following the lifting of the statewide cap as well as the associated 95-percent confidence intervals. As the figure reveals, there is no evidence of significant 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. Consistent with our main results, the test scores for students who are treated by non-horizontally-differentiated charter schools only start to clearly increase following the policy change.

To further rule out differential trends as a confound for our results, we also re-estimate equation (3.2) but additionally include neighborhood-specific 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 block group in which each student resided in the 2011-12 school year as his or her neighborhood. If 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, we should not continue to observe a positive and statistically significant effect of charter school expansion after accounting for such trends. Column (4) in Table 3 presents the estimated effects that control for neighborhood-specific linear trends in test scores (as well as student fixed effects). The estimates are again very similar to those from our main specifications in columns (1) and (2), suggesting that strategic charter school entry based on pre-existing test score trends is unlikely to be driving our results.

4.2.3 Sensitivity Checks

In this subsection, we explore the sensitivity of our results to the specification of our estimating equation. In particular, we consider varying the distance radius that we use to define treatment and alternatively defining treatment using a continuous measure of distance. We also verify that our results are not driven by a small number of outlier charter schools with particularly large or small effects.

Varying the Treatment Radius

Figure 5 displays how our main treatment effect estimates from column (2) 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 2.5 miles and our main treatment effect estimate for the expansion of non-horizontally differentiated charter schools is 3.4 percent of a standard deviation. The profile in Figure 5 shows that the estimated treatment effect is stable for radii ranging from 1.5 miles to 7.5 miles – in each case, the estimated treatment effect is not statistically different from our main estimate. However, the point estimates do begin to decline as the radius grows, eventually fading to zero for radii of 8.5 miles or greater. This fadeout is expected given that students are less likely to attend charter schools that are farther away from their residences (as shown in Table 2), implying that both the competitive and direct effects of charter schools are muted at greater distances. We also find that the effect of horizontally differentiated charter school expansion is both economically and statistically insignificant at all radii used to define treatment.

Measuring Treatment Using Continuous Distance

To further assess our empirical specification, we re-estimate our main equation (using our main sample of students living within 5 miles of a newly-opened charter school) while measuring treatment using a continuous measure of distance between student residence and charter school location instead of a binary cutoff. 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 C.3, which reproduces all of the results from Table 3 while measuring treatment using continuous distance. The estimate in column (3) 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 0.019 standard deviations. A back-of-the-envelope calculation shows that this estimate is remarkably close to our main estimate that uses binary cutoff at 2.5 miles to define treatment.[30]

Ensuring the Results are Not Driven by Outliers

A concern with our analysis is that our results may be sensitive to how particular charter schools are classified as either horizontally or non-horizontally differentiated. To assess this concern, we augment our empirical specification to estimate separate difference-in-differences regressions for each entering charter school in our sample, recovering twenty-three estimates of the effect charter school expansion.[31] We then plot the estimated effect for each charter school against the number of observations in Figure C.2.[32] Consistent with our main findings,

[30]Among treated students in our main specification, the average distance between their residences and the nearest non-horizontally differentiated charter school 1.70 miles. Among untreated students, the average distance is 3.77 miles, implying a difference in average distances of 2.07 miles. Using the estimate of 0.019 standard deviations per mile implies a test score difference between treated and untreated groups of 0.04 standard deviations ($0.019\sigma \times 2.07$ miles), which is very similar to the estimate of 0.05 standard deviations in column (3) of Table 3.

[31]Each regression includes demographic controls and student fixed-effects (i.e., the set of controls from column (3) of Table 3).

[32]Three charters are omitted from Figure C.2 due to extremely noisy estimates (all three omitted charters have less than 100 student-year observations within a five mile radius).

the figure reveals that most charter schools that we identify as non-horizontally differentiated have positive effects with magnitudes very close to our main (overall) estimate. Moreover, no single non-horizontally differentiated charter school appears as an outlier in its estimated impact. In contrast, the figure shows that the estimated impacts of horizontally differentiated charter schools cloud around zero, with both a positive and negative outlier. In addition, we find qualitatively similar results when again estimating a pooled difference-in-differences regression but removing the sub-samples of students attached to any two charter schools of a given differentiation category from the analysis, confirming that our results are robust to the classification of charter schools and the influence of outliers.

5 Mechanisms

We discuss the mechanisms underlying our results in this section. First, we show that our results are primarily driven by competitive responses by public schools rather than direct effects on students who choose to switch to charter schools. We then discuss the importance of student sorting and peer effects. In the third subsection, we show that these competitive responses of public schools occur across schools rather than within school. Finally, we test for the possibility that our effects are driven by vertical (rather than horizontal) differentiation of charter schools.

5.1 Direct Effects and the Competitive Channel

In this subsection, we examine how the aggregate effect that we estimate depends on the direct effects of charter expansion. The aggregate effect we estimate represents a combination of the direct and competitive effect of charters: our empirical approach, in its agnosticism to students' ex-post schooling choices, treats symmetrically students who choose to attend a new charter school and students who choose to remain in public schools. We investigate the relative importance of the direct and competitive channels for the aggregate effect in two ways.

First, we examine the value-added of the charter school entrants. To do so, we estimate school-level value-added using standard methods, regressing 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-year's fixed effect as its value-added estimate.[33] Figure 6 depicts the average charter school-level value-added in each post-policy-change year. 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, the average value-added among traditional public schools is approximately 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 slightly higher average value-added than traditional public schools. Horizontally differentiated charter schools, on the other hand, continue to lag behind for the duration of the sample period.

While the variation in Figure 6 suggests that direct effects may account for a share of the aggregate effect of non-horizontally differentiated charters we estimate, few students in our sample actually switch to a charter school: among 'treated' students, just 2.37 percent attend the nearby charter school by the end of our sample period.[34] Thus, an accounting exercise strongly suggests that direct effects cannot explain the aggregate increase in student math scores.

Second, to quantitatively isolate the influence of the indirect channel, we estimate our main specification while effectively 'netting-out' the direct channel. To do so, we reproduce our difference-in-difference results from equation (3.2) except that we re-code the test score gains of students who switch from public schools to the newly-opened charter school to zero.[35]

[33]We normalize the fixed effects to sum to zero.

[34]The analogous number for 'control' students is 1.76 percent.

[35]Specifically, we code every public-charter switcher to have the same test score (in standard deviation units) as they had in 2011-12, the year before they could switch into the newly-opened charter school.

This re-coding shuts down any test score increases caused by the charter school themselves and so can be considered a test for the presence of the competitive channel.[36] These results are presented in column (1) of Table C.4 and are virtually identical to our main estimates, indicating that a large majority of our effects are driven by indirect competitive responses by public schools. Likewise, columns (2) and (3) of Table C.4 follow the spirit of Lee bounds (Lee, 2009) and recode test score gains of switching students to the 5th and 95th percentile of test score gains. This exercise provides very tight 'bounds,' ruling out the possibility that the direct effect of charters substantially contributes to the net effect of charter school expansion.

5.2 Peer Effects

In this subsection, we explore whether the indirect impacts on public schools reflect these schools becoming more productive in response to competition or students resorting across schools and achievement being influenced by changing peer composition (Hsieh and Urquiola 2006). If peer effects are important for student learning, and high-achieving students leave traditional public schools for charter schools, the indirect effect we estimate could represent a composite of a positive effect on student learning through the competitive channel and a negative effect through the peers channel. We discuss the relative importance of these channels for both non-horizontally and horizontally differentiated charter schools.

Table C.5 shows that students who switch from public schools to both non-horizontally and horizontally differentiated charter schools are positively selected relative to students who stay, as they score 0.24 to 0.37 standard deviations higher in math and 0.07 to 0.38 standard deviations higher in reading.[37] Because switchers are relatively high performers, their departure from traditional public schools likely implies a worsening in peer quality and perhaps correspondingly negative peer effects on achievement. Any negative effects that

[36] Unfortunately, these results do not bound the size of the competitive channel since the causal effect of charter schools on public-charter switchers is unknown and could in principle be negative.

[37] Switchers are more likely to be white, less likely to be a racial minority, and less likely to be disadvantaged.

operate through the peers channel are likely to be quite small in our setting, however, as a very small fraction of students switch to charter schools. Therefore, the positive effect we estimate for non-horizontally differentiated charter schools can reasonably be viewed as a *lower bound* for the (true) competitive effect on traditional public schools.[38]

On the other hand, students who switch into horizontally differentiated charter schools are also positively selected relative to students who do not. It is therefore possible that these charter schools cause traditional public schools to become more productive, but the competitive effect is swamped by negative effects stemming from worse peers quality. Tables C.6 and C.7 show some evidence consistent with this possibility. Table C.6 indicates that both the switching rate to charter schools and the differential in the switching rates among treated and control students are highest in third grade and smallest in fifth grade. Table C.7 shows that grade-specific treatment effects of horizontally differentiated charter schools are negative in third grade, where the largest change in peer composition occurred, and positive in fifth grade, where the smallest change occurred (although these effects are never statistically significant).

While these results are suggestive of negative peer effects confounding positive competitive effects, we believe this is unlikely for two reasons. First, the highest switching rate into non-horizontally differentiated schools also occurs in third grade and switchers into these schools are even more positively selected than switchers into horizontally differentiated schools. If negative peer effects are important, public schools facing non-horizontally differentiated competition in third grade should therefore be at a greater disadvantage than those facing horizontally differentiated competition. Yet the point estimate for non-horizontally differentiated charters in third grade in Table C.7 is the largest among all grades. Instead, we believe that the higher response in earlier grades is likely due to higher competitive pressure in those grades as they have higher switching rates than later grades (see Table C.6). Second,

[38]Given that we do not observe any significant shifts in class sizes or teachers in these public schools (resulted available upon request), the public school response is most likely driven by the schools using a given set of inputs more productively, as in Petronijevic (2016).

even if peer effects are much more important in public schools exposed to competition from horizontally differentiated charters, the total number of switching students is very small in our setting, implying that magnitude of peer effects would have to be implausibly large for the peers channel to totally confound the competitive channel.[39]

5.3 The Level of Treatment

Our main treatment variable is defined at the student-level, capturing the intuitive idea that (all else equal) students are more likely to attend a public school that responds competitively (or to consider switching to a charter school) when they live within closer proximity of an entering charter. While this definition is attractive for its transparency as an 'intent-to-treat,' we also re-estimate all of our main results from equation (3.2) by instead defining treatment at the school-level.

Under the school-level definition of treatment, a student is treated if the nearest entering charter school is within 2.5 miles of the traditional public school that the student attended in the 2011-12 academic year. A student is untreated if the nearest school is between 2.5 and 5 miles away from their public school. The results are presented in Table C.8. Although the corresponding point estimates are slightly larger than their counterparts in Table 3, they are never statistically distinguishable and our main qualitative findings remain unchanged. The stability of our main results across levels of treatment definition is consistent with the effect of charter school exposure operating uniformly across students within a traditional public school. Continuing to define treatment at the school level, in results not reported here, we also find that among students within a treated traditional public school, the effect of charter school exposure does not vary across students by their proximity to the charter.[40]

[39] With 2.5 percent of students switching and these students scoring 0.3σ higher than stayers, public school peer quality should decline by about 0.007σ. Even using the high-end of peer effects found in the literature this change in peer quality will not have a substantive effect: e.g., peer effects estimated in Graham (2008) indicate the change in peer quality will decrease test scores of students staying in public schools by 0.006σ.

[40] These results are available upon request.

5.4 Vertical Differentiation

While the preceding results indicate that the indirect channel is the principal source of aggregate gains, we examine in this subsection whether it is horizontal or *vertical* differentiation of charter schools that accounts for competitive effects. As Figure 6 reveals, non-horizontally differentiated charter schools are better in vertical terms than horizontally differentiated charters on average. This suggests that the effect we estimate may be explained by public schools simply increasing quality in response to higher quality competitors rather than alternative educational programs.

To assess the importance of vertical differentiation, we therefore modify our main estimating equation by also including the value-added of the nearest charter school for each student in the regression (along with the appropriate interaction terms):

$$\begin{aligned}
y_{isgt} = {} & \alpha + \delta_g + \lambda_t + \zeta X_{isgt} + \mu_h treat_i^r + \phi Post_t + \beta_h Post_t * treat_i^r + \cdots \\
& NH_i \Big(\alpha_{nh-h} + \delta_{g,nh-h} + \lambda_{t,nh-h} + \zeta_{nh-h} X_{isgt} + \mu_{nh-h} treat_i^r + \cdots \\
& \phi_{nh-h} Post_t + \beta_{nh-h} * Post_t * treat_i^r \Big) + \cdots \\
& \nu_h^1 \mathrm{VA}_{ic} + \nu_h^2 \mathrm{VA}_{ic} * treat_i^r + \nu_h^3 \mathrm{VA}_{ic} * Post_t + \nu_h^4 \mathrm{VA}_{ic} * treat_i^r * Post_t + \cdots \\
& NH_i \Big(\nu_{nh-h}^1 \mathrm{VA}_{ic} + \nu_{nh-n}^2 \mathrm{VA}_{ic} * treat_i^r + \cdots \\
& \nu_{nh-h}^3 \mathrm{VA}_{ic} * Post_t + \nu_{nh-h}^4 \mathrm{VA}_{ic} * treat_i^r * Post_t \Big) + \epsilon_{isgt}.
\end{aligned} \tag{5.1}$$

If public schools respond to the vertical differentiation of non-horizontally differentiated schools, 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 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 attenuate or even fall to zero.

Table 4 reports the results from estimating equation (5.1). 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 nearby charter school to test whether vertical differences between charter schools explain the findings. 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 estimated effect of non-horizontally differentiated charter schools remains unchanged with the inclusion of value-added measures. Furthermore, the value-added of charter schools is unrelated to student outcomes. This result is consistent with two facts in our setting: First, we have few public-charter switchers in our sample and so the direct effect of charter school quality is limited. Second, public schools respond to charter entry *prior* to actual entry, implying public schools likely make quality decisions *before* observing charter school quality. Columns (4) to (6) demonstrate similar patterns using specifications that are estimated with student fixed effects and neighborhood-specific trends. Our main results are robust, with the estimated association between charter school value-added and student outcomes remaining small and 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 competitive responses to vertical – as opposed to horizontal – differentiation of charter schools explains the aggregate effect of charter expansion. One caveat to this interpretation of these results, which we are unfortunately unable to test, is that some of the effect that loads on horizontal differentiation may be because public schools cannot perfectly observe the quality of entering charter schools and use educational program as a proxy.

6 Conclusion

School choice policies, such as charter schools, aim to expand educational opportunity by raising the quality of education even for students who may remain in public schools. 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.

With this motivation, we estimate 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 contain 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 in their application.

The difference-in-differences research design that we implement combines the timing of the policy change with the distances between students' *pre-policy-change* residences 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 farther 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 charters and for those students exposed to entry by non-horizontally differentiated charters irrespective of the students' ex-post schooling choices.

We find 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. This effect, however, is driven entirely by non-horizontally differentiated charter schools: the estimates indicate that the causal effect of non-horizontally differentiated charter school expansion is 0.05 standard deviations while the expansion of horizontally differentiated charter schools has no effect on student test scores. Our results findings are robust to several robustness checks, such as student fixed effects and neighborhood-level trends designed to rule out student sorting and strategic charter school location as confounders. In examining the mechanisms driving these results, we show that, while non-horizontally differentiated charter schools have higher value-added on average, the aggregate effect comes almost entirely through the indirect channel and that vertical quality differentiation across charter school entrants, as captured by value-added differences, is unable to account for the results.

Our findings are important for evaluating the expansion of school choice policies and of charter schools in particular. When considering whether to allow expansion of school choice, policymakers will want to know how all students are likely to be affected regardless of whether students remain in public schools or switch to a private or new charter school. The magnitude of the effect of exposure to a entering non-horizontally differentiated charter we find is in line with estimates of the competitive impacts of voucher programs.[41] In addition, our results suggest policymakers can bolster the social gains of school choice expansion by screening charter school applicants. In particular, given that we identify charter schools'

[41] Using variation from Florida's scholarship program, Figlio and Hart (2014) estimate that 10 additional private schools nearby a public school raises test scores by around 0.02 standard deviations. Figlio and Karbownik (2016) find spillovers in the neighborhood of 0.1 standard deviations on growth from Ohio's EdChoice program.

types solely from information contained on their application (i.e., *ex ante* to the school's opening), policymakers may be able to reliably predict an applicant's likelihood of generating competitive externalities on educational quality. In addition, the direct and competitive channels of charter school expansion appear to be complementary as non-horizontally differentiated charter schools, a number of which describe "No Excuses"-type practices, are also higher value-added.

Nonetheless, our paper has several limitations that point to directions for future work. For example, few students subsequently switch to charter schools in the grades we are able to examine test score impacts. While this emphasizes the role of the competitive mechanism for our findings, it also suggests that a longer-run view of the effects, wherein selection by new cohorts of elementary schoolers may influence peer compositions at public schools and public schools learn about their residual demand curves, would be valuable. In addition, examining charter expansion impacts on private schools – many of which are similarly differentiated along horizontal dimensions – may yield new insights about how students and households sort across schools. An additional direction for future work would be to quantify the role of strategic differentiation by schools for educational quality to estimate the social value of screening charter school applicants.

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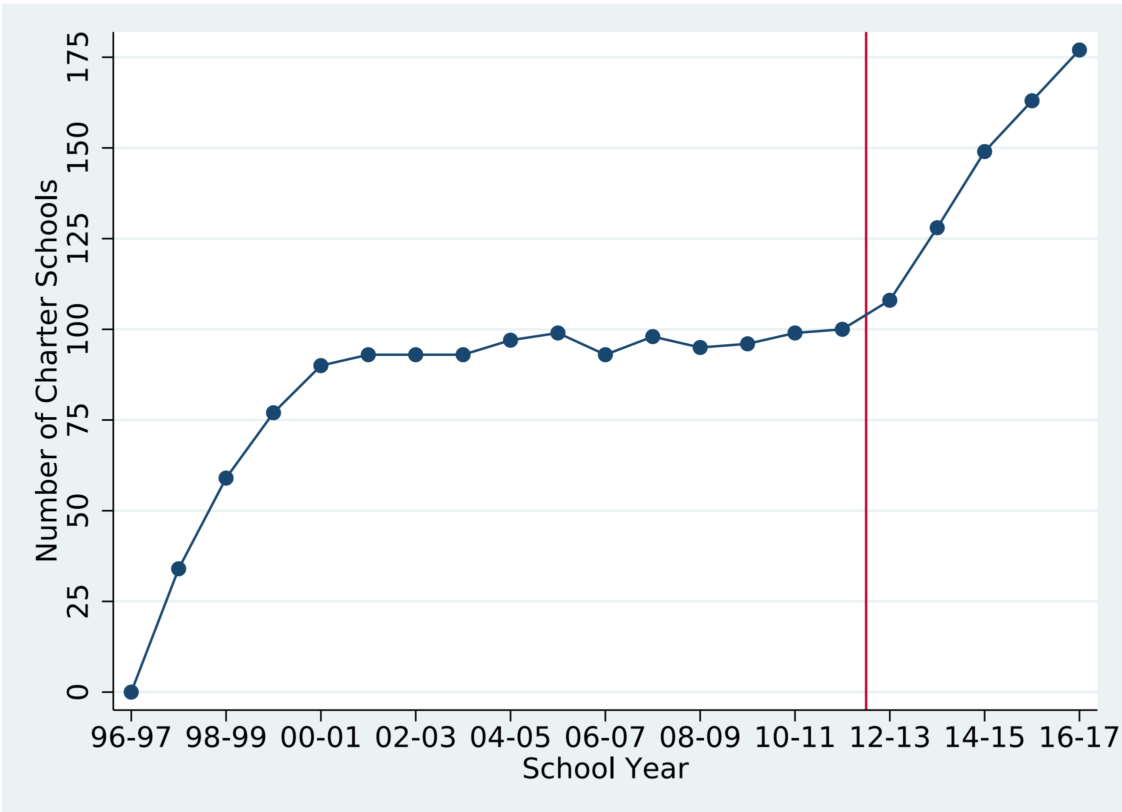
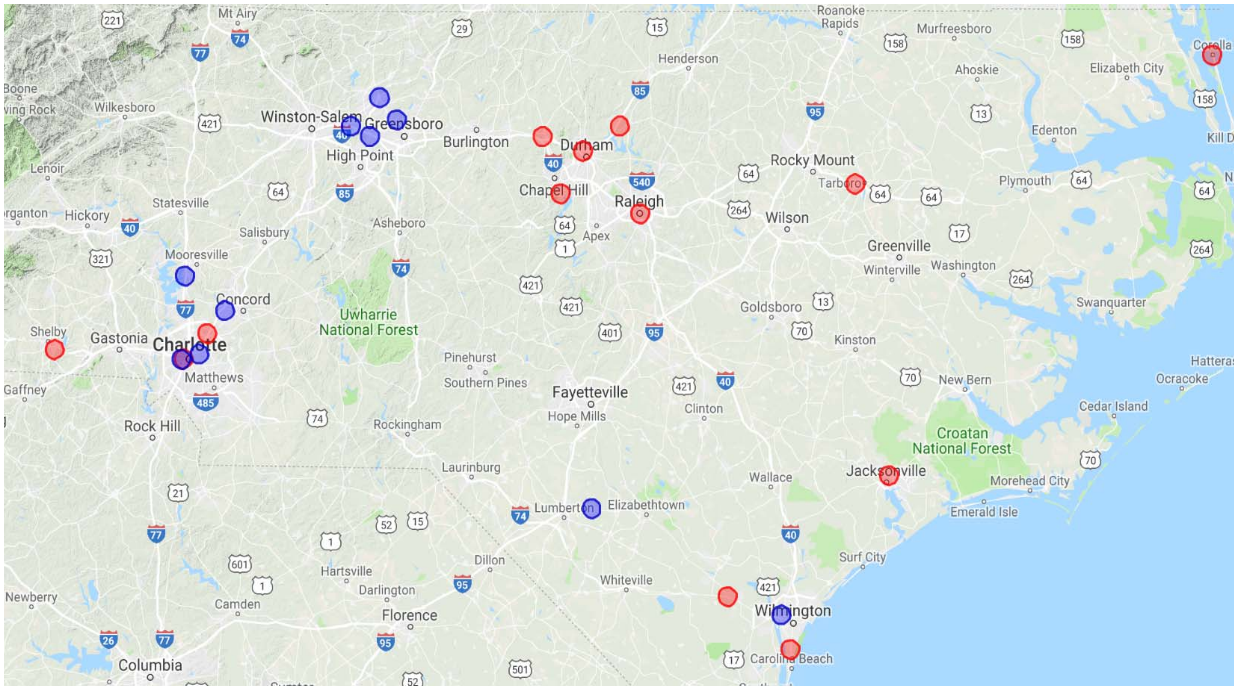
Figures

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.

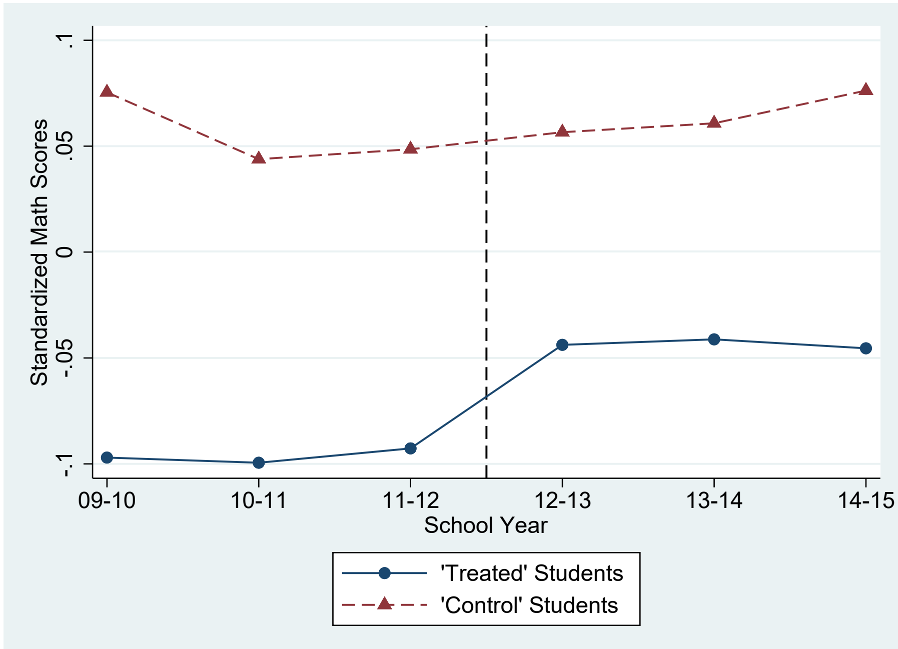
Figure 2: Locations of Charter Schools Opening in 2012-13 or 2013-14



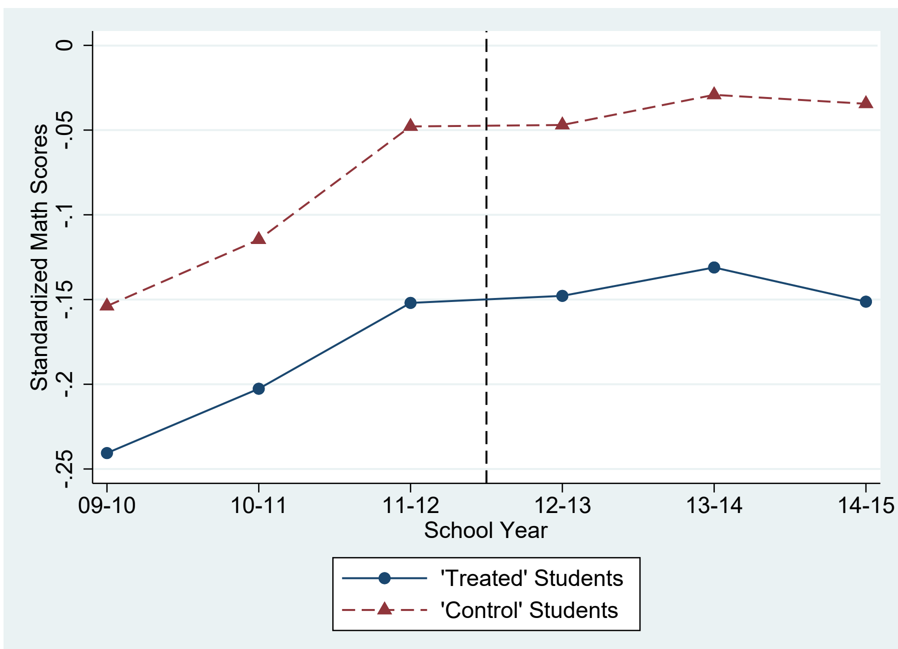
Notes: This figure draws circles with a 2.5 mile radius around the 23 charter schools in our data that opened in the 2012-13 or 2013-14 school year. Blue circles indicate that the charter is non-horizontally differentiated from the local public school while red circles indicate that the charter is horizontally differentiated from the local public school (as described in Section 2.2). Students residing within these circles are considered 'treated' in our main specifications. For students residing in regions where the circles intersect, the student is assigned to the nearest charter school so that no student is double counted in our regressions (see Appendix B).

Figure 3: Test Score Trends over Time by 'Treatment' and 'Control'

(a) Non-Horizontally Differentiated



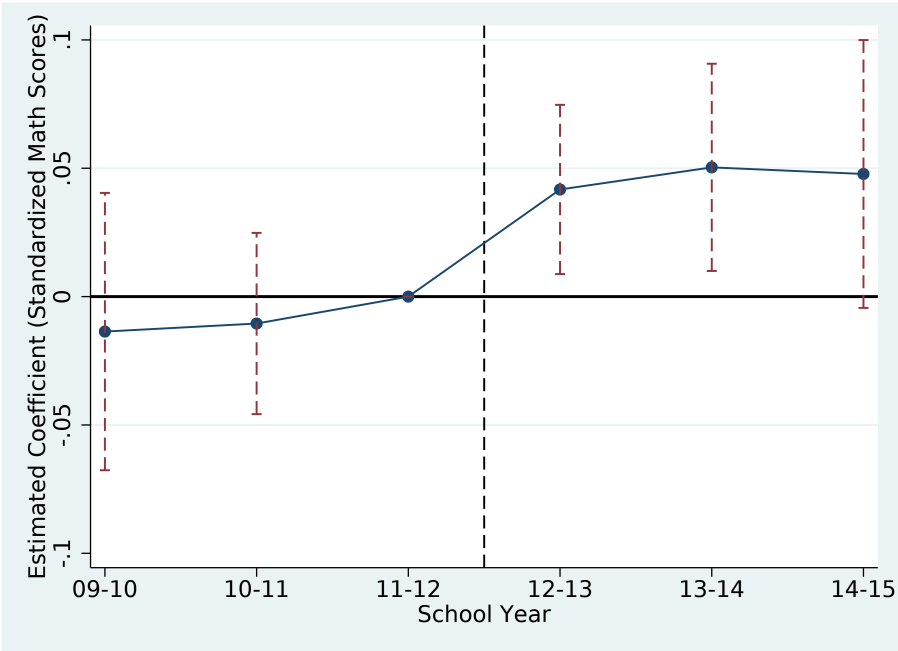
(b) Horizontally Differentiated



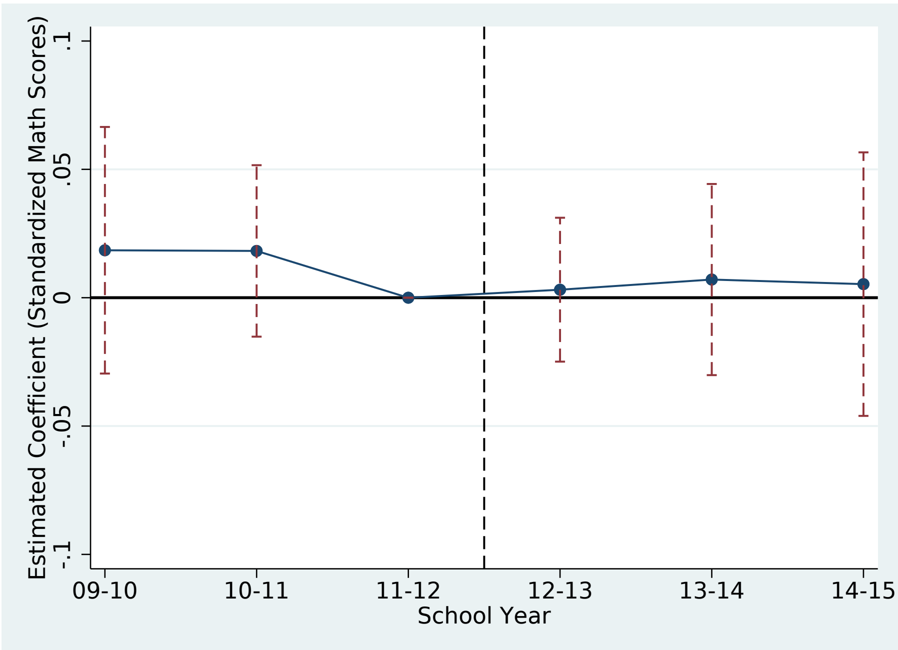
Notes: This figure shows raw test scores in math over time for 'treated' and 'control' students. We define students as 'treated' if the live within 2.5 miles of a charter school that opened in 2012-13 or 2013-14. 'Control' students are defined as students living between 2.5 and 5 miles of a charter school 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. The dashed vertical line separates the years before the charter opened from the years after the charter opened. Note that we always consider 2012-13 to be the year the charter opened because although the charters themselves opened in either 2012-13 or 2013-14, public schools would have known by the start of 2012-13 whether or not a charter was opening nearby in either 2012-13 or 2013-14.

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

(a) Non-Horizontally Differentiated

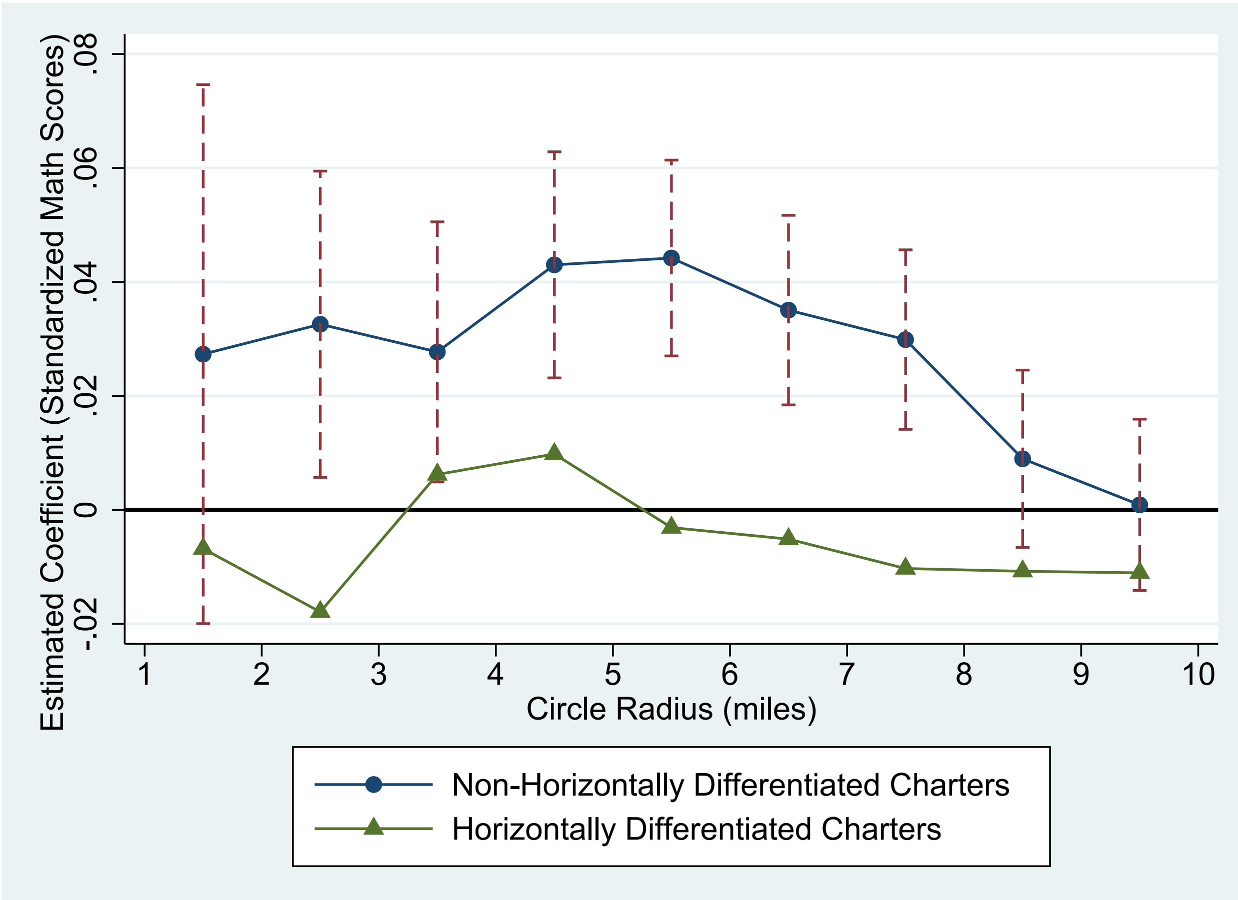


(b) Horizontally Differentiated



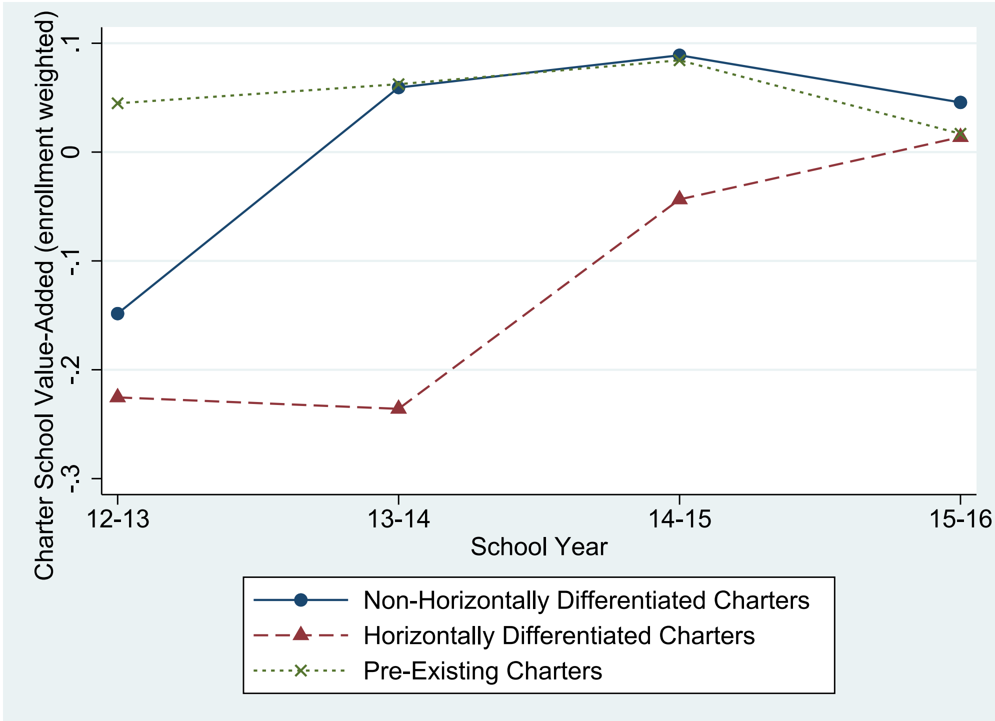
Notes: This figure shows the estimated difference between student 'treated' by a newly-opened charter relative to 'control' students by year as described in equation (4.2). Treated students are defined as students living within 2.5 miles of a charter school that opened in the 2012-13 or 2013-14. Control students are defined as students living between 2.5 and 5 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



Notes: This figure shows sensitivity of our main result in equation (3.2) 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 with radius r considers all student whose residential distance to the newly-opened charter in 2011-12 is between 0 and r miles as treated, while considering all students who live between r miles and $2r$ 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 'pre-existing' 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 North Carolina Students	Newly-Opened Charters	Non-Horizontally Differentiated	Horizontally Differentiated
	(1)	(2)	(3)	(4)
Math Score (standardized)	0.015	-0.035	0.018	-0.092
ELA Score (standardized)	0.006	-0.064	-0.031	-0.099
Percent White	52.3	37.0	41.2	32.3
Percent Black	25.3	37.9	33.8	42.3
Percent Hispanic	14.5	17.2	16.3	18.1
Percent Asian	2.7	3.6	4.4	2.8
Percent Disadvantaged	55.6	58.2	56.7	59.7
Percent with Disability	12.6	12.3	12.8	11.7
Percent Gifted	14.6	15.5	15.6	15.4
Value-Added (charters)	0.037	-0.041	0.047	-0.144
Value-Added (nearby public)	-0.001	0.003	0.017	-0.010
Observations (student-year)	1,117,142	165,313	85,853	79,460
# of charters	168	23	10	13

Notes: The sample of all North Carolina students is defined as all grade 3-5 North Carolina students who we observe at least once with a valid math or ELA score before and after charter entry in 2012-13 and has a valid address in 2011-12. Value-added of schools are calculated as the school fixed effect residual of a regression of math scores on prior test scores and demographic controls using data on all North Carolina students from 2009-10 through 2014-15. For charter schools, the value-added reported is the enrollment-weighted value added of the charter schools. The value added of nearby public schools is the enrollment-weighted value-added of all public schools within 5 miles of the newly-opened charter school, except for column (1) which reports enrollment-weighted value-added of all public schools in North Carolina.

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' (0-2.5 miles) vs. 'Control' (2.5-5 miles) (1)	(2)	(3)	(4)
Panel A. Pooled				
All Newly-Opened Charters	0.019	0.016	0.025**	0.023*
	(0.013)	(0.012)	(0.011)	(0.013)
Panel B. Heterogeneous				
Non-Horizontally Differentiated	0.043**	0.035**	0.049***	0.038*
$(\beta_h + \beta_{nh-h})$	(0.018)	(0.017)	(0.015)	(0.020)
Horizontally Differentiated	-0.008	-0.005	-0.003	0.007
(β_h)	(0.017)	(0.016)	(0.016)	(0.015)
Test of Equality by Differentiation Status p-value of H_0: $\beta_{nh-h} = 0$ vs. H_1: $\beta_{nh-h} \neq 0$	0.04	0.08	0.02	0.21
Demographic Controls	No	Yes	Yes	Yes
Student Fixed Effects	No	No	Yes	Yes
Census Block Group Time Trends (linear)	No	No	No	Yes
Observations (student-year)	164,959	164,959	164,959	164,959

Notes: This table shows difference-in-differences estimates from equation (3.2), whereby students living within 2.5 miles of a newly-opened charter school are considered 'treated' while those living 2.5-5 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. 'Test of Equality by Differentiation Status' reports the p-value of the hypothesis test that the point estimate for non-horizontally differentiated charters is the same as the one for horizontally differentiated charters; this is equivalent to testing the hypothesis of H_0: $\beta_{nh-h} = 0$ vs. H_1: $\beta_{nh-h} \neq 0$ in (3.2). 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 with Vertical Differentiation

	'Treated' (0-2.5 miles) vs. 'Control' (2.5-5 miles)					
Mathematics Test Scores	(1)	(2)	(3)	(4)	(5)	(6)
Non-Horizontally Differentiated	0.035**	0.036**	0.033**	0.038*	0.036*	0.037*
$(\beta_h + \beta_{nh-h})$	(0.017)	(0.017)	(0.016)	(0.020)	(0.020)	(0.020)
Horizontally Differentiated	-0.005	-0.005	-0.004	0.007	0.005	0.004
(β_h)	(0.016)	(0.016)	(0.016)	(0.015)	(0.015)	(0.014)
Charter VA	-	0.017	0.019	-	-0.021	-0.023
(ν_h^4)		(0.091)	(0.112)		(0.106)	(0.109)
Charter VA*Non-Horizontally Diff.	-	-	0.019	-	-	-0.023
$(\nu_h^4 + \nu_{nh-h}^4)$			(0.146)			(0.205)
Test of Equality by Differentiation Status p-value of H_0: $\beta_{nh-h} = 0$ vs. H_1: $\beta_{nh-h} \neq 0$	0.08	0.08	0.11	0.21	0.21	0.19
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Student FEs & Census Tract Trends	No	No	No	Yes	Yes	Yes
Observations (student-year)	164,959	164,959	164,959	164,959	164,959	164,959

Notes: This table shows difference-in-differences estimates controlling for vertical differentiation as described by equation (5.1). '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. 'Test of Equality by Differentiation Status' reports the p-value of the hypothesis test that the point estimate for non-horizontally differentiated charters is the same as the one for horizontally differentiated charters; this is equivalent to testing the hypothesis of H_0: $\beta_{nh-h} = 0$ vs. H_1: $\beta_{nh-h} \neq 0$ in (5.1). 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.

A Assigning Horizontal Differentiation

This appendix briefly describes the sample of charter schools that were approved in the first wave of charter school applications after the lifting of the charter school cap. It then describes exactly how we classify charters into 'horizontally differentiated' and 'non-horizontally differentiated' based on their charter school application and shows how schools' philosophies and practices differ based on differentiation status.

All data on charter school applications come from the State Board of Education, which has data on shortlisted and approved charters and applications for all charter schools that applied to the State Board of Education from 2012 onwards.[42] Table A.1 reports the full list of newly-opened charter schools used in our sample along with their LEA code,[43] their location, their horizontal differentiation status and the reason they were classified as horizontally differentiated based on their charter school application (if applicable).

The so-called 'fast track' charter applications for charters planning to open in the 2012-13 school year were due in November 2011, approximately 5 months after the lifting of the 100 charter school cap. There were 27 'fast track' applications, of which 9 were approved to open by the North Carolina Public Charter Schools Advisory Council. Of those 9, we drop 3 schools from our analysis: two for being high schools and one for never opening.[44] This leaves us with a sample of 6 schools opening in 2012-13, of which 4 are designated as 'horizontally differentiated' and 2 are designated as 'non-horizontally differentiated.'

For the 'normal track' charter schools that planned to open for the 2013-14 school year, applications were due in April 2012. There were 63 applications, of which 30 were shortlisted in June 2012. Applications of 24 of the shortlisted charters were then approved in March 2013. Of those 24 schools, we drop 7 schools from our analysis: five for being high schools,

[42] Available at `http://www.ncpublicschools.org/charterschools/applications/`.

[43] 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.

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

one for being a private-charter conversion, and one for never opening.[45] This leaves us with 17 charter schools opening for the 2013-14 school year of which 9 are designated as 'horizontally differentiated' and 8 are designated as 'non-horizontally differentiated.' Our final sample of newly-opened charter schools thus consists of 23 schools, where 13 are designated as 'horizontally differentiated' and 10 are designated as 'non-horizontally differentiated.'

In addition to reading for differentiation of educational program, we coded characteristics of schools' philosophy and practices based on the content of the applications. To do this, we followed Angrist et al. (2013) to identify important characteristics of focus. Applications were read to asses whether a school's application indicates strict adherence to standards, a focus on discipline, a focus on college preparation, adherence to a strict dress code, the curricula includes extended math instruction, a focus on social and physical well-being, a focus on cultural awareness, a focus on leadership development, and whether group projects are a principal element of instruction. From these indicators, we created three summary index values of each charter school's philosophy and practices, including an index capturing alignment with "No Excuses" (based on the correlations reported in Angrist et al. (2013)). The construction of the indices is detailed in the notes of Table A.2, which summarizes the associations of the indices with horizontal differentiation.

[45]The 5 high schools were Flemington Academy, Longleaf School of the Arts, Oxford Preparatory High, Paul Brown Leadership Academy and Uwharrie Charter Academy, while the approved charter of The Howard and Lillian Lee never opened (the same school whose fast track application was approved but never opened). The conversion school was Student First Academy, which converted from a private school to a charter school and later closed at the end of the 2013-14 school year due to financial mismanagement.

Table A.1: List of Newly-Opened Charters and Reason for Designating Charter as Horizontally Differentiated

School name (LEA)	Opened	(Lat, Lon)	Diff.	Reason
Cabarrus Charter Academy (13B)	2013-14	(35.4104, -80.6691)	N	
Willow Oak Montessori (19C)	2013-14	(35.855, -79.0253)	Y	Montessori
Pinnacle Classical Academy (23A)	2013-14	(35.2611, -81.5043)	Y	Classical education
STEM Education for a Global Society Academy (24C)[1]	2013-14	(34.3127, -78.2063)	Y	"seeks to emphasize personalized learning... for students who enter school with challenges and who are frequently underperforming" (Goals, p. 7)
Waters Edge Village School (27A)	2012-13	(36.37826, -75.832041)	Y	"hands-on curriculum empower students by instilling a sense of social and environmental responsibility while nurturing both body and mind" (Mission, p. 6)
The Institute for the Development of Young Leaders (32P)	2013-14	(36.0163, -78.9139)	Y	"project based, child centered educational environment that is inspiring, intellectually stimulating, personally affirming and emotionally supportive" (Mission, p. 4)
North East Carolina Preparatory School (33A)	2012-13	(35.891794, -77.58057)	Y	"teach and inspire through a challenging curriculum that integrates technology, experiential learning and critical thinking skills" (Mission, p. 8)
North Carolina Leadership Academy (34H)	2013-14	(36.1099, -80.0515)	N	
Falls Lake Academy (39A)	2013-14	(36.1104, -78.7351)	Y	"believe students benefit from challenging experiential and traditional learning experience" (Mission, p. 6)
Cornerstone Academy (41G)	2012-13	(36.13432, -79.827041)	N	
The College Prep and Leadership Academy of High Point (41H)	2012-13	(36.070916, -79.959375)	N	
Summerfield Charter Academy (41J)	2013-14	(36.2179, -79.9124)	N	
Langtree Charter Academy (49F)	2013-14	(35.5413, -80.8652)	N	
Corvian Community School (60M)	2012-13	(35.32301, -80.756351)	Y	"use the Basic School educational philosophy to provide an optimum environment for learning in which... students are intrinsically motivated as lifelong learners" (Mission, p. 5)
Aristotle Preparatory Academy (60N)	2013-14	(35.2246, -80.8819)	N	
Charlotte Choice Charter (60P)	2013-14	(35.2441, -80.7949)	N	
Invest Collegiate Transform (60Q)	2013-14	(35.2254, -80.8732)	Y	"the entire school community builds upon the collaboration across six active domains of learning: imagine, nurture, value, engage, sustain, and transform" (Educational Focus, p. 9)
Douglass Academy (65C)[2]	2013-14	(34.242, -77.9434)	N	
Island Montessori Charter (65D)	2013-14	(34.1079, -77.8985)	Y	Montessori
ZECA School of Arts and Technology (67B)	2013-14	(34.7791, -77.4152)	Y	"staff will participate in staff development covering the following topics; Social and Emotional Teaching, Technology Instruction, Project Based Learning" (Goals, p. 6)
The Expedition School (68C)[2]	2013-14	(36.07067, -79.113701)	Y	"provide excellent and innovative education to students through experiential and project based learning and STEM focused curriculum" (Mission, p. 9)
Southeastern Academy (78B)	2013-14	(34.6517, -78.8738)	N	
Triangle Math and Science Academy (92T)	2012-13	(35.77853, -78.635361)	Y	"employs an inquiry-based curriculum" (Curriculum Design, p. 50)

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

Table A.2: School Philosophy and Practices Correlated with Horizontal Differentiation

	Skills (1)	Comportment (2)	Well-Being (3)	"No Excuses" (4)
Non-Horizontally Differentiated	0.303	0.424	-0.502	0.464
Horizontally Differentiated	-0.217	-0.303	0.359	-0.331
Difference	0.520	0.726	-0.861	0.795

Notes: This table reports average normalized index values by horizontal differentiation and the difference in normalized values between horizontally and non-horizontally differentiated charter schools in the sample. The 'Skills' index takes into account whether a school's application indicates a college preparatory focus and extended math instruction. The 'Comportment' index considers whether the application indicates a focus on discipline, adherence to strict standards, and a strict dress code. The 'Well-Being' index takes into account whether the application indicates a focus on social and physical well-being and on cultural awareness. The "No Excuses" index is constructed from application philosophy and practices correlates reported in Angrist et al. (2013).

B Assigning Treatment Status

A key variable necessary to our analysis is the distance between each student's residence in the 2011-12 school year and all newly-opened charter schools. To construct this variable, we start with the 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, according to the 2010 Census definitions. Unfortunately, the residential data is not available for students attending charter schools, so students are not included in our sample if they attended a charter school in 2011-12.[46]

In the next step, we use the cartographic boundary shapefiles for U.S. Census block groups according to the 2010 boundary definitions[47] and get the longitude and latitude of the centroid of each block group. The centroid of the block group in which each student resides is then assigned as the residential location for that 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 (and range between 600 and 3,000 individuals) and a median size of about 2.2 square miles (with a range between 0.5 and 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 Appendix A for list of all newly-opened charters and their latitude and longitude coordinates). 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 student's residential census block group centroid is within 2.5 miles of the nearest newly-opened charter and a value of zero if the student's residential census block group centroid is between 2.5 and 5 miles away from the nearest newly-opened charter.

[46]This is a general data limitation we face: residential location data is not reported for students attending charter schools.

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

Once we have determined the distance between each student's residence and the nearest charter school, we restrict the sample to students for whom we observe at least one test score both before and after new charter schools enter in the 2012-13 academic year. After matching to test scores, we have a sample of 1,117,142 student-year observations covering 285,601 students in grades 3-6. Further restricting to students living within 5 miles of a newly-opened charter school, our sample consists 170,776 student-year observations covering 43,819 students.

The last data issue we address is the few instances of overlapping treatment and control regions. These few cases can be seen in Figure 2, which plots circles with a radius of 2.5 miles around each charter school in our sample. Students who live within these circles are treated in our main specifications and those who live between 2.5 and 5 miles of each charter school (i.e., the mid-point of each circle) are in the control group. Although we do not distinguish between treated and control students in Figure 2 for the sake of readability, one can see that some students living in the Charlotte area are treated by both a horizontally and non-horizontally differentiated charter school, while other students live in the treated region of one charter school but the control region of another.

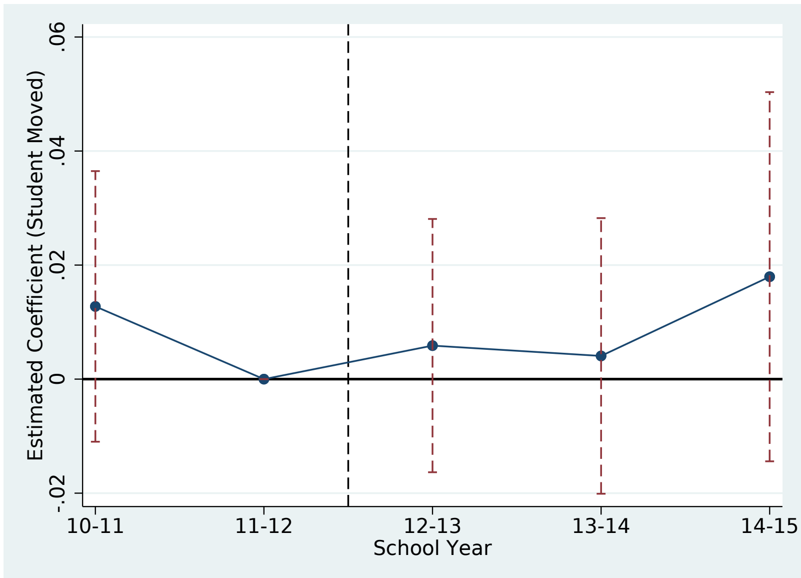
For students residing in these overlapping regions, we assign treatment using the *closest* newly-opened charter to their residence and drop all observations where there is another charter school of a different horizontal differentiation status within the 'treatment' region (i.e., within 2.5 miles of the student's residence) and all observations when there is another charter school with the same horizontal differentiation status within the 'control' region (i.e., between 2.5 and 5 miles of the student's residence).[48] This sample restriction eliminates 5,463 student-year observations (about three percent of our sample) leaving us with a final sample of 165,313 student-year observations covering 42,440 unique students.

[48] A similar sample restriction setup is implemented for Figure 5 whereby we drop all observations where there is another charter school of a different horizontal differentiation status within the 'treatment' region (i.e., within r miles of the student's residence) and all observations when there is another charter school with the same horizontal differentiation status within the 'control' region (i.e., between r and $2r$ miles of the student's residence).

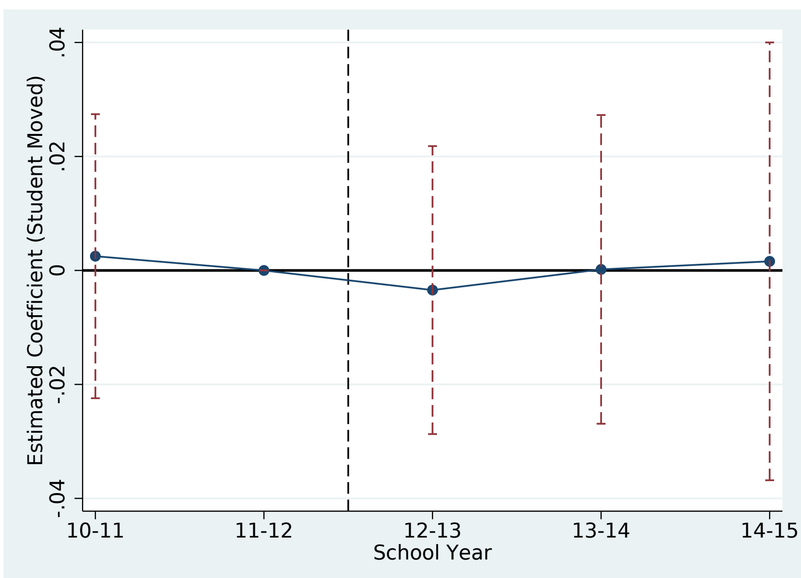
C Appendix Figures and Tables

Figure C.1: Robustness: Difference-in-Differences Results for Moving

(a) Non-Horizontally Differentiated

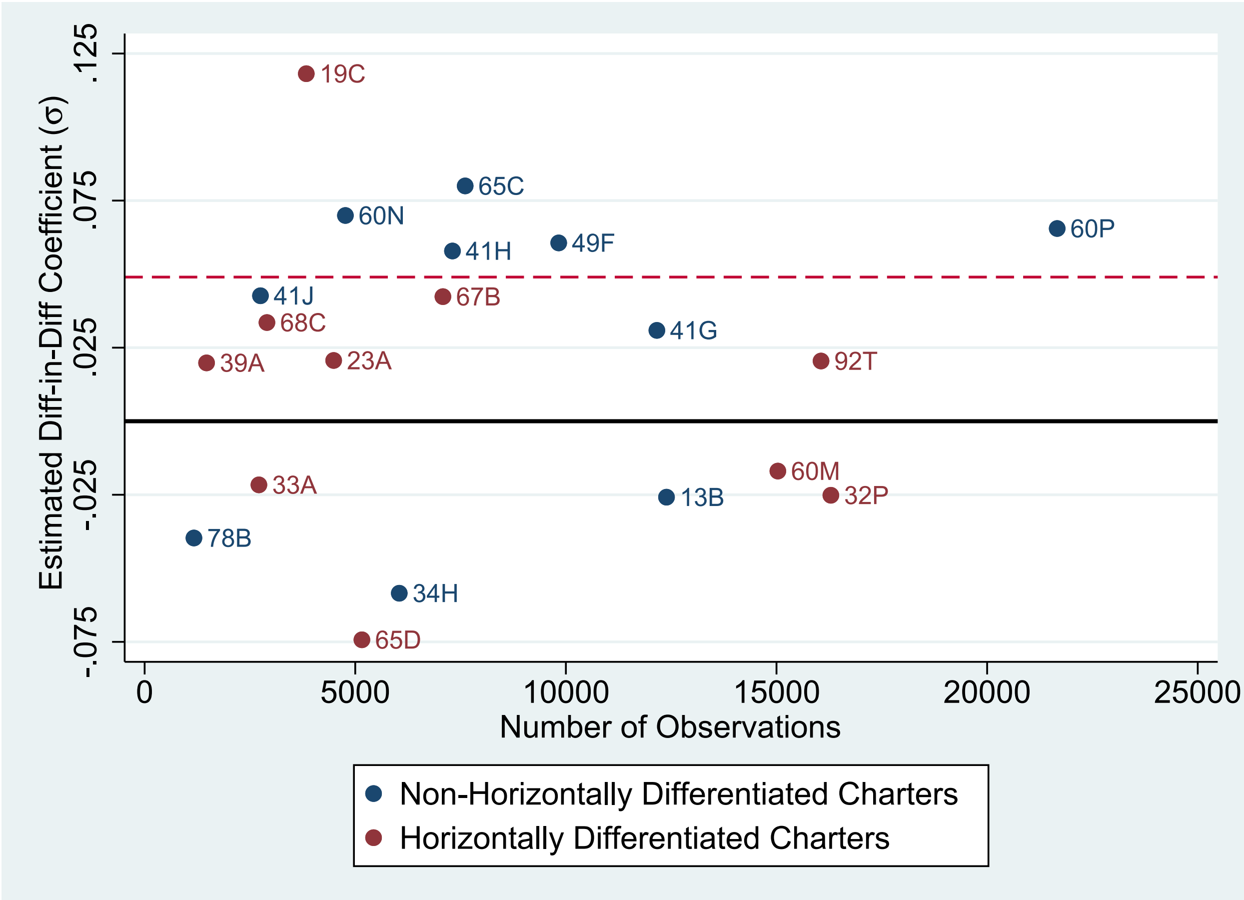


(b) Horizontally Differentiated



Notes: This figure shows a difference-in-differences estimate where the test score outcome from equation (3.2) has been replaced with a moving indicator as described in equation (4.1). A student is coded as having moved if their residential location has changed and is over one mile away from their residential location in the prior year. Treated areas are defined as neighborhoods within 2.5 miles of a charter school that opened in 2012-13 or 2013-14. Control areas are defined as neighborhoods between 2.5 and 5 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 C.2: Difference-in-Differences Estimates by Charter



Notes: This figure shows results from the difference-in-differences regression defined by equation (3.2) whereby students living within 2.5 miles of a newly-opened charter school are considered 'treated' while those living 2.5-5 miles from a newly-opened charter are considered 'control' for *each* charter school in our sample. These regressions include demographic controls and student fixed-effects (i.e., the set of controls from column (3) of Table 3). Three newly-opened charters are omitted due to a lack of observations creating extremely noisy point estimates (all three omitted charters have less than 100 student-year observations within a five mile radius). The labels represent the LEA codes of the newly-opened charter school, which can be matched to charter school names and locations in Table A.1.

Table C.1: Summary Statistics of Students by School Type: 2012-13 to 2015-16

	All Non-Charter Students	All Students in Pre-Expansion Charters	Students in Expansion Charters: Non-Horizontally Differentiated	Students in Expansion Charters: Horizontally Differentiated
	(1)	(2)	(3)	(4)
Math Score (standardized)	-0.006	0.134	0.208	-0.095
ELA Score (standardized)	-0.011	0.233	0.304	0.113
Percent White	50.4	59.1	66.3	54.2
Percent Black	25.3	26.4	19.3	26.1
Percent Hispanic	16.2	7.4	5.4	4.9
Percent Asian	3.0	2.6	5.9	10.7
Percent Disadvantaged	55.4	32.3	19.0	24.8
Percent with Disability	15.9	14.4	12.8	12.9
Percent Gifted	13.9	2.8	0.3	1.1
Observations (student-year)	1,312,788	56,299	4,115	3,538

Notes: Sample is restricted to grade 3-6 students during the school years 2012-13 through 2015-16. Column (1) shows summary statistics for students in public (non-charter) while column (2) displays summary statistics for students attending a 'pre-existing' charter that opened before the 2012-13 school year. Columns (3) and (4) then show summary statistics for students attending the 23 'newly-opened' charters that opened in the 2012-13 or 2013-14 school years, with the statistics subdivided into charters we label as 'non-horizontally differentiated' in column (3) and 'horizontally differentiated' in column (4).

Table C.2: Difference-in-Differences Results (English)

English Test Scores	'Treated' (0-2.5 miles) vs. 'Control' (2.5-5 miles) (1)	(2)	(3)	(4)
A. Pooled				
All Newly-Opened Charters	0.004	0.000	0.002	0.001
	(0.010)	(0.009)	(0.009)	(0.010)
B. Heterogeneous				
Non-Horizontally Differentiated	0.008	-0.005	0.001	-0.002
$(\beta_h + \beta_{nh-h})$	(0.015)	(0.013)	(0.012)	(0.013)
Horizontally Differentiated	0.000	0.006	0.004	0.004
(β_h)	(0.015)	(0.013)	(0.013)	(0.014)
Test of Equality by Differentiation Status p-value of H_0: $\beta_{nh-h} = 0$ vs. H_1: $\beta_{nh-h} \neq 0$	0.72	0.55	0.79	0.90
Demographic Controls	No	Yes	Yes	Yes
Student Fixed Effects	No	No	Yes	Yes
Census Block Group Time Trends (linear)	No	No	No	Yes
Observations (student-year)	164,084	164,084	164,084	164,084

Notes: This table shows difference-in-differences estimates from equation (3.2), whereby students living within 2.5 miles of a newly-opened charter school are considered 'treated' while those living 2.5-5 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. 'Test of Equality by Differentiation Status' reports the p-value of the hypothesis test that the point estimate for non-horizontally differentiated charters is the same as the one for horizontally differentiated charters; this is equivalent to testing the hypothesis of H_0: $\beta_{nh-h} = 0$ vs. H_1: $\beta_{nh-h} \neq 0$ in (3.2). 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 C.3: Difference-in-Differences Results: Continuous Treatment

	Continuous Treatment (restricted to $\leq$ 5 miles)			
Mathematics Test Scores	(1)	(2)	(3)	(4)
A. Pooled				
All Newly-Opened Charters	-0.007	-0.008*	-0.009**	-0.005
	(0.005)	(0.005)	(0.005)	(0.005)
B. Heterogeneous				
Non-Horizontally Differentiated	-0.011	-0.013*	-0.019***	-0.008
$(\beta_h + \beta_{nh-h})$	(0.008)	(0.007)	(0.006)	(0.006)
Horizontally Differentiated	-0.000	-0.002	0.001	-0.002
(β_h)	(0.007)	(0.007)	(0.007)	(0.005)
Test of Equality by Differentiation Status p-value of H_0: $\beta_{nh-h} = 0$ vs. H_1: $\beta_{nh-h} \neq 0$	0.28	0.23	0.03	0.84
Demographic Controls	No	Yes	Yes	Yes
Student Fixed Effects	No	No	Yes	Yes
Census Block Group Time Trends (linear)	No	No	No	Yes
Observations (student-year)	164,959	164,959	164,959	164,959

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 5 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. 'Test of Equality by Differentiation Status' reports the p-value of the hypothesis test that the point estimate for non-horizontally differentiated charters is the same as the one for horizontally differentiated charters; this is equivalent to testing the hypothesis of H_0: $\beta_{nh-h} = 0$ vs. H_1: $\beta_{nh-h} \neq 0$ in (3.2). 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 C.4: Bounding Competitive Effects

Mathematics Test Scores	Public-Charter Switchers Test Gains Set to Zero (1)	Public-Charter Switchers Test Gains Set to 5^{th} Percentile (2)	Public-Charter Switchers Test Gains Set to 95^{th} Percentile (3)
A. Pooled			
All Newly-Opened Charters	0.023**	0.015	0.030***
	(0.011)	(0.011)	(0.011)
B. Heterogeneous			
Non-Horizontally Differentiated	0.044**	0.043**	0.054***
$(\beta_h + \beta_{nh-h})$	(0.015)	(0.016)	(0.016)
Horizontally Differentiated	0.001	-0.004	0.002
(β_h)	(0.016)	(0.017)	(0.016)
Test of Equality by Differentiation Status p-value of H_0: $\beta_{nh-h} = 0$ vs. H_1: $\beta_{nh-h} \neq 0$	0.05	0.04	0.02
Demographic Controls	Yes	Yes	Yes
Student Fixed Effects	Yes	Yes	Yes
Observations (student-year)	164,964	164,964	164,964

Notes: This table attempts to bound the competitive effects of charters by making different assumptions on the test score gains of public-charter switchers when estimating equation (3.2). Column (1) sets the test score gains of public-charter switchers to zero, while columns (2) and (3) set the test score gains of public-charter switchers to the 5^{th} and 95^{th} percentile, respectively. Setting test score gains of public-charter switchers to the 5^{th} and 95^{th} percentile mimics the intuition behind Lee (2009) bounds. Students living within 2.5 miles of a newly-opened charter school are considered 'treated' while those living 2.5-5 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. 'Test of Equality by Differentiation Status' reports the p-value of the hypothesis test that the point estimate for non-horizontally differentiated charters is the same as the one for horizontally differentiated charters; this is equivalent to testing the hypothesis of H_0: $\beta_{nh-h} = 0$ vs. H_1: $\beta_{nh-h} \neq 0$ in (3.2). Regressions include the controls in column (3) of Table 3, which consist of grade and year fixed effects and demographic controls which incorporate 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 C.5: Characteristics of Public School Stayers and Public-Charter Switchers (2012-13 Third Grade Cohort)

Newly-Opened Charter is:	Non-Horizontally Differentiated		Horizontally Differentiated	
	Public Stayers	Public-Charter Switchers	Public Stayers	Public-Charter Switchers
	(1)	(2)	(3)	(4)
2012-13 Math Score (standardized)	0.032	0.371	-0.035	0.240
2012-13 ELA Score (standardized)	-0.009	0.384	-0.046	0.073
Percent White	41.2	75.8	33.2	48.6
Percent Black	32.3	13.3	40.2	40.4
Percent Hispanic	17.6	5.5	19.1	5.5
Percent Asian	4.8	4.2	3.1	0.9
Percent Disadvantaged	58.7	20.0	59.6	55.0
Percent with Disability	12.5	12.1	10.9	8.3
Percent Gifted	10.0	17.0	9.8	2.8
Observations (student-year)	6,467	165	5,966	109

Notes: This table shows the characteristics of students in the 2012-13 third grade cohort within five miles of a newly-opened charter school by whether they remained in the local public school or switched to the newly-opened charter school. The third grade cohort is chosen as this cohort switched at far higher rates that the fourth or fifth grade cohorts. Student characteristics are set at their values in the 2012-13 school year. Column (1) shows summary statistics for students that remained in the local public (non-charter) school while column (2) displays the characteristics of student that switched from the public school to the newly opened non-horizontally differentiated charter. Columns (3) and (4) do the same for when the newly-opened charter is horizontally differentiated.

Table C.6: Public-Charter Switch Rates by 2012-13 Grade

% Switching from Public to Newly-Opened Charter	Third Grade (1)	Fourth Grade (2)	Fifth Grade (3)
	2012-13 Grade		
Panel A. Non-Horizontally Differentiated			
Among 'Treated' ('Treated' Reside <2.5 miles from Charter)	3.29	2.70	0.54
Among 'Control' ('Control' Reside 2.5-5 miles from Charter)	2.14	2.18	0.38
Difference	1.15	0.52	0.16
Panel B. Horizontally Differentiated			
Among 'Treated' ('Treated' Reside <2.5 miles from Charter)	2.63	1.24	0.72
Among 'Control' ('Control' Reside 2.5-5 miles from Charter)	1.48	1.23	0.41
Difference	1.15	0.01	0.31

Notes: This table shows the percent of student that switched from a public school to a newly-opened charter by treatment status, where students residing 0-2.5 miles from the newly-opened charter are considered 'treated' while those 2.5-5 miles away are considered 'control.' Panels A and B split the sample by whether the newly-opened charter school is horizontally differentiated or not from the local public school as described by Section 2.2. The table shows the switching rates by grade of the student in the 2012-13 school year, which is the first year more than one hundred charters could operate in North Carolina.

Table C.7: Difference-in-Differences Results by 2012-13 Grade

	'Treated' (0-2.5 miles) vs. 'Control' (2.5-5 miles)		
	Third Grade	Fourth Grade	Fifth Grade
Mathematics Test Scores	(1)	(2)	(3)
Panel A. Pooled			
All Newly-Opened Charters	0.018	0.020	0.038**
	(0.018)	(0.015)	(0.018)
Panel B. Heterogeneous			
Non-Horizontally Differentiated	0.054**	0.048**	0.047***
$(\beta_h + \beta_{nh-h})$	(0.024)	(0.022)	(0.025)
Horizontally Differentiated	-0.022	-0.009	0.027
(β_h)	(0.028)	(0.021)	(0.024)
Test of Equality by Differentiation Status p-value of H_0: $\beta_{nh-h} = 0$ vs. H_1: $\beta_{nh-h} \neq 0$	0.04	0.06	0.56
Demographic Controls	Yes	Yes	Yes
Student Fixed Effects	Yes	Yes	Yes
Observations (student-year)	55,828	55,250	53,112

Notes: This table shows difference-in-differences estimates from equation (3.2) by grade. Grade is defined as the grade the student was in during the 2012-13 school year, which is the first year more than one hundred charters could operate in North Carolina. Students living within 2.5 miles of a newly-opened charter school are considered 'treated' while those living 2.5-5 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. 'Test of Equality by Differentiation Status' reports the p-value of the hypothesis test that the point estimate for non-horizontally differentiated charters is the same as the one for horizontally differentiated charters; this is equivalent to testing the hypothesis of H_0: $\beta_{nh-h} = 0$ vs. H_1: $\beta_{nh-h} \neq 0$ in (3.2). Each column represents a different regression and regressions include the controls in column (3) of Table 3, which represents the (enrollment-weighted) average effect over all grades. Regressions include grade and year fixed effects and 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 C.8: Difference-in-Differences Results: Defining Treatment by School Attended in 2011-12

	Attend 'Treated' School (0-2.5 miles) vs. 'Control' School (2.5-5 miles)			
Mathematics Test Scores	(1)	(2)	(3)	(4)
A. Pooled				
All Newly-Opened Charters	0.024**	0.009	0.028***	0.023*
	(0.012)	(0.010)	(0.010)	(0.012)
B. Heterogeneous				
Non-Horizontally Differentiated	0.053***	0.040***	0.055***	0.035*
$(\beta_h + \beta_{nh-h})$	(0.017)	(0.015)	(0.014)	(0.019)
Horizontally Differentiated	-0.006	-0.021	-0.000	0.015
(β_h)	(0.016)	(0.014)	(0.013)	(0.014)
Test of Equality by Differentiation Status p-value of H_0: $\beta_{nh-h} = 0$ vs. H_1: $\beta_{nh-h} \neq 0$	0.01	0.00	0.01	0.43
Demographic Controls	No	Yes	Yes	Yes
Student Fixed Effects	No	No	Yes	Yes
Census Block Group Time Trends (linear)	No	No	No	Yes
Observations (student-year)	173,430	173,428	173,428	173,428

Notes: This table shows difference-in-differences estimates from equation (3.2), whereby students attending a public school in 2011-12 within 2.5 miles of a newly-opened charter school are considered 'treated' while those attending a public school 2.5-5 miles from a newly-opened charter in 2011-12 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. 'Test of Equality by Differentiation Status' reports the p-value of the hypothesis test that the point estimate for non-horizontally differentiated charters is the same as the one for horizontally differentiated charters; this is equivalent to testing the hypothesis of H_0: $\beta_{nh-h} = 0$ vs. H_1: $\beta_{nh-h} \neq 0$ in (3.2). 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 by the 2011-12 census block group level. ***,** and * denote significance at the 1%, 5% and 10% levels, respectively.