

Horizontal Differentiation and the Policy Effect of Charter Schools[†]

By MICHAEL GILRAINE, UROS PETRONIJEVIC, AND JOHN D. SINGLETON*

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 removal of North Carolina’s statewide cap to compare test score changes for students who lived near entering charters to those farther away. We find learning gains that are driven by public schools responding to increased competition from non-horizontally differentiated charter schools, even before those charters actually open. (JEL H75, I21, I28)

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

*Gilraine: Department of Economics, New York University (email: mike.gilraine@nyu.edu); Petronijevic: Department of Economics, York University (email: upetroni@yorku.ca); Singleton: Department of Economics, University of Rochester (email: john.singleton@rochester.edu). C. Kirabo Jackson was coeditor for this article. We are grateful to Christian Buerger, Michael Dinerstein, David Figlio, Amy Ellen Schwartz, Miguel Urquiola, and seminar participants at Columbia, Rochester, Syracuse, and the 14th Urban Economics Association meetings for insightful comments. Samuel Frank provided excellent research assistance. We thank the North Carolina Education Research Data Center for providing the data. All remaining errors are our own.

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¹This 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 nonurban 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.

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 from a variety of contexts indicates that parents and students view schools as differentiated products² 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 skills through traditional instruction. This classification allows us to account for horizontal differentiation of charter programs in estimating the effect of charter expansion. While our classification is an *ex ante* measure of curriculum, this is likely the relevant margin in our setting since we

²See, for instance, Bayer, Ferreira, and McMillan (2007); Burgess et al. (2015); Arcidiacono et al. (2017).

³Evidence 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; Abdulkadiroğlu et al. 2017b).

are identifying responses that occur before or immediately after charter entry, when ex post curriculum choices are unobserved by the incumbent public schools.

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.

While these main findings are consistent with the demand for horizontally differentiated charter schools being unresponsive to adjustments in public school quality, they do not isolate the exact channel of influence. For instance, the aggregate gains caused by non-horizontally differentiated charter school expansion could stem from students switching to those schools, which may be higher value-added, rather than via the indirect channel. Likewise, changes in peer quality at public schools could attenuate student learning and confound the effect of competition, an issue highlighted by Hsieh and Urquiola (2006). As a result, we re-estimate our main specification but focus specifically on just the 2012–2013 impacts (the first year post-cap-lifting) of only those charter schools that opened *the next year*. In these cases, the charter schools have yet to open—so there cannot be any student sorting—but public schools know that charters will open the following year. This test is analogous to the natural experiment that Figlio and Hart (2014) leverage for isolating the effect of competition.⁴ We find that our main results are essentially

⁴Figlio and Hart (2014) similarly exploit the fact that, for a year following the expansion of a means-tested voucher program in Florida, students applied for vouchers the next year (exposing public schools to competition), but could not yet use them.

unchanged. That the effect appears even prior to these charter schools opening indicates it is driven by incumbent public schools responding to competitive pressure arising from horizontal position pre-entry.

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 private school 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 US induce competitive test score responses from traditional public schools remains an unsettled question, with mixed findings in the prior literature (see Epple, Romano, and Zimmer 2016 for a recent review). Our results, obtained from a new empirical strategy, suggest that this ambiguity stems in part from neglecting important differences among charter schools. We identify the mechanism driving the results as competition by showing that test scores of exposed students respond to non-horizontally differentiated charter school entry even before students can switch.⁵ Moreover, the school choice literature has largely focused on the quality and location dimensions of schools.⁶ Our findings, however, 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; Angrist, Pathak, and Walters 2013, Dobbie and Fryer 2013) and the equilibrium implications of behavioral differences across charter school types (Singleton 2019). 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 II. We present the main results including robustness checks in Section III before examining the interpretation of our findings in Section IV. Section V concludes.

⁵By showing traditional public schools respond to the threat of competitive pressure prior to charter schools commencing operations, our results are consistent with research on competition between public and private schools (Figlio and Hart 2014) and research on competition between airlines (Goolsbee and Syverson 2008).

⁶A notable exception is Bau (2019), who examines how schools in Pakistan competitively tailor material to different student populations.

I. Background and Data

North Carolina lifted its statewide cap on the number of charter schools in the state on June 6, 2011. Figure 1 displays the number of charters in North Carolina for school years 1996–1997 through 2016–2017. 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 2000–2001. 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 2012–2013 when the charter school cap was removed: Nine charter schools opened for the 2012–2013 school year, with another twenty-three approvals following in 2013–2014. By 2016–2017, 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.⁷

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; Abdulkadiroğlu et al. 2011; Angrist, Pathak, and Walters 2016; Dobbie and Fryer Jr. 2015; Abdulkadiroğlu et al. 2017a; Unterman 2017; Coen, Nichols-Barrer, and Gleason 2019; Davis and Heller 2019).⁸ These gains are pronounced at “No Excuses” charter schools (Angrist, Pathak, and Walters 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 the quality of education across the board (Hoxby 2002).⁹ 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

⁷By contrast, the 2011 Massachusetts charter school expansion, analyzed by Cohodes et al. (2019) and Ridley and Terrier (2018), was restricted to underperforming 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.

⁸Other 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 15 states and Washington, DC, 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.

⁹As we discuss in more detail below, we show that our results are driven by spillover effects on traditional public schools by focusing exclusively on the 2012–2013 impacts (the first year after the cap was lifted) of only those charter schools that opened the following school year. Doing so eliminates the possibility of estimating a direct effect (and eliminates concerns about student sorting), because the charter schools in question have yet to commence operations.

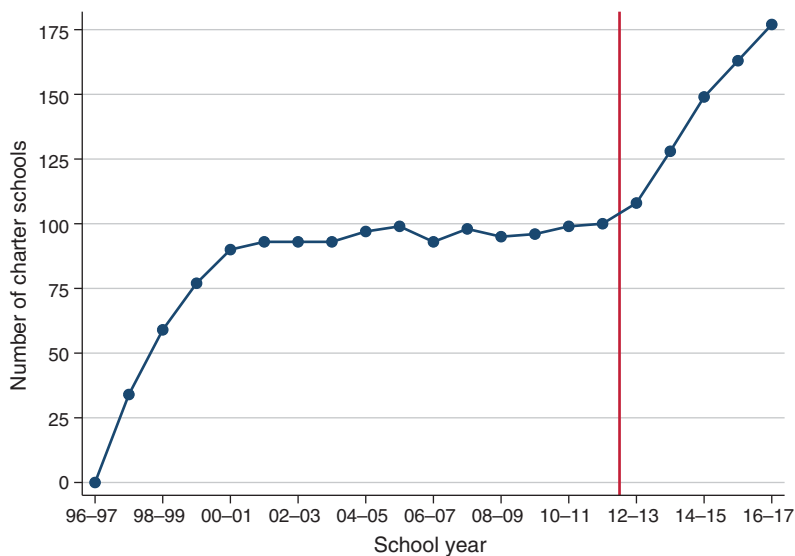


FIGURE 1. NUMBER OF CHARTER SCHOOLS IN NORTH CAROLINA BY YEAR

Notes: This figure displays the number of charter schools by year in North Carolina from 1996–1997 to 2016–2017, excluding two virtual charter schools that opened in 2015–2016. The vertical line represents the lifting of the 100 school charter cap for the 2012–2013 school year.

2006, Abdulkadiroğlu et al. 2017b), 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.”¹⁰ 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

¹⁰In 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.

is for the direct one and may partly explain the ambiguous conclusions in the prior literature.¹¹ Below, we formalize this intuition in a simple model that serves to motivate our subsequent empirical analysis.

A. 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. The term $D_c(q; \sigma)$ 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.¹² An immediate implication of this setup is that the public school would set quality at zero absent competition from the charter school.

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 rearranging, the solution is given by

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

¹¹Ferreira 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.

¹²This 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.

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.

B. 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.

Data Sources.—We use detailed, student-level administrative records from the North Carolina Education Research Data Center (NCERDC) (1995-2017). The records include information about all North Carolina public school students (charter and traditional public) for the 2009–2010 to 2014–2015 school years. The data contain test scores for each student in mathematics and reading on standardized end-of-grade exams in grades three through six, 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.¹³

In addition, we obtain information regarding students' residential locations in each school-year from the NCERDC. As we detail in the next section, this

¹³We also gather data for whether a student is repeating or skipping a grade.

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.¹⁴ 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 using data from the United States Census Bureau (2010).¹⁵ We restrict our dataset to students for whom we observe a valid test score both before and after the 2012–2013 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 2009–2010 through 2014–2015.

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.¹⁶ 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 online Appendix A, with the classification of individual charter schools provided in Table A.1.

Data Summaries.—Our data consist of twenty-three elementary charter schools that opened in either 2012–2013 and 2013–2014, 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 sixth grade.¹⁷ We divide these newly opened charters by their horizontal differentiation to traditional public schools. We

¹⁴Residential information for students in charter schools is not contained in the NCERDC data.

¹⁵The median area of a census block group in North Carolina is 2.2 square miles.

¹⁶One possible concern with classifying charter schools in this way is that they may not follow through with their expressed intentions after opening, e.g., potentially offering a different curriculum than the one that we originally categorized as horizontally or non-horizontally differentiated based on their application. To address this, one could classify schools based on the content subsequently contained on their websites (after they commence operations). As we document later, however, there is evidently strong signal content in the applications: the policy effect we estimate emerges in the 2012–2013 academic year even for the 17 charter schools in our sample that did not open until the following year. Prior to their opening, parents and traditional public schools (and members of the Charter Schools Advisory Board) only had access to the information in the charter schools' applications.

¹⁷In total, 32 charter schools opened following the cap removal, 9 of which were non-elementary schools. Of the 23 schools covering elementary grades, 6 planned to also cover grades above the sixth grade, 8 planned to cover up to grade six, and 9 planned to cover up to a grade less than sixth in their first year of operation.

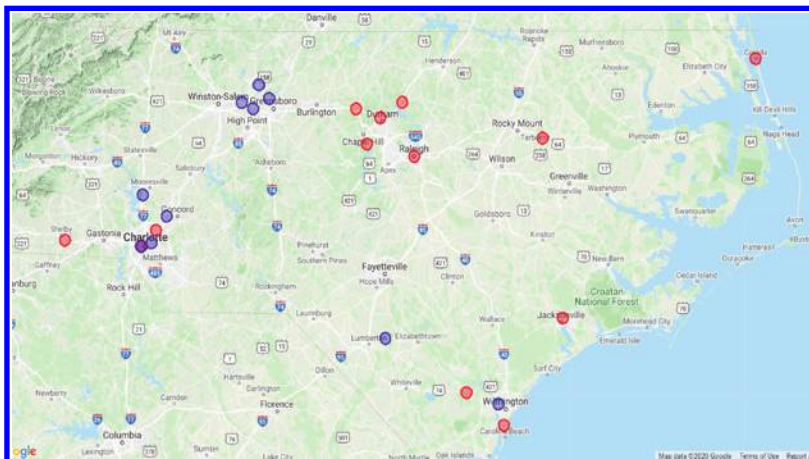


FIGURE 2. LOCATIONS OF CHARTER SCHOOLS OPENING IN 2012–2013 OR 2013–2014

Notes: Map of North Carolina (Map data: Google 2020). This map features circles with a 2.5-mile radius around the 23 charter schools in our data that opened in the 2012–2013 or 2013–2014 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 IB). 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 online Appendix B).

designate 13 schools as horizontally differentiated and 10 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.¹⁸

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

¹⁸Specifically, 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.

TABLE 1—SUMMARY STATISTICS

	All North Carolina students (1)	Students within five miles of:		
		Newly opened charters (2)	Non-horizontally differentiated (3)	Horizontally differentiated (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
Number 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 2012–2013 with a valid address in 2011–2012. 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 2009–2010 through 2014–2015. 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 five miles of the newly opened charter school, except for column 1, which reports enrollment-weighted value added of all public schools in North Carolina.

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 online Appendix Table D.1, where we document the average characteristics of students who attend each school type. Contrasting column 2 of online Appendix Table D.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 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, online Appendix Table D.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, online 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, Pathak, and Walters 2013).¹⁹ One caveat these differences raise for interpreting our findings, detailed in the next section, is that our results may not represent the effect of educational program differentiation per se, but also embed the effects of attributes that are correlated with curricular choice (although Section IVC shows that they are not picking up the effects of vertical differentiation). Section IVA investigates this possibility in-depth.

II. Research Design

Credibly estimating the effect of charter school expansion requires addressing three main empirical challenges. First, because students choose between attending a traditional public school or charter school, one must account for student selection into schools. Second, charter schools do not locate randomly within school districts, but rather select where to operate strategically (Singleton 2019). 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.

A. 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 initially attempting to identify the two effects separately, our approach relies on weaker assumptions about student selection than strategies used in prior work. We later use the fact that, in some cases, public schools knew one year in advance that the charter would enter the *following* year, allowing us to isolate the (pure) competitive effect of charter school expansion. 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

¹⁹While 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.

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.

B. Details

Our empirical analysis focuses on charter schools that opened in the immediate two years, 2012–2013 and 2013–2014, 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 17 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.²⁰ 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 2012–2013 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 2012–2013 school year, the first post-policy change year, regardless of when each charter school opened (2012–2013 or 2013–2014).²¹ 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 2011–2012 school year—the academic year before any new charter schools opened—is within r miles of one of the charter school entrants that opened in either 2012–2013 or 2013–2014. 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 online Appendix B

²⁰To be more specific about the timing for the first two waves of charter schools, schools hoping to open for the 2012–2013 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 2013–2014 academic year had to submit their application by April 2012 and were shortlisted in June 2012. Twenty-three of the 30 shortlisted schools were then approved in March 2013, at which point they began preparing to open in August 2013.

²¹The majority of charter schools in our sample (17 out of 23) did not open until 2013–2014. In Section IV we also estimate the effects of only those charter schools in 2012–2013—i.e., the year *before* they opened. Doing so eliminates the direct effect of charter schools, as students could not yet attend them, allowing us to hone in on solely the competitive response of traditional public schools.

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 2011–2012 academic year and our 23 entering charter schools indexed by c .²² Letting c_i^* indicate the closest such charter school to student i 's 2011–2012 residence, we define student i as treated by charter expansion when his or her 2011–2012 residence is within r miles of c_i^* :

$$(1) \quad \text{treat}_i^r = \begin{cases} 1, & \text{if } d(i, c_i^*) \leq r; \\ 0, & \text{otherwise.} \end{cases}$$

Our main treatment variable (treat_i^r) is best viewed as measuring an intention to treat, as students who move from their 2011–2012 residence after the policy change may not necessarily be treated by the charter school expansion that came after the policy change.²³ 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 to recover the effect of charter school expansion while allowing for potentially differential effects across horizontally and non-horizontally differentiated schools:

$$(2) \quad y_{isgt} = \alpha + \delta_g + \lambda_t + \zeta X_{isgt} + \mu_h \text{treat}_i^r + \phi \text{Post}_t + \beta_h \text{Post}_t \times \text{treat}_i^r \\ + NH_i (\alpha_{nh-h} + \delta_{g,nh-h} + \lambda_{t,nh-h} + \zeta_{nh-h} X_{isgt} + \mu_{nh-h} \text{treat}_i^r \\ + \phi_{nh-h} \text{Post}_t + \beta_{nh-h} \text{Post}_t \times \text{treat}_i^r) + \epsilon_{isgt}.$$

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, socioeconomically disadvantaged status, and grade skipping or repeating status. The variable Post_t indicates that the observation is from the academic year 2012–2013 or later. To ensure that treated and untreated students are as comparable as possible, we restrict the analysis sample to students whose 2011–2012 residence is within $2r$ miles of their

²² As mentioned, we restrict our sample to students who are in third to sixth grade for whom we observe at least one test score before and after 2012–2013. 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 nine (out of twenty-three) covered sixth grade (with all but four of the remaining nine planning to cover sixth grade after one or two years of operation).

²³ 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.

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.²⁴ We cluster the standard errors at the census block level.²⁵

The parameters β_h and β_{nh-h} are the main parameters of interest in equation (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 effect relative to β_h) when students are treated by non-horizontally differentiated charter schools. The sum $\beta_h + \beta_{nh-h}$ 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.²⁶

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.²⁷ Students are treated (i.e., exposed to charter school expansion) simply if their 2011–2012 residence is sufficiently close to a charter school that opens in the post-policy change period.

²⁴We discuss our choice of r below, and we also show that our main results are robust to alternative choices of r in Subsection IIIB.

²⁵Alternatively, we have clustered standard errors at the student and school district levels. Standard errors clustered at the census block level are the most conservative of these options.

²⁶For 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.

²⁷Our 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. Online Appendix Table D.4 mimics Cordes (2018) by assigning treatment based on pre-charter expansion public school rather than residence.

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 sudden timing, it is possible that students anticipated the new charter school openings and moved across neighborhoods prior to the 2012–2013 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 preexisting performance trend rather than the effect of charter school expansion. Relatedly, students whom we define as untreated (according to their 2011–2012 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.

By remaining agnostic as to whether a student remained in their traditional public school or switched into a charter school, we credibly identify the aggregate effect of charter school expansion. Student sorting patterns, however, have implications for which mechanisms drive the aggregate effect. For example, if high-achieving students leave traditional public schools for charter schools (as we show is the case below), then spillover effects on traditional public schools could represent a composite of a positive effect on student learning through competitive responses and a negative effect through worse peer quality. We address such concerns over the interpretation of our estimates in Section IV, where we estimate the effects in 2012–2013 of only those charter schools that opened one year later in 2013–2014. Doing so provides us with a setting in which public schools knew charter schools would locate nearby soon, but their students could not yet switch schools, allowing us to credibly separate the competitive effect from changes in peer quality while also eliminating the potential for direct effects of charter schools on student performance.

Charter School Location Choice: By fixing treatment status according to students' residences in 2011–2012 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.²⁸

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 preexisting 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

²⁸ Arcidiacono et al. (2020), 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.

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 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 (2)) allows for this heterogeneity by uniquely drawing on information from entrants' applications to open. While our classification measures charter schools' planned curricula pre-entry (rather than actual charter school curricula), our empirical setting relies on identifying effects before or immediately after charter entry when ex post curriculum choices are likely unobserved by the incumbent public schools.

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 (1). As Table 2 demonstrates, however, nontrivial proportions of students transfer from traditional public schools to charter schools when their place of residence (in 2011–2012 academic year) is both closer to and farther away from newly opened charter schools. Among students observed attending a public school in 2011–2012 and living in a residence that is within 2 miles of any newly opened charter school, 2.6 percent transferred to a charter school by the 2013–2014 academic year.²⁹ As the distance between student residence and the charter school increases, the proportion of students transferring monotonically declines, with only 0.3 percent of students living between 10 and 15 miles of a 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.

III. Results

A. Main Results

Before presenting our main difference-in-differences estimates from equation (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

²⁹ Among students who lived within two miles of non-horizontally and horizontally differentiated charter schools, respectively, 2.8 and 2.4 percent transferred to a charter school of each type by the 2013–2014 academic year.

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 (1)	2–4 miles (2)	4–6 miles (3)	6–8 miles (4)	8–10 miles (5)	10–15 miles (6)
All newly opened charters	2.57	1.51	0.99	0.66	0.44	0.34
Non-horizontally differentiated	2.78	1.54	1.07	0.62	0.48	0.28
Horizontally differentiated	2.36	1.82	0.88	0.71	0.38	0.38
Observations (non-horizontally)	4,345	11,394	18,753	22,248	24,147	16,641
Observations (horizontally)	4,366	12,305	12,387	15,106	17,443	20,013

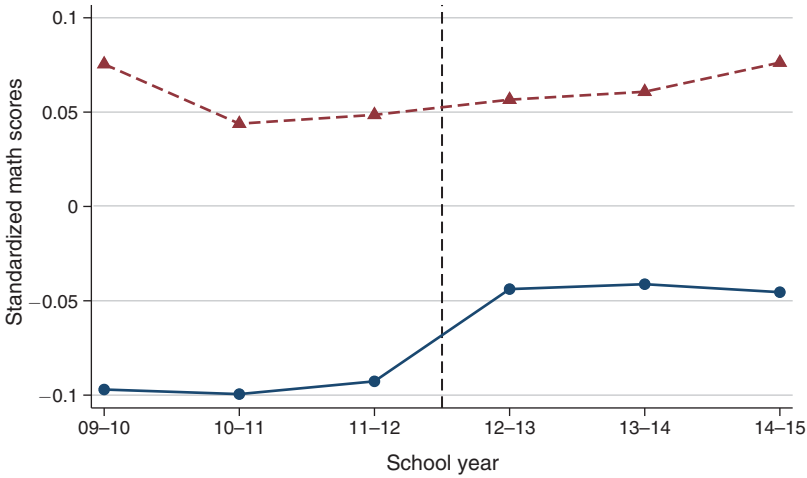
Notes: This table shows the proportion of students in the 2013–2014 school year whose 2011–2012 residence is within a given distance band of charter schools that opened in the 2012–2013 and 2013–2014 school years and who switched from a public school to a newly opened charter school. The data is then further subdivided into students within the distance band of non-horizontally and horizontally differentiated charter schools. Due to data constraints (see Section IB), 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 2013–2014 school year who attended a *public school* in the 2011–2012 school year. This data may therefore not be representative of the general population of charter school attendees.

2011–2012 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, panel A) and horizontally differentiated (Figure 3, panel 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 2012–2013, 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 2012–2013 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 (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 is 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

Panel A. Non-horizontally differentiated



Panel B. Horizontally differentiated

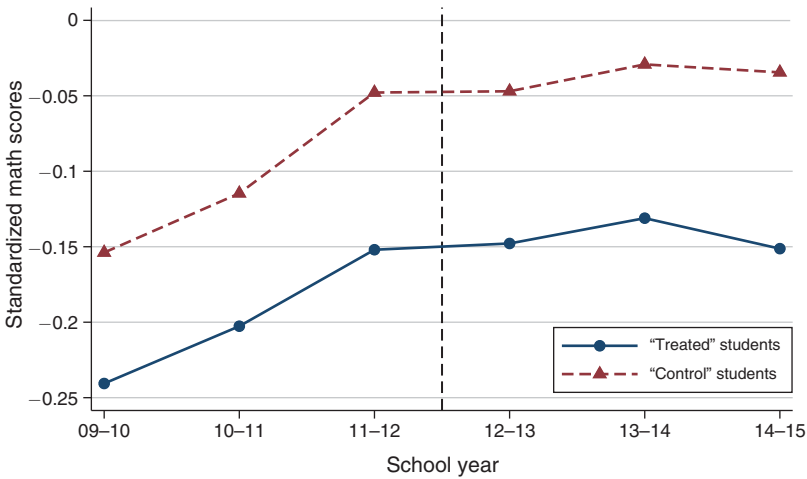


FIGURE 3. TEST SCORE TRENDS OVER TIME BY “TREATMENT” AND “CONTROL”

Notes: This figure shows raw test scores in math over time for “treated” and “control” students. We define students as “treated” if they live within 2.5 miles of a charter school that opened in 2012–2013 or 2013–2014. “Control” students are defined as students living between 2.5 and 5 miles of a charter school that opened in 2012–2013 or 2013–2014. Results are subdivided by whether the nearby charter was horizontally differentiated or not from the local public school as described in Section IB. The dashed vertical line separates the years before the charter opened from the years after the charter opened. Note that we always consider 2012–2013 to be the year the charter opened because although the charters themselves opened in either 2012–2013 or 2013–2014, public schools would have known by the start of 2012–2013 whether or not a charter was opening nearby in either 2012–2013 or 2013–2014.

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

TABLE 3—DIFFERENCE-IN-DIFFERENCES RESULTS

Mathematics test scores	“Treated” (0–2.5 miles) versus “Control” (2.5–5 miles)			
	(1)	(2)	(3)	(4)
<i>Panel A. Pooled</i>				
All newly opened charters	0.019 (0.013)	0.016 (0.012)	0.025 (0.011)	0.023 (0.013)
<i>Panel B. Heterogeneous</i>				
Non-horizontally differentiated ($\beta_h + \beta_{nh-h}$)	0.043 (0.018)	0.035 (0.017)	0.049 (0.015)	0.038 (0.020)
Horizontally differentiated (β_h)	-0.008 (0.017)	-0.005 (0.016)	-0.003 (0.016)	0.007 (0.015)
Test of equality by differentiation status p -value of $H_0: \beta_{nh-h} = 0$ versus $H_1: \beta_{nh-h} \neq 0$	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 (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 IB. 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 $H_0: \beta_{nh-h} = 0$ versus $H_1: \beta_{nh-h} \neq 0$ in (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, socioeconomically disadvantaged status, gifted status, disability designation and an indicator if the student is repeating or skipping a grade. Standard errors are clustered at the 2011–2012 census block group level.

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.³⁰

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.

B. 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

³⁰In online Appendix Table D.2, we examine treatment effects overall and by charter type on English language test scores and find no effects across the board.

in student performance across neighborhoods. We also consider several alternative ways of defining treatment status.

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.³¹ 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 D.1 in online Appendix D, which plots the results, is constructed by estimating the following equation:

$$\begin{aligned}
 (3) \quad m_{isgt} = & \alpha + \delta_g + \lambda_t + \zeta X_{isgt} + \mu_h \text{treat}_i^r + \beta_h^{2010-2011} \mathbf{1}_{\{\text{year}=2010-2011\}} \times \text{treat}_i^r \\
 & + \sum_{t=2012-2013}^{2014-2015} \beta_h^t \mathbf{1}_{\{\text{year}=t\}} \times \text{treat}_i^r \\
 & + NH_i \left(\alpha_{nh-h} + \delta_{g,nh-h} + \lambda_{t,nh-h} + \zeta_{nh-h} X_{isgt} + \mu_{nh-h} \text{treat}_i^r \right. \\
 & \quad \left. + \beta_{nh-h}^{2010-2011} \mathbf{1}_{\{\text{year}=2010-2011\}} \times \text{treat}_i^r \right. \\
 & \quad \left. + \sum_{t=2012-2013}^{2014-2015} \beta_{nh-h}^t \mathbf{1}_{\{\text{year}=t\}} \times \text{treat}_i^r \right) + \epsilon_{isgt},
 \end{aligned}$$

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.³² We then plot the estimated β_h^t and $\beta_h^t + \beta_{nh-h}^t$ terms (in

³¹ 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 2011–2012 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.

³² The last academic year before the policy change (2011–2012) 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 2009–2010 academic year) in this regression. Although it is possible to

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 online Appendix Figure D.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 D.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 (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 of Table 3. The results are very similar to the main estimates presented in columns 1 and 2, again implying that students exposed 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.

Charter School Location Choice.—Another concern is that our identification strategy is potentially vulnerable to charter schools choosing to locate in neighborhoods based on preexisting 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

calculate a value for the dependent variable in 2009–2010, we opt not to because the 2008–2009 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.

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:

$$\begin{aligned}
 (4) \ y_{isgt} = & \alpha + \delta_g + \lambda_t + \zeta X_{isgt} + \mu_h treat_i^r + \sum_{t=2009-2010}^{2010-2011} \beta_h^t \mathbf{1}_{\{year=t\}} \times treat_i^r \\
 & + \sum_{t=2012-2013}^{2014-2015} \beta_h^t \mathbf{1}_{\{year=t\}} \times treat_i^r \\
 & + NH_i \left(\alpha_{nh-h} + \delta_{g,nh-h} + \lambda_{t,nh-h} + \zeta_{nh-h} X_{isgt} + \mu_{nh-h} treat_i^r \right. \\
 & \quad \left. + \sum_{t=2009-2010}^{2010-2011} \beta_{nh-h}^t \mathbf{1}_{\{year=t\}} \times treat_i^r \right. \\
 & \quad \left. + \sum_{t=2012-2013}^{2014-2015} \beta_{nh-h}^t \mathbf{1}_{\{year=t\}} \times treat_i^r \right) + \epsilon_{isgt},
 \end{aligned}$$

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) 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 (2) but additionally include neighborhood-specific time trends in the specification. Because we use the distance between each student’s 2011–2012 residence and the newly opened charter schools to define treatment, we record the census block group in which each student resided in the 2011–2012 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 preexisting test score trends is unlikely to be driving our results.

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

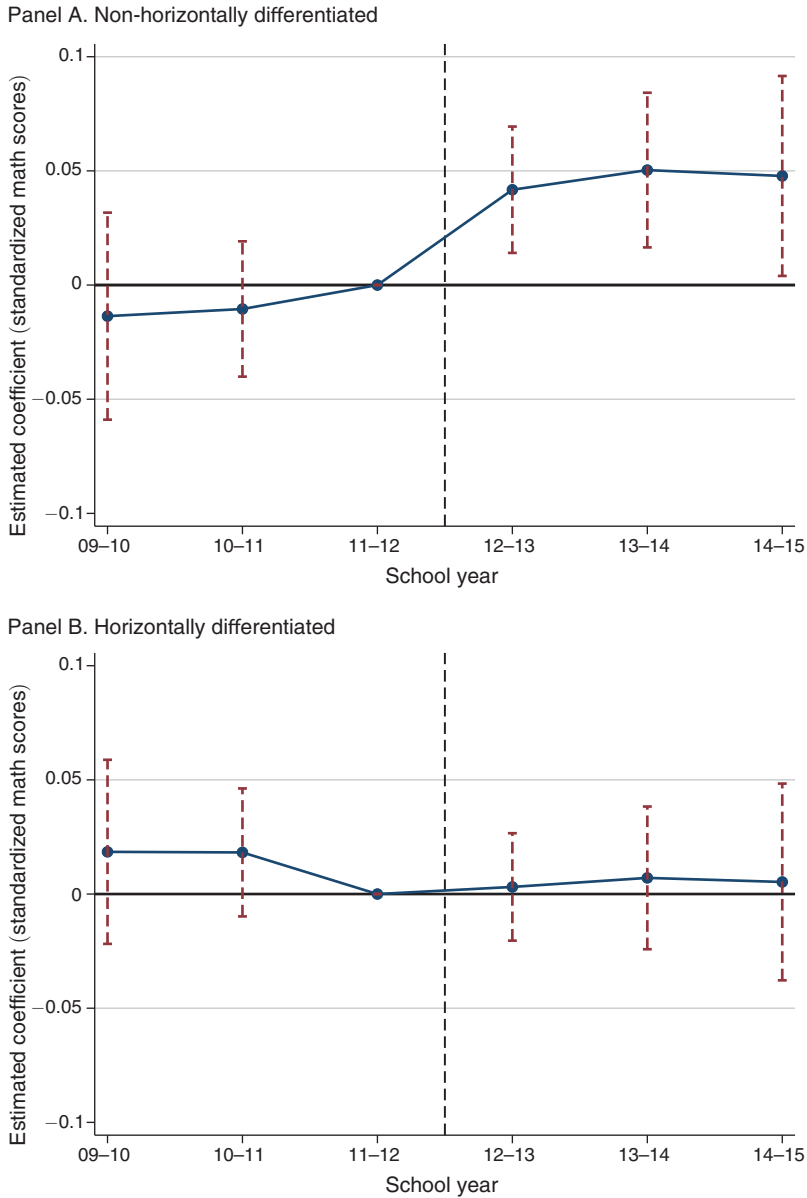


FIGURE 4. DIFFERENCE-IN-DIFFERENCES RESULTS BY YEAR AND CHARTER TYPE

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). Treated students are defined as students living within 2.5 miles of a charter school that opened in the 2012–2013 or 2013–2014 school year. Control students are defined as students living between 2.5 and 5 miles of a charter school that opened in the 2012–2013 or 2013–2014 school year. Results are subdivided by whether the nearby charter was horizontally differentiated or not from the local public school as described in Section IB. Note that 2012–2013 is considered the first “treated” year, because although the charters themselves opened in either the 2012–2013 or 2013–2014 school year, public schools would have known by the start of 2012–2013 whether or not a charter was opening nearby or would open nearby in 2013–2014. 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 student, grade and year fixed effects are included. The dashed “whiskers” represent 90 percent confidence intervals with standard errors clustered at the census block group level.

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 6 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 6 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 D.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' 2011–2012 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.³³

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 23 estimates

³³ Among treated students in our main specification, the average distance between their residences and the nearest non-horizontally differentiated charter school is 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.

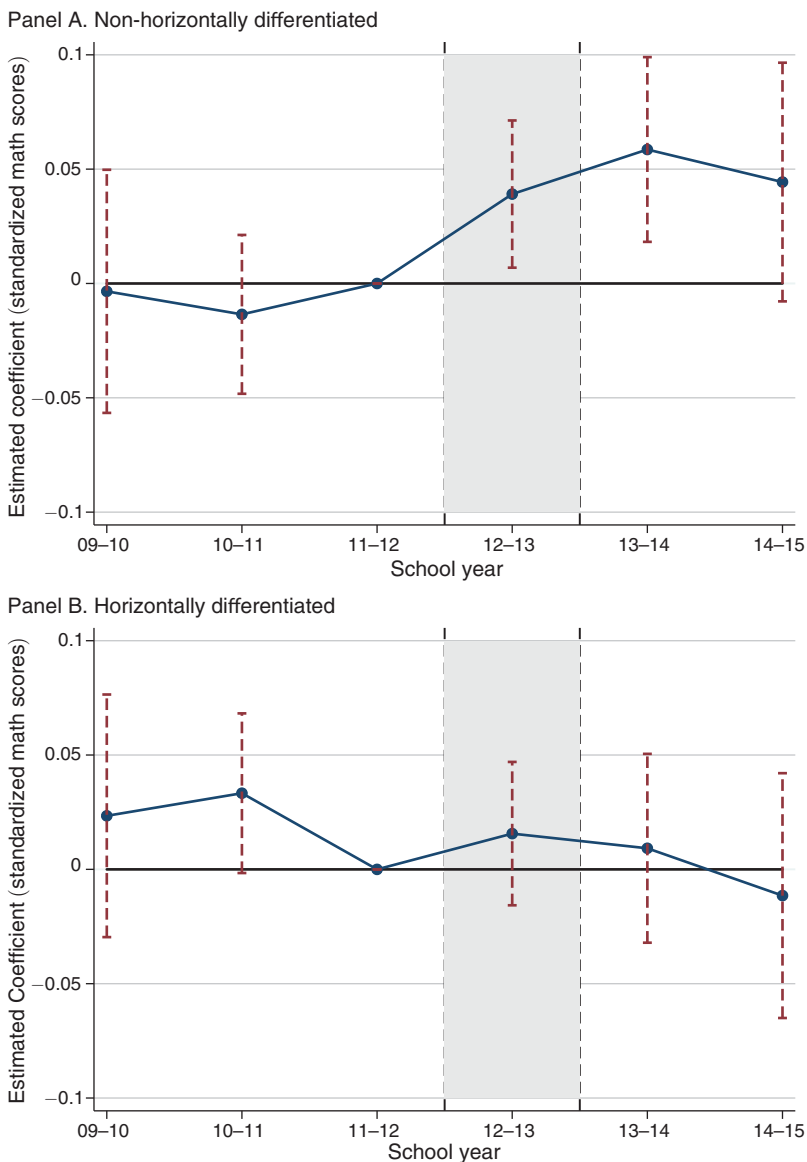


FIGURE 5. DIFFERENCE-IN-DIFFERENCES RESULTS BY YEAR AND CHARTER TYPE: “LATE” OPENERS ONLY

Notes: This figure restricts to the seventeen (out of our sample of 23) charter schools that applied using the normal (rather than “fast track”) application process after the charter cap was lifted and shows the estimated difference between students “treated” by a newly opened charter relative to “control” students by year as described in equation (4). For these schools, nearby public schools knew in 2012–2013 whether the charter school would open nearby, but the charter did not open until the 2013–2014 school year (which is represented by the shaded area in the figures). Since the charter had yet to open, there cannot be any sorting of students from public schools to charters at this point in time, and so any effects must reflect a productive response by local public schools to anticipated competition (rather than the direct effect of charters or changing peer composition). In addition, as the charter schools had yet to open, differential responses by charter curriculum can only be driven by the ex ante curriculum choices of charter schools that are available in the charter school applications (rather than the curriculum charters actually implement). Results are subdivided by whether the nearby charter was horizontally differentiated or not from the local public school as described in Section IB. The horizontal line represents a point estimate of zero. Demographic controls along with student, grade, and year fixed effects are included. The dashed “whiskers” represent 90 percent confidence intervals with standard errors clustered at the census block group level.

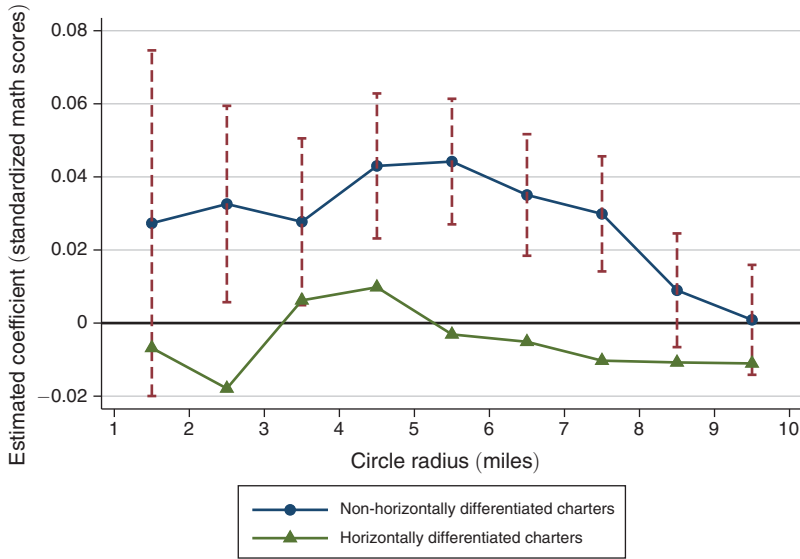


FIGURE 6. ROBUSTNESS: DIFFERENCE-IN-DIFFERENCES RESULTS BY CHARTER TYPE FOR DIFFERENT TREATMENT DEFINITIONS

Notes: This figure shows sensitivity of our main result in equation (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 2011–2012 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.

of the effect charter school expansion.³⁴ We then plot the estimated effect for each charter school against the number of observations in Figure D.2.³⁵ Consistent with our main findings, 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 subsamples 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.

³⁴ Each regression includes demographic controls and student fixed effects (i.e., the set of controls from column 3 of Table 3).

³⁵ Three charters are omitted from online Appendix Figure D.2 due to extremely noisy estimates (all three omitted charters have less than 100 student-year observations within a five mile radius).

IV. 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, and that these responses are unlikely to be attenuated (or amplified) by student sorting and changing peer quality at traditional public schools. In the second subsection, we show that the 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.

A. *Spillover versus Direct Effects and the Role of Peer Quality*

In this subsection, we address two related questions pertaining to the mechanisms driving our main effects. First, the aggregate effect we estimate represents a combination of the direct and indirect effect of charter schools. 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. Yet for fully understanding the policy implications of our findings, it is important to disentangle direct and spillover effects. Second, spillover impacts on public schools could themselves 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). That is, if peer effects are important for student learning, and high-achieving students leave traditional public schools for charter schools, any spillover effects 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 use a unique feature of our policy environment to shed light on both issues. As mentioned in Section IIB, charter schools applying in the time period immediately following the removal of the state cap could apply using the "fast track" or normal application process. Those applying using the normal application process (17 of the 23 charter schools in our sample) had to submit their applications by April 2012 (and were short-listed in June 2012) but could not open until the 2013–2014 academic year. This timing implies that local traditional public schools knew during the 2012–2013 academic year that new charter schools would locate in close proximity to them the following year. Importantly, however, these charters did not operate in 2012–2013, implying that any effects found in that year must solely represent spillover effects on traditional public schools. Further, because students attending traditional public schools could not yet switch to the charter schools that would open a year later, any spillover effects must reflect a productive response to anticipated competition and not changing peer composition.³⁶

³⁶Figlio and Hart (2014) take advantage of a similar setup in Florida, where public schools felt competitive pressure before students could move because access to private school vouchers would become available the following year.

Building on this logic, Figure 5 replicates the results in Figure 4 but restricts the sample to charter schools that opened in 2013–2014. The results across the two figures are nearly identical. Importantly, the results in Figure 5 clearly indicate that effects on test scores from non-horizontally differentiated charter schools arise in 2012–2013, the year *before* these schools opened. As before, there are no effects from horizontally differentiated schools.

Table 4 presents the corresponding regression estimates from our main estimating equation (equation (2)) after restricting the sample to only student observations near the charter schools in our sample that opened in 2013–2014 and, importantly, restricting the sample period to end in the 2012–2013 academic year. These restrictions imply that the main parameters of interest represent the effects of the two types of charter schools *before* they opened. As such, the estimates do not include the direct effect of charter schools or peer effects driven by student sorting out of public schools. Yet the point estimates are nearly identical to those reported in Table 3 for the full sample, implying that our main results are likely driven entirely by traditional public schools exhibiting a productivity response to competitive pressure.³⁷

These results strongly suggest that the effect we estimate of non-horizontally differentiated charter school expansion consists entirely of a competitive response. We further emphasize this point with supplemental analyses on our full sample (which includes a time period when all charter schools operate and students can switch schools) in online Appendix C. We first rule out direct effects in the full sample by showing that, despite non-horizontally differentiated charter schools having slightly higher average value-added than traditional public schools, there are too few students in our sample who switch to charter schools for the direct effect to meaningfully contribute to our main estimates. We underscore this point by estimating nearly identical effects after recoding the test score gains of students who switch from public schools to the newly opened charter schools to zero and thus shutting down the direct effect channel. We also show in online Appendix C that students who eventually switch from public schools to both non-horizontally and horizontally differentiated charter schools are positively selected relative to students who stay, but that the magnitude of peer effects in the full sample would have to be implausibly large for changing peer composition to confound the competitive effect we estimate.

B. *What Are Public Schools Responding To?*

Features Correlated with Curriculum Differentiation: While our classification of charters aims to capture differentiation from public school instruction, charter curriculum is correlated with other forms of differentiation. Specifically, Table A.2 shows that non-horizontally differentiated schools are (i) more likely to be “No Excuses,” (ii) more likely to focus on comportment, (iii) less likely to focus on well-being, and (iv) more likely to focus on academics. Given the correlation

³⁷ Given that we do not observe any significant shifts in class sizes or teachers in these public schools (results 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).

TABLE 4—DIFFERENCE-IN-DIFFERENCES RESULTS IDENTIFIED FROM RESPONSES **BEFORE** CHARTER SCHOOL OPENINGS

Mathematics test scores	“Treated” (0–2.5 miles) versus “Control” (2.5–5 miles)			
	(1)	(2)	(3)	(4)
<i>Panel A. Pooled</i>				
All newly opened charters	0.028 (0.013)	0.026 (0.014)	0.027 (0.015)	0.025 (0.018)
<i>Panel B. Heterogeneous</i>				
Non-horizontally differentiated ($\beta_h + \beta_{nh-h}$)	0.047 (0.017)	0.046 (0.018)	0.046 (0.018)	0.037 (0.026)
Horizontally differentiated (β_h)	0.001 (0.019)	-0.004 (0.022)	0.002 (0.023)	0.007 (0.021)
Test of equality by differentiation status <i>p</i> -value of $H_0: \beta_{nh-h} = 0$ versus $H_1: \beta_{nh-h} \neq 0$	0.08	0.07	0.10	0.25
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)	84,085	84,085	84,085	84,085

Notes: This table redoes the difference-in-differences estimates from equation (2) shown in Table 3, but restricts the data to the seventeen (out of our sample of 23) charter schools that applied using the normal (rather than “fast track”) application process after the charter cap was lifted *and* up to the first post-policy year (i.e., 2009–2010 to 2012–2013). For these schools, nearby public schools knew in 2012–2013 whether the charter school would open nearby but the charter did not open until the 2013–2014 school year. Since the charter had yet to open in the data here, there cannot be any sorting of students from public schools to charters at this point in time and so any effects must reflect a productive response by local public schools to anticipated competition (rather than the direct effect of charters or changing peer composition). In addition, as the charter schools had yet to open, differential responses by charter curriculum can only be driven by the ex ante curriculum choices of charter schools that are available in the charter school applications (rather than the curriculum charters actually implement). About 60 percent of total observations come from non-horizontally differentiated charters with the remaining 40 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 $H_0: \beta_{nh-h} = 0$ versus $H_1: \beta_{nh-h} \neq 0$ in (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, socioeconomically disadvantaged status, gifted status, disability designation, and an indicator if the student is repeating or skipping a grade. Standard errors are clustered at the 2011–2012 census block group level.

between charters’ curriculum and these other policies it may be that public schools respond to charters that utilize these policies rather than curriculum differentiation.

We investigate this possibility in online Appendix Table A.3. To do so, we define the non-horizontally differentiated status of charter schools using the correlations found in online Appendix Table A.2. For instance, column 1 defines “horizontal differentiation” based on “No Excuses” philosophy and practices as described in Angrist, Pathak, and Walters (2013). We find that there are no significant differences in student achievement by differentiation status when “non-horizontally differentiated” is defined as charters following “No Excuses.” Similarly, we see no major differences when defining “non-horizontally differentiated” as not focusing on emotional or physical well-being.

We do see a large difference in math test scores by differentiation status when we define “non-horizontally differentiated” as charters focusing on academic skills. We would argue that this supports our hypothesis, however, as a focus on academic skills effectively captures a charter focusing on math and English, which are the cornerstone of the public school curriculum. Indeed, having a curriculum focus sim-

ilar to public schools (i.e., “non horizontally differentiated” charters) and a focus on academic skills are highly correlated, and therefore both definitions are likely picking up that public schools respond to charters with similar academic focus.³⁸

Intended versus Offered Curriculum: Traditional public schools responding to the *anticipated* opening of non-horizontally differentiated charter schools lends credence to our method for classifying charter school curriculum differentiation. While we use the information available in charter school applications in our classification system, charter schools may not follow through with their expressed intentions after opening, instead offering a different curriculum than the one that we originally label as horizontally or non-horizontally differentiated. If these *ex post* curriculum choices are correlated with the information contained in the applications, we would be incorrectly attributing the public school response to the curriculum choices expressed in the applications.

The results above, however, imply that this is not the case, as traditional public schools do indeed respond to the signal content contained in charter school applications. The policy effect we estimate emerges in the 2012–2013 academic year even for the 17 charter schools in our sample that did not open until the following year. Because traditional public schools only had access to the charter schools’ applications at the time of responding, these estimates strongly suggest that they are primarily reacting to the information contained in these applications.

C. 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 (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 2011–2012 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 online Appendix Table D.4. 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

³⁸ Alternatively, we could control for these different charter school focuses directly in equation (2). Doing so, we find $\beta_h + \beta_{nh-h} = 0.034$ and $\beta_h = -0.006$, with the difference between these coefficients being significant at the 10 percent level. Those results are remarkably similar to those we find in column 2 of Table 3 (student fixed effects cannot be included as they are collinear with the various charter school focuses).

school, the effect of charter school exposure does not vary across students by their proximity to the charter.³⁹

D. Vertical Differentiation

While the preceding results indicate that the indirect channel is the principal source of aggregate gains, we examine whether it is horizontal or *vertical* differentiation of charter schools that accounts for competitive effects. As online Appendix Figure C.1 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):

$$\begin{aligned}
 (5) \quad y_{isgt} = & \alpha + \delta_g + \lambda_t + \zeta X_{isgt} + \mu_h treat_i^r + \phi Post_t + \beta_h Post_t \times treat_i^r \\
 & + NH_i(\alpha_{nh-h} + \delta_{g,nh-h} + \lambda_{t,nh-h} + \zeta_{nh-h} X_{isgt} + \mu_{nh-h} treat_i^r \\
 & \quad + \phi_{nh-h} Post_t + \beta_{nh-h} \times Post_t \times treat_i^r) \\
 & + \nu_h^1 VA_{ic} + \nu_h^2 VA_{ic} \times treat_i^r + \nu_h^3 VA_{ic} \times Post_t + \nu_h^4 VA_{ic} \times treat_i^r \times Post_t \\
 & + NH_i(\nu_{nh-h}^1 VA_{ic} + \nu_{nh-h}^2 VA_{ic} \times treat_i^r + \nu_{nh-h}^3 VA_{ic} \times Post_t \\
 & \quad + \nu_{nh-h}^4 VA_{ic} \times treat_i^r \times Post_t) + \epsilon_{isgt}.
 \end{aligned}$$

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 5 reports the results from estimating equation (5). 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

³⁹These results are available upon request.

TABLE 5—DIFFERENCE-IN-DIFFERENCES RESULTS WITH VERTICAL DIFFERENTIATION

Mathematics test scores	“Treated” (0–2.5 miles) versus “Control” (2.5–5 miles)					
	(1)	(2)	(3)	(4)	(5)	(6)
Non-horizontally differentiated ($\beta_h + \beta_{nh-h}$)	0.035 (0.017)	0.036 (0.017)	0.033 (0.016)	0.038 (0.020)	0.036 (0.020)	0.037 (0.020)
Horizontally differentiated (β_h)	–0.005 (0.016)	–0.005 (0.016)	–0.004 (0.016)	0.007 (0.015)	0.005 (0.015)	0.004 (0.014)
Charter VA (ν_h^4)	—	0.029 (0.091)	0.018 (0.112)	—	–0.010 (0.105)	–0.017 (0.109)
Charter VA \times non-horizontally differentiated ($\nu_h^4 + \nu_{nh-h}^4$)	—	—	0.060 (0.147)	—	—	–0.011 (0.200)
Test of equality by differentiation status p -value of $H_0: \beta_{nh-h} = 0$ versus $H_1: \beta_{nh-h} \neq 0$	0.08	0.08	0.11	0.21	0.21	0.18
Demographic controls	Yes	Yes	Yes	Yes	Yes	Yes
Student fixed effects and 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). “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 interacted with grade indicators 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 from 2012–2013 through 2016–2017. 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 $H_0: \beta_{nh-h} = 0$ versus $H_1: \beta_{nh-h} \neq 0$ in (5). Demographic controls include ethnicity, gender, limited English proficiency status, socio-economically disadvantaged status, gifted status, disability designation, and an indicator if the student is repeating or skipping a grade. Standard errors are clustered at the 2011–2012 census block group level.

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 explain 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.

V. 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 the policy effect we

estimate arises via the competitive 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 an entering non-horizontally differentiated charter we find is in line with estimates of the competitive impacts of voucher programs.⁴⁰ 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' 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, we isolate the influence of competition alone by examining responses by public schools even before exposed students are able to switch. A longer-run view of the effects, however, 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 for educational quality in the aggregate to estimate the social value of screening charter school applicants.

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