School Choice, Competition, and Aggregate School Quality*

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Abstract

Does charter school expansion improve learning outcomes for the average student? Separate literatures have focused on 1) identifying treatment effects on students who choose charter schools; and 2) estimating competitive impacts on students who remain in public schools. This paper develops and estimates an empirical framework that puts these two pieces together to evaluate the impact of charter school choice on education quality in the aggregate. We estimate the model using student-level data from North Carolina. We find that North Carolina's lifting of its statewide charter school cap raised the average public school's value-added by around 0.01σ (on the student test score distribution). We calculate the total human capital returns of the expansion at above $100,000 per charter school enrollee. We further show that competition drives the aggregate gains; test score impacts on students induced into charter schools by the policy are negative.

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1 Introduction

Large-scale school choice policies aim to improve education quality in the aggregate. Two mechanisms potentially support this objective: First, expansion of school choice may increase access to high-quality alternatives to students' assigned public schools. School vouchers, for example, defray tuition for eligible students at private schools, whereas charter schools—the primary vehicle for school choice policy in the U.S.—are publicly-funded (and tuition-free) schools that are operated privately. The second mechanism is that school choice can create incentives for public schools to raise their productivity. Analogized to a "tide that lifts all boats" (Hoxby, 2002), this competitive effect prospectively redounds to the benefit of those students, typically the vast majority, who do not themselves exercise school choice.

Do charter schools improve education quality for the average student? Although laws authorizing charter schools were first adopted nearly thirty years ago, there remains little consensus regarding their aggregate effect on student learning. This is in part because, lacking plausibly random variation in charter school choice at the *market-level*, the literature tackles pieces of the larger puzzle separately.[1] The first strand of work asks whether students who choose to attend charter schools benefit. While compelling lottery-based evidence indicates that certain charter schools are highly-effective (Chabrier et al., 2016), test score value-added and matching estimates suggest that many charters are not better than the average traditional public school.[2] The second branch of literature examines whether charter schools have positive spillovers on students who remain in public schools. In this regard, recent evidence indicates competitive impacts are spatially-localized and suggests they depend on the type of charter school that a public school is exposed to (Gilraine et al., 2021).[3] If charters make choices—such as what kind of curriculum to teach or where to locate—that makes them imperfectly substitutable with public education, incentives to compete

[1] Surveys of the charter school literature include Epple et al. (2016) and Cohodes and Parham (2021).

[2] Lottery-based evidence includes Hoxby and Murarka (2009); Abdulkadiroglu et al. (2011); Angrist et al. (2016b); Dobbie and Fryer Jr. (2015). In contrast, CREDO (2009) uses matching with student level-data from fifteen states and D.C. and finds notable heterogeneity in average charter quality. Singleton (2019) estimates school-level value-added in Florida and finds that the average charter's quality is somewhat lower than the average public school's (and the charter distribution has fatter tails).

[3] Gilraine et al. (2021) show that test scores of students residing relatively nearer where a non-"horizontally differentiated" charter school opens rise on average. This paper builds on that finding and tests the proposed explanation—that demand for charters offering an alternative-type curriculum is inelastic to public school quality. Slungaard Mumma (2022), using a different design, also reports that spillover effects of academically-focused charters are more positive. Earlier evidence on the competitive impacts of charters does not consider curricular differentiation and is mixed (e.g. Bettinger 2005; Sass 2006; Zimmer and Buddin 2009; Imberman 2011).

will be attenuated and learning gains may not be meaningful.

In this paper, we develop an empirical framework to put the above two pieces together in order to evaluate the effect of charter school choice on education quality in the aggregate. We do this using a model in which students choose where to attend school, public schools choose what level of quality to supply, and charter schools select a location in a market. We estimate the model using rich, geocoded student-level data from North Carolina. These data allow us to estimate each individual school's quality as its value-added to student learning and to estimate demand for schools as a function of distance to residence, school type, and quality. To quantify how public schools respond to competition, we leverage a pair of spatial difference-in-differences estimates based around public schools' exposure to charter school entry following the lifting of North Carolina's statewide cap in the estimation. We use the estimated model for policy evaluation: We assess the aggregate effects of the cap lifting as well as estimate the returns to screening charter entrants using counterfactual simulations.

We focus the analysis on elementary grades in North Carolina's three largest Commuting Zones (CZs). Around 8% of students attended one of the 70 charter schools in these three markets during the 2015-16 school year. The dataset we assemble includes detailed information on students and schools. Longitudinal student-level records are provided by the North Carolina Education Research Data Center (NCERDC) and contain data on test scores, demographics, residence, and school attended. We merge these data with two variables central to the analysis: The first is school value-added, which we estimate from the student test score panel and use to measure school quality year-by-year.[4] The second is an indicator for whether a charter school offers a "traditional" skills-focused curriculum or not. A traditional curriculum stands in contrast with those that offer, for example, project-based or experiential learning. This variable is manually-coded using information gleaned from charter schools' applications to the State Board of Education to open.

We begin our empirical investigation by documenting several key empirical facts related to charter school entry following the cap removal in North Carolina. First, we show that while public schools and charter schools that opened prior to the cap being lifted are of a similar quality on average, the average post-cap charter entrant is appreciably lower value-added. This raises the question whether students who attend the entering charter schools benefit, which will jointly

[4] As discussed later, we employ an Empirical Bayes estimator to address issues of noise.

depend on where charter schools locate and the nature of selection into charter schools. Second, we test whether competition leads public schools to raise their quality. We do this using a difference-in-differences framework that compares the changes in value-added of public schools located nearby a newly-opened charter school with those located further away. On average, more competitively-exposed public schools following the cap lifting increase their quality, but there is important heterogeneity: public schools' value-added does not increase following the entry a charter school offering of a non-traditional curriculum nearby.[5] This finding suggests that curriculum choice horizontally differentiates charter schools, a possibility our model of school demand allows and tests for. Further, as highlighted below, this reduced-form variation provides an input to the estimation of the model parameters.

The empirical framework we develop links school choices of students with the incentives of public schools to supply quality in the presence of charter school competition. On the demand side, we model students as valuing school alternatives based on their distance, quality, and type (public or charter), and curriculum (traditional or alternative) if a charter school. We allow for the presence of unobserved school-level characteristics and model observed and unobserved heterogeneity across students in preferences over school attributes. We model public schools as choosing a value-added level to maximize "rent," allowing for the possible role of direct incentives to supply higher quality or to maintain enrollment. This implies an intuitive first-order condition for value-added choice that can be expressed as a "perfect competition" level of quality less a markdown, the latter of which is a function of a public school's elasticity of demand. This set of equations links the availability of charter school choice with public school quality in equilibrium.

Estimating the model presents several empirical challenges. The first challenge is recovering elasticities of substitution. The student-level "micro" data are important to this step by allowing us to link a student's school in the data to their geocoded residence and demographic characteristics while controlling for time-varying school-level unobservables (Berry and Haile, 2020). We use data from markets pre- and post-cap lifting in the demand estimation, which provides variation in students' choice sets. The second challenge concerns the link between demand incentives and public schools' choice of quality, measured by their estimated value-added, in the data. The approach

[5]This finding mirrors the result of Gilraine et al. (2021), though the research design is different in several ways. Gilraine et al. (2021) define treatment based on *students'* location and look at test scores as outcomes, whereas this paper tests for competitive impacts on public school value-added directly.

we take is new and leverages the differential exposure of public schools to competition following the lifting of the charter school cap, described above. Specifically, we instrument public schools' elasticities of demand using distance to post-2012 charter school entrants (by curriculum type). This estimation step maintains the spatial difference-in-differences assumption that charter school are not sorting to locations based on within-district trends and provides a tight link between the model estimates and the reduced-form patterns.

The model estimates allow us to characterize strategic differentiation by charter schools in terms of elasticities of substitution and to quantify the competitive incentives facing public schools. For example, we examine the predicted impact on charter schools' enrollments if all public schools raised their quality by 0.05σ (on the student test score distribution). The results show that the average charter school would lose fewer than 10% of students—indicative that demand for charters is rather quality-inelastic—but the average traditional curriculum charter school would lose around twice as many students as the average non-traditional charter school. This finding is consistent with the alternative curriculum protecting them from vertical quality competition, as previously hypothesized by Gilraine et al. (2021). The differences in elasticities have important implications for public schools' competitive incentives: we show that reducing travel costs to all charter schools by 20% would appreciably raise average public school value-added. However, the same increase in school quality is almost entirely achieved by reducing travel costs to just traditional curriculum charter schools.

The first policy analysis we conduct with the model assesses the aggregate effects on student learning of North Carolina's lifting of its statewide cap on charters for 2012-13. We solve for counterfactual outcomes in an equilibrium where the 29 post-2012 charter school entrants in the data are removed. In this scenario, the average public school would supply 0.012σ lower school quality (on the student test score distribution) and the average student's test scores would be about 0.005σ lower than in the data. The results further indicate that economically disadvantaged students benefit relatively more from the charter school expansion, though the gains are fairly equitably distributed across student groups.

Is the aggregate effect on student learning from raising the charter school cap that we estimate economically meaningful? We benchmark this with Chetty et al. (2014)'s estimates of the causal impact of higher teacher quality on lifetime earnings. The impact of raising the cap on the average

student translates into approximately a 0.22σ increase in teacher value-added one year or to about 1/5th the impact from replacing the bottom 5% of teachers with an average teacher. These effect sizes imply about a $1,500 increase in lifetime income (present value) on average. Per marginal enrollee in charter schools due to the cap lifting, the surplus gain exceeds $100,000. This benefit compares to a per enrollee cost to Durham County, a relatively urban school district in North Carolina, of up to $7,000, as calculated by Ladd and Singleton (2020).

How do competitive incentives contribute to the gains from removing the charter school cap? We assess this in two ways: First, we perform a counterfactual that allows new charters to enter post-2012, but in which public schools may not respond competitively. Relative to the data, the average student's test score would be around 0.007σ lower, indicating that competition is driving the aggregate gains. The second way we answer this question is by decomposing the aggregate gains into its effects on students who would attend a charter school regardless of the cap lifting ("always takers"), those who attend a charter *because* of the cap lifting ("compliers"), and those inframarginal students who would not attend a charter school regardless ("never takers"). This latter group is affected by the cap lifting only via the competitive channel and experiences a 0.007σ increase in test scores—greater than the gain to the average student. This is because the students who choose to attend charter schools in the data actually pay a cost to do so (in terms of human capital gains): the test scores of compliers are 0.03σ lower than what they would be if the cap remained in place.[6]

We further use the model to study the aggregate returns to screening charter schools. A major policy question, which little existing work speaks directly to, is how authorizers should evaluate new charter school applicants. One approach, similar to the policy environment in several states, would aim to foster competition by keeping entry barriers low and largely restricting prior review to ensuring compliance with state standards. A different approach would explicitly consider the proposed education program's (expected) quality as a criterion. Recent evidence shows that replicated charter schools of "proven providers" in Boston are highly-effective (Cohodes et al., 2019). The question raised is whether screening for high-quality—but fewer overall—charter schools yields aggregate gains. Moreover, a second question, stimulated by the results in this paper, concerns *how*

[6]This negative effect on compliers has an interesting parallel with recent findings from U.S. voucher programs (e.g. Abdulkadiroglu et al. 2017). The negative effect implies a MWTP for charter school choice of about $9,000 for compliers on average.

to screen. In contrast with difficult-to-predict quality, an authorizer could instead screen entrants based on curriculum.

To assess these policy trade-offs, we first consider a policy counterfactual that limits post-2012 charter entrants to just those 14 with above average (i.e. > 0) value-added. The results indicate that the average public school's value-added would be lower due to the reduction in competition, but that average test scores would be essentially unchanged relative to the data. We then examine a counterfactual that instead restricts post-2012 entrants to only the 16 traditional charter schools. Though this policy also contracts school choice, we find that it yields a net increase in student test scores (of 0.002σ). This result is consistent with traditional charter schools tending to be higher-quality and, because they compete with public schools more directly on quality, generating more positive externalities.

These findings are informative about the aggregate impacts on student learning of policies that expand school choice. In particular, the results illustrate that expanded school choice can yield gains to the average student via the competitive channel, even if school choice alternatives are not on average better than traditional schooling options—and even if the students who exercise choice do not benefit in human capital terms. In addition, the results highlight the role that strategic differentiation by schools, on dimensions other than location such as curriculum, can play in education markets and its importance for evaluating policy impacts. Lastly, the findings have potentially broad implications for the design of school choice programs. U.S. states, for example, have taken different approaches to screening and authorizing new charter schools, with some preferring to restrict entry to fewer providers of higher quality. Our results speak directly to the fundamental policy trade-off between the quantity and quality of school choice options.

This paper connects with a growing literature doing empirical policy analysis in K-12 education markets. Most often focused on international settings—e.g. Chile (Neilson, 2017), Peru (Allende, 2019), Dominican Republic (Dinerstein et al., 2020)—a hallmark of this literature is the combination of quasi-experimental identification with structural models of school choice and competition. Previous structural work on charter school markets in the U.S. has tended to focus on the demand (Ferreyra and Kosenok, 2018; Walters, 2018) and supply sides (Singleton, 2019) separately, whereas our paper builds and estimates an equilibrium model of supply and demand.[7] Importantly, our

[7]Mehta (2017) also models competition between charter and public schools in North Carolina, though for an earlier

estimates are built on student-level longitudinal data, allowing us to estimate and assess policy impacts on school value-added to student learning. A distinct emphasis of our paper is the empirical significance of strategic differentiation of curricula by schools (Hotelling, 1929), highlighted by MacLeod and Urquiola (2013) as theoretically important. Horizontal competition, though along the lines of match quality, is likewise examined among schools in Pakistan by Bau (2022).

The remainder of this paper is organized as follows. Section 2 discusses the data used in our analysis, highlighting the institutional features and key data patterns. We then present the model in Section 3 and outline our estimation approach in Section 4. We present the estimated model parameters in Section 5 and, in Section 6, use the model to counterfactually evaluate the effects of several policies of interest on student outcomes. Section 7 offers concluding remarks.

2 Data Description and Key Data Patterns

In this section, we describe the setting and data. At a high level, three features of the data and institutional environment are of particular importance to our approach. First, the detailed student–level data allow us to construct choice sets for each student based on their residence and estimate the quality of each school. Second, charter school applications allow us to determine whether each charter school offers a traditional or non-traditional curriculum. And third, the removal of the state-wide cap on charter schools provides variation in both choice sets and the competitive pressure faced by public schools, which is used to inform the model estimates. We now discuss each of these features in turn.

2.1 Data

Our detailed, student-level administrative records are provided by the North Carolina Education Research Data Center (2009-2017). These data include information on all North Carolina public school students (charter and traditional public) for the 2008-09 through 2016-17 school years. The data contain test scores for each student in mathematics and reading on standardized end-of-grade exams in grades three through five. We standardize these test scores at the student level to have a

period. Beyond several differences in modeling framework, the present paper is distinct in a) connecting the model to school value-added, which has been linked to student's long-term outcomes (Deming et al., 2014; Kirkebøen, 2022); and b) using policy variation related to the 2011 cap lifting in North Carolina.

mean of zero and a variance of one for each grade-year to ensure comparability of test scores across grades. We use these test scores to construct measures of school quality (see Section 2.2.1).

In addition to test scores, the student data contain information regarding each student's grade, socioeconomic status, and ethnicity. We also obtain information regarding students' residential locations in each school-year from the NCERDC, a crucial input into our demand model given the importance of distance in determining school choice. 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.[8] We link these student-level data to the universe of public and charter schools in the state. School locations are available from the Common Core of Data, allowing us to compute distances between students' residences and all schools in their education market.

Our empirical model also allows student demand to be a function of the type of curriculum a charter school offers. We classify charter school curricula following the methodology in Gilraine et al. (2021) using charter school applications. In particular, we use the information contained in the applications to manually classify each charter school as either following a "traditional" or "non-traditional" curriculum.[9] To do so, we classify charter schools that emphasize project-based or experiential learning (including Montessori) in their application as following a non-traditional curriculum. Charters are otherwise classified as following a traditional curriculum, which usually entails a focus on core skills. Importantly, we classify all charter schools in this way, including both those who opened prior to the charter school cap being lifted and those who opened after the removal of the statewide cap.

Defining Markets: We focus our analysis on elementary grade-level school markets. As charter schools have no defined attendance zones, students can attend these schools even if they live outside the geographic school district the charter is located in. We therefore define education markets in our data based on Commuting Zones ('CZs'), which are aggregations of counties based on commuting patterns in the 1990 Census. These Commuting Zones are designed to span the area in which people live and work and therefore provide a natural way to partition markets. We focus our analysis on the

[8]The median area of a Census block group in North Carolina is 2.2 square miles.

[9]The recent charter school applications are available online at `https://www.dpi.nc.gov/students-families/alternative-choices/charter-schools/applications/submitted-apps`. Charter school applications pre-2012 are also available by request from the North Carolina Department of Public Instruction.

three largest commuting zones in North Carolina: Charlotte, the 'Research Triangle' (i.e., Raleigh-Durham-Chapel Hill), and Greensboro-High Point. These CZs cover 60 percent of students in North Carolina. Furthermore, as charter schools tend to locate in more urban areas and these CZs include the five largest cities in the state, our data consist of 70 of the 114 elementary charters in the state (as of 2015-16).

Data Summary: Table 1 reports summary statistics, with column (1) doing so for the entire state while column (2) restricts the sample to our CZs of interest.[10] Overall, our restricted sample looks similar to the state overall in terms of demographics. Our three CZs are, however, more urban portions of the state and so students reside closer to schooling options and charter share is higher.

Columns (3) and (4) show summary statistics for our CZs of interest pre-charter cap lifting (2011-12) and post-charter cap (2015-16). Crucially, we observe a large increase in the number of charters post-cap which leads to a corresponding increase in charter share (from 4 to 8 percent) and a decline in distance to the nearest charter option (from 7.8 to 6.7 miles). Column (3) also indicates that pre-charter cap there was very limited enrollment in charters that follow a non-traditional curriculum. The post-cap charter share increase, however, was disproportionately concentrated in these non-traditional charters as their share increased by 138% (versus a 82% increase in charter share).

2.2 School Quality, Policy Backdrop, and Quasi-Experimental Variation

Central to our analysis is a measure of school quality. In this section, we first briefly describe how we estimate the academic quality of each school, followed by a description of several key patterns related to the removal of North Carolina's statewide cap on charter schools.

2.2.1 Estimating School Quality

We estimate school quality using a standard model of student achievement in which education inputs (including school quality) are additive in their effects. The achievement of a student i at

[10] Table 1 reflects the sample restriction to students in grades K through 2 in the demand estimation, which we discuss later.

Table 1: Summary Statistics

	All K-2 North Carolina Students[1] (1)	Demand Estimation Sample[2] (2)	Split by School-Year: Demand Estimation Sample (2011-12) (3)	Split by School-Year: Demand Estimation Sample (2015-16) (4)
Student Demographics				
% White	50.2	48.8	51.2	46.2
% Black	23.7	24.0	23.0	24.9
% Hispanic	17.8	19.1	18.3	19.9
% Asian	3.1	4.2	3.6	4.9
% Economically Disadv.	52.2	47.6	50.7	44.3
% Charlotte Commuting Zone	26.2	45.3	46.4	44.0
% Research Triangle Commuting Zone	21.2	36.6	36.6	36.7
% Greensboro-High Point Commuting Zone	10.5	18.1	17.0	19.3
School Attendance Summaries				
% Attend Assigned Public[3]	70.1	57.5	57.7	57.2
% Attend Charter School	5.39	6.30	4.45	8.11
% Attend Non-Traditional Charter School	1.85	1.70	1.00	2.38
Distance to School of Attendance[4]	2.29	2.09	2.09	2.10
Distances to School Options				
Distance to Nearest Public School (miles)[4]	1.70	1.48	1.49	1.46
Distance to Nearest Charter School (miles)[4]	8.73	7.22	7.75	6.66
Observations (students)	117,753	66,862	33,030	33,832
# of public schools	1,237	606	597	595
# of charters	118	73	44	70

Notes:
[1] Data coverage: Twenty percent random sample of Kindergarten through second grade students for the 2011-12 and 2015-16 school years.
[2] Same as for column (1), but restricted to the three largest commuting zones in North Carolina: Charlotte, the Research Triangle, and Greensboro-High Point.
[3] Only reported when student residence is observed and can be assigned to a school attendance zone. The sample for these summary statistics is 72,010 and 55,573 student-year observations in columns (1) and (2), respectively. Note that while the link between student residence and school attendance zone can be missing for some rural areas it is near-universal in urban areas.
[4] Only reported when student residence is observed. The sample for these summary statistics is 96,860 and 56,148 student-year observations in columns (1) and (2), respectively.

school s in year t is written as:

$$y_{ist} = X'_{ist}\beta + q_{st} + \epsilon_{ist} \tag{1}$$

where y_{ist} is the student's test score, X_{ist} is a large vector of observable individual and school-level student characteristics, and ϵ_{ist} is a random test score shock which is assumed to be iid normal with variance σ^2_ϵ. The estimated value of q_{st} represents the contribution of school s in year t to test scores that is unexplained by student characteristics, or simply its value-added.

Crucial to the validity of value-added models is that the control vector, X_{ist}, is sufficiently rich so that potential test scores are independent of a student's school choice, conditional on observables. While not perfectly unbiased, school value-added measures tend to feature limited bias when lagged test scores are included in the control vector (Deming, 2014; Angrist et al., 2016a, 2017). We therefore control for flexible functions of lagged test scores, along with student-level and school-grade demographics.[11] We estimate school value-added using all fourth and fifth students in North Carolina from 2008-09 through 2015-16. Table A.1 provides summary statistics for the sample used to estimate school VA. Following the literature, the school's value-added in each year, q_{st} is estimated using empirical Bayes to minimize mean squared error.[12]

2.2.2 Lifting of Charter School Cap, New Charter Entrants, and Effects on Public Schools

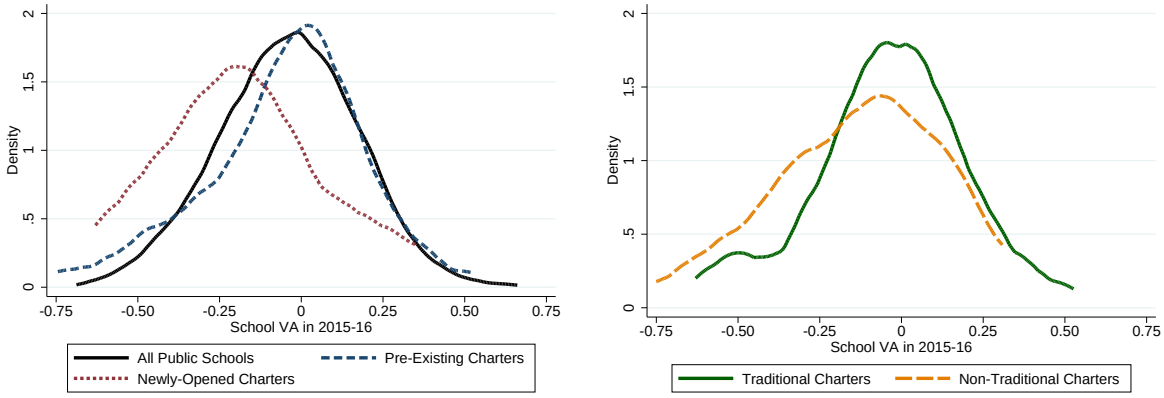
We use the lifting of North Carolina's cap on the number of charter schools allowed to operate statewide to generate plausibly exogenous variation in choice sets and competition. Initiated by its receipt of a federal Race to the Top grant, on June 6, 2011, North Carolina lifted its 100-school cap on the number of charter schools allowed to operate—a limit that was in place since charter schools first emerged in the state in the 1996-97 academic year. Rapid growth of charter schools

[11] Specifically, X_{ist} includes: (i) lagged test scores using a cubic polynomial in prior-year scores in math and English, interacted with grade dummies, (ii) demographics, including: economically disadvantaged status, ethnicity (six ethnic groups), gender, limited English status, gifted status, and disability status. We also include the following school-grade level controls: (iii) cubics in school-grade means of prior-year test scores in math and English (defined based on those with non-missing prior scores) interacted with grade dummies, (iv) cubics in school-grade means of all the demographic covariates, (v) school-grade size, and (vi) grade-by-year dummies.

[12] Formally, $q_{st} = y_{st}\frac{\sigma^2_s}{\sigma^2_s+\sigma^2_\epsilon/n_{st}}$ where $y_{st} \equiv \frac{\sum_i^{n_{st}}(y_{ist}-X'_{ist}\beta)}{n_{st}}$ is the fixed effect of school s in year t in equation (1), n_{st} is the number of students in school s at year t, and σ^2_s and σ^2_ϵ are the variances of q_{st} and ϵ_{ist} (which we estimate via maximum likelihood estimation and plug-in).

followed, with there being just shy of 100 schools prior to the cap's lifting and 176 charter schools in operation by the 2016-17 academic year. (Figure A.1 displays the number of charters in North Carolina and our three markets of interest from 2008-09 through 2015-16.)

In Figure 1(a), we show the distribution of school value-added across public and charter schools in North Carolina, subdividing the charter schools among pre-existing charters that opened before the charter cap was lifted and those that opened after. VA for pre-existing charters is, on average, slightly below that of public schools in North Carolina (by 0.05), but the distributions are relatively similar. The newly-opened charters, however, have significantly lower VA than the public schools (by 0.14). Figure 1(b) then contrasts the VA distribution for charters by curriculum type: The value-added for charters that follow a traditional curriculum is significantly higher than those that follow a non-traditional curriculum (by 0.10). These descriptive statistics are suggestive that the lifting of the charter school cap may actually make students who take up the new charter options *worse off* due to the lower quality in these schools.



(a) Value-Added for Public Schools and Pre- and Post-Cap Charters

(b) Charter School Value-Added by Curriculum

Figure 1: Distribution of Public and Charter School Value-Added

Notes: This figure shows the distribution of school value-added for the 2015-16 school year. Figure 1(a) displays the value-added distributions separately for public and charter schools. The charter school VA distribution is further subdivided into 'pre-existing charters' which opened prior to the charter cap being lifted (i.e, pre-2012-13) and 'newly opened' charters that opened after the charter cap was lifted (i.e., 2012-13 or later). Figure 1(b) then presents the value-added distributions separately for charter schools that follow a traditional and non-traditional curriculum.

The Impact of Charter Openings on Nearby Public School Quality and Enrollment: We leverage variation following the removal of the statewide charter school cap and compare school enrollment and quality for public schools located near the newly-opened charter schools (treatment) to those farther away (control). These reduced-form findings motivate the analysis in the rest of the paper whereby charter schools induce competitive effects on nearby schools, although the

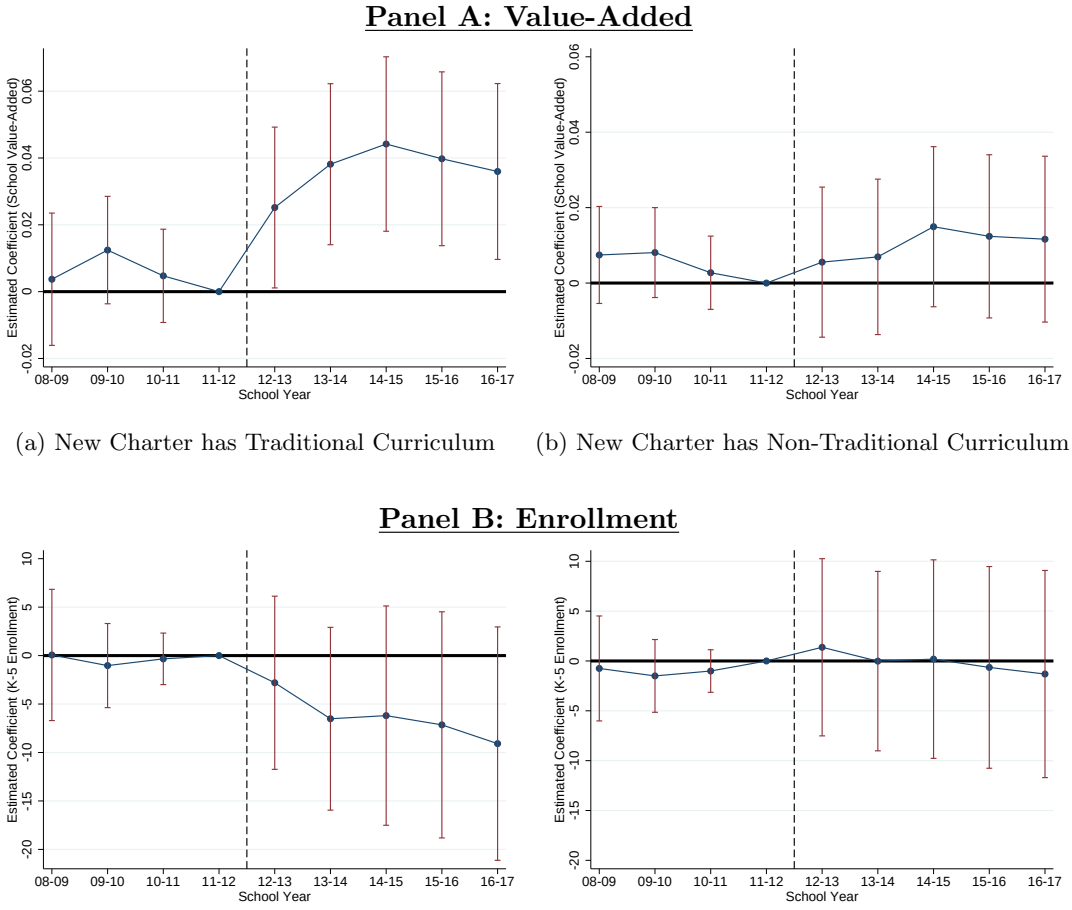
competitive effects are muted when charter schools choose a non-traditional curriculum.

We focus on charters opening in the first two years after the charter cap was lifted, as nearby schools knew that these charters would open and where in the 2012-13 school year.[13] A total of 23 elementary charters opened in these years, with 15 of these openings occurring in our CZs of interest. In our demand model below, students can choose schools up to 30 miles from their residence. We therefore take all schools within 30 miles of a newly-opened charter and define 'treated' schools as those within 20 miles of the newly-opened charter and all those further away as 'control' schools so that we are left with a roughly equal number of 'treated' and 'control' schools. Since public schools sometimes have more than one charter open nearby, we define each charter entry as an event and stack our data. We then regress our outcome (either school enrollment or value-added) on this treatment indicator interacted with a post-charter cap dummy, controlling for treatment distance by type of exposure, school-by-event fixed effects, and district trends. We further interact these variables with whether the newly-opened charter follows a traditional or non-traditional curriculum so that we can see whether effects differ based on the curriculum of newly-opened charters. (See Appendix B for more details.)

Panel A of Figure 2 plots the estimated coefficients from an event-study specification that interacts the treatment indicator with year dummies (rather than a simple post-charter cap dummy), restricting data to our CZs of interest. It does so separately for when the newly-opened charters follow a traditional curriculum (Figure 2(a)) and a non-traditional curriculum (Figure 2(b)). The figures reveal that there is no evidence of significant differential trends prior to the cap lifting between control schools and schools that were treated by the entry of a traditional or non-traditional charter school. Once the charter cap lifts, however, we see a substantive increase in value-added in the treated schools compared to the control schools when the nearby charter follows a traditional curriculum. No such value-added increase is observed when the nearby charter follows a non-traditional curriculum. Panel B shows the results for school enrollment: a similar pattern is evident whereby schools near a newly-opened charter following the traditional curriculum lose enrollment, while those near a newly-opened charter using a non-traditional curriculum are unaf-

[13]This comes from the fact that after the charter cap was lifted there was a 'fast track' application where the waiting period was waived. As charters usually had to wait one year to open, this meant charters applying to open in either the 2012-13 or 2013-14 school year had their publicly-available applications submitted and approved by 2012-13 and so nearby schools would know of these entry plans and be able to respond in 2012-13. See Gilraine et al. (2021) for a more detailed timeline on the application and approval dates.

fected.



(a) New Charter has Traditional Curriculum (b) New Charter has Non-Traditional Curriculum

(c) New Charter has Traditional Curriculum (d) New Charter has Non-Traditional Curriculum

Figure 2: Event Studies: Newly-Opened Charter on Nearby School Value-Added and Enrollment

Notes: This figure shows the estimated value-added (Panel A) and enrollment (Panel B) difference between schools 'treated' by a newly-opened charter relative to 'control' schools by year. Data are restricted to schools in the Charlotte, the 'Research Triangle' (i.e., Raleigh-Durham-Chapel Hill), and Greensboro-High Point commuting zones. Treated schools are defined as schools located within 20 miles of a newly-opened charter that opened in 2012-13 or 2013-14. Control schools are defined as schools located between 20 and 30 miles of a charter schools that opened in 2012-13 or 2013-14. Results are subdivided by whether the newly-opened charter follows a traditional curriculum or not. Note that 2012-13 is considered the first 'treated' year because although the charters themselves opened in either the 2012-13 or 2013-14 school year, public schools would have known by the start of 2012-13 whether or not a charter was opening nearby or would open nearby in 2013-14. The dashed vertical line therefore separates the 'pre-years' from the 'post-years'. The horizontal line represents a point estimate of zero. The dashed 'whiskers' represent 90 percent confidence intervals with standard errors clustered at the school level.

Table A.2 reports the results of the difference-in-differences regressions, which indicate that, relative to the control schools, treated schools near a newly-opened traditional curriculum charter see a 0.03 increase in value-added and a seven student decline in enrollment. In line with the visual evidence, minimal effects are seen for schools located near non-traditional curriculum charters. Finally, our selection of schools within 20 miles being treated is relatively ad hoc, and so Figure

A.3 shows our difference-in-differences point estimates for various definitions of treated. The figure makes clear that we obtain similar results when we shrink the treatment radius around a newly-opened charter all the way to 5 miles (below that we have too few schools for meaningful results). These event-study results show that charter competition causes nearby schools to lose enrollment which forces them to compete by raising quality, at least when the newly-opened charter follows a traditional curriculum.

The lack of enrollment response among public schools nearby charters that follow a non-traditional curriculum raises the natural question of where students attending these charters come from. We resolve this dissonance by investigating how nearby private school enrollment responds in Figure A.2. We find enrollment declines among private schools nearby a newly-opened non-traditional charter. No such enrollment declines are observed for private schools nearby charters that follow a traditional curriculum. Since private schools also have the leeway to offer a non-traditional curriculum, these results support the hypothesis that charter schools offering traditional curriculum compete with public schools while those offering non-traditional curriculum are competing with private school or home schooling options.

3 Empirical Model

Having described the data and key variation that will inform the estimates and evaluation to come, we now turn to our empirical model of student demand for schooling options and school supply. We build the model to leverage the detailed data described above as well as the policy-driven variation in charter school supply and public school quality.

3.1 Demand

On the demand side of the model, students choose from among the schools – public and charter – in their choice set to maximize utility. The indirect utility to student i from attending school j is given by:

$$u_{ij} = \beta_i^V q_j + \beta_i^C Charter_j + \beta_i^H NonTrad_j + \gamma_i \log d_{ij} + \gamma_i^C Charter_j \times \log d_{ij} + \kappa_i Assigned_{ij} + \xi_j + \epsilon_{ij} \tag{2}$$

$Charter_j$ and $NonTrad_j$ are indicators for whether j is a charter school and, if a charter, whether the instruction offered is non-traditional. q_j represents school j's quality (as measured by estimated value-added to student learning); β_i^V is thus i's "marginal utility" of value-added.[14] d_{ij} is the student's residence's distance (in miles) from school j's location, while $Assigned_{ij} = 1$ if j is their assigned local public school.[15] We allow the travel cost to differ by whether or not the school is a charter school. Finally, ξ_j is a structural error that represents (an index of) unobserved school qualities or amenities that is valued in common by students.

It is useful to re-write equation (2) as follows:

$$\begin{aligned} u_{ij} &= \beta_i^V q_j + \beta_i^C Charter_j + \beta_i^H NonTrad_j + \gamma_i \log d_{ij} + \gamma_i^C Charter_j \times \log d_{ij} + \kappa_i Assigned_{ij} + \xi_j + \epsilon_{ij} \\ &= \delta_j + \underbrace{\tilde{\beta}_i^V q_j + \tilde{\beta}_i^C Charter_j + \tilde{\beta}_i^H NonTrad_j + \gamma_i \log d_{ij} + \gamma_i^C Charter_j \times \log d_{ij} + \kappa_i Assigned_{ij}}_{\mu_{ij}(\theta)} + \epsilon_{ij} \end{aligned}$$

δ is the vector of "mean" utilities (which absorb the ξ's), while the ϵ's are idiosyncratic T1EV choice shocks. In contrast, the $\mu_{ij}(\theta)$ terms capture systematic heterogeneity in preferences. We allow for observed and unobserved heterogeneity in the demand model:

$$\begin{pmatrix} \tilde{\beta}_i^V \\ \tilde{\beta}_i^C \\ \tilde{\beta}_i^H \end{pmatrix} = \begin{pmatrix} \tilde{\beta}^V \\ \tilde{\beta}^C \\ \tilde{\beta}^H \end{pmatrix} W_i + \begin{pmatrix} \upsilon_i^V \\ \upsilon_i^C \\ \upsilon_i^H \end{pmatrix}$$

W_i is a vector of household characteristics and $\upsilon_i \sim N(0, \Sigma)$ is a vector of random coefficients on tastes for value-added, charter schools, and non-traditional instruction. Preferences over distance and assigned public school only depend on observed characteristics.[16] We include indicators for economic disadvantage and underrepresented minority in W_i.

[14] While we use the term "preference parameters" throughout the paper for convenience, it should be understood that these valuations of school characteristics likely represent a combination of both student tastes and their information about schools.

[15] This information is obtained from the NCES School Attendance Boundary Survey. Rather than assuming students cannot possibly attend public schools outside their attendance zone, the inclusion of this variable in the indirect utility estimates the "cost," which is held constant in the policy counterfactuals to come, of doing so.

[16] We also allow them to vary by market in the estimation.

3.2 Supply

On the supply side, we model both the decisions of public schools over how to set educational quality and the location decisions of charter schools. We make explicit how the quality of public schools depends on the prevailing student demand in their local areas as well as the supply of traditional and non-traditional charter schools.

3.2.1 Public School Value-Added

In the empirical model, public schools choose value-added (taking other schools' choices as given) in order to maximize a utility function. For public school j at time t, this is given by:

$$U_j = F_j(D_j(\mathbf{q})) + G_j(q_j) - C(\mathbf{q})$$

where $C(\mathbf{q}) = mc_j(q_j)D_j(\mathbf{q}) + \eta q_j$. $mc_{jt}(q_j)$ is their marginal cost per pupil and depends on their value-added choice, η represents an adjustment cost. $D_j(\mathbf{q})$ is total enrollment at the school, which is derived from the demand model and depends on all schools' quality choices, $\mathbf{q}$. This formulation of public school utility nests "rent-seeking" (e.g. McMillan (2004)) and profit-maximizing behavior, but allows for preferences for enrollment (over and above revenue) and quality, potentially reflective of accountability incentives, directly through $F_j()$ and $G_j()$.

The first-order condition of the maximization problem implies:

$$(\tau_j - mc_j(q_j^*))\sigma_j(\mathbf{q}^*) + \frac{\lambda_j}{D_j(\mathbf{q}^*)} = mc_j'(q_j^*)$$

where $\tau_j = F_j'()$ and $\lambda_j = G_j'() - \eta$. In this expression, $\sigma_j(\mathbf{q}) = \frac{1}{D_j(\mathbf{q})}\frac{\partial D_j(\mathbf{q})}{\partial q_j}$ is public school j's own-value-added semi-elasticity of demand. Note that this object depends on the demand parameters and has no closed-form representation. The system of these equations for all schools in each market defines a Nash equilibrium in qualities.

We assume that $mc_j(q_j) = \pi_j + \kappa q_j$. Simplifying the first-order condition yields an important

and intuitive expression for school j's equilibrium choice of value-added. This is given by:

$$q_j^* = \underbrace{\frac{\tau_j - \pi_j}{\kappa} + \frac{\lambda_j}{\kappa}[\frac{\partial D_j(\mathbf{q}^*)}{\partial q_j}]^{-1}}_{\text{Perfect competition VA}} - \underbrace{\frac{1}{\sigma_j(\mathbf{q}^*)}}_{\text{VA "markdown"}} \tag{3}$$

The expression consists of two parts: the level of quality that would be supplied under perfect competition (which depends on the public school utility function and cost parameters and the slope of their demand curve) and a value-added "markdown." This latter term embeds public schools' incentive to supply higher value-added when competitive pressure is higher, as captured by the own-value-added semi-elasticity. Conversely, the "markdown" to value-added will be large for public schools that effectively operate as local monopolists.

3.2.2 Charter School Location

Let $\mathbf{s} = (\mathbf{a}, \mathbf{z}, \mathbf{VA^P}, \mathbf{x}, \upsilon)$ where $\mathbf{a}$ is list of all schools' locations, $\mathbf{z}$ is a list of the schools' type (public, charter, $NonTrad$), $\mathbf{x}$ is list of all locations' exogenous characteristics, and $\mathbf{VA^P}$ is list of all schools' qualities.[17] $a_{ij} = 1$ if school i is located in location j. For charters, $a_{i0} = 1$ indicates no entry. υ are a list of private information draws for charter schools. We assume that type and locations and qualities of public schools are exogenous and common knowledge.

We model charter schools' utility from operating in a given location as depending on the number (and quality of) nearby public schools and the number of nearby charter schools plus depending location-specific demand and cost shifters, such as household income. To capture the spatial nature of competition, we use distance bands (indexed by b) around each location, similar to Seim (2006) and Singleton (2019). Charter school j's expected utility from location k is given by:

$$\begin{aligned} E[u_{jk}] = &\sum_b \beta_{jb}^P N_{kb}^P + \gamma_j X_k + \sum_b \gamma_{jb} \bar{q}_{kb}^P \\ &+ \beta_{i1}^H (E[N_{k1}^H] - NonTrad_j) + \sum_{b=2} \beta_{jb}^H E[N_{kb}^H] \\ &+ \beta_{i1}^N (E[N_{k1}^N] - (1 - NonTrad_j)) + \sum_{b=2} \beta_{jb}^N E[N_{kb}^N] \end{aligned}$$

[17] The latter is the estimated value-added for each charter school, which is revealed post-entry, and the estimated "perfect competition" quality for each public school.

In addition to the J possible locations in the market, a charter school may choose not to enter: $u_{i0} = \alpha_i$. N_{kb}^P indicates the number of public schools, while $E[N_{kb}^H]$ and $E[N_{kb}^N]$ indicate the number of $NonTrad$ and $Trad$ charter schools j's *expects* in distance band b of location k. $\bar{q}_{kb}^P$ represents average public school quality in distance band b. The β and γ parameters thus capture competitive effects, which are heterogeneous by a charter school's own type:

$$\begin{pmatrix} \beta_{jb}^m \\ \gamma_j \\ \alpha_j \end{pmatrix} = \begin{pmatrix} \beta_{0b}^m \\ \gamma_0 \\ \alpha_0 \end{pmatrix} + \begin{pmatrix} \beta_{1b}^m \\ \gamma_1 \\ \alpha_1 \end{pmatrix} NonTrad_j$$

Thus, for example, $Trad$ charter schools may be more sensitive to competition with public schools (given a distance band) and competition between charters may be more intense when they share a curriculum type.

4 Estimation

We estimate the empirical model in several steps. We first estimate the heterogeneous demand parameters and recover mean utilities. This step is described below. We then leverage the spatial difference-in-differences based around charter exposure following North Carolina's cap removal to estimate $\bar{\beta}^V$ and public schools' quality policy function.

4.1 Estimating Demand

The demand model generates expressions for choice probabilities that can be mapped to the student-level choices via maximum likelihood. The probability that student i chooses school j in their choice set is given by:

$$\int \frac{\exp \delta_j + \mu_{ij}(\theta)}{\sum_{k \in C_i} \exp \delta_k + \mu_{ik}(\theta)} f(\tilde{\upsilon}_i) d\tilde{\upsilon}_i \tag{4}$$

We restrict choice sets (C_{it}) to public schools within 7 miles and charter schools within 30 miles.[18] θ represents the vector of heterogeneous demand parameters optimized over. The estimation proce-

[18]Given this, we drop any student whose school of attendance falls outside their choice set. This restriction drops five percent of our sample. We suspect at least half of these cases are driven by coding errors as the distance between a student's residence and school of attendance is improbable (e.g., a student is attending a school over 100 miles from their home). Public school students' median travel distance to their school is a little over 2 miles.

dure recovers the vector of mean utilities δ using the BLP contraction mapping to match predicted and observed shares and uses simulation to form the choice probabilities. We use quadrature to integrate the random coefficients and, for those students whose residence location is not known, we integrate out over demographic-specific densities estimated from the residential data.[19] We specify the random coefficient structure with a standard normal and estimate the preferences over value-added, charter schools, and non-traditional curricula associated with the unobserved type.[20]

We estimate the demand model on 20% random samples drawn from Kindergarten to 2nd grade students in six markets: 2011-12 (pre-removal of the charter cap) and 2015-16 (post-removal) for each of the three major Commuting Zones in North Carolina. The estimation sample includes 66,862 student-year observations.

4.2 Identifying $\bar{\beta}^V$ Using Spatial DiDs

For public school j, we have two structural equations from the empirical model. For their mean utility (recovered in the above step), we have:

$$\delta_{jt} = \bar{\beta}^V q_{jt} + \xi_{jt} \tag{5}$$

Mean utility depends on the education quality, as measured by value-added, of the public school, q_{jt}, and the quality unobserved to the econometrician, ξ_{jt}. We then have the quality policy function, equation (3), which depends on the demand parameters (including $\bar{\beta}^V$) and cost and objective function parameters.

It is commonplace to first decompose equation (5) using instruments for q_{jt} (since quality will be correlated with ξ_{jt}) and then to estimate the policy function in a second step (conditional on the estimate of $\bar{\beta}^V$). Crucially, relevant and valid instruments for q_{jt} are needed to carry this out. In practice, many applications typically rely on a combination of 1) market-level price indices; 2) product location space instruments (Berry et al. 1995); and 3) natural experiments, such as arising from policy changes.

In this paper, we instead identify and estimate $\bar{\beta}^V$ and the policy function in a single step

[19] We use 50 residence location draws. We estimate the residential densities from the 2012 (i.e. pre-charter cap removal) data.

[20] Note that this is an equivalent normalization to jointly estimating the variance on preferences for one characteristic, e.g. value-added, and its correlation with the other characteristics.

that is based on the spatial difference-in-differences variation summarized earlier. Intuitively, this estimation approach asks: what value of $\bar{\beta}^V$ rationalizes the reduced-form effects of charter school exposure on public school value-added (given the utility function estimates)? The estimating equation is thus derived from the policy function, which links the competitive environment to a public school's choice of quality:

$$q_{jt} = [-1 + \tilde{\lambda}(X_{jt})D_{jt}^{-1}]\frac{1}{\sigma_{jt}(\bar{\beta}^V)} + \tau X_{jt} + \pi_j + \psi_{d(i)t} + \omega_{jt} \tag{6}$$

This equation re-writes the policy function, equation (3), such that the parameters other than $\bar{\beta}^V$ represent reduced-form objects that are directly estimated and held constant in the policy analyses (plus an error term ω_{jt}). These objects include linear functions of observed pre-determined cost shifters X_{jt}, the school fixed effects π, and district-specific trends ψ.[21] From the demand model, the semi-elasticity as a function of $\bar{\beta}^V$ is given by:

$$\sigma_{jt}(\bar{\beta}^V) = \frac{1}{D_{jt}} \sum_i \int (\bar{\beta}^V + \tilde{\beta}_i^V) p_{ijt}(1 - p_{ijt}) f(\tilde{\upsilon}_i) d\tilde{\upsilon}_i$$

Note that $\tilde{\beta_i^V}$, p_{ijt}, and $f(\tilde{\upsilon}_i)$ are estimated alongside the other heterogeneous demand parameters in the previous estimation step.

For identification, note that $\sigma_{jt}(\bar{\beta}^V)$ is endogenous, but is shifted by exposure to charter school entry post-2012. We make the assumption, analogous with the spatial difference-in-differences estimates presented earlier, that charter schools do not choose location based on within-district *innovations* to ω.[22] The exposure variables then isolate exogenous variation in $\sigma_{jt}(\bar{\beta}^V)$, which can be used to estimate $\bar{\beta}^V$. This set of assumptions implies a nonlinear GMM estimator, which we detail next.[23]

[21] For example, $\tilde{\lambda}(X_{jt}) = 0$ would indicate no direct utility returns and no adjustment costs to quality (or that these two objects cancel each other out).

[22] Note that this condition does not imply that charters do not sort on innovations to ξ, the demand shifter, whereas using the charter exposure variable as IVs to decompose the mean utilities would. From this perspective, an advantage of our estimation approach based around the policy function is that it avoids exclusion restrictions that are implied by the decomposition.

[23] If equation (6) were instead linear in parameters, estimation could carried out by simply using 2SLS (controlling for school fixed effects and district trends).

4.2.1 GMM

We estimate equation (6) using data from 2012 and 2016. To deal with incidental parameters, we first difference the equation:

$$\Delta q_j = -[\frac{1}{\sigma_{j2016}(\bar{\beta}^V)} - \frac{1}{\sigma_{j2012}(\bar{\beta}^V)}] + [\frac{\lambda(X_{j2016})}{\sigma_{j2016}(\bar{\beta}^V)} - \frac{\lambda(X_{j2012})}{\sigma_{j2012}(\bar{\beta}^V)}] + \tau \Delta X_j + \psi_{d(i)} + \Delta\omega_j \qquad (7)$$

The differencing cancels out the school fixed effects in equation (6) and the district-specific trends become district fixed effects. The parameters to be estimated are therefore $\bar{\beta}^V$ (which enters non-linearly), $\lambda(\cdot)$, τ, and $\psi_{d(i)}$.

The moment condition is $E[\Delta\omega|Z] = 0$, where Z is a vector of instruments. These instruments include the right-hand side controls in equation (7)—X_{j1}, X_{j0}, and district fixed effects—as well as excluded instruments. Variables in X, discussed below, include a district cost index and treatment by an accountability program. The excluded instruments are the charter entry exposure variables. Because public schools may be exposed to multiple charters, we create rows for each public-charter entrant (within 30 miles) pair. The baseline four excluded IVs are then: whether the public school is treated (i.e. the charter entrant is within 20 miles) and the entrant is $NonTrad$, whether treated and the entrant is $Trad$, and the treatment distance interacted with each treatment indicator.[24] In our preferred specification, we generate four additional IVs by interacting these with the cost index in 2012 and further include the full set of interactions with the controls as excluded IVs.

4.2.2 The Costs of Providing School Quality and Accountability Pressure

We now briefly discuss the cost index and accountability program pressure we include the X vector above.

The Comparable Wage Index for Teachers A key determinant of the educational quality schools deliver is the cost of the labor involved in providing that quality, especially in the form of teacher salaries. We measure variation in these labor costs with the comparable wage index for teachers (CWIFT), an index made available by the National Center for Education Statistics and designed to identify geographic variation in (regression-adjusted) wages for college-educated workers

[24] Our main results equally weight the rows, but we examine the sensitivity of the estimates to weighting the rows by the 1 over the total number of charter exposures (so that each public school represents 1 effective observation).

outside of teaching, thereby serving as a proxy for the area-specific costs of hiring teachers.[25] We use data on the CWIFT at the school district level in 2012 and 2016, and using equation (7), assess how the provision of school quality changed with changes in the cost of employing teachers.[26]

Turning Around North Carolina's Lowest-Achieving Schools Our second determinant of the change in school quality is a school's membership in North Carolina's Turning Around Lowest-Achieving Schools (TALAS) initiative. As part of its Race to the Top grant, North Carolina created the TALAS program to target schools and school districts for improvement plans based on inadequate proficiency or graduation rates. TALAS implementation started in the 2010-11 academic year and was fully implemented by 2011-12 (Henry and Guthrie, 2019). The program was multi-faceted, as treated schools experienced principal replacement, instructional reform, increased learning time, and financial incentives for teachers and principals when students realized adequate test score growth (Heissel and Ladd, 2018). We provide more detail about the TALAS program in Appendix C and, using a difference-in-differences framework, illustrate the TALAS-driven variation in school quality that informs estimates of equation (7).

5 Estimation Results

In this section, we present estimation results. We first present parameter estimates and report elasticities of substitution, which speak to how curriculum choice differentiates charters. We then counterfactually reduce travel costs to charter schools to study the effects of differentiation on equilibrium public school quality.

5.1 Estimates

Table 2 presents demand estimates corresponding to the heterogeneous parameters in $\mu_{ij}(\theta)$ in equation (4). While not directly interpretable, the estimates are indicative of several important qualitative features of school demand. First, travel costs and school assignment are highly-salient. Students, especially non-disadvantaged majority students, are more willing to travel to charter schools,

[25] For a full description of the CWIFT, including a discussion of measurement and interpretation, see Cornman et al. (2019).

[26] Because we only have CWIFT data at the school district level and our main specification absorbs district trends, the cost index only enters our analysis through *interactions* with other variables included in equation (7). We thus allow the effects of changes in other determinants of school quality to depend on the costs of hiring teachers in the school district.

Table 2: Demand Estimates

	Coef	SE
Log distance	-1.54	0.03
Log distance × Econ disadv	0.10	0.02
Log distance × URM	-0.01	0.02
Log distance × Charter	0.66	0.05
Log distance × Charter × Econ disadv	-0.57	0.08
Log distance × Charter × URM	-0.23	0.06
Assigned public	1.50	0.03
Charter × Econ disadv	-0.62	0.18
Charter × URM	0.19	0.14
Charter × unobs type	-3.65	0.20
VA × Econ disadv	-0.46	0.10
VA × URM	-0.88	0.10
VA × unobs type	1.10	0.25
NonTrad × Econ disadv	-1.05	0.12
NonTrad × URM	0.06	0.10
NonTrad × unobs type	-0.91	0.18
Student-years	66,862	

all else equal. Second, disadvantaged have weaker preferences (relative to non-disadvantaged, white and Asian students) for charter schools, for school value-added and for Non-Traditional charters. Students from underrepresented minority backgrounds display similar preferences for charter schooling and for Non-Traditional charters as non-disadvantaged majority students, but have relatively weaker preferences for value-added. The estimates on students' unobserved type, which corresponds to a draw from a standard normal distribution, reveals how preferences along unobserved lines are correlated across school characteristics. The estimates indicate that preferences for value-added and for charter schooling are strongly negatively correlated, with students who value value-added highly especially disliking Non-Traditional charter schools. In the next subsection, we examine the implications of these estimates for elasticities of substitution

Table 3 presents estimates of the value-added policy function, including $\bar{\beta}^V$, the "marginal utility" of value-added (for non-disadvantaged, non-URM students). Column (1) presents the estimate for $\bar{\beta}^V$ assuming that public schools are pure rent-maximizers (i.e. $\lambda = 0$) and using only the entry exposure variables as excluded IVs. Column (2) adds TALAS and a district cost index to the control set. Column (3) interacts the entry variables with the cost index in 2012 for additional

Table 3: Policy Function Estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$\bar{\beta}^V$	3.09**	2.93***	3.24***	4.15***	4.53***	3.64***	4.38***
	(1.26)	(1.02)	(0.77)	(0.52)	(0.82)	(0.40)	(0.69)
λ							1.00
							(2.03)
District FE	Y	Y	Y	Y	Y	Y	Y
TALAS × cost controls	N	Y	Y	Y	Y	Y	Y
Excluded IVs	Entry	Entry	Entry × cost		Entry × cost × controls		
wt = 1/exposures	N	N	N	N	Y	Y	N
+ demand moment	N	N	N	N	N	Y	N

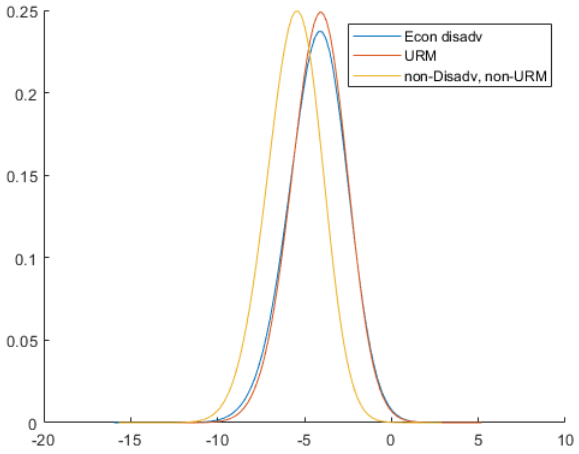


Figure 3: MWTP for Value-added
in terms of travel distance

IVs, while column (4) includes the full set of interactions between the entry variables, 2012 cost index, and controls as excluded IVs. We use the estimate from this column, $\bar{\beta}^V = 4.15$, for all of the results that follow. Columns (5) through (7) examine robustness of the estimate to weighting, to inclusion of the decomposition moment as well, and to estimating λ.

To better understand the economic meaning of the $\bar{\beta}^V$ estimate, Figure 3 plots the implied willingness-to-pay for an increase in school value-added in terms of travel distance to school. The figure shows that the average student would be willing to travel more than 6 miles to school to raise their school quality by 1σ. As suggested by the utility function estimates, this willingness-to-pay is heterogeneous across students' observed characteristics. This is shown in the figures by

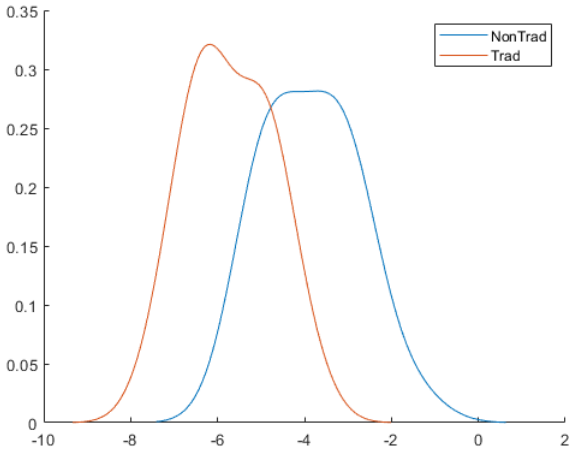


Figure 4: Elasticities of Substitution
Counterfactual: 0.05 SD increase in VA at ALL public schools

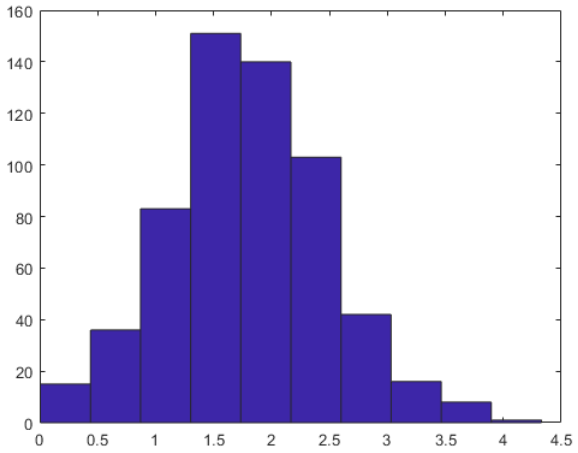
the right shift for underrepresented minority and economically disadvantaged students, indicating these students would pay a little less than 5 miles of distance to school for the same increase in quality on average.

5.2 Elasticities, Differentiation, and Competition

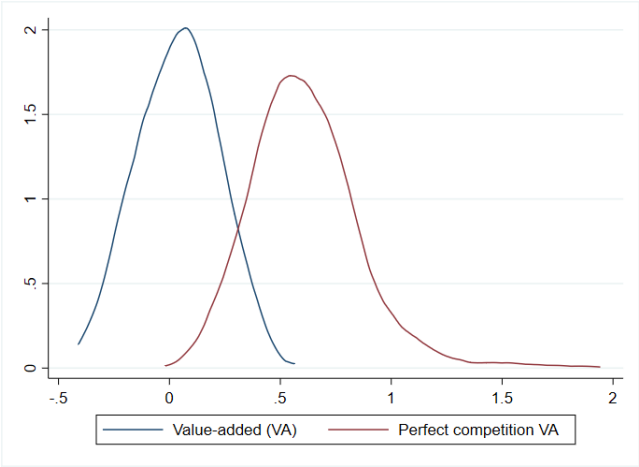
In this subsection, we explore what the model estimates mean for how public and charter schools compete.

Figure 4 shows the elasticity of demand for charter schools with respect to the quality of public schools. We counterfactually increase the quality of all public schools by 0.05σ (on the student distribution) and then compute the percentage change in enrollment. The figure plots densities of enrollment losses by type of charter school. The average charter schools loses fewer than 8% of its overall enrollment, indicative that charter demand is relatively inelastic to public school quality. Moreover, the figure shows that demand for Non-Traditional charters is relatively more inelastic than demand for Traditional charters, on average; the average enrollment loss for Non-Traditional charter schools is around half the average enrollment loss for Traditional charters. This finding indicates that, as hypothesized, curricular differentiation softens quality competition with public schools.

Figure 5 panel (a) plots public schools' own-value-added semi-elasticity of demand. These elasticities measure how elastic demand for a public school is with respect to its choice of value-



(a) Own-VA Semi-elasticity of Demand



(b) Value-added Markdowns

Figure 5: Competition and Supply of School Quality

added. These estimates can then be used to recover public schools' markdowns, which are reflective of their degree of market power. Figure 5 panel(b) displays their "perfect competition" quality levels alongside the distribution of value-added in the data. The figure indicates that, on average, quality is marked down by around 0.6σ (on the student distribution) from the level that would be supplied under perfect competition. The figure in panel (b) also shows that there is a right-tail of public schools whose perfect competition value-added is estimated to be extremely high. Our view is that the model does not fit these handful of schools well since their estimated elasticity of demand is very low.

Table 4: Equilibrium Effects of Strategic Differentiation
Counterfactual: 20% reduction in travel cost

	(1) All	(2) NonTrad	(3) Trad
Δ charter share	0.007	0.002	0.006
Δ VA (school-level)	0.010	0.002	0.009
Δ test scores (student-level	0.009	0.003	0.009

What does strategic differentiation by charter schools (in location and curricula) mean for the equilibrium level of public school quality? We explore this in Table 4 by counterfactually reducing travel costs to charter schools and re-computing equilibrium value-added. Column (1) reports the effects of a 20% reduction in travel costs to all charter schools (holding charter locations fixed). In this world, the charter share would increase by 0.7 percentage points and the average value-added

of public schools would rise by 0.007σ (on the student distribution). Column (2) carries out the same counterfactual, but leaves travel costs to Traditional charter schools unchanged. The results show that value-added only increase by 0.002σ when only travel costs to Non-Traditional charters in reduced. This is indicative that the competitive externality from these schools is limited. Indeed, column (3) shows that essentially all of the aggregate gains arise from reducing travel costs only to Traditional charter schools.

6 Counterfactual Policy Analyses

In this section, we present results of several policy analyses of interest. First, we consider the aggregate effects of North Carolina's removal of the charter school cap in 2011. We examine the effects across students, compare the benefits to costs (in dollar terms), and explore the supporting mechanisms.

We then turn to analyses of policies that explore the aggregate returns to screening new charter schools. As examples, we consider a counterfactual that disallows Non-Traditional charter schools from entering post-2012 as well as another that places a quality minimum on new charter entrants.

6.1 Charter Cap Not Lifted

We first use the model to evaluate the aggregate effects on student learning of North Carolina's lifting of its statewide cap on charters. We examine this by solving for counterfactual school enrollments and value-added in an equilibrium where the 29 post-2012 charter school entrants are removed from the three markets.[27]

Figure 6 summarizes the change in school-level value-added between the data and the no entry equilibrium. The figure shows that the modal public school is unaffected, but that a left tail reduces its quality meaningfully absent charter entry.[28] The average value-added reduction is 0.012σ (on the student distribution), as shown in Table 5. Table 5 also shows that the average student's test scores would be about 0.005σ lower than in the data. The student-level reduction is lower in magnitude

[27] We use a contraction mapping to find new equilibria, using the data values of value-added as starting values. For this counterfactual and those that follow, we fix value-added at the the data for those public schools whose estimated "perfect competition" level exceeds 2.5σ. We also restrict value-added choices to lie between -1 and 1 when solving (but this constraint does not bind in the new equilibria found).

[28] The second largest bin are actually schools who *increase* their quality somewhat because their residual demand becomes relatively more inelastic, as in McMillan (2004).

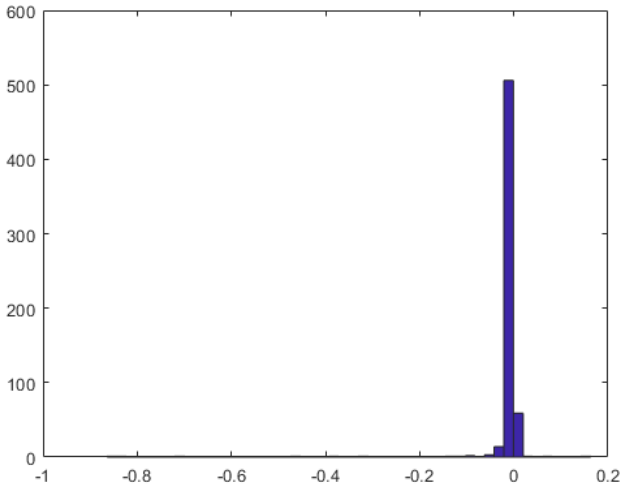


Figure 6: Change in Value-added (school-level)
Counterfactual: No post-2012 charter entry

than the school-level reduction because students re-optimize their enrollment choices. The test score impacts across student groups are also summarized in Table 5. Economically disadvantaged students benefit relatively more from charter school entry, which causes a 0.006σ increase in their test scores on average.

Table 5: Counterfactual: No post-2012 charter entry

Δ charter share	-0.014
Δ VA (school-level)	-0.012
Δ test scores (student-level)	
On average	-0.005
Non-disadv. & non-URM	-0.004
Econ. disadv.	-0.006
URM	-0.004

Is the aggregate effect on student learning from raising the charter school cap – a 0.005σ increase in test scores on average— economically meaningful? We benchmark this with Chetty et al. (2014)'s estimates of the causal impact of higher teacher quality on lifetime earnings. The impact of raising the cap on the average student translates into approximately a 0.22σ increase in teacher value-added one year or to about 1/5th the impact from replacing the bottom 5% of teachers with an average teacher.[29] These effect sizes translate into about a \$1,500-\$1,900 increase in lifetime

[29]These calculations reflect, respectively, an assumption that the policy effect is experienced for six years (K-5)

income (present value) for all students from lifting the charter schools cap.[30] Per marginal enrollee in charter schools, the surplus gain is over $100,000.[31] This benefit compares to a per enrollee cost to Durham, a relatively urban school district in North Carolina, of up to $7,000, as estimated by Ladd and Singleton (2020).

How do competitive incentives contribute to the gains from lifting the charter school gap? One way to assess this is by comparison with a counterfactual that allows new charters to enter post-2012, but in which public schools do not respond competitively. These results are shown in Table 6. Relative to the data, the average student's test score would be around 0.007σ lower with the competitive channel turned off. This finding is indicative that it is public schools' competitive responses to charter school entry that is driving the aggregate gains from lifting the charter cap.

Table 6: Role of Competition?
Compare with entry, but no competition counterfactual

	Average test scores
Entry + competition (data)	0.048
Counterfactuals	
No charter entry	0.044
Entry, no competition	0.042

We further investigate the mechanisms driving the results by decomposing the aggregate gains into the treatment effects on three group of students: those who would attend a charter school regardless of the cap lifting ("always takers"), those who attend a charter *because* of the cap lifting ("compliers"), and those inframarginal students who would not attend a charter school regardless ("never takers"). This latter group is affected by the cap lifting only via the competitive channel. This decomposition is shown in Table 7. Inframarginal students experience a 0.007σ increase in test scores on average. This magnitude is greater than the gain to the average student. This is because the students who choose to attend charter schools in the data actually pay a cost to do so in terms of human capital gains: the test scores of compliers, who would otherwise remain in public schools,

and that the test score impact of the bottom 5% for the average student is 0.0219σ, as reported by Gilraine et al. (2020).

[30] For the first calculation, Chetty et al. (2014) report that a 1σ increase in teacher value-added raises lifetime earnings by 1.34%. That translates the policy impact of lifting the cap into a $0.22\times1.34=0.29\%$ earnings increase. 0.29% of $522,000 (lifetime income in present value terms) is $1,517. For the second calculation, Chetty et al. (2014) estimate an impact of $250,000 per class or, approximately, $9,000 per student. $\$9{,}000\times\frac{0.005}{0.219}=\$1{,}931$.

[31] This number is arrived at by dividing the value of the average gain by the change in the charter school share under the no entry counterfactual (0.014).

are 0.03σ lower in the data than what they would be if the cap remained in place. This negative effect on compliers has an interesting parallel with recent findings from U.S. voucher programs (e.g. Abdulkadiroglu et al. 2017) and implicitly values the utility gains from choice expansion, which includes valuations places on other non-test outcomes, for these families.[32]

Table 7: Treatment Effects
Counterfactual: No post-2012 charter entry

	Data	No Entry	Share	Effect
Always takers	Charter	Charter	0.05	0.027
Compliers	Charter	Public	0.01	0.030
Never takers	Public	Public	0.93	-0.007

6.1.1 Effect of Removing All Charter Schools

While the previous counterfactual evaluates the effect of the 2011 removal of the statewide charter school cap by removing the 29 subsequent entrants, the model structure also allows us to compute outcomes in the counterfactual outcomes where all charter schools are removed from the markets. This policy analysis examines the effect of reducing the amount of charter school choice to zero.

Table 8: Counterfactual: No charter schools

Δ relative to data	No post-2012 entry	No charters
Charters	-29	-70
Charter share	-0.014	-0.069
Average VA	-0.012	-0.038
Average test scores	-0.005	-0.027
Non-disadv. & non-URM	-0.004	-0.027
Econ. disadv.	-0.006	-0.029
URM	-0.004	-0.026

Table 8 reports the results for school-level value-added and student test scores from removing all 70 charter schools and reducing charter school enrollment to zero. The results indicate that the average public school's value added would be nearly 0.04σ lower than in the data. For the average student, the reduction translates into a nearly 0.03σ reduction in test scores. The average test score impact is very similar across student groups. The effect size on test scores is several times the effect

[32]Compliers' MWTP for the cap lifting works out to about $9,000 on average.

of turning off just post-2012 charter school entry. Likely contributors to this larger magnitude are the facts that charter schools who entered prior to the cap lifting tend to be higher quality and are more likely to be traditional in their curriculum.

6.2 Screening Policies

We further use the model to study the aggregate returns to screening charter schools. These counterfactual simulations, which are stimulated by the policy question of how authorizers should evaluate new charter schools, provide the first evidence on the trade-off between more vs. fewer, but (prospectively) better charter schools on average. Moreover, an authorizer could screen charter entrants based on curriculum rather than difficult-to-predict quality. The results above suggest why this approach could in principle yield higher aggregate returns: curriculum choice differentiates charter schools horizontally, muting competition. Note that for these policy evaluations, we compute the new equilibrium but do not allow charter schools to re-optimize entry and location decisions.

Table 9: Screening Policies

Δ relative to data	No entry	No VA < 0	No NonTrad	Replace NonTrad w/ Trad VA $= -0.05\sigma$	Replace NonTrad w/ Trad VA $= 0.20\sigma$
Charters	-29	-15	-13	0	0
Charter share	-0.014	-0.005	-0.005	0.026	0.010
Average VA	-0.012	-0.005	0.002	0.007	0.002
Average test scores	-0.005	0.000	0.002	0.010	0.005
Non-disadv. & non-URM	-0.004	0.001	0.002	0.012	0.002
Econ. disadv.	-0.006	-0.002	0.003	0.011	0.003
URM	-0.004	0.000	0.002	0.012	0.003

We first consider a policy counterfactual that limits post-2012 charter entrants to just those 14 with above average (i.e. > 0) value-added. Table 9 shows that charter school share would be half a percent lower than in the data in this scenario. With fewer charters, competition is also lessened, which is reflected in the lowered value-added of the average public school. Notwithstanding this, the average student's test scores would be essentially unchanged as compared with the data. We next consider a a counterfactual that instead restricts post-2012 entrants to only the 16 Traditional

charter schools. Though this policy also contracts school choice (and reduces the charter share), we find that it yields a net *increase* in student test scores (of 0.002σ). This result is consistent with traditional charter schools tending to be higher-quality and—because they compete with public schools more directly on quality—generating more positive externalities.

We consider two additional counterfactuals which leave the total number of charter schools unchanged, but replace the Non-Traditional curriculum charters with Traditional charter schools (keeping the locations fixed). In one counterfactual, we replace the Non-Traditional charters with average quality (value-added = -0.05σ) Traditional charters, while the other replaces them with high-quality (value-added = 0.20σ) ones. As reported in Table 9, replacing all Non-Traditional charters with average quality Traditional charters would raise the charter share by nearly 3 percentage points, increase the average public school's value-added, and raise the average student's test scores by 0.01σ. This effect size is twice the impact of lifting the charter school cap. Interestingly, the aggregate effects of replacement with high-quality Traditional charter schools, while positive, are not as large in magnitude. There may be several reasons for this result, including that demand for public schools becomes more inelastic, similar to the theoretical result in McMillan (2004), since preferences for charter schooling and value-added are inversely correlated.

7 Conclusion

It has been over a quarter of a century since the first charter schools in the U.S. opened, but consensus regarding their aggregate effects on students learning remains elusive. A major reason for this is the bifurcated focus of prior empirical work on, on the one hand, treatment effects on students who attend charter schools or, on the other hand, competitive impacts on students who remain in public schools. Simply put, this paper asks: what do these prior findings add up to?

To answer this question, this paper combines school-level value-added estimates and quasi-experimental identification of public schools' competitive responses to charter entry in an equilibrium model of U.S. elementary education markets. In the model, students choose schools, public schools choose quality (value-added), and charter schools choose entry and location. We estimate the model using geocoded student-level data from North Carolina, whose lifting of the statewide charter school cap in 2011 both provides important variation that we leverage in the estimation and

the focus policy that we evaluate using the estimated model. The combination of data, model, and identification thus allow us to assess the aggregate returns to charter school choice—e.g. whether (and how much) the average student benefits. This is important as it permits, for the first time, calculating the benefits relative to costs and comparing charter school choice with other large-scale U.S. education reforms in dollar terms.

We report several major findings. The first is that curriculum choice horizontally differentiates charter schools, as hypothesized in Gilraine et al. (2021), and this has implications for competitive incentives. The second is that lifting the charter school cap generated economically-meaningful aggregate human capital returns (around $1,500 per student in lifetime income on average). We further show that competition is the channel driving these overall gains, as students induced into choosing charter schools due to the expansion experience negative test score impacts. Finally, counterfactual results suggest that returns to screening charter school entrants on quality are limited, but that screening on curriculum can maximize the positive competitive externalities from charter school choice.

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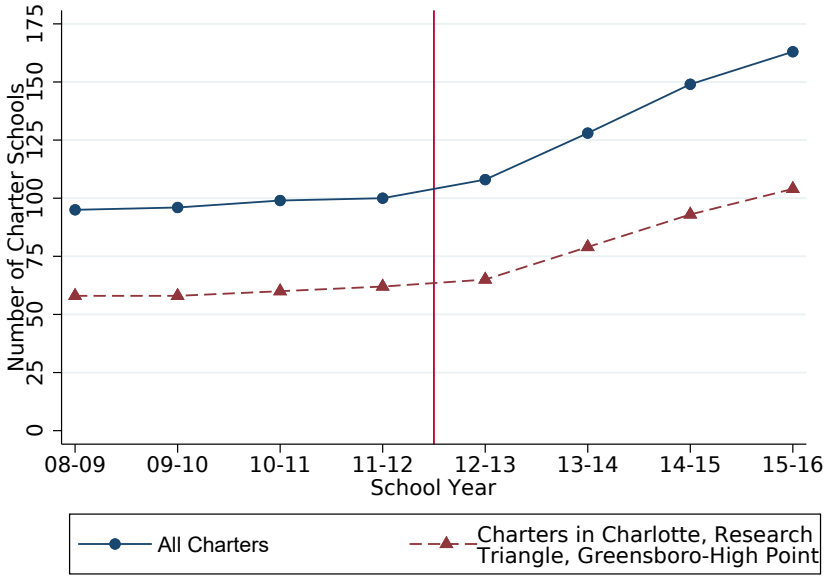
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A Appendix: Supplemental Statistics

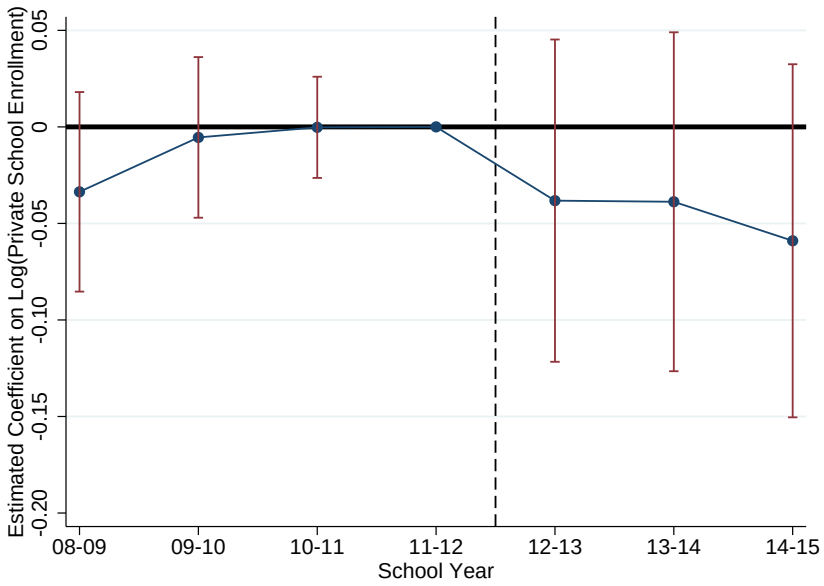
Figure A.1: Number of Charter Schools in North Carolina by Year



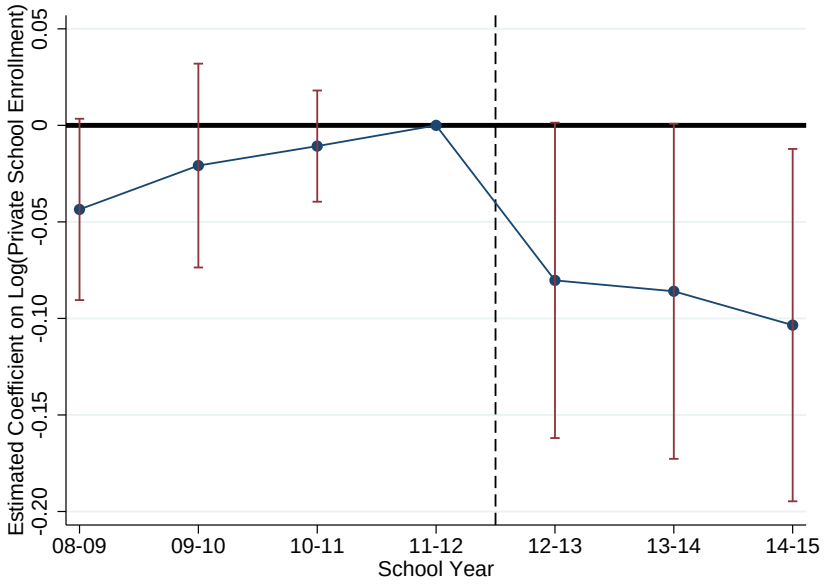
Notes: This figure displays the number of charter schools by year in North Carolina from 2008-09 to 2015-16, excluding two virtual charter schools that opened in 2015-16. The dashed line then displays the number of charter schools by year in the three commuting zones (Charlotte, Research Triangle, and Greensboro-High Point) that make up the "educational markets" that we analyze. The vertical line represents the lifting of the 100 school charter cap for the 2012-13 school year.

Figure A.2: Event Studies: Newly-Opened Charter on Nearby **Private** School Enrollment

(a) New Charter has Traditional Curriculum



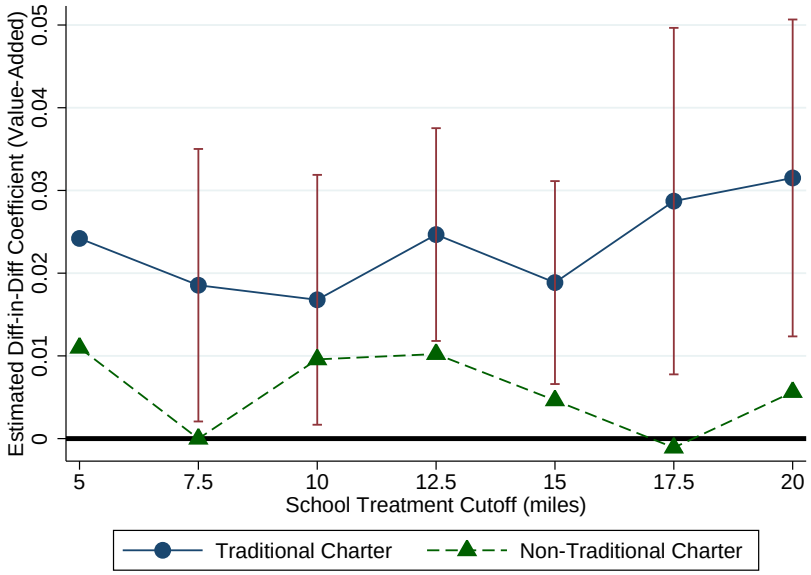
(b) New Charter has Non-Traditional Curriculum



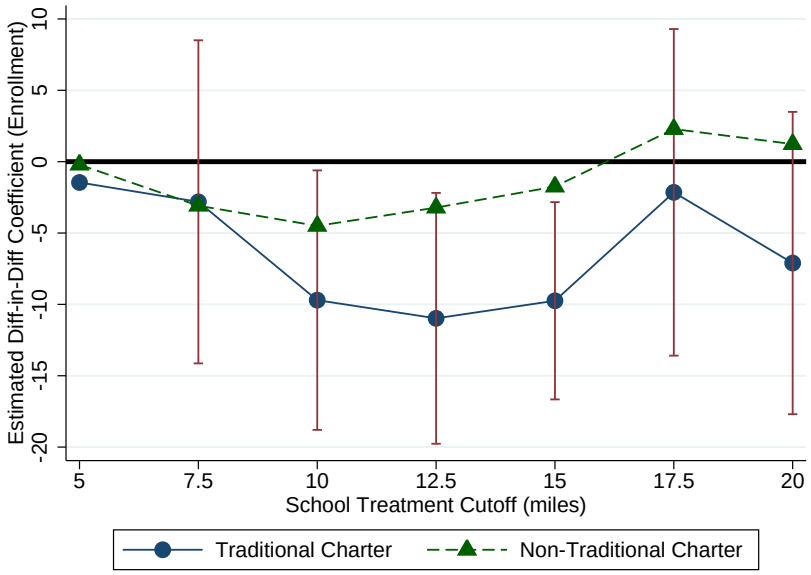
Notes: This figure shows the estimated school-level enrollment difference between **private** schools 'treated' by a newly-opened charter relative to 'control' private schools by year. Data are restricted to private schools that cover a K-2 grade and are located in Charlotte, the 'Research Triangle' (i.e., Raleigh-Durham-Chapel Hill), and Greensboro-High Point commuting zones. Treated private schools are defined as schools located within 20 miles of a newly-opened charter that opened in 2012-13 or 2013-14. Control private schools are defined as schools located between 20 and 30 miles of a charter schools that opened in 2012-13 or 2013-14. Results are subdivided by whether the newly-opened charter follows a traditional curriculum or not. Note that 2012-13 is considered the first 'treated' year because although the charters themselves opened in either the 2012-13 or 2013-14 school year, private schools would have known by the start of 2012-13 whether or not a charter was opening nearby or would open nearby in 2013-14. The dashed vertical line therefore separates the 'pre-years' from the 'post-years'. The horizontal line represents a point estimate of zero. The dashed 'whiskers' represent 90 percent confidence intervals with standard errors clustered at the school level.

Figure A.3: Robustness: Varying 'Treatment' Definition for Reduced-Form Difference-in-Differences Regressions

(a) School Value-Added



(b) School Enrollment



Notes: This figure shows robustness to Table A.2 in terms of the radius where schools are defined as 'treated.' In particular, we shrink the treatment radius from the 20 miles we used in Table A.2 in 2.5 miles increments all the way down to 5 miles. We then report the coefficients from this regression, both for traditional newly-opened charters (solid line) and non-traditional newly-opened charters (dashed line). Data are restricted to schools in the Charlotte, the 'Research Triangle' (i.e., Raleigh-Durham-Chapel Hill), and Greensboro-High Point commuting zones. Therefore the point estimates we report for the treatment radius of 20 miles are identical to those in columns (2) and (4) of Table A.2. Whiskers represent 90 percent confidence intervals with standard errors clustered at the school level.

Table A.1: Summary Statistics: Value-Added Sample

	Full Sample[1] (1)	Value-Added Sample[2] (2)
Mean of Student Characteristics		
Mathematics Score (σ)	0.00	0.02
Reading Score (σ)	0.00	0.02
Lagged Mathematics Score (σ)	0.02	0.02
Lagged Reading Score (σ)	0.02	0.02
% White	52.0	52.4
% Black	25.4	25.3
% Hispanic	14.4	14.3
% Asian	2.9	2.7
% Free or Reduced Price Lunch	52.0	52.2
% English Learners	6.1	5.5
% Gifted	15.1	15.9
% Students with Disability	13.2	12.9
# of Students	1,284,838	1,191,936
Observations (student-year)	2,238,703	2,084,317

[1] Data coverage: grades 4-5 from 2008-09 through 2016-17.
[2] The difference in sample sizes comparing columns (1) and (2) arises because we drop 154,386 million student-year observations that do not have contemporaneous or lagged math scores.

Table A.2: Difference-in-Differences Results: Public School Value-Added and Enrollment

Outcome:	Value-Added		Enrollment	
	All Schools (1)	Charlotte, Triangle, Greensboro CZs (2)	All Schools (3)	Charlotte, Triangle, Greensboro CZs (4)
Panel A. Pooled				
All Newly-Opened Charters	0.018*	0.019*	-2.79	-2.65
	(0.010)	(0.010)	(2.14)	(2.46)
Panel B. Heterogeneous				
Newly-Opened Charter follows Traditional Curriculum	0.028***	0.032***	-6.05**	-7.10**
	(0.011)	(0.012)	(2.74)	(3.25)
Newly-Opened Charter follows Non-Traditional Curriculum	0.003	0.006	0.65	1.23
	(0.009)	(0.009)	(2.80)	(3.13)
Test of Equality by Curriculum (p-value)	0.02	0.02	0.06	0.04
Observations (school-year)	25,596	21,525	44,694	37,273

Notes: This table shows difference-in-differences estimates from equation (8), whereby schools located within 20 miles of a newly-opened charter school are considered 'treated' while those located 20-30 miles from a newly-opened charter are considered 'control' and the effect is allow to differ by whether the newly-opened charter school follows a traditional or non-traditional curriculum. 'Test of Equality by Curriculum' reports the p-value of the hypothesis test that the point estimate for traditional curriculum charters is the same as the one for non-traditional curriculum charters. Standard errors are clustered at the school level. ***,** and * denote significance at the 1%, 5% and 10% levels, respectively.

B Appendix: Further Details on Charter Entry Event Studies

This Appendix sets out the estimating equations we use in Section 2.2.2 to capture the impact of nearby charter openings on public school quality and enrollment. To construct our data, we create a separate dataset for each of the 15 relevant charter school entries (see Section 2.2.2) that consist of all public schools within 30 miles of the newly-opened charter school. We then create dataset indicators and append our data together. We then run the following stacked difference-in-differences regression:

$$\begin{aligned} y_{scdt} = \delta_{sc} + \lambda_t + \gamma_d * t + \beta_{non\text{-}trad} Post_t * treat_{sc} + \mu distance_{sc} * Post_t * treat_{sc} \\ + Trad_c \left(\lambda_t + \gamma_d * t + \beta_{trad} Post_t * treat_{sc} + \nu distance_{sc} * Post_t * treat_{sc}\right) + \epsilon_{scdt}\,, \end{aligned} \tag{8}$$

where y_{scdt} is school VA or K-2 enrollment in school s nearby newly-opened charter c in district d at time t, $Post_t$ is a post-charter cap indicator (e.g., equals 1 if 2012-13 or later), $treat_{sc}$ is an indicator equal to one if a school is within 30 miles of the newly-opened charter and zero within 20-30 miles, $Trad_c$ is an indicator equal to on if the newly-opened charter follows a traditional curriculum, $distance_{sc}$ is the distance to the newly-opened charter, $\gamma_d * t$ are district (linear) time trends, and δ_{sc} and λ_t are school-by-charter opening and year fixed effects. The parameter $\beta_{nontrad}$ captures the average change between treated and untreated schools when a non-traditional charter opens, while the sum $\beta_{non\text{-}trad} + \beta_{trad}$ estimates the effect when traditional charters open. The results of equation (8) are reported in Table A.2.

To build Figure 2, we estimate an event-study version of equation (8) where we use event time indicators in lieu of the post indicator. Specifically, we regress:

$$\begin{aligned} y_{scdt} = \delta_{sc} + \lambda_t + \gamma_d * t + \sum_{\tau \neq -1} \beta^{\tau}_{non\text{-}trad}(D^{\tau}_t * treat_{sc}) + \mu distance_{sc} * Post_t * treat_{sc} \\ + Trad_c \left(\lambda_t + \gamma_d * t + \sum_{\tau \neq -1} \beta^{\tau}_{trad}(D^{\tau}_t * treat_{sc}) + \nu distance_{sc} * Post_t * treat_{sc}\right) + \epsilon_{scdt}\,, \end{aligned} \tag{9}$$

where D^{τ}_t are indicators equal to one if year t is τ years after (or before, if negative) 2012-13 (i.e., the year of charter entry) and 0 otherwise. All other variables are defined in equation (8). The coefficients β^{τ}_{trad} and $\beta^{\tau}_{non\text{-}trad}$ (along with their confidence intervals) are then plotted in Figures

2(a) and 2(b), respectively.

Private School Enrollment: To investigate the impact of nearby charter entry on private school enrollment we gather enrollment data from all private schools in the state from 2008-09 to 2014-15.[33] (We lack private school test scores so we cannot investigate quality responses by the private schools.) Unfortunately, these data only report school-level enrollment and the grades taught by the school. Therefore, we cannot create a K-2 enrollment measure, although we do drop all private schools that do not teach grades K-2. We also focus on schools with an enrollment of 10 or more students leaving us with a sample of 335 private schools of which 189 are located in our three CZs of interest.

We conduct a similar exercise as above to look at the impact of nearby charter entry on private school enrollment. We once again construct our data by creating a separate dataset for each of the 15 relevant charter school entries (see Section 2.2.2) that consist of all private schools within 30 miles of the newly-opened charter school. We then create dataset indicators and append our data together. We then run the event-study regression described by equation (9) using (log) school-level private school enrollment as the outcome. The coefficients (along with their confidence intervals) are then plotted in Figure A.2.

[33] Data are available from `https://ncadmin.nc.gov/public/private-school-information/nc-directory-private-schools`.

C Appendix: Further Details about TALAS

In this Appendix, we provide a greater overview of the TALAS program in North Carolina. Schools were placed into TALAS in two ways. Individual schools statewide fell into the program if their 2009-10 proficiency rates or (in the case of high schools) graduation rates were below a given threshold. In addition, the District and School Transformation division of the North Carolina Department of Public Instruction identified 12 school districts with substandard (district-wide) aggregate performance composites in the 2009-10 year and placed *all* schools in those districts into a similar district-level turnaround program. A total of 118 schools qualified as TALAS schools due to substandard performance composites or graduation rates, while an additional 188 schools fell into the program via the district route, resulting in a total of 306 treated schools.[34]

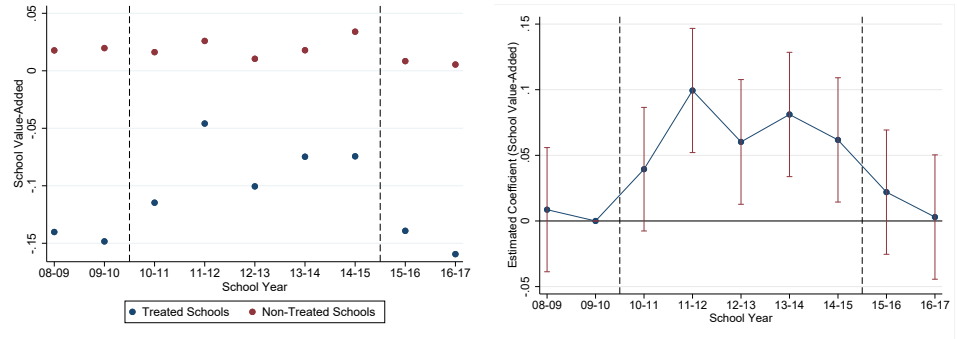
Previous evaluations of North Carolina's TALAS program are sensitive to whether an average or local average treatment effect is estimated and, by extension, whether schools that entered TALAS under the district-level program are included as treated schools in the analysis. Using a difference-in-differences framework to identify an average treatment effect, Henry et al. (2014) and Henry et al. (2015) find positive effects of TALAS on school proficiency rates. Importantly, the difference-in-differences framework also includes in the analysis schools that were treated under the district-level program and indicates especially strong gains among the previously lowest-performing schools. In contrast, Heissel and Ladd (2018) and Henry and Guthrie (2019) both use a regression discontinuity design to estimate a local average treatment effect among schools who qualified for TALAS because their 2009-10 proficiency rate was below the set threshold, thereby discarding schools that were treated under the district-level program and restricting the comparison to schools with previous proficiency rates close to the cutoff. Both studies find no (or even a small negative) effect on student achievement among elementary and middle schools.

To maximize the available data variation, in this paper we use a difference-in-differences approach to estimate an average treatment effect, thereby allowing us to include in the analysis all TALAS schools, not just those with a 2009-10 proficiency rate near the TALAS threshold. Figure C.1 shows the effect of TALAS on year-by-year school value-added. We show raw trends in value-added over time for TALAS treated and untreated schools in Figure C.1(a). The value-added

[34]For more information on how North Carolina implemented its Race to the Top turnaround programs, see `https://web.archive.org/web/20120919064916/http:/www.ncpublicschools.org/schooltransformation/overview/`.

of TALAS schools jumps when the program is fully implemented in 2011-12 and then exhibits a steady decline in subsequent years, eventually reverting back to pre-reform levels when the funding for TALAS program expires after the 2014-15 school year. As expected, the value-added of untreated schools follows a more stable trend. Figure C.1(b) presents regression-adjusted estimates (along with 95-percent confidence intervals) of year-by-year differentials in value-added between TALAS treated and untreated schools. Relative to untreated schools, the value-added of treated schools increases by 0.1 units, or 43 percent of a standard deviation, in the first year of the program's full implementation; by the 2015-16 academic year, however, the effect of the program disappears.

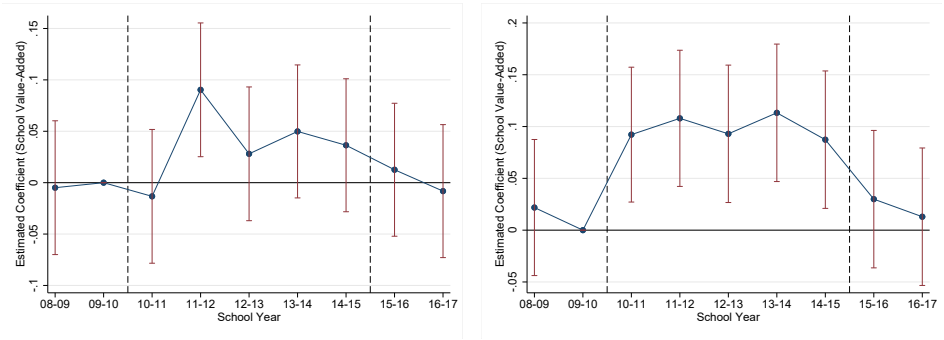
In Figure C.2, we show that average TALAS treatment effects are remarkably similar across the two types of treated school (that is, those that entered the program via either the school- or district-level routes), lending credence to our approach of pooling all treated schools to improve precision (as we do in Figure C.1) in our estimation approach in equation (7).



(a) Trends in School Value-Added (b) Regression Adjusted Differential Changes

Figure C.1: TALAS-Driven Variation in School Value-Added

Notes: This figure shows trends in school value-added over time for TALAS and non-TALAS schools. Panel (a) shows mean school-level value-added in each year for both types of school. Panel (b) plots regression-adjusted estimates of the difference in mean value-added across TALAS and non-TALAS schools in each year. The blue circles in panel (b) represent the estimated coefficients on academic-year-TALAS-indicator variables from a regression of school-year value-added on these variables, year fixed effects, and school fixed effects. The red bars represent the 95-percent confidence intervals associated with the coefficient estimates, with standard errors clustered at the school level.



(a) Effects of District-Level Treatment (b) Effects of School-Level Treatment

Figure C.2: TALAS-Driven Variation in School Value-Added by Treatment Type

Notes: This figure shows regression-adjusted estimates of the difference in mean value-added across TALAS and non-TALAS schools in each year. In panel (a), we define TALAS schools as only those schools that entered the program because the aggregate performance composite of their district in 2009-10 was below a threshold; in panel (b), we define TALAS schools as only those schools that entered the program because their school-specific performance composite in 2009-10 was below a threshold. In both panels, the blue circles represent the estimated coefficients on academic-year-TALAS-indicator variables from a regression of school-year value-added on these variables, year fixed effects, and school fixed effects. The red bars represent the 95-percent confidence intervals associated with the coefficient estimates, with standard errors clustered at the school level.