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Using a rich dataset that merges student-level school records with birth records, and leveraging three alternative identification strategies, we explore how increase in access to charter schools in twelve districts in Florida affects students remaining in traditional public schools (TPS). We consistently find that competition stemming from the opening of new charter schools improves reading—but not math—performance and it also decreases absenteeism of students who remain in the TPS. Results are modest in magnitude.

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I. Introduction

The role of charter schools as a means to expand public school choice has grown rapidly over the last thirty years. Forty-five states, plus Washington, D.C., Guam, and Puerto Rico, currently allow the creation of charter schools, and around 3.7 million students nationwide attend charter schools (National Association of Public Charter Schools, 2023).

A common argument for the expansion of school choice programs is that in addition to providing new educational options for students who enroll in schools of choice, these schools may also put competitive pressures on traditional public schools and incentivize them to deliver higher quality education to their remaining students. But despite how pervasive the charter school sector is, to date there are only a handful of studies that have explored the competitive effects of charter schools. Furthermore, we know little about the effects of charter competition on outcomes beyond test scores, while even the estimates for cognitive outcomes are mixed and appear to be context-specific.

While charter schools comprise a growing share of the public school marketplace, they still enrolled only about 6% of public school students nationwide as of 2017-18 (National Center for Education Statistics, 2019). This means that most students are affected by the availability of charter schools more indirectly, through the effects that the charter school sector has on the students who remain behind in the traditional public schools (TPS). Choice advocates have touted the potential for school choice programs to both provide outlets for students who feel poorly matched to their traditional public schools, and to stimulate competition that can incentivize all schools to improve the quality of education offered (Wolf & Egalite, 2016). Survey-based research (Loeb, Valant, & Kasman, 2011; Kasman & Loeb, 2013) and interview-based studies (e.g., Jabbar, 2015b; Bickmore, 2020) suggest that school principals respond to school competition through several channels, such as organizational changes, curricular improvements, and increasing efforts to market the school. While some of these changes (like advertising) are unlikely to be educationally productive, others may improve student outcomes (e.g., offering STEM curricular enhancements that may increase student achievement) or engagement (e.g., offering extracurriculars that may drive improved attendance (Krasman & Loeb, 2013; Bickmore, 2020; Jabbar, 2015b)).

On the other hand, choice skeptics have worried that charter schools may depress performance of students who remain behind in the traditional public school sector. For instance, losing students to charter schools may pose costs to schools as they lose per-pupil funding (e.g., Mann & Bruno, 2022); charter schools may recruit away high-quality public school teachers (Gao & Semykina, 2020); and students remaining in TPS may have different peer groups depending on the characteristics of students who exit TPS for charter schools, all of which may affect students' achievement. Because charter schools are generally anticipated to serve a minority of any given school population, the competition channel represents the mechanism posited to affect more students, and so whether charter competition affects public school students positive or negatively is a first-order question.

Despite these theoretical predictions, the competitive effects of charters has remained a relatively under-studied topic, and results are not consistent across papers (e.g., Hoxby, 2003; Sass, 2006; Booker et al., 2008; Zimmer & Buddin, 2009; Bettinger, 2005; Imberman, 2011; Winters, 2012; Davis, 2013; Jinnai, 2014; Cordes, 2018; Ridley and Terrier, 2018; Mann & Bruno, 2022; Gilraine et al., 2021; Slungaard Mumma, 2022). These inconsistencies likely spring from multiple sources, including different empirical strategies subject to different limitations, as well as differences in the institutional settings used across the variety of case-studies. While each investigation provides an important data point on the debate on competitive effects of charter schools, important gaps in the literature remain.

Several of the studies have been limited to single districts or a small set of districts (e.g., Winters, 2012; Cordes, 2018), while studies that have used statewide data or larger number of districts generally look at the very early years of charter policies and over relatively short periods of time (e.g., Bettinger, 2005; Bifluco and Ladd, 2006; Sass, 2006; Booker et al., 2008; Zimmer & Buddin, 2009 but see Jinnai, 2014 or Slungaard Mumma, 2022). Other studies that take a national perspective are limited to district-level data (Han & Keefe, 2020) or to state-level measures of charter competition (Davis, 2013). Updating and extending this literature is critical from policy perspective because competitive effects may change as charter sector matures and grows. In addition, due to data limitations, many prior studies cannot offer comprehensive heterogeneity analyses to understand who is impacted by the competition and do not investigate outcomes beyond test scores, yielding a dearth of evidence on how charter competition affects behavioral outcomes such as attendance. Finally, there has been scant evidence on how competitive effects of charter schools compare to and interact with different school choice options e.g., voucher schools. This is an important avenue to study as more states experiment with multiple options for school choice. These facts imply a need for well-identified studies to continue to build and update a body of evidence, and to scrutinize to what extent the mixed results reflect differences in the empirical or institutional settings that prior studies have looked at.

We propose to fill this gap in the literature, using a detailed longitudinal set of data from twelve large and diverse districts in Florida with an unusually rich set of measures and background characteristics allowing us to (a) provide novel and strong causal estimates of the competitive effects of charter schools using different fixed effects approaches (student-level and sibling-groups) and instrumental variables strategies estimating effects off of expected competition levels based on students' ZIP codes at birth; (b) look at heterogeneity in effects for different types of students and schools; (c) compare our preferred estimates to those obtained using methods previously used by other researchers; (d) address a novel question in the literature around whether competition from charter schools and private schools are substitutes or complements for each other; (e) explore potential mechanisms underlying the hypothesized competitive effects including changes in class size as well as peer and teacher composition; and finally (f) explore whether local- and district-level competition independently matter for the uncovered effects.

II. Background

Florida's charter school statute took effect in 1996, authorizing the creation of charters or the conversion of existing public schools to charter status with the agreement of 50% of existing teachers and 50% of current parents (Florida Statute 228.056, 1996)

While they operate with more freedom than traditional public schools, charter schools in Florida are subject to many of the same restrictions. For instance, charters are required to be non-sectarian, so they cannot incorporate religious practices. They are required to follow anti-discrimination statutes and to participate in state accountability assessments, and are prohibited from charging tuition (Florida Statute 1002.33, 2019). Charter schools, like traditional public schools, receive letter grades (A-F) from the state based on their student outcomes.

In addition to being subject to accountability through the state's testing system, advocates of charter schools argue that they are subject to multiple additional layers of accountability. Because no children are automatically "zoned" to charter schools, advocates argue that these schools are particularly accountable to parents and children. If families are dissatisfied with the charter school, they have ready alternatives in the form of their zoned public schools. Additionally, charter authorizers—generally local education agencies—are expected to exercise oversight over charter schools, and decline renewal where charter schools fail to meet performance expectations.[1]

Schools are generally expected to accept all grade-eligible students, with random selection to allocate spots if applicants exceed slots in the schools. However, schools may impose additional restrictions targeting specific populations (such as students at risk of academic failure). They may also include some additional requirements for admission, such as showing academic or artistic capability, when those requirements are aligned with the school's mission and purpose (Florida Statute 1002.33, 2019).

The charter school sector in Florida has grown explosively in the twenty years after its inception. Figure 1 traces the growth from 1998-99 to 2016-17 in terms of both the numbers of K-8 charter schools (solid blue line) and the share of public school K-8 students enrolled in charters (dashed blue line). Since our data are limited to 12 school districts, we also present the same statistics for this sample (orange lines). By 2016-17, the charter sector in Florida comprised 472 schools (309 of which, or 65%, operated in school districts for which we have the data) and served 11.6% of K-8 students in the state. This rate is comparable, at 13.3%, in the 12 school districts which bolsters our confidence that our results might generalize to the whole state.

Charter school growth has not been even across the state. Figure 2 provides a sense of the growth over time, as well as the variability in charter school enrollment across districts. It

[1] State universities can also serve as charter authorizers but for lab schools only, and community college district boards of trustees can serve as authorizers for career-technical education-oriented charters (Florida Statute 1002.32, 2019; Florida Statute 1002.34, 2019)

presents cumulative density functions for the share of children enrolled in charter schools in each district in the state (panel a) and in the 12 districts for which we have data (panel b) in each of the four years: 2000 (black line), 2005 (navy line), 2010 (maroon line), and 2015 (orange line). The dashed lines represent the highest share observed in each sample in a given year. Several points stand out. First, irrespective of the sample, there is substantial growth of the sector over time which is visible as a rightward shift in the CDFs. This confirms the evidence in Figure 1. Second, there is clearly substantive variation in charter school penetration across districts. For example, consider the 12 districts in 2015 where the average share of K-8 students attending charter schools was 3.8% in the bottom third and 15.1% in the top third. Equivalent numbers in 2000 were 0.3% and 1.9%, respectively. This suggests that we have ample variation, both across districts and in terms of growth within districts across time, from which to measure effects.

III. Methods

A. Data and Sample

This project draws on a unique and rich set of data constructed by merging student data from twelve Florida school districts with birth records from the Florida Department of Health. The former data includes information on all students in those districts, including basic demographics, test scores, absences, suspension, and exceptionality data for students in Grades PK-12. This educational data is merged to the birth record data for all students born in Florida between 1992 and 2002, which provide detailed measures of families' socioeconomic status at birth and place of birth within Florida. The latter data source facilitates construction of our instrument and detailed heterogeneity analysis by measures like parental education or income that have not been available in most prior research.

We place two limitations on our sample beyond the twelve district restriction. The first is that we primarily focus on outcomes for students in grades 3 to 8, because test scores serve as one of our main outcomes and they are most consistently available for this set of grades. When analyzing attendance, however, we expand the sample to grades 1 to 8. The second is that due to data availability and in order to have complete coverage of the rich set of measures provided by the birth records data, we restrict our sample only to those students with Florida birth certificates. Roughly 81 percent of children represented in our Florida birth records were ultimately observed in the statewide Florida public school data, which tracks closely with the share of Florida-born students who appear in Florida public schools according to the American Community Surveys (Figlio, Guryan, Karbownik, & Roth, 2014; Figlio, Hart & Karbownik, 2023). Records of children who started in a public Florida kindergarten but left the state prior to the start of testing in third grade, or who had missing test score information in all years, accounted for 14.8 and 0.8 percent of the remaining matched sample, respectively. This suggests that our data provides good coverage of the overall universe of students affected by the competitive pressures from charter schools.

Our main analytic sample includes student data for between roughly 225,000 and 865,000 unique students in the 2000-01 to 2016-17 school years, depending on the exact empirical specification and identification, although we use several additional prior years of data to characterize the initial competition levels for students in earlier cohorts as well. The lower-end of sample size range pertains to our sibling fixed effects strategy, where we require at least two siblings to be born between 1994 and 2002 as well as attend the same traditional public school in a given grade in order for a sibling set to enter the sample. In this sample the approximately

225,000 siblings come from 106,000 families.[2] The larger samples are for all births (singleton as well as siblings), which we use in individual fixed effects and instrumental variables models.

B. Models

We are interested in estimating how increased competition from charter schools affects achievement and behavioral outcomes among students remaining in traditional public schools. In estimating this parameter, we need to overcome several empirical challenges. First, on the demand side, parents and students may select into or out of particular TPS for unobserved reasons that are correlated with student achievement and behavior. Second, on the supply side, initial location and expansion of charter schools is unlikely to be random with respect to quality of TPS.

Previous research has sought to overcome the former problem by using student or student-by-school fixed effects which control for student and parent selection based on time-invariant characteristics (Sass, 2006; Bifulco & Ladd, 2006; Zimmer & Buddin, 2009; Winters, 2012; Gao & Semykina, 2017). Intuitively, models that employ student fixed effects use each student as their own comparison group, so that a student's relative performance in a year where their school faces little charter competition is compared to their own performance in a year where their charter faces more competition due to charter openings or closings. As a first step for comparability to prior research, we estimate similar models.

Specifically, we estimate equations of the following form, where Y_{igst} is an outcome for student i in grade g attending school s in year t:

[2] We cannot link siblings from 1992 and 1993 birth cohorts. The lower bound of 200,787 siblings from 96,302 families reflects data availability for our absence rate measure which we can observe between 2002-03 and 2009-10. The sample size is larger for mathematics (225,984 siblings from 106,418 families) and even larger for reading (231,408 siblings from 108,677 families) because the former data are available until 2013-14 while the latter until 2016-17, respectively. Both test score outcomes are available since 2000-01.

$$Y_{igst} = \beta CharterComp_{st} + \delta StudChar_{igst} + \gamma SchoolChar_{st} + \theta_{is} + \pi_g + \omega_t + \varepsilon_{igst} \quad (1)$$

Because these models include student-by-school fixed effects (θ_{is}), our main parameter of interest, β, identifies the effects of charter competition (*CharterComp)* based on changes in the extent of charter competition over time and across grade levels for students who remain in the same TPS for multiple years. We describe the competition measures more thoroughly below, but briefly they include the number of charters operating or number of charter students served within a given radius (usually 5 miles).[3] We include vectors of controls for time-varying student characteristics (like free and reduced-price lunch use; *StudChar*) and time-varying school characteristics (like demographic composition; *SchoolChar*). We further include year fixed effects (ω_t) and grade fixed effects (π_g) to account for year- and grade-specific shocks to outcomes. The error term, ε_{igst}, is clustered at school level.

A drawback of this method, however, is that estimated coefficients necessarily compute value-added style estimates and should be interpreted as growth rather than level effects. Beyond these two estimands being conceptually different, it could also matter from a policy perspective if there are non-linearities in effects across the distribution of baseline test scores as documented by some prior work (Nissar, 2017).

We propose two solutions to overcome this problem. In our first alternate identification strategy we use sibling-school-grade fixed effects. These models compare the outcomes of two or more siblings, each attending a given grade level in the same traditional public school in different years. Siblings serve as comparisons for each other, and we determine whether the outcomes of students who attend a given grade in a given school are systematically better (or poorer) when the school experiences more charter competition, compared to their siblings

[3] We can also measure competition at school-year-grade level, but these results are very similar to our preferred competition measure computed at school-year level.

attending the same grade in the same school under conditions of lighter charter competition. By using within-family comparison of siblings' same-grade outcomes, we control for unobserved family characteristics that affect child outcomes, such as parental expectations, preferences for non-traditional schools, or the ability to competently help with homework at a given grade level.

Note that the use of within-family comparisons assumes that changes in charter competition will not be systematically related to sibling performance (except through competitive pressures). That is, within all sibling pairs, there will generally be some variability in performance, with one sibling performing better (or attending school more consistently) than the other on average at a given grade. However, we assume that on average, these performance differentials should be randomly distributed across siblings. If these performance differentials are randomly distributed, they should be unrelated to charter competition, unless charter competition itself is driving any gaps in performance.

We estimate equations of the following form:

$$Y_{ifgst} = \beta CharterComp_{st} + \delta StudChar_{ifgst} + \gamma SchoolChar_{st} + \theta_{fgs} + \omega_t + \varepsilon_{ifgst} \qquad (2)$$

In Equation (2), the subscript *f* is used to identify families. The parameter of interest, β, is identified off of siblings attending the same public school in the same grade (captured by the sibling-grade-school fixed effect θ_{fgs}), but whose TPS faced different levels of charter school competition over time. We also include extensive set of individual controls ($StudChar_{ifgst}$), that vary across siblings, including birth order, birth timing, and socioeconomic conditions at birth to address characteristics that vary between siblings and ensure that they provide valid empirical contrasts. Other terms are defined as above in Equation (1). The error term, ε_{ifgst}, is clustered at school level.

One drawback to the sibling fixed effects approach is that it misses information from singleton children. We therefore complement this empirical strategy with a secondary, novel instrumental variables approach. In order to avoid potential endogeneity resulting from student sorting, we utilize an instrument that (1) predicts the charter school competition that a child will likely be exposed to, but that (2) cannot logically be a product of strategic decisions by families responding to the same conditions that concurrently shape charter location decisions. We use this instrument to predict *actual* competitive pressure faced by TPS students (determined by the TPS that they ultimately attend) by using information on their *expected* competitive pressure exposure based on their date and ZIP code of birth.

Specifically, we instrument with average level of school competition experienced by students born in ZIP code *z* and school cohort *c*. We construct this expected competition measure by simply calculating the average competition measure, excluding the student in question (i.e., akin to leave-one-out approach), for students born in a given ZIP code in a given academic cohort (September-August; $AvgChComp_{izc}$). We limit our analyses to ZIP codes that have had 500 or more births so that we can get stable competition estimates across cohorts and years. We obtain the realized competition measure based on each student-year in grades 3-8, and average these to create a single average competition measure for each student. We then use these to generate leave-one-out means of the expected grade 3-8 competition for students born in each ZIP code-cohort. Our first-stage equation predicts the level of competition faced by school *s* for a student *i* attending it in year *t* and grade *g*, given that the student was born in zip code *z* and cohort *c*:

$$CharterComp_{igzcst} = \tau AvgChComp_{izc} + \delta StudChar_{igzcst} + \gamma SchoolChar_{st}$$

$$+\pi_g + \omega_t + \varphi_z + \mu_c + \varepsilon_{igzcst} \qquad (3)$$

We also include grade and year fixed effects as well as individual- and school-level controls as explained above, and add ZIP code fixed effects (φ_z) and birth cohort fixed effects (μ_c). In the second stage we use the predicted values of competition $\widehat{CharterComp}_{igzcst}$ in place of the measure of actual charter competition experienced, and include the new ZIP and birth cohort fixed effects in addition to the year and grade fixed effects:

$$Y_{igzcst} = \beta \widehat{CharterComp}_{igzcst} + \delta StudChar_{igzcst} + \gamma SchoolChar_{st} + \pi_g + \omega_t + \varphi_z + \mu_c + \varepsilon_{izcgst} \qquad (4)$$

The use of this instrument addresses unobserved selection into competition-heavy or scarce environments. The identifying assumption is that parents do not select their residential location at the time of birth with an eye towards future (unpredictable) changes in quality of TPS. Therefore, incorporating information about residential location at birth to predict the level of charter competition expected in the absence of any strategic enrollment decisions by families should purge the estimates of bias from any strategic decisions that parents make in response to perceived changes in quality of TPS. The error term, ε_{izcgst}, is clustered at school level.

A primary concern with an IV approach is that it depends on the validity of the exclusion restriction. In our application, concerns would arise if changes in average charter competition faced by students born in a given ZIP code were correlated with student scores in some way other than through affecting students' own levels of competition faced (holding constant other factors that are invariant across students, cohorts and ZIP codes). This might be the case if, for example, the spread of charter schools were correlated with other initiatives that affected student scores. Since our ZIP code analyses rely on over 15 birth cohorts of students, we acknowledge that the policy environments faced by the earliest cohorts are likely different from those faced by later cohorts on factors other than charter competition. Nonetheless, we find it less likely that these policy changes are meaningfully correlated with charter penetration given that we also include year fixed effects. Furthermore, since the main concern in this case is changes in policy environments over time, we would still expect our sibling and individual fixed effects strategies to produce valid estimates since those estimates generally compare observations that have smaller differences in time between them.

Overall, the three empirical strategies allow for different set of strengths and weaknesses. To the extent that they produce similar results in terms of direction and statistical significance, as is the case in our application, we view them as complementary and supporting the notion of robust positive association between competition and student achievement. The fact that exact magnitudes of the estimated effects differ stems both from different sources of variation used and from somewhat different estimands.

C. Measures

Student Outcomes. We explore the effects of charter competition on several types of student outcomes which are measured at student-grade-year level and related to charter competition that students' schools experience in the same year. Our main student achievement measures are grade-by-year standardized math and reading test scores. Note that we have information on math scores for a more limited set of years than for reading scores; our final year of math scores is spring 2014 while it is spring 2017 for reading. We multiply our standardized test measures by 100 to more easily capture the modest effect sizes.

We also look at one main behavioral outcome for students: the absence rate, which reflects the ratio of the number of days students miss school due to unexcused absences or suspensions to the total number of days of attendance possible. This outcome is only available for school years 2002-03 to 2009-10. In other specifications, we look separately at absence rates (excluding suspension days) and suspension rates.

Competition. We use geocoded data on the location of public schools to construct measures of competition. Data on locations (latitude, longitude, and physical addresses) of traditional public schools, charter schools, and private schools are drawn from the Common Core of Data files maintained by the National Center for Education Statistics as well as from data provided by the Florida Department of Education.[4] These files also include data on the grade levels that each school serves, allowing us to directly measure the charter competition faced by a given school rather than assuming that these schools exert pressure uniformly regardless of grades served.

We construct two measures of competition from charter schools, for each traditional public school: **density** and **slots**. The "density" measure captures the number of charter schools serving the same grade range of students within a given radius of each traditional public school. The "slots" measure captures the number of students educated by charter schools in the same grade range within a given radius of each traditional public school.[5] We examine competition within a five miles radius in our main analyses, but look at other radii as well in robustness tests. In order to contribute to identifying variation, it is important that the measures of competition that we create vary within the units that they are grouped in for fixed effects analyses. In other

[4] Public school data includes latitude/longitude and physical addresses, while only physical addresses are provided for private schools. NCES data for private schools is incomplete but we were able to obtain annual lists of private schools from the Florida Department of Education.
[5] In select analyses we also use pre-determined (Figlio, Hart, and Karbownik 2023) and contemporaneous measures of private school competition. The former one is used in the heterogeneity analyses while the latter in a descriptive mediation analysis. We measure the predetermined competition based on private school landscapes in place in 2000, prior to the introduction of the Florida Tax Credit school voucher program, and use a single variable competitive pressure index. This variable is based on a principal components analysis of the following measures: private school proximity, private school density, diversity of potential competitors, the religiosity of the community, and private school enrollments. When it comes to the contemporaneous private school competition, we use density of private schools in a 5 mile vicinity of each TPS because most of the year 2000 measures are not available.

words, only students with non-constant levels of charter competition will contribute to identifying variation in the student-by-school fixed effects analyses, while only siblings who experience different charter landscapes within the same school-grade cell will contribute to identifying variation in the sibling fixed effects analyses.[6]

Control Variables. We create measures for several student and family attributes that will be used in some models as control variables, and in other models to determine whether there is heterogeneity of effects by these characteristics. We include standard controls drawn from student records, such as current economic disadvantage (proxied by use of free or reduced price lunch), but we also include indicators from our rich set of birth records, including whether the child's mother was born in the United States, whether the birth was paid for by Medicaid, mother's age at birth, mother's marital status at birth, mother's years of education at the child's birth, child sex, birth order, and the mother's race and ethnicity (Hispanic vs. non-Hispanic).

Finally, we construct several measures of school characteristics. The set of grade level fixed effects implicitly captures whether the school serves elementary (K-5) or middle school-grade (6-8) students. We also include school-level averages of the demographic variables; for instance, we capture the share of students who are male, who come from different racial/ethnic groups, and the share of students using subsidized lunch. Finally, we use data from the National Center of Education Statistics on student-teacher ratios and total enrollments.

Appendix Table A1 shows descriptive characteristics of our samples. Column (1) considers all births in Florida linked to school records in the 12 counties while columns (2) to (4)

[6] See Online Appendix for additional graphs (Figures A1, A2, and A3) showing the remaining variation in density and slots measures when we regress them onto relevant sets of fixed effects. Note that for instrumental variables we use the instrument as the dependent variable rather than the potentially endogenous contemporaneous competition measure. The figures show that we have the greatest remaining identifying variation in our sibling fixed effects analyses, while instrumental variables offer more variation than do individual FE. An exception here being the slots measure where we have more variation for the instrument than for sibling fixed effects.

consider different analytical samples we use. The characteristics are broadly similar across the different empirical samples and are comparable to the overall birth cohort from which we draw. A notable deviation is that our analytic samples (and especially our sibling fixed effect sample) have an overrepresentation of Black children compared to the overall birth cohort. Furthermore, the sibling fixed effects sample has fewer children whose mothers are foreign born. The table also shows that students in schools that face higher rates of competition from charter schools (above-median levels based on density measures and our sibling fixed effects sample) have lower test scores and absence rates compared to students in low-competition schools (Columns 5 and 6). This may reflect differences in the relative disadvantage of children attending high-competition public schools; students in high-competition schools are disproportionately Black, born to immigrant mothers, and born to mothers unmarried at the time of birth. It thus suggests the importance of using analytic strategies that purge the estimates of bias resulting from different student background characteristics correlated with both competition measures and student outcomes.

IV. Results

A. Main Results

Our main results (Table 1) present estimates from our student fixed effects (Columns 1, 4, and 7), sibling fixed effects (Columns 2, 5, and 8), and instrumental variables models (Columns 3, 6, and 9). The first three columns present results for math scores, columns 4-6 present reading scores, and columns 7-9 present results for absence rates. Panel A provides results using the density competition measure while Panel B provides results using the slots competition measure. For our IV results, additional test statistics are reported beneath the estimates. These show that our estimates meet the thresholds for strong instruments whether we use conventional F-statistics or Anderson-Rubin F statistics, as the *F* statistics are consistently above 100. Furthermore, we report both Anderson-Rubin 95% confidence intervals (Anderson & Rubin, 1949) and tF confidence intervals (Lee et al., 2022).

A few patterns stand out. First, we see variation in the pattern of results across outcomes. Charter competition is not consistently related to math scores; while some coefficients are positive and some are negative, none are significantly different than 0 at $p<0.05$. By contrast, we see that more charter competition is consistently associated with higher reading scores and lower absence rates. Both of these sets of outcomes suggest benefits to students attending schools with more charter competition.

Second, results are very similar in pattern whether we use our density or slots measure of competition. While the magnitude of the results differs—consistent with the fact that the underlying distributions of the density and slots measures are different—the qualitative take-aways are the same regardless of the competition measure used. In order to streamline presentation, we therefore concentrate on showing the results from the density measures in subsequent sections, as we believe that the density measure is less likely to be endogenous to school quality compared to the slots measure. Results are generally not sensitive to the specific measure used.

Third, except for mathematics, results are consistent in pattern across estimation strategies, although there is a fair amount of variation in the size of the coefficients. For instance, our density estimates using sibling fixed effects suggest that an increase of 10 charter schools within a 5-mile radius of a TPS would be associated with a 0.035 standard deviation increase in reading scores; our instrumental variables estimates suggest that the same increase would be associated with an increase of 0.079 standard deviation in reading scores while the individual fixed effects imply an increase of 0.041 standard deviations. Given the standard errors, the confidence intervals for mathematics and reading overlap but for absences the instrumental variables estimates are noticeably bigger. Notwithstanding this difference, the instrumental variables across all outcomes produce the largest (most favorable in terms of student outcomes) point estimates. On the other hand, individual and sibling fixed effects produce comparable point estimates for reading and absences (but not for mathematics where the coefficients are mostly statistically insignificant). In what follows we choose to focus on sibling fixed effects analyses since these give more conservative estimates and do not suffer from the value-added nature of the estimand.

We note that readers might plausibly think about the larger instrumental variables estimates as the upper bound on the competitive effects while the more conservative (and thus preferred by us) sibling fixed effects represent the lower bound on the competitive effects. Irrespective, in the Online Appendix, we also report relevant coefficients using individual fixed effects and instrumental variables strategies.

It is not clear ex ante if an expansion of ten charter schools is even remotely possible in the context of Florida, and in fact, as we have documented in Online Appendix Figures A1 to A3 the effective variation remaining in the treatment variable after taking into account fixed effects in our models is in the range of 0.57 (individual fixed effects for absences) to 0.85 (sibling fixed effects for reading) SD with majority of the variation falling in the +/- one school range. With that in mind, assuming an increase of one additional charter school, our preferred point estimates using sibling fixed effects would imply that for mathematics we can rule out negative effect sizes larger than 0.29 percent of a SD and positive effect sizes larger than 0.37 percent of a SD. At the same time, for reading, we find statistically significant effect size of 0.35 percent of a SD and we also identify statistically significant reductions in absenteeism of 0.74 percent of the sample mean.

At first glance, these point estimates may appear small, but we need to compare them to other findings in the school competition literature. For example, Figlio and Hart (2014), who looked at the introduction of the voucher program in Florida, found that an additional private school in a 5-mile vicinity of a traditional public school increases reading test scores by 0.2 percent of a standard deviation. We view these estimates as very comparable but note that Figlio and Hart (2014) also find gains in mathematics while our mathematics estimates are statistically insignificant at conventional level. Considering a broader expansion of Florida's voucher program Figlio, Hart, and Karbownik (2023) find that over the first ten years of the program – when it grew by more than 300% – students remaining in public schools accrued gains of approximately 10-15 percent of a standard deviation. Here, likewise our contemporaneous gains from just one additional charter school within 5 miles of a TPS look modest but economically meaningful.

Interestingly, our results contrast with those in Gilraine et al. (2021) who find no effects of expansion of charter schools in North Carolina on reading test scores and positive effects only for mathematics. More consistently with what we find, Ridley and Terrier (2018) document gains in reading due to the expansion of charter schools in Massachusetts. Their effect sizes imply that a 5 percentage points increase in share of students attending charter schools increases ELA scores by 2 percent of a SD, but they also find statistically significant gains in mathematics.

Almost no prior work has explored effects on behavioral outcomes, but our conservative effect sizes of -0.033 percentage points (or 0.74 percent of a sample mean) may appear relatively small at first glance. To put it in context, however, it is worth highlighting that in the same sample, the gap in absences between children with mothers who dropped out of high school and mothers who graduated from college is only 2.95 percentage points while the gap between children of White and Black mothers is only 0.49 percentage points. Compared to these longer-standing gaps our point estimates appear quantitatively meaningful.

Although the three methods we use (student fixed effects, sibling fixed effects, and instrumental variables) address—under assumptions of varying strength—student and family choices that may be correlated with competition, another source of endogeneity could stem from supply side decisions of charter schools. In particular, one may be concerned that these schools locate or increase their capacity strategically with an eye towards student achievement (either increasing capacity in places with higher-achieving students to post better scores themselves, or in places with lower-achieving students to draw students away from poor-performing public schools). To address this issue, we investigate whether past changes in student outcomes for traditional public schools are correlated with future changes in charter competition measured at school-by-year level. These results are presented in Table 2. Our outcome variables are changes in our two competition measures (density and slots in 100) between contemporaneous and prior school year, and we regress these on changes in test scores and absences which are lagged by one school year compared with the outcomes. In other words, the growth in competition between year *t* and year *t-1* is regressed on growth in student outcomes between year *t-1* and *t-2.* We do not find any statistically significant relationships between these variables for our preferred measure of competition (density at 5 miles). At the same time, the significant coefficients on reading and math scores for the slots measure suggest, if anything, that increases in test scores (i.e., improving environment in public schools) lead to future declines rather than increases in competitive pressures. Since these are opposite-signed compared to our main results reported in

Table 1, they suggest that any bias in our results would be in the direction of making charter competition seem less beneficial than it actually is.

B. Robustness

While the stability of the pattern of results across estimation strategy bolsters our confidence in our main results, we further probe whether results from our preferred sibling fixed effects estimation strategy remain robust to different modeling assumptions.[7]

Table 3 presents these results for the density measure of competition. Column 1 replicates our main results from Table 1, and Column 2 provides an alternative clustering at the family level. Statistical significance is unaffected. Column 3 shows that results are similar if we exclude all control variables except for the fixed effects. One concern with the sibling fixed effects models may be that by demanding that siblings attend the same school and same grade, we throw out a lot of information (e.g., in cases where we observe an older sibling's grade 8 scores but not the younger siblings because the panel ends when the younger sibling is in grade 6). Column 4 addresses this issue by using sibling-school rather than sibling-school-grade fixed effects. Results are similar in pattern, though the coefficient for the reading outcome increases under this specification, suggesting that our main results are conservative. Another concern in sibling fixed effects estimates is that our results may be driven by larger families, who both have more family members represented in analyses and may be more likely to have identifying variation given the likelihood that the competitive landscapes faced by a typical first-born and fourth-born sibling pair may be more different—introduce more variation—than the relative landscapes faced by first- and second-born sibling pairs. In Column 5, we check whether our

[7] We further performed a series of robustness analyses for the individual fixed effects and instrumental variables estimation strategies. These results are reported in Tables A2 and A3 in the Online Appendix, respectively. The conclusions remain unchanged.

results hold if we limit our sample to first- and second-born sibling pairs; we find that the pattern of results holds. Finally, Column 6 restricts the absence rate sample to include only grades 3-8, the same grades for which we observe test scores. Again, our conclusions remain the same as for the main specification, though the coefficient grows in magnitude which is likely related to the fact that absenteeism is lower in grades one and two.

Our preferred combined measure of absenteeism includes both days absent as well as days suspended. In Appendix Table A4 we further present effects of charter school competition on suspension rates and absence rates that net out days missed due to suspensions separately. These results suggest that total absenteeism result is primarily driven by reductions in truancy rather than in suspensions. In fact, in select specifications we see some increases in suspensions.

In all specifications thus far we used a 5-mile radius to define charter schools competition; however, this threshold, while popular in the extant literature, is arbitrary. Thus, in Figure 3, we present robustness of our results to measuring density of competition at various radii, at one-mile intervals, from 3 miles to 15 miles. We do not have sufficient statistical power to explore radii below 3 miles. We consistently find no statistically significant effects on mathematics test scores, positive effects on reading test scores, and negative effects on absences using different radii of competition. Effects are larger, but also less precisely estimated, at smaller radii. Even at a 15-mile radius, however, the effects remain statistically significant. Investigating more closely these point estimates suggests that, using 95% confidence intervals, we can reject negative effects on mathematics larger than 0.09 percent of a standard deviation, positive effects on reading smaller than 0.08 percent of a standard deviation, and reductions in absenteeism smaller than 0.25 percent of a sample mean. These results suggest that competitive pressures persist in a meaningful way even beyond the very localized level, a point which we come back to in Section V. Overall, these robustness checks bolster our confidence that the main findings are real and are not driven by our arguably arbitrary choice of 5-miles radius to measure competition in the preferred specification.

C. Heterogeneity

We conducted heterogeneity analyses for different student- and school-characteristics, again focusing on our sibling fixed effects measures and our density measure of competition.[8] Table 4 shows heterogeneity based on student characteristics. Because we use student characteristics as stratifying variables, we exclude those controls from our models. Column 1 therefore replicates our main results excluding student controls for sake of comparison; the results are very similar to the main results. Columns 2 to 5 then report results for different racial/ethnic/immigrant groups while columns 6 and 7 divide students by the median based on their socioeconomic status index which we generate using principal components analysis. The underlying variables in the PCA, reported in Online Appendix Table A5, include mother's years of education, marital status at birth, age at birth, the indicator for Medicaid-paid birth.[9]

As our sample is cut into smaller slices, our results become somewhat less stable and less precisely estimated across subgroups, which is evident in columns 2-7. The most consistent set of results is for absences: Results are null to negative across groups (i.e., more competition is associated with fewer student absences). While results are null for Black students with US-born mothers, and for students with foreign-born mothers (of any race), competition is associated with reductions in absences for White students of US-born mothers, and for Hispanic students of US-

[8] As with the robustness analyses, we also investigated heterogeneity using individual fixed effects and instrumental variables. These results are reported in Tables A6 to A9 in the online Appendix.

[9] Unequal number of observations above and below median in Table 4 stems from the fact that the index is constructed for the full population while our sibling sample is somewhat positive selected as reported in Online Appendix Table A1. Samples are almost balanced when considering the instrumental variables (Online Appendix Table A6) and the individual fixed effects (Online Appendix Table A7).

born mothers. Furthermore, we also see somewhat larger improvements in absenteeism in high SES households; while both above- and below-median SES students see reductions in absenteeism with charter competition, the result is significant only for more affluent students.

Considering reading test scores, the demographic and socioeconomic status picture is more mixed; especially when we compare the results across different identification strategies. For math, like in our main results, most coefficients are statistically insignificant although we note negative statistically significant effects for White students of US-born mothers (Column 2); this result is not statistically significant at conventional levels when using the instrumental variables (Online Appendix Table A6) or the individual fixed effects (Online Appendix Table A7) strategies, though coefficients remain negative. We do not have a clear explanation why the results would be more sensitive when considering test scores compared to discipline and opt for careful and caveated interpretation of these findings.

Table 5 provides heterogeneity analyses based on school characteristics. Column 1 includes a version of our baseline models where we exclude grade fixed effects since we are interested in studying effects of competition at different grade levels; test score results are somewhat larger than those reported in Table 1. Columns 2 and 3 show results from elementary and middle school grades, respectively. The main divergence comes in math; while competition has statistically insignificant relationship with student math scores at both school levels, coefficients are negative in middle school and positive in elementary school.[10] The pattern of results for reading appears more comparable across elementary and middle school grades, with somewhat more positive coefficients in middle school grades. For absences, we see statistically

[10] Instrumental variables results reported in Online Appendix Table A8 suggest that these opposite signed results are larger and statistically significant while Online Appendix Table A9 implies smaller estimates for middle compared to elementary school grades albeit both are positive and statistically insignificant.

significant reductions only in elementary school grades, though in light of the results presented in Online Appendix Table A9 when individual fixed effects are used, we suspect that the null results in middle school may be an artifact of the relatively small set of sibling pairs where both are observed in middle school during the relatively shorter analytic window available for this outcome.[11]

In the subsequent columns of Table 5, we investigate whether competitive effects of charter schools are muted or magnified by private school penetration. On the one hand, charter competition can be thought of as a substitute for private school competition and then we would expect our effects to be smaller in locations where there is more private school choice. On the other hand, it could be that more competition is always better, irrespective of its institutional source, and then we expect our effects to be larger in places with a lot of private schools. We divide the sample by the median of private school competition, based on private school landscapes measured in 2000, at the beginning of our analytic sample window and prior to the introduction of the Florida Tax Credit school voucher program (which may have induced private school entry that could have been endogenous to charter competition). To provide a context for this measure, traditional public schools above the median of the private competition measure have, on average, 30 to 32 private schools within 5-mile radius depending on the sample. On the other hand, those below the median have only about ten schools. Because our private competition measure relies on competitive landscapes as of 2000, we eliminate TPS that were

[11] Since our discipline data end in school year 2009-10, we have relatively few families where both siblings make it to this stage of schooling. This is visible in sample sizes that are one-eight of those in elementary school grades. For comparison, when considering individual fixed effects (Online Appendix Table A9), the middle school sample is about one-third of the elementary school sample. In this case the estimates for both stages of education are comparable. For instrumental variables (Online Appendix Table A8), the sample size in middle school is likewise much smaller because when constructing the instrument, we have to drop birth cohorts 1992 and 1993 – the eldest children born in our sample. Consistently these results are similar to those obtained using sibling fixed effects identification strategy.

not established as of 2000. Therefore, Column 4 first confirms that our main results are similar when limited to this subsample.

For mathematics, results are statistically insignificant regardless of the level of private school penetration and the identification strategy. For reading, positive results between charter competition and achievement seem to be driven by the schools facing larger degree of private school competition (although coefficients are more similar when considering the instrumental variables and the individual fixed effects). By contrast, for absences, we find that charter effects are larger when there is less private school competition in the local area, but here likewise the results differ across identification strategies. Given the instability of the results across identification strategies we are wary about drawing strong conclusions on whether private and public school choice are complements or substitutes. One hypothesis that we can reject, however, across multiple approaches is that more private school competition in places with elevated public school competition meaningfully harms students' outcomes. We view this as an important findings from education policy perspective.

V. Mechanisms

A final set of questions involves the potential mechanisms at play that may explain the relationship between charter competition and student outcomes. We explore several potential mechanisms in Table 6. Specifically, one possibility is that charter competition affects student outcomes by impacting the mix of peers that students are exposed to. If exposure to higher-achieving peers (or peers less likely to be absent) is a factor in improving students' own outcomes, then changes in peer groups may explain the relationship between charter schools' and beneficial changes in students' reading and absence outcomes. In Columns 1-3, we explore whether charter competition is associated with changes in average predicted levels of outcomes in math, reading, and absences. Rather than using the actual peer outcomes as dependent variables, here we use predicted values of math, reading, and absences based on the demographic characteristics of peers. Peer characteristics used to predict student outcomes include demographic, health, economic, and social variables. Predicted test scores for each student are then aggregated to the school-by-year level, and regressions of these predicted scores on charter density measures include school and year fixed effects.

Our results provide mixed evidence on the possibility of changes in peer compositions acting as a mechanism for our results. On one hand, we do see evidence that charter competition is associated with changes in peer composition that would suggest higher reading scores. On the other hand, there is no sizeable and statistically significant association with predicted mathematics scores or absences. Given that in Table 1 we reported gains in both reading and absenteeism, it is unlikely that peer effects meaningfully moderate the uncovered competitive effects for both outcomes. Nonetheless, we acknowledge that estimate in Column 2 of 0.110, computed at school level, is 32, 27, and 14 percent of the respective estimates reported in panel A of Table 1 using sibling fixed effects, individual fixed effects, and instrumental variables. At least the first two numbers of 32 and 27 percent appear non-trivial. However, note that some past literature suggests that a one standard deviation increase in peer quality would be associated with an approximately 5 percent of a standard deviation change in students' own scores (de Gendre and Salamanca, 2020); this suggests that effects of the magnitude that we estimate would likely translate to a 0.006 standard deviation change in students' own scores, reflecting 1.6% of our estimated effects from our sibling fixed effect models. On the other hand, some other papers suggest null average effects of peer's ability highlighting the importance of non-linearities (Lavy, Silva, and Weinhardt 2012; Imberman, Kugler, and Sacerdote 2012).

Another channel through which charter competition may affect student outcomes may be through changes in class size. It is not clear *ex ante* whether charter competition would increase or decrease class size. Smaller school enrollment due to charter competition could decrease class size, but it could also lead to a decrease in the number of sections of a given course (or grade level) offered; if there is contraction in the number of sections offered of a given course, class size could actually increase. Empirically, we find that more charter competition is indeed associated with smaller class size. Each additional charter school is associated with a decrease in average class size of 0.078 students. This is potentially a non-trivial effect given that past literature suggests that a reduction in class size of 7 students is associated with a 22 percent of a standard deviation increase in test scores (Krueger 1999; effects of similar magnitude are found by Angrist & Lavy,1999; Lindahl, 2005; Chetty et al., 2011; and Fredriksson, Ockert, & Oosterbeek, 2013). Assuming the same proportionate effect would apply to class size declines associated with charter competition, a 0.078 reduction in class size would translate to expected test scores improvements of 0.25 percent of a standard deviation (0.078 x 22/7 = 0.25) which is not that dissimilar from our preferred reading point estimate of 0.35. On the other hand, one would expect that smaller class sizes should likewise positively affect mathematics test scores (perhaps even more so), but we consistently find null effects for this outcome. Nonetheless, we do not discard class-size from the pool of possible mediators of the competitive effects.

We also investigate the relationships between charter school competition and teacher characteristics, including teacher experience (Columns 5-9) and teacher race/ethnicity (Columns 10-12). These outcomes are measured at school-by-year level and our data do not allow us to link students with their teachers in order to compute either value added (Chetty et al., 2014) or classroom level concordance effects (Gershenson et al. 2022). On one hand, in the short run, given the cost of firing and hiring teachers it could be unlikely that we observe meaningful changes in teacher composition. On the other hand, prior literature documented that teacher respond to changes in the characteristics of their students (Karbownik, 2020) and to competitive forces (Hensvik, 2012) even in the short-run.

We find statistically significant changes in both teacher experience as well as their racial/ethnic composition. One additional charter school increases average experience of teachers in TPS by 0.15 years and this effect is driven by declines in the share of teaches with relatively less experience (those below 5 years) and concurrent increases in the share of more experienced teachers (those with 6 or more years of experience). Although experience is an imperfect proxy for teacher quality, prior research showed that it is correlated with student achievements (Harris & Sass, 2011; Ladd & Sorensen, 2017). Despite highly statistically significant estimates, these effect sizes appear small. For example, the share of teachers with 0 to 2 years of experience declines by 2.7% (relative to the baseline mean) while the share of teachers with 13+ years of experience increases by 2.2%. When it comes to teacher race/ethnicity, we find reductions in the share of White teachers and increases in the share of Hispanic teachers. At the same time, there is no change in the share of Black teachers. Recall from Online Appendix Table A1 that schools located in above-median-competition locations have more minority and fewer White students as schools located in relatively less competitive areas. Thus, if there is indeed racial complementarity between students and their teachers, such sorting could be a mechanism at play; at least for the Hispanic students.

Given that prior research from Florida suggested that private school competition at both local- and district-level could matter independently (Figlio, Hart, & Karbownik 2023), we further attempt to verify this proposition when it comes to the charter competition. The role of a district-level competition could be particularly important in the public school choice context given that TPS and charter schools may be regulated by the same superintendent. Of course, school-level competition – measured in terms of geographic proximity – is also likely measured with more error than district-level competition, as counties have defined boundaries, so that could also help to explain a finding of district-level competition mattering more than school-level competition.

To examine this question, we create a district-level version of our competition measure which is created as a simple district-by-year level mean of our 5-mile radius local competition measure, with each school representing one unit in calculating the district mean for each year. Student-weighted Pearson correlation coefficient (r = 0.63 to r = 0.73 depending on the sample) between the two measures suggests modest to strong correlation, but nonetheless there is a degree of independence between school- and district-level competition.

Results are given in Table 7. Column 1 replicates our main specification but controls for imputations of mechanisms when these are not available, while column 2 supplements the school-level competition measure with district-level competition.[12] Akin to almost all our previous results we do not find any statistically significant effects of either of the measures for mathematics test scores. On the other hand, for both reading test scores and absences, we find that district-level competition dominates the school-level measure and the coefficients become meaningfully larger. Furthermore, in both cases the latter coefficient becomes statistically insignificant and, for reading, it even flips the sign. This pattern of results suggests that unlike for voucher competition (Figlio, Hart, & Karbownik, 2023), for charters the district-level

[12] Information about class size is available for the years 2006-2007 to 2016-2017, information for average salaries is available for the years 2004-2005 to 2016-2017, information on teacher characteristics is available for the years 2002-2003 to 2011-2012. Information about peer effects and private school penetration is available for all years used in respective samples for mathematics, reading, and absences. To maintain constant sample size, we perform the following imputations for variables with missing values due to differential coverage of years: (i) if available, impute mean school level values, and (ii) if school-level information is not available, impute sample average. Column 1 controls for these imputations, but not for any of the mechanism variables themselves.

competitive forces are driving student improvements with little additional role for school-level competition once district-level competition is controlled for.[13] Furthermore, the finding has an important potential implication for the feasibility of other charter competition studies as it might be sufficient to collect competition data at that district level, rather than at the very local level, in order to obtain reliable estimates.

In subsequent columns of Table 7, we explore to what extent various mechanisms, including those documented in Table 6, district-level teacher salaries, and the penetration of private schools could explain our competitive effects. This is a descriptive exercise. First, in Column 3, we control for teacher salaries, which addresses the issue that districts with more competition could also be offering higher salaries to recruit and retain a more stable and potentially higher-quality teaching workforce. Adding this control has minimal effect on the reading coefficient and modest effect for absences. Second, there is a concern that private school choice options, which have been shown to improve student outcomes in Florida, have been growing concurrently with the charter competition and we misattribute these effects of public school choice effects. Thus, in Column 4, we control for density of private schools within 5-miles of the TPS. Unlike the fixed, year-2000 private school landscape used in the heterogeneity analysis, this control variable measures contemporaneous level of private school penetration, and thus varies at school-by-year level. Although there is little change in reading score, the estimate for absences declines by over 40 percent compared to Column 2. Nonetheless, both effects remain statistically significant at conventional levels and retain their original directions. We then explore, in Columns 5 to 7, whether the inclusion of, respectively, school-level class-size

[13] This finding is not driven by using a narrow radius of 5 miles to define local competition. When considering a radius of 15 miles – the largest we included in Figure 3 – we find qualitatively similar results; neither level of competition significantly affects mathematics scores while for reading scores and absences the gains are solely due to district-level competition measures.

information, teacher characteristics, or predicted peer outcomes introduced in Table 6 alters the results. Despite statistically significant effects for some of these mechanisms documented above, adding these variables as controls does little to meaningfully move the coefficients on district-level charter school competition. Finally, in Column 8, we include all district- and school-level variables simultaneously. For all three outcomes, the school-level competition coefficient is never statistically significant while district-level competition remains a statistically significant predictor for both reading test scores and absences. When all mechanism variables are added, the coefficient on district-level competition declines by 35% for reading estimates compared to the estimates in Column 2, while the same coefficient declines by 23% for our absences estimates. This means that, depending on the outcome, between a quarter and a third of the student gains can be descriptively associated with changes in the mechanism variables considered in this paper. The remainder could be due to either other unobserved mechanisms or direct effects of competition.

VI. Conclusions

School choice programs – including public charter and private voucher options – have been growing in the United States and worldwide over the past two decades, and thus there is considerable interest in how these policies affect students remaining in traditional public schools. From a policy perspective, charter schools are especially important as they often compete for the same students, educators, and resources as traditional public schools. On the one hand, increased competition from the charter sector could lead to a decline in outcomes of students left behind if they indeed cream-skim best students and drain the resources. On the other hand, choice advocates have touted the potential for such public choice programs to both provide positive alternatives for students who feel poorly matched to their default traditional public schools and to stimulate competition that can incentivize all schools to improve the quality of education offered. We find, across multiple complementary identification strategies, consistent evidence that increased charter school penetration improves reading test scores and absenteeism of students remaining in traditional public schools. At the same time, unlike some prior work, our effects for mathematics test scores are not statistically significant at conventional levels. The results are robust to plausible alternative empirical specifications.

The null finding on math scores is particularly notable because a more common pattern in education research is to find results on math but not reading (see, e.g., Dee and Jacob (2011) or Gilraine et al. (2021)). This is often explained through non-school factors—like family involvement—that are considered to be important inputs into literacy outcomes, while math outcomes tend to depend more heavily on school—rather than non-school—factors. Our null findings are therefore somewhat surprising and deserve further exploration in future work from researchers better poised to study in-school mechanisms through which competition may affect student outcomes, such as curricular or policy adjustments.

Our results comport with some notable public school achievement trends in Florida. In particular, the state had been singled out for relatively strong growth on well-established, low-stakes test like the National Assessment for Educational Progress (Postal, 2018). While the results we present here cannot account for the full level of improvement that Florida has seen over the last several decades, they do suggest that charter school competition may be contributing to those results; at least for reading and general literacy. This is in addition to benefits from private school competition documented in Figlio and Hart (2014) and Figlio, Hart, and Karbownik (2023).

We acknowledge several significant limitations to our work. Our study is based in Florida, which has a particularly robust degree of school competition; our results may not replicate in other states, and indeed, may not replicate in other parts of the state of Florida not included in this analysis (though the twelve districts in question appear to be representative of the broader state-level charter competitive landscape). Moreover, despite large samples sizes – compared with much prior literature – our heterogeneity analyses still mostly lack precision and these results are particularly sensitive to the choice of identification strategy. This suggest that caution should be applied to the heterogeneity analyses presented in other studies – especially if they use only a single identification strategy.

In addition, while we are able to provide strong causal evidence of the (modest) effects of competition, we are limited by data constraints from looking too deeply into the mechanisms that might either magnify or mute the effects of geographically proximate competitors on school leaders' perceived levels of competition. Additional work on this question building on extant qualitative literature (e.g., Jabbar, 2015a; 2015b; Blickmore, 2020) would be welcome to help further explain the important results we derive on effects of charter competition in the Florida context; especially the differential mathematics and reading findings.

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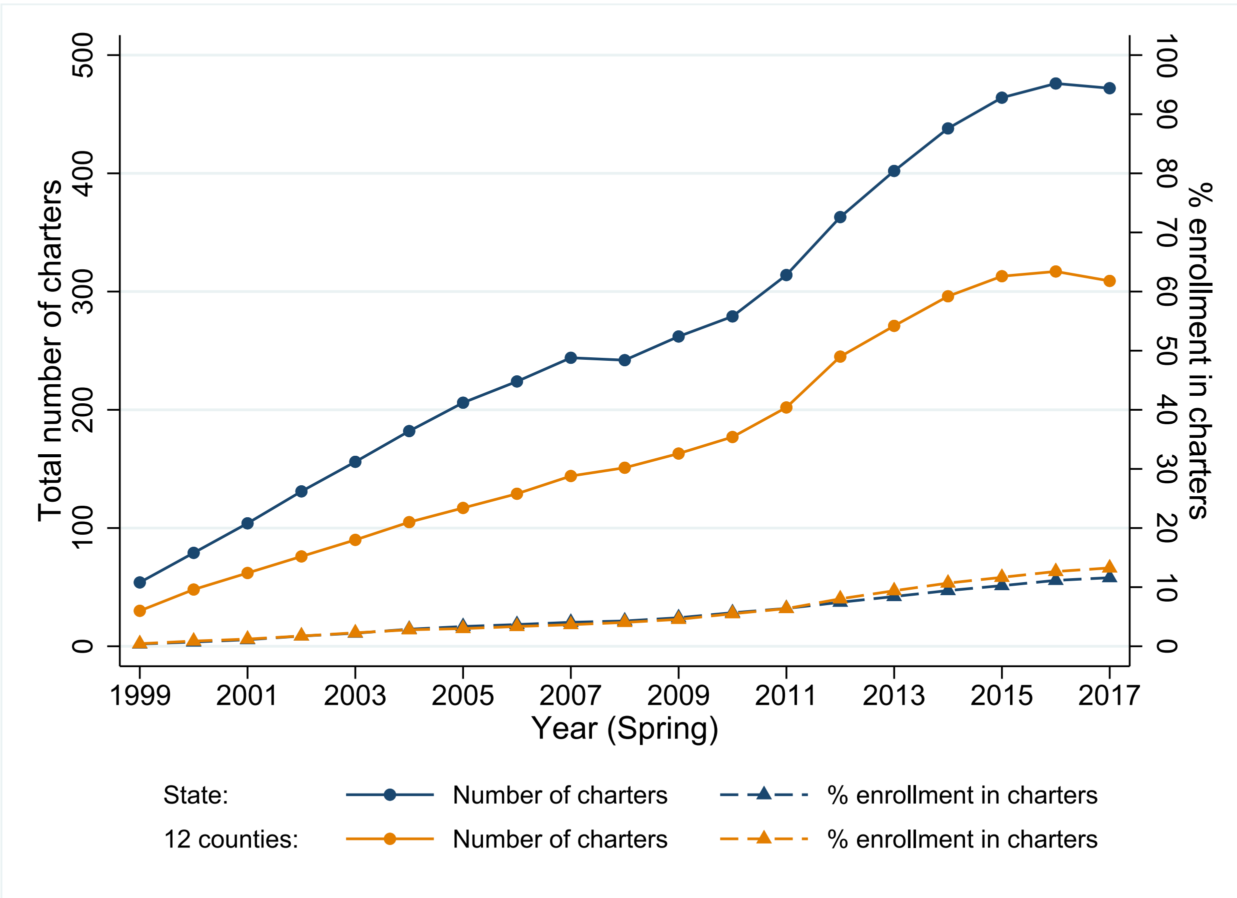
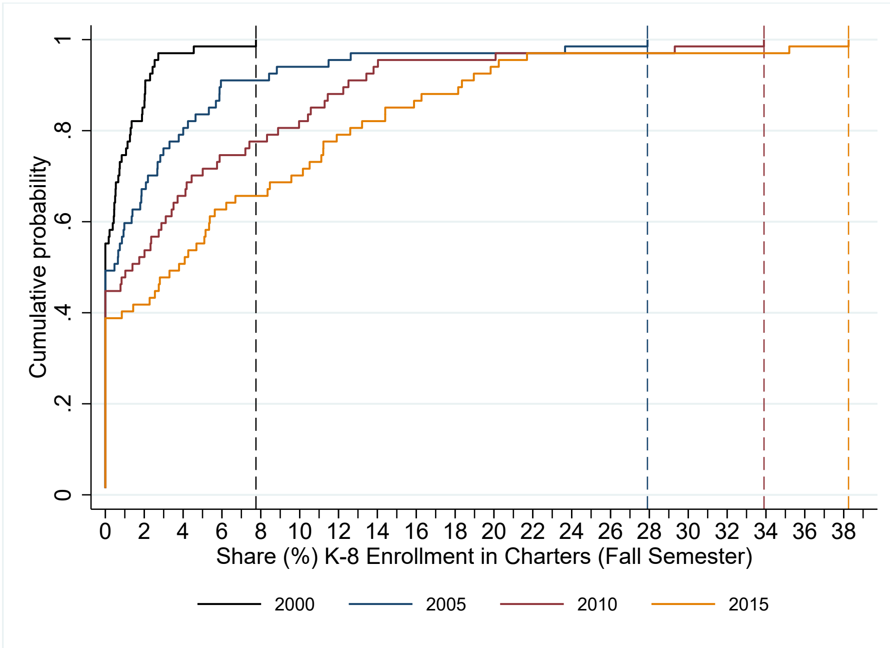
Figures

Figure 1: Growth of Charter Schools (Counts) and Enrollment Over Time

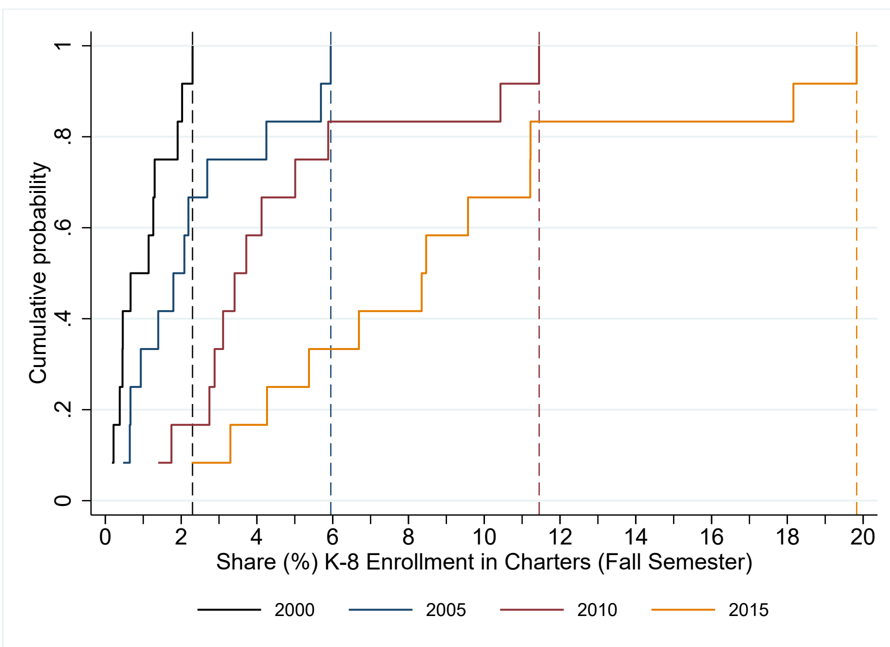


Notes: This figure depicts total number of K-8 charter schools in a given year operating in Florida (blue solid line) and in the 12 counties for which we have data (orange solid line) as well as the fraction of K-8 students enrolled in charter school in the state (blue dashed line) and in the 12 counties (orange dashed line).

Figure 2: Cumulative Density Functions of Charter School Enrollment



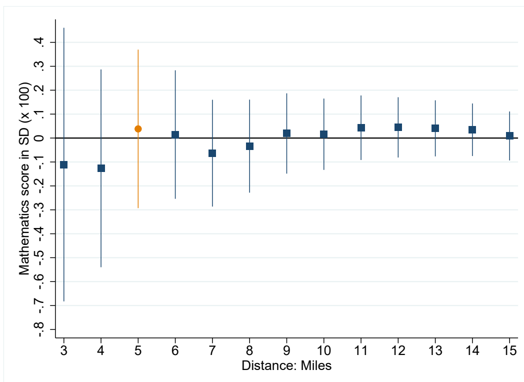
(a) State data



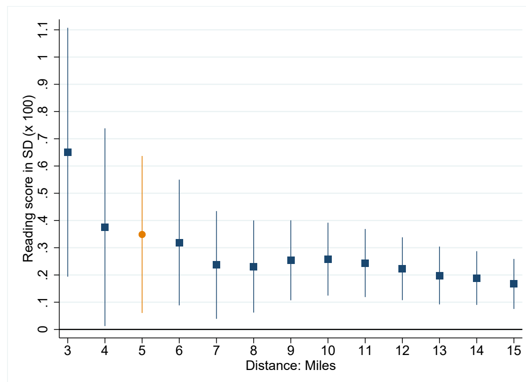
(b) 12 counties

Notes: These figures present CDF plots for the share of K-8 children enrolled in charter schools in Florida. Panel (a) presents graph for the full state (67 school districts) while panel (b) presents graph for the 12 districts used in this paper. Black line denotes density in the Fall of 2000, navy line denotes density in the Fall of 2005, maroon line denotes density in the Fall of 2010, and orange line denotes density in the Fall of 2015. The dashed lines reference highest share in a given year and sample.

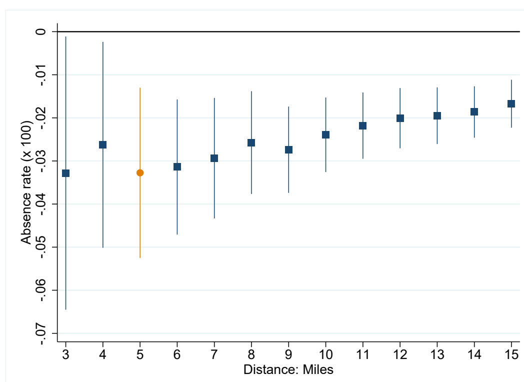
Figure 3: Robustness to Changing Distance in Treatment Definition: Sibling Fixed Effects



(a) Mathematics



(b) Reading



(c) Absence rates

Notes: These figures present estimates based on specifications from panel A of columns 2, 5, and 8 in Table 1 where we replace our competition treatment variable with competition measured at a given radii from 3 to 15 miles. Panel (a) presents results for mathematics test scores, panel (b) for reading test scores, and panel (c) for absence rates. Orange marked estimates present our preferred specifications from Table 1. Standard errors clustered at school level and spikes reflect 95% confidence intervals.

Tables

Table 1: Main Results: Individual Fixed Effects, Sibling Fixed Effects, and Instrumental Variables

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Mathematics score (x 100)			Reading score (x 100)			Absence rate (x 100)		
	Individual FE	Sibling FE	IV	Individual FE	Sibling FE	IV	Individual FE	Sibling FE	IV
A. Density	-0.221*	0.038	0.251	0.409***	0.349**	0.791***	-0.032***	-0.033***	-0.263***
	(0.125)	(0.169)	(0.251)	(0.095)	(0.147)	(0.178)	(0.009)	(0.010)	(0.052)
F/Anderson-Rubin F			812.2/555.0			701.8/836.6			189.6/345.4
A-R 95% conf. set			(-0.235, 0.737)			(0.445, 1.137)			(-0.372, -0.171)
tF 95% conf. interval			(-0.240, 0.742)			(0.442, 1.141)			(-0.365, -0.161)
B. Slots in 100	-0.067	0.064	0.192*	0.281***	0.261***	0.272***	-0.019***	-0.019***	-0.122***
	(0.059)	(0.075)	(0.086)	(0.039)	(0.067)	(0.062)	(0.004)	(0.004)	(0.020)
F/Anderson-Rubin F			531.0/552.0			390.5/827.0			158.8/338.3
A-R 95% conf. set			(0.024, 0.359)			(0.153, 0.391)			(-0.164, -0.087)
tF 95% conf. interval			(0.023, 0.361)			(0.152, 0.392)			(-0.160, -0.084)
Mean of Y	0.745	12.969	3.678	0.373	8.630	3.067	5.256	4.476	4.997
# students	862,066	225,984	663,088	865,120	231,408	665,927	836,707	200,787	615,749
Observations	4,006,932	808,419	3,051,507	4,289,265	912,514	3,319,642	3,633,474	617,017	2,792,787

Note: This table presents main results for mathematics test scores in grades 3 to 8 (columns 1 to 3), reading test scores in grades 3 to 8 (columns 4 to 6), and absence rate in grades 1 to 8 (columns 7 to 9). Treatment variable of interest in panel A is charter school density measured within 5 miles of each traditional public school while in panel B it is number of charter slots in 100 measured within 5 miles of each traditional public school. Test scores are standardized based on the full sample with mean 0 and standard deviation 100. Absences combine days suspended in school and days absent in school which we divide by total number of school days. Absence rate is multiplies by 100. Columns 1, 4, and 7 present individual fixed effects analysis; columns 2, 5, and 8 present sibling fixed effects analysis; and columns 3, 6, and 9 present instrumental variables analysis. Mathematics test scores are available for school years 2000/01 to 2013/14; reading test scores are available for school years 2000/01 to 2016/17; and absence information is available for school years 2002/03 to 2009/10. Sibling fixed effects and instrumental variables estimations are limited to birth cohorts 1994 to 2002 while individual fixed effects are limited to students born between 1992 and 2002. Individual fixed effects models include individual-by-school, grade and year fixed effects as well as following school-by-year level controls: fraction males, fraction Black students, fraction Hispanic students; student-teacher ratio, and total enrollment. We also control for free and reduced price lunch status in these regressions. Sibling fixed effects models are limited to families with at least two siblings in the sample and we only include observations where at least two siblings are observed in each school and grade. The models include mother-by-school-by-grade fixed effects, years fixed effects, and the following individual level control variables: married at the time of birth, maternal age at birth, maternal education groups (high school dropout, high school graduate, and college graduate), birth order fixed effects, month and year of birth fixed effects, gender indicator, Medicaid paid birth indicator, free and reduced price lunch indicator, and the following school-by-year level variables: fraction males, fraction Black students, fraction Hispanic students; student-teacher ratio, and total enrollment. Instrumental variables models use zip code of birth-by-birth cohort leave-one-out average competition measures as an instrument. In that, for each individual we first compute the average competition measures they face in grades 3 to 8 across all school years we observe them. Then, for each individual we compute the average competition measure they face based on their zip code and cohort of birth but excluding themselves from these averages. Cohort of birth is defined based on school starting age i.e., it runs from September of year t to August of year t+1. Both conventional and Anderson-Rubin Wald test F-statistics from first stage regressions are displayed below the standard errors in each IV column. We additionally provide the 95% Anderson and Rubin (1949) confidence sets and the 95% tF confidence intervals following Lee et al. (2022). We include the following set of fixed effects in the instrumental variables models: zip code of birth, cohort of birth, as well as school year and grade at which we measure outcomes. Additional control variables include indicators for Black and Hispanic students, indicator for mother born outside of US, indicator for mother married at birth, maternal age at birth, maternal education groups (high school dropout, high school graduate, and college graduate), indicators for number of prior births to the mother, gender indicator, Medicaid paid birth indicator, and free or reduced price lunch indicator. We also include the following school-by-year level variables: fraction males, fraction Black students, fraction Hispanic students; student-teacher ratio, and total enrollment. Standard errors in all models are clustered at school level. ***, ** and * mark estimates statistically different from zero at the 99, 95 and 90 percent confidence level.

Table 2: Testing Identifying Assumptions

	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent variable: Competition in year t minus competiton in year t-1.					
	Density at 5 miles			Slots/100 at 5 miles		
Treatment	Math score	Reading score	Absences	Math score	Reading score	Absences
	Panel A. No controls					
Treatment in year t-1 minus treatment in t-2	-0.014	-0.070	0.012	-0.285**	-0.207*	-0.002
	(0.056)	(0.054)	(0.009)	(0.118)	(0.109)	(0.017)
	Panel B. Including control variables					
Treatment in year t-1 minus treatment in t-2	-0.014	-0.067	0.012	-0.284**	-0.205*	-0.003
	(0.056)	(0.054)	(0.008)	(0.118)	(0.109)	(0.017)
Mean of Y	0.306	0.301	0.184	0.878	0.881	0.478
Observations	14,027	15,211	7,167	14,027	15,211	7,167

Note: This table presents results of regressions where the dependent variables are changes in charter density (columns 1 to 3) and charter school slots (columns 4 to 6) between contemporaneous school year and a year before while treatment variables are lagged changes in math test scores (columns 1 and 4), reading test scores (columns 2 and 5), and absences (columns 3 and 6). Changes in outcome variables are measured between school year t and school year t-1 while lagged changes in treatment variables are measured between school year t-1 and school year t-2. Unit of observation is at school-by-year level. All regressions include school fixed effects and year fixed effects and are weighted by numbers of students in school-by-year cells. Additional controls in panel B include changes in fraction boys, changes in fraction Black students, changes in fraction of Hispanic students, change in student-teacher ratio, and changes in enrollment. These changes akin to outcomes are measures between school year t and school year t-1. Standard errors are clustered at school level. ***, ** and * mark estimates statistically different from zero at the 99, 95 and 90 percent confidence level.

Table 3: Sibling Fixed Effects: Robustness

	(1) Main results	(2) Main results (SE clustered at family)	(3) No control variables	(4) Sibling-by-school fixed effects	(5) First-two births only	(6) Grades 3 to 8
			Panel A. Mathematics score (x 100)			
Density at 5 miles	0.038	0.038	0.069	0.011	-0.094	N/A
	(0.169)	(0.131)	(0.180)	(0.151)	(0.172)	
Observations	808,419	808,419	808,419	808,419	741,496	
			Panel B. Reading score (x 100)			
Density at 5 miles	0.349**	0.349***	0.428***	0.460***	0.337**	N/A
	(0.147)	(0.127)	(0.159)	(0.131)	(0.168)	
Observations	912,514	912,514	912,514	912,514	831,168	
			Panel C. Absence rate (x 100)			
Density at 5 miles	-0.033***	-0.033***	-0.037***	-0.033***	-0.039***	-0.043***
	(0.010)	(0.007)	(0.010)	(0.010)	(0.010)	(0.011)
Observations	617,017	617,017	617,017	617,017	566,818	374,707

Note: This table presents robustness of our sibling fixed effects estimates presented in columns 2, 5, and 8 of panel A of Table 1. Panel A presents results for mathematics test scores, panel B presents results for reading test scores, and panel C presents results for absence rates. Column (1) replicates the main results for convenience. Column (2) presents the same results but with standard errors clustered at family rather than school level. Column (3) excludes all control variables except for sibling-by-school-by-grade and year fixed effects. Column (4) replicates results from column (1) but replaces sibling-by-school-by-grade fixed effects with sibling-by-school and grade fixed effects. Column (5) limits the sample to first two siblings born in the family. Column (6) replicates absence results for grades 3 to 8 for which we observe test scores. Standard errors are clustered at school level. ***, ** and * mark estimates statistically different from zero at the 99, 95 and 90 percent confidence level.

Table 4: Sibling Fixed Effects: Heterogeneity Demographics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
		Race, ethnicity, and immigrant backgroud				Socioeconomic status index	
	Baseline w/o individual level controls	White, non-Hispanic, non-immigrant	Black, non-Hispanic, non-immigrant	Hispanic, non-immigrant	Mother born outside the US	Below median	Above median
				Panel A. Mathematics score (x 100)			
Density at 5 miles	0.038	-0.552**	-0.258	-0.061	-0.050	0.049	-0.136
	(0.169)	(0.269)	(0.341)	(0.383)	(0.237)	(0.260)	(0.213)
Mean of Y	12.969	39.085	-49.473	6.926	19.901	-32.590	47.268
Observations	808,419	374,379	171,720	70,653	188,824	347,215	461,204
				Panel B. Reading score (x 100)			
Density at 5 miles	0.346**	-0.031	0.280	-0.432	-0.198	0.420*	0.320
	(0.146)	(0.267)	(0.254)	(0.329)	(0.221)	(0.224)	(0.196)
Mean of Y	8.630	35.679	-52.564	2.172	12.968	-37.587	43.409
Observations	912,514	420,279	193,578	80,765	214,624	391,829	520,685
				Panel C. Absence rate (x 100)			
Density at 5 miles	-0.033***	-0.056***	-0.001	-0.066***	-0.008	-0.023	-0.038***
	(0.010)	(0.016)	(0.021)	(0.024)	(0.012)	(0.017)	(0.011)
Mean of Y	4.476	4.538	5.422	4.657	3.372	5.487	3.637
Observations	617,017	279,260	138,413	53,095	144,379	279,664	337,353

Note: This table presents heterogeneity analysis by demographic (columns 2 to 5) and socioeconomic characteristics (columns 6 and 7). Panel A presents results for mathematics test scores, panel B presents results for reading test scores, and panel C presents results for absence rates. Column (1) replicates baseline results presented in columns 2, 5, and 8 of panel A of Table 1 but excluding individual level controls. Columns (2) to (5) present results for four mutually exclusive demographic categories: White, non-Hispanic, non-immigrant (column 2); Black, non-Hispanic, non-immigrant (column 3); Hispanic, non-immigrant (column 4); and immigrant children (column 5). We define children as having immigrant origin if their mother was born outside of the US. Subsequent two columns divide the main sample by population median of socioeconomic status index which is based on PCA analysis including the following variables: maternal years of education at birth, indicator if mother is married at the time of birth, indicator whether the birth was not paid by Medicaid, and maternal age at birth. Standard errors clustered at school level in all models. ***, ** and * mark estimates statistically different from zero at the 99, 95 and 90 percent confidence level.

Table 5: Sibling Fixed Effects: Heterogeneity School-Level Variables

	(1)	(2)	(3)	(4)	(5)	(6)
	Baseline w/o grade fixed effects	School grades		Baseline on school competition sample	Private school penetration	
		Elementary school	Middle school		Below median	Above median
			Panel A. Mathematics score (x 100)			
Density at 5 miles	0.141	0.099	-0.322	0.163	-0.307	0.035
	(0.148)	(0.181)	(0.259)	(0.168)	(0.370)	(0.192)
Mean of Y	12.969	12.444	13.822	12.137	20.841	2.725
Observations	808,419	500,494	307,925	717,152	370,348	343,813
			Panel B. Reading score (x 100)			
Density at 5 miles	0.654***	0.344**	0.433*	0.485***	-0.224	0.339*
	(0.142)	(0.147)	(0.249)	(0.152)	(0.324)	(0.176)
Mean of Y	8.630	8.570	8.703	7.437	15.936	-2.073
Observations	912,514	501,444	411,070	805,358	424,826	377,534
			Panel C. Absence rate (x 100)			
Density at 5 miles	-0.033***	-0.036***	0.007	-0.027***	-0.034*	-0.011
	(0.010)	(0.010)	(0.036)	(0.011)	(0.020)	(0.014)
Mean of Y	4.476	4.380	5.228	4.478	4.419	4.536
Observations	617,017	547,134	69,883	575,043	288,652	282,945

Note: This table presents heterogeneity analysis by school observables. Columns (2) and (3) present results by school level while columns (5) and (6) present results by student-weighted median of private school penetration within 5 miles of the traditional public school in question. Panel A presents results for mathematics test scores, panel B presents results for reading test scores, and panel C presents results for absence rates. Column (1) replicates baseline results presented in columns 2, 5, and 8 of panel A of Table 1 but replacing sibling-by-school-by-grade fixed effects with sibling-by-school fixed effects and omitting grade fixed effects. Column (2) presents results for elementary school grades (grades 3 to 5 for test scores and grades 1 to 5 for absences) while column (3) presents results for middle school grades (grades 6 to 8). Column (4) replicates baseline results presented in columns 2, 5, and 8 of panel A of Table 1 for the sample of schools for which we observe the private school competition measures. Private school competition is based on the competitive landscape in place in 2000 prior to the establishment of FTC school voucher (see Figlio and Hart (2014) and Figlio, Hart, Karbownik (2023)). We divide the competition by the median of student-weighted school-level competition measured at 5 miles within traditional public school. Standard errors are clustered at school level. ***, ** and * mark estimates statistically different from zero at the 99, 95 and 90 percent confidence level.

Table 6: Mechanisms

	(1)	(2)	(3)	(4)	(5)		
	School-level peer effects			School-level class size	School-level teacher experience		
	Math	Reading	Attendance				
Density at 5 miles	-0.004	0.110**	-0.000	-0.078***	0.152***		
	(0.054)	(0.055)	(0.002)	(0.014)	(0.019)		
Mean of Y	-2.382	-3.009	5.137	17.095	10.148		
Observations	14,208	15,636	9,163	11,547	11,666		
	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Teacher experience groups (%)				Teacher racial/ethnic composition		
	0 to 2 years	3 to 5 years	6 to 12 years	13+ years	% White	% Black	% Hispanic
Density at 5 miles	-0.617***	-0.534***	0.382***	0.710***	-0.269***	0.036	0.245***
	(0.107)	(0.077)	(0.105)	(0.091)	(0.072)	(0.076)	(0.056)
Mean of Y	22.800	18.924	26.033	31.911	65.818	18.655	13.084
Observations	11,666	11,666	11,666	11,666	11,666	11,666	11,666

Note: Columns (1) to (3) present the effects of charter school competition on school-level peer effects where the dependent variables are predicted rather than actual mathematics test scores (column 1), reading test scores (column 2), and absences (column 3). Column (4) presents the effects of charter school competition on school-level class size information. Columns (5) to (9) present the effects of charter competition on school-level measures of teacher experience. We present the effects on mean years of experience in column (5) and fractions of teachers in specific experience-range bins in columns (6) to (9). Columns (10) to (12) present the effects of charter school competition on school-level measures of teacher racial and ethnic composition. Each regression is based on cells aggregated to school-by-year level. The table displays the coefficient of interest, which is average school-by-year density of charter schools within 5 miles of each traditional public school. The regressions further include school and year fixed effects. Predicted test scores and absences are based on predicted values from a regression of actual test scores or absences on year- and month-of-birth dummies, gender, birth weight, maternal years of education dummies, gestational age dummies, marital status, mother's place of birth, race, ethnicity, maternal age at birth, prior number of births to mother, month when prenatal care began, complications of labor and delivery, abnormal conditions at birth, congenital anomalies, maternal health problems, and Medicaid-paid birth. R-squares from these regressions are 0.21, 0.21, and 0.06 for math, reading, and absences, respectively. These predicted values are then aggregated at school by year level. Data on class size for school years 2006–2007 to 2016–2017 are based on reports provided by FLDOE (`http://www.fldoe.org/finance/budget/class-size/class-size-reduction-averages.stml`) separately for grades pre-kindergarten (PK) to 3, 4 to 8, and 9 to 12. For each school and year, we weight these reported class sizes according to actual grades served e.g., if school is serving grades PK to 8, then we compute school-level class size as $CS = 0.5CS_{PK-3} + 0.5CS_{4-8} + 0CS_{9-12}$. Data on teacher experience and demographics are available for school years 2002–2003 to 2011–2012. Variables in columns 1 to 3 as well as 6 to 12 are multiplied by 100. Standard errors are clustered at school level. ***, ** and * mark estimates statistically different from zero at the 99, 95 and 90 percent confidence level.

Table 7: Competition Level and Endogenous Controls

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Baseline	Additional time-varying control variables						
Panel A. Mathematics x 100 (N = 808,419; # families = 106,418; mean of Y = 12.969; SD of Y = 99.898)								
Density at 5 miles (school)	0.038	0.156	0.149	0.150	0.149	0.180	0.154	0.162
	(0.169)	(0.191)	(0.191)	(0.190)	(0.190)	(0.190)	(0.191)	(0.187)
Density at 5 miles (district)		-0.332	-0.387	-0.466	-0.372	-0.363	-0.306	-0.564*
		(0.321)	(0.321)	(0.336)	(0.320)	(0.324)	(0.317)	(0.337)
Panel B. Reading x 100 (N = 912,514; # families = 108,677; mean of Y = 8.630; SD of Y = 100.561)								
Density at 5 miles (school)	0.348**	-0.018	-0.032	-0.022	-0.022	-0.007	-0.024	-0.035
	(0.147)	(0.172)	(0.171)	(0.171)	(0.171)	(0.170)	(0.172)	(0.169)
Density at 5 miles (district)		1.043***	0.965***	0.908***	1.022***	0.917***	1.073***	0.673**
		(0.292)	(0.292)	(0.311)	(0.292)	(0.292)	(0.291)	(0.311)
Panel C. Absences x 100 (N = 617,017; # families = 96,302; mean of Y = 4.476; SD of Y = 5.118)								
Density at 5 miles (school)	-0.033***	-0.012	-0.011	-0.008	-0.012	-0.013	-0.013	-0.009
	(0.010)	(0.012)	(0.011)	(0.012)	(0.012)	(0.012)	(0.012)	(0.011)
Density at 5 miles (district)		-0.071***	-0.056***	-0.040*	-0.069***	-0.093***	-0.069***	-0.055**
		(0.021)	(0.021)	(0.022)	(0.021)	(0.023)	(0.022)	(0.023)
District-level salaries			X					X
School-level private competition				X				X
School-level class sizes					X			X
Teacher characteristics						X		X
Peer-effects in all domains							X	X

Note: All regressions include sibling-by-school-by-grade and year fixed effects as well as control variables listed in analyses presented in columns 2, 5, and 8 of panel A of Table 1. These are replicated in Column (1) of the table for reference but we also add controls for missing values of other input variables considered in this table (see below). Panel A presents results for mathematics test scores, panel B presents results for reading test scores, and panel C presents results for absence rates. Column 2 presents estimates from a horse race between competition measured at school and at school district level. District-level competition is a simple district-by-year level mean of our 5-mile radius local competition measure. Columns 3 to 7 further add control variables that are time varying (at an annual level) at either the school or district level. These are assigned based on school- or school-district-by-year level. Column 3 controls for district-level average public school teachers salaries. Column 4 controls for density of private schools within 5 miles of the traditional public school. Column 5 controls for school-level average class size. Column 6 controls for school-level teacher characteristics, including the fraction of teachers with 0 to 2 years of experience, the fraction of teachers with 3 to 5 years of experience, the fraction of teachers with 6 to 12 years of experience, the fraction of teachers with 13 or more years of experience, the fraction of White teachers, the fraction of Black teachers, and the fraction of Hispanic teachers. Column 7 controls for school-level peer effects (based on predicted outcomes) in math test scores, reading test scores, and absences. Column 8 includes all controls from columns 3 to 7 jointly. Information on class size is available for the years 2006–2007 to 2016–2017, information for average salaries is available for the years 2004–2005 to 2016–2017, information on teacher characteristics is available for the years 2002–2003 to 2011–2012, and information on predicted peer effects is available for the years 2002–2003 to 2013–2014 for math, for the years 2002–2003 to 2016–2017 for reading, and for the years 2002–2003 to 2011–2012 for absences. To maintain constant sample size, we perform the following imputations for variables with missing values due to differential coverage of years: (i) if available, impute mean school level values, and (ii) if school-level information is not available, impute sample average. Standard errors are clustered at school level. ***, ** and * mark estimates statistically different from zero at the 99, 95 and 90 percent confidence level.

Online Appendix Tables

Table A.1: Descriptive Statistics

	(1) All births attending school in the 12 counties	(2) Empirical sample Individual FE	(3) Empirical sample Sibling FE	(4) Empirical sample IV	(5) Sibling FE sample Competition (density at 5 miles) Below median	(6) Sibling FE sample Competition (density at 5 miles) Above median
			Panel A. Sociodemographic characteristics			
White	42.3	40.4	42.4	39.5	55.3	23.0
African-American	21.6	23.3	26.4	22.5	23.2	31.2
Hispanic	8.2	7.9	8.9	8.4	7.1	11.7
Mother foreign born	27.5	28.1	21.9	29.2	14.0	33.9
Male	51.0	50.7	50.8	50.7	50.7	51.0
Mother HS dropout	21.9	22.7	22.0	21.6	20.4	24.5
Mother HS graduate	60.1	60.2	58.6	60.1	58.3	59.2
Mother college graduate	18.0	17.1	19.4	18.3	21.4	16.3
Mother age at birth	26.9	27.0	26.4	27.2	26.4	26.4
Parents married at birth	60.5	59.2	60.9	59.7	65.0	54.8
			Panel B. Competition measures			
Density	N/A	2.7	2.8	3.0	0.8	5.8
Number of slots		6.4	6.6	7.2	1.5	14.3
			Panel C. Outcomes			
Math score		0.7	2.9	3.5	8.8	-6.1
Reading score	N/A	0.4	0.5	3.1	7.8	-10.5
Absence rate		5.3	5.0	5.0	5.1	4.8
Maximum # observations	1,024,151	4,484,979	1,480,355	3,457,556	890,808	589,547

Note: Panel A presents means of sociodemographic variables (all indicator variables are multiplied by 100); panel B presents means of the two charter competition measures considered (more positive values indicate higher competition); panel C presents outcome variables (all multiplied by 100). There is one observation per individual in Column (1) and multiple observations per individual in Columns (2) to (6). Column (1) presents characteristics of full sample of births between 1992 and 2002 that occured in Florida and are linked to at least one school observation in the 12 anonymous school districts in Florida; Column (2) presents characteristics of our individual fixed effects estimation sample; Column (3) presents characteristics of our sibling fixed effects estimation sample (this is the preferred sample); Column (4) presents characteristics of our instrumental variables estimation sample. Columns (5) and (6) divide the preferred sibling fixed effects sample from Column (3) into two mutually exclusive categories based on student-weighted school-level median of the 5 mile charter school density competition measure: below median (Column 5) and above median (Column 6).

Table A.2: Individual Fixed Effects: Robustness

	(1)	(2)	(3)	(4)	(5)
	Main results	Main results (SE clustered at individual)	No control variables	Individual fixed effects	Grades 3 to 8
			Panel A. Mathematics score (x 100)		
Density at 5 miles	-0.221*	-0.221***	-0.218*	-0.226**	N/A
	(0.125)	(0.039)	(0.126)	(0.101)	
Observations	4,006,932	4,006,932	4,006,932	4,006,932	
			Panel B. Reading score (x 100)		
Density at 5 miles	0.409***	0.409***	0.406***	0.243***	N/A
	(0.095)	(0.039)	(0.096)	(0.075)	
Observations	4,289,265	4,289,265	4,289,265	4,289,265	
			Panel C. Absence rate (x 100)		
Density at 5 miles	-0.032***	-0.032***	-0.034***	-0.030***	-0.026**
	(0.009)	(0.003)	(0.009)	(0.010)	(0.012)
Observations	3,633,474	3,633,474	3,633,474	3,633,474	2,605,470

Note: This table presents robustness of our individual fixed effects estimates presented in columns 1, 4, and 7 of panel A of Table 1. Panel A presents results for mathematics test scores, panel B presents results for reading test scores, and panel C presents results for absence rates. Column (1) replicates the main results for convenience. Column (2) presents the same results but with standard errors clustered at individual level. Column (3) excludes all control variables except for individual-by-school, grade, and year fixed effects. Column (4) replicates results from column (1) but replaces individual-by-school fixed effects with individual and school fixed effects. Column (5) replicates absence results for grades 3 to 8 for which we observe test scores. Standard errors are clustered at school level except for column (2). ***, ** and * mark estimates statistically different from zero at the 99, 95 and 90 percent confidence level.

Table A.3: Instrumental Variables: Robustness

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Main results	Main results (SE clustered at zip code)	No control variables	G1-G8 information for instrument construction	G1 information for instrument construction	Grade-specific instrument	Alternative weighted instrument	Grades 3 to 8
				Panel A. Mathematics score (x 100)				
Density at 5 miles	0.251	0.251	0.181	0.112	-0.336	-0.293	0.197	N/A
	(0.251)	(0.285)	(0.273)	(0.258)	(0.357)	(0.194)	(0.345)	
Observations	3,051,507	3,051,507	3,051,507	3,074,199	2,775,100	3,074,118	3,051,587	
				Panel B. Reading score (x 100)				
Density at 5 miles	0.791***	0.791***	1.050***	0.717***	0.421	1.350***	0.912***	N/A
	(0.178)	(0.204)	(0.213)	(0.184)	(0.250)	(0.167)	(0.258)	
Observations	3,319,642	3,319,642	3,319,642	3,344,190	3,020,517	3,344,053	3,319,731	
				Panel C. Absence rate (x 100)				
Density at 5 miles	-0.263***	-0.263***	-0.216***	-0.266***	-0.269***	-0.094***	-0.253***	-0.168***
	(0.052)	(0.041)	(0.053)	(0.049)	(0.049)	(0.023)	(0.051)	(0.039)
Observations	2,792,787	2,792,787	2,792,787	2,856,604	2,698,739	2,856,560	2,838,247	1,859,995

Note: This table presents robustness of our instrumental variables estimates presented in columns 3, 6, and 9 of panel A of Table 1. Panel A presents results for mathematics test scores, panel B presents results for reading test scores, and panel C presents results for absence rates. Column (1) replicates the main results for convenience. Column (2) presents the same results but with standard errors clustered at zip code level (the level of geography at which we construct our instrument). Column (3) excludes all control variables except for zip code, school cohort, year, and grade fixed effects. Column (4) replicates results from column (1) but uses grades 1 to 8 information (rather than 3 to 8) for the construction of the instrument. Column (5) replicates results from column (1) but uses only grade 1 information (rather than 3 to 8) for the construction of the instrument. Column (6) replicates results from column (1) but constructs grade-specific (rather than across grades) instrument. Column (7) replicates results from column (1) but uses weighted instrument where competition measures' weights are constructed as a fraction of children from specific zip code and school cohort attending a particular school. Column (8) replicates absence results for grades 3 to 8 for which we observe test scores. Standard errors are clustered at school level except for column (2). ***, ** and * mark estimates statistically different from zero at the 99, 95 and 90 percent confidence level.

Table A.4: Differentiating Absences and Suspensions

	(1)	(2)	(3)	(4)	(5)	(6)
	Individual FE		Sibling FE		IV	
	Suspension rate (%)	Absence rate (%)	Suspension rate (%)	Absence rate (%)	Suspension rate (%)	Absence rate (%)
A. Density at 5 miles	0.002	-0.035***	0.009***	-0.042***	0.031*	-0.294***
	(0.003)	(0.008)	(0.003)	(0.010)	(0.015)	(0.045)
F/Anderson-Rubin F					189.6/9.3	189.6/43.6
A-R 95% conf. set					(-0.077, 0.072)	(-0.639, -0.120)
tF 95% conf. interval					(-0.077, 0.073)	(-0.621, -0.107)
B. Slots in 100 at 5 miles	-0.002	-0.018***	0.001	-0.021***	0.011*	-0.133***
	(0.001)	(0.003)	(0.001)	(0.004)	(0.005)	(0.018)
F/Anderson-Rubin F					158.8/9.3	158.8/44.8
A-R 95% conf. set					(-0.028, 0.025)	(-0.263, -0.064)
tF 95% conf. interval					(-0.027, 0.026)	(-0.253, -0.054)
Mean of Y	0.342	4.956	0.168	4.318	0.246	4.803
# students	867,624	867,624	207,108	207,108	661,090	661,090
Observations	3,633,474	3,633,474	617,017	617,017	2,792,787	2,792,787

Note: This table replicates results from Columns (7) to (9) of Table 1 but differentiates between the two components of our absenteeism measure: number of days suspended over the total number of days (suspension rate) and number of days absent (net of suspension days) over the total number of days (absence rate). Both outcomes are multiplied by 100. Results for the former outcome are displayed in odd-numbered columns while results for the latter outcome are displayed in even-numbered columns. Treatment variable of interest in panel A is charter school density measured within 5 miles of each traditional public school while in panel B it is number of charter slots in 100 measured within 5 miles of each traditional public school. Columns (1) and (2) present individual fixed effects results; Columns (3) and (4) present sibling fixed effects results (preferred specification); and Columns (5) and (6) present instrumental variables results. For the IV results we report both conventional and Anderson-Rubin Wald test F-statistics from first stage regressions along with the 95% Anderson and Rubin (1949) confidence sets and the 95% tF confidence intervals following Lee et al. (2022). Standard errors are clustered at school level. ***, ** and * mark estimates statistically different from zero at the 99, 95 and 90 percent confidence level.

Table A.5: Construction of Principal Components SES Index

	(1) First component	(2) Second component
Mother years of education	0.34	-0.16
Mother married at birth	0.65	-0.04
Mother's age at birth	0.12	0.98
Birth not paid by Medicaid	0.66	-0.06
Eigenvalue	1.58	0.99

Note: This table reports results of a principal components analysis of mother's education in years, non-Medicaid birth indicator, mother being married at the time of birth indicator, and maternal age at birth in years. The eigenvectors associated with the first and second components are reported, as well as their associated eigenvalues.

Table A.6: Instrumental Variables: Heterogeneity Demographics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
		Race, ethnicity, and immigrant backgroud				Socioeconomic status index	
	Baseline w/o individual level controls	White, non-Hispanic, non-immigrant	Black, non-Hispanic, non-immigrant	Hispanic, non-immigrant	Mother born outside the US	Below median	Above median
				Panel A. Mathematics score (x 100)			
Density at 5 miles	-0.098	-0.627	-1.179*	0.936*	0.776*	0.105	0.190
	(0.268)	(0.358)	(0.483)	(0.428)	(0.332)	(0.306)	(0.304)
Mean of Y	3.678	28.554	-48.628	0.017	10.242	-29.868	36.250
Observations	3,051,507	1,220,191	678,363	254,169	888,318	1,503,296	1,548,211
				Panel B. Reading score (x 100)			
Density at 5 miles	0.535**	0.376	-0.217	0.897*	0.957***	0.595**	0.897***
	(0.200)	(0.287)	(0.344)	(0.359)	(0.265)	(0.228)	(0.228)
Mean of Y	3.067	29.495	-47.704	0.035	6.592	-30.722	36.010
Observations	3,319,642	1,310,675	738,469	280,514	978,376	1,638,772	1,680,870
				Panel C. Absence rate (x 100)			
Density at 5 miles	-0.265***	-0.309***	-0.104	-0.590***	-0.479***	-0.189**	-0.335***
	(0.054)	(0.062)	(0.089)	(0.117)	(0.073)	(0.068)	(0.049)
Mean of Y	4.997	5.241	5.837	5.274	3.933	5.933	4.076
Observations	2,792,787	1,127,215	621,801	225,137	810,085	1,385,460	1,407,327

Note: This table presents heterogeneity analysis by demographic (columns 2 to 5) and socioeconomic characteristics (columns 6 and 7) similar to Table 4 but using instrumental variables rather than sibling fixed effects. Standard errors clustered at school level in all models. ***, ** and * mark estimates statistically different from zero at the 99, 95 and 90 percent confidence level.

Table A.7: Individual Fixed Effects: Heterogeneity Demographics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
		Race, ethnicity, and immigrant backgroud				Socioeconomic status index	
	Baseline w/o individual level controls	White, non-Hispanic, non-immigrant	Black, non-Hispanic, non-immigrant	Hispanic, non-immigrant	Mother born outside the US	Below median	Above median
				Panel A. Mathematics score (x 100)			
Density at 5 miles	-0.221*	-0.204	-0.392**	-0.207	-0.383**	-0.204	-0.273*
	(0.125)	(0.179)	(0.168)	(0.198)	(0.164)	(0.142)	(0.156)
Mean of Y	0.745	26.599	-52.093	-2.560	7.434	-30.244	36.129
Observations	4,006,932	1,633,071	926,838	312,147	1,122,417	1,618,045	1,593,357
				Panel B. Reading score (x 100)			
Density at 5 miles	0.409***	0.293**	-0.116	0.266	0.161	0.500***	0.372***
	(0.095)	(0.135)	(0.130)	(0.178)	(0.109)	(0.122)	(0.114)
Mean of Y	0.373	27.621	-50.306	-2.814	3.606	-30.950	36.005
Observations	4,289,265	1,728,774	991,637	339,619	1,215,581	1,763,876	1,729,658
				Panel C. Absence rate (x 100)			
Density at 5 miles	-0.033***	-0.053***	-0.010	-0.033**	-0.004	-0.022**	-0.032***
	(0.009)	(0.014)	(0.017)	(0.014)	(0.008)	(0.011)	(0.008)
Mean of Y	5.256	5.437	6.223	5.522	4.115	6.029	4.124
Observations	3,633,474	1,487,517	841,290	277,820	1,016,492	1,509,371	1,469,303

Note: This table presents heterogeneity analysis by demographic (columns 2 to 5) and socioeconomic characteristics (columns 6 and 7) similar to Table 4 but using individual fixed effects rather than sibling fixed effects. Standard errors clustered at school level in all models. ***, ** and * mark estimates statistically different from zero at the 99, 95 and 90 percent confidence level.

Table A.8: Instrumental Variables: Heterogeneity School-Level Variables

	(1)	(2)	(3)	(4)	(5)	(6)
	Baseline w/o grade fixed effects	School grades		Baseline on school competition sample	Private school penetration	
		Elementary school	Middle school		Below median	Above median
				Panel A. Mathematics score (x 100)		
Density at 5 miles	0.088	0.808**	-2.049***	0.354	-0.214	0.422
	(0.271)	(0.259)	(0.481)	(0.249)	(0.489)	(0.315)
Mean of Y	3.678	2.950	4.704	2.868	11.709	-5.794
Observations	3,051,507	1,784,918	1,266,589	2,635,428	1,299,295	1,326,307
				Panel B. Reading score (x 100)		
Density at 5 miles	0.566**	0.615***	0.983**	0.801***	1.008**	0.842***
	(0.199)	(0.181)	(0.352)	(0.177)	(0.347)	(0.230)
Mean of Y	3.067	2.675	3.525	1.783	10.457	-6.846
Observations	3,319,642	1,787,974	1,531,668	2,857,751	1,425,662	1,422,250
				Panel C. Absence rate (x 100)		
Density at 5 miles	-0.249***	-0.185***	0.085	-0.286***	-0.139*	-0.317***
	(0.055)	(0.023)	(0.088)	(0.054)	(0.068)	(0.073)
Mean of Y	4.997	4.835	5.620	4.998	4.944	5.052
Observations	2,792,787	2,215,507	577,279	2,540,372	1,264,159	1,263,705

Note: This table presents heterogeneity analysis by school observables similar to Table 5 but using instrumental variables rather than sibling fixed effects. Column (1) replicates the results from Table 1 but excludes individual level controls. Column (4) replicates the results from Table 1 but limits the sample to observations for which predetermined private school competition measures are available. Columns (2) and (3) present results by school level while columns (5) and (6) present results by student-weighted median of private school penetration within 5 miles of the traditional public school in question. Standard errors are clustered at school level. ***, ** and * mark estimates statistically different from zero at the 99, 95 and 90 percent confidence level.

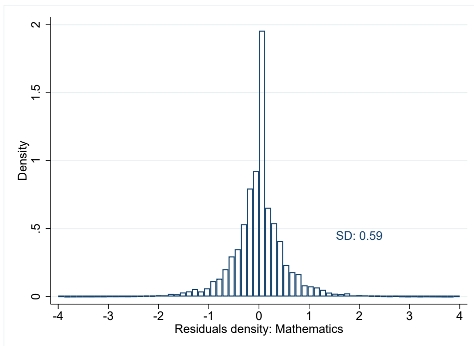
Table A.9: Individual Fixed Effects: Heterogeneity School-Level Variables

	(1)	(2)	(3)	(4)	(5)	(6)
	Baseline w/o grade fixed effects	School grades		Baseline on school competition sample	School level	
		Elementary school	Middle school		Below median	Above median
			Panel A. Mathematics score (x 100)			
Density at 5 miles	0.211	0.162	0.052	-0.184	-0.335	-0.025
	(0.129)	(0.160)	(0.198)	(0.127)	(0.205)	(0.144)
Mean of Y	0.745	0.496	1.080	0.001	8.843	-8.685
Observations	4,006,932	2,300,902	1,706,030	3,460,434	1,708,648	1,738,636
			Panel B. Reading score (x 100)			
Density at 5 miles	0.812***	0.471***	0.646***	0.392***	0.232	0.282***
	(0.113)	(0.113)	(0.165)	(0.096)	(0.159)	(0.106)
Mean of Y	0.373	0.196	0.579	-0.929	7.897	-9.717
Observations	4,289,265	2,304,056	1,985,209	3,693,514	1,843,012	1,837,344
			Panel C. Absence rate (x 100)			
Density at 5 miles	-0.038***	-0.030***	-0.032	-0.028***	-0.020	-0.015
	(0.010)	(0.008)	(0.031)	(0.009)	(0.016)	(0.011)
Mean of Y	5.256	4.919	6.167	5.249	5.207	5.292
Observations	3,633,474	2,652,648	980,826	3,287,926	1,634,357	1,638,299

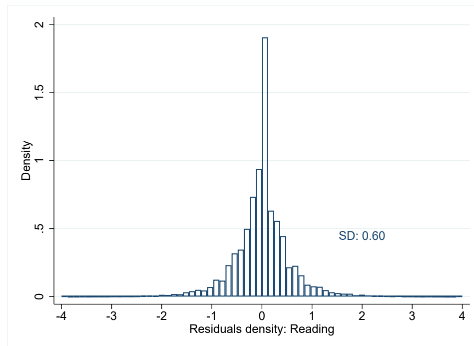
Note: This table presents heterogeneity analysis by school observables similar to Table 5 but using individual fixed effects rather than sibling fixed effects. Column (1) replicates the results from Table 1 but excludes individual level controls. Column (4) replicates the results from Table 1 but limits the sample to observations for which predetermined private school competition measures are available. Columns (2) and (3) present results by school level while columns (5) and (6) present results by student-weighted median of private school penetration within 5 miles of the traditional public school in question. Standard errors are clustered at school level. ***, ** and * mark estimates statistically different from zero at the 99, 95 and 90 percent confidence level.

Online Appendix Figures

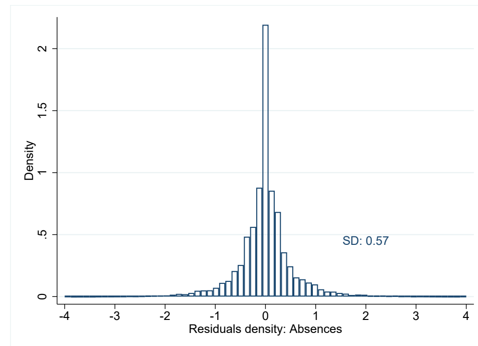
Figure A.1: Residual Variation in Charter Schools Penetration in Florida: Individual Fixed Effects



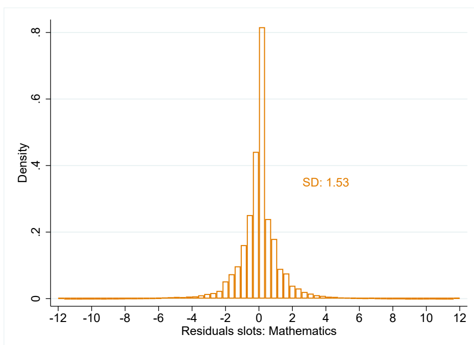
(a) Mathematics sample



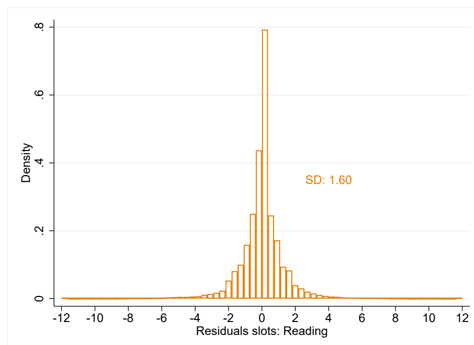
(b) Reading sample



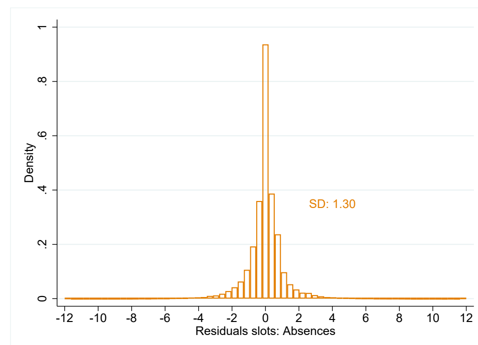
(c) Absences sample



(d) Mathematics sample



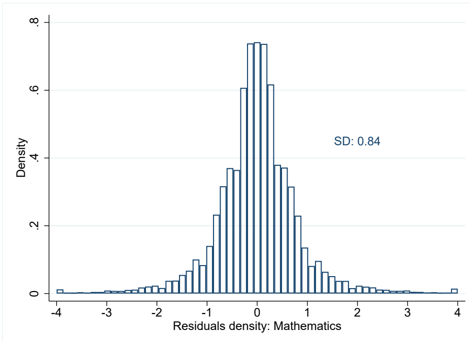
(e) Reading sample



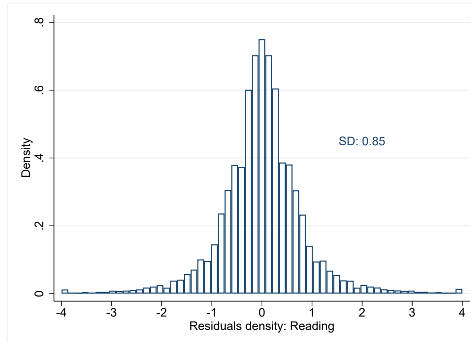
(f) Absences sample

Notes: These figures present residuals from regressing our treatment variables - density of charter schools within 5 miles of traditional public school (top row) and number of charter schools slots (in 100) available within 5 miles of traditional public school (bottom row) - on fixed effects used in individual fixed effects specification in Columns (1), (4), and (7) of Table 1. The fixed effects include student-by-school, grade and year effects. Panels (a) and (d) present residuals for mathematics samples; panels (b) and (e) present residuals for reading sample; and panels (c) and (f) present residuals for absences sample.

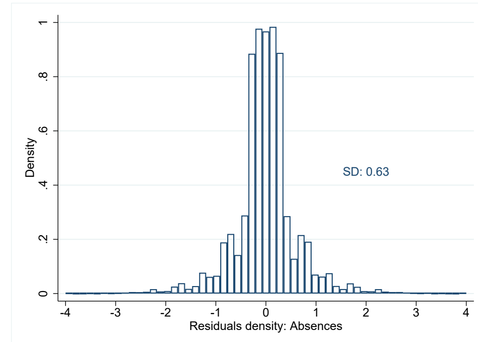
Figure A.2: Residual Variation in Charter Schools Penetration in Florida: Sibling Fixed Effects



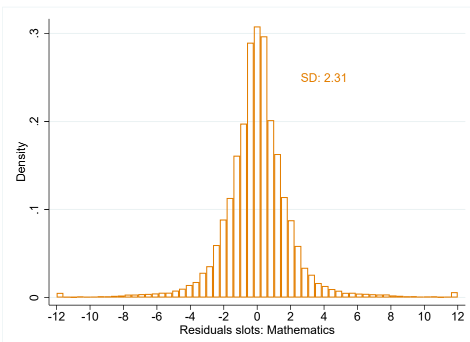
(a) Mathematics sample



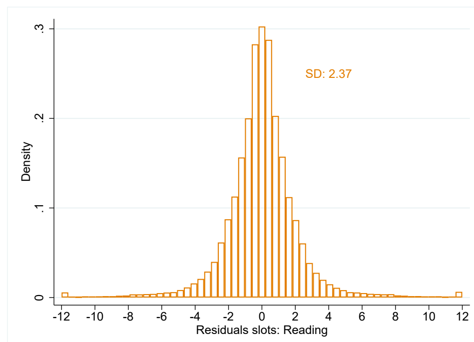
(b) Reading sample



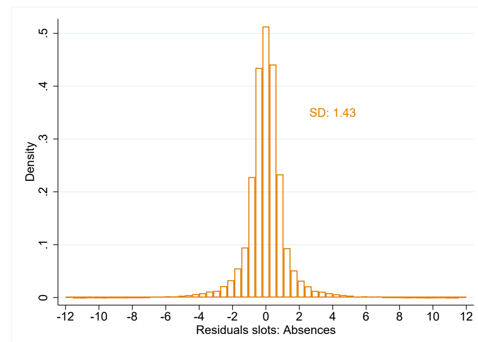
(c) Absences sample



(d) Mathematics sample



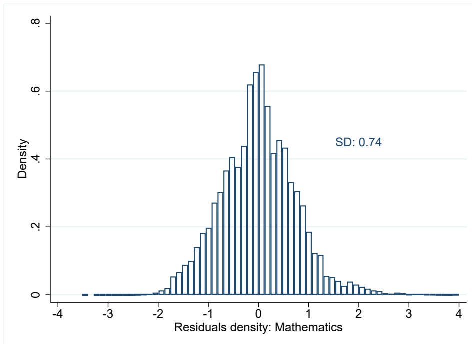
(e) Reading sample



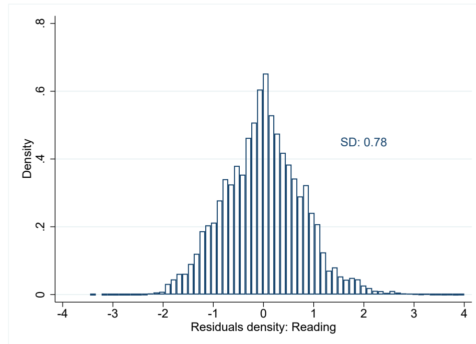
(f) Absences sample

Notes: These figures present residuals from regressing our treatment variables - density of charter schools within 5 miles of traditional public school (top row) and number of charter schools slots (in 100) available within 5 miles of traditional public school (bottom row) - on fixed effects used in sibling fixed effects specification in Columns (2), (5), and (8) of Table 1. The fixed effects include family-by-school-by-grade and year effects. Panels (a) and (d) present residuals for mathematics samples; panels (b) and (e) present residuals for reading sample; and panels (c) and (f) present residuals for absences sample.

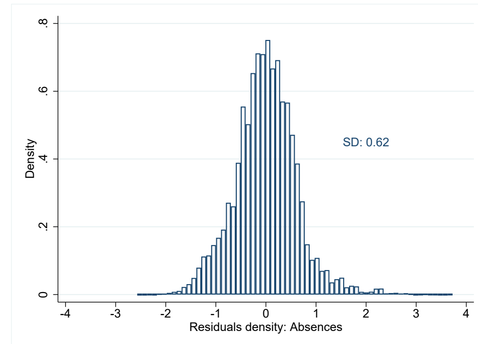
Figure A.3: Residual Variation in Charter Schools Penetration in Florida: Instrumental Variables



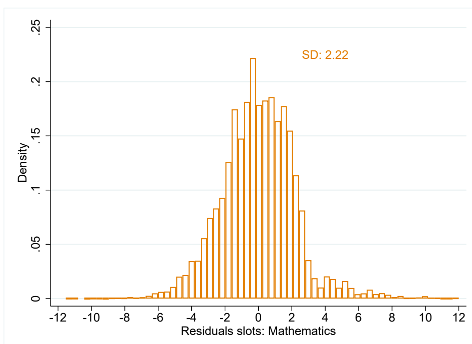
(a) Mathematics sample



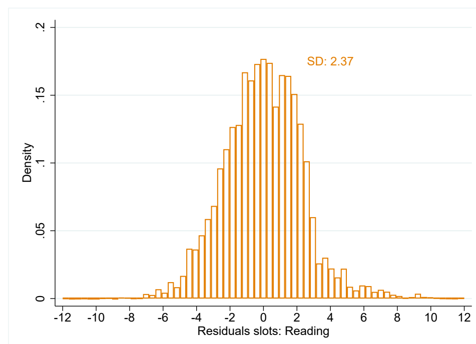
(b) Reading sample



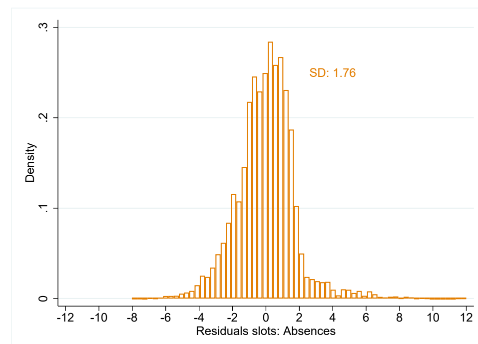
(c) Absences sample



(d) Mathematics sample



(e) Reading sample



(f) Absences sample

Notes: These figures present residuals from regressing our instruments - student-level predicted density of charter schools within 5 miles of traditional public school (top row) and student-level predicted number of charter schools slots (in 100) available within 5 miles of traditional public school (bottom row) - on fixed effects used in instrumental variables specification in Columns (3), (6), and (9) of Table 1. The fixed effects include zip code of birth, school-cohort of birth, year, and grade effects. Panels (a) and (d) present residuals for mathematics samples; panels (b) and (e) present residuals for reading sample; and panels (c) and (f) present residuals for absences sample.