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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 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. We further discover that competitive forces induced by the presence of charter and private schools appear to be complementary, at least when it comes to behavioral outcomes.

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Introduction

The use of charter schools to expand public school choice has grown rapidly over the last thirty years. Forty-four states, plus Washington, D.C., Guam, and Puerto Rico, currently allow the creation of charter schools, and over 3.2 million students nationwide attend charter schools (National Association of Public Charter Schools, 2019).

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. To date a handful of studies have explored the competitive effects of charter schools; however, most of those have contended with empirical limitations including short longitudinal timeframes, a focus on single districts, or lack of plausible sources of exogenous variation. 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 school sector. 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). On the other hand, choice skeptics have worried that charter schools may depress performance of students who remain behind in the traditional public school (TPS) sector. For instance, losing students to charter schools may pose costs to schools as they lose per-pupil funding (e.g., Mann & Bruno, 2020); 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; Zimmer & Buddin, 2009; Bettinger, 2005; Imberman, 2011; Winters, 2012; Cordes, 2018; Ridley and Terrier, 2018; Mann & Bruno, 2020; Gilraine et al., 2021). These inconsistencies likely spring from multiple sources, including different, and often far from perfect, empirical strategies and 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., Zimmer and Buddin, 2009; Winters, 2012; Cordes, 2018), while studies that have used statewide data generally look at the very early years of charter policies and over short periods of time (e.g., Bettinger, 2005; Bifluco and Ladd, 2006; Sass, 2006). Other studies that take a national perspective are limited to district-level data (Han & Keefe, 2020). 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 and interact with different school choice options like voucher schools or other traditional public 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, statewide set of data 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; (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; and (d) address a novel question in the literature around whether competition from charter schools and private schools are substitutes or complements for each other.

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 nonsectarian, 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, similar to 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 1999-2017 in terms of both the numbers of K-8 charter schools and the share of public school K-8 students enrolled in charters. By 2017, the

[1] State universities can also serve as charter authorizers 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)

charter sector in Florida comprised over 450 schools and served over 10% of K-8 students in the state. This growth has not been even across the state. Figure 2 provides snapshots of the share of K-8 enrollment in the charter sector statewide as of fall 2000, 2005, 2010, and 2015. Note that in 2000-01, over half of Florida school districts (37) had no charter enrollment, and only one district had a K-8 charter enrollment share exceeding 5%. In 2015-16, there were still a significant minority of districts with no charter enrollment (26); however, there were also 20 districts with more than 10% of enrollment in charter schools. While these high-charter-penetration districts included some large districts like Dade and Broward (with 18% and 20% of K-8 enrollment in charters in 2015, respectively), it also included small districts like Franklin and Sumter, which each had over one-third of K-8 public school students enrolling in charters.

Methods

A. Data and Sample

This project draws on a unique and rich set of data constructed by merging student data from the Florida Department of Education with birth records from the Florida Department of Health. The former data includes information on all students, including basic demographics, test scores, absences, suspension, and exceptionality data for students in Grades PK-12. This educational data is merged to the birth records 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 detailed heterogeneity analysis by measures like parental education that have not been available in most prior research.

We place two limitations on our sample. 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 are ultimately observe in our 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). 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 350,000 and 1.4 million 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 order for a sibling set to enter the sample. In this sample the approximately 354,000 siblings come from 165,000 families. 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:

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

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).[2] We include vectors of controls for time-varying student

[2] 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.

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 term ε_{igst} is an independent, identically distributed error term 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 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 will also include extensive 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). Standard errors are clustered based on the actual school attended in a given year, though we examine robustness to different choices of clustering.

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; $AvgChComp_{zc}$). 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 attending it in year *t,* given that the student was born in zip code *z* and cohort *c*:

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

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

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 $\widehat{CharterComp}_{izcst}$ 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:

$$Y_{izcgst} = \beta \widehat{CharterComp}_{izcst} + \delta StudChar_{izcgst} + \gamma SchoolChar_{st} + \pi_g + \omega_t + \varphi_z$$

$$+ \mu_c + \varepsilon_{izcgst} \qquad (4)$$

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 to 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. Standard errors in our instrumental variable models are clustered based on the actual school attended in a given year, though we examine robustness to different choices of clustering.

C. Measures

Student Outcomes. We explore the effects of charter competition on several types of student outcomes. Our main measures are grade-by-year standardized math and reading test scores. Note that we have math scores for a more limited set of years than we have for reading scores; our final year of math scores is spring 2014.

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 through the 2009-10 school-year. In other specifications, we look separately at absence rates (excluding suspension days) and suspension indicators (i.e., probability that a student is suspended in a given year), and these data are available to us through 2011-12 school-year. For our models, these measures reflect student-grade-year-specific outcomes that are related to charter competition that students' schools experience in the same year.

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.[3] 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 and private schools, for each traditional public school: **density** and **slots**. The "density" measure captures the number of charter (private) 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 served by charter (private) schools in the same grade range within a given radius of each traditional public school. 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 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.[4]

[3] 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.
[4] See Appendix A for additional graphs (Figure A1, A2, and A3) showing the remaining variation in density measure when we regress our competition measures onto relevant sets of fixed effects. They 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. Results for slots measure are similar.

Control Variables. We create measures for several student or 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 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 race/ethnic groups, and the share of students using subsidized lunch. We also include data from the National Center of Education Statistics on student-teacher ratios.

Results

A. Main Results

Our main results (Table 1) present estimates from our sibling (Columns 1, 4, and 7), instrumental variables (Columns 2, 5, and 8), and student fixed effect 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 absence measure. Panel A provides results using the density competition measure while Panel B provides results using the slots competition measure. For our IV results, *F*-statistics indicating very strong first stages (all *F* values are well over 100) are reported beneath the IV estimates.

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. 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.

Third, results are consistent in pattern across estimation strategies, although there is a fair amount of variation between coefficients. For instance, our density estimates using sibling fixed effects suggest that an increase of 10 charter schools within a 5-mile radius would be associated with a 0.036 standard deviation increase in reading scores; our instrumental variables estimates suggest that the same increase would be associated with an increase of 0.098 standard deviation in reading scores. The estimates are not within each other's 95% confidence intervals. The individual fixed effects approach produces results that are in-between the siblings fixed effects and the IV.

It is not clear ex ante if an expansion of 10 charter schools is even remotely possible in the context of Florida, and in fact, as we have documented in 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 0.5 to 0.8 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 point estimates would imply that for mathematics we can rule out negative effect sizes larger than 0.3 percent of a SD and positive effect sizes larger than 0.5 percent of a SD. At the same time, for reading, we find effect sizes in the range of 0.4 to 1 percent of a SD and we also identify reductions in absenteeism of between 0.8 to 2.2 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. Having in mind that a SD of their competition measure is about 4 times as large as ours, the standardized effect would imply increase in test scores of 2.3 percent of a SD in their case and between 1 to 3 percent of a SD in our case. We view these estimates as very comparable but note that Figlio and Hart (2014) also find gains in mathematics while we estimate a relatively precise zero effect of charter school competition on math test scores.

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. At least their reading estimate, however, appears comparable to our findings.

Almost no prior work has explored effects on behavioral outcomes but our effect sizes of (or 1 to 2 percent of a sample mean) may appear relatively small at first glance. To put it in context, however, it is worth highlighting that gap in absences between children with high school dropout moms and college graduates moms in Florida is only 3.3 percentage points while the gap between children of White and Black mothers is only 0.3 percentage points. Compared to these longer-standing gaps our point estimates appear quantitatively meaningful.

Although the three methods discussed above 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 to 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 TPSes 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 between contemporaneous and prior school year in our two competition measures (density and slots in 100), and we regress these competition measures 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.* For the most part, we do not find any statistically significant relationships between these variables, and they are not consistently signed across treatment and outcomes. At the same time, the marginally significant coefficients suggest, if anything, that increases in test scores and declines in absences (i.e., improving environment in public schools) lead to declines rather than increases in competitive pressures. This suggests 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 each of these estimation strategies remain robust to different modeling assumptions. Table 3 presents robustness tests for the IV estimates. Our main results are replicated in Panel A for ease of comparison. There we also provide an alternative way for clustering our standard errors – at zip code level – which may be more appropriate given that our instrument aggregates expected competition at zip code by school cohort level. This change does not alter the statistical significance of our results. Panel B excludes control variables; if the IV carves out exogenous variation in the charter competition measure, the inclusion of our controls should not meaningfully change the magnitude of the estimates and should primarily affect our estimates by improving our standard errors. Indeed, the estimates without controls are very similar in magnitude to, but somewhat less precise than, our main estimates.

Panel C broadens the competition measure by using information from grades 1-8 (rather than 3-8 as for the main instrument), while Panel D narrows it by using competition information only from grade 1 (under the concern that subsequent years may represent endogenous transfer patterns). Neither change substantially affects the results. Panel E allows the instrument to predict not just grade 3-8 competition cumulatively, but the grade-specific competition that each student would face in a given year. The pattern of results remains the same, though the magnitude of the reading coefficient is somewhat larger under this construction. Panel F then uses an alternative approach to constructing the instrument where we weight zip code-by-cohort competition measures with actual flows of students to schools. In that, for each zip code and cohort combination we compute a probability that a student ends up in a specific school, and then we weight this school's actual competition with the zip code-by-cohort flows. The results remain unchanged. In a final exercise, we limit the sample to students who remain in traditional public schools for at least six years – an amount of time it would take to advance from grade 3 to grade 8 under normal progression. Since we measure absenteeism starting in first grade, we consider progression starting in grade one for this outcome. Our reading and absences results remain largely unchanged; however, for the sample of children who remain for a least six years in traditional public schools, we find large and negative effects on test scores in mathematics. Overall, aside from the last mathematics result, which is likely driven by composition of students remaining in public schools, our instrumental variables results are very robust to plausible alternative specifications.

Table 4 presents robustness tests for the sibling fixed effects estimates and an alternative identification strategy that relies on within family variation in exposure to competition. As in Table 3, Panel A replicates our main results, but here we provide an alternative clustering at the family level. Panel B shows that results are qualitatively similar if we exclude control variables. 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). Panel C addresses this issue by using sibling-school rather than sibling-school-grade fixed effects. Results are similar in pattern, though the coefficient on reading in particular grows under this specification, suggesting our main results are conservative. Panel D of Table 4 shows results for the sample of children whom we observe for six consecutive years in traditional public schools. Here, similar to the IV results presented in Table 3, we find comparable reading and absences results; however, estimates for mathematics test scores turn negative and statistically significant. The next set of results, in panel E, likewise leverage within-family variation in exposure but depart from using sibling fixed effects. Here, we instrument competitive pressures faced by younger siblings' schools with their closest older siblings school choices. We consistently find positive and statistically significant gains in reading and reductions in absenteeism; however, akin to panel results discussed above the mathematics point estimates are negative. The final panel uses a "movers" analysis akin to Autor et al. (2016) or Chetty and Hendren (2018); however, instead of school or neighborhood quality, we measure effects of school competition faced by students' schools. Here the assumption is that although families' school choices can be endogenous, the endogenous factors should affect both siblings in the same way, while age difference between siblings generates variation in the number of years children are exposed to different levels of competition. This analysis confirms negative effects on absenteeism and positive effects on reading test scores, at least when it comes to our slots measure. For our density measure, the pattern of reading and absence results are similar to our main estimates, but the larger standard errors render the reading estimates non-significant at conventional levels. The point estimates for mathematics test scores are negative, but are not statistically significant at conventional levels.

Finally, Table 5 presents the robustness of our individual fixed effects models. First, in Panel A we replicate our main results but also provide an alternative clustering approach at an individual level, since we observe each student multiple times. The standard errors, if anything, decrease under this alternative approach. Panel B drops the only time-varying individual control (contemporaneous free or reduced-price lunch status), as well as all time varying school-level controls. The results remain unchanged. In Panel C we include individual and school fixed effects separately—thus not constraining the individual effects to be constant within schools—and this likewise yields comparable results. Finally, in panel D we restrict the sample to children for whom we have six consecutive observations in traditional public schools. The reading and absences results remain unchanged; however, unlike in Tables 3 and 4 we do not find statistically significant negative results for mathematics.

Our preferred combined measure of absenteeism includes both days absent as well as days suspended, but it is limited to observations until school year 2009-10. While they are not entirely comparable to the measures available through 2009-2010, we do have additional information about suspensions and absences available for two more school years until 2011-12. Thus, in Table A1 we further present effects of charter school competition on probability of being suspended and absence rate. These results suggest that total absenteeism is primarily driven by truancy rather than suspensions. Nonetheless, our absence rate results are robust to including these two additional school years.

In all specifications to date we used 5 miles radius to define charter schools competition; however, this threshold, while popular in the extant literature, is arbitrary. Thus, in Figures A4 to A6, we present robustness of our results to measuring 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. Irrespective of the exact empirical approach or competition measure, we consistently find positive effects on reading test scores and negative effects on absences. These are larger, but also less precisely estimated, at smaller radii. Even at a 15-mile radius, however, the effects remain statistically significant. Such an empirical pattern suggests that competitive pressures are stronger when they are more localized to a school. At the same time, akin to the main specification and irrespective of the identification strategy, we do not find consistently signed or statistically significant effects on mathematics test scores. 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

Table 6 provides heterogeneity analyses based on school characteristics. Panels A1 and A2 show results from elementary and middle school grades, respectively. The main divergence comes in math; while competition has a null to positive relationship with student math scores in elementary school grades, results are mixed in middle school based on model specification. While the sibling fixed effects and IV models suggest a negative relationship between competition and middle school math scores, the individual FE models show null to positive results depending on measures of competition used. Reading and absence results appear comparable across elementary and middle school grades.

In panels B1 and B2 we further 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. Here we divide the sample by median private school density measured at a 5-mile radius. To provide a context for this measure, traditional public schools facing below median private school competition have on average 6 private schools within a 5 mile radius. Those TPSes with above-median competition have on average 41 private schools within a 5 mile radius.

For mathematics, as with the main results, we do not find any conclusive results and the point estimates appear to be sensitive to the exact empirical strategy used. For reading, all estimates are positive and most are statistically significant. While we see some differences in effect sizes between TPS that face more vs. less private school competition, the direction of these effects is not consistent between methodological approaches. By contrast, we find striking results for absences that are consistent across methods. Specifically, our negative effects are present only in traditional public schools that also face above-median private school penetration. This would suggest that for behavioral outcomes private and public school competition is complementary. On the other hand, in locations with little additional private school competition, additional charter schools actually increase (rather than decrease) absences.

Table 7 shows heterogeneity based on student characteristics. As our sample is cut into smaller slices, our results become somewhat less stable across estimation strategies. 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 fairly consistently null for Black students, competition is associated with reductions in absences for Hispanic students and students with immigrant parents. The relationship between competition and reading scores likewise generally range between null to positive for the different student subgroups. For Black students, they are general null and modest in magnitude, while Hispanic students, White students, and students with immigrant parents fare more consistently positively in reading (albeit with generally imprecise null results for the sibling fixed effects models). Math results are generally null to negative for different student subgroups; the one exception is that IV results are positive for children of immigrant mothers.

Finally, Figures 3, 4, and 5 show results by family socioeconomic status for our sibling FE, IV, and individual FE analyses respectively. We break our sample into terciles of socioeconomic status based on an index SES variable that we generate using principal components analysis. The underlying variables in the PCA include mother's years of education, marital status at birth, age at birth, the indicator for Medicaid-paid birth, and median zip code income in residence at time of birth. The patterns are fairly consistent for all SES subgroups: greater charter competition is consistently associated with lower absence rates and higher reading scores (though these effects are sometimes non-significant). For absences, the results are especially consistent, suggesting that the lowest-SES students see the smallest reductions in absences as competition grows under all three analytic approaches.[5] By contrast, the SES gradients are less consistent for reading results across analytic approaches. Math results are more mixed: While the lowest two terciles consistently have null results across all analytic approaches, the relationship between charter competition and math scores is significant and negative for our individual FE estimates, while the relationship is significant and positive for our IV estimates. Overall, the broad pattern of results suggests that there are not consistently stronger relationships between competition and math scores based on SES, while the lowest-SES students see the smallest reductions in absences associated with charter competition.

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

[5] For siblings whose families are represented in multiple SES terciles due to different maternal characteristics at different births, we place them in the highest tercile they are observed in. Results are broadly similar if we place families in the lowest tercile they are observed in, or if we drop families with varying SES terciles across children from the sample.

they indeed cream-skim best students and drain district 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 schools and to stimulate competition that can incentivize all schools to improve the quality of education offered.

Despite this theoretical ambiguity and policy relevance, the competitive effects of charter schools remains a relatively understudied topic, and the results in the extant literature are not consistent across papers, and many papers are plagued with empirical challenges. Here, we investigate this question using data from the state of Florida and multiple complementary identification strategies, including individual fixed effects, sibling fixed effects, and instrumental variables based on place of birth. Across these strategies, we find 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, we find precise zero effects for mathematics test scores. The results are robust to plausible alternative empirical specifications. Our preliminary heterogeneity analysis likewise reveals some interesting patterns. First, it appears that Black students gain the least from charter competition. Second, we find that for behavioral outcomes, private and public competitive pressures appear to be complementary as reductions in absenteeism from additional charter school competition are present solely in locations where there is also above-median private school penetration.

As we expand our work over the next couple of months, we intend to further incorporate preliminary analyses that we have conducted on long-run (middle-school) effects of cumulative charter exposure from students' elementary years; expand the heterogeneity analysis; and address potential mechanisms including peer effects, school resources, and student mobility.

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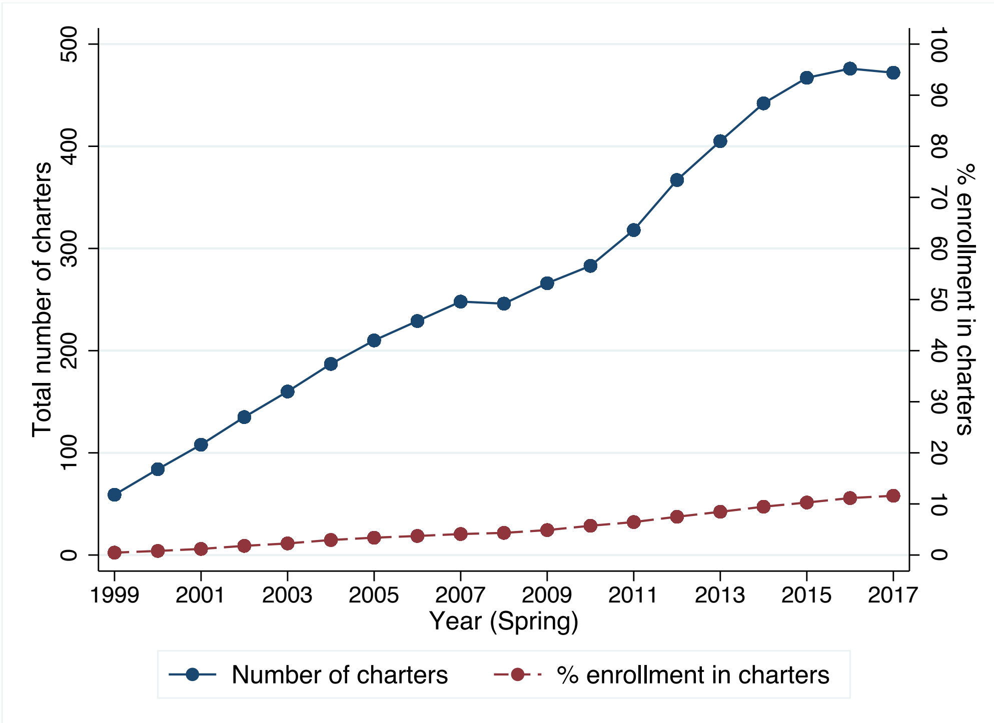
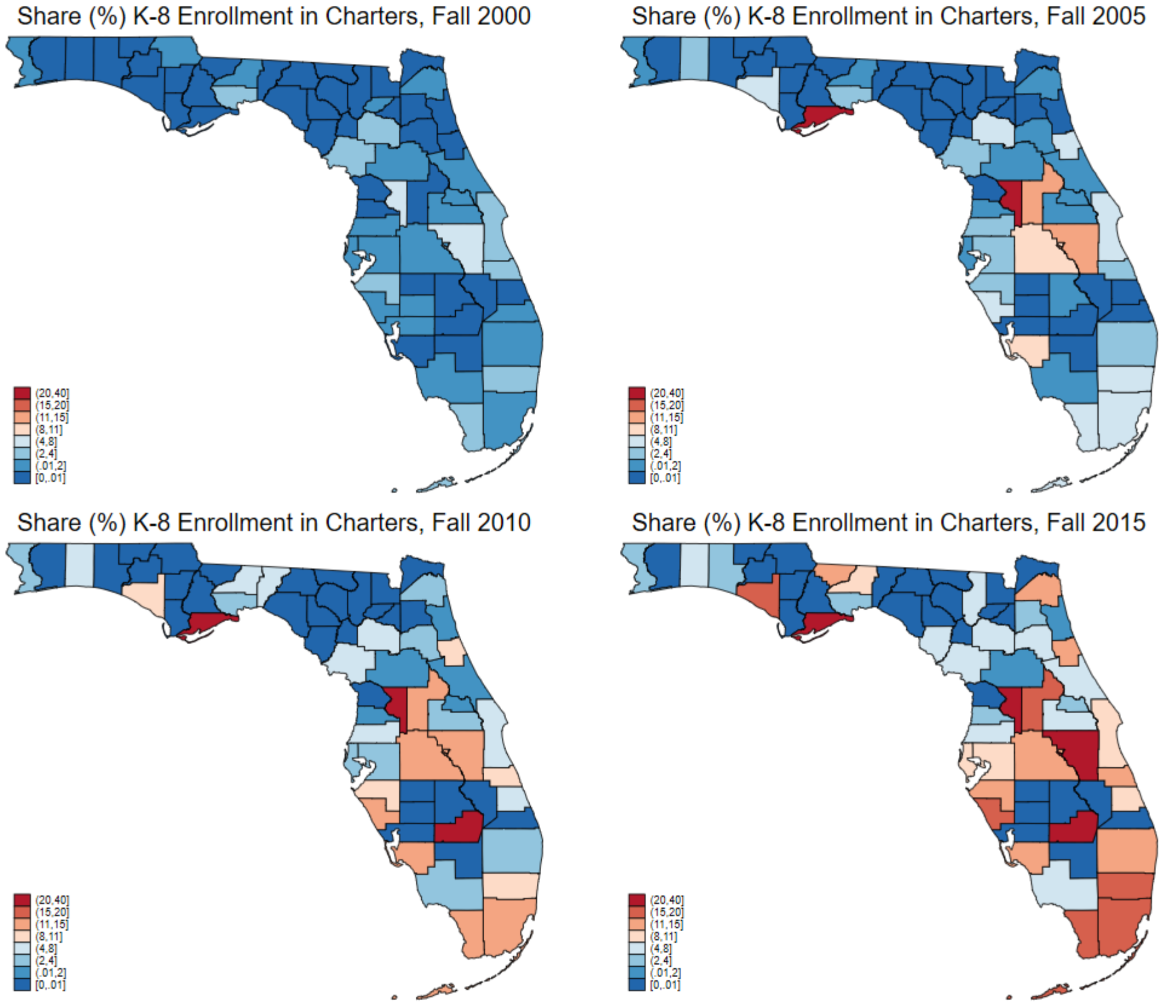
Figures

Figure 1: Growth of charter schools (counts) and enrollment over time



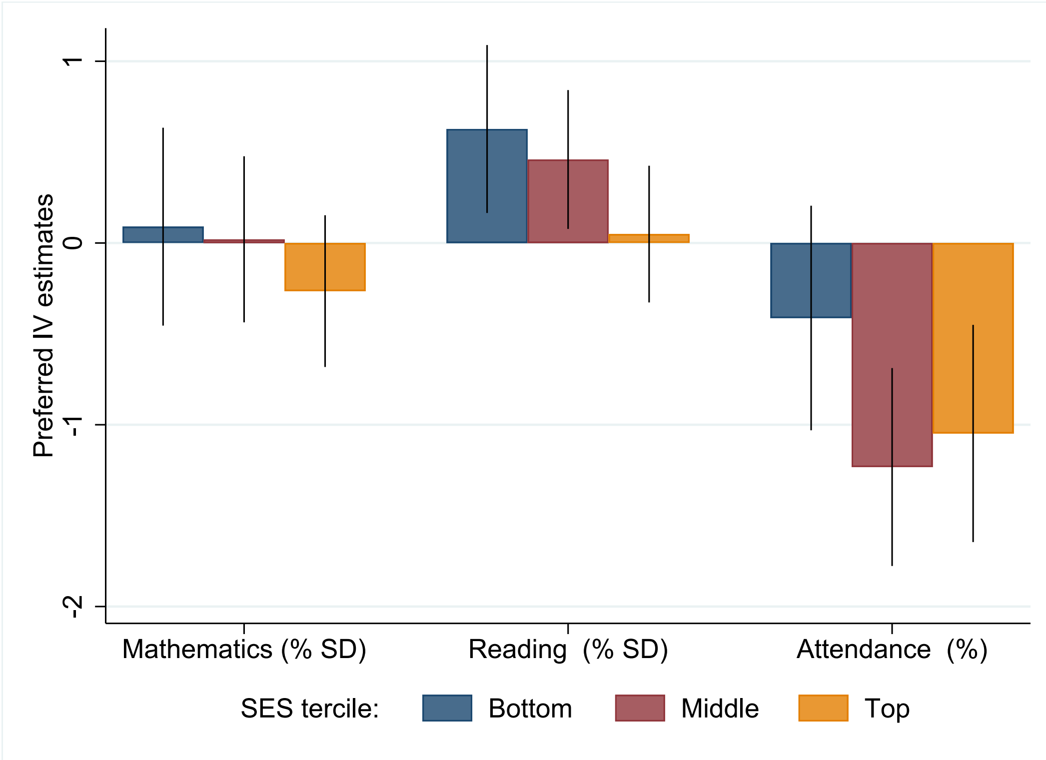
Note: This figure depicts total number of K-8 charter schools in a given year operating in Florida (blue solid line) and fraction of K-8 students enrolled in a charter school (red dashed line).

Figure 2: Spatial and time variation in charter schools penetration in Florida



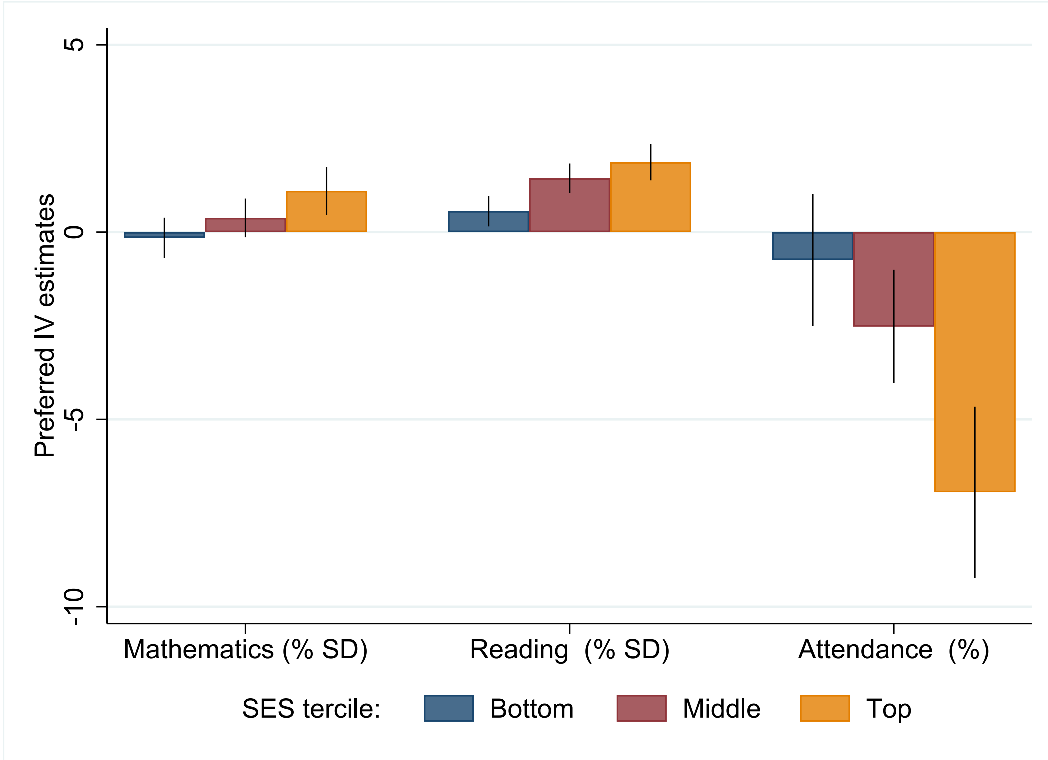
Note: These figures present shares of K-8 students, as a fraction of all K-8 students in a district and year, enrolled in charter schools in a given year (2000, 2005, 2010, and 2015) and school district.

Figure 3: Heterogeneity by SES - sibling FE



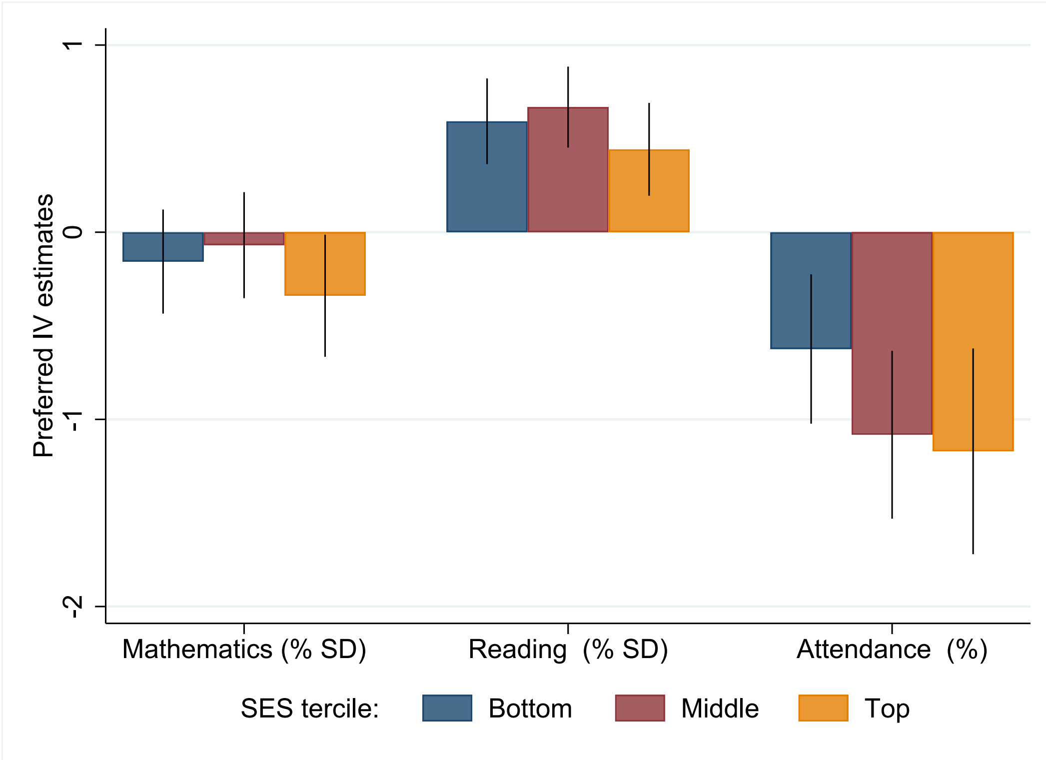
Note: This figure presents heterogeneity analysis by terciles of SES status. SES status is based on PCA analysis using the following variables: mom years of education, marital status, age at birth, indicator for Medicaid paid birth, and median zip code income of place of residence at the time of birth. Treatment variable is charter school density within 5 miles of traditional public school that a child attends. Regressions are based on those from panel A in Table 2 in columns 1, 4, and 7 but where we socioeconomic status variables. Because terciles of SES are measured at individual level for about 25% of the sample we observe variation in SES terciles across siblings we do three adjustments to account for this. We take the highest SES tercile observed when dividing the sample. Standard errors in all models are clustered at school level and used to compute 95% confidence intervals depicted with spikes.

Figure 4: Heterogeneity by SES - instrumental variables



Note: This figure presents heterogeneity analysis by terciles of SES status. SES status is based on PCA analysis using the following variables: mom years of education, marital status, age at birth, indicator for Medicaid paid birth, and median zip code income of place of residence at the time of birth. Treatment variable is charter school density within 5 miles of traditional public school that a child attends. Regressions are based on those from panel A in Table 2 in columns 2, 5, and 8 but where we exclude socioeconomic status control variables. Standard errors in all models are clustered at school level and used to compute 95% confidence intervals depicted with spikes.

Figure 5: Heterogeneity by SES - individual FE



Note: This figure presents heterogeneity analysis by terciles of SES status. SES status is based on PCA analysis using the following variables: mom years of education, marital status, age at birth, indicator for Medicaid paid birth, and median zip code income of place of residence at the time of birth. Treatment variable is charter school density within 5 miles of traditional public school that a child attends. Regressions are based on those from panel A in Table 2 in columns 3, 6, and 9 but where we exclude free and reduced price lunch control variable. Standard errors in all models are clustered at school level and used to compute 95% confidence intervals depicted with spikes.

Tables

Table 1: Main results - 5 miles radius

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
		Mathematics			Reading			Absences	
	Sibling FE	IV	Individual FE	Sibling FE	IV	Individual FE	Sibling FE	IV	Individual FE
A. Density	-0.025	0.090	-0.100	0.358***	0.976***	0.562***	-0.036***	-0.110**	-0.043***
	(0.156)	(0.220)	(0.117)	(0.134)	(0.159)	(0.091)	(0.009)	(0.038)	(0.010)
F-statistic		939.7			842.5			227.1	
B. Slots in 100	0.050	0.086	-0.001	0.229***	0.318***	0.316***	-0.014***	-0.061***	-0.018***
	(0.063)	(0.074)	(0.051)	(0.057)	(0.054)	(0.035)	(0.003)	(0.014)	(0.004)
F-statistic		664.8			510.8			178.0	
Mean of Y	12.501	3.534	0.629	7.754	2.901	0.219	4.446	5.082	5.334
# students	344,427	1,120,196	1,417,795	353,961	1,125,443	1,423,309	309,127	1,074,953	1,416,641
Observations	1,235,741	5,312,929	6,838,823	1,406,051	5,808,820	7,354,599	947,690	4,978,518	6,367,514

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 sibling fixed effects analysis; columns 2, 5, and 8 present instrumental variables analysis; and columns 3, 6, and 9 present individual fixed effects 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. 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, 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, we 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. F-statistics from first stage regressions are displayed below the standard errors in each IV column. Density measure first stages are 0.960 (0.031), 0.981 (0.034), 0.406 (0.027) for mathematics, reading, and absences, respectively. Slots measure first stages are 0.973 (0.038), 0.995 (0.044), 0.406 (0.031) for mathematics, reading, and absences, respectively. 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. Individual fixed effects models include individual-by-school 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. Standard errors in all models are clustered at school level.

Table 2: 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	0.021	-0.037	0.009*	-0.132	-0.144*	0.005
minus treatment in t-2	(0.039)	(0.038)	(0.006)	(0.083)	(0.077)	(0.012)
	Panel B. Including control variables					
Treatment in year t-1	0.023	-0.035	0.009	-0.132	-0.143*	0.004
minus treatment in t-2	(0.039)	(0.038)	(0.005)	(0.084)	(0.077)	(0.012)
Mean of Y	0.197	0.196	0.111	0.593	0.596	0.341
Observations	25,166	27,343	12,832	25,166	27,343	12,832

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 African-American 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.

Table 3: Robustness: IV

	(1)	(2)	(3)	(4)	(5)	(6)
		Density			Slots in 100	
	Math	Reading	Absences	Math	Reading	Absences
			Panel A. Main results			
Treatment	0.090	0.976***	-0.110**	0.086	0.318***	-0.061***
	(0.220)	(0.159)	(0.038)	(0.074)	(0.054)	(0.014)
[cluster zip]	[0.250]	[0.201]	[0.030]	[0.070]	[0.059]	[0.011]
Observations	5,312,929	5,808,820	4,978,518	5,312,929	5,808,820	4,978,518
			Panel B. No control variables			
Treatment	0.0854	1.204***	-0.0986*	0.0957	0.400***	-0.0530***
	(0.244)	(0.188)	(0.0410)	(0.0830)	(0.0647)	(0.0148)
Observations	5,312,929	5,808,820	4,978,518	5,312,929	5,808,820	4,978,518
			Panel C. Grades 1 to 8 information used for instrument construction			
Treatment	0.077	0.972***	-0.116**	0.110	0.349***	-0.064***
	(0.224)	(0.162)	(0.036)	(0.074)	(0.055)	(0.013)
Observations	5,331,945	5,830,110	5,057,443	5,331,945	5,830,110	5,057,443
			Panel D. Only grade 1 information used for instrument construction			
Treatment	0.066	0.986***	-0.167***	0.291**	0.555***	-0.073***
	(0.309)	(0.220)	(0.039)	(0.099)	(0.075)	(0.014)
Observations	4,910,730	5,371,564	4,746,910	4,910,730	5,371,564	4,746,910
			Panel E. Grade-specific instrument			
Treatment	-0.118	1.479***	-0.109***	0.016	0.428***	-0.051***
	(0.169)	(0.151)	(0.020)	(0.059)	(0.050)	(0.007)
Observations	5,331,841	5,829,970	5,057,365	5,331,841	5,829,970	5,057,365
			Panel F. Alternative instrument: zip code by cohort measure weighted with school flows			
Treatment	-0.222	0.946***	-0.095*	-0.000	0.280***	-0.049***
	(0.272)	(0.206)	(0.038)	(0.083)	(0.063)	(0.012)
Observations	5,313,116	5,809,030	5,040,229	5,313,116	5,809,030	5,040,229
			Panel G. 6-years panel for each outcome			
Treatment	-6.442***	1.108***	-0.127*	-2.758***	0.374***	-0.078***
	(0.413)	(0.172)	(0.053)	(0.208)	(0.061)	(0.022)
Observations	3,065,316	4,426,062	1,686,078	3,065,316	4,426,062	1,686,078

Note: This table presents robustness of our instrumental variables estimates presented in columns 2, 5, and 8 of Table 1. Columns 1 to 3 present results for charter density measure while columns 4 to 6 present results for charter slots in 100 measure. Panel A replicates main results from Table 1 for convenience. Standard errors in squared brackets in this panel are based on clustering at zip code rather than school level as in the main specification. Panel B presents results without auxiliary control variables i.e., including the fixed effects only. Panel C uses grades 1 to 8 to construct the instrument (rather than 3 to 8 as in the main specification). Panel D uses only grade 1 competition to construct the instrument. Panel E uses grade specific instruments e.g., for grade 5 outcome we use zip code-by-cohort leave one out aggregation based on competition faced by students in grade 5. Panel F uses alternative instrument where we weight our zip code-by-cohort measure with actual flows of students from zip code-by-cohort to all possible schools in Florida and we use these flows to weight the competition. Panel G limits the sample to students for whom we observe six consecutive observations during which normal progression would have taken them from grades 3 to 8 when it comes to test scores and grades 1 to 6 when it comes to absences.

Table 4: Robustness: Sibling analysis

	(1)	(2)	(3)	(4)	(5)	(6)
		Density			Slots in 100	
	Math	Reading	Attendnace	Math	Reading	Attendnace
			Panel A. Main results			
Treatment	-0.025	0.358***	-0.036***	0.050	0.229***	-0.014***
	(0.156)	(0.134)	(0.009)	(0.063)	(0.057)	(0.003)
[cluster family]	[0.120]	[0.116]	[0.007]	[0.043]	[0.042]	[0.002]
Observations	1,235,741	1,406,051	947,690	1,235,741	1,406,051	947,690
			Panel B. No control variables			
Treatment	-0.008	0.451***	-0.040***	0.094	0.298***	-0.017***
	(0.166)	(0.143)	(0.009)	(0.068)	(0.062)	(0.003)
Observations	1,235,741	1,406,051	947,690	1,235,741	1,406,051	947,690
			Panel C. Sibling by school fixed effects			
Treatment	-0.096	0.517***	-0.039***	0.042	0.296***	-0.017***
	(0.122)	(0.107)	(0.009)	(0.047)	(0.041)	(0.003)
Observations	1,872,933	2,042,467	1,798,364	1,872,933	2,042,467	1,798,364
			Panel D. 6-years panel for each outcome			
Treatment	-0.703***	0.451***	-0.033***	-0.132**	0.266***	-0.017***
	(0.154)	(0.114)	(0.012)	(0.060)	(0.046)	(0.005)
Observations	1,119,900	1,572,691	572,726	1,119,900	1,572,691	572,726
			Panel E. Instrumenting younger sibling competition with expected competition based on older sibling school trajectory			
Treatment	-0.518***	0.317**	-0.077***	-0.174**	0.206***	-0.022***
	(0.195)	(0.157)	(0.018)	(0.082)	(0.058)	(0.006)
Observations	836,085	961,281	657,260	836,085	961.281	657,260
			Panel F. Sibling movers			
Treatment	-0.396	0.287	-0.025*	-0.098	0.222**	-0.014**
	(0.290)	(0.254)	(0.015)	(0.110)	(0.093)	(0.006)
Observations	525,912	596,157	420,072	525,912	596,157	420,072

Note: This table presents robustness of our sibling fixed effects estimates presented in columns 1, 4, and 7 of Table 1. Columns 1 to 3 present results for charter density measure while columns 4 to 6 present results for charter slots in 100 measure. Panel A replicates the main results for convenience. Standard errors in squared brackets in this panel reflect clustering at family rather than school level. Panel B presents results where we drop all auxiliary control variables and only include the fixed effects. Panel C includes sibling-by-school as well as year and grade fixed effects rather than sibling-by-school-by-grade as well as year fixed effects. Panel D limits the sample to students for whom we observe six consecutive observations during which normal progression would have taken them from grades 3 to 8 when it comes to test scores and grades 1 to 6 when it comes to absences. Panel E presents IV estimation where we instrument competition facd by a younger sibling with expected competition based on their older sibling school trajectory. In that we limit the sample to second to fourth born siblings, and for each younger sibling we match their closest older sibling school trajectory at grade level. We choose school first observed in any given grade. The instrument is constructed as contemporaneous competition a younger sibling would have faced if they attended schools which their older sibling attended. These regressions additionally include individual fixed effects, school fixed effects, grade fixed effects, year fixed effects as well as school-level demographic controls. Standard errors are clustered at school level. First-stage F-statistics are 1600, 1954, 1110, 582, 658, and 231 for columns 1 to 6, respectively. Panel F utilizes sibling fixed effects strategy but limits the sample to mover families. Treatment variable is running average of competition experienced up to a given grade akin how school quality is defined in Autor et al. (2016). Move is defined at either changing county or changing school to one which is more than 5 miles away. Regressions include family-by-school-by-grade fixed effects, year fixed effects as well as individual- and school-level controls. Standard errors are clustered at school level.

Table 5: Robustness: Individual FE

	(1)	(2)	(3)	(4)	(5)	(6)
		Density			Slots in 100	
	Math	Reading	Attendnace	Math	Reading	Attendnace
			Panel A. Main results			
Treatment	-0.100	0.562***	-0.043***	-0.001	0.316***	-0.018***
	(0.117)	(0.091)	(0.010)	(0.051)	(0.035)	(0.004)
[cluster student]	[0.035]	[0.034]	[0.003]	[0.013]	[0.012]	[0.001]
Observations	6,838,823	7,354,599	6,367,514	6,838,823	7,354,599	6,367,514
			Panel B. No control variables			
Treatment	-0.096	0.563***	-0.045***	-0.002	0.314***	-0.019***
	(0.118)	(0.091)	(0.010)	(0.052)	(0.035)	(0.004)
Observations	6,838,823	7,354,599	6,367,514	6,838,823	7,354,599	6,367,514
			Panel C. Individual fixed effects			
Treatment	-0.140	0.353***	-0.044***	0.033	0.233***	-0.016***
	(0.094)	(0.070)	(0.011)	(0.039)	(0.026)	(0.004)
Observations	6,838,823	7,354,599	6,367,514	6,838,823	7,354,599	6,367,514
			Panel D. 6-years panel for each outcome			
Treatment	-0.170	0.542***	-0.036***	0.025	0.313***	-0.020***
	(0.135)	(0.096)	(0.011)	(0.061)	(0.036)	(0.005)
Observations	4,166,148	5,575,512	1,743,984	4,166,148	5,575,512	1,743,984

Note: This table presents robustness of our individual fixed effects estimates presented in columns 3, 6, and 9 of Table 1. Columns 1 to 3 present results for charter density measure while columns 4 to 6 present results for charter slots in 100 measure. Panel A replicates the main results for convenience. Standard errors in squared brackets in this panel reflect clustering at individual rather than school level. Panel B presents results where we drop all auxiliary control variables and only include the fixed effects. Panel C includes individual, school, grade and year fixed effects rather than individual-by-school as well as grade and year fixed effects. Panel D limits the sample to students for whom we observe six consecutive observations during which normal progression would have taken them from grades 3 to 8 when it comes to test scores and grades 1 to 6 when it comes to absences.

Table 6: Heterogeneity - school characteristics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
		Mathematics			Reading			Absences	
	Sibling FE	IV	Individual FE	Sibling FE	IV	Individual FE	Sibling FE	IV	Individual FE
					Panel A1. Elementary school grades				
Density	0.099	0.660**	0.049	0.346**	0.931***	0.269***	-0.032***	-0.140***	-0.028***
	(0.188)	(0.229)	(0.140)	(0.152)	(0.163)	(0.097)	(0.009)	(0.020)	(0.008)
Slots in 100	0.076	0.283***	-0.012	0.217***	0.286***	0.208***	-0.013***	-0.084***	-0.012***
	(0.076)	(0.080)	(0.054)	(0.061)	(0.057)	(0.041)	(0.003)	(0.010)	(0.003)
Mean of Y	11.768	2.792	0.372	7.228	2.540	0.046	4.335	4.869	4.944
Observations	749,318	3,076,437	3,888,779	750,804	3,082,632	3,895,276	837,177	3,946,537	4,640,667
					Panel A2. Middle school grades				
Density	-0.473*	-1.654***	0.072	0.253	1.103***	0.636***	-0.083*	-0.132	-0.068**
	(0.246)	(0.389)	(0.182)	(0.233)	(0.292)	(0.152)	(0.042)	(0.075)	(0.029)
Slots in 100	-0.174*	-0.557***	0.268***	0.130	0.381***	0.357***	-0.027**	-0.062	-0.030***
	(0.096)	(0.133)	(0.084)	(0.085)	(0.094)	(0.064)	(0.012)	(0.032)	(0.010)
Mean of Y	13.630	4.557	0.967	8.358	3.309	0.414	5.292	5.898	6.384
Observations	486,423	2,236,492	2,950,044	655,247	2,726,188	3,459,323	110,513	1,031,981	1,726,847
					Panel B1. High private school penetration				
Density	-0.246	-0.003	-0.545***	0.141	0.564**	0.193**	-0.037***	-0.210***	-0.049***
	(0.166)	(0.259)	(0.128)	(0.146)	(0.188)	(0.097)	(0.011)	(0.056)	(0.011)
Slots in 100	-0.082	0.120	-0.188***	0.163**	0.192**	0.193***	-0.017***	-0.098***	-0.024***
	(0.067)	(0.090)	(0.061)	(0.064)	(0.065)	(0.038)	(0.004)	(0.021)	(0.005)
Mean of Y	8.086	0.129	-2.807	2.695	-1.293	-4.060	4.481	5.032	5.333
Observations	613,251	2,586,038	3,079,841	696,311	2,832,364	3,335,162	439,647	2,270,086	2,828,381
					Panel B2. Low private school penetration				
Density	0.253	-2.246*	0.838***	0.045	1.739*	0.327	0.066**	0.659**	0.129***
	(0.489)	(1.140)	(0.306)	(0.440)	(0.812)	(0.216)	(0.032)	(0.239)	(0.047)
Slots in 100	0.213*	-0.941**	0.228**	0.100	0.366	0.228***	0.011	0.108*	0.009
	(0.117)	(0.287)	(0.095)	(0.114)	(0.190)	(0.067)	(0.008)	(0.051)	(0.007)
Mean of Y	15.493	6.823	4.231	11.191	6.806	4.483	4.483	5.159	5.454
Observations	519,691	2,573,152	3,084,232	597,458	2,820,274	3,342,623	338,302	2,199,376	2,770,313

Note: This table presents heterogeneity analysis by school level. Panel A presents results for elementary school grades, 3 to 5 for mathematics and reading and 1 to 5 for absences. Panel B presents results for middle school grades, 6 to 8 for all outcomes. Econometric specifications mimic those from Table 2. Standard errors clustered at school level in all models.

Table 7: Heterogeneity - demographic characteristics

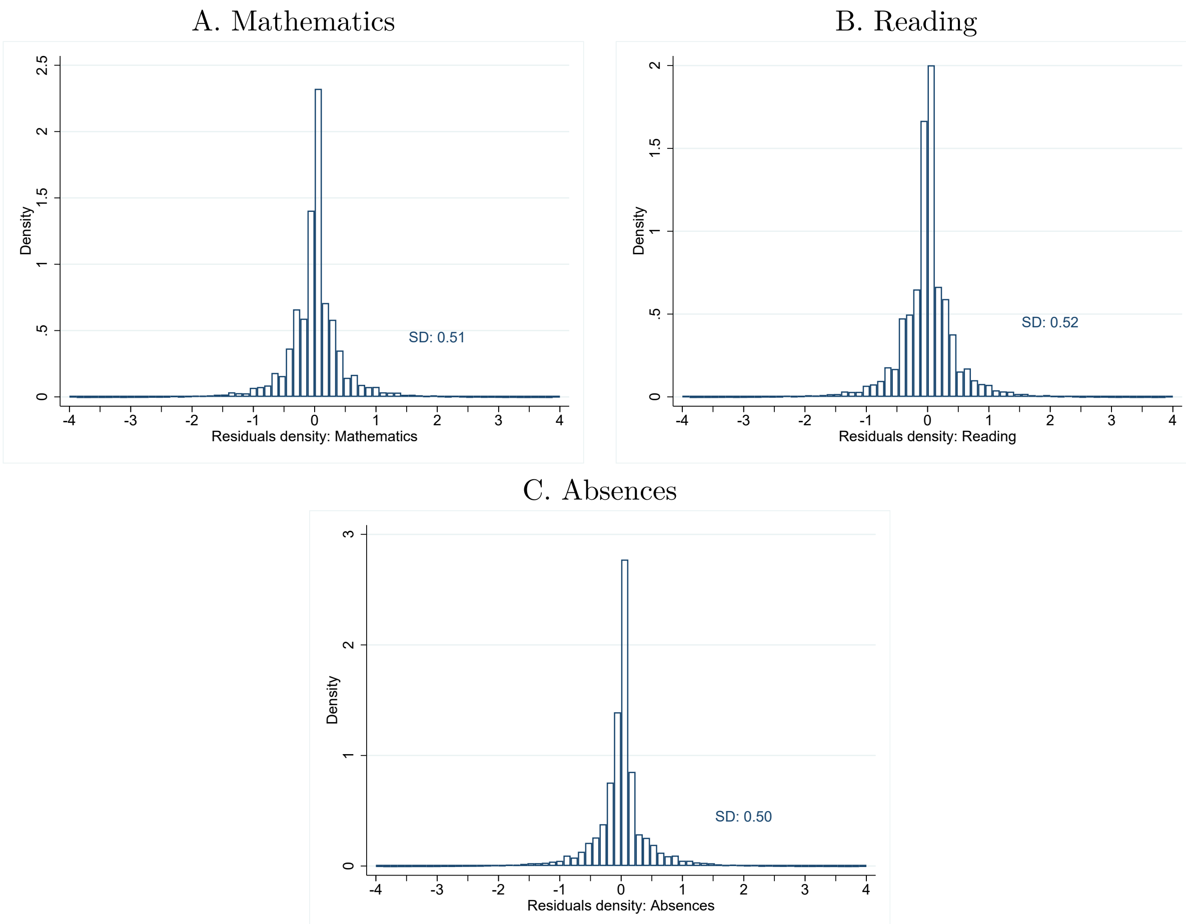
	(1)	(2) Mathematics	(3)	(4)	(5) Reading	(6)	(7)	(8) Attendance	(9)
	Sibling FE	IV	Individual FE	Sibling FE	IV	Individual FE	Sibling FE	IV	Individual FE
Panel A. White, non-Hispanic, non-immigrant									
Density	-0.575**	-0.518	0.016	-0.211	0.954***	0.287**	-0.043***	-0.095	-0.024
	(0.229)	(0.300)	(0.140)	(0.231)	(0.241)	(0.114)	(0.014)	(0.049)	(0.017)
Slots in 100	-0.114	-0.295**	0.100*	0.036	0.286***	0.194***	-0.019***	-0.078***	-0.012**
	(0.079)	(0.105)	(0.052)	(0.087)	(0.083)	(0.045)	(0.005)	(0.017)	(0.005)
Mean of Y	35.847	23.834	21.822	31.624	24.485	22.544	4.468	5.282	5.493
Observations	639,436	2,615,864	3,400,075	724,432	2,834,574	3,626,572	484,285	2,486,950	3,200,079
Panel B. Black, non-Hispanic, non-immigrant									
Density	-0.403	-0.960*	-0.315*	0.294	0.117	-0.094	-0.013	-0.043	-0.023
	(0.315)	(0.413)	(0.164)	(0.232)	(0.282)	(0.122)	(0.019)	(0.070)	(0.017)
Slots in 100	-0.006	-0.222	-0.136*	0.189*	-0.013	0.026	-0.001	-0.067*	-0.002
	(0.149)	(0.140)	(0.077)	(0.109)	(0.090)	(0.052)	(0.008)	(0.027)	(0.008)
Mean of Y	-50.693	-50.145	-53.900	-53.460	-48.896	-51.796	5.300	5.705	6.088
Observations	239,196	1,023,817	1,378,754	272,046	1,119,737	1,481,067	191,910	958,212	1,280,777
Panel C. Hispanic, non-immigrant									
Difference	-0.165	0.525	-0.408**	-0.229	1.133***	0.315**	-0.051**	-0.303***	-0.043***
	(0.347)	(0.366)	(0.183)	(0.303)	(0.293)	(0.161)	(0.023)	(0.077)	(0.017)
Slots in 100	-0.030	0.160	-0.128*	0.040	0.336***	0.143***	-0.013	-0.090***	-0.017***
	(0.118)	(0.119)	(0.073)	(0.109)	(0.097)	(0.052)	(0.008)	(0.022)	(0.006)
Mean of Y	0.463	-6.409	-8.667	-4.627	-6.516	-9.031	4.746	5.456	5.678
Observations	105,914	418,925	504,219	122,353	465,026	551,875	79,464	378,438	458,434
Panel D. Immigrant									
Density	-0.135	0.615*	-0.483***	-0.098	1.246***	0.239**	-0.020*	-0.266***	-0.012
	(0.229)	(0.284)	(0.162)	(0.215)	(0.221)	(0.102)	(0.011)	(0.051)	(0.008)
Slots in 100	-0.079	0.349***	-0.206***	0.017	0.445***	0.146***	-0.009**	-0.091***	-0.010***
	(0.099)	(0.098)	(0.060)	(0.086)	(0.076)	(0.035)	(0.004)	(0.018)	(0.003)
Mean of Y	12.671	3.194	0.662	5.956	-0.607	-3.321	3.437	4.055	4.245
Observations	222,937	1,127,981	1,404,952	254,682	1,248,730	1,529,458	171,529	1,042,547	1,293,523

Note: This table presents heterogeneity analysis by racial/ethnic demographics. These categories are mutually exclusive. Panel A presents results for children whose mother was born in the US and who are White, non-Hispanic; panel B presents results for children whose mother was born in the US and who are Black, non-Hispanic; panel C presents results for children whose mother was born in the US and who are Hispanic (either White or Black); and panel D presents results for children whose mother was born outside of the US irrespective of their race/ethnicity. Econometric specifications mimic those from Table 2. Standard errors clustered at school level in all models.

Appendix

Figures

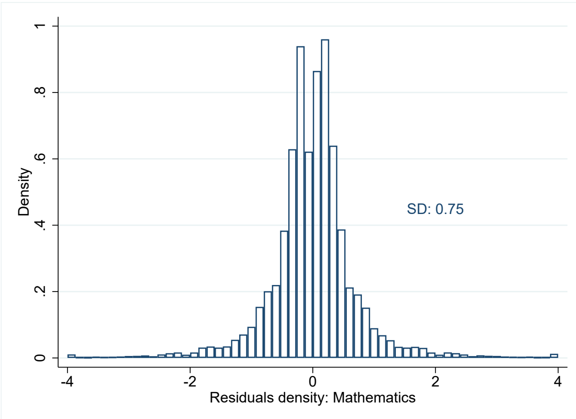
Figure A1: Variation in treatment after taking out fixed effects - individual FE



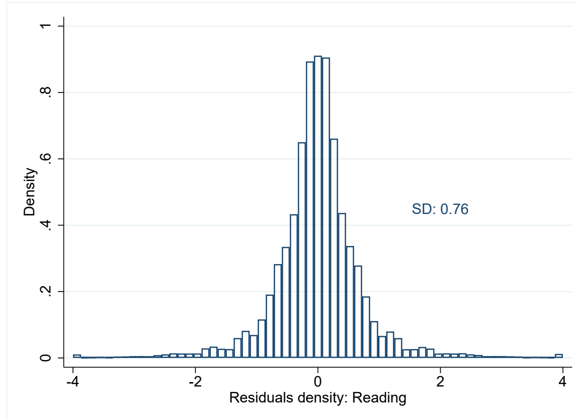
Note: These figures present residuals from regressing our treatment variable - density of charter schools within 5 miles of traditional public school - on fixed effects used in individual fixed specification in panel A and columns 3, 6, and 9 in Table 2. The fixed effects include: student-by-school, school year, and grade. Panel A presents residuals for mathematics sample, panel B presents residuals for reading sample, and panel C presents residuals for absences sample.

Figure A2: Variation in treatment after taking out fixed effects - sibling FE

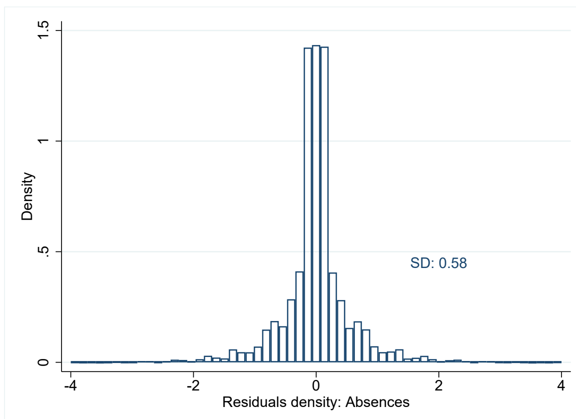
A. Mathematics



B. Reading



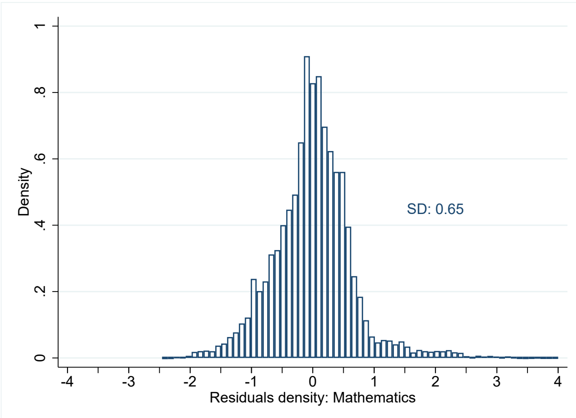
C. Absences



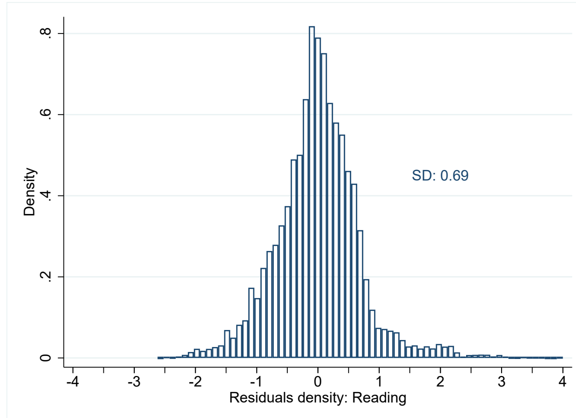
Note: These figures present residuals from regressing our treatment variable - density of charter schools within 5 miles of traditional public school - on fixed effects used in sibling fixed specification in panel A and columns 1, 4, and 7 in Table 2. The fixed effects include: mother-by-school-by-grade and school year. Panel A presents residuals for mathematics sample, panel B presents residuals for reading sample, and panel C presents residuals for absences sample.

Figure A3: Variation in treatment after taking out fixed effects - instrumental variables

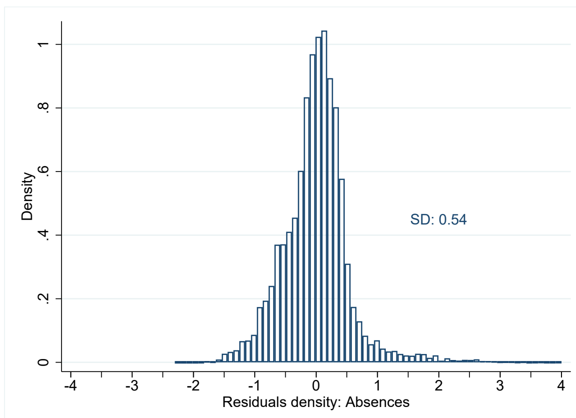
A. Mathematics



B. Reading

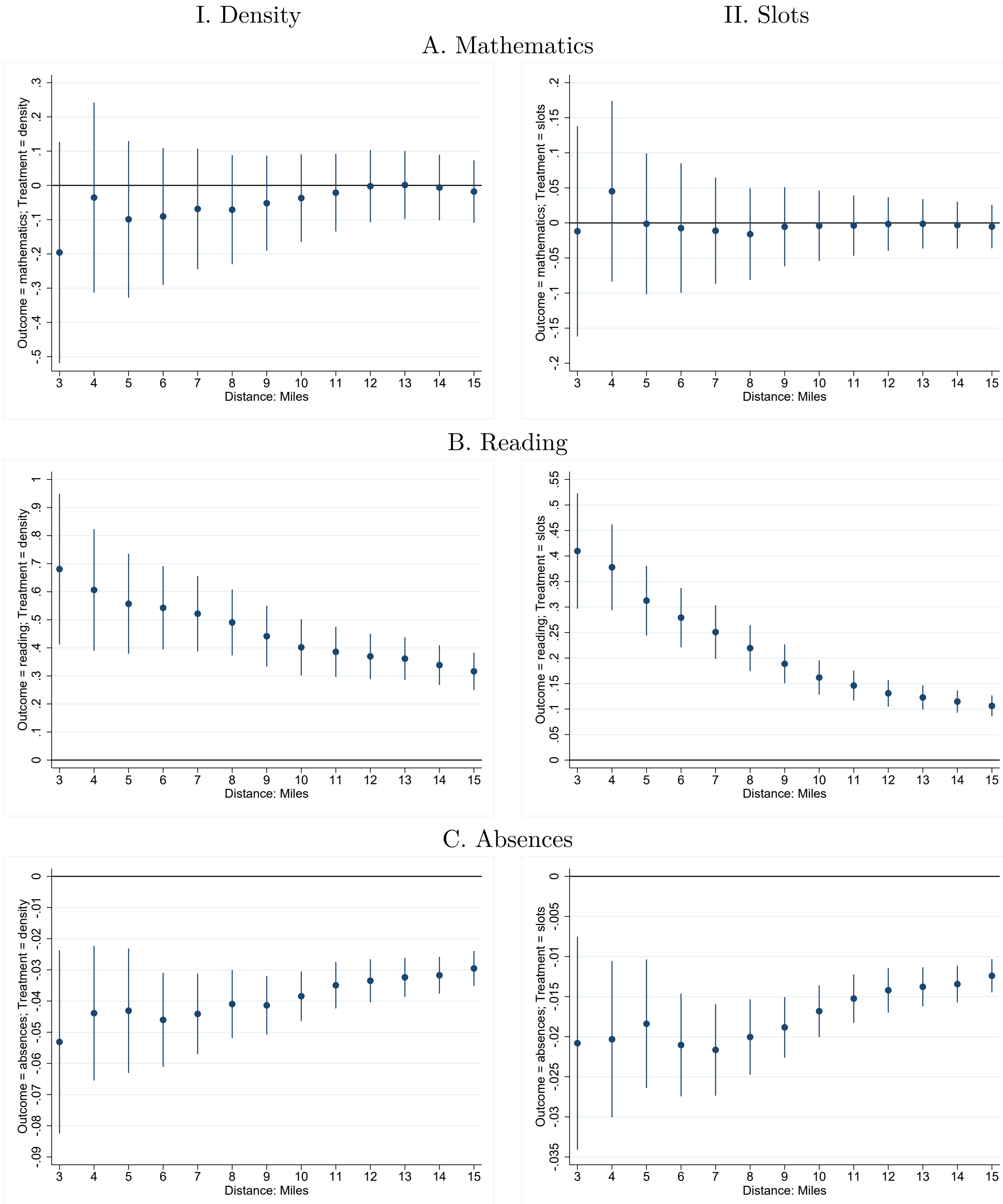


C. Absences



Note: These figures present residuals from regressing our instrument - predicted based on leave one out zip code-by-cohort density of charter schools within 5 miles of traditional public school - on fixed effects used in sibling fixed specification in panel A and columns 2, 5, and 8 in Table 2. The fixed effects include: zip code, school cohort, school year and grade. Panel A presents residuals for mathematics sample, panel B presents residuals for reading sample, and panel C presents residuals for absences sample.

Figure A4: Robustness to changing distance in treatment definition - individual FE

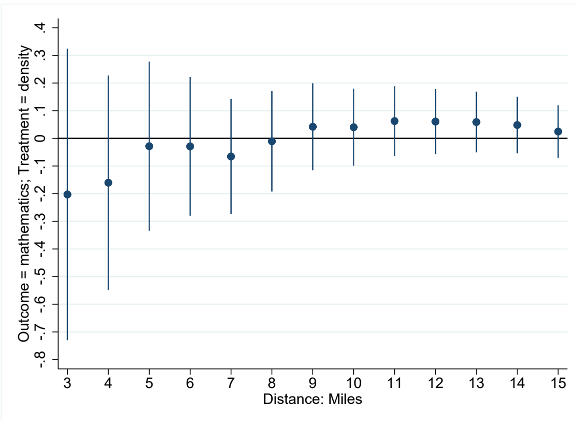


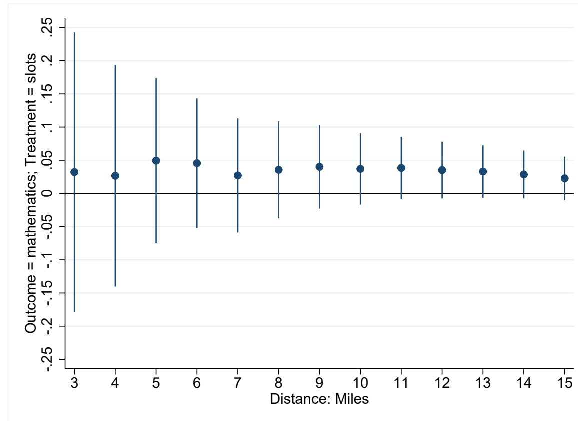
Note: These figures present estimates based on specifications from panels A and B of columns 3, 6, and 9 in Table 1 where we replace our competition treatment variable with competition measured at a given radii from 3 to 15 miles. Standard errors clustered at contemporaneous school level and spikes reflect 95% confidence intervals.

Figure A5: Robustness to changing distance in treatment definition - sibling FE

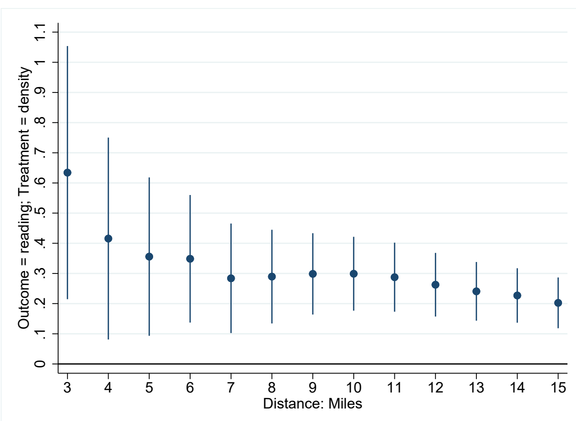
I. Density | II. Slots

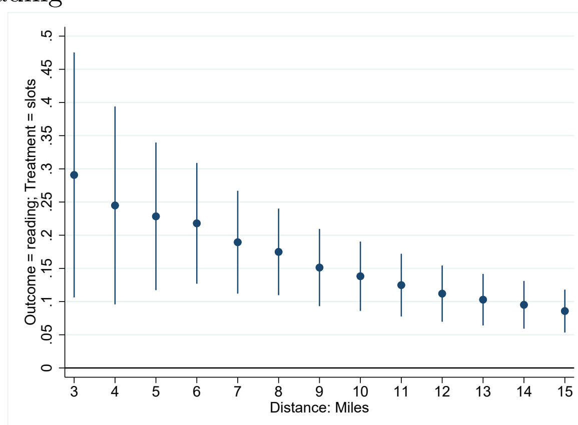
A. Mathematics



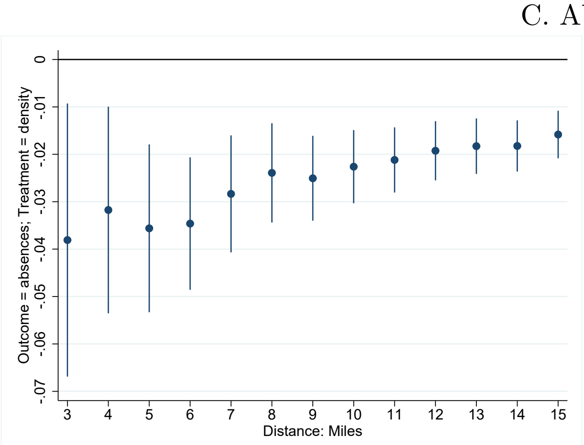


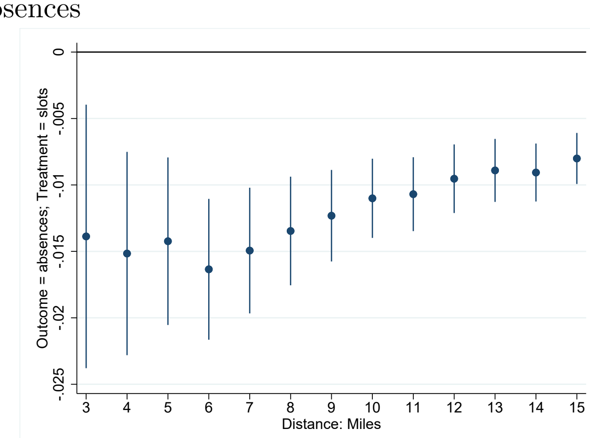
B. Reading





C. Absences



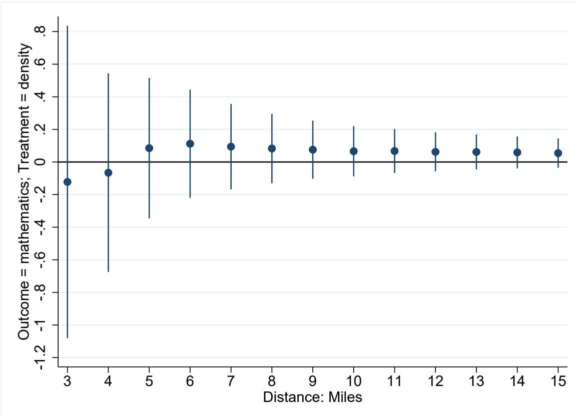


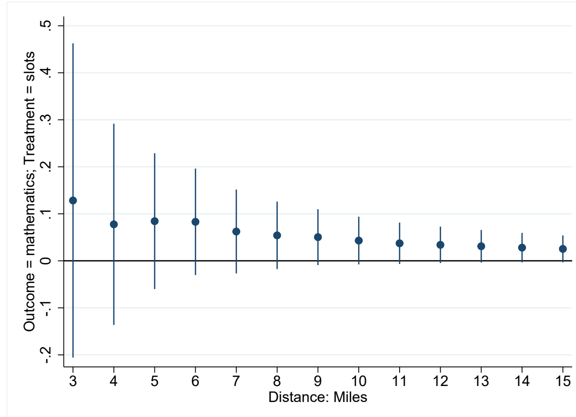
Note: These figures present estimates based on specifications from panels A and B of columns 1, 4, and 7 in Table 1 where we replace our competition treatment variable with competition measured at a given radii from 3 to 15 miles. Standard errors clustered at contemporaneous school level and spikes reflect 95% confidence intervals.

Figure A6: Robustness to changing distance in treatment definition - instrumental variables

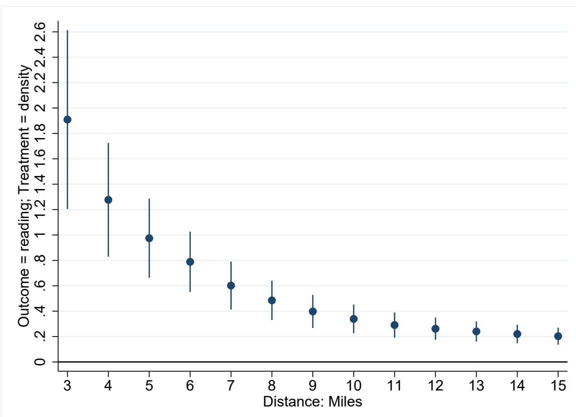
I. Density | II. Slots

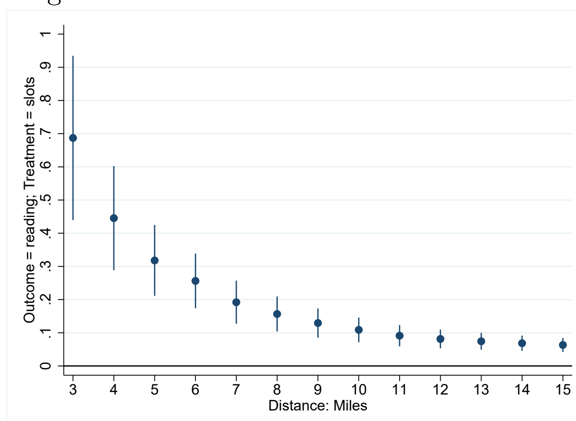
A. Mathematics



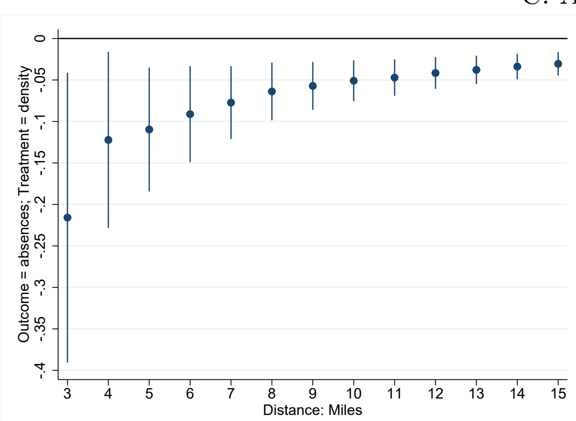


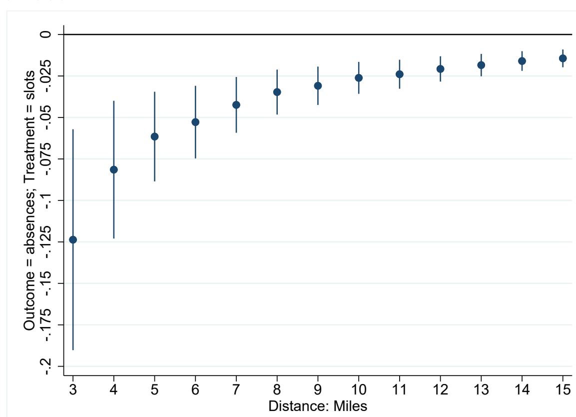
B. Reading





C. Absences





Note: These figures present estimates based on specifications from panels A and B 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. Standard errors clustered at contemporaneous school level and spikes reflect 95% confidence intervals.

Tables

Table A1: Suspensions and absences - 5 miles radius

	(1)	(2)	(3)	(4)	(5)	(6)
		Suspensions			Absences	
	Sibling FE	IV	Individual FE	Sibling FE	IV	Individual FE
A. Density	-0.047	0.017	-0.219***	-0.027***	-0.084***	-0.033***
	(0.043)	(0.103)	(0.046)	(0.007)	(0.017)	(0.006)
F-statistic		616.5			616.5	
B. Slots in 100	-0.047***	0.015	-0.093***	-0.004*	-0.031***	-0.008***
	(0.015)	(0.033)	(0.016)	(0.002)	(0.006)	(0.002)
F-statistic		428.5			428.5	
Mean of Y	7.989	9.474	10.867	4.283	4.823	4.983
# students	354,196	1,135,454	1,480,720	354,196	1,135,454	1,480,720
Observations	1,325,615	6,254,668	7,690,326	1,325,615	6,254,668	7,690,326

Note: This table presents results based on specifications from Table 1 where we decompose absences into probability of being suspended (columns 1 to 3) and absence rate (columns 4 to 6). Due to this decomposition we extend the sample to school year 2011/12. Standard errors in all models are clustered at school level.