The Total Effects of Charter Schools on Student Outcomes: A National Analysis of School Districts

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Abstract: We study the total effects of charter schools, and their mechanisms, on a national level and across multiple outcomes. Using a generalized difference-in-differences method, we find that increasing the charter market share by 10 percentage points increases ELA elementary/middle test scores in the geographic district by 0.01 standard deviations and increases high school graduation rate by 1-2 percentage points. Positive effects for math also emerge, but they are less robust. More generally, effect sizes are non-linear in initial charter market share with local maxima in the 30-60% charter share range. We also find some evidence that charter entry leads to the closure of low-performing traditional public schools, which represents some of the first evidence on this mechanism. The results are reinforced by analysis of potential endogeneity of charter school location and timing using placebo analysis and other methods. By focusing on a narrower set of mechanisms (e.g., participant effects), prior research has likely understated the total effects of charter schools.

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I. Introduction

Charter schools have become one of the most hotly contested topics in American education since they first started three decades ago. These publicly funded schools are exempt from many of the state laws and regulations that govern traditional public schools (TPSs), allowing charter schools to operate with more autonomy in hiring and managing teachers and in choosing curricula and instructional methods. Families can also choose charter schools regardless of whether they live within the neighborhood attendance zones that apply to traditional public schools. Governments still maintain significant control, but their role in charters is more akin to government contracting than government management.

Proponents argue that charter schools and other market-driven approaches could increase innovation, create a better fit between schooling options and student needs, reduce bureaucratic inefficiencies, and increase competition among schools. Theoretically, this improves outcomes for all students, including families who do not actively choose (Goldhaber and Eide 2003), so that a "rising tide lifts all boats" (Hoxby 2003). Others, however, argue that charter schools engage in strategic behavior by selecting motivated, high-performing students (Bergman and McFarlin 2020) and focusing on superficial improvements, such as marketing, rather than improving actual school efficiency (Lubienski 2007, Loeb, Valant, and Kasman 2011, Jabbar 2015, Harris 2020). Charter schools might also divert funds in ways that make it more difficult for TPSs to succeed, due to economies of scale (Ni 2009, Ladd and Singleton 2020, Buerger and Harris 2021).

Descriptively, charter outcomes are consistent with this theory. While trends in student math scores are ambiguous, ELA test scores and high school graduation rates have increased slightly faster in districts with charter schools compared with no-charter districts (Figure 1). The question is whether this reflects a causal effect and, if so, through which mechanisms. Empirical research has examined some of these mechanisms (e.g., participant effects and some types of competitive effects), but only a few other studies have examined the total or "system" effects of charter schools (Ridley & Terrier, 2022; Gilraine et al., 2021; Harris & Larsen, forthcoming) and only with regard to test scores in individual states and cities. We apply a generalized difference-in-differences (GDD) method using two decades of nationwide data from the National Longitudinal School Database (NLSD), which includes nearly all districts in the U.S. from school years 1995 to 2018. To capture the total effects, we estimate the effects of charter entry on a weighted average of TPS and charter outcomes occurring within district boundaries. We think of these geographic districts as being the relevant local markets or school systems.

We find that charter entry increases system-level student outcomes. When charter market share increases by 10 percentage points, geographic district ELA test scores increase by 0.01-0.02 standard deviations and high school graduation rates increase by 2 percentage points. Positive effects also emerge for math in certain specifications, but they are less robust. Identification of these estimates in the GDD is based on within-district changes in charter school entry over time, as in a fixed effects model, while also controlling for state-grade-year fixed effects in outcomes and district-level covariates. While the estimates may seem small, we note that they are average marginal effects across all students—not just charter school students—in districts with charter schools.

The identification strategy accounts for all time-invariant differences across districts and assumes that there are no time-varying unobserved factors that are correlated with changes in charter market share *and* changes in student outcomes. That assumption might be plausible given prior evidence that charter schools tend to locate based on time-invariant, observable factors

(Bettinger 2005, Glomm, Harris, and Lo 2005, Bifulco and Buerger 2015). But we still test for endogeneity in five ways: (1) we apply placebo analysis that tests for "effects" of the entry of charter *high schools* on *elementary/middle* school outcomes and vice versa; (2) we modify the usual DD parallel trends test for this GDD setting; (3) we estimate "treatment effects" on population levels and demographics to test for sorting bias; (4) we estimate effects by state and compare these with the two prior well-identified studies that also incorporate multiple charter mechanisms (Ridley & Terrier, 2022; Gilraine et al., 2021); and (5) we use prior well-identified studies of the individual mechanisms comprising the system-level effect of charter schools (participant effects and near competitive effects) and "add them up" to test whether our estimated effect magnitudes are plausible. All five of the above tests reinforce that these are causal effects of charter school entry.

A second broad contribution of our study is identifying the broad array of potential mechanisms from which these positive effects emerge. First, charter schools might serve their own students more or less effectively than other schools, i.e., participant effects. The empirical strategies in this growing literature include matching (Furgeson et al. 2012, CREDO 2013), fixed effects (Brewer et al. 2003, Bifulco and Ladd 2006, Sass 2006, Booker et al. 2007, Hanushek et al. 2007), and lotteries (Hoxby and Rockoff 2004, Abdulkadiroğlu et al. 2011, Curto and Fryer 2014, Angrist et al. 2016). Positive participant effects could emerge if charter schools are innovative, use more efficient management designs, and/or are placed under greater pressure through their performance-based contracts (Harris, 2020). Participant effects vary, but the average is small, positive, and improving over time (Cohodes and Parham 2021).

Studies of the competitive effects of charter schools on student outcomes in TPSs are also common and also generally find small positive effects on student achievement, which also vary across contexts, research methods, and measures of competition (Hoxby 2003, Bettinger 2005, Bifulco and Ladd 2006, Sass 2006, Ni 2009, Zimmer and Buddin 2009, Linick 2014, Cordes 2018, Ridley and Terrier 2022, Griffith 2019, Slungaard Mumma 2022). In a few cases, charter competition reduced student outcomes (Ni 2009, Imberman 2011, Han and Keefe 2020).[1,2]

But these analyses of participant and competitive effects miss at least three other potential mechanisms. One, less recognized, mechanism is that charter schools might induce low-performing TPS to close, improving overall outcomes by leading more students to attend high-performing schools. While this can be viewed as an extension of competitive effects, the above competition literature focuses only on the effects on TPS that *remain open*. If charter schools induce low-performing schools to exit, as we expect in other markets, then this might improve student outcomes by forcing students into higher-performing schools.[3] Previous studies have examined the effects of school closure on student performance (Engberg et al. 2012, Bross, Harris, and Liu 2016, Carlson and Lavertu 2016) and charter entry effects on private school enrollment (Chakrabarti and Roy 2016), but not how charter entry affects the closure of TPS.

To our knowledge, no prior research has assessed these "closure competitive effects." We find that a 10 percent increase in charter market share increases TPS school closure rate increases by 2 percent. Also, the TPS that close have lower student growth/value-added than those that remain open, suggesting that these closures partly contribute to the positive system-level effect. Relatedly, we find little evidence that charter entry reduces private school

[1] Results from Arizona (Hoxby 2003), Florida (Sass 2006), and Texas (Booker et al. 2008) suggest that there are positive competitive effects of charters on TPSs, and results from California (Zimmer and Buddin 2009) and North Carolina (Bifulco and Ladd 2006) suggest no effects. However, results from Michigan are mixed, with positive effects (Hoxby 2003), zero effects (Bettinger 2005), and negative effects (Ni 2009).

[2] A related literature exists on the competitive effects on TPS from private school vouchers (e.g. Figlio and Hart (2014).

[3] We also note that, if charter schools induce low-performing TPS to close, then this might bias "participant effects" downward as charter schools get compared with increasingly effective TPS.

enrollment.[4] This finding for private schools is useful both for: (a) ruling out selection bias from charter schools inducing students to move in or out of private schools where we cannot measure their outcomes, and (b) understanding possible effects of charter schools on private schools, which could also affect educational outcomes and social welfare.

A fourth possible mechanism is that school district leaders respond to charter entry by changing policies and practices for TPS on a districtwide level. This, too, is a type of competitive effect, but most competitive effect studies do not capture this mechanism either because they compare TPS located *nearby* charter schools to those located further away. But individual traditional public schools are usually funded at the discretion of district administrators, not based on the number of students, so school further away may also be affected. Charter entry might spur district administrators to make broad-based changes and create a "far competitive effect," especially in geographically large districts where many TPS are located far away from newly entering charter schools. This potential mechanism of charter influence has apparently not received much attention either.[5]

Fifth, in addition to participant effects, and the various types of competitive effects, the extension of choice could improve the match between students and schools. Campos and Kearns (2022) find that students in Los Angeles did better when attending their most preferred schools (measured by ranked preferences in a unified enrollment system), even after controlling for

[4] One might argue that this is inconsistent with the evidence that voucher programs create competition that improves TPS (Figlio and Hart 2014); however, those situations are different because voucher programs make private schools accessible to students who typically attend TPS. Here, we are focusing on the effects of charter schools on private school that are financially inaccessible.

[5] Case studies of school districts do sometimes suggest that school districts respond to charter competition (Hess 2004, Holley, Egalite, and Lueken 2013). Also, Figlio, Hart, and Karbownik (2022) find effects on whole districts from school vouchers. There is also evidence of Tiebout competition between districts (Hoxby 2000). On this point, see the response from Rothstein (2007). Also, we note that, if this far competitive effects exist, then many of the competitive effects studies under-state (over-state) effects on TPS because they identify those effects from comparisons of near and far TPS, assuming the latter are untreated.

school value-added.[6] The mechanism behind this "match effect" comes from the way that charter school introduce or expand choice and perhaps how they create a more differentiated set of options.

The national, system-level effects described above—0.01 s.d. on test scores and 1-2 percentage points on high school graduation—reflect the total effect of all of the above mechanisms.[7] Our analysis is most similar to three other studies.[8] Ridley and Terrier (2022) and Gilraine, Petronijevic, and Singleton (2021) study the net effects of multiple mechanisms on test scores in Massachusetts and North Carolina, respectively, leveraging exogenous changes in charter caps. In addition to being limited to individual states, these studies do not apparently capture all five charter mechanisms noted above.[9] Harris and Larsen (forthcoming) study the system level effects of charter schools in New Orleans on test scores, high school graduation, and college-going, though this study is limited to a particular and unusual city. We build on this prior work by going beyond individual states and districts, studying the total effect of all the potential market mechanisms, and going beyond test scores to include high school graduation.

We also examine several novel forms of effect heterogeneity, starting with separate effects for each state. Arizona, Massachusetts, and Wisconsin are the only states where the point

[6] While not specifically focused on charter schools, see evidence on the welfare effects of matching students to schools through centralized enrollment systems (Abdulkadiroglu et al., 2017).

[7] A sixth possible mechanism is that just the threat of charter entry could lead TPS to improve, but this is really just an element of the near, far, and closure competitive effects. That is, TPS might respond to the threat and/or to actual charter entry.

[8] Campos and Kearns (2022) focus on the introduction of Zones of Choice in Los Angeles, though this does not involve the entry of new schools or changes in charter market share. Han and Keefe (2020) also use nationwide SEDA district-level data and district-fixed-effects models to study the effects of charter competition, but their study is more descriptive in nature. Their analysis does not weight by enrollment, which has the effect of counting outcomes in small districts too heavily and missing important effect heterogeneity. Also, their study does not test for or address endogeneity. Finally, they frame their analysis as a study of competitive effects only, which does not reflect the full range of mechanisms.

[9] Ridley and Terrier (2022) explicitly omit charter outcomes and therefore focus on the various competitive effects. Gilraine, Petronijevic, and Singleton (2021) do use a weighted average of charter and TPS outcomes, but it is not clear how they handled TPS schools that close during the panel. An advantage of the present study is explicitly calling out these various mechanisms and how they are captured in the data.

estimates are positive for all three outcomes and precise for at least two of them. While these state results are perhaps of interest in their own right, they also provide an informal test for identification of the national average treatment effects reported above. Our positive results for Massachusetts are qualitatively similar to those found by Ridley and Terrier (2022); and our small and imprecise effects for North Carolina are similar to those found by Gilraine, Petronijevic, and Singleton (2021). In both cases, our point estimates also fall well within the confidence intervals of the other studies.[10] As further corroboration, our results also align with prior findings that charter school effects are more positive in urban areas and for some students of color (CREDO 2013, Chabrier, Cohodes, and Oreopoulos 2016, Cohodes and Parham 2021, Campos and Kearns 2022).

We also study non-linearities in effects by charter market share. This issue has significant policy implications given the caps used in many states and the rising the number of districts, such as New Orleans, that have moved to near-100-percent charter shares. We find consistent evidence of diminishing returns to charter share in graduation rates and ELA scores; and some evidence of this in math scores. We pose various explanations for this pattern that warrant further investigation.

The paper proceeds as follows. Section II introduces the data. Sections III discuss the methods. Average treatment effects are discussed in section IV and heterogeneous analyses in Section V. Section VI provides some discussion and Section VII concludes.

[10] We cannot compare our results to Louisiana (Harris & Larsen, forthcoming) as the vast majority of elementary/middle school charters entered in the aftermath of Hurricane Katrina in 2005 and the test score data do not start until 2009. Also, our high school graduation data end in 2010 and few high schools had been turned into charter schools at that point.

II. Data and Descriptive Statistics

This paper uses data from the National Longitudinal School Database (NLSD), which contains some data for a near-census of all TPSs, charter schools, and private schools in the U.S. from 1991 to the most recent academic year.[11] The NLSD merges school and district level data from Common Core of Data (CCD), Stanford Education Data Archive (SEDA), Census Small Area Income and Poverty Estimates (SAIPE), and other sources. We are most interested in the student outcomes, test scores and graduation rates, but also include a vector of district-level covariates in some models (enrollment, district location, district finance, student-teacher ratio, teacher salary, number of magnet schools). The addresses of charter school also allow us to place the location of each charter school within its geographic school district.[12] These districts define the scope of the market, i.e., which schools are assumed to compete with one another.

The specific dependent variables are the average freshmen graduation rate (AFGR) (hereafter, graduation rate) and student Math and English Language Arts (ELA) test scores. The AFGR uses aggregate student enrollment data to estimate the size of an incoming freshman class and aggregate counts of the number of diplomas awarded four years later. For example, the AFGR for a school year in 2006 is the total number of diploma recipients in 2006 is divided by the average enrollment of the 8th-grade class in 2002, the 9th-grade class in 2003, and the 10th-grade class in 2004.

The test scores are available in 3rd through 8th grade in Math and ELA over the 2009-2018 school years from the Stanford Education Data Archive (SEDA). The SEDA provides nationally

[11] All the school years mentioned in this paper are spring school years unless specifically stated otherwise.
[12] The SEDA team uses the 2019 Elementary and Unified School District Boundaries https://nces.ed.gov/programs/edge/Geographic/DistrictBoundaries) to define the Geographic School District for test score sample (except for special education or virtual schools). We use the same boundaries to define the Geographic School District for the graduation sample.

comparable, publicly available test score data for U.S. public school districts[13] (Ho 2020). For interpretation purposes, we normalize the test scores to have means of zero and standard deviations of unity within the grade, year, and subject.

We also sometimes include the following (time-varying) covariates in the GDD: total enrollment (log form); the share of students who are Hispanic, Black, white; the share of students who are in special education programs; the share of students eligible for free or reduced-price lunches (FRL); as well as student-teacher ratio, average teacher salary, number of magnet schools, total revenue per student, and total expenditure per student. Since these measures are potentially endogenous, we view results with these controls as an endogeneity test, not as the preferred effect estimates.

The graduation rate sample includes schools covering grades 9-12 annually from 1995 to 2010. We use the charter enrollment share, or the percentage of public-school students enrolled in charter schools to define district treatment status. These market share measures are created separately by grade level under the theory that charter schools only affect the geographic district outcomes in the specific grades being served (Jinnai 2014, Slungaard Mumma 2022).[14]

For both outcomes, data are aggregated to the geographic district level by averaging TPS outcomes with those of charter schools located within their boundaries (weighted by enrollment share). [15] This is crucial to the analysis as it allows us to capture charter effects operating through

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[13] To make estimates are comparable across states, grades, and years. The SEDA research team took the following steps: (1) estimate the location of each state's proficiency "thresholds"; (2) place the proficiency thresholds on the same scale using the National Assessment of Educational Progress (NAEP); (3) estimate the mean test scores in each school, district, county, metropolitan statistical area, commuting zone, and state from the raw data and the threshold estimates, and (4) create estimates of average scores, learning rates, and trends in average scale scores. See details in SEDA website https://edopportunity.org/methods/.

[14] We also expect the four other mechanism effects to be concentrated in the grades served by charter schools. Nearby TPS should be most pressured by charters serving the same grades, and so on.

[15] SEDA provides test score data at the geographic district level. We converted the graduation rate data to the geographic district level ourselves. Please refer to Appendix B for the description that how the sample was created.

all of the various market mechanisms. In contrast, prior studies of charter schools have focused on the outcomes of individual schools in the examination of school participant effects or the competitive effects of individual charter schools on nearby TPS. Here, we are interested in the total effect of all the mechanisms, which means we need to account for the outcomes of all traditional *and* charter schools. Aggregating these to the geographic district level also allows us to sidestep the main problem with prior studies, i.e., selection bias from students moving across schools.

The test score (graduation) data are available for 11,399 (10,658) districts, including 1,597 (1,073) districts that have at least one charter school. The final samples for the test scores (graduation) analyses include 95 (95) percent of the nation's publicly funded schools and 98 (83) percent of nation's charter schools.[16] Table 1 presents the summary statistics for the outcome and control variables. Compared with TPSs, charter schools nationwide tend to enroll a larger proportion of African American and Hispanic students. Charter schools also more likely to be in urban districts and where achievement is relatively low.

We are identifying total charter effects leveraging the timing of charter entry. One key factor affecting this is the timing of the passage of charter laws that allow them to entry. The first law allowing the establishment of public charter schools was passed in Minnesota in 1991. As of fall 2022, charter school legislation had been passed in 45 states and the District of Columbia.[17] Appendix Table A1 presents the years of charter school legislation as of 2022. As a result of the growing number of states allowing charter schools, the percentage of publicly funded schools

[16] Since districts are the unit of analysis, an observation is only missing if all the schools in the unit-by-school-type are missing. There are likely some missing schools within districts. Appendix B describes in detail how we compose the samples.
[17] The states in which public charter school legislation had not been passed by that time were Montana, Nebraska, North Dakota, South Dakota, and Vermont.

that are charter schools increased from 0 to 7.3 percent, and their percentage of enrollment increased from 0 to 6.2 percent.[18]

Some districts, however, have much larger shares. New Orleans is the most well-known example and one of very few that are essentially 100 percent charter. Appendix Table A2 lists the top-20 districts in terms of charter market share, among districts with total enrollments larger than 10,000 students. We note that while much of the focus of research and public discussion focuses on a handful of districts (Boston, New Orleans, and so on), many districts meet these criteria.

III. Identification Strategy

The GDD identifies effects from within-district variation in dosage over time (for those ever treated) as well as differences between treated and untreated units (Mouganie, Ajeeb, and Hoekstra 2020, Dow et al. 2020). This is preferable to a simple DD, and its more recent variants, in cases such as this with continuous, time-varying treatment. A simple DD would ignore most of the variation in charter market share and focus only on the point where the first charter school enters and treatment begins.

The dependent variables in our GDD analysis are test scores ($Test_{igt}$) and high school graduation (GR_{it}) in district *i* and grade *g* during year *t* (test scores are grade-specific; graduation is not). We include unit fixed effects μ_i (in this case, for geographic districts) as well as state-grade-year fixed effects λ_{sgt}. This implies the following models:

[18] Figure A1 in the Appendix presents the trends in charter school share and charter enrollment share from spring 1991 to spring 2018.

$$GR_{it} = \alpha + \beta_1 Charter_{it} + X_{it}\gamma + \mu_i + \lambda_{st} + \varepsilon_{it} \quad (1)$$

$$Test_{igt} = \alpha + \beta_2 Charter_{igt} + X_{it}\gamma + \mu_i + \lambda_{sgt} + \varepsilon_{igt} \quad (2)$$

Treatment is the continuous, time-varying charter market share, $Charter_{it}$. The coefficients of interest are represented by $\boldsymbol{\beta}$. X_{it} is a vector of district characteristics. As these covariates are potentially endogenous, they are only included in certain specifications and as partial tests for charter effects on the student population.

The error term ε_{igt} is clustered at the district level in the main specification. (We report results clustered at the state level in Appendix Table A3, which only slightly reduces precision.) The estimates are weighted by the district size, so that the estimates are nationally representative; specifically, we weight by graduation rate denominator in the high school sample and by grade-level enrollment for Math and ELA. In our baseline analyses, we estimate the GDD model for both the current year charter enrollment share and with a one-year lag to allow delayed effects.[19] For test scores, we specifically estimate using the prior year's $Charter_{it}$ for the same cohort.[20]

Our method accounts for time-invariant unobserved differences across districts and our main identifying assumption is that there are no unobserved time-varying factors that are correlated with both changes charter market share *and* changes in student outcomes. We note that, even if charter market share changed based on time-invariant unobserved district characteristics that affected student outcomes, this would not introduce bias if the timing of charter entry were conditionally random, which is possible given all the steps involved in opening a charter school.

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[19] For example, we use the charter enrollment share of high school (grades 9-12) in 2009 to estimate the effects on graduation rate in 2010.
[20] For example, we use the charter enrollment share of Grade 7 in 2010 to estimate the test score of Grade 8 in 2011.

Still, it is worth considering specific scenarios under which this might be violated. First, charter schools are more likely to locate in districts with low contemporaneous student outcomes (Glomm, Harris, and Lo 2005), signaling that we also might expect charter schools to also locate where *expected* future performance in TPS is lower, in ways that are unobservable in our analysis.[21] This would lead to a downward bias in our estimates because TPS outcomes comprise the majority of the weighted average outcomes.

Alternatively, local governments might change non-charter education policies at the same time they introduce charter schools, as part of a package of reforms. Since school districts are often charter authorizers, it may be that a change in school board politics leads both to an increase in charter schools and, for example, new reading and math curricula in TPS. In this case, we might falsely attribute outcome changes to charter schools that were actually caused by other policy changes.

We discuss various tests for these and related types of violations in the next section, after describing our main results.

[21] Relatedly, charter schools might open where districts experience idiosyncratic shocks, in which case future outcomes regress toward the mean. If charter schools prefer to locate near low-performing school, as noted above, then this would yield an upward bias (i.e., charter schools enter because of the negative shock in existing schools, but then bounce back with positive shocks in the next period). We do not see this scenario as very likely because it takes several years to create an organization that can assemble a charter application, submit the application and gain approval, hire personnel, purchase necessary capital, and recruit students. Also, with such a long-term investment, charter organizations are likely to consider multiple years of information in making their entry decisions, which reduces the chance of regression to the mean.

IV. Results

IV.A. Average Treatment Effects from GDD

We find consistent evidence that charter entry improves geographic-district-level outcomes. Table 2 reports estimates from the GDD models in equations (1) and (2) where a one-unit change in charter market share means going from zero to 100 percent charter market share. Column (4), for example, shows that a 10-percentage point increase in high school charter enrollment share increases the high school graduation rate one year later by a bit less than two (1.72) percentage points; the same change in elementary charter enrollment share increases math scores by 0.0025 standard deviation (s.d.) and ELA by 0.0113 s.d. The estimates for high school graduation and ELA scores are generally larger and more precisely estimated and those for math. Contemporaneous effects are also smaller than the lagged effects (compare columns 1-3 versus 4-6). This delay could reflect gradual improvement of charter schools (participant effects) in their early years, a slightly delayed competitive response by TPS, or improved matching as students learn more about the new schools and sort over time.

Examining the other columns, we see that the results are robust to the addition of student and district controls and to altering the dependent variable to be one year in the future (with a partial exception of Math). The results are also similar when we switch the charter market treatment variable from the share of *students*, in Table 2, to the share of *schools* (Appendix Table A4). Our main specification also includes state-grade-year fixed effects. The results are also generally robust to the addition of district-specific time trends (Appendix Table A5) for high school graduation, but less so for test scores.

The above main specification includes all districts (with non-missing data). However, as discussed later, the effects of the first charter entrant might differ from the effects of subsequent ones; and, especially in the test score sample, many districts had their first charter schools enter *before* the first year in the panel. To address this, Table A6 reports the results using only treatment districts where the first charter school entered after the first year of data. These results show more positive effects in math and ELA scores, while the graduation results remain simikar to above.

Again, the dependent variable is a weighted average of outcomes for all publicly funded schools in the geographic district, so that the coefficients reflect estimates of the system-level average treatment effects. They capture the full range of mechanisms discussed earlier (participant effects, near competitive effects, and so on).

IV.B. Threats to Identification

The identifying assumption of our estimates is that there are no time-varying unobserved factors that are correlated with both changes charter market share *and* changes in student outcomes. In this section, we test this assumption in five ways.[22]

IV.B.1. Placebo using misaligned grades

First, we use placebo analyses that leverage the fact that the entry of charter high schools cannot directly affect elementary student outcomes.[23] The effects of elementary charter entry on high school graduation should also be minimal, within the scope of our panel, because there are so many years in between elementary school attendance and these long-term outcomes.

Table 3 shows results for these placebo analyses. The first pair of columns reports the effects of elementary charter entry on high school graduation, while the last two pairs of columns

[22] We also carried out a version of the Oster bounds. However, this is implicated by the fact that the covariates are almost entirely fixed effects. Results for this analysis can be found in Appendix Table A7.

[23] It is conceivable that charter high schools have indirect effects on elementary outcomes, e.g., by leading families with more resources or higher education expectations to move into the district. However, this would be captured in the test for demographic change described below. Even if this type of sorting effect did occur, It would not show up in the effects of high school entry on high school graduation until many years later.

show the estimated effects of high school entry on elementary/middle scores. As predicted, the estimated effects are much smaller, about one-tenth the size, of treatment effects in Table 2. We also re-estimated using a one-year lag in outcomes, to allow for possible delayed effects (as in Table 2), but the results are very similar.

IV.B.2. Parallel trends

We also apply a version of the usual parallel trends tests common to simple DD analyses. This test is less definitive with the GDD, which identifies effects using all the within-district variation in charter schools and outcomes over time. Still, we might expect evidence of endogeneity to show up regardless of whether we are examining the first charter entrants or later entrants. We implement the parallel trends test, first, with a simple two-way fixed effects (TWFE) event study:

$$GR_{it} = \alpha + \sum_{r=-m}^{q} \beta_r \left(T_i \cdot d_{i,r}\right) + X_{it}\gamma + \mu_i + \lambda_{st} + \varepsilon_{it} \quad (3)$$

$$Test_{ijt} = \alpha + \sum_{r=-m}^{q} \beta_r \left(T_i \cdot d_{i,r}\right) + X_{it}\gamma + \mu_i + \lambda_{sgt} + \omega_j + \varepsilon_{ijt} \quad (4)$$

where T_i is a treated district and $d_{i,r}$ is an indicator of the *r* years of leads or lags since district *i* initiated the first charter school.[24] We assign districts to treatment in two different ways: (a) districts are assigned to the treatment group (T_i=1) if they have zero charter schools at least one year and then experience their first charter entry during this panel (always-treated districts are dropped); or (b) T_i=1 for districts that experience a spike in charter market share of at least two percentage points (regardless of the initial charter market share level) and the treatment turns on at the point of the spike. In the graduation (test score) analysis, five or more (three or more) years leads/lags for graduation rate sample are assigned into a single indicator given the length of sample period. The vector $\boldsymbol{\beta_r}$ represents measures of cohort-specific effects compared with the comparison group.

Figure 2a shows tests for parallel trends from the DD using traditional TWFE event studies (left column) and the Sun and Abraham (2021) IW method (right column) with method (a) above for assignment to treatment. Figure 2b reports the results focusing on charter market share spikes, i.e., method (b) above. All of the results pass pre-trends for high school graduation, as well as ELA (albeit with a one-year drop in Figure 2b at t=-2). With the math sample, we see some evidence of a declining pre-trend in the Figure 2b only.

One reason for reporting the parallel trends tests using event studies is that we can also see the post-treatment patterns. In Figure 2b, the results closely mirror the main treatment effects in Table 2 with clear evidence of positive effects on high school graduation and ELA scores. With math, it appears that charter schools stop the pre-treatment decline in scores. This may explain why the math results look less positive than ELA generally.

IV.B.3. Effects on Population and Demographics

Student outcomes are positively correlated with household socio-economic status, so it could be that charter entry arises in conjunction with changes in local social and economic conditions that are related to demographic sorting patterns. While the addition of covariates in the GDD suggests that population change is not an important factor, we can test this more directly by estimating "treatment effects" on population levels and demographics using the same method (see equations (1) and (2)).

In Table 4, we see no precisely estimated effects on either enrollment size or student poverty (as measured by free or reduced-price lunch (FRL) eligibility). The point estimates are also very small in magnitude. For example, the largest (in absolute value) coefficient suggests that increasing charter entry by 10 percentage points increases FRL by 0.34 percentage points. In short, we see no evidence that charter effects really reflect changing household demographics.

IV.B.4. Comparison with Massachusetts and North Carolina Studies

Our analysis is most similar to Ridley and Terrier (2022) and Gilraine, Petronijevic, and Singleton (2021) who study the net effects of multiple mechanisms on test scores in Massachusetts and North Carolina, respectively, leveraging exogenous changes in charter caps. While we do not have access to this form of exogenous variation in all states, we can test for bias by re-estimating effects for each state separately and comparing our results to these two states (for test scores only as their estimates do not include graduation).

Our state-by-state results, shown in Appendix C are qualitatively similar to those from Massachusetts where Ridley and Terrier (2022) also find positive effects on test scores; and similar to those from North Carolina where Gilraine, Petronijevic, and Singleton (2021) also find null effects. In both cases, our point estimates also fall well within the confidence intervals of the other studies.

Our national results also happen to line up well with those of the third study of system-level effects. In New Orleans, the charter school share went from almost zero to 100 percent and increased test scores, high school graduation, and college outcomes (Harris & Larsen, forthcoming). In Table 2, this type of shift implies effects of 17 percentage points on high school graduation and 0.13-0.24 s.d. increase in ELA test scores—figures quite close to those reported by Harris and Larsen (forthcoming).

IV.B.5. Comparisons with Other Studies

As a final approach to testing for endogeneity, we use prior well-identified studies of the individual mechanisms and “add them up” to provide a rough test of whether our estimated effects of the system-level charter effects are plausible. If the system-level effects seem to go in the wrong direction or are too large in magnitude, then this would indicate that the identifying assumption is violated.

The effect sizes in Table 2 appear plausible given the prior literature. CREDO (2013) reports an average national treatment effect on reading scores, across 16 states, of 0.01 s.d.[25] Since charter schools are only a fraction of schools (7 percent nationally), this translates to essentially a null effect at the system level where these effects are weighted by the charter share (0.07*0.01=0.0007 s.d.). The "near competitive effects" are also usually small and positive. Bifulco and Ladd (2006), for example, find that TPS exposure to a charter school increases TPS achievement by 0.02 s.d. Again, this affect only applies to the fraction of TPS that are near charter schools, but this, too, points to the idea of a small, positive system-level effect of the sort found in Table 2.

IV.C. Non-Linear Effects

There are various reasons to expect that the effect of the marginal charter school is non-linear in the baseline charter market share. Here, again, reference to the five mechanisms can help us understand the possible patterns. Charter participant effects might display diminishing marginal returns (DMR) if some charter authorizers have "low standards" and decide to accept a large number of charter applications where the marginal applicants are less efficient. On the other hand, we might expect the match effects to display increasing marginal returns as the entry of each new charter school not only improves the match for that school, but other schools as well. The various competitive effects could go in various directions. TPS might respond

[25] We focus on the most recent years of data (2010 and 2011) as these most closely align with the present study. This report finds no effect on math.

aggressively as the first charter schools open, leaving them with few potential responses as the charter market share rises; with large charter share, TPS might also struggle to improve if revenue drops to the point that involuntary staff reductions become necessary.[26] Finally, it is possible that competition *between sectors* is more powerful than competition between individual schools, so that very high charter share would be counter-productive.

We tested for non-linear system effects by modifying equations (1) and (2) so that the treatment variable, $Charter_{it}$, is entered as a polynomial: quadratic (Appendix Table D1), cubic (Table D2), and quartic (Table D3). We see clear evidence of diminishing returns in the quadratic where the linear term is positive/precise and the squared term is negative/precise for high school graduation (contemporaneous and lagged) and ELA (lagged only). The math results, as above, continue to be an outlier.

We show these results visually in Figures 3a-3b for all four functionall forms. All of the results reflect diminishing returns with an inflection point in the 30-60 percent range in graduation and ELA. Since the distribution of charter market shares is heavily skewed to low shares (Figure D1), we also re-estimated, restricting to the 0-30 percent charter market share range and see the same pattern (Figure D2). Only one of the nine estimates (for math scores with a quadratic form) does not clearly display some form of diminishing returns.

[26] In theory, cutting staff and schools could improve efficiency if lower-performing staff are cut first, but TPS do not have the information or the autonomy to operate this way. Staff cuts would generally be on a last-in-first out (LIFO) basis. Also, even if they did fire lower-performers, the re-allocation of staff would be disruptive and probably have some negative short-term shocks on efficiency.

V. Effect Heterogeneity

Prior research has suggested that charter school participant effects vary by urbanicity and student demographics. We also find these patterns. Column (1) on Table 5 presents the heterogeneous effects in Metropolitan Areas (MA) and non-MA. The results show that compared with non-MA districts, a 10 percent increases in charter share improves the graduation rate in Mas by 1.5 percentage points and improves Math and ELA scores in Mas by 0.049-0.055 standard deviations. This pattern is consistent with prior charter school participant effects by urbanicity (CREDO 2013, Chabrier, Cohodes, and Oreopoulos 2016).

We also find more positive effects for Hispanic students (though these are only precise with high school graduation), but this same result does not hold for Black students. For free lunch students, the results are also positive for high school graduation, but negative for test scores. Since these results by demographic group seem to run against the conventional wisdom, we note again that we are not estimating the same effects as prior studies. The conclusion that racial/ethnic minorities benefit more comes mainly from participant effect estimates, but these estimates seem to comprise a small share of the total effect. As noted earlier, the charter market share is so small that the role of the participant effect is likely small. Further, some additional evidence in Florida suggests, consistent with our findings, that competitive effects are negative for Black students (Figlio, Hart, and Karbownik 2021).

It is also possible that charter effects vary because some traditional public schools are performing so poorly, so that charter schools address the "low hanging fruit" of school improvement. To test this, we estimate versions of equations (1) and (2) but adding an interaction between treatment and baseline achievement level. All of these interaction coefficients are negative, which is consistent with the above theory, though they are imprecisely estimated.[27]

Our average treatment effects are also a weighted average across states with different charter policies (e.g., how well funded charter schools are relative to traditional public schools, charter authorization and shutdown policies). Since there are many dimensions to these policies we interacted treatment with state charter law scores reported by the National Alliance for Public Charter Schools (NAPCS) in 2020 (Ziebarth 2020).[28] Results in Table 5 column (6) suggest consistent evidence of a negative interaction between charter effects and the policy index for all three outcomes.

Finally, we estimate heterogeneous effects by school grade levels for test scores and report the results in Table 6. The results show precisely estimated positive effects on both Math and ELA test scores for middle school students (grades 6-8), but smaller and more sporadic effects for elementary school students (grades 3-5). Specifically, increasing the charter middle school market share increases Math and ELA scores of middle schools by 0.03-0.05 standard deviations.

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[27] Another possibility is that certain types of charter schools, such as those using the "No Excuses" approach (Angrist, Pathak, and Walters 2013), are more likely to locate in urban areas, (Chabrier, Cohodes, and Oreopoulos 2016), but this is difficult to test in these data.

[28] NAPCS rated each state on 21 key components of state law, such as accountability, authorization, flexibility, performance-based contracts, and funding equity. NAPCS gave each component a weight of 1 to 4 and multiplied these by the component rating to obtain an index for each state. We divided these scores by the total score and therefore, the value of scores ranges from zero to one with the mean of 0.53 and the standard deviation of 0.23.

VI. Mechanisms of the Total Charter Effect

The above analyses estimate the total effects of charter entry on entire school districts, capturing all five mechanisms, collectively. Prior research provides ample examples of participant effects (Brewer et al. 2003, Hoxby and Rockoff 2004, Bifulco and Ladd 2006, Sass 2006, Booker et al. 2007, Hanushek et al. 2007, Abdulkadiroğlu et al. 2011, Furgeson et al. 2012, CREDO 2013, Curto and Fryer 2014) and competition effects (Hoxby 2003, Bettinger 2005, Bifulco and Ladd 2006, Sass 2006, Ni 2009, Zimmer and Buddin 2009, Imberman 2011, Linick 2014, Cordes 2018, Griffith 2019, Han and Keefe 2020). In this section, we switch from district- to *school-level* data to provide new evidence on a mechanism that has received less attention in the literature: how charter schools induce closure of low-performing TPS, as well as private schools.

VI.A. Charter Entry Effects on TPS Closures

We evaluate the effects of charter entry on TPS closure during the sample period using the TPS school closure measure created by Harris and Martinez-Pabon (forthcoming). A closed TPS occurs when the school building is no longer used as a school. We therefore switch the dependent variable in the above GDD analyses from test scores and graduation rates to closure.

Table 7 presents the results. We find increases in TPS closure when charters enter. The magnitudes are fairly consistent across specification. Taking the estimate in column (1), a 10 percent increase in charter enrollment share increases the school closure rate by about 0.023 percentage points (a 2 percent increase over the baseline rate). In two additional analyses, shown in Panels B and C, we also carry out the placebo version, examining "effects" of elementary charter school entry on high school closure (vice versa). The closure effects are much smaller in the placebo estimates (though sometimes still precisely estimated), reinforcing that the main estimates partially reflect causal effects.

Unlike the effects on student outcomes, the effects on school closures arise immediately; the lag effects are smaller and less precise. This may suggest that charter entry is part of district plans, especially in cases where districts are charter authorizers. They might, for example, close one TPS because they are aware of an incoming charter school entrant. (In some cases, the charter entrant might even be taking over the building being vacated by a TPS.)

Whether these induced TPS closures improve district performance depends on whether the closed schools are low-performing relative to the entering charter schools and the other TPSs to which students in closed schools may be forced to move (Bross, Harris, and Liu 2016). The SEDA school-level data provides overall performance measure and overall achievement growth in Math and ELA for grades 3-8 in the sample period. Our primary interest is in the SEDA school achievement growth measure, which is based on the difference between average scores of all students in a specific grade in a school and the average scores of students in the previous grade in the prior year. For example, the SEDA growth measure captures how much student test scores changed, on average, from 3rd grade in one year to 4th grade in the following year (i.e., cohort growth). On average, SEDA-style cohort growth measures are useful proxies for longitudinal growth measures similar to school value-added (Reardon et al. 2019).[29]

These data suggest that the closed TPSs had lower performance than non-closed TPSs. The SEDA overall growth measure of Math & ELA test scores is 0.0041 s.d. for non-closed TPSs in treated districts, but -0.0023 s.d. for closed TPSs in treated districts.[30] This suggests that charter

[29] In our own analysis of the Louisiana SEDA measures, we find a correlation of at least +0.6 between SEDA growth and conventional school value-added measures based on student-level data.
[30] The practical effects of these differences might be smaller because the low-performing teachers in closed TPS may end up moving to the remaining TPS and charter schools. However, we note: (a) prior research suggests that

entry leads lower-performing schools to close, which increases overall district student outcomes through mechanisms other than the usual participant and competitive effects.

VI.B. Effects of Charter Entry on Private School Enrollment

So far, the above analyses only look at the charter effects on student outcomes in public school systems, however, charter schools may also have some impacts on enrollment patterns in private schools, which also matter for economywide human capital and therefore for social welfare. Also, if some private school students end up in publicly funded schools as a result of the charter entry, then our estimates on district-level graduation rate and test scores (which only capture TPSs and charter schools) might be biased upwards. For example, Toma, Zimmer, and Jones (2006) found that, in Michigan, approximately 17 percent of the students who enroll in charter schools were previously enrolled in private schools. Buddin (2012) conducted a national evaluation and found similar results, especially in urban districts. However, Chakrabarti and Roy (2016) found no causal evidence that charter schools in Michigan led to declines in private school enrollment.[31]

We focus on the districts that have at least one private school at baseline.[32] The sample period is 1996-2018 and note that the private school data are only available biannually. We find no significant effects of charter entry on the share of private schools' enrollment (Table 8). A further implication is that our earlier estimates on district-level graduation rate and test scores are not biased by shifts in enrollments to or from private schools.

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teachers in closed/takeover schools are more likely than other teachers to leave the profession entirely and/or move into non-teaching positions (Lincove, Carlson, and Barrett 2019); and (b) schools and school leaders vary in their ability to convert individual teacher skill into student outcomes (Branch, Hanushek, and Rivkin 2012), so, even if teachers were all re-sorted to other schools, average school performance would still improve to some degree.

[31] They used a fixed-effects model as well as an instrumental variables strategy that exploits exogenous variation from Michigan charter law.

[32] A total of 6,449 school districts (roughly one-half of all geographic districts) have one private school during the sample period.

VII. Conclusion

This study makes many new contributions to the research on charter schools and market-based school reforms generally. First, we provide some of the first analysis of the total combined effect of charter school entry on student outcomes at a national level. Using the GDD strategy combined with various methods for addressing endogeneity, and with a sample including half of all charter schools in the country, our analysis suggests that increasing charter market share by 10 percentage points increases elementary and middle school test scores by 0.01-0.02 s.d. nationally. In the case of math, these positive effects emerge in some specifications, but are not as robust. Our placebo estimates, analyses of changing student demographics, parallel trends tests, and comparisons with prior, well-identified studies suggest that these are causal effects and are not noticeably influenced by endogenous charter location and timing.

Second, this is the first study to our knowledge to identify the mechanisms of charter entry effects—participant, near competitive, far competitive, closure, and matching effects—and to show what mechanisms are, and are not incorporated, in different types of research designs. We also find that a portion of the system effects discussed above are due to charter schools replacing low-performing TPS—the closure effect. Understanding why charter effects emerge is as important as estimating the effects themselves.

Third, we contribute to a small but growing literature to study effects on student outcomes other than test scores (Imberman 2011, Booker et al. 2011, Furgeson et al. 2012, Angrist et al. 2013, Angrist, Pathak, and Walters 2013, Dobbie and Fryer 2013, Wong et al. 2014, Booker et al. 2014). We find that increasing charter market share by 10 percentage points increases high school graduation rate by about two percentage points. This is important given how strongly predictive high school graduation is for long-term life outcomes.

Fourth, with such a large sample, we are able to provide new evidence on effect heterogeneity. In addition to reinforcing past evidence regarding the concentration of effects in urban/metropolitan areas and in middle schools, we provide some initial evidence on why this might be. Specifically, we find evidence that charter effects are larger in lower-performing districts. The effects also seem to be larger in middle schools and depend on stay policies.

Fifth, the rising number of districts, such as Detroit, New Orleans and Washington, DC, that are majority-charter also raises the question: Is there some limit regarding how much charter market share is good for students? We find that there may be such as plateau. While the results in New Orleans have been especially positive (Harris and Larsen forthcoming), with near 100 percent charter market share, this may be an outlier. We find positive effects up to 30-60 percent market share, but the effects drop noticeably after that point in all specifications.

Charter schooling has been arguably the most influential school reform efforts of the past several decades. Informing these ongoing debates, our analysis helps better understand the effects on multiple student outcomes, at national level, as well as when, how, and for whom charter school effects operate.

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Table 1 Summary statistics

	Graduation sample			Test score sample		
Districts	All	No-charters	With charters	All	No-charters	With charters
Graduation rate	0.76	0.80	0.70	N.A.	N.A.	N.A.
Math	N.A.	N.A.	N.A.	0.00	0.24	-0.24
ELA	N.A.	N.A.	N.A.	0.00	0.28	-0.28
Enrollment	60,146	12,777	137,653	57,478	10,254	104,979
White	0.63	0.74	0.46	0.54	0.68	0.39
Black	0.16	0.12	0.23	0.17	0.11	0.23
Hispanic	0.15	0.09	0.24	0.24	0.15	0.32
FRL	0.29	0.25	0.36	0.52	0.45	0.59
Special education	0.12	0.12	0.13	0.13	0.14	0.12
Urban	0.29	0.16	0.52	0.29	0.12	0.46
Suburb	0.40	0.40	0.39	0.42	0.41	0.42
Town	0.12	0.17	0.05	0.11	0.17	0.05
Rural	0.19	0.27	0.05	0.18	0.29	0.07
Revenue per student	9,105	9,148	9,034	12,578	13,085	12,067
Expenditure per student	9,271	9,279	9,257	12,574	13,027	12,118
Student-teacher ratio	16.77	16.16	17.78	16.66	15.95	17.38
Teacher salary	80,168	78,054	83,629	101,674	101,698	101,650
No. magnet school	1.14	0.14	2.79	4.64	0.62	8.69
Observation	152,639	137,751	14,888	525,816	447,831	77,985
N (district)	10,658	9,585	1,073	11,399	9,802	1,597

Notes: This table presents weighted means of outcome variables (graduation rate, math, and ELA) and control variables. graduation rate sample is weighted by the first-period enrollment. Data source: National Longitudinal School Database.

Table 2 Effects of charter entry on student outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Graduation rate						
	Same year share			Lag one year share		
Charter share	0.087	0.091	0.093	0.172***	0.173***	0.177***
	[0.070]	[0.070]	[0.072]	[0.033]	[0.032]	[0.033]
R-squared	0.827	0.828	0.828	0.828	0.829	0.829
Observations	152,609	152,609	152,609	152,609	152,609	152,609
N(district)	10,658	10,658	10,658	10,658	10,658	10,658
Panel B: Math						
	Same year share			Same cohort last year		
Charter share	-0.059	-0.123	-0.076	0.025	-0.019	0.021
	[0.088]	[0.090]	[0.072]	[0.060]	[0.058]	[0.049]
R-squared	0.861	0.861	0.862	0.861	0.861	0.862
Observations	525,502	525,502	525,502	525,502	525,502	525,502
N(district)	11,399	11,399	11,399	11,399	11,399	11,399
Panel C: ELA						
	Same year share			Same cohort last year		
Charter share	0.138*	0.057	0.092	0.113*	0.055	0.089*
	[0.070]	[0.066]	[0.064]	[0.060]	[0.053]	[0.051]
R-squared	0.890	0.891	0.891	0.890	0.891	0.891
Observations	525,502	525,502	525,502	525,502	525,502	525,502
N(district)	11,399	11,399	11,399	11,399	11,399	11,399
District FE, State-by-year(-by-grade) FE	Yes	Yes	Yes	Yes	Yes	Yes
Student control	No	Yes	Yes	No	Yes	Yes
District control	No	No	Yes	No	No	Yes

Notes: The table shows estimates of the effects of charter entry on student outcomes. Charter share in the data set range from 0 to 1. In columns (1), (2), and (3), charter enrollment share of grades 9-12 is used for graduation rate, and charter enrollment share of each grades is used for test scores. In columns (4), (5), and (6), the lag-one year charter enrollment share of grade 9-12 is used for graduation rate, and the last-year same cohort grade enrollment share is used for test scores. Student control includes log of enrollment, share of black, white, Hispanic, FRL, special education. District control includes revenue per student, expenditure per student, student-teacher ratio, teacher salary, number of magnet schools, and an indicator of charter law effectiveness. Regressions are weighted by the first-period enrollment. Standard errors are clustered at the district level.

Table 3 Placebo tests using charter shares of misaligned grades or subsequent years

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
		Graduation			Math			ELA	
Charter	0.022***	0.021***	0.021***	-0.012	-0.013	-0.011	-0.003	-0.004	-0.002
	[0.005]	[0.005]	[0.005]	[0.017]	[0.017]	[0.016]	[0.015]	[0.015]	[0.015]
R-squared	0.828	0.829	0.829	0.861	0.861	0.862	0.890	0.891	0.891
Observations	152,609	152,609	152,609	525,502	525,502	525,502	525,502	525,502	525,502
N(district)	10,658	10,658	10,658	11,399	11,399	11,399	11,399	11,399	11,399
District FE, State-by-year(-by-grade) FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Student control	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
District control	No	No	Yes	No	No	Yes	No	No	Yes

Notes: The table shows three placebo tests using misaligned grade levels of charter share: grade levels (1-5) for graduation rate, charter shares of grade 9, 10, and 11 for Math and ELA of grade 3, 4, and 5; charter shares of grade 1, 2, and 3 for Math and ELA of grade 6, 7, and 8. See notes of Table 2 for controls, clusters, and sample weight.

Table 4 Effects of charter share on cohort size and FRL students

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Graduation sample				Test score sample			
	Same year share		Lag one year share		Same year share		Same cohort last year	
Panel A log of cohort size								
Charter share	-0.030	0.011	-0.110	-0.048	-0.034	-0.046	-0.022	-0.040
	[0.122]	[0.094]	[0.158]	[0.124]	[0.081]	[0.077]	[0.061]	[0.057]
R-squared	0.995	0.996	0.995	0.996	0.994	0.994	0.994	0.994
Observations	152,609	152,609	152,609	152,609	525,502	525,502	525,502	525,502
N(district)	10,658	10,658	10,658	10,658	11,399	11,399	11,399	11,399
Panel B FRL Students								
Charter share	0.011	-0.025	0.034	0.015	0.002	0.012	0.007	0.014
	[0.052]	[0.049]	[0.037]	[0.038]	[0.018]	[0.015]	[0.016]	[0.013]
R-squared	0.924	0.930	0.924	0.930	0.960	0.961	0.960	0.961
Observations	152,609	152,609	152,609	152,609	525,777	525,777	525,777	525,777
N(district)	10,658	10,658	10,658	10,658	11,399	11,399	11,399	11,399
District FE, State-by-year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District control	No	Yes	No	Yes	No	Yes	No	Yes

Notes: The table shows estimates of the effects of charter share on student enrollment and FRL. Enrollment is defined as enrollment per 1,000 students, and the FRL is the proportion of students in free lunch or reduced lunch program. See notes of Table 2 for controls, clusters, and sample weight.

Table 5 Effect heterogeneity

	(1)	(2)	(3)	(4)	(5)	(6)
Group	MA	Black	Hispanic	FRL	Performance	Policy
Panel A: Graduation rate						
Charter share* group	-0.004	0.232	0.377	0.197	-0.821	-0.501***
	[0.095]	[0.167]	[0.235]	[0.194]	[0.591]	[0.136]
Charter share	0.092**	0.016	0.041	0.019	0.606*	0.214***
	[0.040]	[0.118]	[0.067]	[0.071]	[0.336]	[0.074]
R-squared	0.823	0.824	0.824	0.824	0.824	0.826
Observations	152,152	152,152	152,152	152,152	152,152	149,288
N(district)	10,626	10,626	10,626	10,626	10,283	10,283
Panel B: Math						
Charter share* group	0.424**	-0.302	0.525	-0.235	1.065	-0.703**
	[0.178]	[0.253]	[0.360]	[0.253]	[1.088]	[0.337]
Charter share	-0.226	0.146	-0.011	0.194	-0.57	0.336**
	[0.150]	[0.099]	[0.100]	[0.132]	[0.662]	[0.157]
R-squared	0.832	0.832	0.832	0.832	0.832	0.834
Observations	471,144	471,144	471,144	471,144	471,144	454,920
N(district)	10,286	10,286	10,286	10,286	9,586	9,586
Panel C: ELA						
Charter share* group	0.407**	-0.418*	0.629**	-0.259	1.123	-0.556**
	[0.179]	[0.237]	[0.314]	[0.193]	[0.859]	[0.276]
Charter share	-0.245	0.137	-0.062	0.173	-0.637	0.249*
	[0.153]	[0.098]	[0.092]	[0.111]	[0.523]	[0.145]
R-squared	0.865	0.865	0.865	0.865	0.865	0.866
Observations	471,144	471,144	471,144	471,144	471,144	454,920
N(district)	10,286	10,286	10,286	10,286	9,586	9,586
District FE, State-by-year(-by-grade) FE	Yes	Yes	Yes	Yes	Yes	Yes
Student control	Yes	Yes	Yes	Yes	Yes	Yes
District control	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table shows estimates of heterogeneous effects of the charter share on student outcome by groups. The results are based on the contemporaneous, which is similar to column (3) in main result. *MA* is an indicator for metropolitan areas. *Black, Hispanic, and FRL* are continuous and time-constant (with value equals first year in the sample, therefore, the always treated districts was removed from analyses), which ranges from 0 to 1. *Performance* is the variable of baseline performance, it ranges from 0 to 1 by decile, and 0 refers to the least 10 percentile base performance, 0.1 refers the 10-20 percentile base performance, and so force. *Policy* is a variable from based on policy scores reported by the NAPCS in 2020. Higher values indicate a better policy as determined by the rating organization. All scores are divided by the highest score and therefore, the highest value of *Policy* is one and the lowest value of *Policy* is zero. Districts with missing baseline performance was not included in analyses. See notes of Table 2 for controls, clusters, and sample weight.

Table 6 Effect heterogeneity: elementary (middle) schools

	(1)	(2)	(3)	(4)	(5)	(6)
	Same year share			Same cohort last year		
	Panel A: Math Grade 3-5					
Charter share	0.014	-0.067	0.258**	0.014	-0.03	0.025
	[0.122]	[0.116]	[0.124]	[0.085]	[0.080]	[0.067]
R-squared	0.876	0.877	0.862	0.876	0.877	0.877
Observations	278,528	278,528	278,557	278,528	278,528	278,528
N(district)	11,257	11,257	11,257	11,257	11,257	11,257
	Panel B: ELA Grade 3-5					
Charter share	0.113	0.020	0.077	0.058	0.006	0.050
	[0.109]	[0.093]	[0.092]	[0.079]	[0.069]	[0.068]
R-squared	0.908	0.909	0.909	0.908	0.909	0.909
Observations	278,528	278,528	278,528	278,528	278,528	278,528
N(district)	11,257	11,257	11,257	11,257	11,257	11,257
	Panel C: Math Grade 6-8					
Charter share	0.111	0.024	0.044	0.157**	0.097	0.115*
	[0.093]	[0.086]	[0.084]	[0.070]	[0.065]	[0.064]
R-squared	0.892	0.893	0.893	0.892	0.893	0.893
Observations	246,647	246,647	246,647	246,647	246,647	246,647
N(district)	11,113	11,113	11,113	11,113	11,113	11,113
	Panel D: ELA Grade 6-8					
Charter share	0.432***	0.321***	0.323***	0.315***	0.236***	0.239***
	[0.097]	[0.087]	[0.082]	[0.074]	[0.066]	[0.060]
R-squared	0.905	0.906	0.906	0.905	0.906	0.906
Observations	246,647	246,647	246,647	246,647	246,647	246,647
N(district)	11,113	11,113	11,113	11,113	11,113	11,113
District FE, State -by-year-by-grade FE	Yes	Yes	Yes	Yes	Yes	Yes
Student control	No	Yes	Yes	No	Yes	Yes
District control	No	Yes	Yes	No	Yes	Yes

Notes: The table shows estimates of heterogeneous effects of charter entry on student test scores by middle (elementary) schools. See notes of Table 2 for controls, clusters, and sample weight.

Table 7 Effects of charter share on TPS closure

	(1)	(2)	(3)	(4)	(5)	(6)
	Same year share			Lag one year share		
Panel A: district charter share on all TPS closure (main effect)						
Charter share	0.023*	0.022*	0.018	-0.005	-0.004	-0.010
	[0.012]	[0.013]	[0.012]	[0.007]	[0.008]	[0.010]
R-squared	0.134	0.136	0.138	0.133	0.136	0.138
Observations	279,652	279,652	279,652	279,652	279,652	279,652
N(district)	12,435	12,435	12,435	12,435	12,435	12,435
Panel B: high school charter share on elementary TPS closure (placebo)						
Charter share	0.012*	0.012	0.009	-0.005	-0.005	-0.009
	[0.007]	[0.007]	[0.007]	[0.005]	[0.006]	[0.007]
R-squared	0.084	0.086	0.088	0.084	0.086	0.088
Observations	279,652	279,652	279,652	279,652	279,652	279,652
N(district)	12,435	12,435	12,435	12,435	12,435	12,435
Panel C: elementary charter share on high school TPS closure (placebo)						
Charter share	0.007**	0.007**	0.005*	0.000	0.000	-0.002
	[0.003]	[0.003]	[0.003]	[0.002]	[0.002]	[0.002]
R-squared	0.100	0.101	0.102	0.100	0.101	0.101
Observations	279,652	279,652	279,652	279,652	279,652	279,652
N(district)	12,435	12,435	12,435	12,435	12,435	12,435
District FE, State-by-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Student control	No	Yes	Yes	No	Yes	Yes
District control	No	No	Yes	No	No	Yes

Notes: This table presents estimates of the effects of the charter share on the share of TPS closure. Panel B presents results using elementary TPS closure to regress high school charter share; and Panel C presents results using high school TPS closure to regress elementary school charter share. The sample period is from the year 1995 to 2018. Regressions are weighted by the total number of public schools. See notes of Table 2 for controls, and clusters.

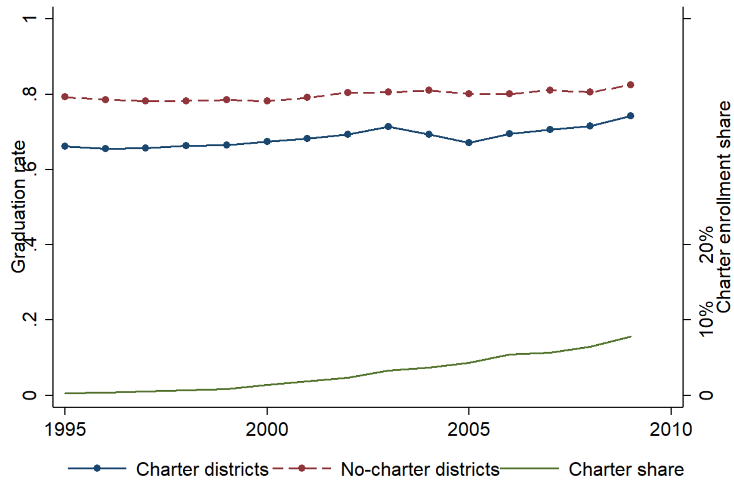
Table 8: Effects of charter share on private school enrollment share

	(1)	(2)	(3)	(4)	(5)	(6)
	Same year share			Lag one year share		
Panel A: district charter share on private school enrollment share						
Charter share	0.032	0.020	0.014	0.049	0.037	0.034
	[0.036]	[0.031]	[0.033]	[0.045]	[0.039]	[0.043]
R-squared	0.898	0.902	0.902	0.899	0.902	0.903
Observations	65,255	65,255	65,255	65,255	65,255	65,255
N(district)	6,449	6,449	6,449	6,449	6,449	6,449
Panel B: high school charter share on elementary private school enrollment (placebo)						
Charter share	0.119	0.083	0.067	0.142	0.108	0.097
	[0.103]	[0.085]	[0.087]	[0.127]	[0.112]	[0.118]
R-squared	0.886	0.889	0.89	0.887	0.889	0.89
Observations	65,255	65,255	65,255	65,255	65,255	65,255
N(district)	6,449	6,449	6,449	6,449	6,449	6,449
Panel C: elementary charter share on high school private school enrollment (placebo)						
Charter share	0.067	0.054	0.05	0.067	0.056	0.053
	[0.054]	[0.045]	[0.047]	[0.059]	[0.053]	[0.057]
R-squared	0.875	0.877	0.877	0.876	0.877	0.877
Observations	65,255	65,255	65,255	65,255	65,255	65,255
N(district)	6,449	6,449	6,449	6,449	6,449	6,449
District FE, State-by-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Student control	No	Yes	Yes	No	Yes	Yes
District control	No	No	Yes	No	No	Yes

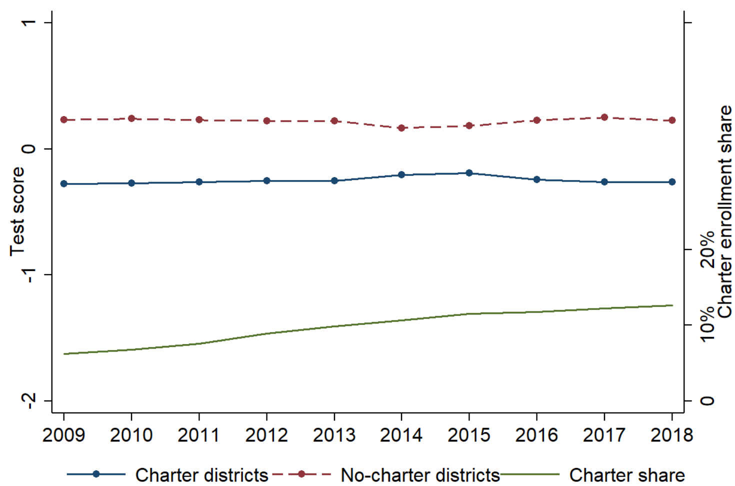
Notes: This table presents estimates of the effects of the charter share on the share of private school enrollment. The sample period is from the year 1996 to 2018 biannually. Regressions are weighted by the district total enrollment. See notes of Table 2 for controls and clusters.

Figure 1 Trends in graduation rate, Math, and ELA performance

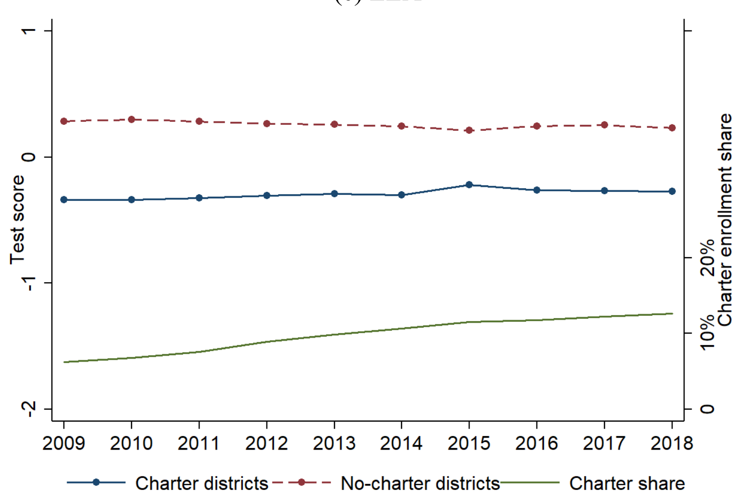
(a) Graduation rate



(b) Math



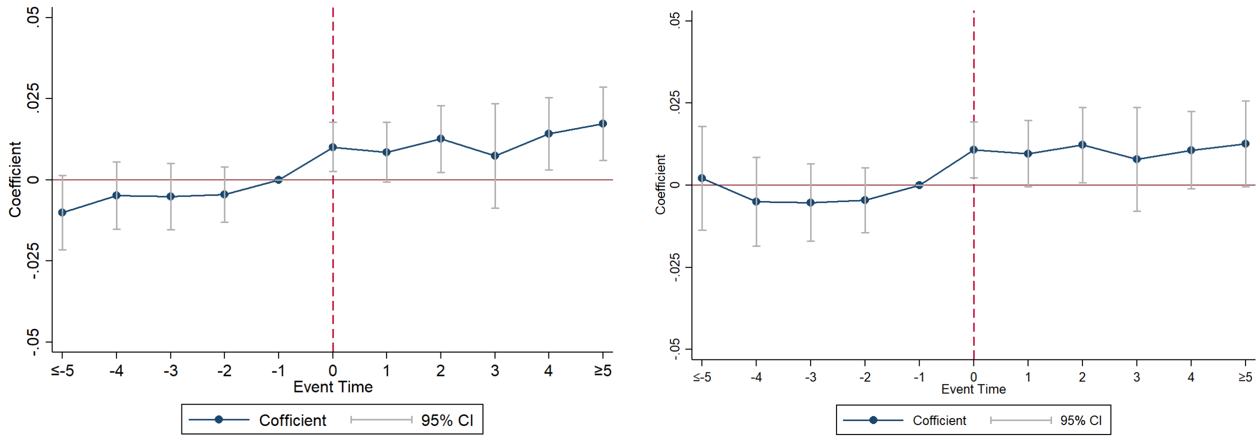
(c) ELA



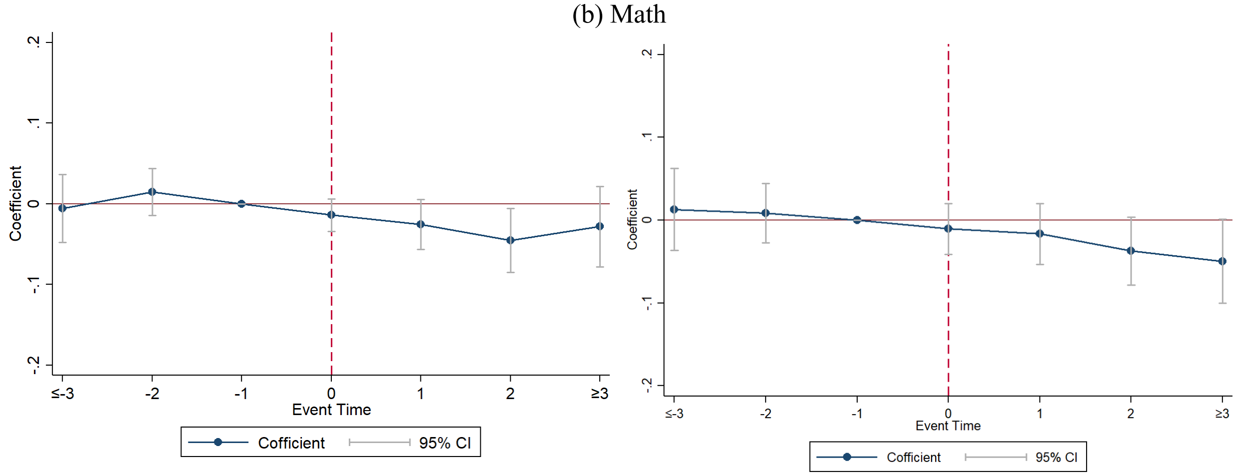
Notes: This figure plots the trends in Graduation rate, Math & ELA of charter districts (in the solid line), and no-districts (in the dashed line). The green solid line plots the charter enrollment share of treated districts.

Figure 2a Event Study (simple and IW) Tests for Parallel Trends

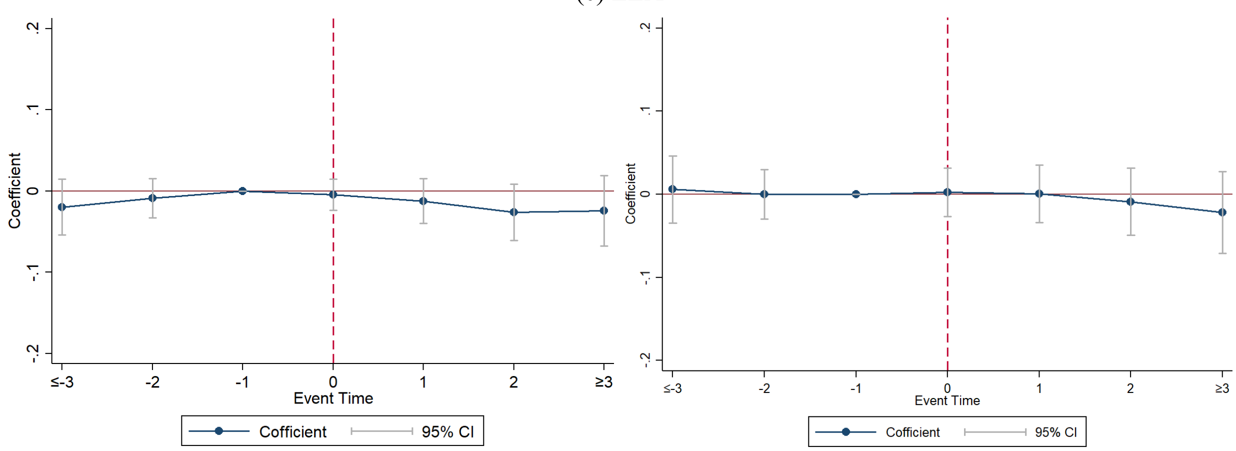
(a) Graduation rate



(b) Math



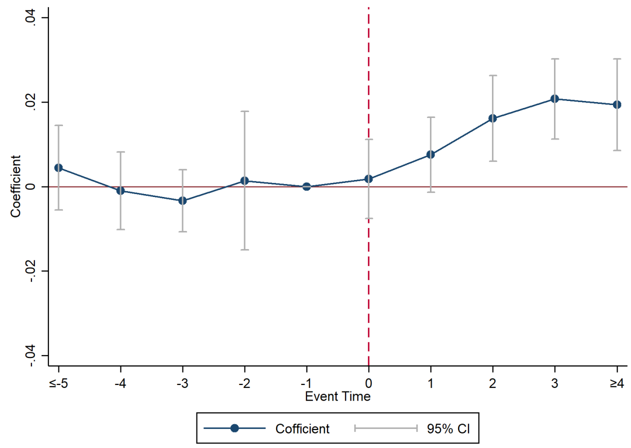
(c) ELA



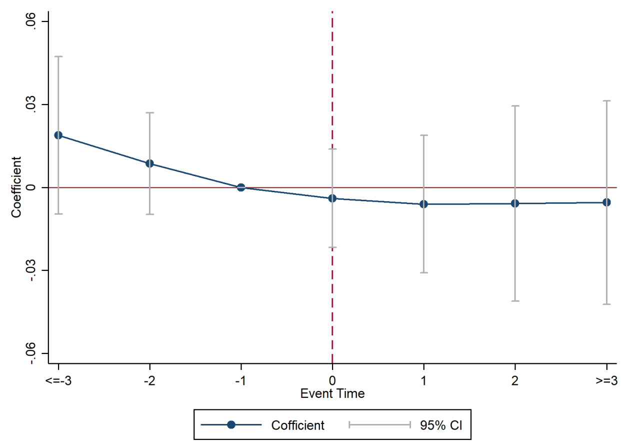
Notes: This figure presents the results of event study (left) and IW estimators (right) for graduation rate, Math, and ELA. The event time zero is the first year of charter entry.

Figure 2b Event Study (IW) – Treated Districts limited to Positive Shocks in Charter Share

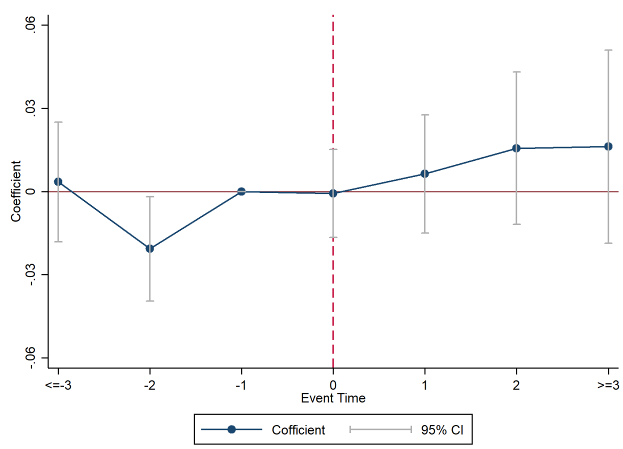
(a) Graduation rate



(b) Math



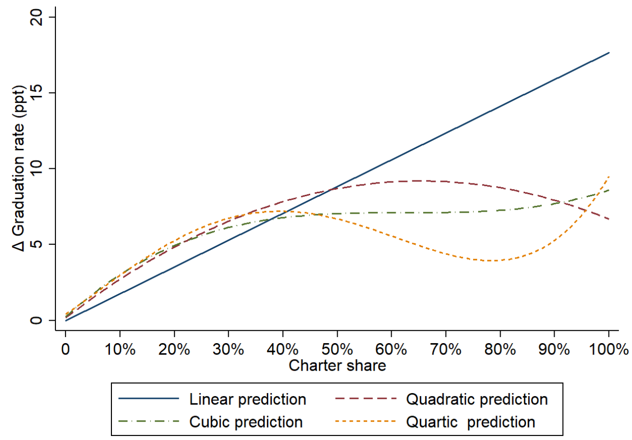
(c) ELA



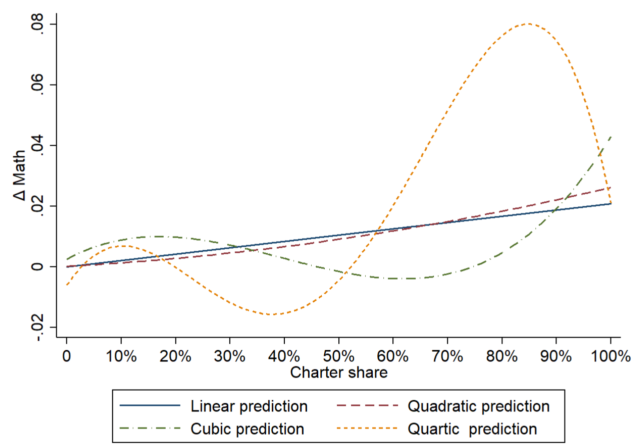
Notes: See notes to Figure 2a. The key difference is that, in Figure 2b, treatment is begins when districts experience a spike of greater than two percentage points in charter market share. The comparison group continues to be never-charter districts.

Figure 3a Non-linear estimates of charter share on student outcomes

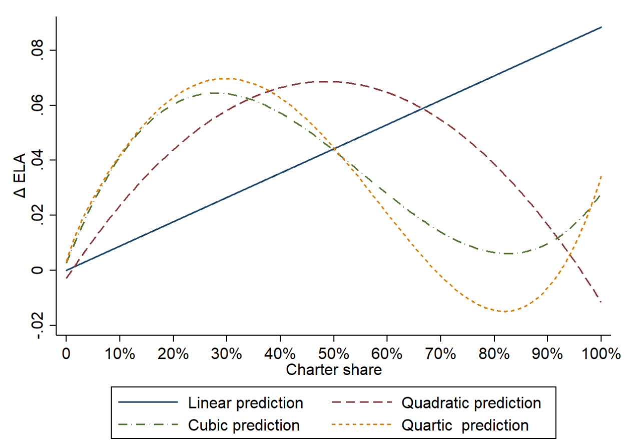
(a) Graduation rate



(b) Math



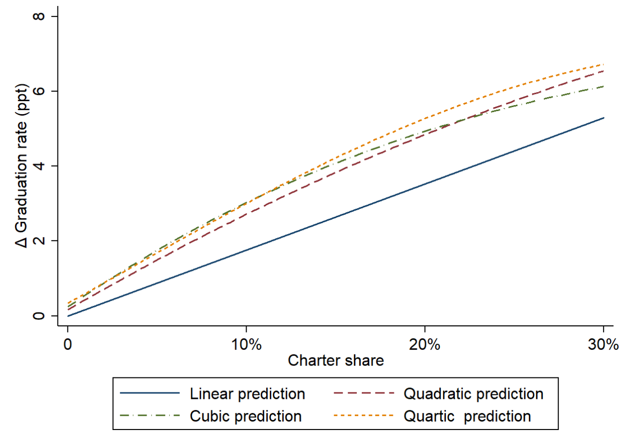
(c) ELA



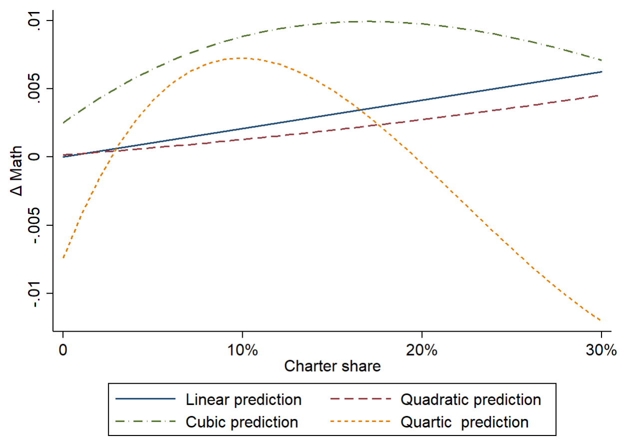
Note: This figure presents the linear and non-linear estimates of charter share on student outcomes using the same specification as equations (1) and (2), but the $f(Charter_{it})$ shown in the legend. This includes the entire sample of charter schools.

Figure 3b Non-linear estimates of charter share on student outcomes (0-30% market share)

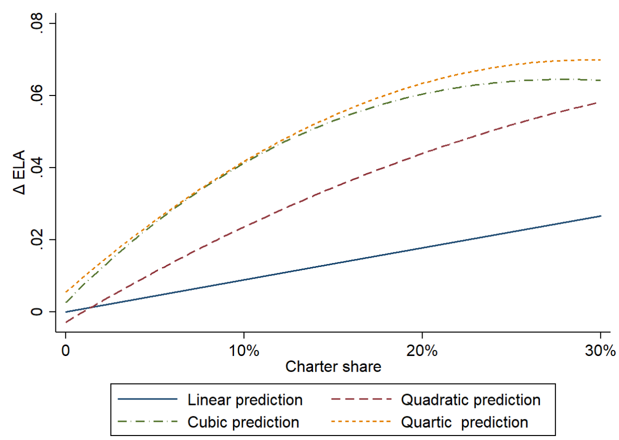
(a) Graduation rate



(b) Math



(c) ELA

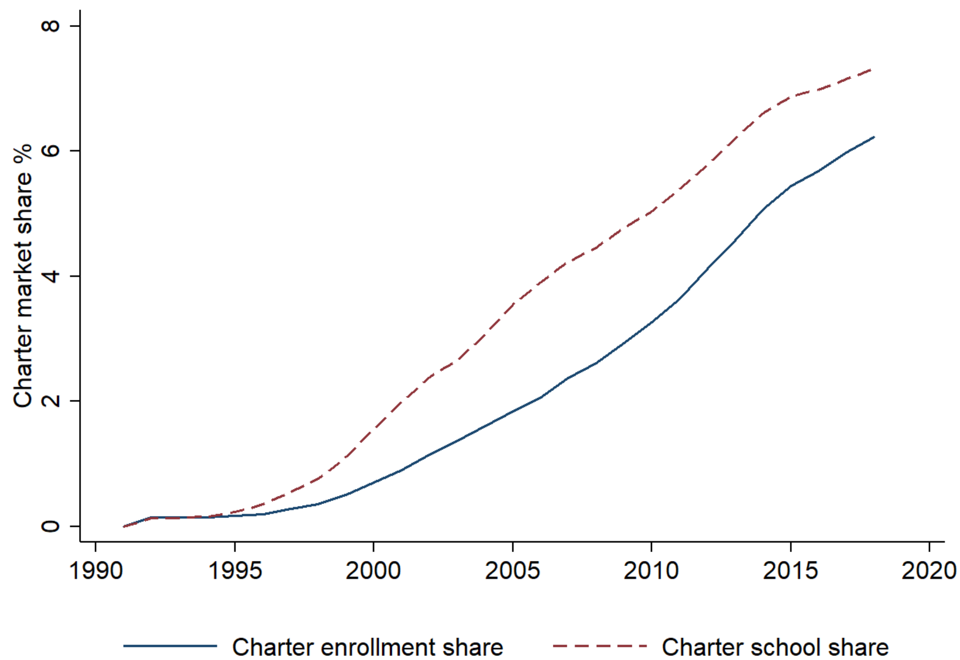


Note: See notes to Figure 3a. In this case, the same was restricted to the 0-30% charter market share, which is the range for the vast majority of districts.

The Total Effects of Charter Schools (Chen and Harris)

Online Appendix A

Figure A1 Trends in charter school share and charter enrollment share



Notes: This figure plots the trends in charter enrollment share (in the solid line) and charter school share (in the dashed line).

Table A1 Year charter law passed by state

Year	State
1991	Minnesota
1992	California
1993	Colorado, Massachusetts, Michigan, New Mexico, Wisconsin
1994	Arizona, Georgia, Hawaii, Kansas
1995	Alaska, Arkansas, Delaware, Louisiana, New Jersey, Rhode Island, Texas, Wyoming
1996	Connecticut, District of Columbia, Florida, Idaho, Illinois, New Hampshire, North Carolina, South Carolina
1997	Nevada, Ohio, Pennsylvania
1998	Missouri, New York, Utah, Virginia
1999	Oklahoma, Oregon
2001	Indiana
2002	Iowa, Tennessee
2003	Maryland
2010	Mississippi
2011	Maine
2015	Alabama
2016	Washington
2017	Kentucky
2019	West Virginia
NA	Montana, Nebraska, North Dakota, South Dakota, Vermont

Data source: National Longitudinal School Database.

Table A2 Lists of top 20 large districts with high charter share

Graduation rate sample (1995-2009)				Test score sample (2009-2018)			
State	District	Max district enrollment	Max high school charter share	State	District	Max district enrollment	Max grade charter share
Louisiana	Orleans Parish School District	86,028	66.71%	Louisiana	Orleans Parish School District	51,786	96.21%
Ohio	Columbus City School District	74,405	38.82%	Texas	San Antonio Independent School District	61,136	60.36%
District Of Columbia	District of Columbia Public Schools	80,450	33.07%	District Of Columbia	District of Columbia Public Schools	91,474	55.44%
Colorado	Denver County School District	80,264	22.18%	Michigan	Detroit City School District	125,455	50.70%
Arizona	Tucson Unified District	71,194	22.18%	California	Oakland Unified School District	50,231	40.62%
Wisconsin	Milwaukee School District	101,253	22.06%	New Jersey	Newark City School District	60,419	40.05%
California	San Juan Unified School District	52,212	20.51%	Pennsylvania	Philadelphia City School District	206,555	39.77%
Michigan	Detroit City School District	192,639	20.43%	Ohio	Cleveland Municipal School District	60,849	38.69%
Pennsylvania	Philadelphia City School District	213,045	20.13%	Massachusetts	Boston School District	66,795	35.90%
California	Oakland Unified School District	55,051	19.52%	Colorado	Denver County School District	109,766	35.75%
California	Sacramento City Unified School District	54,620	19.50%	Minnesota	St. Paul Public School District	52,137	34.71%
Minnesota	Minneapolis Public School District	50,416	19.15%	South Carolina	Richland School District	50,747	31.72%
Arizona	Mesa Unified District	106,563	18.81%	Ohio	Columbus City School District	84,863	31.67%
New Mexico	Albuquerque Public Schools	97,040	16.77%	Georgia	Clayton County School District	69,763	31.24%
Ohio	Cincinnati City School District	52,852	16.60%	Oklahoma	Oklahoma City Public Schools	59,761	30.76%
Texas	Lewisville Independent School District	54,050	15.77%	California	Los Angeles Unified School District	775,364	30.67%
Texas	Dallas Independent School District	176,148	14.81%	Tennessee	Metropolitan Nashville Public School District	97,303	29.42%
Ohio	Cleveland Municipal School District	78,512	14.24%	Maryland	Baltimore City Public Schools	85,382	27.51%
Texas	Houston Independent School District	235,403	13.65%	Texas	Houston Independent School District	258,315	27.35%
California	San Diego City Unified School District	144,924	13.50%	Tennessee	Shelby County School District	159,540	27.09%

Source: National Longitudinal School Database.

Table A3 Effects of charter share on student outcomes (cluster at the state level)

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Graduation rate						
	Same year share			Lag one year share		
Charter share	0.087	0.091	0.093	0.172***	0.173***	0.177***
	[0.062]	[0.061]	[0.065]	[0.033]	[0.032]	[0.033]
R-squared	0.827	0.828	0.828	0.828	0.829	0.829
Observations	152,609	152,609	152,609	152,609	152,609	152,609
N(district)	10,658	10,658	10,658	10,658	10,658	10,658
Panel B: Math						
	Same year share			Same cohort last year		
Charter share	-0.059	-0.123	-0.076	0.025	-0.019	0.021
	[0.116]	[0.110]	[0.096]	[0.076]	[0.067]	[0.062]
R-squared	0.861	0.861	0.862	0.861	0.861	0.862
Observations	525,502	525,502	525,502	525,502	525,502	525,502
N(district)	11,399	11,399	11,399	11,399	11,399	11,399
Panel C: ELA						
	Same year share			Same cohort last year		
Charter share	0.138	0.057	0.092	0.113	0.113	0.055
	[0.107]	[0.102]	[0.097]	[0.082]	[0.082]	[0.071]
R-squared	0.890	0.891	0.891	0.890	0.890	0.891
Observations	525,502	525,502	525,502	525,502	525,502	525,502
N(district)	11,399	11,399	11,399	11,399	11,399	11,399
District FE, State-by-year (-by-grade) FE	Yes	Yes	Yes	Yes	Yes	Yes
Student control	No	Yes	Yes	No	Yes	Yes
District control	No	Yes	Yes	No	Yes	Yes

Notes: The table replicates Table 2 but clusters standard errors at the state level.

Table A4 Alternative measure of charter share: charter *school* share

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Graduation rate						
	Same year share			Lag one year share		
Charter share	0.047***	0.048***	0.050***	0.046***	0.046***	0.047***
	[0.013]	[0.013]	[0.013]	[0.011]	[0.011]	[0.011]
R-squared	0.827	0.828	0.829	0.827	0.828	0.829
Observations	152,609	152,609	152,609	152,609	152,609	152,609
N(district)	10,658	10,658	10,658	10,658	10,658	10,658
Panel B: Math						
	Same year share			Same cohort last year		
Charter share	0.038	-0.047	0.026	0.057	-0.006	0.054
	[0.096]	[0.095]	[0.080]	[0.080]	[0.078]	[0.070]
R-squared	0.861	0.861	0.862	0.861	0.861	0.862
Observations	525,502	525,502	525,502	525,392	525,392	525,392
N(district)	11,399	11,399	11,399	11,399	11,399	11,399
Panel C: ELA						
	Same year share			Same cohort last year		
Charter share	0.191*	0.087	0.137	0.137	0.056	0.105
	[0.107]	[0.098]	[0.090]	[0.101]	[0.094]	[0.086]
R-squared	0.890	0.891	0.891	0.890	0.891	0.891
Observations	525,502	525,502	525,502	525,392	525,392	525,392
N(district)	11,399	11,399	11,399	11,399	11,399	11,399
District FE, State-by-year (-by-grade) FE	Yes	Yes	Yes	Yes	Yes	Yes
Student control	No	Yes	Yes	No	Yes	Yes
District control	No	Yes	Yes	No	Yes	Yes

Notes: The table shows estimates of the effects of the charter school share on student outcomes. See notes of Table 2 for controls, clusters, and sample weight.

Table A5 Effects of charter share on student outcomes
(include district time trends)

	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Graduation rate					
	Same year share			Lag one year share		
Charter share	-0.003	-0.002	-0.007	0.124*	0.122*	0.115*
	[0.126]	[0.127]	[0.128]	[0.070]	[0.067]	[0.064]
R-squared	0.860	0.861	0.861	0.861	0.861	0.861
Observations	152,609	152,609	152,609	152,609	152,609	152,609
N(district)	10,658	10,658	10,658	10,658	10,658	10,658
	Panel B: Math					
	Same year share			Same cohort last year		
Charter share	-0.100	-0.106	-0.103	0.041	0.040	0.042
	[0.098]	[0.098]	[0.096]	[0.052]	[0.052]	[0.051]
R-squared	0.885	0.885	0.885	0.885	0.885	0.885
Observations	525,502	525,502	525,502	525,502	525,502	525,502
N(district)	11,399	11,399	11,399	11,399	11,399	11,399
	Panel C: ELA					
	Same year share			Same cohort last year		
Charter share	-0.017	-0.025	-0.023	0.030	0.028	0.030
	[0.088]	[0.088]	[0.088]	[0.045]	[0.044]	[0.044]
R-squared	0.909	0.909	0.909	0.909	0.909	0.909
Observations	525,502	525,502	525,502	525,502	525,502	525,502
N(district)	11,399	11,399	11,399	11,399	11,399	11,399
District FE, State-by-year(-by-grade) FE & District time trend	Yes	Yes	Yes	Yes	Yes	Yes
Student control	No	Yes	Yes	No	Yes	Yes
District control	No	Yes	Yes	No	Yes	Yes

Notes: The table replicates Table 2 but includes district-specific linear time trends.

Table A6 Effects of charter share on student outcomes
(includes district time trends and drop always treated districts)

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Graduation rate						
	Same year share			Lag one year share		
Charter share	-0.015	-0.014	-0.019	0.123	0.120	0.112
	[0.134]	[0.136]	[0.137]	[0.085]	[0.081]	[0.078]
R-squared	0.856	0.856	0.857	0.856	0.857	0.857
Observations	152,152	152,152	152,152	152,152	152,152	152,152
N(district)	10,626	10,626	10,626	10,626	10,626	10,626
Panel B: Math						
	Same year share			Same cohort last year		
Charter share	0.030	0.022	0.023	0.141**	0.133**	0.138**
	[0.080]	[0.079]	[0.080]	[0.067]	[0.067]	[0.067]
R-squared	0.858	0.858	0.858	0.858	0.858	0.858
Observations	471,144	471,144	471,144	471,144	471,144	471,144
N(district)	10,286	10,286	10,286	10,286	10,286	10,286
Panel C: ELA						
	Same year share			Same cohort last year		
Charter share	0.038	0.026	0.028	0.105	0.094	0.099
	[0.071]	[0.072]	[0.072]	[0.072]	[0.072]	[0.073]
R-squared	0.884	0.885	0.885	0.884	0.885	0.885
Observations	471,144	471,144	471,144	471,144	471,144	471,144
N(district)	10,286	10,286	10,286	10,286	10,286	10,286
District FE, State-by-year(-by-grade) FE & District time trend	Yes	Yes	Yes	Yes	Yes	Yes
Student control	No	Yes	Yes	No	Yes	Yes
District control	No	Yes	Yes	No	Yes	Yes

Notes: The table replicates Table A5 but drops always treated districts.

Table A7 Oster bound analysis

Outcome	Charter share	Estimate	R2	Oster bound (R2=0.95)	Oster bound (R2=1)
Graduation rate	Same year share	0.202	0.801	0.293	0.324
Graduation rate	Lag one year share	0.178	0.801	0.259	0.286
Math	Same year share	0.091	0.848	0.473	0.661
Math	Same cohort last year	0.161	0.848	0.524	0.701
ELA	Same year share	0.156	0.883	0.402	0.585
ELA	Same cohort last year	0.166	0.883	0.392	0.560

Note: This table reports point estimates, the R-squares and Oster bounds of our main estimates. All Oster bounds are calculated based on the comparison with estimates that do not include controls and fixed effects. We follow Oster (2019) to assume $\tilde{\delta} = 1$ and $Rmax$=1.3$R0$, however, in our case, 1.3$R0$ for all estimates are greater than 1, so, we instead, calculates Oster bounds for $Rmax$=0.95 and $Rmax$=1, respectively. R2 refers to the R-squared of the specification with full control variables and fixed effects.

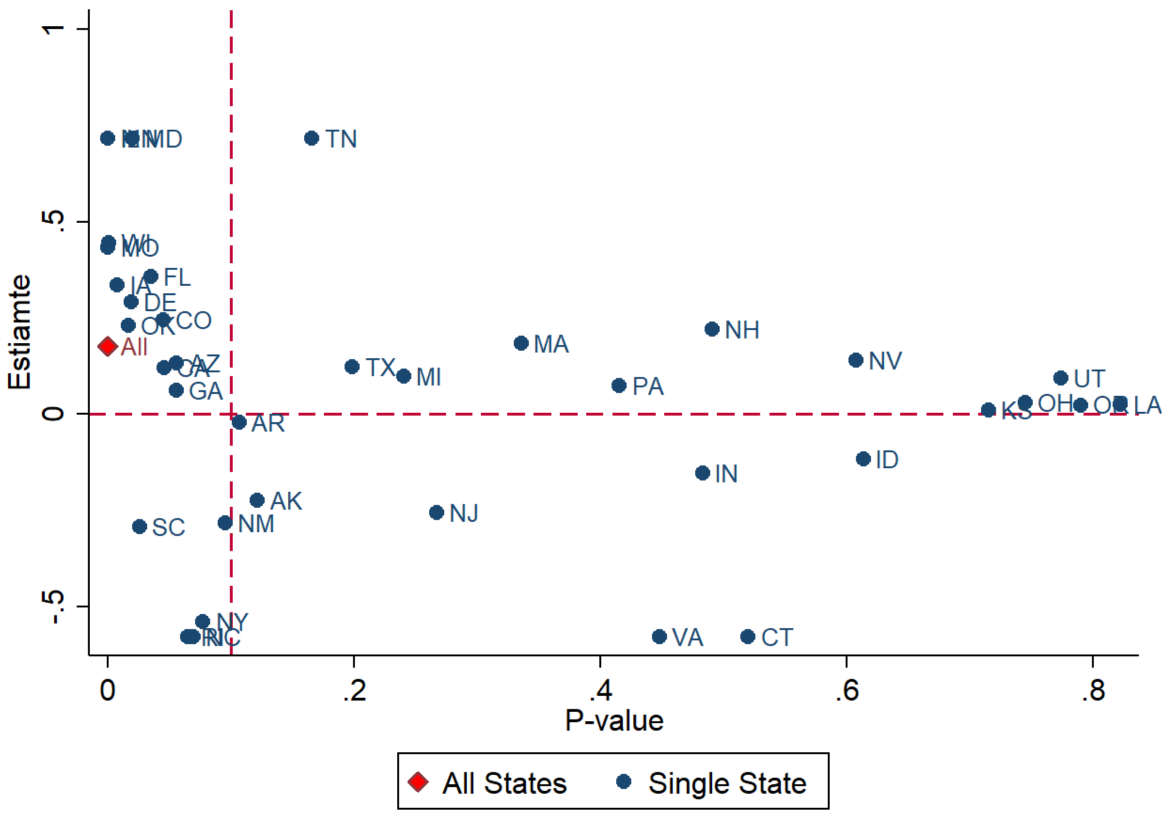
Appendix B Sample Description

This section describes how we create our samples. For the graduation sample, we start from a sample with all public schools that have enrollments in high school grades (9-12). This sample contains 35,258 public schools (including 3,059 charter schools). Then, we merge the school-level enrollment data (used to calculate charter share) to geographic school district graduation data, and schools in districts with missing graduation information are dropped. The remained sample contains 35,093 public schools (including 3,046 charter schools). To make all specifications of our main results have the sample, we removed a few districts with missing information of lag one-year charter or those omitted by the including districts fixed Effects regressions due to limited waves of data. The final sample contains 34,929 public schools (including 3,023 charter schools).

For the test score sample, we start from a sample with all public schools that have enrollments in grades 3-8, which has 85,359 public schools (including 7,385 charter schools). Then, we merge the school-level enrollment data (used to calculate charter share) to geographic school district test score data, and schools in districts with missing test scores are dropped. The remained sample contains 81,695 public schools (including 6,964 charter schools). To make all specifications of our main results have the sample, we removed a few districts with missing information of lag one-year charter or those omitted by the including districts fixed Effects regressions due to limited waves of data. The final sample contains 80,859 public schools (including 7,247 charter schools).

Appendix C State by State Results

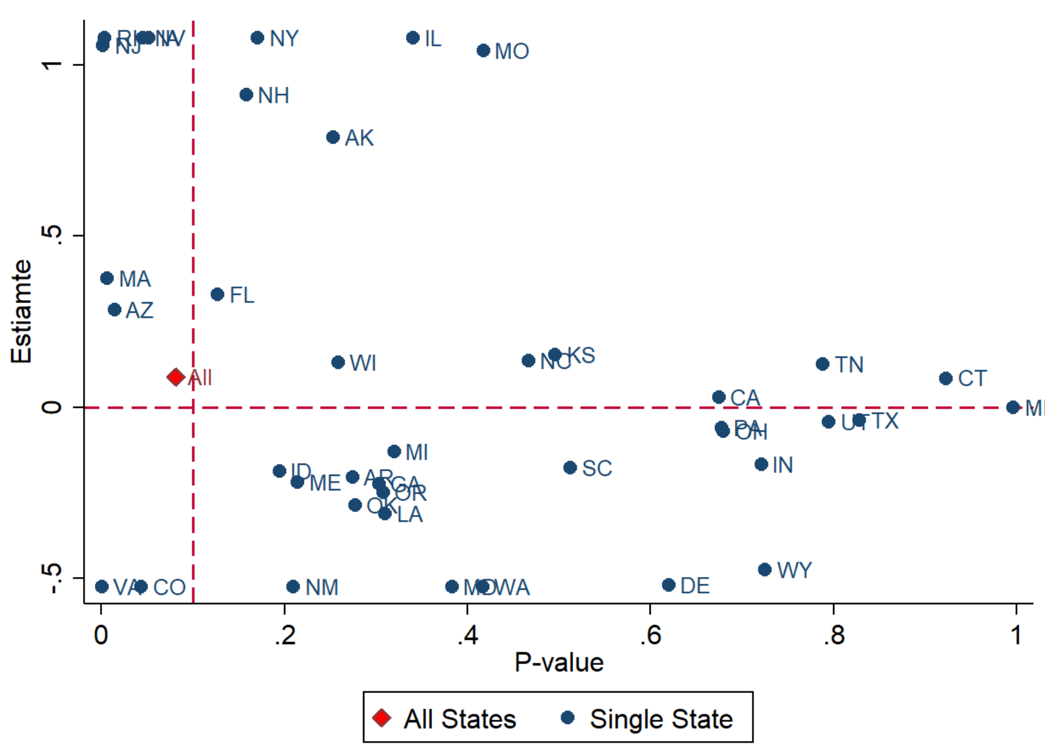
(a) Graduation rate



(b) Math

Estiamte
P-value
1.5
1
.5
0
-.5
-1
0
.2
.4
.6
.8
1
NJ
RI
VA
IA
NY
MO
CT
MA
TN
WI
AZ
All
AK
AR
NC
ID
KS
GA
LA
PA
OR
FL
WY
DE
All States
Single State

(c) ELA



Estiamte
P-value
1
.5
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0
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.4
.6
.8
1
NY
IL
MO
NH
AK
MA
AZ
FL
WI
All
KS
TN
CT
CA
MN
MI
SC
IN
WY
DE
NM
CO
All States
Single State

Appendix D Non-Linear Estimates

Table D1 Estimates of non-linear (square) effects of charter share on student outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A: Graduation rate			
	Same year share			Lag one year share		
Charter share	0.422***	0.423***	0.433***	0.267***	0.267***	0.276***
	[0.067]	[0.067]	[0.067]	[0.066]	[0.065]	[0.065]
Charter share square	-0.695***	-0.686***	-0.697***	-0.205**	-0.202**	-0.211**
	[0.167]	[0.169]	[0.169]	[0.089]	[0.087]	[0.086]
R-squared	0.829	0.83	0.83	0.828	0.829	0.829
Observations	152,609	152,609	152,609	152,609	152,609	152,609
N(district)	10,656	10,656	10,656	10,656	10,656	10,656
			Panel B: Math			
	Same year share			Same cohort last year		
Charter share	-0.294*	-0.377**	-0.293**	0.007	-0.086	0.01
	[0.168]	[0.168]	[0.139]	[0.146]	[0.141]	[0.113]
Charter share square	0.422**	0.454**	0.388**	0.027	0.099	0.016
	[0.190]	[0.185]	[0.164]	[0.158]	[0.150]	[0.128]
R-squared	0.861	0.861	0.862	0.861	0.861	0.862
Observations	525,502	525,502	525,502	525,502	525,502	525,502
N(district)	11,399	11,399	11,399	11,399	11,399	11,399
			Panel C: ELA			
	Same year share			Same cohort last year		
Charter share	0.155	0.055	0.094	0.352**	0.236**	0.295**
	[0.150]	[0.138]	[0.134]	[0.137]	[0.116]	[0.116]
Charter share square	-0.032	0.002	-0.003	-0.355**	-0.268*	-0.304**
	[0.215]	[0.196]	[0.186]	[0.163]	[0.138]	[0.139]
R-squared	0.89	0.891	0.891	0.89	0.891	0.891
Observations	525,502	525,502	525,502	525,502	525,502	525,502
N(district)	11,399	11,399	11,399	11,399	11,399	11,399
District FE, State-by-year (-by-grade) FE	Yes	Yes	Yes	Yes	Yes	Yes
Student control	No	Yes	Yes	No	Yes	Yes
District control	No	Yes	Yes	No	Yes	Yes

Notes: The table shows estimates of non-linear effects of charter effects on student outcomes for districts with any charter schools during the sample period. See notes of Table 2 for weight, controls, and clusters.

Table D2 Estimates of non-linear (cube) effects of charter share on student outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Graduation rate						
	Same year share			Lag one year share		
Charter share	0.385***	0.393***	0.404***	0.312***	0.317***	0.328***
	[0.089]	[0.089]	[0.088]	[0.088]	[0.086]	[0.086]
Charter share square	-0.481	-0.515	-0.529	-0.471	-0.5	-0.524
	[0.388]	[0.401]	[0.410]	[0.461]	[0.452]	[0.477]
Charter share cube	-0.188	-0.151	-0.148	0.238	0.266	0.279
	[0.397]	[0.408]	[0.418]	[0.423]	[0.414]	[0.438]
R-squared	0.829	0.83	0.83	0.828	0.829	0.829
Observations	152,609	152,609	152,609	152,609	152,609	152,609
N(district)	10,656	10,656	10,656	10,656	10,656	10,656
Panel B: Math						
	Same year share			Same cohort last year		
Charter share	-0.19	-0.179	-0.159	0.063	0.037	0.097
	[0.226]	[0.220]	[0.206]	[0.221]	[0.217]	[0.194]
Charter share square	-0.031	-0.414	-0.204	-0.215	-0.435	-0.363
	[0.792]	[0.784]	[0.714]	[0.745]	[0.745]	[0.696]
Charter share cube	0.381	0.73	0.498	0.195	0.432	0.307
	[0.635]	[0.629]	[0.557]	[0.583]	[0.579]	[0.543]
R-squared	0.861	0.861	0.862	0.861	0.861	0.862
Observations	525,502	525,502	525,502	525,502	525,502	525,502
N(district)	11,399	11,399	11,399	11,399	11,399	11,399
Panel C: ELA						
	Same year share			Same cohort last year		
Charter share	0.357	0.373	0.33	0.551**	0.518**	0.499**
	[0.269]	[0.266]	[0.236]	[0.248]	[0.245]	[0.227]
Charter share square	-0.911	-1.387	-1.042	-1.217	-1.493	-1.193
	[0.936]	[0.938]	[0.818]	[0.951]	[0.966]	[0.864]
Charter share cube	0.739	1.168*	0.874	0.696	0.991	0.72
	[0.721]	[0.706]	[0.622]	[0.755]	[0.756]	[0.678]
R-squared	0.89	0.891	0.891	0.89	0.891	0.891
Observations	525,502	525,502	525,502	525,502	525,502	525,502
N(district)	11,399	11,399	11,399	11,399	11,399	11,399
District FE, State-by-year (-by-grade) FE	Yes	Yes	Yes	Yes	Yes	Yes
Student control	No	Yes	Yes	No	Yes	Yes
District control	No	Yes	Yes	No	Yes	Yes

Notes: The table shows estimates of non-linear effects of charter effects on student outcomes for districts with any charter schools during the sample period. See notes of Table 2 for weight, controls, and clusters.

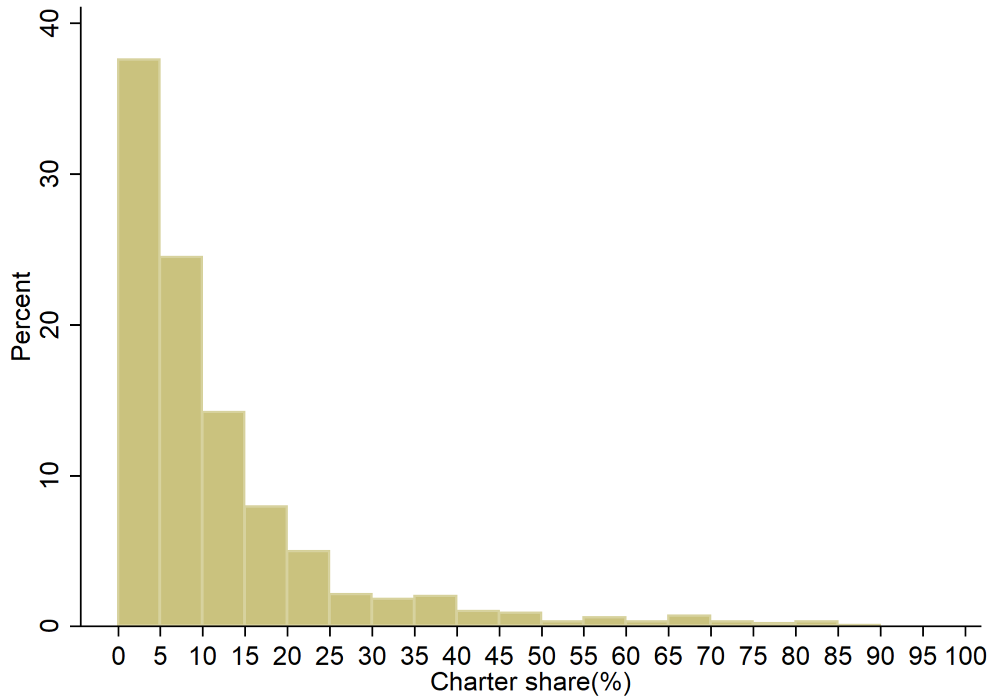
Table D3 Estimates of non-linear (quartic) effects of charter share on student outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Graduation rate						
	Same year share			Lag one year share		
Charter share	0.254*	0.260*	0.290*	0.228	0.238	0.267
	[0.153]	[0.148]	[0.150]	[0.174]	[0.169]	[0.172]
Charter share square	0.863	0.839	0.64	0.417	0.334	0.119
	[1.351]	[1.319]	[1.327]	[1.643]	[1.611]	[1.640]
Charter share cube	-3.458	-3.447	-2.993	-1.949	-1.789	-1.302
	[3.038]	[2.994]	[2.985]	[3.375]	[3.325]	[3.364]
Charter share quartic	2.073	2.091	1.805	1.395	1.311	1.009
	[1.889]	[1.880]	[1.866]	[1.911]	[1.887]	[1.900]
R-squared	0.829	0.83	0.83	0.828	0.829	0.829
Observations	152,609	152,609	152,609	152,609	152,609	152,609
N(district)	10,658	10,658	10,658	10,658	10,658	10,658
Panel B: Math						
	Same year share			Same cohort last year		
Charter share	0.052	0.25	0.125	0.266	0.421	0.337
	[0.360]	[0.356]	[0.316]	[0.329]	[0.323]	[0.306]
Charter share square	-1.984	-3.883	-2.505	-1.877	-3.592	-2.343
	[2.595]	[2.651]	[2.160]	[2.293]	[2.283]	[2.068]
Charter share cube	4.717	8.426	5.596	3.88	7.429	4.693
	[5.132]	[5.294]	[4.251]	[4.565]	[4.570]	[4.067]
Charter share quartic	-2.645	-4.692	-3.104	-2.236	-4.244	-2.659
	[2.895]	[3.001]	[2.407]	[2.600]	[2.617]	[2.316]
R-squared	0.861	0.861	0.862	0.861	0.861	0.862
Observations	525,502	525,502	525,502	525,502	525,502	525,502
N(district)	11,399	11,399	11,399	11,399	11,399	11,399
Panel C: ELA						
	Same year share			Same cohort last year		
Charter share	0.293	0.537	0.4	0.37	0.563*	0.428
	[0.364]	[0.355]	[0.314]	[0.344]	[0.340]	[0.311]
Charter share square	-0.396	-2.713	-1.611	0.271	-1.863	-0.614
	[2.058]	[1.997]	[1.857]	[2.116]	[2.029]	[1.897]
Charter share cube	-0.405	4.11	2.134	-2.602	1.811	-0.564
	[3.872]	[3.770]	[3.627]	[3.932]	[3.695]	[3.548]
Charter share quartic	0.698	-1.793	-0.767	2.002	-0.497	0.778
	[2.200]	[2.164]	[2.098]	[2.196]	[2.060]	[1.997]
R-squared	0.89	0.891	0.891	0.89	0.891	0.891
Observations	525,502	525,502	525,502	525,502	525,502	525,502
N(district)	11,399	11,399	11,399	11,399	11,399	11,399
District FE, State-by-year(-by-grade) FE	Yes	Yes	Yes	Yes	Yes	Yes
Student control	No	Yes	Yes	No	Yes	Yes
District control	No	Yes	Yes	No	Yes	Yes

Notes: The table shows estimates of non-linear effects of charter effects on student outcomes for districts with any charter schools during the sample period. See notes of Table 2 for weight, controls, and clusters.

Figure D1 Distribution of Maximum Charter Shares

(a) Graduation Sample



(b) Test Score Sample