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

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December 30, 2022

Abstract: We study the combined 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 elementary/middle test scores in the geographic district by 0.01-0.02 standard deviations and increases high school graduation rate by about 2 percentage points. The effect sizes are non-linear in charter market share with local maxima in the 20-60% charter share range. The analysis addresses potential endogeneity of charter school location and timing using placebo analysis, bounding, and other methods. We also explain and test for the wide range of mechanisms comprising these effects. Charter entry leads, for example, to closure of traditional public schools that are, on average, low performing, which increases average student outcomes. By focusing on a small number of these mechanisms (e.g., participant effects), prior research has understated the total effects of charter schools.

Keywords: Charter schools; competition; high school graduation; student achievement
JEL Codes: H75, I21, I28

Acknowledgements: This research was carried out under the auspices of the National Center for Research on Education Access and Choice (REACH) based at Tulane University, which is supported by the Institute of Education Sciences, U.S. Department of Education, through Grant R305C100025 to The Administrators of the Tulane Educational Fund. The opinions expressed are those of the authors and do not represent views of the Institute, the U.S. Department of Education, Tulane University, or any other organization. For useful comments, we thank Joshua Cowen, Scott Imberman, and Douglas Lauen; seminar participants at Tulane and Stanford, the Education Research Alliance for New Orleans, and the National Center for Research on Education Access and Choice (REACH); and conference participants at the annual meetings of the Association for Education Finance and Policy (AEFP), Association for Public Policy and Management (APPAM), and the Southern Economic Association (SEA). We also thank Daniel Oliver and Valentina Martinez-Pabon who helped create the National Longitudinal School Database (NLSD), which is the basis for this analysis. All remaining errors are those of the authors.

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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). For example, they operate with more autonomy in hiring teachers and in choosing curricula and instructional methods, allowing charter schools to differentiate themselves from other schools. 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 some control, but their role in charters is more akin to government contracting rather 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 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 this appears to be the first study of the "system level" or total effects of charter schools. 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 within the district boundaries.

We find that charter entry increases system-level student outcomes. When charter market share increases by 10 percentage points, geographic district test scores increase by 0.01-0.02 standard deviations and high school graduation rates increase by 2 percentage points. 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 national trends in outcomes and changes in 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 assumes that there are no unobserved factors that are correlated with both changes 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 use Sun and Abraham (2021) DD method to test for pre-trends; (3) we estimate

"treatment effects" on population levels and demographics to test for sorting bias; (4) we use Oster (2019) bounds that leverage the role of observables to measure the potential impact on unobservables; and (5) we use prior well-identified studies of the individual mechanisms comprising the system-level effect of charter schools 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, and improving understanding of, 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, but these effects 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 2018, Griffith 2019, Slungaard Mumma 2022), and there are a

few examples where charter competition reduced student outcomes (Ni 2009, Imberman 2011, Han and Keefe 2020).[1,2]

But these analyses 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, conceptually, as an extension of competitive effects, the 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).

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 7 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 enrollment.[4] This finding for private schools is useful both for: (a) ruling out selection bias from

[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.
[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.

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, might be viewed as 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. If a whole district changes, then this means charter schools also create unmeasured competitive effects on schools further away. Like the effects on closure, this potential mechanism of charter influence has apparently not received much attention.[5][6] We call this the "far competitive effect" and it is most likely to matter in geographically large districts where there are many TPS located far away from newly entering charter schools.

Fifth, even aside from various participant and 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 school value-added. 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 system-level effects described above—0.01-0.02 s.d. on test scores and 2 percentage points on high school graduation—reflect the total effect of all of the above mechanisms

[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.

[6] There is 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.

combined.[7] Our analysis is most similar to two other studies. Gilraine, Petronijevic, and Singleton (2021) also take a weighted average of outcomes across sectors. However, their study is limited to a single state and is not designed to capture either the closure effect or the district-level response. Also, 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.[8] We build on this 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 heterogeneous effects by location, grade levels, and baseline achievement levels. Prior literature suggests that charter schools have more positive effects in urban areas (CREDO 2013, Chabrier, Cohodes, and Oreopoulos 2016, Cohodes and Parham 2021, Campos and Kearns 2022), a finding that we confirm. One possible reason is that urban districts have lower baseline achievement, which makes improvement somewhat easier to achieve. We find suggestive evidence that student outcomes in districts with lower baseline performance improve more than that in districts with higher baseline performance. Additionally, we find that the effects are concentrated in middle schools and high schools, not elementary schools.

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.

[7] A sixth possibility is that just the threat of charter entry could lead TPS to improve. The other five mechanisms all involve actual charter entry.

[8] 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.

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.[9] 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.[10] 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

[9] All the school years mentioned in this paper are spring school years unless specifically stated otherwise.
[10] 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[11] (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).[12]

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). [13] This is crucial to the analysis as it allows us to capture charter effects operating through

[11] 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/.

[12] 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.

[13] 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.[14] 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.[15] 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

[14] 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.

[15] 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.[16]

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.

We begin by simply plotting the descriptive trends in graduation rate, Math, and ELA scores throughout the sample period for treated districts and comparison districts in Figure 1. The outcomes of zero-charter districts are consistently below the districts with charters. Figure 1 also plots the gradual increase in the overall charter market share to show the average intensity of treatment over time. For all three outcomes, the zero-charter districts and districts with charters display parallel trends in the early years and generally show convergence in outcomes by the last period.

Figure 1 provides some visual, descriptive evidence of a possible system effect of charter schools. But these trends reflect both the types of districts that charters locate in *and* the effects of charter entry on those same outcomes. We are interested in isolating the system-level causal effect and proceed by describing our identification strategy.

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

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 make false assumptions about treatment timing.

The dependent variables in our analysis are test scores ($Test_{ijt}$) and high school graduation (GR_{it}) in district *i* and grade *j* during year *t* (test scores are grade-specific; graduation is not). As in most simple DD models, we include unit fixed effects μ_i (in this case, for geographic districts) and year fixed effects λ_t. It is therefore a two-way fixed effects model, but unlike the simple DD, we allow treatment to be continuous and vary over time through the treatment variable $Charter_{it}$. This implies the following models:

$$GR_{it} = \alpha + \beta Charter_{it} + X_{it}\gamma + \mu_i + \lambda_t + \varepsilon_{it} \quad (1)$$

$$Test_{ijt} = \alpha + \beta Charter_{ijt} + X_{it}\gamma + \mu_i + \lambda_t + \omega_j + \varepsilon_{ijt} \quad (2)$$

In equation (2), ω_j is a vector of grade fixed effects, and other terms are defined same as equation (1). X_{it} is a vector of district characteristics and ε_{it} is the error term. The coefficient of interest is β, which measures the charter effects on the graduation rate. Standard errors are clustered at the district level, and the estimates are weighted by high school enrollment for graduation rate and by grade-level enrollment for Math and ELA. (We report results clustered at the state level in Appendix Table A3, but this has minimal influence on inference.)

In our baseline analyses, we estimate the GDD model using the current year charter enrollment share. In one robustness check, we estimate the model for the graduation rate using

the lag one year charter enrollment share.[17] For test scores, we estimate using the prior year's charter enrollment share (same cohort), as this may better reflect the timing of the TPS responses.[18]

Our main identifying assumption is that there are no unobserved factors that are correlated with 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.

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.[19] This would lead to a downward bias in our estimates because TPS outcomes comprise the majority of the weighted average outcomes.

Alternatively, local or state policy changes beyond charter schools might coincide with the timing of charter entry and disproportionately affect the outcomes of either the treated or untreated districts. For example, since school districts are often charter authorizers, it may be that a change in school board politics leads to a variety of simultaneous policy changes in district

[17] 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.
[18] For example, we use the charter enrollment share of Grade 7 in 2010 to estimate the test score of Grade 8 in 2011.
[19] 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.

policy. Since charter schooling is only one of these policies, we might falsely attribute outcome changes to charter schools that were actually caused by other policy changes. If these other policies had positive (negative) effects, then this would yield an upward (downward) bias.

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

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. The first column, for example, shows that a 10-percentage point increase in high school charter enrollment share increases the high school graduation rate by 0.0203 (2.03 percentage points); the same change in elementary charter enrollment share increases math scores by 0.0132 standard deviation (s.d.) and ELA by 0.0239 s.d.

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). 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).

The effects are also generally robust to the addition of district-specific time trends (Appendix Table A5), but this requires some elaboration. Many of the districts start with positive charter market share at the beginning of the panel, which means charter entry in prior years might be influenced by delayed effects of prior charter school entry. In that case, if there is a positive effect, then the inclusion of district-specific time trends will bias the treatment effect estimates downward.

To test this, we estimated models with district-specific time trends for the whole sample (Appendix Table A5) and the sub-sample that of districts whose first charter school enters after the first year of data in the panel (Appendix Table A6). In the whole sample, the high school graduation results are robust but the estimates effects on test scores are null. But, consistent with the above theory, when we use only the sub-sample, all of the estimates for all of the outcomes are positive and sometimes precisely estimated.

IV.B. Threats to Identification

The identifying assumption for the estimates in Table 2 is that there are no 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.

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.[20] 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.

[20] 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.

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 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 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, not just the initial start of treatment.[21] Still, the DD is useful for testing whether the timing and location of charter schools is exogenous in the GDD. If the DD pre-trends test shows that student outcomes in treated districts were trending upwards relative to never-charter districts just before the first charter entered, then this would be a signal that subsequent changes in charter market share within districts that are the basis for the GDD identification, are also endogenous. This means the usual DD parallel trends test is still useful in a GDD setting and we implement this as follows:

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

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

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.[22] Districts are assigned to the treatment group (T_i=1) if they have any charter schools during the sample period; and, finally, those that always have zero

[21] Though there is no explicit comparison group, non-charter districts still contribute to identification indirectly, through the covariates, which are correlated with treatment and outcomes.

[23] 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.

charter schools are in the control group. The vector β_r represents measures of cohort-specific effects compared with the comparison group. We drop always-treated districts in the event study, as suggested in previous studies (Sun and Abraham 2021, Meinhofer et al. 2021). 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.

Figure 2 shows tests for parallel trends from the DD using event studies (left column), both the standard TWFE and the Sun and Abraham (2021) method (right column). No significant differences in the pre-periods emerge for any of the outcomes. With high school graduation rates, using the TWFE method, there are some qualitative signs of a positive pre-trends, though this is less the case in the Sun and Abraham method.

While we are mainly interested in the pre-trends in these event studies, we also show the post-treatment effects. The results are similar to the GDD for graduation rates and reading scores where we also see an indication of improvement, but we do not see much evidence of positive effects in math. Again, we do not give much weight to these post-treatment results because it ignores the gradually increasing dosage of our continuous, time-varying treatment. The key point is that we see limited evidence of pre-trends, which gives us some confidence that charter entry is not endogenous in the GDD results reported earlier.

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. To test this, we estimate "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 direction of the coefficients suggests a possible increase in FRL when charters enter, which would suggest a downward bias in our main GDD treatment effects from Table 2. The direction is the opposite for test scores, but the coefficients are very small. A 10 percentage point increase in the charter market share reduces the FRL share by just 0.1 percentage points in the test score sample. In short, we see no evidence that charter effects really reflect household effects.

IV.B.4. Oster Bounds

We also use Oster (2019) bounds that leverage the role of observables to measure the potential impact on unobservables. The analysis compares the estimate that does not include controls with estimate that includes controls, scaled by the change in R^2. The exact formula is $\beta^* = \tilde{\beta} - \tilde{\delta}\frac{(\beta_0 - \tilde{\beta})(R_{max} - \tilde{R})}{\tilde{R} - R_0}$. Following Oster (2019), we assume $\tilde{\delta} = 1$ and $R_{max} = 1.3R_0$ (but no greater than 1), where β_0 and R_0 are, respectively, the point estimate and the R^2 for the baseline model without control variable; and $\tilde{\beta}$ and $\tilde{R}$ are the point estimate and the R^2 of a model with control variables. If the effect size changes relatively little compared with the increase in the variation explained, a large bias caused by unobservables is unlikely. Others have proposed similar methods (Altonji, Elder, and Taber 2005, Oster 2019, Gibbons, Overman, and Sarvimäki 2021, Gibbons, Heblich, and Timmins 2021).

All of our estimates of Oster bounds are positive and slightly larger in magnitude than our baseline estimates (Table 5). We also carried out a version of the Oster method where the fixed effects are added in the baseline model and only (time-varying) covariates are added. This naturally yields a smaller change in the R^2, and a much larger point estimate. We have chosen to report the more conservative bounds in the main text.

IV.B.5. Plausibility

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.[23] 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. 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.

We can also compare these estimates with system-level analyses in other cities. In New Orleans, for example, the charter school share went from almost zero to 100 percent. In Table 2, this type of shift implies effects of 20 percentage points on high school graduation and 0.13-0.24 s.d. increase in test scores. The figures for test scores are quite close to what Harris and Larsen (forthcoming) report for New Orleans. The high school graduation rate results are larger than in the New Orleans study.

[23] 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.

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 market share. For example, some charter authorizers might have "low standards" and decide to accept a large number of applications where the marginal applicants are less efficient. Also, TPS might respond 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.[24]

We tested for non-linear effects by modifying equations (1) and (2) so that the treatment variable, $Charter_{it}$, is entered as a polynomial: quadratic (Table A7), cubic (Table A8), and quartic (Table A9). The quadratic results show fairly consistent, precise, positive coefficients on the linear term and precise negative coefficients on the squared term, suggesting diminishing returns. (The only exceptions are the Math results where the dependent variable is specific contemporaneously.) We see the same pattern when switching to the cubic, except the estimates get less precise and the coefficient on the cubed term is positive. Finally, as we turn to the quartic, only a handful of coefficients are precisely estimated (only on the linear terms). This is not surprising given how few districts there are with charter market shares above 50 percent.

To see these patterns more clearly, we plot the lines implied by these coefficients in Figure 3. All but one of the non-linear estimates show a local maximum effect in the 20-60 percent charter market share range, but the results diverge at the very high charter market shares, perhaps, again, because of imprecision. (We also plotted these curves with confidence intervals,

[24] 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.

but these are difficult to display because of how wide the confidence intervals are in the high charter market share band.)

V. Effect Heterogeneity

Prior research has suggested that charter school participant effects vary by urbanicity and student demographics. One possible explanation is that it is simply easier to induce improvement in districts that are very low-performing to start with (Harris and Larsen forthcoming) and urban schools have lower outcome levels.

We start by confirming what others have found. Column (1) on Table 6 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 or 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. 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.

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).[25] Results in Table 6 column (6) suggest some evidence of a positive interaction between charter effects and the policy index for test scores (the estimate for ELA is positive and precise) but the point estimate is negative and imprecise for high school graduation).

Finally, we estimate heterogeneous effects by school grade levels for test scores and report the results in Table 7. 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

[25] 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.

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.

VI. Mechanisms of the Total Charter Effect

The above analyses estimate the total effects of charter entry on entire school districts, capturing the participant, competition, and closure effects, 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 (1995-2018) 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 8 presents the results. We find increases in TPS closure when charters enter. The magnitudes are fairly consistent across specification. Taking the estimate in column (6), a 10

percent increase in charter enrollment share increases the school closure rate by about 0.08 percentage points (a 7 percent increase over the baseline rate). In two additional analyses, we also carry out the placebo version, examining "effects" of elementary charter school entry on high school closure (vice versa). Panels B and C show that the closure effects are much smaller in the placebo estimates (though sometimes still precisely estimated), reinforcing that the main estimates partially reflect causal effects. Given the possibly delayed nature of TPS closures, we also estimated the effects with 2- and 4-year lags. The effects get smaller, but are still precise in the 2-year lag and are null in the 4-year lag.

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).[26]

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

[26] 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.

treated districts, but -0.0023 s.d. for closed TPSs in treated districts.[27] This suggests that charter 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.[28]

We focus on the districts that have at least one private school at baseline.[29] 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 9). A

[27] 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 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.
[28] They used a fixed-effects model as well as an instrumental variables strategy that exploits exogenous variation from Michigan charter law.
[29] A total of 6,449 school districts (roughly one-half of all geographic districts) have one private school during the sample period.

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.

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.

Second, this is the first study to our knowledge to identify, and simultaneously study and compare, the mechanisms of the effects of charter entry: participant, near competitive, far competitive, closure, and matching effects. In particular, we 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/MSA areas, 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 20-60 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	-0.23	0.24
ELA	N.A.	N.A.	N.A.	0.00	-0.28	0.29
Enrollment	60,146	12,777	137,653	58,412	105,269	10,543
White	0.63	0.74	0.46	0.53	0.39	0.68
Black	0.16	0.12	0.23	0.17	0.22	0.11
Hispanic	0.15	0.09	0.24	0.24	0.33	0.15
FRL	0.29	0.25	0.36	0.52	0.59	0.45
Special education	0.12	0.12	0.13	0.13	0.12	0.13
Urban	0.29	0.16	0.52	0.29	0.45	0.12
Suburb	0.40	0.40	0.39	0.42	0.42	0.42
Town	0.12	0.17	0.05	0.11	0.05	0.17
Rural	0.19	0.27	0.05	0.18	0.07	0.29
Revenue per student	9,105	9,148	9,034	12,537	12,050	13,035
Expenditure per student	9,271	9,279	9,257	12,543	12,107	12,989
Student-teacher ratio	16.77	16.16	17.78	16.67	17.35	15.97
Teacher salary	80,168	78,054	83,629	101,537	101,464	101,613
No. magnet school	1.14	0.14	2.79	4.63	8.53	0.64
Observation	152,639	137,751	14,888	525,816	77,985	447,831
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 high school enrollment, and Math & ELA sample is weighted by grade level 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	0.203***	0.203***	0.202***	0.180***	0.178***	0.178***
	[0.041]	[0.041]	[0.041]	[0.037]	[0.037]	[0.037]
R-squared	0.800	0.801	0.801	0.800	0.801	0.801
Observations	152,639	152,639	152,639	152,639	152,639	152,639
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	0.132*	0.061	0.091	0.185***	0.134**	0.161***
	[0.079]	[0.078]	[0.073]	[0.066]	[0.062]	[0.060]
R-squared	0.847	0.848	0.848	0.847	0.848	0.848
Observations	525,816	525,816	525,816	525,816	525,816	525,816
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	0.239**	0.142	0.156**	0.221**	0.148*	0.166**
	[0.097]	[0.088]	[0.075]	[0.096]	[0.086]	[0.072]
R-squared	0.881	0.882	0.883	0.881	0.882	0.883
Observations	525,816	525,816	525,816	525,816	525,816	525,816
N(district)	11,399	11,399	11,399	11,399	11,399	11,399
District, (grade) & 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: 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 high school enrollment for graduation rate and grade-level enrollment for Math and ELA. 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	0.021***	0.020***	0.019***	0.010	0.009	0.011	0.018	0.017	0.019
	[0.006]	[0.005]	[0.006]	[0.014]	[0.014]	[0.014]	[0.013]	[0.013]	[0.013]
R-squared	0.799	0.800	0.801	0.847	0.848	0.848	0.881	0.882	0.883
Observations	152,639	152,639	152,639	525,816	525,816	525,816	525,816	525,816	525,816
N(district)	10,658	10,658	10,658	11,399	11,399	11,399	11,399	11,399	11,399
District,(grade) & year 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 8, 9, and 10 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 enrollment and FRL

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Enrollment								
	Graduation sample				Test score sample			
	Same year share		Lag one year share		Same year share		Same cohort last year	
Charter share	0.110	0.020	0.109	0.020	0.078	0.046	0.051	0.020
	[0.198]	[0.118]	[0.193]	[0.108]	[0.082]	[0.078]	[0.058]	[0.052]
R-squared	0.977	0.989	0.977	0.989	0.994	0.994	0.994	0.994
Observations	152,639	152,639	152,639	152,639	525,816	525,816	525,816	525,816
N(district)	10,658	10,658	10,658	10,658	11,399	11,399	11,399	11,399
Panel B: FRL								
	Graduation sample				Test score sample			
	Same year share		Lag one year share		Same year share		Same cohort last year	
Charter share	0.045	0.019	0.023	0.004	-0.014	-0.006	-0.007	-0.001
	[0.048]	[0.046]	[0.046]	[0.043]	[0.020]	[0.018]	[0.018]	[0.015]
R-squared	0.904	0.910	0.904	0.910	0.951	0.952	0.951	0.952
Observations	152,639	152,639	152,639	152,639	525,816	525,816	525,816	525,816
N(district)	10,658	10,658	10,658	10,658	11,399	11,399	11,399	11,399
District, (grade) & 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 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.

Table 6 Effect heterogeneity

	(1)	(2)	(3)	(4)	(5)	(6)
Group	MA	Black	Hispanic	FRL	Performance	Policy
			Panel A: Graduation rate			
Charter share* group	0.151**	0.154	0.584***	0.374*	-0.622***	-0.196
	[0.068]	[0.106]	[0.223]	[0.215]	[0.122]	[0.469]
Charter share	0.103**	0.185***	0.149***	0.107*	0.403***	0.349
	[0.046]	[0.060]	[0.038]	[0.057]	[0.068]	[0.288]
R-squared	0.797	0.797	0.797	0.797	0.799	0.797
Observations	152,181	152,181	152,181	152,181	149,317	152,181
N(district)	10,626	10,626	10,626	10,626	10,283	10,283
			Panel B: Math			
Charter share* group	0.554***	-0.560*	0.085	-0.773***	-1.058***	1.643
	[0.201]	[0.316]	[0.374]	[0.254]	[0.377]	[1.133]
Charter share	-0.275	0.248**	0.124	0.496***	0.542***	-0.872
	[0.168]	[0.112]	[0.116]	[0.138]	[0.176]	[0.684]
R-squared	0.821	0.821	0.821	0.821	0.823	0.821
Observations	471,419	471,419	471,419	471,419	455,195	471,419
N(district)	10,286	10,286	10,286	10,286	9,586	9,586
			Panel C: ELA			
Charter share* group	0.486**	-0.560**	0.430	-0.429**	-0.868***	1.921**
	[0.204]	[0.284]	[0.311]	[0.199]	[0.304]	[0.901]
Charter share	-0.180	0.292**	0.101	0.382***	0.506***	-0.999*
	[0.171]	[0.115]	[0.105]	[0.125]	[0.167]	[0.547]
R-squared	0.858	0.858	0.858	0.858	0.859	0.858
Observations	471,419	471,419	471,419	471,419	455,195	471,419
N(district)	10,286	10,286	10,286	10,286	9,586	9,586
District, (grade) & year 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 7 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	0.323*	0.230	0.289**	0.203*	0.146	0.184**
	[0.167]	[0.158]	[0.135]	[0.113]	[0.105]	[0.092]
R-squared	0.861	0.862	0.862	0.861	0.862	0.862
Observations	278,717	278,717	278,717	278,717	278,717	278,717
N(district)	11,257	11,257	11,257	11,257	11,257	11,257
	Panel B: ELA Grade 3-5					
Charter share	0.308**	0.194	0.241**	0.197*	0.126	0.160*
	[0.156]	[0.139]	[0.121]	[0.120]	[0.107]	[0.094]
R-squared	0.9	0.901	0.902	0.9	0.901	0.902
Observations	278,717	278,717	278,717	278,717	278,717	278,717
N(district)	11,257	11,257	11,257	11,257	11,257	11,257
	Panel C: Math Grade 6-8					
Charter share	0.315**	0.232*	0.245**	0.369***	0.308***	0.321***
	[0.133]	[0.121]	[0.110]	[0.112]	[0.105]	[0.095]
R-squared	0.884	0.885	0.885	0.884	0.885	0.885
Observations	246,809	246,809	246,809	246,809	246,809	246,809
N(district)	11,113	11,113	11,113	11,113	11,113	11,113
	Panel D: ELA Grade 6-8					
Charter share	0.569***	0.439***	0.433***	0.454***	0.359***	0.356***
	[0.151]	[0.137]	[0.113]	[0.129]	[0.120]	[0.096]
R-squared	0.899	0.9	0.901	0.899	0.9	0.901
Observations	246,809	246,809	246,809	246,809	246,809	246,809
N(district)	11,113	11,113	11,113	11,113	11,113	11,113
District, (grade) & year 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 8 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	0.012**	0.017***	0.014***	0.007	0.011***	0.008**
	[0.005]	[0.005]	[0.004]	[0.004]	[0.003]	[0.003]
R-squared	0.08	0.083	0.084	0.08	0.082	0.084
Observations	279,698	279,698	279,698	279,698	279,698	279,698
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	0.007***	0.010***	0.007***	0.004*	0.006***	0.004**
	[0.003]	[0.002]	[0.002]	[0.002]	[0.002]	[0.002]
R-squared	0.064	0.065	0.067	0.064	0.065	0.067
Observations	279,698	279,698	279,698	279,698	279,698	279,698
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	0.002	0.003**	0.003	0.001	0.002	0.002
	[0.002]	[0.002]	[0.002]	[0.002]	[0.002]	[0.001]
R-squared	0.07	0.071	0.072	0.07	0.071	0.072
Observations	279,698	279,698	279,698	279,698	279,698	279,698
N(district)	12,435	12,435	12,435	12,435	12,435	12,435
District, (grade) & 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.

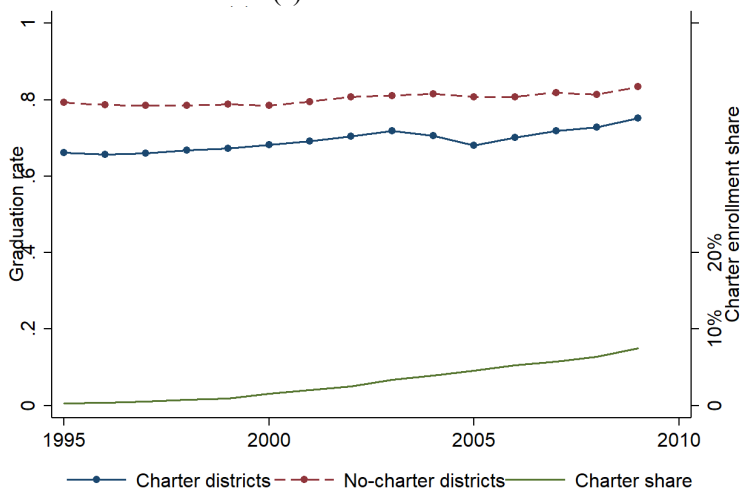
Table 9: Effects of charter share on private school enrollment share

	(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	-0.007	-0.012	-0.011	0.008	0.005	0.006
	[0.020]	[0.021]	[0.020]	[0.019]	[0.020]	[0.020]
R-squared	0.912	0.913	0.913	0.912	0.913	0.913
Observations	65,279	65,279	65,279	65,279	65,279	65,279
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	0.019	0.006	0.012	0.040	0.031	0.035
	[0.052]	[0.050]	[0.049]	[0.048]	[0.048]	[0.047]
R-squared	0.897	0.897	0.897	0.897	0.897	0.897
Observations	65,279	65,279	65,279	65,279	65,279	65,279
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	0.019	0.022	0.024	0.028	0.031	0.033
	[0.033]	[0.033]	[0.032]	[0.028]	[0.027]	[0.027]
R-squared	0.884	0.884	0.884	0.884	0.884	0.884
Observations	65,279	65,279	65,279	65,279	65,279	65,279
N(district)	6,449	6,449	6,449	6,449	6,449	6,449
District, (grade) & year FE	Yes	Yes	Yes	Yes	Yes	Yes
Student control	No	Yes	Yes	No	Yes	Yes
District control	No	No	Yes	No	No	Yes

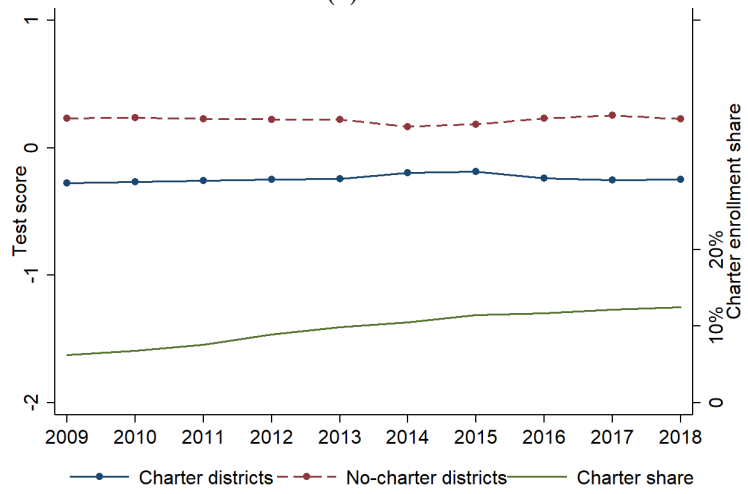
Notes: This table presents estimates of the effects of the charter share on the share of private school enrollment. The sample period is from the year 1996 to 2018 biannually. Regressions are weighted by the district total enrollment. See notes of Table 2 for controls and clusters.

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

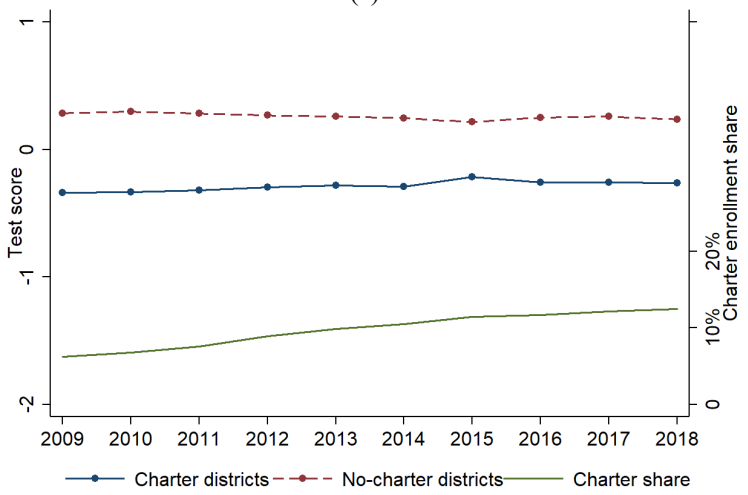
(a) Graduation rate



(b) Math



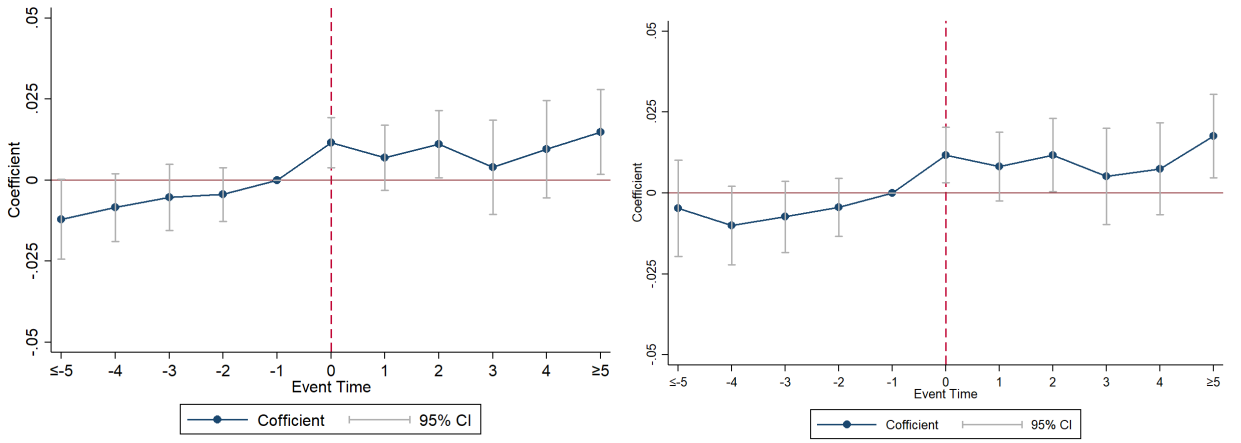
(c) ELA



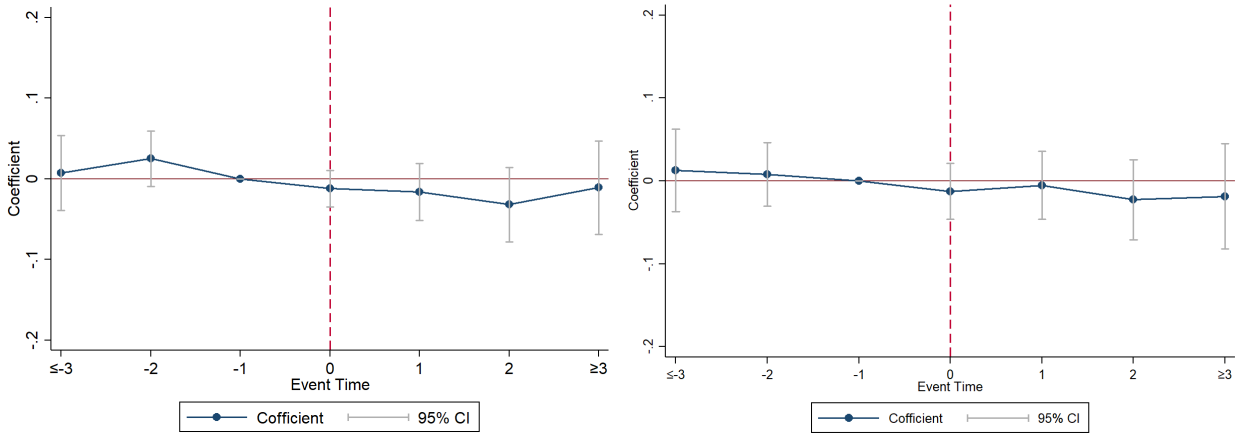
Notes: This figure plots the trends in Graduation rate, Math & ELA of charter districts (in the solid line), and no-districts (in the dashed line). The green solid line plots the charter enrollment share of treated districts.

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

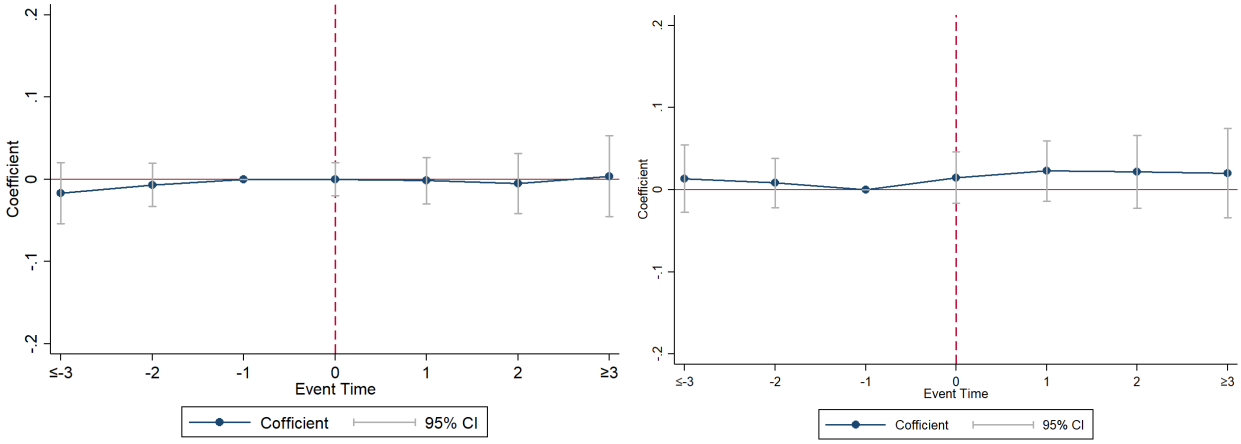
(a) Graduation rate



(b) Math



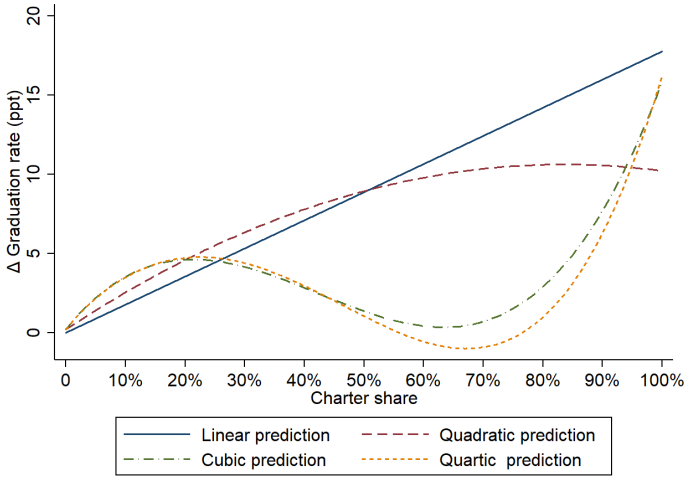
(c) ELA



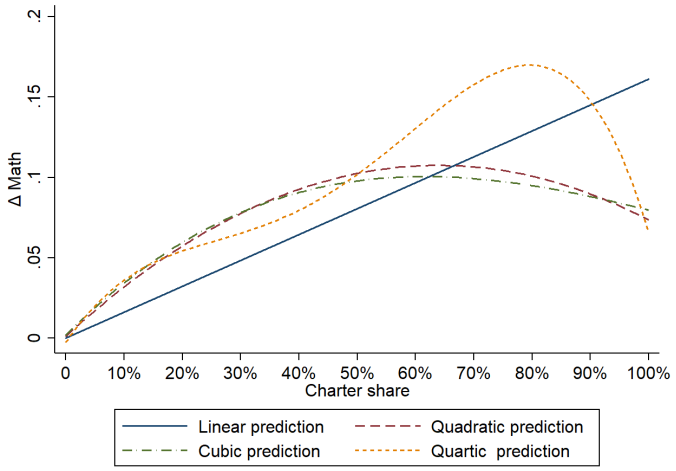
Notes: This figure presents the results of event study (left) and IW estimators (right) for graduation rate, Math, and ELA. The event time zero is the first year of charter entry.

Figure 3 Linear and non-linear estimates of charter share on student outcomes

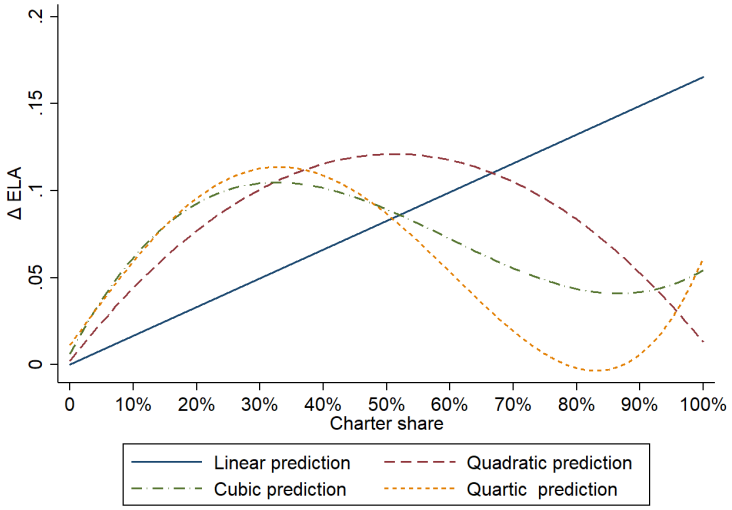
(a) Graduation rate



(b) Math



(c) ELA



Note: This figure presents the linear and non-linear estimates of charter share on student outcomes.