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How Do Charter Schools Affect System-Level Test Scores and Graduation Rates? A National Analysis

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

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Abstract: We study the combined effects of charter schools, and their various mechanisms, on a national level and across multiple outcomes. Using difference-in-differences and fixed effects methods, we find that charter entry (above 10 percent market share) increases high school graduation rate in geographic districts by about 2-4 percentage points and increases test scores by 0.06-0.16 standard deviations. Charter effects peak with 5-15 percent charter market share. Also, total effects are comprised not only of participant and competitive effects, but also the charter-induced closure of low-performing traditional public schools. The analysis addresses potential endogeneity of charter school location and timing.

Keywords: Charter schools; competition; high school graduation; student achievement
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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.

Proponents argue that this more market-driven approach could increase innovation, create a better fit between schooling options and student needs, reduce the inefficiencies of bureaucracy, 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).

Empirical research has examined parts of these theories, but not their combined effects on whole school systems. We provide arguably the first evidence on this topic, especially on a national level, using two decades of data from the National Longitudinal School Database (NLSD), which includes nearly all districts in the U.S. from school years 1995 to 2016. Using a matched difference-in-differences (DD) and fixed-effects (FE) identification strategies, we study the effects of charter entry on geographic-district-level high school graduation rates and test scores.

A second contribution of our study is understanding the roles played by the three main mechanisms of these systemwide influences: competitive effects, participant effects, and closure effects. Studies of the competitive effects of charter schools on student outcomes in TPSs 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), and there are a few examples where charter competition reduced student outcomes (Ni 2009, Imberman 2011, Han and Keefe 2020).[1] Overall, the existing literature suggests that charter competition effects on TPSs are small.[2]

Participant effects represent the second key branch of literature. Even aside from their competitive effects on TPS, charter schools might serve their own students more or less effectively than other schools. 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). As with the competitive effect literature, the results vary, but the average is small, positive, and

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

improving over time. The one other national study of participant effects, in particular, suggests that the average charter participant effect is small and positive (CREDO 2013).

A third mechanism through which charter schools might affect student outcomes is through the closure of TPS. While this can be viewed 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 the market, 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) and the effects of charter schools on private school enrollment (Chakrabarti and Roy 2016). However, to our knowledge, no prior research has assessed the causal effect of charter entry on TPS closure.

We start by analyzing the combined effects of all these mechanisms on whole school systems. To accomplish this, our dependent variables are a weighted average of TPS *and* the charter schools located in the same geographic district. Using DD and FE analysis, combined with propensity score matching (PSM) or propensity score weighting (PSW), we find that charter entry increases district-level student outcomes. For example, districts with more than 10 percent charter market share increased high school graduation rate by 2-4 percentage points and increase test scores by 0.06-0.16 standard deviations. Applying the approach proposed by Goodman-Bacon (2021), we find that the effects are dominated by the treated versus never-treated groups. We also employ Sun and Abraham's (2021) method in our event study model to address potential heterogeneity in effects across cohorts and time (De Chaisemartin and d'Haultfoeuille 2020, Callaway and Sant'Anna 2021, Sun and Abraham 2021).

[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.

While this average effect is of considerable policy interest, there are also good reasons to explore heterogeneity by market share. Some states, for example, have charter caps that limit charter entry to a small share of statewide enrollment, but there is little research on whether there might be an optimal charter school market share that would justify these state-level or district-level caps. When we vary the charter threshold from 1-20 percent, we find that the effects become evident at about five percent market share and plateau at 15 percent market share. While this implies that some sort of charter cap might be efficient, these results suggest different kinds of caps than those employed by some states.

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), a finding that we confirm. Harris (2020) hypothesizes that this might be because urban districts have lower baseline achievement, which makes improvement somewhat easier to achieve. We find some suggestive evidence of this, though the estimates are imprecise. Additionally, we find that the effects are concentrated in middle schools and high schools, not elementary schools.

Our analysis of the mechanisms behind these overall system effects suggests that participant, competitive, and closure effects likely all play a role. In particular, we find that when charter market share reaches 10 percent, the TPS school closure rate increases by 40 percent. Finally, we find little evidence that charter entry reduces private school enrollment.[4] This finding for private schools is useful both for ruling out selection bias from charter schools induced students to move

[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.

in or out of private schools, and for understanding possible effects of charter schools on private schools, which could also affect educational outcomes and social welfare.

The main threat to identification is that charter school location and timing are endogenous. There is evidence about where charter schools tend to locate based on time-invariant, observable factors (Bettinger 2005, Glomm, Harris, and Lo 2005, Bifulco and Buerger 2015), which the DD and FE methods are effective in addressing. However, these methods, by themselves, cannot rule out time-varying unobserved factors. We address this by leveraging the timing of state laws in two ways: (a) we restrict the comparison group in the DD to districts in states that had no charter laws in place during the entire panel; and (b) we compare districts in the early-adopting state to those in late adopting states. The assumption in these analyses is that the existence and/or timing of state laws is independent of the time-varying changes arising specifically in the districts that are likely to be treated. While we conclude that this assumption is reasonably plausible, we also use placebo analyses as a third method for addressing endogenous charter entry and location. In one case, we test for effects of charter market share in particular grades on the outcomes in grades where effects are implausible and, in the other version, we test for effects of subsequent charter entry on pre-treatment outcomes. Fourth, and finally, we follow Oster (2019) and use selection on observables to evaluate the extent of potential of selection on unobservables. These analyses generally reinforce the conclusion that charter schools improve test scores and/or high school graduation.

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.

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.[5] 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, school type, district finance, school-age population and the poverty rate). The addresses of charter school also allow us to place the location of each charter school within its geographic school district.[6] 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)[7] (hereafter, graduation rate) and student Math and English Language Arts (ELA) test scores. The test scores are available in 3rd through 8th grade in Math and ELA over the 2009-2016 school years from the Stanford Education Data Archive (SEDA). The SEDA provides nationally comparable, publicly available test score data for U.S. public school districts[8] (Ho 2020). For

[5] All the school years mentioned in this paper are spring school years unless specifically stated otherwise.
[6] We note that some charter schools located within a given geographic district are authorized or regulated by a government agency or delegate located outside the district, such as a state government or a university. Also, some states have elementary and high school districts that overlap one another; in these cases, we assign the charter school to the district whose schools serve the same grade levels as the charter school.
[7] 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.
[8] 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/.

interpretation purposes, we normalize the test scores to have means of zero and standard deviations of unity within the grade, year, and subject.

The graduation rate sample includes schools covering grades 9-12 annually from 1995 to 2010. These data include both TPS and charter schools, which constitute the policy treatment in this study. 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 TPSs compete with charters when there is a threat their students will leave for another school.

For both outcomes, we then calculate the geographic district outcome by averaging TPS and charter school outcomes (weighted by enrollment share). This is crucial to the analysis as it allows us to capture charter effects operating through 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 and the competitive effects of individual charter schools on nearby. Here, we are interested in the net 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 (see further discussion later).

One potential threat to identification in our analysis is that charter entry coincides with changes in the population of students attending publicly funded schools (charter or TPS). To test this, we also sometimes include the following (time-varying) covariates: 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); student-teacher ratio; average teacher salary; number of magnet schools; number of schools; total revenue per student; total expenditure per student; and the poverty rate of the school-age population. Since these measures are potentially endogenous, we view results with these controls as an endogeneity test, not as the preferred effect estimates.

Table 1 presents the summary statistics for the outcome and control variables.[9] 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. These observable differences are minimized after matching (see later discussion).

The test score (graduation) data are available for 10,439 (9,278) districts, including 607 (416) districts that have at least one charter school. The final samples for the test scores (graduation) analyses include 83 (70) percent of the nation's publicly funded schools and 85 (78) percent of those schools' enrollment.[10] We also have complete data for 30 (31) percent for charter schools in the analysis. Appendix B tests for observable differences between included and missing districts. The student demographics between the two are very similar; however, unsurprisingly, the enrollment sizes of the missing districts are much smaller than those in the included districts. Also, the missing charter schools are more likely missing if they were authorized by an entity other than the local district (e.g., a state agency or university).[11] Prior research suggests that the performance of charter schools is not related to authorizer type

[9] Table 1 is weighted by enrollment size. Table A1 in the Appendix presents the unweighted summary statistics for the outcome and control variables of our samples.

[10] Some districts have data missing for all years in the panel, while others are missing for so many years that imputation is a questionable strategy. For test score (graduation) data, our main estimates remove 16 (9) percent of districts with charters where we have some data, but too few time periods. We also ran a version of results that includes all available observation-years and these estimates are similar to those reported when these districts are dropped (results are available upon request).

[11] The missing data is most likely because charter schools authorized by non-local-district entities do not report data through the local districts; they function as their own Local Education Agencies (LEA) for governing and reporting purposes.

(Carlson, Lavery, and Witte 2012), so we do not expect this to influence our main conclusions.[12] We discuss potential implications in the discussion of results.

The timing of the passage of charter laws also figures prominently in our analysis. Charter schools have seen dramatic growth over the last three decades. The first law allowing the establishment of public charter schools was passed in Minnesota in 1991. As of fall 2020, charter school legislation had been passed in 45 states and the District of Columbia.[13] Table 2 presents the years of charter school legislation as of 2020. As a result of the growing number of states allowing charter schools, the percentage of publicly funded schools that are charter schools increased from 0 to 7.3 percent, and their percentage of enrollment increased from 0 to 6.2 percent.[14]

The analysis that follows focuses on the effects of charter entry on whole districts. Table A2 in the Appendix presents the Top 20 districts with the largest charter enrollment share among districts with at least 10,000 total students in the 2018 spring year. New Orleans tops this list, followed by Gary (Indiana), Queen Creek (Arizona), Washington DC, and Detroit. The question of interest in this study is whether charter school entry in these (and other) districts has improved or reduced student outcomes years after they have opened and, if so, through what mechanisms.

We begin by simply plotting the descriptive trends in graduation rate and Math, and ELA scores throughout the sample period for treated districts and comparison districts in Figure 1. For purposes here, we define a treated district as one that is ever at or above 10 percent market share and the comparison group is districts that never have charter schools. The comparison group

[12] Zimmer et al. (2014) find that Ohio charter schools authorized by non-profit organizations are typically more effective, but it is extremely rare, nationally, for non-profit organizations to be charter authorizers.
[13] The states in which public charter school legislation had not been passed by that time were Montana, Nebraska, North Dakota, South Dakota, and Vermont.
[14] Figure A1 in the Appendix presents the trends in charter school share and charter enrollment share from spring 1991 to spring 2018.

outcomes are consistently below the treatment group, reflecting that charter schools tend to locate in low-performing districts. Figure 1 also plots the gradual increase in the overall charter market share to show the intensity of treatment over time. For all three outcomes, the comparison and treatment groups display parallel trends in the early years and generally show convergence by the last period. This provides some visual evidence of a system effect of charter schools, at least at a descriptive level. In what follows, we describe our identification strategy.

III. Identification Strategy

III.A. Difference-in-Differences

We rely mainly on a DD strategy to evaluate the effect of charter enrollment share on student achievement. The treatment group includes those districts whose charter market share is ever at or above the threshold τ. In our baseline model, we use $\tau = 10$ percent of charter enrollment share.[15] The first comparison group includes districts (in all states) that did not have charter schools during the panel (i.e., never treated). Districts with charter shares that are above zero but below the threshold are omitted from the analysis to create a clear treatment contrast.

We use "ever above" the threshold because TPSs and other local education-related organizations (e.g., university schools of education) are likely to be aware in advance that their areas are going to have large charter market shares and they may therefore start reacting before districts reach the τ threshold. Since τ is inherently arbitrary, we estimate the models assuming a wide variety of threshold levels. In addition to providing a robustness check on the threshold choice, this provides initial evidence regarding non-linear market share effects.

We estimate the DD effects using equations (1) and (2):

[15] The charter market share here is for the sector of public schools and does not include private schools.

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

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

In equation (1), the dependent variable is the high school graduation rate (GR_{it}) for the high schools in district *i* in year *t*. T_i is an indicator variable equal to unity if the district *i* charter enrollment share is ever above the threshold and equal to 0 if district *i* has no charter schools during the sample period; $Post_{it}$ is an indicator set to unity starting in the period that district *i* had its first charter entrant and continuing thereafter. Finally, μ_i is district fixed effects; λ_t is year fixed effects; 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 graduation rate.

In equation (2), Math and English Language Arts (ELA) test scores are the dependent variables, where $Test_{ijt}$ is the test score in district *i* and grade *j* (specifically, grades 3-8) during the year t, ω_j is a vector of grade fixed effects, and other terms are defined same as equation (1). In other words, (2) is the same as equation (1) except for the dependent variable and the fact that the outcome is grade specific. Standard errors are clustered at the district level, and the estimates are weighted by high school enrollment for graduation rate and grade-level enrollment for Math and ELA. (We report results clustered at the state level in Table A3 in the appendix, but this has minimal influence on inference.)

In some specifications, we combine our DD design with matching methods to minimize the differences between treated districts and comparison districts (i.e., to achieve common support). For example, states may intervene in districts that are persistently low-performing and/or otherwise similar. In this case, matching on observables reduces comparison-treatment differences in unobserved policy interventions that may also shape student outcomes. Matching therefore may increase the probability that the identifying assumptions hold for the DD analysis.

We use district covariates to match treated to comparison districts (nearest neighbor).[16] This yields the Propensity Score Matching (DD-PSM) sample. We also use the propensity score as the inverse probability weight for a Propensity Score Weighting (DD-PSW) analysis. These approaches minimize the difference between the control and treated groups, as reflected in density plots before and after matching (Appendix Figure A2).

In the above two-way fixed effects DD specification, the indicator for charter entry switches from 0 to 1 at varying times for different treated districts. Goodman-Bacon (2021) notes that the overall DD estimate is a weighted combination of each possible pairwise DD estimate comparing treated districts with: (a) never-treated districts, (b) districts that are not yet treated but are treated later, and (c) districts previously treated. For this reason, we apply the Goodman-Bacon (2021) decomposition to determine the underlying source of identification.

Event study analyses also partially address this concern and provide evidence about parallel trends, intercept shifts and dynamic effects. However, with staggered treatment start dates, the coefficient on a given lead or lag can be contaminated by effects from other periods due to effect heterogeneity (De Chaisemartin and d'Haultfoeuille 2020, Callaway and Sant'Anna 2021, Sun and Abraham 2021). To address this concern, we implement Sun and Abraham's (2021) interaction-weighted (IW) estimator for dynamic treatment effects.[17] Equations (3) and (4) are specifications of event study for graduation rate and test scores.

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

[16] We specifically match on the first-year values of the variables listed in Table 1. Table A4 in Appendix presents the probit regression results of the matching model.

[17] The IW estimator is implemented in three steps. First, estimate the interacted regression, where the interactions are between relative time indicators and cohort indicators. Second, estimate the cohort shares underlying each relative time period. Third, take the weighted average of estimates from the first step, with weights set to the estimated cohort shares (Sun and Abraham 2021). We also considered other methods, especially the Callaway and Sant'Anna (2021) and Chaisemartin and D'Haultfoeuille (2020) methods, but these are focused on recovering a single treatment effect and we are interested in dynamic treatment effects.

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

where $d_{i,r}$ is a dummy of the *r* years of leads or lags since district *i* initiated the first charter school[18]. 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). Five or more years leads/lags for graduation rate sample (three years or more for test score sample) are assigned into a single indicator given the length of sample period.

III.B. Threats to Identification

The main assumption in DD analysis is that the comparison group would have followed the same trend as the treatment group if the latter had not been treated. The standard initial test is whether the two groups follow parallel trends prior to treatment. We test this as follows:

$$GR_{it} = \alpha + \gamma(T_i \cdot Time_t) + X_{it}\gamma + \mu_i + \lambda_t + \varepsilon_{it} \tag{5}$$

$$Test_{ijt} = \alpha + \gamma(T_i \cdot Time_t) + X_{it}\gamma + \mu_i + \lambda_t + \omega_j + \varepsilon_{ijt} \tag{6}$$

where $Time_t$ is a linear calendar time trend (pre-treatment years only) and other terms are defined as before. The coefficient of interest is γ. We present our estimate of γ in every table with DD specifications. While our results usually pass this test, we note exceptions and interpret these results with caution.

The pre-trends test is insufficient to establish parallel trends. Specifically, we still have to assume: (1) that charter location was conditionally exogenous (i.e., that treatment was not assigned based on unobserved factors that are correlated with student outcome trends); and (2) and that there were no other idiosyncratic shocks that happened to coincide with treatment

[18] Table A5 in Appendix presents the number of districts by the year the first charter was initiated.

timing.[19] Put differently, the DD analysis described above addresses selection on observable time-invariant unobservable factors, but it does not address time-varying unobservable factors.

It is worth considering specific scenarios under which either of these assumptions might be violated. First, especially given the investment required to open a charter school, it may be that charter schools locate in districts where decreases in TPS student outcomes are expected in the future, yielding a downward bias in effect estimates. Charter schools are more likely to locate in districts with low contemporaneous student outcomes (Glomm, Harris, and Lo 2005), signaling that we might expect charter schools to also locate where *expected* future performance in TPS is lower, in ways that are unobservable in our analysis.

Second, 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 second scenario as very likely because it takes several years to create an organization that can put together 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.

Third, 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

[19] Since we are interested here in long run effects, it is worth noting that idiosyncratic shocks occurring after the start of treatment, and disproportionately affecting either one group, could also create bias if the shocks are correlated with treatment status. The longer the analysis goes into the future, the more likely this is to occur.

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 a district. Since charter schooling is only one of these policies, we might falsely attribute outcome changes to charter schools that were actually caused by other policies. If the other policies had positive effects, then this would yield an upward bias, though the bias could also work in the other direction.

To account for these possibilities, we estimate four variations of the DD. First, we restrict the comparison group to districts located in states that do not have charter laws up to the year 2016 and whose districts always, therefore, have charter shares of zero.[20] This requires the assumption that states do not pass and implement laws at the same that unobserved changes are occurring in the specific districts where charter schools end up locating. This is plausible in the sense that state lawmakers do not know *ex ante* where charter schools will open within their states. They might expect them to locate in "failing districts" or urban districts, but these factors are observable and accounted in the analysis.

Still, we cannot rule out that this assumption is violated. Therefore, using similar logic, we also estimate versions of equations (1) and (2) that compare districts in early and late adopting states.[21] In this case, even if there are unobserved factors associated with *whether* states eventually adopt these policies, the assumption is still satisfied so long as the timing of the charter law passage is somewhat arbitrary. This is also possible given that it usually takes multiple legislative attempts to pass a charter law and that passage depends on an alignment of

[20] These states are Kentucky, West Virginia, Montana, Nebraska, North Dakota, South Dakota, and Vermont. We chose the year 2016 because this is the last year of our data.

[21] The logic here is similar to the Goodman-Bacon method. The early adopter states are those passed state charter law before 1997 (27 states), and the late adopter states are those passed state charter law during 1997 to 2016 (17 states). See Table 2 for the list of states.

political conditions whose timing can be arbitrary. In that case, we can expect the treated districts (in the early-adopting states) to be similar to the comparison districts (in late-adopting states) on even time-varying unobserved factors.

Third, we carry out two placebo analyses. Our main estimates examine the effect of charter school entry in specific grades on the outcomes in those same grades. This sets up the first placebo because the entry of charter high schools cannot directly influence test scores in grades 3-8; likewise, the entry of elementary charter schools cannot influence charter high school graduation until many years later, after students have passed through middle and high school grades. The placebo test therefore involves restricting the treatment variable to charter schools that are mis-aligned with the grades of the dependent variables. In another placebo test, we analyze the "effect" of subsequent high charter market share on student outcomes in periods prior to the entry of charters. Since later entry cannot impact current student outcomes, any apparent effect would be another measure of potential bias.

Fourth, treatment effect estimates can be bounded by adding available covariates (Oster, 2019). If the effect size changes relatively little compared with the increase in the variation explained, it is unlikely that the bias caused by unobservables is large. Others have proposed similar methods (Altonji, Elder, and Taber 2005, Oster 2019, Gibbons, Overman, and Sarvimäki 2021, Gibbons, Heblich, and Timmins 2021).

III.C. Fixed Effects Model

The advantages of the DD approach include providing visual and formal tests for parallel trends, facilitating the estimation of dynamic effects, and testing for non-linear effects based on market share. A disadvantage of the DD is that the treatment gradually increases in dosage (i.e., districts do not instantly shift to the highest market share we observe), yet we must choose s specific year for the treatment to start. To address this, we also employ a FE model using only those districts that have at least one charter school at some point during the panel. In this case, the charter market share is used as a continuous measure of charter enrollment share. We estimate the FE effects based on the following equations:

$$GR_{it} = \alpha + \beta Charter_{it} + X_{it}\gamma + \mu_i + \lambda_t + \varepsilon_{it} \tag{7}$$

$$Test_{ijt} = \alpha + \beta Charter_{ijt} + X_{it}\gamma + \mu_i + \lambda_t + \omega_j + \varepsilon_{ijt} \tag{8}$$

where the $Charter_{ijt}$ is the continuous measure of charter enrollment share of district *i* year *t*. In our baseline analyses, we estimate the FE model using the current year charter enrollment share. In one robustness check, we estimate the FE model for the graduation rate using the average charter enrollment share in the last four years (because it takes four years for a high school student to graduate and student outcomes may be affected by charter share through the four years).[22] For test scores, we estimate the FE model using the prior year's charter enrollment share (same cohort), as this may better reflect the timing of the TPS responses.[23]

The identifying assumptions are similar between the DD and the FE, but not identical and these differences in assumptions provide additional tests for identification. Both the DD and FE account for fixed unobserved factors that are correlated with treatment status and with student outcomes. However, with the DD, we also have ways to account for *time-varying* unobserved factors, so long as these are similar in the comparison group. For this reason, the DD is our preferred approach, but if the DD and FE results differ, this would suggest that time-varying unobserved factors are an issue. Also, the FE assumes that each unit increase in charter market share has an immediate effect on outcomes whereas the DD assumes the timing coincides with

[22] For example, we use the average charter enrollment share of high school (grades 9-12) in 2006-2010 to estimate the effects on graduation rate in 2010 .
[23] For example, we use the charter enrollment share of Grade 7 in 2010 to estimate the test score of Grade 8 in 2011.

the first entry. Again, the comparison of the DD and FE provides evidence about whether the results are sensitive to that decision.

IV. Results

IV.A. Difference-in-Differences and Event Studies

We find consistent evidence that charter entry improves geographic-district-level outcomes. Figure 2 shows event study analyses for the graduation rate, Math, and ELA, using both the traditional and Sun and Abraham (2021) methods, with the latter being our most preferred estimates. The results also suggest an immediate small positive effect on graduation, which roughly doubles one year later and then plateaus. One possible reason for the immediate effect is that TPSs, which still comprise the vast majority of schools when the first charter school enters, anticipate charter entry and work on improving prior to, or during, the first treatment year.[24] Also, if the effect operates through the participant effect, it could be that students who were going to drop out of their TPS instead moved to a charter school that offered a different opportunity immediately. While it is difficult to determine the reason, we are not the first to find this result. Harris and Larsen (forthcoming) also find an immediate effect on high school graduation rates than test scores in New Orleans. The graduation estimates are also robust with the Sun and Abraham (2021) method, and to other robustness checks discussed below. The results are also positive with ELA scores, though these taper off in the Sun and Abraham (2021) specification in the third year post-treatment. With Math, we find positive effects, although we do not pass the pre-trends tests.

[24] Measurement error in the graduation rate is also a possibility. Recall that the AFGR is the number of graduates divided by the lagged number 8th, 9th, and 10th graders. This means, if there is a trend of in-migration, then the number of graduates will increase mechanically and faster than the (lagged) denominator. Thus, this could be an artifact of changing demographics. However, when we use the enrollment level as the dependent variable in the DD analysis, we find no evidence of enrollment change.

Table 3 therefore presents the DD estimates of the effect of charter treatment (i.e., having a charter enrollment share ever above 10 percent) on graduation rate, Math, and ELA test scores, which averages the pre- and post-treatment periods. Columns (1) and (2) are estimates of the DD model with districts from all states; Columns (3) and (4) are estimates of the DD-PSW model; Columns (5) and (6) are estimates of the DD-PSM model.

Here, we find positive and precisely estimated effects for all three outcomes. The magnitudes of the coefficients imply that having a large share of charter enrollment increases the graduation rate by 3-4 percentage points and improves Math and ELA scores by 0.08-0.16 standard deviations.[25] These estimates also generally pass parallel trends tests, presented at the bottom of Table 3, except in Math. In ELA, two of six the tests reject the null, but these two test coefficients are small relative to effect estimates.[26]

To better understand the source of variance underlying these estimates, we follow Goodman-Bacon (2021) in decomposing the DD model and showing the weights attached to each comparison. Table 4 shows that the majority of the weight (60-70 percent) comes from the treated versus never-treated comparison as well as the treated versus never-treated effects; moreover, for these highly weighted comparisons, the effect estimates are positive for all three outcomes.[27]

[25] Note that the sample size drops dramatically in the DD-PSM because we match each treatment district to only one control district.
[26] We conduct additional analysis in appendix Table A6 which add state linear trends. The graduation rate results are very similar. The point estimates remain positive and sometimes statistically significant, but they are smaller and less precise. It could be that neighboring districts are responding to the threat of competition from potential charter schools. In that case we are "over-controlling" with the state trends; that is, if we are interested in the total effect, then the state trends absorb part of the charter effect--that part coming from neighboring districts that don't contribute to the estimation of in the other specifications. The same general pattern emerges when we switch from state trends to state-by-year fixed effects.
[27] See Figure A3 in the Appendix for the plots of Goodman-Bacon decomposition.

One advantage of the DD method is that it allows us to estimate non-linear effects. We therefore also estimated the model allowing a wide range of thresholds, from 1-20 percent charter share. Figure 3 plots the point estimates along with the associated thresholds. When raising the threshold, note that a larger number of districts have $0 < Charter < \tau$, such that they are dropped entirely from the estimation, while at τ =1 percent, almost no districts are dropped.

The expected pattern of results across threshold levels is unclear as this depends on: the source of the effects (participant versus competitive effects) at each threshold; whether the marginal charter school is as effective as the prior ones; how quickly charter market share reaches equilibrium; and whether there is effect heterogeneity that correlates with the long-run district charter share.[28]

As Figure 3 indicates, there is a fairly consistent rise in effect estimates on graduation rate and test scores as the threshold rises, though noticeable effects begin emerging at 5 percent charter market share and continue to about 15 percent market share, after which they plateau. It could be that, beyond a certain point, either the marginal charter entrant is less efficient than earlier entrants or that TPS efficiency wanes when they have to cope with large reductions in resources due to a loss of enrollment to charter schools. This pattern is consistent across all the outcomes and DD strategies. It also does not appear to be due to missing data.[29] We provide additional robustness checks below in the FE analysis, which further reinforce the presence of non-linear effects by market share.

[28] Also, note that we mark the start of treatment when the first charter school enters. In larger districts, more enter later and therefore probably have later effects that are not captured in these estimates.
[29] As Table A2 in the Appendix shows, the districts with the largest market shares are all included in the test score analysis. Six of these 20 districts are omitted in the graduation analyses, but we see the same plateauing pattern with both outcomes.

One of the treated districts with $\tau > 20$ is New Orleans with the largest charter enrollment share among districts with at least 10,000 total students. It is also arguably the most successful charter reform effort documented to date (Harris and Larsen forthcoming). To test whether the $\tau > 20$ is driven by New Orleans, we drop this district and re-run the analysis. The results are very similar to those in Figure 3, suggesting that the pattern of results by threshold level is not driven by New Orleans (see Appendix Figure A4).

To understand the effects of the charter-heavy districts generally, we also estimated effects for the subset of districts with at least one charter school, but not more than 10-percent charter penetration (see Appendix Table A7). Here, we find small but significant positive effect on graduation rate (increase 1-2 percentage points) and positive but imprecise effects on test scores, as expected given the smaller dosage and associated smaller effects in low-charter-share districts shown in Figure 3.[30]

IV.B. Tests for Endogeneity

The above results only address endogeneity caused by time-invariant factors and time-varying observables. In this section, we present results that address endogeneity from time-varying unobserved factors. First, we restrict the control sample to districts in states without charter schools as of 2016 to minimize the potential endogeneity of charter reform in our main results. Table 5 presents these results. The results are very similar to our main results (Table 3)

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[30] As one additional robustness check, we also switched from the charter enrollment share to charter school share as the dependent variable. The results are similar to that of charter enrollment share (see Table A8 in the Appendix), except that the magnitude of estimates is somewhat smaller. It is not obvious which should be preferred. The charter share measure may better reflect the number of TPSs that are under pressure (more small charter schools might be spread across a district, competing with more TPS), but, to the degree that school size is similar across places, the enrollment share (i.e., the number of students who have left) may be a better indicator of effective competitive pressure. We also considered other treatment measures that have been used in prior studies, but these studies have focused just on competition; specifically, competition between individual schools. Given that we are interested in net effects of all mechanisms (participant, competition, and closure) on entire districts, the measures of competition used in prior studies do not seem appropriate here.

discussed above. Second, we restrict the treated sample to early adopter states and the comparison group to late adopter states to minimize potential endogeneity of statewide charter reform in our main results. As shown in Table A9, the results are, again, very similar to Table 3.

Our third approach to testing for endogeneity involves two placebo methods. The above analyses are based on the charter share that aligned to grade levels and we now conduct analyses using charter share for grades that are "misaligned" with the outcomes. Again, the charter entry of elementary schools should not affect the graduation rate at least in the short run, before those students reach the age of potential high school graduation. Table 6 presents the placebo results for the "effect" of elementary grade charter market share on the high school graduation rate and the high school grade market share on elementary Math and ELA scores. These estimates are noticeably smaller (and less precise) than the main results above. Only two of 18 specifications suggest positive effects, but even these estimates are no longer precise when controlling district covariates. Most of the estimates for test scores are negative, suggesting that our main results may be biased downward.

In the other placebo test, we analyze the "effect" of subsequent high charter market share on student outcomes in periods prior to the entry of charters. Specifically, we falsely assume that charter entry occurred two years earlier and redo the analyses.[31] This is similar to the parallel trends test, which we generally passed for graduation and ELA, but not Math. We see this same pattern in the placebo; the placebo again fails for Math.[32]

In addition, we follow Oster (2019) and use the sensitivity of the estimated coefficients to the added controls to assess the potential for bias due to unobservables.[33] We carried out an Oster

[31] We also tried versions with one, three, and four years earlier than charter entry and got similar results.
[32] As Table A10 in the Appendix shows, the "effects" are 0.12-0.19 s.d. for Math and 0.03-0.04 s.d. for ELA.
[33] The Oster Bound 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

bounds analysis for DD and present results in Table A11 in the Appendix. The Oster bounds analysis estimates the treatment effect if selection on unobservables is as important as selection on observables (assume $\tilde{\delta}$= 1 in their model). All of our estimates of Oster bounds are positive and slightly larger in magnitude than our baseline estimates. Further, the observable characteristics included in our data explain more than 80 percent of the variation in the change of student outcomes.[34]

In summary, our analysis of graduation rates passes all of these endogeneity tests. The analysis of ELA and Math scores also pass the majority of these tests, but we have noted some exceptions.

IV.C. Tests for Enrollment and Compositional Changes

Our analysis focuses on data at the geographic district level, i.e., outcomes are a weighted average of those in the TPS and charter schools located within the same school district boundaries. This has the advantage of avoiding the usual problem caused by sorting of enrollment between charter and traditional public schools within districts. However, it is still possible that the timing and location of charter entry could be correlated with compositional changes in the student population across districts.

To address this issue, we conduct several event study analyses of the effects of charter entry on enrollment and its compositional changes. As Figure 4 shows, there are no statistically significant changes in enrollment, or its composition, after charter entry. The results do suggest some possible increases in the percentage of students who are Black and Hispanic and decreases

assume $\tilde{\delta}$= *1* and R_{max}= *1.3*R_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.

[34] 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 estimates in the main text.

in the percentage who are eligible for free or reduced-price lunches. However, the latter do not appear large enough to change the conclusions. The largest effect on FRL suggests a decrease of four percentage points, which we predict would increase test scores by only 0.01 s.d., a small fraction of the main charter entry effect reported above.[35] This reinforces that demographic change cannot explain our results.

IV.D. Fixed Effects Results

We use FE estimates as an additional robustness check. This approach leverages within-district longitudinal variation in the charter market share (within the previously described treatment group). The first two columns of Table 7 use the contemporaneous charter market share and implicitly assume an immediate effect of charter entry on student outcomes. With the graduation rate, the latter two columns average the last four years of charter market share (because it takes four years of data to calculate a single graduation rate). With test scores, the last two columns lag the charter market share by one year, in case there is a delayed effect.

The results, again, indicate a significant effect of charter entry on the graduation rate. The results are similar between the contemporaneous charter market share and four-year averages. A 10 percent increase in average charter enrollment share increases the graduation rate by 1-2 percentage points. Also, as in the DD, we generally see positive effects on student test scores with the FE, and all estimates of Math are statistically significant. Our results are robust when using contemporaneous or lagged charter market shares.

To compare these FE with the DD results, note that the average eventual charter share using the 10 percent threshold is 18.4 percent.[36] We can therefore take the FE coefficient, which

[35] The coefficient on FRL in our DD estimates is about -0.3. This implies that decreasing the FRL percentage by four percentage point would increase test scores by only -0.3*-0.04=0.01 s.d.
[36] Table A12 in Appendix list the average charter share across different models.

captures the effect of increasing the market share by one percentage point and multiply by 18.4 percent the average eventual max share of the DD, which yields 0.03. This is almost identical to the DD point estimates reported in Tables 3. Given that these various methods rest on different assumptions, the robustness of the results reinforces a causal interpretation.

Earlier, in the DD analyses, we reported that a plateauing of the effects at about 15 percent charter market share. One potential explanation is that the DD "turns on" the treatment for each district when the first charter school opens, but it may take many years for districts to reach their maximum charter shares, which we used to assign district treatment status. Therefore, it could be that the effects for larger market share districts reflect their small actual market shares in the early treatment years. The FE analysis is well-suited to test this theory because we leverage within-district variation in actual charter market share. These results reinforce that the idea of a plateau. Using a simple quadratic, we see that the coefficient on the linear term is still consistently positive and that the coefficient on the squared term is consistently (though not uniformly) negative.[37]

V. Effect Heterogeneity

Prior research has suggested that charter school participant effects vary by urbanicity, baseline achievement level, and grade level. Table 8 presents the charter effects in Metropolitan Areas (MA) and non-MA. The results show that the charter effects mostly come from the MA and little effects are detected in non-MA districts. The magnitudes of the coefficients of DD models imply that having a large share of charter schools (ever above 10 percent) increases the graduation rate in MAs by four percentage points and improves Math and ELA scores in MAs by

[37] The results are presented in Table A13 in the Appendix.

0.13-0.21 standard deviations. This pattern is consistent with prior charter school participant effects by urbanicity (CREDO 2013, Chabrier, Cohodes, and Oreopoulos 2016).

The reasons for this form of effect heterogeneity remain speculative. 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 test this by estimating versions of equations (1) and (2) but adding an interaction between treatment and baseline achievement level (see Table A14 in the Appendix). Most of these interaction coefficients are negative (including eight of the nine coefficients from models including covariates), 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.

We also evaluate the heterogeneous effects by school grade levels for test scores and report the results in Table 9. 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, districts with a large share of charter schools (ever above 10 percent) improve Math and ELA scores of middle schools by 0.11-0.22 standard deviations.

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).[38] Results in Table 10 suggests that charter schools in states with higher scores from NAPCS do more to improve student outcomes relative to those in states with lower scores.

VI. Mechanisms of the Total Charter Effect

The above analyses estimate the net 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 (1995-2016) using the TPS school closure measure created by Harris and Martinez-Pabon (2021). A closed TPS occurs when the school building is no longer used as a school. We therefore switch the dependent variable in the above DD analyses from test scores and graduation rates to closure.

[38] 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.

Table 11 presents the results.[39] We find increases in TPS closure when charters enter. The magnitudes of the coefficients imply that having a charter enrollment share ever above 10 percent increases the school closure rate by about 0.5 percentage points (a 40 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 there are positive but mostly insignificant placebo effects, especially when controlling district covariates, reinforcing that the main estimates reflect causal effects.

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).[40]

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. The practical effects of these

[39] Figure A5 in the Appendix presents the event study result of charter effects on TPSs closure.
[40] 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.

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.

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 from a social welfare perspective. 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.[41] However, Chakrabarti and Roy (2016) found no causal evidence that charter schools in Michigan led to declines in private school enrollment.[42]

We focus on the districts that have at least one private school at baseline, and some small districts without any private schools are not included in the analysis.[43] The sample period is

[41] This was especially true in urban districts.
[42] They used a fixed-effects model as well as an instrumental variables strategy that exploits exogenous variation from Michigan charter law.
[43] About 4,588 school districts have one private school during the sample period.

1996-2016 and note that the private school data are only available biannually. We generally find no significant effects of charter entry on the share of private schools' enrollment as Table A15 in the Appendix presents.[44] 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.

IV.C. Other Mechanisms

Our data only allow suggestive evidence regarding participant and competition effects, which we present briefly. To understand the participant effects, we identified the single nearest TPS to every charter serving the same grades. The average SEDA growth measure for these nearest TPS schools is 0.0018, compared with the overall district average of 0.0055, in districts that have charter schools. In other words, it appears that charter schools tend to locate near TPS that are slightly lower performing, which likely contributes to the net positive effects reported earlier. The average SEDA growth measure of charter schools (0.0167) and is also larger than that for the nearest TPS schools (0.0018), implying a participant effect of 0.0149 s.d. in annual growth. We note that this figure is very similar to what CREDO (2013) found using microdata and analysis of "virtual twins." This may be an upper-bound in the present context, however. While prior research suggests that school value-added measures generally have limited bias on average (Deming 2014, Angrist et al. 2017), Reardon et al. (2019) warn that their charter school measures are upwardly biased relative to TPS. In the additional personal communication (Kilbride, 2021), the SEDA charter-TPS bias appears to be 0.018 s.d., which would make the participant effect almost zero.

[44] One specification shows statistically significant reductions in private school enrollment, but that effect disappears when including district controls as column (4) shows. We also carried out a placebo analysis in Panel B and Panel C, which reinforces that the effect is null.

The school-level SEDA growth measures can also be used to gauge competitive effects. We would expect that having a larger charter market share would create more competition and do more to increase performance of TPS. Focusing on those school districts with both charter schools and TPS, we therefore take the weighted average achievement growth among TPS *only* in each district and do the same for charters and present a scatter plot of achievement growth for each sector by district charter share.[45] We then create a linear fit (linear, quadratic, and cubic) to gauge the average slope and then to test for non-linearities.

We are mainly interested in the slope of the TPS line. If competition is occurring, then we expect more charter schools to be associated with improved TPS growth. That slope is relatively flat, but generally upward sloping, as predicted.[46] This suggests that the competitive effect is relatively small. For example, if we take the fitted linear fit line literally, going from zero to 50 percent charter market share would increase annual TPS achievement growth by 0.0031 s.d.. This is consistent with the prior literature, which also finds small competitive effects using rigorous methods applied to smaller samples. Again, this evidence on participant and competitive effects is only descriptive and our main contribution with respect to mechanisms is with respect to school closures.

VII. Conclusion

This study makes many new contributions to the research on charter schools and market-based school reforms generally. First, we provide the first analysis of the total combined effect of charter school entry on student outcomes. Using the DD strategy combined with various methods

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[45] Charter share is the average charter enrollment share during the sample period. The linear fitted line in Figure 5(a) is higher for charter schools than TPSs at all market shares (consistent with our earlier results), reflecting the previously described positive participant effect.

[46] In Figure 5, the slope of achievement growth for charter schools (red line in Figure 5a) is 0.0112, whereas that for TPSs (blue line in Figure 5a) is 0.0063.

for addressing endogeneity, our analysis suggests that districts with more than 10 percent charters have increased elementary and middle school test scores by 0.06-0.16 standard deviations.

Second, we also 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 increased charter market share above 10 percent increases high school graduation rate by about 2-4 percentage points. This is important given how strongly predictive high school graduation is for long-term life outcomes.

Third, this is the first study to our knowledge to identify, and simultaneously study and compare, all of the mechanisms of the effects of charter entry: participant, competitive, and closure 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 just as important as estimating the effects themselves.

Fourth, we provide one of the first studies of charter schools using a sample that includes almost half of all charter schools, and nearly all TPS, in the country. CREDO (2013) is the other national study. National analysis is important given the increasing national, and federal, debates about charter schools.

Fifth, 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 suggestive evidence that charter effects are larger in lower-performing districts. The effects also seem to be larger in middle schools.

Sixth, 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 that noticeable effects begin emerging at 5 percent charter market share and rise and plateau at about 15 percent market share.

Charter school has been arguably the most influential school reform efforts of the past several decades. Informing these ongoing debates, our analysis suggests that charter schools improve student two important outcomes: test scores and high school graduation. Understanding welfare effects also requires, however, understanding when, how, and for whom charter school effects operate. Looking at data from across the country, our analysis makes many contributions toward end.

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Table 1 Summary Statistics

Sample	Graduation rate (1995-2010)						Math & ELA (2009-2016)					
	ALL	DD & DD-PSW		DD-PSM		Fixed Effects	ALL	DD & DD-PSW		DD-PSM		Fixed Effects
		Treated	Control	Treated	Control			Treated	Control	Treated	Control	
Graduation rate	0.75	0.63	0.78	0.63	0.69	0.67	NA	NA	NA	NA	NA	NA
Math	NA	NA	NA	NA	NA	NA	0.00	-0.51	0.13	-0.51	-0.19	-0.40
ELA	NA	NA	NA	NA	NA	NA	0.00	-0.46	0.12	-0.46	-0.20	-0.36
White	65%	30%	71%	30%	33%	43%	56%	30%	63%	30%	46%	36%
Black	16%	23%	14%	23%	37%	22%	16%	23%	14%	23%	19%	23%
Hispanic	14%	38%	10%	38%	22%	26%	22%	42%	18%	42%	27%	34%
FRL	30%	49%	27%	49%	40%	37%	51%	65%	48%	65%	53%	61%
Special education	13%	12%	13%	12%	12%	12%	13%	12%	13%	12%	12%	12%
Ages 5–17 population	16%	24%	15%	24%	20%	19%	19%	23%	18%	23%	19%	22%
Ages 5–17 in poverty	19%	18%	19%	18%	19%	18%	17%	16%	18%	16%	17%	17%
Urban	27%	73%	22%	73%	60%	45%	27%	45%	22%	45%	40%	42%
Suburb	42%	21%	41%	21%	31%	48%	45%	46%	44%	46%	49%	51%
Town	11%	3%	13%	3%	6%	3%	12%	6%	15%	6%	7%	4%
Rural	20%	3%	24%	3%	4%	3%	17%	4%	21%	4%	7%	4%
Revenue per student	8,970	8,900	9,057	8,900	9,448	8,685	12,125	11,438	12,375	11,438	11,351	11,344
Expenditure per student	9,132	9,172	9,215	9,172	9,701	8,861	12,141	11,697	12,356	11,697	11,363	11,467
Teacher salary	79,686	91,449	78,534	91,449	88,776	83,465	99,038	102,014	98,660	102,014	103,108	100,225
Student teacher ratio	17	19	16	19	17	18	16	18	16	18	19	18
No. magnet school	5	52	1	52	4	17	6	35	1	35	6	19
No. public schools	61	311	29	311	135	168	62	290	25	290	79	179
Enrollment	46,690	264,722	18,961	264,722	89,521	137,604	42,851	198,433	16,748	198,433	55,331	124,652
Observation	144,219	2,193	137,780	2,193	2,199	6,439	410,077	10,109	388,064	10,109	11,073	22,013
N (district)	9,278	142	8,859	142	142	416	10,439	297	9,832	297	297	607

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. Treated group refers to the sample of districts that charter enrollment share ever above 10 percent during the sample period. Comparison group refers to the sample of districts without charter schools in all states for DD & DD-PSW, and it refers to the sample of matched districts (nearest neighbor) for DD-PSM. Fixed Effects sample refers to all districts with any charter share in the sample period. Data source: National Longitudinal School Database.

Table 2 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 3 Effects of charter entry on student outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
	DD		DD-PSW		DD-PSM	
Panel A: Graduation rate						
Treated*Post	0.037***	0.038***	0.034***	0.028***	0.039***	0.031***
	[0.009]	[0.011]	[0.011]	[0.009]	[0.010]	[0.010]
R-squared	0.805	0.808	0.817	0.825	0.886	0.894
Observations	139,973	139,973	139,973	139,973	4,383	4,383
N (district)	9,001	9,001	9,001	9,001	284	284
Pre-treatment	0.000	0.001	-0.001	-0.001	-0.001	-0.002
trend	[0.001]	[0.001]	[0.001]	[0.001]	[0.002]	[0.002]
Panel B: Math						
Treated*Post	0.152***	0.155***	0.154***	0.148***	0.136***	0.157***
	[0.040]	[0.043]	[0.055]	[0.052]	[0.048]	[0.051]
R-squared	0.846	0.847	0.863	0.865	0.869	0.873
Observations	398,173	398,173	398,173	398,173	21,007	21,007
N (district)	10,129	10,129	10,129	10,129	594	594
Pre-treatment	0.085***	0.088***	0.023	0.023	0.074***	0.073***
trend	[0.018]	[0.016]	[0.024]	[0.020]	[0.020]	[0.018]
Panel C: ELA						
Treated*Post	0.116***	0.121***	0.075	0.077*	0.075*	0.106**
	[0.035]	[0.038]	[0.049]	[0.045]	[0.043]	[0.046]
R-squared	0.882	0.883	0.884	0.886	0.914	0.917
Observations	398,173	398,173	398,173	398,173	21,007	21,007
N (district)	10,129	10,129	10,129	10,129	594	594
Pre-treatment	0.026***	0.029***	-0.025	-0.019	0.009	0.011
trend	[0.010]	[0.008]	[0.023]	[0.019]	[0.012]	[0.011]
District, (grade) & year FE	Yes	Yes	Yes	Yes	Yes	Yes
District control	No	Yes	No	Yes	No	Yes

Notes: The table shows DD estimates of the effects of the charter entry (enrollment share ever above 10%) on student outcomes. The treated group includes districts with charter enrollment share ever above 10%, and the comparison group includes districts without charter schools in all states; post in an indicator of the period after districts started first charter school. Controls include the log of district enrollment; the share of students who are Hispanic, black, white; the share of students who are in special education programs; the share of students on FRL programs; student-teacher ratio; average teacher salary; the number of magnet school; the total number of schools; the total revenue per student; the total expenditure per student; and whether the district is in an urban, suburban, town, or rural location; the estimate of the school-age population; the estimate poverty rate of the school-age population. Robust standard errors presented in parentheses are clustered at the district level. For DD and DD-PSM, regressions are weighted by high school enrollment for graduation rate and grade-level enrollment for Math and ELA; For DD-PSW, regressions are weighted by weight of DD times the inverse probability of propensity score.

Table 4 Weights and Estimates from the Goodman-Bacon (2021) Decomposition

	Weight	Estimate (1)	Estimate (2)
Panel A: Graduation rate			
Earlier Treated vs. Later Control	0.154	0.015	0.033
Later Treated vs. Earlier Control	0.116	0.022	0.049
Treated vs. Never Treated	0.683	0.011	0.024
Treated vs. Already Treated	0.047	0.028	0.062
Overall DD Estimate		0.014	0.031
Panel B: Math			
Earlier Treated vs. Later Control	0.023	-0.226	-0.645
Later Treated vs. Earlier Control	0.039	-0.018	-0.051
Treated vs. Never Treated	0.585	0.045	0.128
Treated vs. Already Treated	0.353	0.099	0.283
Overall DD Estimate		0.055	0.157
Panel C: ELA			
Earlier Treated vs. Later Control	0.023	-0.063	-0.142
Later Treated vs. Earlier Control	0.039	-0.006	-0.014
Treated vs. Never Treated	0.585	0.027	0.061
Treated vs. Already Treated	0.353	0.094	0.212
Overall DD Estimate		0.047	0.106
Sample weight, Covariates & FE		No	Yes

Notes: The Goodman-Bacon (2021) decomposition above displays the weights and components making up the overall DD estimates of charter entry on student outcomes. Decompositions are documented for graduation rate (panels A), Math (panels B), and for ELA (panels C) using the DD-PSM sample. Column (1) doesn't include sample weight, districts covariates, district fixed effects and year fixed effects while Column (2) does. Column (2) present the same estimates as Column (6) in Table 3. For the decomposition, each components' weight is given along with the point estimate for this comparison. The overall DD estimate is displayed at the foot of each panel.

Table 5 DD estimates: alternative comparison group (states no charter law)

	(1)	(2)	(3)	(4)	(5)	(6)
	DD		DD-PSW		DD-PSM	
Panel A: Graduation rate						
Treated*Post	0.041***	0.035***	0.034***	0.030***	0.037***	0.028***
	[0.009]	[0.010]	[0.013]	[0.011]	[0.010]	[0.011]
R-squared	0.859	0.867	0.808	0.823	0.861	0.874
Observations	13,948	13,948	13,948	13,948	4,420	4,420
N (district)	895	895	895	895	284	284
Pre-treatment trend	0.001	0.002	0.000	0.000	0.000	0.001
	[0.001]	[0.001]	[0.001]	[0.001]	[0.002]	[0.002]
Panel B: Math						
Treated*Post	0.129**	0.154***	0.183***	0.166***	0.123**	0.151***
	[0.051]	[0.047]	[0.057]	[0.055]	[0.052]	[0.048]
R-squared	0.806	0.812	0.865	0.869	0.820	0.826
Observations	32,128	32,128	32,128	32,128	21,481	21,481
N (district)	887	887	887	887	594	594
Pre-treatment trend	0.081***	0.084***	0.002	0.017	0.075***	0.083***
	[0.021]	[0.021]	[0.027]	[0.019]	[0.022]	[0.023]
Panel C: ELA						
Treated*Post	0.076*	0.101**	0.106**	0.102**	0.075	0.107**
	[0.044]	[0.042]	[0.051]	[0.046]	[0.046]	[0.043]
R-squared	0.868	0.872	0.874	0.877	0.883	0.887
Observations	32,128	32,128	32,128	32,128	21,481	21,481
N (district)	887	887	887	887	594	594
Pre-treatment trend	0.045***	0.044***	-0.025	-0.003	0.042***	0.043**
	[0.012]	[0.017]	[0.026]	[0.015]	[0.013]	[0.019]
District, (grade) & year FE	Yes	Yes	Yes	Yes	Yes	Yes
District control	No	Yes	No	Yes	No	Yes

Notes: The table shows DD estimates use an alternative comparison group. The comparison group includes districts in states without charter law as of the last year of the sample (2016). See notes of Table 3 for controls, clusters, and sample weight.

Table 6 DD estimates: Placebo grade level charter share

	(1)	(2)	(3)	(4)	(5)	(6)
	DD		DD-PSW		DD-PSM	
Panel A: Graduation rate						
Treated*Post	0.016*	0.013	0.001	-0.009	0.017*	0.012
	[0.008]	[0.009]	[0.012]	[0.016]	[0.009]	[0.009]
R-squared	0.816	0.819	0.726	0.748	0.877	0.881
Observations	144,112	144,112	144,112	144,112	4,623	4,623
N (district)	9,268	9,268	9,268	9,268	300	300
Pre-treatment	0.000	-0.001	0.001	0.000	-0.002	-0.003*
trend	[0.001]	[0.002]	[0.001]	[0.001]	[0.002]	[0.002]
Panel B: Math						
Treated*Post	-0.091	-0.084	-0.022	-0.049	-0.102*	-0.093*
	[0.056]	[0.053]	[0.059]	[0.048]	[0.058]	[0.055]
R-squared	0.851	0.852	0.872	0.875	0.877	0.880
Observations	409,129	409,129	409,129	409,129	24,268	24,268
N (district)	10,414	10,414	10,414	10,414	662	662
Pre-treatment	-0.006	-0.001	0.000	-0.009	-0.012	-0.018
trend	[0.013]	[0.012]	[0.013]	[0.009]	[0.014]	[0.013]
Panel C: ELA						
Treated*Post	-0.006	-0.001	0.008	-0.009	-0.051	-0.034
	[0.048]	[0.046]	[0.059]	[0.052]	[0.050]	[0.048]
R-squared	0.887	0.888	0.895	0.897	0.917	0.919
Observations	409,129	409,129	409,129	409,129	24,268	24,268
N (district)	10,414	10,414	10,414	10,414	662	662
Pre-treatment	0.010	0.013	0.006	0.006	0.001	0.006
trend	[0.014]	[0.014]	[0.013]	[0.011]	[0.015]	[0.015]
District, (grade) & year FE	Yes	Yes	Yes	Yes	Yes	Yes
District control	No	Yes	No	Yes	No	Yes

Notes: The table shows DD estimates using placebo grade levels of charter share: elementary school grade levels (1-5) for graduation rate, and high school grade levels (9-12) for Math and ELA. See notes of Table 3 for controls, clusters, and sample weight.

Table 7 Estimates of fixed effects model for charter effects on student outcomes

	(1)	(2)	(3)	(4)
	Panel A: Graduation rate			
	Same year share		Average last four years	
Charter share	0.130*	0.159***	0.115	0.125**
	[0.074]	[0.052]	[0.076]	[0.060]
R-squared	0.852	0.860	0.863	0.871
Observations	6,439	6,439	5,191	5,191
N(district)	416	416	416	416
	Panel B: Math			
	Same year share		Same cohort last year	
Charter share	0.390***	0.339***	0.266**	0.211*
	[0.143]	[0.116]	[0.107]	[0.114]
R-squared	0.854	0.858	0.877	0.878
Observations	22,013	22,013	13,728	13,728
N(district)	607	607	604	604
	Panel C: ELA			
	Same year share		Same cohort last year	
Charter share	0.195	0.043	0.138	0.062
	[0.129]	[0.107]	[0.103]	[0.102]
R-squared	0.909	0.912	0.922	0.924
Observations	22,013	22,013	13,728	13,728
N(district)	607	607	604	604
District, (grade) & year FE	Yes	Yes	Yes	Yes
District control	No	Yes	No	Yes

Notes: The table shows estimates of fixed effects model for charter effects on student outcomes for districts with any charter schools during the sample period. In columns (1) and (2), charter enrollment share of grades 9-12 is used for graduation rate, and charter enrollment share of grades 3-8 is used for test scores. In columns (1) and (2), the average last four years 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. See notes of Table 3 for controls and clusters. Regressions are weighted by high school enrollment for graduation rate and grade-level enrollment for Math and ELA.

Table 8 Effect heterogeneity: metropolitan areas VS non-metropolitan areas

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
		Graduation rate			Math			ELA	
	DD	DD-PSW	DD-PSM	DD	DD-PSW	DD-PSM	DD	DD-PSW	DD-PSM
Panel A Metropolitan areas									
Treated*Post	0.042***	0.042***	0.040***	0.170***	0.210***	0.162***	0.137***	0.130***	0.132***
	[0.011]	[0.010]	[0.011]	[0.045]	[0.056]	[0.052]	[0.040]	[0.036]	[0.047]
R-squared	0.838	0.865	0.909	0.873	0.887	0.865	0.907	0.907	0.912
Observations	67,462	67,462	2,424	228,532	228,532	14,743	228,532	228,532	14,743
N(district)	4,355	4,355	158	5,848	5,848	432	5,848	5,848	432
Pre-treatment	0.000	0.000	-0.001	0.101***	0.058***	0.087***	0.035***	-0.002	0.028***
trend	[0.001]	[0.001]	[0.002]	[0.016]	[0.018]	[0.019]	[0.006]	[0.023]	[0.010]
Panel B Non-metropolitan areas									
Treated*Post	-0.015	-0.007	-0.008	0.009	-0.039	-0.007	-0.028	-0.058	0.004
	[0.011]	[0.010]	[0.012]	[0.122]	[0.085]	[0.105]	[0.090]	[0.089]	[0.091]
R-squared	0.679	0.709	0.664	0.676	0.749	0.746	0.719	0.775	0.799
Observations	72,511	72,511	1,955	169,641	169,641	6,182	169,641	169,641	6,182
N(district)	4,646	4,646	126	4,281	4,281	162	4,281	4,281	162
Pre-treatment	0.000	-0.002	-0.004	-0.017	-0.016	-0.006	-0.033**	-0.031	-0.040
trend	[0.002]	[0.002]	[0.003]	[0.024]	[0.019]	[0.028]	[0.015]	[0.020]	[0.025]
District, (grade) & year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table shows DD estimates of heterogeneous effects of charter entry on student outcomes by (non-)metropolitan areas. See notes of Table 3 for controls, clusters, and sample weight.

Table 9 Effect heterogeneity: middle school VS elementary school

	(1)	(2)	(3)	(4)	(5)	(6)
	Math			ELA		
	DD	DD-PSW	DD-PSM	DD	DD-PSW	DD-PSM
Panel A: Middle school (Grade 6-8)						
Treated*Post	0.215***	0.131*	0.211***	0.117***	0.040	0.109**
	[0.048]	[0.076]	[0.054]	[0.042]	[0.054]	[0.048]
R-squared	0.888	0.898	0.925	0.903	0.901	0.936
Observations	183,088	183,088	8,897	183,088	183,088	8,897
N(district)	9,896	9,896	600	9,896	9,896	600
Pre-treatment trend	0.035*	-0.037	0.019	0.006	-0.041**	-0.019
	[0.020]	[0.023]	[0.024]	[0.015]	[0.020]	[0.014]
Panel B: Elementary school (Grade 3-5)						
Treated*Post	0.078	0.060	0.073	0.102**	0.045	0.091
	[0.053]	[0.071]	[0.062]	[0.047]	[0.045]	[0.058]
R-squared	0.860	0.878	0.888	0.902	0.910	0.938
Observations	216,913	216,913	11,699	216,913	216,913	11,699
N(district)	10,002	10,002	556	10,002	10,002	556
Pre-treatment trend	0.125***	0.052***	0.095***	0.048***	0.027	0.023*
	[0.020]	[0.018]	[0.023]	[0.010]	[0.019]	[0.012]
District, grade & year FE	Yes	Yes	Yes	Yes	Yes	Yes
District control	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table shows DD estimates of heterogeneous effects of charter entry on student test scores by middle (elementary) schools. See notes of Table 3 for controls, clusters, and sample weight.

Table 10 Effect heterogeneity: state charter law score

	(1)	(2)	(3)	(4)	(5)	(6)
	DD		DD-PSW		DD-PSM	
Panel A: Graduation rate						
Treated*Score *Post	0.058***	0.057***	0.057***	0.044***	0.060***	0.046***
	[0.016]	[0.018]	[0.021]	[0.017]	[0.017]	[0.017]
R-squared	0.807	0.810	0.819	0.826	0.886	0.895
Observations	126,285	126,285	126,285	126,285	4,336	4,336
N (district)	8,122	8,122	8,122	8,122	281	281
Pre-treatment trend	0.000	0.000	0.000	0.000	0.000	0.000
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Panel B: Math						
Treated*Score *Post	0.256***	0.257***	0.257***	0.247***	0.216***	0.249***
	[0.062]	[0.067]	[0.096]	[0.089]	[0.075]	[0.081]
R-squared	0.848	0.849	0.865	0.867	0.868	0.872
Observations	363,534	363,534	363,534	363,534	20,254	20,254
N (district)	9,261	9,261	9,261	9,261	577	577
Pre-treatment trend	0.003***	0.003***	0.001*	0.001**	0.002***	0.002***
	[0.001]	[0.000]	[0.001]	[0.001]	[0.001]	[0.000]
Panel C: ELA						
Treated*Score *Post	0.182***	0.185***	0.110	0.112	0.117*	0.162**
	[0.055]	[0.061]	[0.090]	[0.081]	[0.067]	[0.074]
R-squared	0.887	0.889	0.887	0.889	0.915	0.918
Observations	363,534	363,534	363,534	363,534	20,254	20,254
N (district)	9,261	9,261	9,261	9,261	577	577
Pre-treatment trend	0.001***	0.001***	-0.001	-0.001	0.000	0.000
	[0.000]	[0.000]	[0.001]	[0.001]	[0.000]	[0.000]
District, (grade) & year FE	Yes	Yes	Yes	Yes	Yes	Yes
District control	No	Yes	No	Yes	No	Yes

Notes: The table shows results for heterogeneous effects by state charter law score by interacting charter share with the state charter law score. *Score* 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 *Score* is one and the lowest value of *Score* is zero. See notes of Table 3 for controls, sample weight, and clusters.

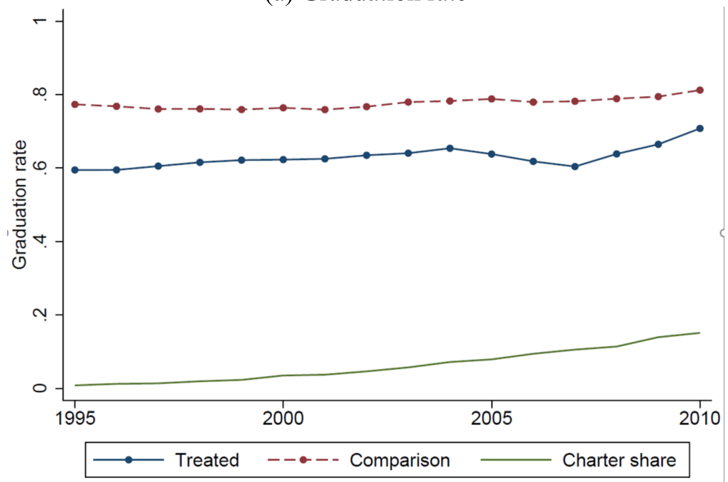
Table 11 Effects of charter entry on TPS closure

	(1)	(2)	(3)	(4)	(5)	(6)
	DD		DD-PSW		DD-PSM	
Panel A: district charter share on all TPS closure						
Treated*Post	0.005***	0.006***	0.005**	0.005***	0.003**	0.004***
	[0.001]	[0.001]	[0.002]	[0.002]	[0.001]	[0.001]
R-squared	0.088	0.098	0.084	0.097	0.112	0.143
Observations	257,368	257,347	257,347	257,347	40,696	40,696
N(district)	11,725	11,725	11,725	11,725	1,852	1,852
Pre-treatment	0.000***	0.000	0.001**	0.000	0.000**	0.000
trend	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Panel B: high school charter share on elementary TPS closure						
Treated*Post	0.005*	0.003	0.002	0.002	0.003	0.002
	[0.003]	[0.003]	[0.002]	[0.003]	[0.002]	[0.002]
R-squared	0.070	0.086	0.075	0.093	0.089	0.257
Observations	260,012	259,991	259,991	259,991	8,611	8,611
N (district)	11,846	11,846	11,846	11,846	392	392
Pre-treatment	0.000	0.000	0.000	0.000	0.000	0.000
trend	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Panel C: elementary charter share on high school TPS closure						
Treated*Post	0.001**	0.001*	0.001	0.001	0.001*	0.000
	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]
R-squared	0.078	0.081	0.075	0.078	0.086	0.112
Observations	263,169	263,148	263,148	263,148	20,163	20,163
N (district)	11,989	11,989	11,989	11,989	918	918
Pre-treatment	0.000	0.000	0.000	0.000	0.000*	0.000*
trend	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
District, (grade) & year FE	Yes	Yes	Yes	Yes	Yes	Yes
District control	No	Yes	No	Yes	No	Yes

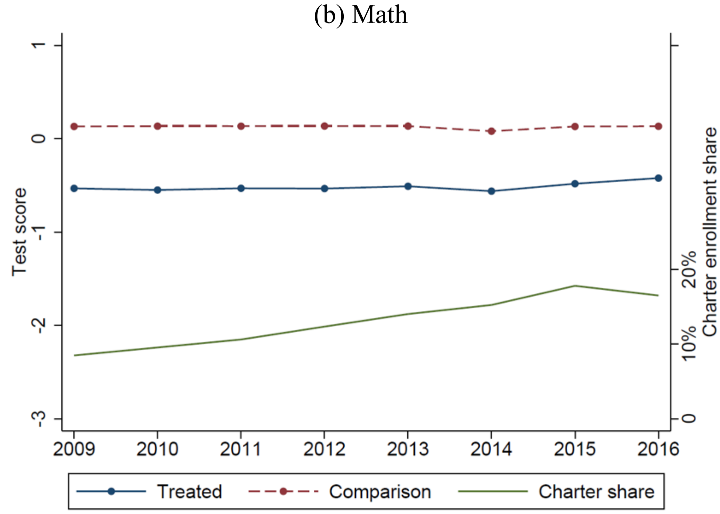
Notes: This table presents DD estimates of the effects of the charter entry (enrollment share ever above 10%) on the share of TPS closure. The sample period is from the year 1995 to 2016. For DD and DD-PSM, regressions are weighted by the total number of schools; For DD-PSW, regressions are weighted by the weight of DD times the inverse probability of propensity score. See notes of Table 3 for controls and clusters.

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

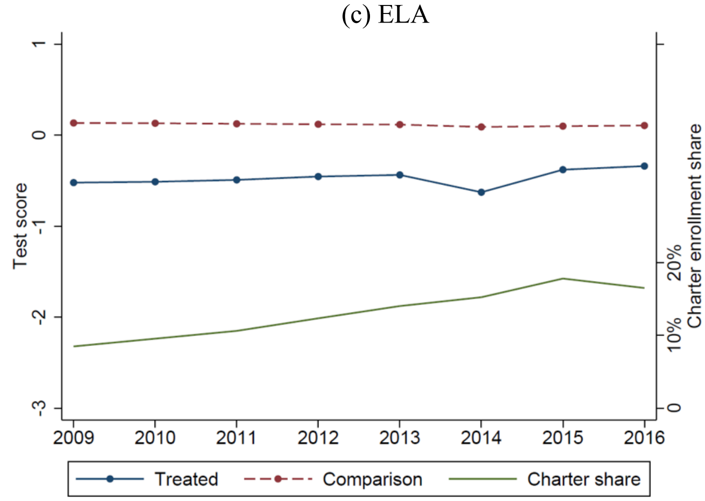
(a) Graduation rate



(b) Math

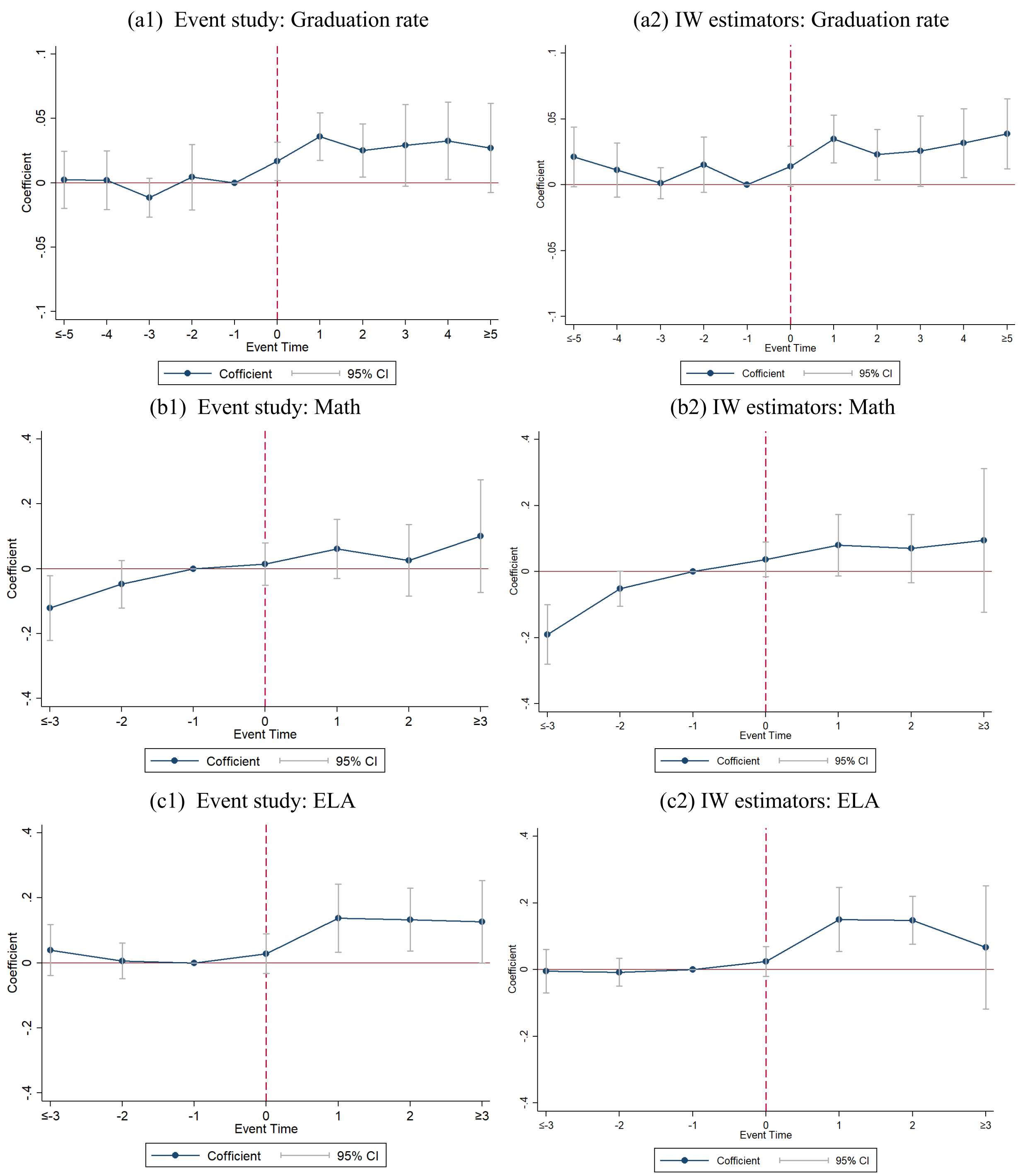


(c) ELA



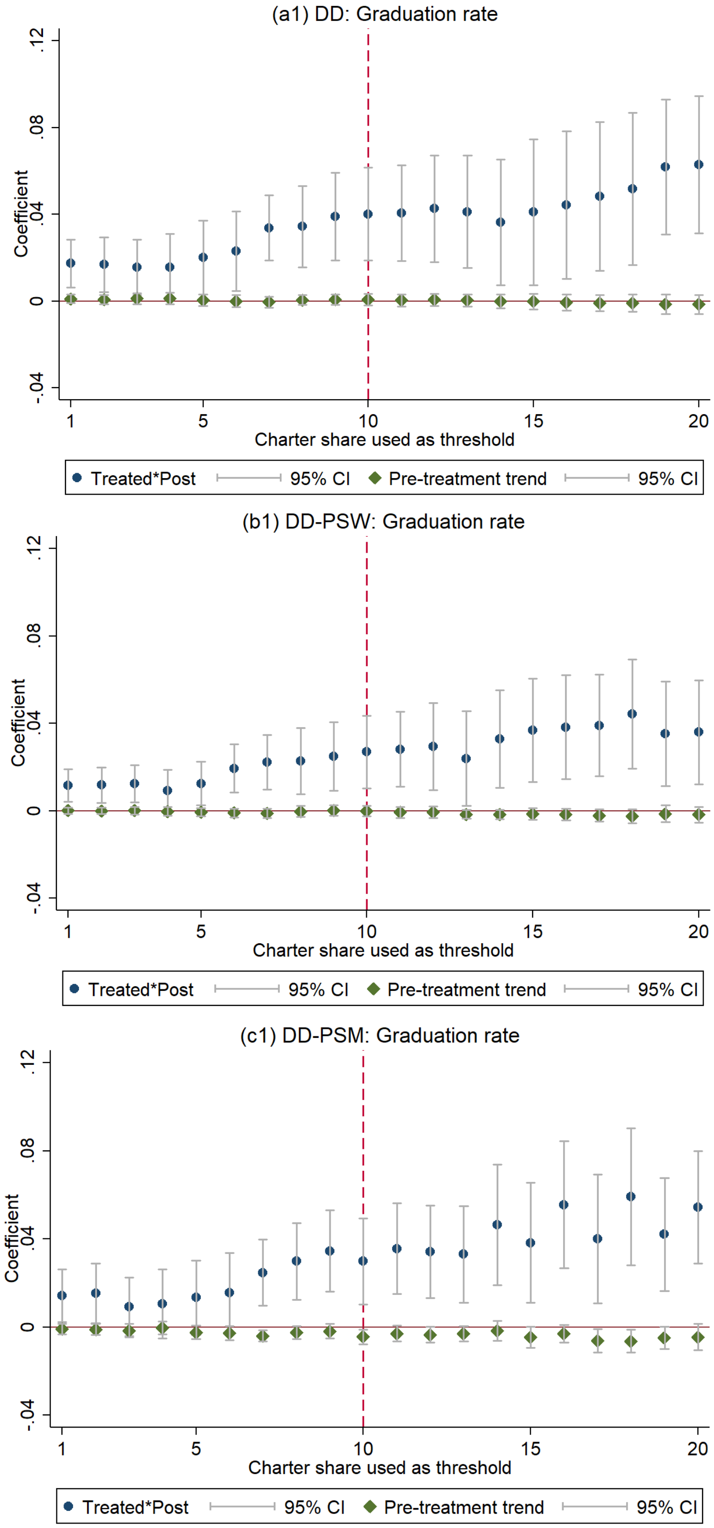
Notes: This figure plots the trends in Graduation rate, Math & ELA of treated districts (in the solid line), and comparison districts (in the dashed line). The green solid line plots the charter enrollment share of treated districts.

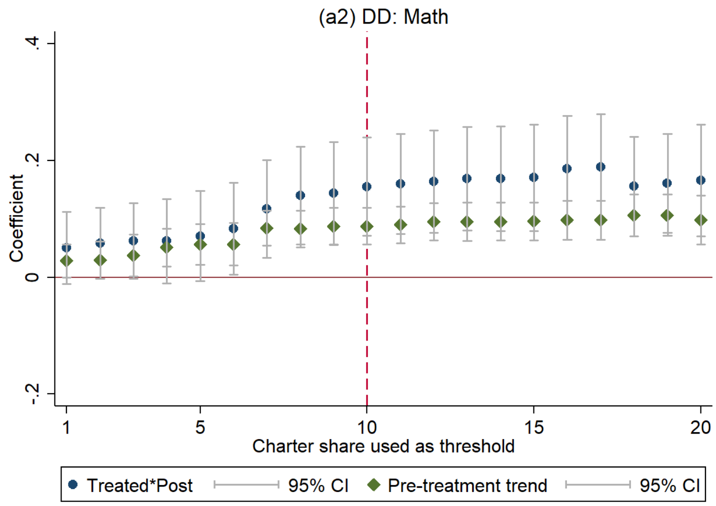
Figure 2 Event study results

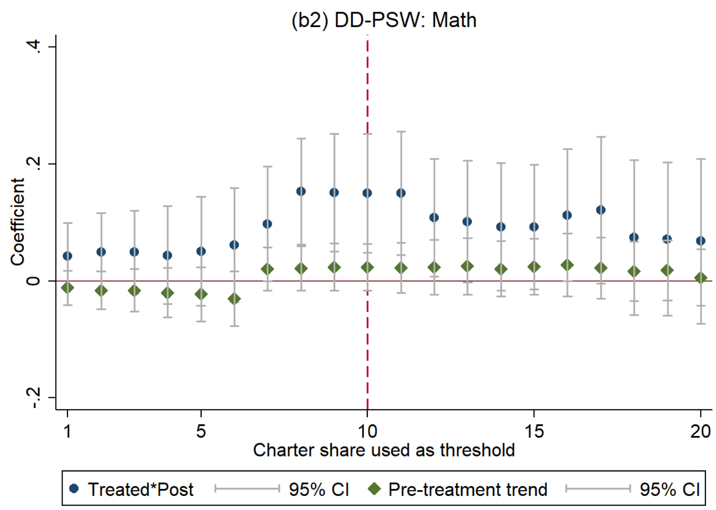


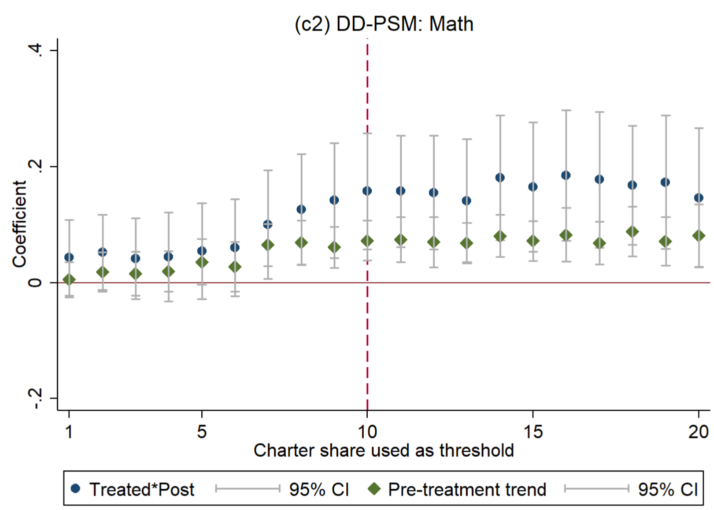
Notes: This figure presents results of event study and IW estimators for graduation rate, Math, and ELA. The event time zero is the first year of charter entry.

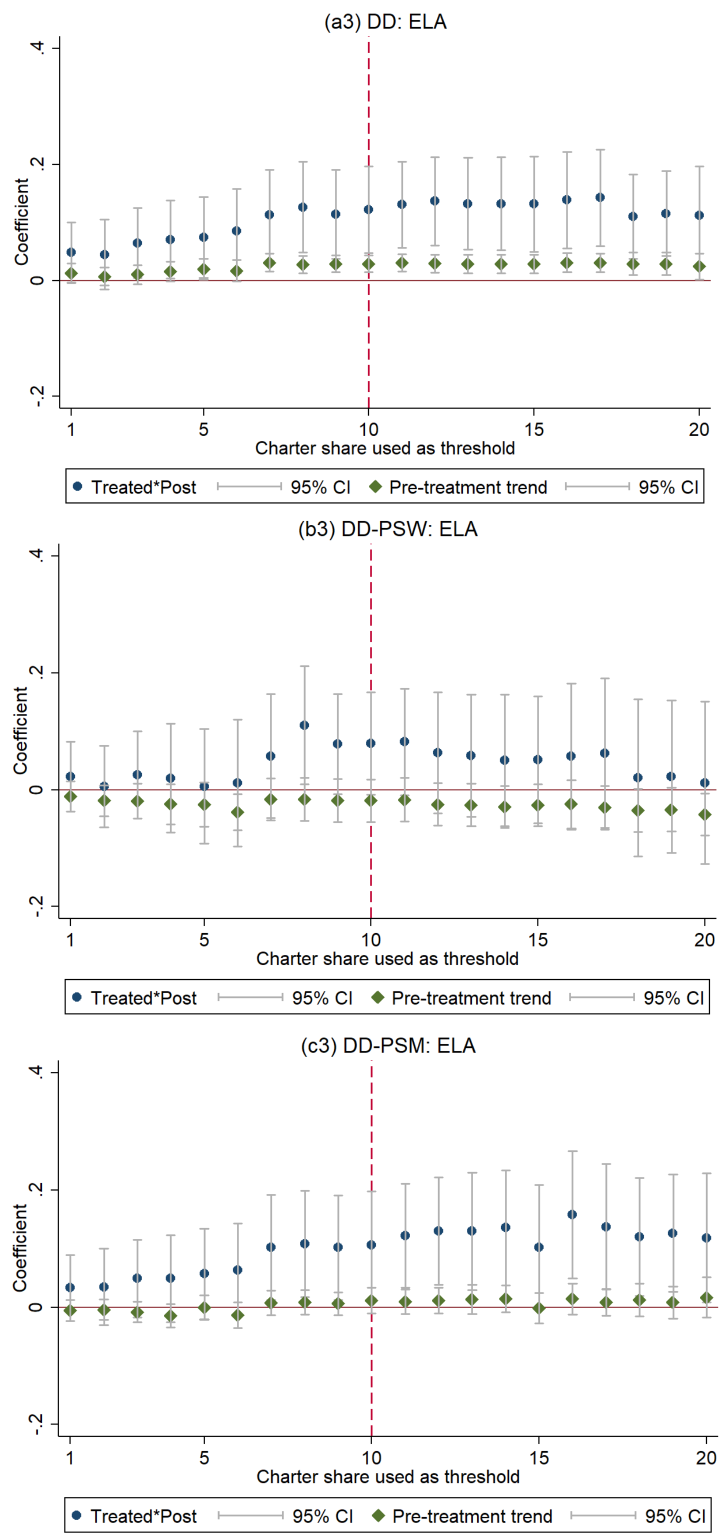
Figure 3 Plots of estimates using 1% to 20% as the threshold of treated districts




(a2) DD: Math
Coefficient
-.2
0
.2
.4
1
5
10
15
20
Charter share used as threshold
Treated*Post
95% CI
Pre-treatment trend
95% CI

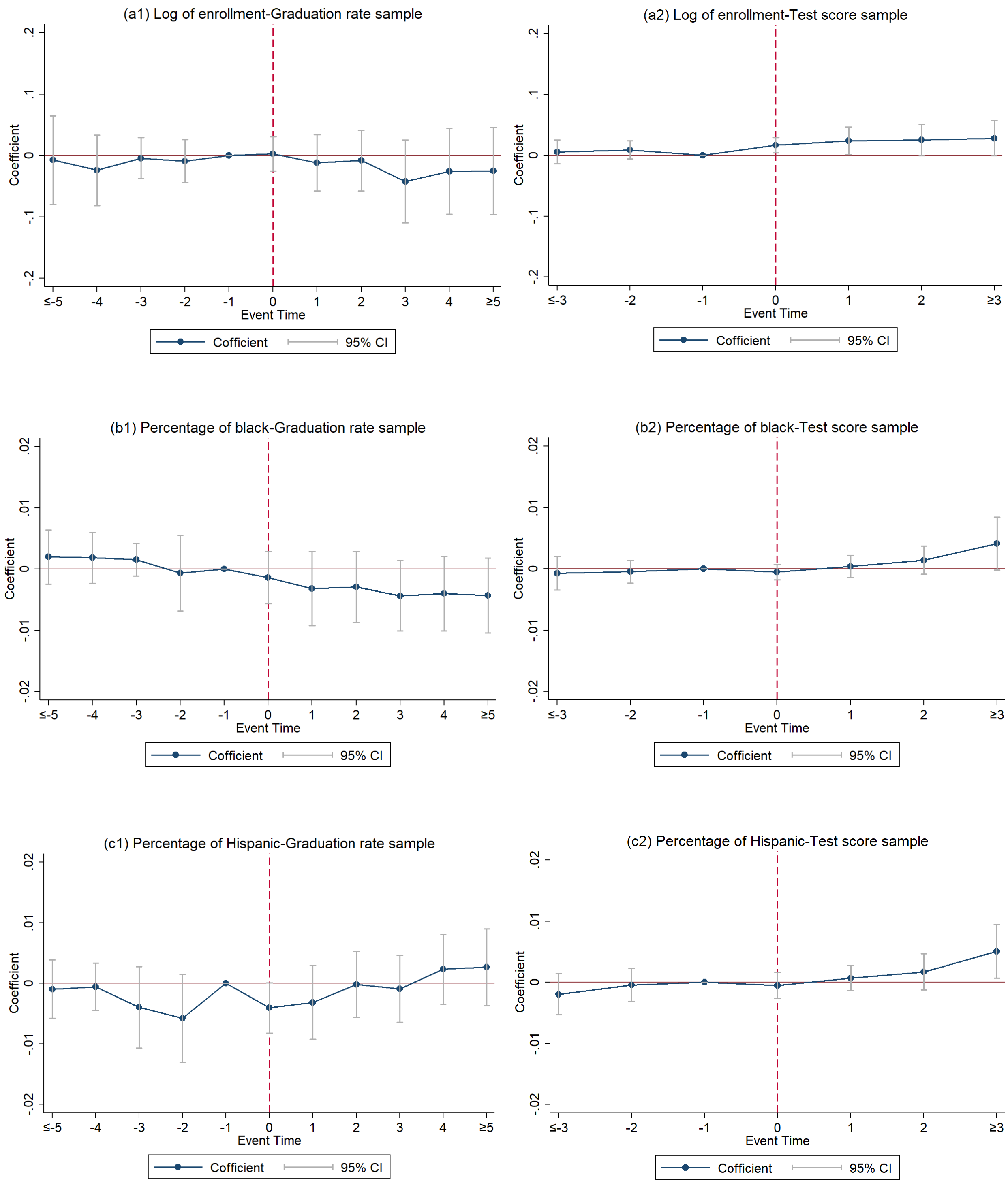

(b2) DD-PSW: Math
Coefficient
-.2
0
.2
.4
1
5
10
15
20
Charter share used as threshold
Treated*Post
95% CI
Pre-treatment trend
95% CI

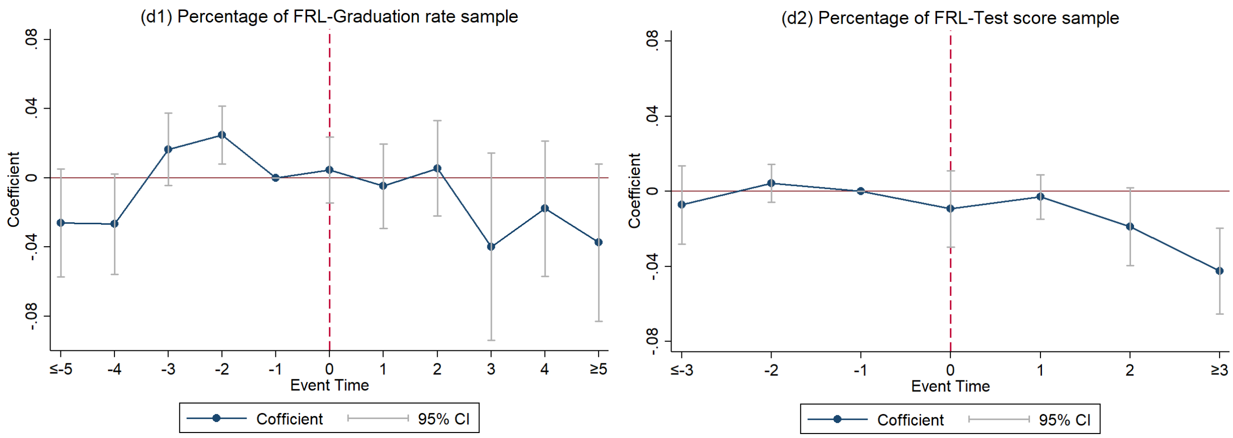

(c2) DD-PSM: Math
Coefficient
-.2
0
.2
.4
1
5
10
15
20
Charter share used as threshold
Treated*Post
95% CI
Pre-treatment trend
95% CI



Notes: This figure plots the estimates in graduation rate, Math & ELA using the charter enrollment share of 1- 20% as the threshold of treated districts.

Figure 4 Event study results of enrollment and its compositions

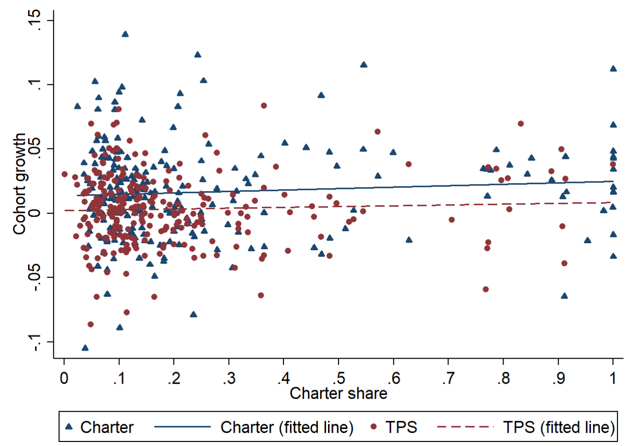




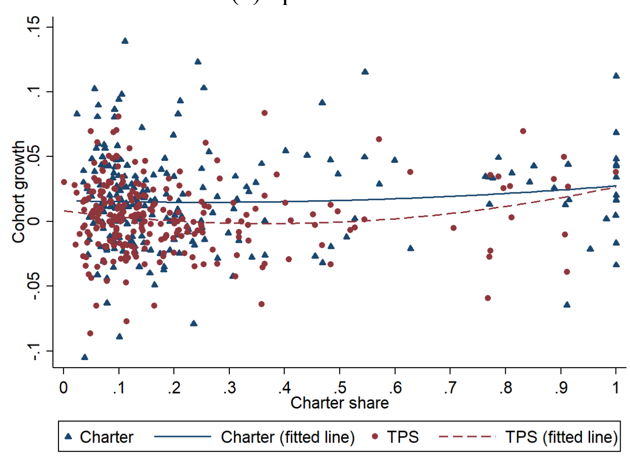
Notes: This figure presents the event study results enrollment and its compositions. The event time zero is the first year of charter entry.

Figure 5 Charter share and cohort growth for test score

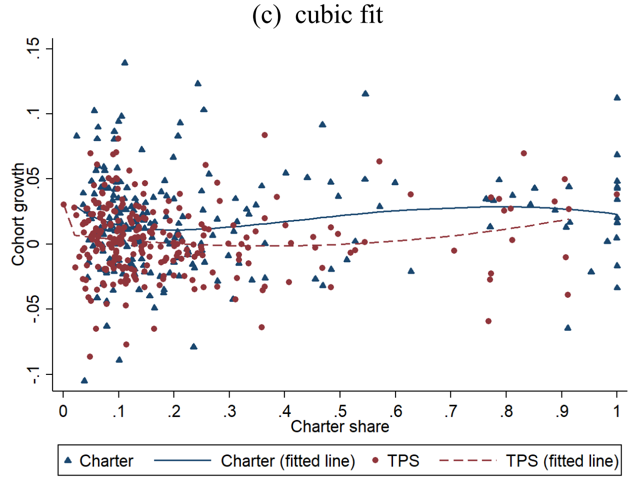
(a) linear fit



(b) quadratic fit



(c) cubic fit



Notes: This figure plot the (a) linear, (b) quadratic, and (c) cubic fit of scatters for the charter share and cohort growth measure of test score at district by charter status level. The sample is district with charter share ever above 10 percent in the sample period, and the charter share is the average charter enrollment share during 2009-2016. In figure (a), the slope of the top (charter) line is 0.0112 and 0.0063 for the bottom (TPS) line.

Appendix

Table A1 Summary Statistics (unweighted)

Sample	Graduation rate (1995-2010)						Math & ELA (2009-2016)					
	ALL	DD & DD-PSW		DD-PSM		Fixed Effects	ALL	DD & DD-PSW		DD-PSM		Fixed Effects
		Treated	Control	Treated	Control			Treated	Control	Treated	Control	
Graduation rate	0.81	0.75	0.812	0.75	0.79	0.75	NA	NA	NA	NA	NA	NA
Math	NA	NA	NA	NA	NA	NA	0.07	-0.47	0.09	-0.47	-0.37	-0.38
ELA	NA	NA	NA	NA	NA	NA	0.10	-0.39	0.12	-0.39	-0.32	-0.35
White	81%	65%	82%	65%	64%	66%	75%	59%	76%	59%	58%	57%
Black	7%	8%	7%	8%	8%	11%	9%	10%	9%	10%	10%	13%
Hispanic	7%	18%	7%	18%	16%	17%	12%	26%	11%	26%	26%	25%
FRL	30%	35%	30%	35%	34%	31%	48%	54%	47%	54%	56%	55%
Special education	13%	12%	13%	12%	13%	13%	14%	12%	14%	12%	12%	13%
Ages 5–17 population	16%	18%	16%	18%	17%	17%	18%	20%	18%	20%	21%	20%
Ages 5–17 in poverty	19%	19%	19%	19%	20%	19%	17%	15%	17%	15%	16%	16%
Urban	5%	16%	5%	16%	14%	23%	6%	16%	5%	16%	16%	25%
Suburb	22%	22%	22%	22%	20%	32%	28%	28%	27%	28%	29%	31%
Town	17%	19%	17%	19%	22%	19%	22%	28%	22%	28%	23%	25%
Rural	56%	43%	57%	43%	44%	25%	45%	32%	47%	32%	36%	23%
Revenue per student	9,276	8,975	9,307	8,975	9,194	8,621	12,882	11,235	12,969	11,235	10,899	11,360
Expenditure per student	9,354	9,068	9,382	9,068	9,312	8,759	12,794	11,277	12,873	11,277	10,933	11,404
Teacher salary	70,445	74,705	70,214	74,705	73,025	75,372	92,218	94,213	92,005	94,213	94,147	95,975
Student teacher ratio	15	17	15	17	16	17	15	18	15	18	18	18
No. magnet school	0	3	0	3	1	2	0	2	0	2	1	2
No. public schools	8	29	7	29	18	33	8	26	7	26	17	31
Enrollment	4,083	18,827	3,236	18,827	10,888	22,219	4,221	15,664	3,373	15,664	10,515	19,184
Observation	144,219	2,193	137,780	2,193	2,199	6,439	410,077	10,109	388,064	10,109	11,073	22,013
N (district)	9,278	142	8,859	142	142	416	10,439	297	9,832	297	297	607

Notes: This table presents unweighted summary statistics.

Table A2 Top 20 districts with largest charter enrollment share

School District	State	NAPCS Charter Enrollment	NAPCS Total Enrollment	NAPCS Enrollment Share	NLSD Charter Enrollment	NLSD Total Enrollment	NLSD Enrollment Share	Sample Graduation rate	Sample Test score
Orleans Parish School District	LA	46,932	49,646	95%	48,495	51,100	95%	Y	Y
Gary Community School Corporation	IN	5,060	10,288	49%	5,060	10,288	49%	Y	Y
Queen Creek Unified District	AZ	6,776	13,858	49%	5,070	12,166	42%	N	Y
District of Columbia Public Schools	DC	43,393	91,528	47%	38,696	86,330	45%	Y	Y
Detroit Public Schools	MI	38,667	83,504	46%	37,235	87,045	43%	Y	Y
Kansas City Public Schools	MO	11,420	26,630	43%	12,602	27,769	45%	Y	Y
Southfield Public School District	MI	4,543	10,697	42%	4,543	10,674	43%	N	Y
Inglewood Unified School District	CA	5,193	13,594	38%	5,453	13,854	39%	Y	Y
Camden City School District	NJ	4,731	12,672	37%	4,892	12,616	39%	Y	Y
Indianapolis Public Schools	IN	15,244	42,874	36%	15,466	42,383	36%	N	Y
Franklin-McKinley School District	CA	3,866	11,152	35%	3,305	10,591	31%	N	Y
Dayton City School District	OH	6,652	19,745	34%	6,828	19,850	34%	Y	Y
Natomas Unified School District	CA	4,952	14,880	33%	4,952	14,880	33%	N	Y
Philadelphia City School District	PA	64,393	195,631	33%	64,970	192,172	34%	Y	Y
Newark City School District	NJ	17,501	53,215	33%	17,204	52,917	33%	Y	Y
Alum Rock Union Elem School District	CA	4,623	14,265	32%	5,089	14,731	35%	N	Y
St. Louis City School District	MO	11,082	34,936	32%	11,022	33,958	32%	Y	Y
Cleveland Municipal School District	OH	16,352	54,641	30%	20,076	58,301	34%	Y	Y
San Antonio Independent School District	TX	18,515	62,119	30%	17,979	58,901	31%	Y	Y
Oakland Unified School District	CA	18,502	52,457	30%	16,070	53,018	30%	Y	Y

Notes: This table compares the top 20 districts (with the largest charter enrollment share among districts with at least 10,000 total students in the 2018 spring year) from a report of the National Alliance for Public Charter Schools with data from NLSD. Source: A Growing Movement: America's Largest Charter School Communities, Thirteenth Edition, January 2019. Please notes that this table only list districts with enrollment more than 10,000 students, there are still some other districts with enrollment less than 10,000 students but also have high charter enrollment share.

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

	(1)	(2)	(3)	(4)	(5)	(6)
	DD		DD-PSW		DD-PSM	
Panel A: Graduation rate						
Treated*Post	0.037***	0.038***	0.034***	0.028**	0.039***	0.031***
	[0.010]	[0.010]	[0.012]	[0.011]	[0.011]	[0.008]
R-squared	0.805	0.808	0.817	0.825	0.886	0.894
Observations	139,973	139,973	139,973	139,973	4,383	4,383
N (district)	9,001	9,001	9,001	9,001	284	284
Pre-treatment	0.000	0.001	-0.001	-0.001	-0.001	-0.002
trend	[0.001]	[0.001]	[0.002]	[0.001]	[0.002]	[0.001]
Panel B: Math						
Treated*Post	0.152**	0.155**	0.154***	0.148***	0.136**	0.157***
	[0.067]	[0.063]	[0.053]	[0.048]	[0.059]	[0.052]
R-squared	0.846	0.847	0.863	0.865	0.869	0.873
Observations	398,173	398,173	398,173	398,173	21,007	21,007
N (district)	10,129	10,129	10,129	10,129	594	594
Pre-treatment	0.085***	0.088***	0.023	0.023	0.074***	0.073***
trend	[0.022]	[0.019]	[0.030]	[0.024]	[0.020]	[0.015]
Panel C: ELA						
Treated*Post	0.116**	0.121**	0.075	0.077	0.075	0.106**
	[0.048]	[0.051]	[0.050]	[0.050]	[0.056]	[0.049]
R-squared	0.882	0.883	0.884	0.886	0.914	0.917
Observations	398,173	398,173	398,173	398,173	21,007	21,007
N (district)	10,129	10,129	10,129	10,129	594	594
Pre-treatment	0.026**	0.029***	-0.025	-0.019	0.009	0.011
trend	[0.010]	[0.008]	[0.018]	[0.013]	[0.012]	[0.008]
District, (grade) & year FE	Yes	Yes	Yes	Yes	Yes	Yes
District control	No	Yes	No	Yes	No	Yes

Notes: The table replicates Table 3 but clusters standard errors at the state level.

Table A4 Estimates of Propensity Score (Probit model)

Matching variables	Graduation rate sample	Test score sample
Log of enrollment	-0.176***	0.058
	[0.056]	[0.040]
Perc. of white	-0.895***	-0.729**
	[0.298]	[0.292]
Perc. of Black	-0.653*	-0.521
	[0.377]	[0.329]
Perc. of Hispanic	0.212	0.172
	[0.314]	[0.307]
Perc. of FRL	-0.292	0.882***
	[0.329]	[0.273]
Perc. of special edu.	1.022	-3.031***
	[0.859]	[0.803]
Perc. of poverty	-0.319	-2.388***
	[0.528]	[0.626]
Perc. of school age	2.765**	-10.270***
	[1.178]	[0.947]
Urban	0.329*	0.065
	[0.189]	[0.195]
Suburb	0.004	0.173
	[0.126]	[0.175]
Town	0.217**	0.388**
	[0.107]	[0.173]
Rural	0.000	0.333*
	[.]	[0.174]
Revenue per student	0.000	0.000
	[0.000]	[0.000]
Expenditure per student	0.000	0.000
	[0.000]	[0.000]
Student teacher ratio	0.097***	0.139***
	[0.016]	[0.013]
Teacher salary	0.000	-0.000***
	[0.000]	[0.000]
No. magnet school	0.000	-0.021**
	[.]	[0.009]
No. public schools	0.015***	0.015***
	[0.002]	[0.002]
Constant	-2.459***	-1.385***
	[0.622]	[0.517]
N(district)	9001	10129

Note: This table shows the results of Probit model to estimate the propensity score.

Table A5 Number of districts by year first charter initiated

Graduation rate (high school)		Math & ELA (grade 3-8)	
Year first charter started	Number of districts	Year first charter started	Number of districts
Before 1995	10	Before 2009	406
1995	11	2009	20
1996	12	2010	35
1997	11	2011	30
1998	18	2012	32
1999	23	2013	27
2000	46	2014	31
2001	40	2015	17
2002	42	2016	9
2003	30	Total	607
2004	19		
2005	27		
2006	30		
2007	31		
2008	20		
2009	25		
2010	21		
Total	416		

Notes: The table presents the number of districts by year first charter initiated.

Table A6 Effects of charter entry on student outcomes (include state time trends)

	(1)	(2)	(3)	(4)	(5)	(6)
	DD		DD-PSW		DD-PSM	
	Panel A: Graduation rate					
Treated*Post	0.037***	0.036***	0.030***	0.024***	0.018	0.014
	[0.008]	[0.009]	[0.010]	[0.009]	[0.015]	[0.010]
R-squared	0.811	0.814	0.826	0.832	0.900	0.909
Observations	139,973	139,973	139,973	139,973	4,383	4,383
N (district)	9,001	9,001	9,001	9,001	284	284
Pre-treatment trend	0.000	0.000	-0.002	-0.001	-0.001	-0.001
	[0.001]	[0.001]	[0.001]	[0.001]	[0.002]	[0.002]
	Panel B: Math					
Treated*Post	0.016	0.027	0.110**	0.110**	0.000	0.017
	[0.038]	[0.040]	[0.052]	[0.051]	[0.047]	[0.052]
R-squared	0.851	0.852	0.870	0.871	0.882	0.885
Observations	398,173	398,173	398,173	398,173	21,007	21,007
N (district)	10,129	10,129	10,129	10,129	594	594
Pre-treatment trend	0.032*	0.033**	-0.012	-0.006	0.001	0.007
	[0.018]	[0.016]	[0.016]	[0.014]	[0.019]	[0.017]
	Panel C: ELA					
Treated*Post	0.033	0.039	0.067	0.065	0.032	0.045
	[0.032]	[0.032]	[0.046]	[0.043]	[0.035]	[0.037]
R-squared	0.885	0.886	0.888	0.889	0.921	0.924
Observations	398,173	398,173	398,173	398,173	21,007	21,007
N (district)	10,129	10,129	10,129	10,129	594	594
Pre-treatment trend	0.001	0.002	-0.045**	-0.039**	-0.010	-0.012
	[0.011]	[0.009]	[0.019]	[0.017]	[0.013]	[0.015]
District, (grade) & year FE	Yes	Yes	Yes	Yes	Yes	Yes
District control	No	Yes	No	Yes	No	Yes

Notes: The table replicates Table 3 but includes state specific linear time trends.

Table A7 Alternative treated group: 0-10 percent charter share

	(1)	(2)	(3)	(4)	(5)	(6)
	DD		DD-PSW		DD-PSM	
Panel A: Graduation rate						
Treated*Post	0.012**	0.009	0.018***	0.014***	0.012*	0.007
	[0.005]	[0.006]	[0.005]	[0.004]	[0.006]	[0.006]
R-squared	0.805	0.808	0.818	0.822	0.853	0.857
Observations	142,026	142,026	142,026	142,026	8,462	8,462
N (district)	9,133	9,133	9,133	9,133	548	548
Pre-treatment trend	0.001	0.000	0.000	0.000	0.000	-0.001
	[0.001]	[0.001]	[0.002]	[0.001]	[0.001]	[0.001]
Panel B: Math						
Treated*Post	0.008	0.006	0.029	-0.015	0.009	0.005
	[0.045]	[0.041]	[0.045]	[0.027]	[0.046]	[0.041]
R-squared	0.848	0.849	0.863	0.867	0.884	0.886
Observations	399,968	399,968	399,968	399,968	23,972	23,972
N (district)	10,142	10,142	10,142	10,142	620	620
Pre-treatment trend	0.029	0.032	-0.006	-0.020	0.022	0.022
	[0.032]	[0.032]	[0.019]	[0.015]	[0.033]	[0.034]
Panel C: ELA						
Treated*Post	0.044	0.036	0.030	0.006	0.023	0.015
	[0.035]	[0.030]	[0.038]	[0.032]	[0.036]	[0.031]
R-squared	0.884	0.885	0.886	0.888	0.912	0.914
Observations	399,968	399,968	399,968	399,968	23,972	23,972
N (district)	10,142	10,142	10,142	10,142	620	620
Pre-treatment trend	0.035	0.035*	0.009	0.005	0.028	0.026
	[0.023]	[0.020]	[0.015]	[0.014]	[0.024]	[0.021]
District, (grade) & year FE	Yes	Yes	Yes	Yes	Yes	Yes
District control	No	Yes	No	Yes	No	Yes

Notes: The table shows results using districts with charter enrollment share greater than 0 but less than 10 percent as the treated group. See notes of Table 3 for controls, sample weight, and clusters.

Table A8 Alternative measure of charter share: charter school share

	(1)	(2)	(3)	(4)	(5)	(6)
	DD		DD-PSW		DD-PSM	
	Panel A: Graduation rate					
Treated*Post	0.016***	0.013**	0.016***	0.011**	0.014**	0.010*
	[0.005]	[0.005]	[0.004]	[0.004]	[0.006]	[0.006]
R-squared	0.816	0.819	0.794	0.800	0.856	0.860
Observations	143,855	143,855	143,855	143,855	12,137	12,137
N (district)	9,252	9,252	9,252	9,252	786	786
Pre-treatment	0.001	0.001	0.000	-0.001	0.000	-0.001
trend	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]
	Panel B: Math					
Treated*Post	0.093**	0.095**	0.091**	0.065*	0.074*	0.058
	[0.036]	[0.038]	[0.043]	[0.036]	[0.040]	[0.038]
R-squared	0.847	0.847	0.863	0.866	0.865	0.867
Observations	404,114	404,114	404,114	404,114	32,629	32,629
N (district)	10,280	10,280	10,280	10,280	904	904
Pre-treatment	0.040**	0.041***	0.007	-0.003	0.029	0.019
trend	[0.016]	[0.015]	[0.013]	[0.011]	[0.017]	[0.017]
	Panel C: ELA					
Treated*Post	0.084***	0.089***	0.067	0.049	0.069**	0.064*
	[0.030]	[0.032]	[0.042]	[0.038]	[0.032]	[0.034]
R-squared	0.882	0.884	0.886	0.888	0.900	0.902
Observations	404,114	404,114	404,114	404,114	32,629	32,629
N (district)	10,280	10,280	10,280	10,280	904	904
Pre-treatment	0.026***	0.028***	0.004	0.003	0.017*	0.009
trend	[0.009]	[0.008]	[0.012]	[0.011]	[0.010]	[0.010]
District, (grade) & year FE	Yes	Yes	Yes	Yes	Yes	Yes
District control	No	Yes	No	Yes	No	Yes

Notes: The table shows DD estimates of the effects of the charter entry (charter school share ever above 10%) on student outcomes. See notes of Table 3 for controls, clusters, and sample weight.

Table A9 DD estimates: Early adopter vs. later adopter

	(1)	(2)	(3)	(4)	(5)	(6)
	DD		DD-PSW		DD-PSM	
Panel A: Graduation rate						
Treated*Post	0.044***	0.045***	0.041***	0.035***	0.052***	0.046***
	[0.009]	[0.011]	[0.013]	[0.010]	[0.011]	[0.013]
R-squared	0.816	0.819	0.817	0.828	0.884	0.891
Observations	63,075	63,075	63,075	63,075	3,562	3,562
N (district)	4,068	4,068	4,068	4,068	232	232
Pre-treatment	0.002	0.003*	0.000	0.000	0.002	0.003*
trend	[0.001]	[0.002]	[0.002]	[0.001]	[0.002]	[0.002]
Panel B: Math						
Treated*Post	0.119***	0.130***	0.159***	0.143***	0.129**	0.165***
	[0.042]	[0.047]	[0.059]	[0.055]	[0.052]	[0.050]
R-squared	0.831	0.834	0.868	0.872	0.857	0.864
Observations	169,357	169,357	169,357	169,357	19,004	19,004
N (district)	4,297	4,297	4,297	4,297	528	528
Pre-treatment	0.081***	0.083***	0.023	0.023	0.071***	0.077***
trend	[0.019]	[0.014]	[0.026]	[0.019]	[0.021]	[0.019]
Panel C: ELA						
Treated*Post	0.094**	0.105**	0.076	0.073	0.078*	0.117**
	[0.038]	[0.042]	[0.051]	[0.047]	[0.047]	[0.048]
R-squared	0.871	0.873	0.885	0.888	0.906	0.909
Observations	169,357	169,357	169,357	169,357	19,004	19,004
N (district)	4,297	4,297	4,297	4,297	528	528
Pre-treatment	0.023**	0.031***	-0.028	-0.016	0.009	0.029*
trend	[0.010]	[0.009]	[0.024]	[0.018]	[0.012]	[0.015]
District, (grade) & year FE	Yes	Yes	Yes	Yes	Yes	Yes
District control	No	Yes	No	Yes	No	Yes

Notes: The table shows DD estimates of treated districts in early adopter states and comparison districts in late adopter states. The early adopter states are those passed state charter law before 1997 (27 states), and the late adopter states are those passed state charter law during 1997 to 2016 (17 states). See Table 2 for the list of states. See notes of Table 3 for controls, clusters, and sample weight.

Table A10 Placebo test: Effect of subsequent charter share on student outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
	DD		DD-PSW		DD-PSM	
Panel A: Graduation rate						
Treated*Post	0.005	0.005	-0.002	-0.003	-0.001	-0.005
	[0.008]	[0.008]	[0.009]	[0.008]	[0.008]	[0.008]
R-squared	0.790	0.793	0.839	0.843	0.915	0.921
Observations	138,889	138,889	138,889	138,889	3,299	3,299
N (district)	8,986	8,986	8,986	8,986	269	269
Panel B: Math						
Treated*Post	0.181***	0.188***	0.067	0.065	0.160***	0.163***
	[0.037]	[0.033]	[0.049]	[0.043]	[0.043]	[0.039]
R-squared	0.842	0.842	0.858	0.861	0.891	0.895
Observations	389,555	389,555	389,555	389,555	12,389	12,389
N (district)	9,924	9,924	9,924	9,807	389	389
Panel C: ELA						
Treated*Post	0.034*	0.043**	-0.037	-0.024	-0.002	0.010
	[0.020]	[0.017]	[0.051]	[0.043]	[0.025]	[0.027]
R-squared	0.877	0.878	0.889	0.890	0.922	0.926
Observations	389,555	389,555	389,555	389,555	12,389	12,389
N (district)	9,924	9,924	9,924	9,807	389	389
District, (grade) & year FE	Yes	Yes	Yes	Yes	Yes	Yes
District control	No	Yes	No	Yes	No	Yes

Notes: The table shows the "effect" of subsequent charter share on student outcomes in periods prior to the entry of charters by falsely assuming that charter entry occurred two years earlier. See notes of Table 3 for controls, sample weight, and clusters.

Table A11 Oster bound analysis

Outcome	Estimate	R^2	Oster bound (R^2=0.95)	Oster bound (R^2=1)
Graduation rate	0.038	0.808	0.073	0.086
Math	0.155	0.847	0.257	0.306
ELA	0.121	0.883	0.176	0.217

Note: This table reports point estimates, the R-squares and Oster bounds of our main estimates of DD and FE. Panel A correspond to the Column 2 of Table 3, and panel B to the Column 2 of Table 7 of the paper. 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 $R_{max} = 1.3R_0$, however, in our case, $1.3R_0$ for all estimates are greater than 1, so, we instead, calculates Oster bounds for $R_{max} = 0.95$ and $R_{max} = 1$, respectively. R^2 refers to the R-squared of the specification with full control variables and fixed effects.

Table A12 Average charter enrollment share across models

	Graduation rate			Math & ELA		
	ALL	DD-Treated	FE	ALL	DD-Treated	FE
(a) Average charter share	0.6%	6.3%	2.7%	1.5%	12.3%	6.3%
(b) Average max charter share	1.9%	18.4%	8.0%	2.3%	18.8%	9.6%

Notes: The table average charter enrollment share across DD and FE models. (a) is the charter shares averaged across years; and (b) is the averaged eventual max share (it is what we're using to place districts in the treatment group).

Table A13 Estimates of non-linear effects of charter share on student outcomes

	(1)	(2)	(3)	(4)
	Panel A: Graduation rate			
	Same year share		Average last four years	
Charter share	0.291***	0.323***	0.267**	0.251**
	[0.096]	[0.091]	[0.126]	[0.109]
Charter share square	-0.314**	-0.317***	-0.334**	-0.274
	[0.122]	[0.111]	[0.154]	[0.174]
R-squared	0.854	0.861	0.864	0.871
Observations	6,439	6,439	5,191	5,191
N (district)	416	416	416	416
	Panel B: Math			
	Same year share		Same cohort last year	
Charter	0.733**	0.741***	0.301	0.173
	[0.339]	[0.274]	[0.272]	[0.288]
Charter share square	-0.659	-0.745*	-0.065	0.069
	[0.431]	[0.384]	[0.336]	[0.348]
R-squared	0.854	0.858	0.877	0.878
Observations	22,013	22,013	13,728	13,728
N (district)	607	607	604	604
	Panel C: ELA			
	Same year share		Same cohort last year	
Charter	0.513*	0.253	0.241	0.035
	[0.300]	[0.251]	[0.262]	[0.264]
Charter share square	-0.61	-0.39	-0.188	0.049
	[0.392]	[0.355]	[0.323]	[0.323]
R-squared	0.909	0.912	0.922	0.924
Observations	22,013	22,013	13,728	13,728
N (district)	607	607	604	604
District, (grade) & year FE	Yes	Yes	Yes	Yes
District control	No	Yes	No	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. In columns (1) and (2), charter enrollment share of grades 9-12 is used for graduation rate, and charter enrollment share of grades 3-8 is used for test scores. In columns (1) and (2), the average last four years 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. See notes of Table 3 for controls and clusters. Regressions are weighted by high school enrollment for graduation rate and grade-level enrollment for Math and ELA.

Table A14 Effect heterogeneity: baseline performance

	(1)	(2)	(3)	(4)	(5)	(6)
	DD		DD-PSW		DD-PSM	
Panel A: Graduation rate						
Treated*post*	-0.001	-0.034	-0.042	-0.032	0.008	-0.016
baseperformance	[0.038]	[0.044]	[0.037]	[0.034]	[0.039]	[0.040]
R-squared	0.805	0.808	0.818	0.825	0.886	0.894
Observations	139,973	139,973	139,973	139,973	4,383	4,383
N (district)	9,001	9,001	9,001	9,001	284	284
Pre-treatment	-0.003	-0.004	0.001	0.001	-0.001	-0.003
trend	[0.003]	[0.004]	[0.004]	[0.004]	[0.003]	[0.003]
Panel B: Math						
Treated*post*	-0.171	-0.226	-0.056	-0.116	-0.181	-0.273*
baseperformance	[0.161]	[0.167]	[0.187]	[0.160]	[0.160]	[0.162]
R-squared	0.846	0.847	0.863	0.865	0.869	0.874
Observations	398,173	398,173	398,173	398,173	21,007	21,007
N (district)	10,129	10,129	10,129	10,129	594	594
Pre-treatment	0.004	-0.019	0.034	0.017	0.005	-0.026
trend	[0.071]	[0.064]	[0.072]	[0.067]	[0.070]	[0.068]
Panel C: ELA						
Treated*post*	-0.094	-0.131	0.090	0.096	-0.092	-0.170
baseperformance	[0.136]	[0.157]	[0.193]	[0.166]	[0.140]	[0.177]
R-squared	0.882	0.883	0.884	0.886	0.914	0.917
Observations	398,173	398,173	398,173	398,173	21,007	21,007
N (district)	10,129	10,129	10,129	10,129	594	594
Pre-treatment	0.015	-0.018	-0.012	-0.041	0.015	-0.018
trend	[0.039]	[0.035]	[0.089]	[0.076]	[0.039]	[0.042]
District, (grade) & year FE	Yes	Yes	Yes	Yes	Yes	Yes
District control	No	Yes	No	Yes	No	Yes

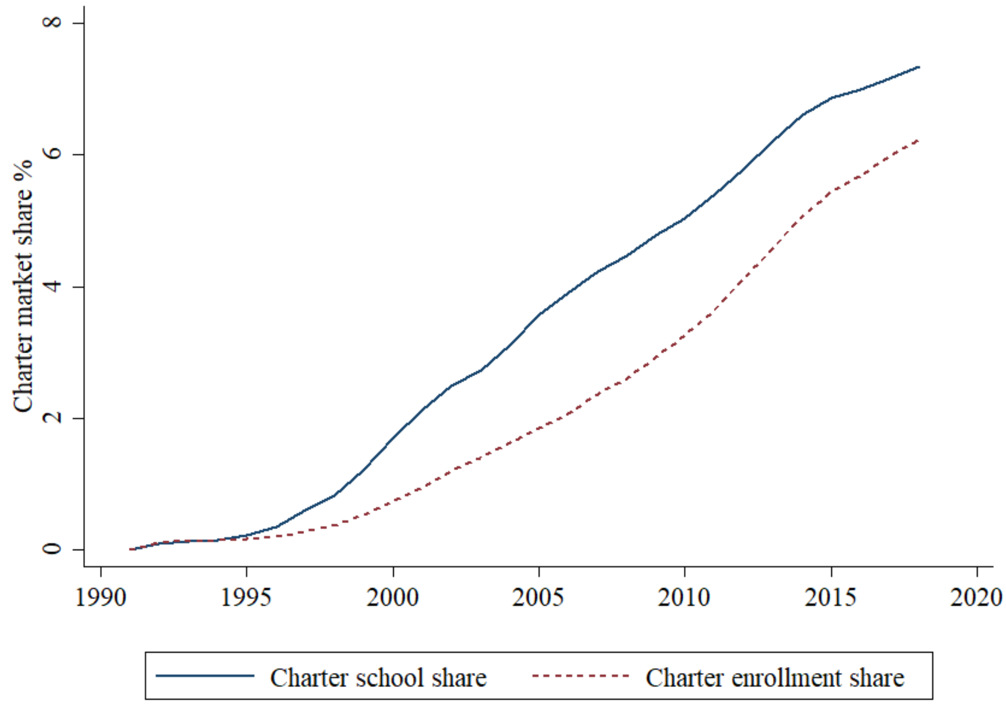
Notes: The table shows DD estimates of heterogeneous effects of the charter share on student outcome by baseline performance. *baseperformance* is the variable of baseline performance, it ranges from 0 to 1 by decile, and 0 refers to the least 10 percentile base performance, 0.1 refers the 10-20 percentile base performance, and so force. See notes of Table 3 for controls, clusters, and sample weight.

Table A15 Effects of charter entry on private school enrollment share

	(1)	(2)	(3)	(4)	(5)	(6)
	DD		DD-PSW		DD-PSM	
Panel A: district charter share on private school enrollment						
Treated*Post	0.000	0.002	-0.007***	0.001	-0.003	0.001
	[0.002]	[0.002]	[0.002]	[0.002]	[0.002]	[0.002]
R-squared	0.904	0.912	0.907	0.920	0.905	0.919
Observations	39,973	39,973	39,973	39,973	10,932	10,932
N(district)	3,779	3,779	3,779	3,779	1,013	1,013
Pre-treatment	0.000	-0.001*	-0.001***	-0.001***	-0.001**	-0.001**
trend	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Panel B: high school charter share on elementary private school enrollment						
Treated*Post	0.014	0.007	-0.011	-0.004	0.011	0.004
	[0.014]	[0.009]	[0.007]	[0.005]	[0.012]	[0.006]
R-squared	0.900	0.911	0.863	0.897	0.840	0.934
Observations	40,786	40,786	40,786	40,786	1,720	1,720
N (district)	3,857	3,857	3,857	3,857	161	161
Pre-treatment	0.001	0.001	0.000	0.000	0.001	0.001
trend	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]
Panel C: elementary charter share on high school private school enrollment						
Treated*Post	0.004	0.001	-0.003	0.002	0.001	0.001
	[0.004]	[0.003]	[0.003]	[0.004]	[0.004]	[0.003]
R-squared	0.899	0.907	0.900	0.913	0.870	0.903
Observations	41,217	41,217	41,217	41,217	5,119	5,119
N (district)	3,897	3,897	3,897	3,897	477	477
Pre-treatment	-0.001	-0.001**	-0.001***	-0.001**	-0.002**	-0.002**
trend	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]
District, (grade) & year FE	Yes	Yes	Yes	Yes	Yes	Yes
District control	No	Yes	No	Yes	No	Yes

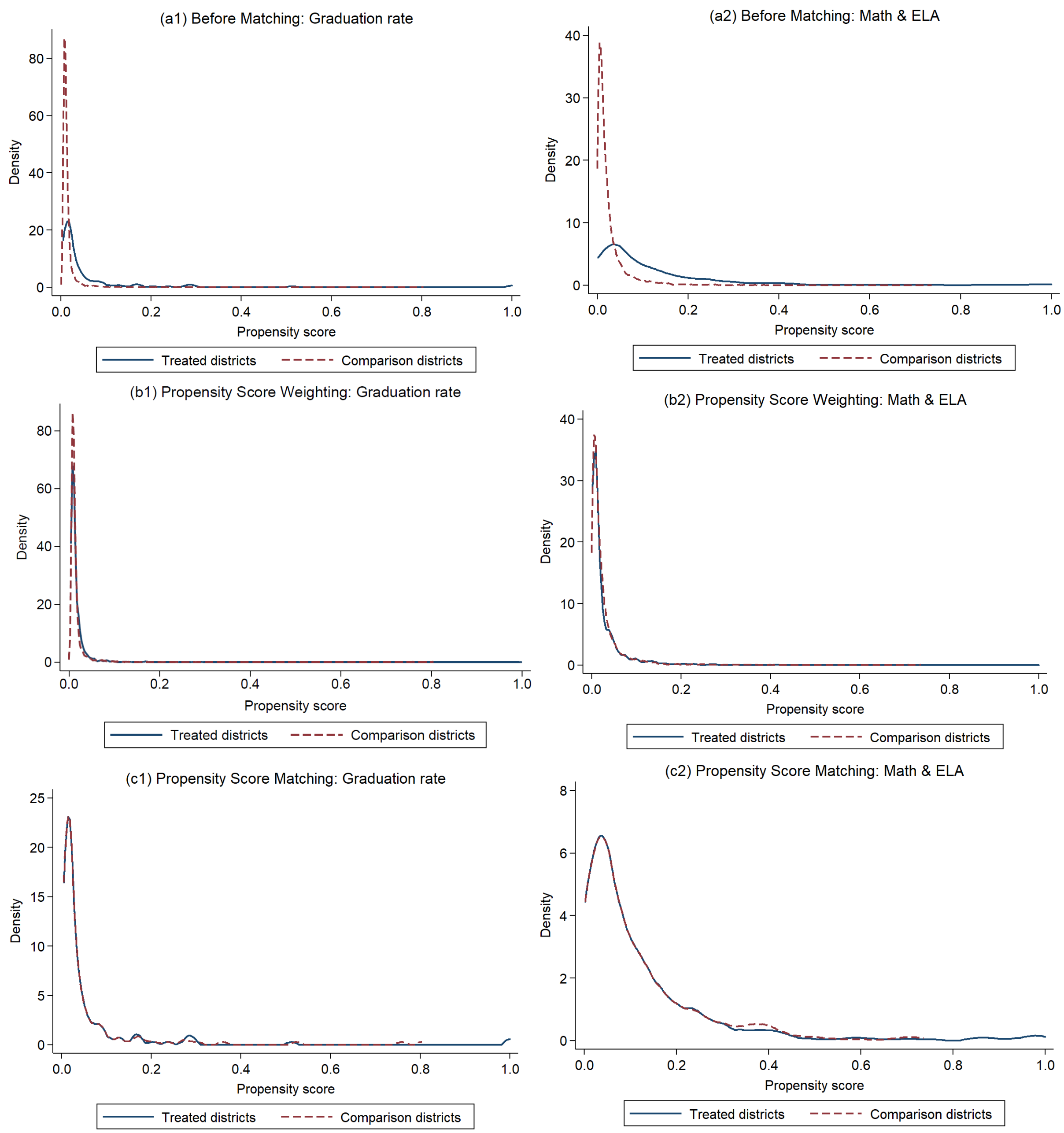
Notes: This table presents DD estimates of the effects of the charter entry (enrollment share ever above 10%) on the share of private school enrollment. The sample period is from the year 1996 to 2016 biannually. For DD and DD-PSM, regressions are weighted by the district total enrollment; For DD-PSW, regressions are weighted by the weight of DD times the inverse probability of propensity score. See notes of Table 3 for controls and clusters.

Figure A1 Trends in charter school share and charter enrollment share



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

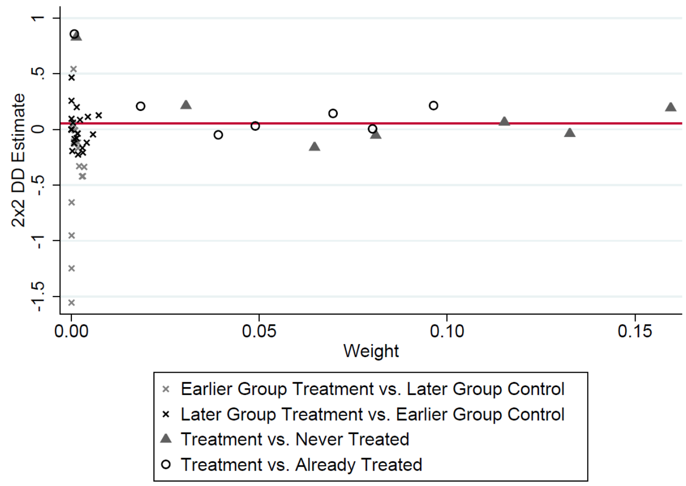
Figure A2 Density of propensity score before and after matching



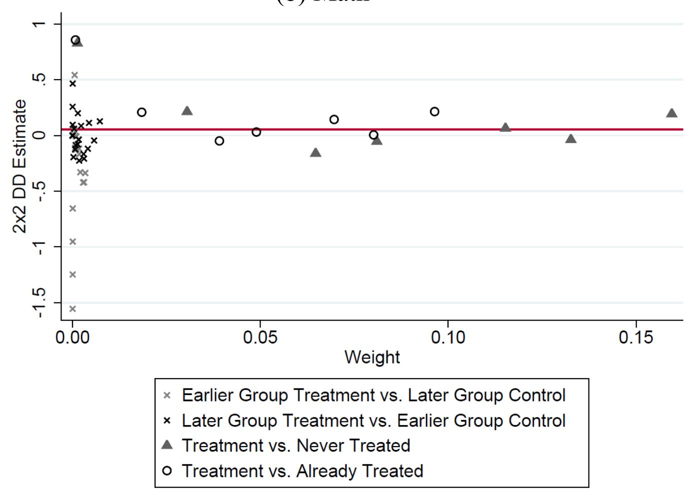
Notes: This figure plots the density of the propensity score of covariates of treated districts (in the solid line) and comparison districts (in the dashed line).

Figure A3 Goodman-Bacon (2021) Decomposition

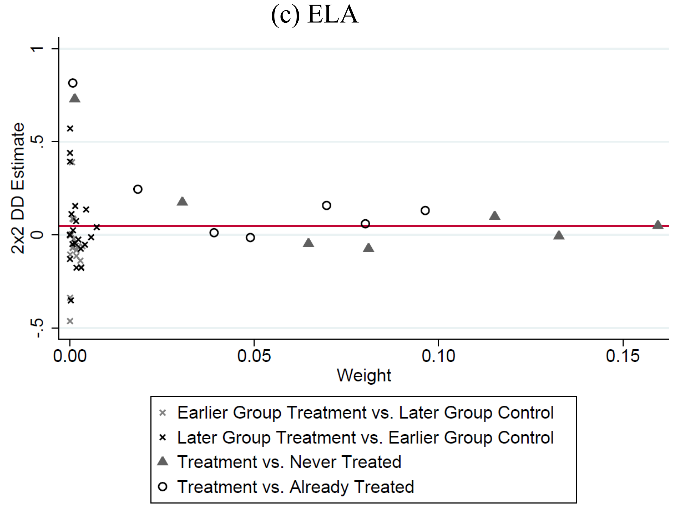
(a) Graduation rate



(b) Math

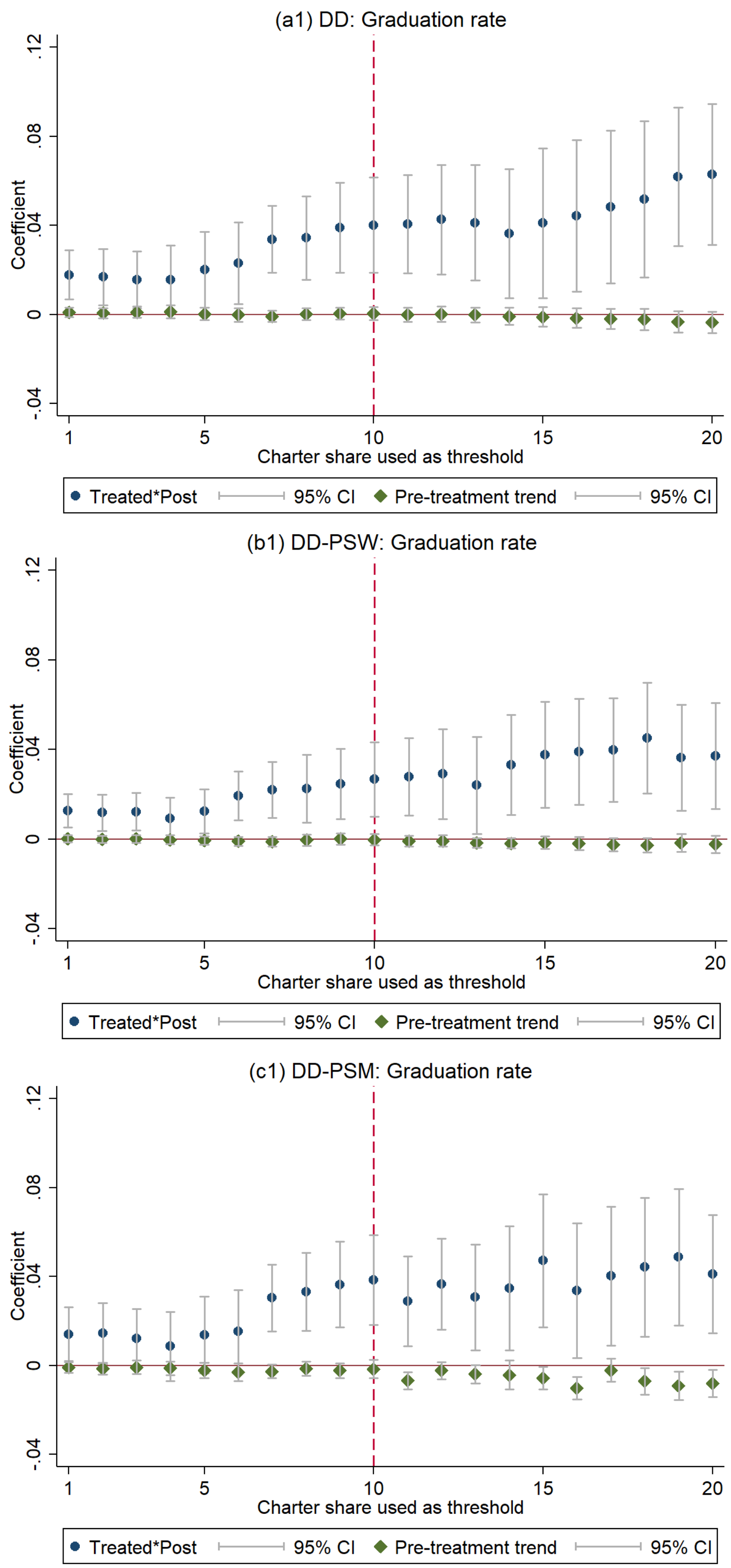


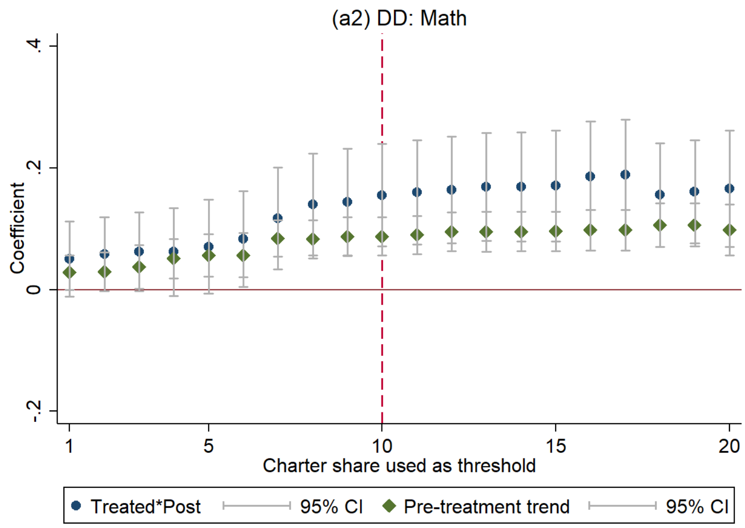
(c) ELA

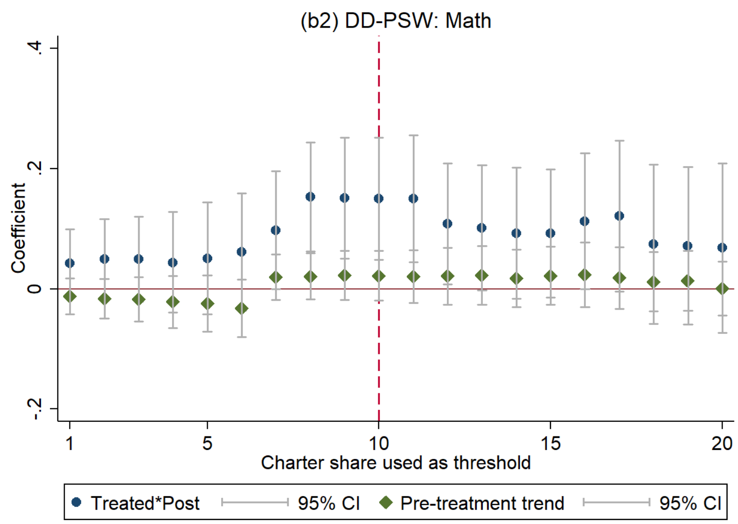


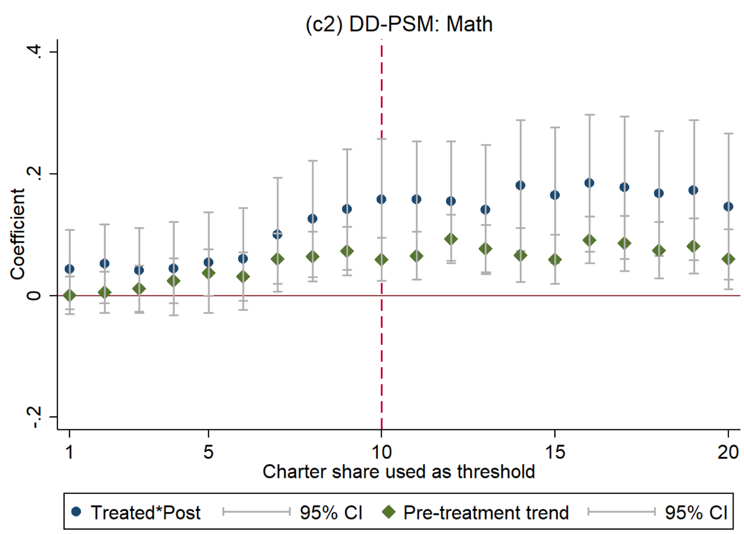
Notes: The above figures document the Goodman-Bacon decomposition into a series of 2×2 difference-in-differences models depending on the type of comparison unit.

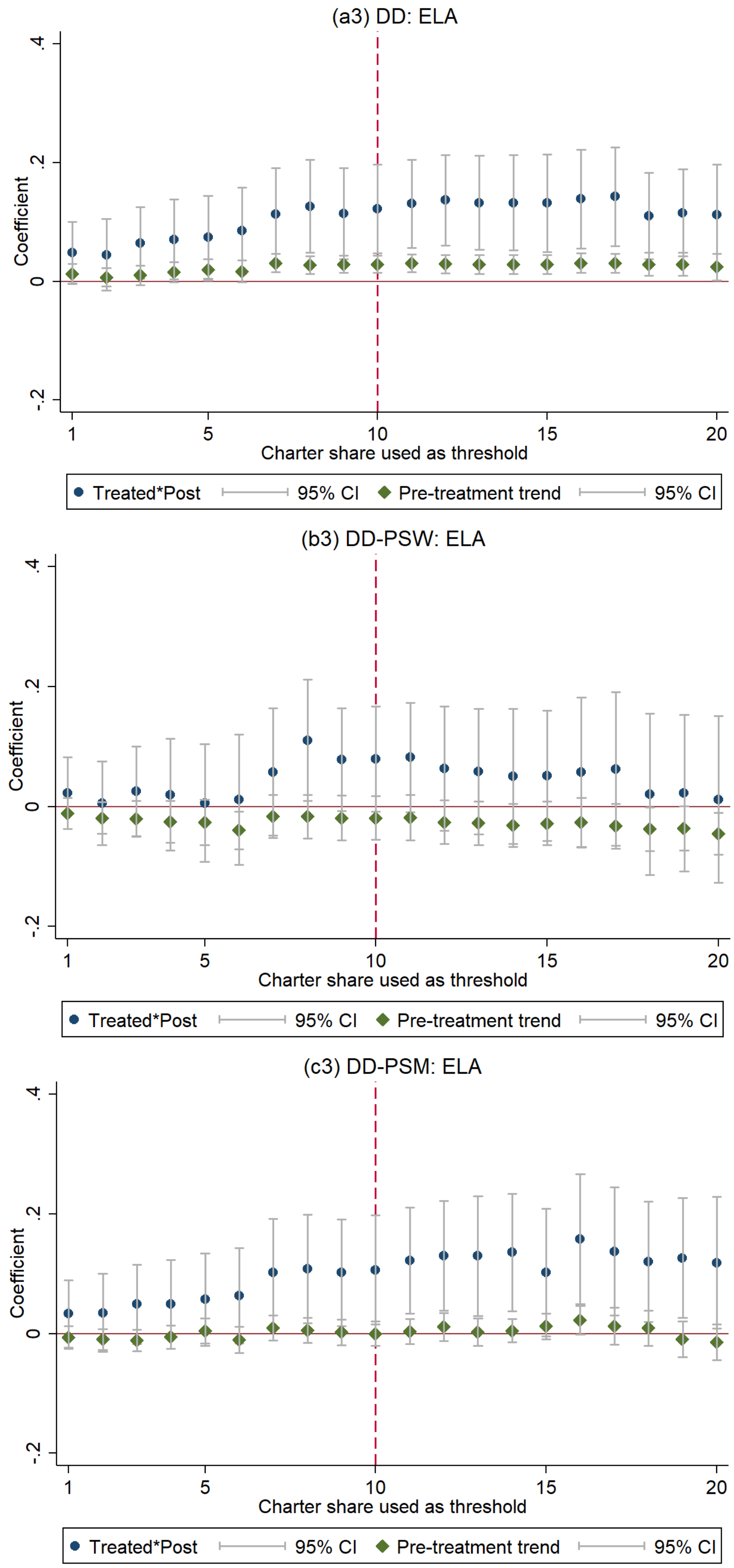
Figure A4 Plots of estimates using 1% to 20% as the threshold of treated districts without New Orleans




(a2) DD: Math
Coefficient
.4
.2
0
-.2
1
5
10
15
20
Charter share used as threshold
Treated*Post
95% CI
Pre-treatment trend
95% CI


(b2) DD-PSW: Math
Coefficient
.4
.2
0
-.2
1
5
10
15
20
Charter share used as threshold
Treated*Post
95% CI
Pre-treatment trend
95% CI

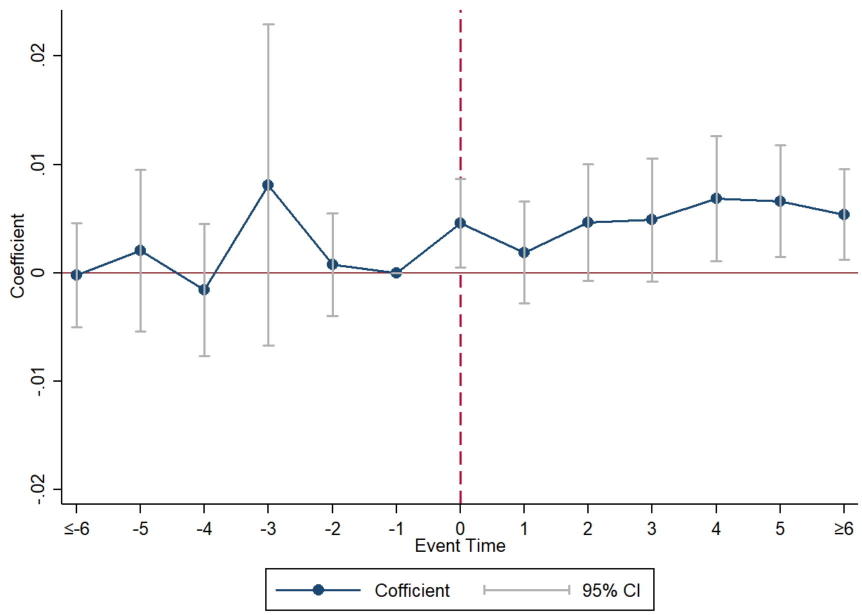

(c2) DD-PSM: Math
Coefficient
.4
.2
0
-.2
1
5
10
15
20
Charter share used as threshold
Treated*Post
95% CI
Pre-treatment trend
95% CI



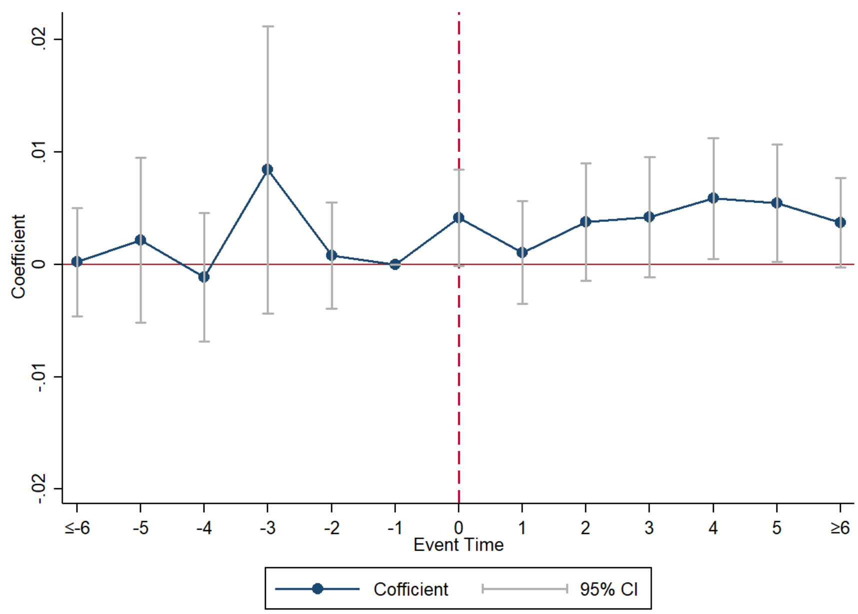
Notes: This figure plots the estimates (without New Orleans school district) in graduation rate, Math & ELA using the charter enrollment share of 1- 20% as the threshold of treated districts.

Figure A5 Event study results of TPS closure

(a) Event study



(b) IW estimators



Notes: This figure presents results of event study and IW estimators for TPS closure. The event time zero is the first year of charter entry.

Appendix B Sample Description

This section describes how we create our sample for analyses. For graduation sample, we start from a conservative sample with schools have enrollments of high school grades (9-12). This sample contains 35,764 public schools (charters included), and 3,222 charter schools of them located in 1,218 geographic school districts. Then, we merge the schools data to district graduation rate data, and schools in districts with missing outcomes are dropped. Apart from removing districts with missing outcomes across the whole sample period, a few districts only have limited waves of data were also removed to ensure more balanced analyses of DD. Specifically, we removed districts with 13 or less waves (full wave is 16) of observations (account for 9% observations). The remined sample contains 25,038 public schools (charters included), and 1,074 charter schools located in 416 districts.

For test score sample, similarly we start from a conservative sample with schools have enrollments of grades 3-8, which has 84,918 public schools (charters included), and 6,719 charter schools of them located in 1,610 geographic school districts. Then, we merge the schools data to district test score data, and schools in districts with missing outcomes are dropped. Similarly, a few districts only have limited waves of data were also removed to ensure more balanced analyses of DD. Specifically, we removed districts with four or less waves (full wave is eight) of observations (account for 6% observations). The remined sample contains 70,493 public schools (charters included) and 2,453 charter schools located in 607 districts. Table B1 presents the summary statistics of in sample schools versus missing ones.

Table B1 Summary statistics for sample schools/districts versus missing ones

	Graduation sample		Test score sample	
	In sample	Missing	In sample	Missing
Panel A Charter schools				
Average student enrollment	336	274	404	411
Black%	20	24	20	33
Hispanic%	24	23	21	16
White%	46	45	39	36
FRL%	40	46	51	59
Urban%	83	84	84	87
Non-LEA authorizer %	5	61	4	76
N(school)	1,074	2,148	2,453	4,266
Obs	5,279	11,094	12,969	22,010
Panel B Districts with charter schools				
Average student enrollment	21,669	6,326	19184	9889
Black%	11	15	13	12
Hispanic%	17	17	25	29
White%	64	58	57	53
FRL%	32	38	55	55
Urban%	75	74	81	80
Non-LEA authorizer %	18	60	14	61
Charter share (ever max)	18	81	18	26
N(district)	416	802	607	1,003
Obs	6,920	7,386	22,013	74,505

Notes: This table presents summary statistics of in sample schools versus missing ones. In Panel B, if the district has at least one Non-LEA authorized charter school, then it is defined as Non-LEA authorizer. The charter share is the max charter share during sample period for a given district.