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The Effects of Charter School Entry on the Supply of Teachers from University-Based Education Programs

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Abstract: Research on charter schools tends to focus on direct and immediate effects on student outcomes. However, there may be unintended indirect effects on, for example, the teacher labor market. Charter schools tend to hire younger, less experienced teachers with fewer traditional teaching credentials, which may reduce the equilibrium quantity of teachers who have traditional credentials and seek to make teaching a career. We test whether charter entry reduces the supply of university-trained teacher education majors, exploiting cross-district variation in the timing of charter school entry in districts containing college teacher preparation programs. Applying a matched difference-in-difference model, we find evidence that, for example, a 10 percent increase in charter market share decreases the supply of traditionally prepared teachers by 13.5 to 15.2 percent on average. This effect is concentrated in metropolitan areas and for elementary, special education, and math education degrees.

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1. Introduction

Charter schools have been growing in the United States since the 1990s (Hohnstein, 2008) and 46 states now have charter policies. These reforms intend to improve student outcomes by granting more school autonomy and creating innovative forms of schooling (Hoxby 2003; Glomm et al., 2005; Jabbar 2018). In addition to creating new, high-quality schools, the chartering approach may affect the entire schooling market through innovation and competition, which, in turn might increase student outcomes (e.g., Angrist et al., 2016; Cohodes & Parham, 2021; Zimmer et al., 2015).

Charter schools might also have more indirect or unanticipated effects on, for example, the teacher pipeline (Chubb & Moe, 1990). As the charter market share has grown to a national average of about 7 percent, with some states as high as 15 percent; at the same time, the number of teacher preparation program completions has steadily declined (Will 2022). Between 2007 and 2016, the new number of new teachers has decreased by about 20 percent; see Figure 1 and Kraft et al. (2020).[1] In this study, we examine whether this correlation partially reflects the causal effects of charter schooling on the number of new graduates with education bachelor's degrees (BA's), which we use to measure the supply of traditionally-prepared teachers.

We focus specifically on the supply of new traditionally prepared teachers graduating from institutions of higher education (IHEs). While a growing share of teachers come into the profession through alternative routes (Grossman & Loeb, 2008;

[1] Figure 1 (Panel A) shows the declining trend in education bachelor's degrees. The figure separates districts that eventually open charter schools from those that never have charter schools during the panel period.

Dee & Goldhaber, 2017), traditionally prepared teachers still comprise the vast majority of the new teacher workforce (Partelow, 2019).[2] In some ways, we might expect the quantity of college graduates with education majors to increase because charter schools hire more younger teachers compared to traditional public schools (TPS).[3] Thus, as more charter schools enter, undergraduates may see potential job opportunities with charter schools and change their college majors to education, ultimately supplanting experienced TPS teachers. This would tend to increase the equilibrium supply of college education-major graduates.[4]

However, the autonomy afforded to charter school leaders may also reduce equilibrium teacher supply from IHEs because charters face fewer limits on teacher certification and qualification (Jabbar, 2018).[5] In the National Teacher and Principal Survey (NTPS), 25 percent of charter school teachers reported having an alternative certification, compared to 17 percent of traditional public school teachers (U.S. Department of Education, 2018). If schools hire more alternatively prepared and non-certified teachers, then the demand for traditionally prepared teachers will decline.

Compared with TPS, charter schools also offer lower salaries and less generous

[2] In 2018, about 77 percent of prospective teachers were enrolled in university teacher preparation programs (Partelow, 2019).

[3] There are various reasons why charter schools might prefer younger teachers. They are less expensive and less likely to unionize (in part because of higher turnover rates).

[4] We say "equilibrium supply" here, in part because the market does not clear and many college majors never apply for teaching jobs (Cowan et al. 2016). Also, we mean specifically the short-run equilibrium supply. The long run is less clear. Charter schools might, for example, produce a permanent increase in supply of recent college graduates majoring in education if charter teachers have high attrition from the profession.

[5] States that allow non-certified teachers (some only up to a certain percent) in charter schools are Alabama, Arizona, Arkansas, District of Columbia, Louisiana, Mississippi, Missouri, New Hampshire, New York, North Carolina, Oklahoma, Oregon, Pennsylvania, South Carolina, and Texas (unless home-rule district). States that allow non-certified teachers under certain circumstances are Colorado, Connecticut, Delaware, Hawaii, Illinois, Indiana, Maine, Michigan, Nevada, Ohio, and Washington. Georgia's statute is not clear (Education Commission of the States, 2020).

fringe benefits (Hoxby, 2000). Charter teachers are often not protected by tenure and union contracts (Carruthers, 2012; Jackson, 2012; Cowen and Winters, 2013), so they also have less job security. Working conditions are also important to teachers (Ingersoll, 2001). The lack of union contract seems to give teachers less autonomy over their work (Weixler et al., 2016). That is, the intent of charter schooling is to give more autonomy to *schools*, but this may not filter down to teachers (Bulkley et al., 2020). Charter schools also have more flexibility to institute strict teacher accountability, which places pressure on teachers and reduces autonomy. Kraft et al (2020) find that such policies reduce the number of new teaching licenses and university-based teacher preparation program degree completers. These factors—reduced job security and autonomy--likely make charter school teaching less attractive to the average college student, especially to those seeking to make teaching a career.[6]

In New Orleans, for example, where all schools became charter schools, all the above factors were at play (Harris, 2020). When charter schools entered the city en masse after Hurricane Katrina, teachers were held to strict accountability standards, and had less job security (Barrett, Carlson, Harris, and Lincove, forthcoming). Perhaps as a result, they also had less control over their work and worked longer hours, which led to lower job satisfaction among teachers who experienced both the pre- and post-reform schools (Weixler et al., 2016). Along with other effects of Katrina and the reforms, this may have contributed to the steep decline in the number of undergraduate education majors coming from local IHEs (see Figure 2).

[6] Teachers are also not likely to agree to work in charter schools without sharing the same teaching philosophy as the charter (Manno et al. 1998; Miron and Nelson 2002; Malloy and Wohlstetter 2003; Cannata and Penaloza, 2012; Burion-Fitzgerald et al., 2004; Stuit and Smith, 2012). The leaders of traditional teacher education programs generally oppose charter schools.

To better understand the effects of charter schools on teacher supply on a national scale, we exploit the variation in timing of charter school entry across states and districts to determine how these reforms impact the number of teacher preparation degree completions. Given the "draw of home" and the tendency of teachers to work near where they live and attend college (Boyd et al., 2005), we focus especially on degrees from commuter institutions located in the same district that charters locate. With this sample, we use a difference-in-differences model (DD), and various robustness checks, for the years 1990 to 2018 to determine the impact of charter school enrollment share on the ratio of teacher preparation degrees to all degrees granted by the same colleges. This DD method helps account for the wide variety of other factors that can affect the supply of teachers, including the business cycle (Flach et al, 2009; Kraft et al, 2020; Nagler et al, 2020). To account for measurable differences in baseline characteristics, we identify a matched comparison group using propensity score weighting (PSW) and propensity score matching (PSM). We also use placebo analyses to test for endogeneity; specifically, we will leverage information about the types of education degrees, e.g., the number of elementary certifications should change more than other degrees when more elementary charter schools open, as opposed to middle and high school charters.

Our results suggest that district charter entry decreases the equilibrium quantity of university-based teacher education graduates (what we call, for short, the "new teacher supply"), especially for education majors in elementary education, special education, and math education degrees. The concentration of effects in elementary education may be because charter schools disproportionately serve elementary and middle school grades, while the decline in math education graduates could be because charter schools are apt to

hire teachers in these fields who have degrees in their disciplines (e.g., math majors) rather than those from schools of education (Podgursky, 2008). The results for special education are consistent with the idea that charter schools are less likely to assign students to special education and less likely to provide specialized pull-out programs (Winters, Carpenter, & Clayton, 2017).

Finally, we see sharp declines in the share of education majors in metropolitan areas. This is noteworthy given the consistent pattern that charter schools are more effective in urban areas (Sass et al., 2016; Harris, 2020; Zimmer et al., 2015). This might be explained by the tendency of charter schools to mimic TPS, which also vary geographically in their mix of certified and uncertified teachers (USDOE, 2016).[7]

Our analysis complements the work of Bruhn et al. (2020) who find that charter schools attract people to the profession who would not have joined otherwise.[8] Taken together, the two studies suggest that charter entry increases the demand for alternatively prepared teachers (Bruhn et al., 2020) and simultaneously decreases demand for new education majors. Recognizing the change in job opportunities, college students and other young people change their career plans accordingly.

These results have several important implications. First, the fact that we see declines in education majors specializing in math and special education is problematic given that these are two of the most commonly reported teacher shortage areas in schools generally (Dee & Goldhaber, 2017). Second, teachers who major in education are more likely to stay in teaching (Redding & Smith, 2016), thus reducing turnover and allowing

[7] Such mimicking behavior would be consistent with what sociologists call isomorphism, or the tendency of institutions to take on the qualities of other institutions (Huerta & Zuckerman, 2009).
[8] Bruhn et al. (2020) also find that high-performing teachers entering because of charter schools end up teaching in traditional public schools.

them to leverage their return to experience; all else equal, high turnover and low experience reduce the average quality of teaching (Ronfeldt, Loeb & Wyckoff, 2013; Harris & Sass, 2011). Third, this is one of few studies to examine the indirect causal effects of charter schools on schooling broadly, prompting the need for studies of other possible indirect effects that have not been recognized.

Section 2 below describes our data. This is followed by a discussion of our econometric methods in section 3. Our results follow in section 4.

2. Data

2.1 Teacher Supply Data

Annual institutional level data on the supply of certified teachers is collected from the Integrated Postsecondary Education Data System (IPEDS) for the years 1990-91 to 2018-19. The U.S Department of Education's (USDOE) National Center for Education Statistics (NCES) collects data for the IPEDS surveys every year for institutions eligible for federal student aid. Institutions that have a program participation agreement with the USDOE's Office of Federal Student Aid are required to report degree completions, institution location, campus housing, dormitory capacity, enrollment, admissions, school rankings, and student demographics. We categorize education degrees based on six-digit classification of instruction program (CIP) codes. An advantage of this dataset is that it allows us to explore heterogeneous effects by race and detailed education major.

We include school districts with at least one public or private four-year institution offering a four-year education preparation degree within its geographic boundaries. In the main analysis, we restrict the sample to institutions categorized as commuter schools, i.e., those with at least a 30 percent average of students living off-campus with family over

the time period or with dormitory capacity below 35 percent of total enrollment.[9] If a student commutes to his or her college or university, then the student is already living in the local community. If charter schools affect the new teacher supply, we hypothesize it is most likely to arise for these commuting students because they live in, and will seek teaching jobs in, the local community (Boyd et al., 2005).

We follow Kraft et al. (2020) to determine which education degrees qualify as K-12 teacher preparation programs and test for heterogeneous effects. For the main specification, the sample is restricted to bachelor's degrees as classified by the IPEDS award-level codes[10] and we sum the institutional level completions by year. We also conduct some analysis combining bachelor's and master's degree completions.[11] As discussed in greater detail in Kraft et al (2020), there are several limitations to using the IPEDS dataset and focusing on new teacher preparation graduates. The most important is the exclusion of alternatively certified teachers. Also, while it includes 6,000 institutions of higher education nationwide, this excludes a small number of institutions that do not receive federal aid and does not include alternatively certified teachers.

[9] Information on living off-campus with family is available for the years 2009-2019. On average, 17 percent of students live off-campus with family. We use the averages over the period 2009-2018 to determine which colleges are above/below the thresholds. For institutions missing information on living off-campus, we use the average dormitory capacity from 1990-2018. We interpolate missing observations for institutions missing less than five years of data and drop institutions missing more than five years of data. About 2 percent of institutions report no information on students living off campus with family or dorm capacity. About 1 percent of graduates are from missing institutions. As robustness checks, we use three more restrictive thresholds to define commuter schools. The alternative commuter samples are defined as: (1) less than 35% living off-campus with family, less than 35% or less dorm capacity; (2) less than 40% living off-campus with family, less than 30% dorm capacity; (3) less than 50% living off-campus with family, less than 30% dorm capacity.

[10] We use the CIP codes 13.0101 to 13.999 to identify education degrees. Teacher preparation degrees and their CIP codes are categorized as: Elementary education: 131202; Special education: 131000-131099; Social studies: 131317, 131318, 131332, 131328; English and language arts: 131305, 131315; Science teacher education: 131316, 131321, 131322, 131323, 131329, 131337; Math: 131311.

[11] Table A10 shows the results combining education bachelor's and master's degrees are similar to the main results.

Our analysis focuses on the sample of districts where there is an IPEDS institution that generated at least one education major in 1995.[12] While the IPEDS includes 3,000 to 7,000 IHEs per year, there are more than 12,000 school districts and IHEs tend to be concentrated in large districts. Also, many institutions do not offer education majors. We identify 290 school districts that had at least one commuter college within its boundaries that granted education degrees in 1995. This constitutes our main sample. We also include analysis using the full sample of institutions, which does not restrict based on commuter status. Results are consistent using the commuter sample or full sample of institutions.

Our dependent variable is the number of education degrees divided by the number of total degrees granted at the institution. This method is used to account for the time-varying fortunes and popularity of colleges as well as general labor market conditions that indirectly affect the number of new education majors. When the local economy is weak, for example, the number (and teaching potential) of students going to college may increase as individuals seek to improve job prospects (Nagler, Piopiunik, & West, 2020). Using the ratio of education-to-total degrees reduces the potential that these idiosyncratic shocks contaminate the estimated effects due to correlations between them and charter entry. As a robustness check, we also use the raw number of education bachelor's degrees as an outcome variable.

2.2 Charter School Entry and Other Data

We use data from the National Longitudinal School Database (NLSD) to obtain

[12] We limit the sample to institutions missing less than six out of 28 years of education degree information. For those remaining institutions with intermittently missing data, we fill in number of education degrees with the previous year.

charter entry by district. The NLSD combines data from the Common Core of Data (CCD) and National Alliance for Public Charter Schools (NAPCS) and other sources. The NLSD contains annual information on district-level demographics, enrollment, school type, student test scores, district finances, school-age population, rural or urban status, unemployment rate, teacher salaries, tax revenue and expenditures, and the poverty rate. All data are collapsed to the district level.

Finally, we use the NLSD to capture the first year in which charter laws were passed in each state. Minnesota was the first state to pass a law that allowed public charter schools in 1991. Table 1 shows the 45 other states and District of Columbia that passed charter school legislation. There are only five states that, as of 2018, had not passed such laws.[13] These states serve as our control group in an alternative specification of the DD model. Figure A1 shows how the share of charter schools and charter school enrollment has increased from spring 1990 to spring 2018.

The analysis is limited to districts that have IHE teacher education programs located within their boundaries and we separate these into two groups. Table 2A compares the average characteristics of treated districts (i.e., those districts with any charter schools) and untreated districts across the school years 1989-1990 to 2017-2018. We do this for both the PSM-selected comparison group and the PSW-selected group though the results are similar. The PSW results generally yield greater control-treatment similarity (and with a larger sample), so we focus our discussion and later interpretation on these results. While the control-treatment differences are often statistically significantly different from one another, these differences seem substantively small. For

[13] States without public charter school laws as of 2020 are Montana, Nebraska, North Dakota, South Dakota, and Vermont.

the most important baseline characteristic—the share of education majors—the differences are generally in the range of 10-20 percent; the baseline share is sometimes higher in the comparison group and sometimes lower.

While we are most interested in baseline balance on the dependent variable (see above, Table 2B shows the enrollment-weighted summary statistics for other measures from the NLSD dataset.[14] Using the DD and DD-PSW sample, treated districts have higher percentages of Black and Hispanic students, free or reduced-price lunch (FRL), and school-age children poverty rates. Notably, only about 3 percent of treated districts are in rural areas compared to 20 percent of control districts. A greater percentage of students in control districts are white (60 percent compared to 34 percent). Schools in control districts spend on average about $3,000 more on students and pay teachers about $25,000 more annually than schools in treated districts. Control districts also have much lower enrollment (15,000 compared to 95,000) and there are fewer schools in each district on average (26 compared to 144). In short, these differences reflect the fact that charter schools tend to locate in urban districts. The above differences can be reduced using other methods and by eliminating outlier treatment districts, but these changes in method and limits in sample lead to only small differences in the effect estimates, as we later show.

We focus our analysis on the commuter sample. Table 2A shows that 91 of the 290 districts with at least one education-degree-granting IHE within its boundaries also have at least one charter school. We also carry out some analysis on the larger sample of 1,143 districts with any education degree granting IHE (commuter or otherwise), of

[14] Tables A2 and A3 present the unweighted summary statistics for the commuter sample.

which 240 have charter schools. Tables A5 and A6 present the summary statistics for this larger sample.[15]

3. Identification Strategy

The empirical analysis uses a matched difference-in-differences approach. As a robustness check, we also use a district fixed effects model without matching. The main dependent variable is the ratio of education-to-total bachelor's degrees by district. We then explore effect heterogeneity by charter enrollment thresholds, school subject, college location, and race of the degree recipients.

Any effect of charter entry on new teacher supply might be delayed, especially if initial charter enrollments are low when they first open. Information about the effect on the demand for teachers might also be delayed in reaching college students and/or some students would have partially completed degrees at the time charter schools enter and might choose to continue even with diminished demand. Nevertheless, rather than lag the treatment, we take t=0 as the point at which the first charter school opens and, if there are effects, we estimate a dynamic model that allows us to observe potential delayed effects, which, if they exist, we expect to arise with at least a 2-4 year lag.

3.1 Difference-in-Differences

We use a generalized DD model to estimate the effect of charter school market share on the teacher labor market. The treatment group is districts with a charter market share above zero at any time during our panel period. For the main specification, we use charter school enrollment but also define treatment using the share of charter schools as a

[15] Tables A6 and A7 show the unweighted summary statistics for the full sample.

robustness check. We use two definitions for the comparison group. First, the comparison group is districts (in all states) that never have any charter enrollments. Second, we use only the districts in states that never had charter laws as of 2018 and whose districts by default never have charter schools.[16]

All models include district fixed effects μ_i to control for average differences across districts that may be correlated with opening charter schools and the new teacher supply, year fixed effects λ_t to account for broader national trends, and district-level-by-year controls X_{it} allow us to account for exogenous population changes. The resulting DD model is:

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

where y_{it} is the outcome variable of interest for district i in year t. T_i is an indicator variable equal to 1 if the district *i* charter enrollment share is ever above zero and equal to 0 otherwise (see above definitions of the comparison group); $Post_{it}$ is an indicator that equals 1 for the first year district that *i* had at least one charter school and the subsequent years.

The coefficient of interest, β, measures the charter effects on the outcome variables. Our key identifying assumption is that the comparison and treatment groups would have followed the same trend in the absence of treatment, which we can partially test using standard methods with observable pre-trends. (See later discussion of threats to identification.)

[16] The states are Washington, Kentucky, West Virginia, Montana, Nebraska, North Dakota, South Dakota, and Vermont.

3.2 Event Studies

We use Sun and Abraham (2021) and a traditional event study version of (1) to estimate the dynamic treatment effects and investigate the parallel trends assumption. As noted above, we expect a 2-4-year lag in the effect of charter entry on teacher supply. The traditional event study model is:

$$y_{it} = \alpha + \sum_{r=-m}^{q} \beta_r (T_i \cdot d_{i,r}) + X_{it}\gamma + \mu_i + \lambda_t + \varepsilon_{it} \quad (2)$$

where $d_{i,r}$ is a dummy of the *r* years of leads or lags since district *i* initiated first charter school.[17] The coefficients β_r are measures of year-specific effects compared with the comparison group. All other terms are defined as in (1) above.

To account for potential heterogeneous effects due to variation in treatment timing as discussed by Goodman-Bacon (2021), we also use the Sun and Abraham (2021) event study model:

$$y_{it} = \sum_{r \in G} \beta_r 1\{t - E_i \in r\} + \mu_i + \lambda_t + X_{it}\gamma + \epsilon_{it} \quad (3)$$

where y_{it} is the share of education preparation degrees and E_i is the year that district *i* initially opened a charter school. The set R collects disjoint sets r of relative periods after charter entry representing districts opening charters at different times. β_r is represents the weighted average charter effect for different charter entry times and districts.[18]

In another recent paper advancing DD analysis, Goodman-Bacon (2021) explains that DD estimates can be difficult to interpret in staggered-start designs; in these cases, identification comes from many sources of variation. For example, comparing districts

[18] See Sun and Abraham (2021) for a full discussion of the estimator.

that opened charter schools early in the time period to those that opened later in the time period or comparing treated districts to districts that never open charters. The main DD estimate is a combination of these 2x2 DD estimates that are made up of two groups and two time periods. The Goodman-Bacon (2021) decomposition tells us what comparisons are being made, what the control group is during these comparisons, and how much weight each carries in the average treatment effect. If the results are driven by late versus early comparisons, the estimated effect may be biased if the effect varies over time. Further, the parallel trends assumption may not hold for these comparisons.

In some of the 2x2 DD comparisons, districts that opened charters early act as the control group, but the share of education preparation degrees in these districts may be contaminated since they represent differences in the effect of charter entry between early charter districts and districts that opened charters later. In this instance, the effect from using districts that opened charters early as controls may be weighted negatively and bias the average charter effect. Sun and Abraham (2021) improve on traditional fixed effects models by accounting for these potential "bad" comparisons.

3.2 Matching and Parallel trends

We match treated and untreated districts within the DD framework to minimize the differences between the two groups in some specifications. We specifically use PSW and PSM to match districts using the 1990 (pre-treatment) share of education bachelor's degrees.[19] The DD-PSM uses nearest neighbor matching and DD-PSW uses the inverse probability of selection. We prefer the DD-PSW because it yields greater similarity on

[19] Figures A2-A3 show the kernel density and balance test results for the main matching analysis. The treated and control districts are more balanced after matching.

baseline characteristics and because it maintains a larger sample and improves precision.[20]

While matching using the 1990 level of the outcome variable ensures a similar share in education bachelor's degrees for treated and control districts prior to treatment, this does not correct for all differences. Table 2B shows differences in average district size for treated and control districts. In the main analysis, enrollment in the treated districts is about 95,000 and is about 15,000 in control districts. A concern is that the estimated effect may be reflecting these differences in district size in addition to any effects of charter entry. Therefore, as a robustness check, we also match by adding the 1990 levels of the control variables and enrollment. Using this method, we match the treated and control districts so that average enrollment in treated districts is 9,571 and 4,935 for control districts in the PSM sample.

This combination of DD and matching has three advantages. First, it helps ensure that the comparison and treatment groups had similar pre-trends. Matching on the probability of treatment tends to generate greater similarity in outcomes.

Second, this method addresses the likely possibility that idiosyncratic shocks (e.g., in state policies) coincide with charter entry and target districts with the same observable characteristics that also correlate with charter entry. For example, charter schools might locate in districts with low-performing TPS and state policies might also target these schools at the same time that charter schools open. In that case, matching will limit the comparison group to districts with similar school performance, thus reducing

[20] The districts are given a propensity score that predicts the likelihood of receiving treatment, regardless of being in a state with a charter law (Austin, 2011).

potential bias. Finally, matching can reduce the unexplained control-treatment variance in the dependent variable, which improves statistical power.

3.4 Threats to Identification

The main identifying assumption in DD analysis is that the treatment districts would have followed the same trajectory as the comparison group in the absence of treatment. We therefore provide parallel trends tests with each estimate later.

The conditional exogeneity assumption might also be violated if non-charter education policies affecting teacher supply are correlated with unobservable district characteristics. For example, the districts that allow or encourage charter schools might also be more likely to institute teacher accountability policies in traditional public schools, which have been shown to affect teacher supply (Kraft et al., 2020).

We take several steps to mitigate possible bias due to endogenous charter location in the DD model. We can control for observables and time-invariant unobservable factors that are correlated with charter location and the teacher labor market. We then use several methods to deal with time-varying unobserved factors and endogenous timing. First, in one of the DD variations, we only include control districts that have no charter laws up until 2018. This reduces the potential extent of endogeneity because these comparison districts never had the opportunity to open charter schools, even the districts with unobservable characteristics that might have made is likely that charter schools would locate there if they had been allowed.

Second, we address endogeneity with robustness checks by comparing districts in states that were early adopters of charter school laws (passed the law prior to 2010) to districts in late-adopting states. By comparing early and late adopters, we exploit the

exogenous *timing* of charter school laws.[21] A disadvantage of this approach is that there are few late-adopter states and many of them are in rural areas; this differs from the treatment group and geography is correlated with the teacher labor market outcomes.

Third, we conduct placebo tests leveraging variation in the types of degrees granted. For example, we should see more limited changes in the supply of elementary education degrees when middle and high school charters enter. If our estimates are robust to all these estimation methods, then we argue that this will provide a strong claim to causal inference.

4. Results

4.1 Difference-in-Differences Results

We start by using DD and event study models to estimate the impact of charter school enrollment on the share of education bachelor's degrees (i.e., the ratio of education BAs to all BAs). Unless otherwise noted, the estimates are based on the commuter sample and weighted by the size of the school-age population, though we note differences with the unweighted results. Our preferred specification is the DD-PSW model since the treated and control districts are more similar in the pre-period.

We first present Goodman-Bacon (2021) decomposition plots and the Sun and Abraham (2021) event study results. Figure 3 shows the decomposition scatter plot. The most heavily weighted DD comparisons are from the treatment group and never treated

[21] States that adopted charter laws between 1991-2003 (early adopter states) are: Minnesota, California, Colorado, Massachusetts, Michigan, New Mexico, Wisconsin, Arizona, Georgia, Hawaii, Kansas, Alaska, Arkansas, Delaware, Louisiana, New Jersey, Rhode Island, Texas, Wyoming, Connecticut, District of Columbia, Florida, Idaho, Illinois, New Hampshire, North Carolina, South Carolina, Nevada, Ohio, Pennsylvania, Missouri, New York, Utah, Virginia, Oklahoma, Oregon, Indiana, Iowa, Tennessee, Maryland. This analysis omits states that passed their first charter laws in the middle years; and states that never adopted charter laws as of 2020 (Montana, Nebraska, North Dakota, South Dakota, Vermont). No states passed laws between 2004-2009.

group, indicating that the DD results are likely not biased even if the effect varies over time. Although there is little evidence of problematic DD comparisons, we use the Sun and Abraham (2021) event study estimates (Figure 4) to account for any potential timing issues for the main DD estimates.

Figure 4 shows no evidence of pre-trends and that the share of education degrees steadily declines after treatment.[22] Table 3 provides the analogous DD (columns 1 and 2), DD-PSW (columns 3 and 4), and DD-PSM (columns 5 and 6) using the weighted commuter and full sample. Columns 1, 3, and 5 are estimated without controls and columns 2, 4, and 6 include controls. In Table 3 Panel A, districts with any charter enrollment saw significant decreases in the education degree shares with coefficients of -0.00815 to -0.00921 in the preferred (matching) specifications with the commuter sample.[23] These might seem small as they imply that charter entry of 10 percent market share reduces the share of all degrees by about one percentage point. However, the pre-charter entry mean education-to-total degrees ratio is 0.0607, which implies a 13.5-15.2 percent decrease. Using the full sample (Panel B), the effect is also negative and similar in magnitude (-0.00911) with estimates in the preferred specifications using matching. As in the event study, the effect is small and insignificant using imprecisely estimated in the unweighted commuter sample.

[22] The event study results are robust to using the weighted full sample of districts, but the effect is smaller in magnitude (Figure A4). Estimates using the commuter sample without weights yield no effect (or positive effects) (Figure A5). As we show later, this reflects effect heterogeneity by district size, which has been observed in studies of other types of charter school effects (e.g., Chen & Harris, 2022). The traditional event study (Figure A6) shows little evidence of pre-treatment trends.

[23] The pre-trend tests in Table 4 show the time trends between treated and control districts are not significantly different, thus providing further evidence that the parallel trends assumption holds.

4.2 Robustness

We conduct several additional robustness by using alternative measures of charter market share, matching, comparison groups, and commuter sample thresholds. We also conducted a Goodman-Bacon (2021) decomposition and include a fixed effects analysis as an additional robustness check.

4.2.1 Alternative treatment measures

First, we use charter share of schools as an alternative to charter share of enrollment to define market share. The results are similar to the main DD model (Table A11) using the commuter and full sample. We then vary the matching specifications. The baseline matching technique matches using the 1990 level of the share of teacher preparation bachelor's degrees. When we change this to match on the 1990 levels of the outcome variable, control variables, and enrollment, the estimates using the commuter sample are again very similar to main DD (Table A12). Using the PSM model that closely matches the enrollment sizes for treated and control districts, the estimated effect is still about a 14.5 percent decline but is less significant at the 5 percent level.

The main results are also robust to using an alternative control group: districts in states that never adopted state charter laws. By using this control group, we are comparing treated districts with districts that never had the opportunity to open charters, thus helping with some endogeneity concerns. Table A13 shows that using this control group provides similar results to the main DD model. Since the commuter sample thresholds are defined somewhat arbitrarily, we use three more restrictive thresholds to define commuter schools. The results using these various thresholds are again similar to the baseline commuter sample (Table A14).

We then use the number of education bachelor's degrees as an outcome variable and the results are not robust to this alternative outcome measure (Table A15). The coefficients are still negative and of roughly the same magnitude, though no longer statistically significant. While we cannot be sure of the reason, there are many factors that might be correlated with both charter entry and demand for college credentials generally. Therefore, we view these results as less credible, but still qualitatively similar.

Finally, Table A16 shows the results using districts in late adopter states as the comparison group and the commuter sample. Using the commuter sample, the effect of charter is consistently negative (like the main DD) across all models, but the coefficients are insignificant. Overall, the results are robust to using an alternative control group and matching on enrollment size, although the estimated effect is less precisely estimated because some of the largest districts could not be matched. The main results are not robust to excluding weights or some of the alternative specification models.

4.2.2 Fixed effects analysis

The DD analysis has the disadvantage of putting all treated observations in the same treatment category even though all of them vary in treatment intensity (i.e., the extent of charter market share). Districts with a larger (smaller) share of charter schools, for example, may experience a larger (smaller) effect on education degrees. Therefore, as a last robustness check, we treat charter market share as a continuous variable and use a fixed effects model, which identifies effects from within-district variation in charter market share over time. Tables A17 and A18 show these results using the same year charter enrollment share and the average of the last four years of charter market share.

Using charter enrollment as a continuous variable with district fixed effects (Table A17), the estimated effects using the commuter (Panel A) and full sample (Panel B) are still negative, but significant in only one specification. When we exclude weights (Table A18), the fixed effects results are insignificant. This loss of precision is generally common with the addition of fixed effects. Still, the direction of the estimates is almost always the same—negative—even as precision varies. These results may reflect our findings that the average charter enrollment share must grow before there is an impact on the teacher supply.

4.2.3. Other Robustness Checks

All the estimates reported so far weight by district enrollment. This is because we are interested, among other things, in explaining the larger national trend in new teacher supply. However, weighting could mask effect heterogeneity by district size, which itself is correlated with urbanicity. Indeed, when we estimate effects unweighted the results become less significant.[24]

The DD results are also robust to using a variation on the Sun and Abraham method, a two-stage difference-in-differences approach Gardner (2021),[25] but the coefficients are smaller in magnitude and less precisely estimated. We explore effect heterogeneity further in section 5. Again, we note here that the results of these robustness checks imply a similar reduction in new teacher supply.

[24] Table A10 shows the unweighted DD results.

[25] The Gardner (2021) DD estimator has two steps to account for staggard treatment timing. The first step regresses the untreated outcomes on the district and year time effects. The second step subtracts these effects from the observed outcomes and then regresses these adjusted outcomes on treatment status. Results are available upon request.

4.3 Tests for Identification

Earlier, we show that our results pass parallel trends tests, which is a useful, but insufficient, starting point. In this section, we also test whether these can be interpreted as causal effects using placebo analyses and tests for other potential mechanism—changes in district population.

4.3.1. Placebo Tests

We conducted several placebo analyses using the share of charter enrollment for different grades. Supply should be most affected in majors aligned with the same grade level of the schools (e.g., the entry of charter elementary schools should affect the number of elementary education degrees more than other degrees). We assume that teachers majoring in education are aware of the types of charter schools entering the district. Table A19 shows that the estimated impacts of charter middle and high schools on elementary degrees are about half the size (in absolute value) as the effects of entering elementary charter schools. This is consistent with the idea that our main estimates mostly reflect causal effects. We also carried out the placebo analysis within the FE model (Table A20). Again, the point estimates are about half the size when examine the effects of charter middle and high schools on elementary education supply.

As an additional placebo test, we change treatment to five years prior to the year of actual treatment. Table A21 shows that the impact of charter schools before actual treatment occurs is insignificant, which provides some evidence that the main DD estimate is likely causal.

4.3.2. Endogenous Student Population

Student outcomes are highly correlated with student demographics, which has led to considerable discussion of the possibility that the apparent effects of charter schools are driven by changes in who is being served. Changes in the student population could also affect the new supply of teachers (Hanushek & Rivkin, YEAR). Therefore, we have estimated many of the models with and without student demographics. However, we can also address this more directly by estimating the effects of charter entry on demographics.

Tables A22 through A24 show the effect of charter entry on district enrollment, share of free or reduced-price lunch recipients, and the racial composition of students in a district. Charter entry does not have a significant effect on enrollment or the share of Black or Hispanic students (Tables A22 and A24) in a district (although there is a marginally significant effect in some specifications), but there is a significant reduction in the share of FRL recipients (Table A23). This indicates that charters may be impacting the composition of students; however, this is a small 1.5 percent decline in FRL recipients.

5. Effect Heterogeneity

5.1 Effects by Charter Market Share Thresholds

The DD model assigns treatment to all districts with any charter enrollment during the sample period. However, there could be differential effects depending on the charter enrollment share. Figures 5 and A11 show how the estimates change when the share of charter enrollment is 1-25 percent. For example, the estimated effect of 15 percent charter enrollment defines the treated group as districts that ever have a charter share of 15 percent or greater and the comparison group as districts that never open

charter schools. Figure 5 shows no effect of charter entry until the charter share grows to about 13 percent. For districts with a least a 13 percent market share, charters lead to a decline in the teacher supply. For these districts (charter share of at least 13 percent), the point estimate is about -0.01.

The full sample (Figure A7) also shows a negative relationship between charter entry and teacher supply across the different thresholds, but the effect begins earlier than the commuter sample at around five percent. This could reflect several factors: (a) non-linearities in the effects on teacher supply; (b) compositional differences in the types of districts that reach large charter market shares; and (c) differences in the timing of charter entry (districts with more charters had their first charter earlier).

5.2 Effects by metropolitan area status

Table 4 reports effects for metropolitan areas and non-metropolitan areas separately.[26] In metropolitan areas, the effect of charter entry is consistently negative and significant across the three models. There is a significant 0.00845 percentage point decline in teacher preparation degrees, or about a 13.9 percent decrease compared to the pre-charter entry mean (0.0607). However, in non-metropolitan areas, charter school entry is associated with an increase in teacher preparation degrees, with magnitudes ranging from 0.0182 to 0.0453 (significant at the 10 and 1 percent levels). Charter entry leads to about a 29.4 percent increase using the DD-PSW model compared to the mean (0.0619). These results show that the main DD effect is likely driven by districts in metropolitan areas.

[26] Metropolitan areas have at least one urban school in the district, as defined by the CCD.

5.3 Effects by school subject

Table 5 presents the effect of charter entry by the school subject, where the outcome variable is the ratio of specific education preparation degrees to total bachelor's degrees. We find that, although the effect is consistently negative for all types of education degrees, only three of these are significant. The point estimates suggest a decrease in elementary education (-0.00516), special education (-0.00139), and math (-0.000280) degrees in our preferred specification. Compared to the pre-charter entry means, these translate into a 19.1 percent decline for elementary education (0.0270 mean), a 25.3 percent decline for special education degrees (0.00549 mean), and a 28.8 percent decrease for math education degrees (0.000971 mean) using the DD-PSW model.

5.4 Effects by race

We then explore whether the impact of charter school entry differs by race of teachers. The outcome variable is the share of education degrees for each race. Table 6 shows that the negative effect of charter entry on the teacher supply is concentrated among white and Black education preparation completers, which is not surprising since white individuals have the highest share of pre-charter entry education preparation degrees (0.0670) followed by Black education degrees (0.0456). The point estimate -0.00749 translates to a 11.2 percent decline in the supply of white teachers compared to the pre-charter entry mean. The effect for Black education degree completers is similar (-0.00770) but less precisely estimated compared to white individuals. Using the preferred model, districts opening charter schools decreases the share of Black completers by 16.9 percent on average compared to the pre-charter entry mean. The effect of charter entry on

Asian and Hispanic degree completers is also negative but is insignificant or marginally significant.

6. Discussion and Conclusion

Charter schools potentially change the market for schooling in many ways. The original intent was to create new and innovative schools, offer families alternatives, and create competition with traditional public schools. Since charter schools have more autonomy, especially over personnel, it is also not hard to imagine changes in recruiting, hiring, development, compensation, evaluation, and dismissal that affect the teacher labor market as well (Chubb and Moe, 1990).

Our results suggest that charter schools have complex effects on the supply of teachers from college-based education degree programs. Charter entry decreases the supply of traditionally prepared teachers on average, but these effects seem to be driven by changes in large districts. The direction and magnitude of the coefficients are generally robust to using alternative comparison groups, alternative identification strategies and specifications, and the alternative (and less credible) dependent variable. In some of these robustness checks, the results become less precise, but the general similarity of the estimates across many different estimates and the precise estimation in our preferred estimates, reinforces the validity of our findings. The main exception is that the results are not robust to dropping the weights, but this reflects the effect heterogeneity noted above.

It is also important to note that our main sample only includes districts with commuter institutions, which is where we expect the effects to be the largest. Therefore, there are likely smaller effects on average in most of the country. When the charter

market share grows to about 7 percent, we expect about a 10 percent decrease in the share of new education majors nationwide. This estimate is likely an upper bound. Our findings support previous studies that find students are knowledgeable and responsive to labor market conditions (Acton 2020; Baker et al. 2018).

We also see differential effects by district characteristics and teacher's race. We find evidence of declines in total degrees in metropolitan (mostly urban) areas. This may be related to the larger pattern of results showing that charter schools tend to be more effective in urban locations, e.g., in raising student achievement. It may be that urban charter schools are more likely to hire alternatively prepared teachers and that they are effective in doing so, as suggest by Bruhn et al. (2018). These results may also reflect differences in the average charter share for different school districts. The average charter share in metropolitan areas is 17.81 percent compared to 0.76 percent in rural areas, which the heterogeneity analysis shows is important. We also find evidence that charter entry leads to a decrease in the supply of both white and Black teachers. Compared to the mean, this decline is larger for Black teachers.

Elementary, special education, and math majors also seem to decline when charter schools enter. Most charter schools serve elementary students, so this may be why there is a larger effect on elementary than secondary education majors when charters enter. A decline in math education degrees may reflect interest among charter schools in hiring teacher candidates who have degrees in the discipline instead of in education. For example, our data only include education majors; students majoring in math or biology for example could also be minoring in education to make themselves attractive to charter

schools.[27] Finally, charters seem to impact special education majors. This may be related to charter schools assigning fewer students to special needs programs compared to public schools, thus lowering demand for these types of teachers as more charters open (Estes, 2004).

While our results imply a decline in the supply of new traditionally prepared teachers, the share of education degrees is an imperfect proxy for the teacher supply. Teachers may find jobs in a district or state different from where the institution is located, which our study is unable to address. Also, not all education preparation degree completers become teachers or intend to teach in public schools. On average, 45 to 65 percent of education graduates become professional teachers, therefore, our findings may be a lower bound in this regard (Kraft et al. 2020). Finally, our findings cannot comment on the number of new alternatively prepared teachers. It is possible that universities are responding to charters schools opening by offering more of these alternatively preparation programs.

While a decline in the share of graduates from IHEs is important, this does not mean the average quality of the teacher workforce has declined. Some non-traditional routes, such as *Teach for America*, suggest that alternately prepared teachers are sometimes more effective (Glazerman, Mayer, and Decker, 2006; Clark et al., 2013). Bruhn et al. (2018) find that charter schools induce more alternatively prepared teachers into the market and that some of these teachers are quite effective and end up staying in the profession. Our analysis focuses on the quantity of teachers at a national scale, where

[27] We find that charter entry marginally declines education preparation awards that require 2-4 years to complete. We find no effect on education preparation awards that require 1-2 years or less than 1 year (Table A25).

analysis of quality is infeasible. Ultimately, a combination of studies will be necessary to understand these effects: small-scale analyses where average teacher quality measures are available for both TPS and charter schools, along with national analyses, like ours, of the quantity of teachers.

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Tables and Figures

Table 1 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 2A Summary statistics, IPEDS data

	ALL	DD & DD-PSW		DD-PSM	
		Treated	Control	Treated	Control
Total bachelor's degrees	6,769	7,007***	5,738	7,007***	7,362
Share of ed. prep. degrees	0.0499	0.0490***	0.0538	0.0490***	0.0408
Share of social studies ed. degrees	0.00122	0.00126	0.00106	0.00126***	0.000845
Share of elementary ed. degrees	0.0226	0.0222***	0.0242	0.0222***	0.0131
Share of special ed. degrees	0.00386	0.00409**	0.00286	0.00409	0.00241
Share of English and L.A. ed. degrees	0.00110	0.00116***	0.000850	0.00116	0.000623
Share of science ed. degrees	0.000562	0.000562***	0.000560	0.000562	0.000529
Share of math ed. degrees	0.000826	0.000894***	0.000533	0.000894***	0.000346
Black share of ed. degrees	0.0340	0.0345	0.0319	0.0345***	0.0296
White share of ed. degrees	0.0532	0.0528***	0.0554	0.0528***	0.0415
Asian share of ed. degrees	0.0218	0.0208***	0.0260	0.0208***	0.0162
Hispanic share of ed. degrees	0.0362	0.0356***	0.0390	0.0356***	0.0327
Ed. awards (2≤years<4)	0.000930	0.000300***	0.00430	0.000300***	0.00455
Ed. awards (1≤years<2)	0.0196	0.0131***	0.0491	0.0131**	0.0102
Ed. awards (years≤1)	0.0120	0.0124	0.0100	0.0124	0.0126
Junior high, middle school, intermediate education	0.00150	0.00171***	0.000573	0.00171***	0.000611
Secondary school education	0.00256	0.00227***	0.00385	0.00227***	0.00197
Institutions per district	2	2***	2	2***	2
Observations	8,256	2,639	5,617	2,639	2,592
N (districts)	290	91	199	91	91

Notes: Treatment and control group means significantly different at 1-percent level (***), 5 percent level (**), or 10 percent level (*). This table presents the summary statistics for the outcome variable and control variables for the commuter sample for the (spring) years 1990-2018. Variables are weighted by number of school age children. The treated group are districts that ever have a charter enrollment share. The comparison groups are districts that never have charter schools in all states for the DD, DD-PSW, and DD-PSM. The number of institutions is rounded to the nearest whole number.
Data source: IPEDS NCES Database.

Table 2B Summary statistics, NLSD data

	ALL	DD & DD-PSW		DD-PSM	
		Treated	Control	Treated	Control
Enrollment	79,895	94,901***	14,732	94,901***	15,673
White	0.391	0.341***	0.608	0.341***	0.578
Black	0.358	0.401***	0.172	0.401***	0.176
Hispanic	0.196	0.204***	0.162	0.204	0.186
Free or reduced-price lunch	0.594	0.620	0.483	0.620***	0.446
Population 5-17	0.169	0.170	0.161	0.170***	0.161
Population 5-17 in poverty	0.186	0.193	0.154	0.193***	0.148
Special education	0.128	0.128***	0.127	0.128	0.121
Urban	0.885	0.905***	0.798	0.905***	0.847
Suburb	0.425	0.424***	0.430	0.424	0.481
Town	0.0278	0.0135***	0.0929	0.0135***	0.0887
Rural	0.0597	0.0291***	0.201	0.0291***	0.120
Revenue per student	6,848	6,274***	9,331	6,274***	9,426
Expenditure per student	6,953	6,377**	9,448	6,377***	9,526
Student-teacher ratio	18	18	17	18***	17
Average teacher salary	59,634	54,910***	80,085	54,910***	84,000
Number magnet schools	20	22***	5	22***	6
Number schools	122	144***	26	144***	27
Observations	8,256	2,639	5,617	2,639	2,571
N (districts)	290	91	199	91	91

Notes: Treatment and control group means significantly different at 1-percent level (***), 5 percent level (**), or 10 percent level (*). This table presents the summary statistics for the outcome variable and control variables for the commuter sample for the (spring) years 1990-2018. Variables are weighted by number of school-age children. The treated group are districts that ever have charter enrollment. The comparison groups are districts that never have charter schools in all states for the DD, DD-PSW, and DD- PSM. Degrees are bachelor's degrees. Revenue per student, expenditure per student, average teacher salary, number of magnet schools, and number of schools are rounded to the nearest whole number.
Data source: National Longitudinal School Database.

Table 3 Effects of charter entry on teacher supply

	(1)	(2)	(3)	(4)	(5)	(6)
	DD		DD-PSW		DD-PSM	
Panel A: Commuter Sample						
Impact of Charters	-0.00764**	-0.00840***	-0.00725**	-0.00815***	-0.00866***	-0.00921***
	[0.00301]	[0.00290]	[0.00312]	[0.00297]	[0.00317]	[0.00297]
Pre-trend Test	-0.000798	-0.000860	-0.00100	-0.00112	-0.000693	-0.000875
	[0.000667]	[0.000795]	[0.000687]	[0.000823]	[0.000696]	[0.000958]
Observations	7,507	7,507	7,507	7,507	4,727	4,727
N (district)	289	289	289	289	182	182
Panel B: Full Sample						
Impact of Charters	-0.00558	-0.00735*	-0.00723*	-0.00911**	-0.00711**	-0.00854**
	[0.00396]	[0.00435]	[0.00423]	[0.00441]	[0.00358]	[0.00373]
Pre-trend Test	0.000504 [0.000439]	0.000369 [0.000568]	0.000212 [0.000483]	-0.000354 [0.000654]	0.000114 [0.000494]	-0.000562 [0.000671]
Observations	29,465	29,465	29,465	29,465	12,489	12,489
N (districts)	1,135	1,135	1,135	1,135	481	481
District, year FE	Yes	Yes	Yes	Yes	Yes	Yes
District controls	No	Yes	No	Yes	No	Yes

Notes: Significantly different from zero at 1-percent level (***), 5 percent level (**), or 10 percent level (*). This table shows DD estimates of the effect of charter entry on the share of teacher preparation education bachelor's degrees. The treated group includes districts that ever have any charter enrollment and the comparison group includes districts without charter schools in all states. Controls include the log of district enrollment; the share of Hispanic students; the share of students on FRL programs; student-teacher ratio; the number of institutions; whether the district is in an urban location; the estimate of the school-age population; and 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 population (ages 5-17). For DD-PSW, regressions are weighted by the population (ages 5-17) times the inverse probability of the propensity score. The outcome variable is lagged by four years. The pre-charter entry mean share of education bachelor's degrees in the commuter sample is 0.0607. The pre-charter entry mean share is 0.0575 for the full sample. The pre-trend test compares the outcome variable time trends between treated and control districts using the years 1990 to 1997.
Data source: See data source notes to Tables 2A and 2B.

Table 4 Effect heterogeneity: metropolitan areas VS non-metropolitan areas

	(1)	(2)	(3)	(4)	(5)	(6)
	DD		DD-PSW		DD-PSM	
Panel A: Metropolitan area						
Impact of Charters	-0.00825***	-0.00895**	-0.00754**	-0.00845***	-0.00862***	-0.00922***
	[0.00305]	[0.00291]	[0.00317]	[0.00300]	[0.00321]	[0.00301]
Pre-trend test	-0.000421 [0.000753]	-0.000302 [0.000889]	-0.000626 [0.000785]	-0.000691 [0.000952]	-0.0000117 [0.000842]	0.00000180 [0.00111]
Observations	5,343	5,343	5,343	5,343	3,707	3,707
N (district)	246	246	246	246	162	162
Panel B: Non-metropolitan area						
Impact of Charters	0.0274***	0.0220**	0.0211**	0.0182*	0.00394	0.0453***
	[0.00928]	[0.00938]	[0.0103]	[0.00998]	[0.00886]	[0.0167]
Pre-trend test	-0.00111 [0.00392]	0.000433 [0.00476]	-0.000181 [0.00392]	0.00214 [0.00428]	-0.00233 [0.00418]	-0.000323 [0.00455]
Observations	2,100	2,100	2,100	2,100	979	979
N (district)	111	111	111	111	53	53
District, year FE	Yes	Yes	Yes	Yes	Yes	Yes
District controls	No	Yes	No	Yes	No	Yes

Notes: See notes to Table 3. Significantly different from zero at 1-percent level (***), 5 percent level (**), or 10 percent level (*). The table shows DD estimates of heterogeneous effects of charter entry on teacher supply by metropolitan and non-metropolitan areas using the commuter sample. The pre-charter entry mean share of education bachelor's degrees in the metropolitan areas is 0.0607. The pre-charter entry mean share is 0.0619 for non-metropolitan areas. The pre-trend test compares the outcome variable time trends between treated and control districts using the years 1990 to 1997.
Data source: See data source notes to Tables 2A and 2B.

Table 5 Effect heterogeneity: school subject

	(1)	(2)	(3)	(4)	(5)	(6)
	DD		DD-PSW		DD-PSM	
Panel A: Social studies education degrees						
Impact	-0.0000980	-0.000184	-0.000136	-0.000206	-0.000168	-0.000238
	[0.000183]	[0.000206]	[0.000183]	[0.000206]	[0.000188]	[0.000210]
Pre-trend test	0.0000149	0.0000204	0.00000716	0.00000435	-0.00000780	-0.0000193
	[0.0000431]	[0.0000531]	[0.0000435]	[0.0000552]	[0.0000432]	[0.0000572]
Panel B: Elementary education degrees						
Impact	-0.00347*	-0.00409**	-0.00350**	-0.00422**	-0.00516***	-0.00567***
	[0.00182]	[0.00185]	[0.00178]	[0.00177]	[0.00181]	[0.00180]
Pre-trend test	0.000395	0.000301	0.000264	0.000132	0.000428	0.000427
	[0.000441]	[0.000437]	[0.000438]	[0.000447]	[0.000417]	[0.000539]
Panel C: Special education degrees						
Impact	-0.00146**	-0.00153**	-0.00126**	-0.00123**	-0.00139**	-0.00138**
	[0.000600]	[0.000644]	[0.000575]	[0.000611]	[0.000581]	[0.000620]
Pre-trend test	-0.000344**	-0.000370**	-0.000341**	-0.000386**	-0.000264*	-0.000247
	[0.000174]	[0.000182]	[0.000164]	[0.000173]	[0.000160]	[0.000174]
Panel D: English and Language Arts education degrees						
Impact	-0.000266	-0.000279	-0.000316	-0.000318	-0.000302	-0.000304
	[0.000198]	[0.000210]	[0.000217]	[0.000228]	[0.000203]	[0.000215]
Pre-trend test	-0.0000291	-0.0000282	-0.0000351	-0.0000428	-0.0000771*	-0.0000952
	[0.0000619]	[0.0000718]	[0.0000618]	[0.0000721]	[0.0000445]	[0.0000671]
Panel E: Science education degrees						
Impact	-0.0000980	-0.0000737	-0.000123	-0.000101	-0.0000958	-0.0000774
	[0.000114]	[0.000113]	[0.0000933]	[0.0000968]	[0.000125]	[0.000125]
Pre-trend test	-0.0000382	-0.0000312	-0.0000373	-0.0000343	-0.0000521	-0.0000401
	[0.0000286]	[0.0000288]	[0.0000291]	[0.0000305]	[0.0000459]	[0.0000407]
Panel F: Math education degrees						
Impact	-0.000299***	-0.000278***	-0.000272**	-0.000238**	-0.000280**	-0.000256**
	[0.000113]	[0.000106]	[0.000112]	[0.0000998]	[0.000111]	[0.000101]
Pre-trend test	-0.000107**	-0.000119***	-0.000114***	-0.000131***	-0.000114***	-0.000133***
	[0.0000413]	[0.0000442]	[0.0000424]	[0.0000445]	[0.0000395]	[0.0000424]
Obs.	7,507	7,507	7,507	7,507	4,727	4,727
N (district)	289	289	289	289	182	182

District, year FE	Yes	Yes	Yes	Yes	Yes	Yes
District controls	No	Yes	No	Yes	No	Yes

Notes: See notes to Table 3. Significantly different from zero at 1-percent level (***), 5 percent level (**), or 10 percent level (*). The table shows the DD estimates of effect heterogeneity of charter entry on teacher supply by school subject for the commuter sample. Treatment is defined as any charter enrollment market share. The pre-charter entry mean share of social studies degrees is 0.00143, the mean share of elementary education degrees is 0.0270, the mean share of special education degrees is 0.00549, the mean share of English degrees is 0.00137, the mean share of science education degrees is 0.000657, and the mean share of math education degrees is 0.000971. The pre-trend test compares the outcome variable time trends between treated and control districts using the years 1990 to 1997.
Data source: See data source notes to Tables 2A and 2B

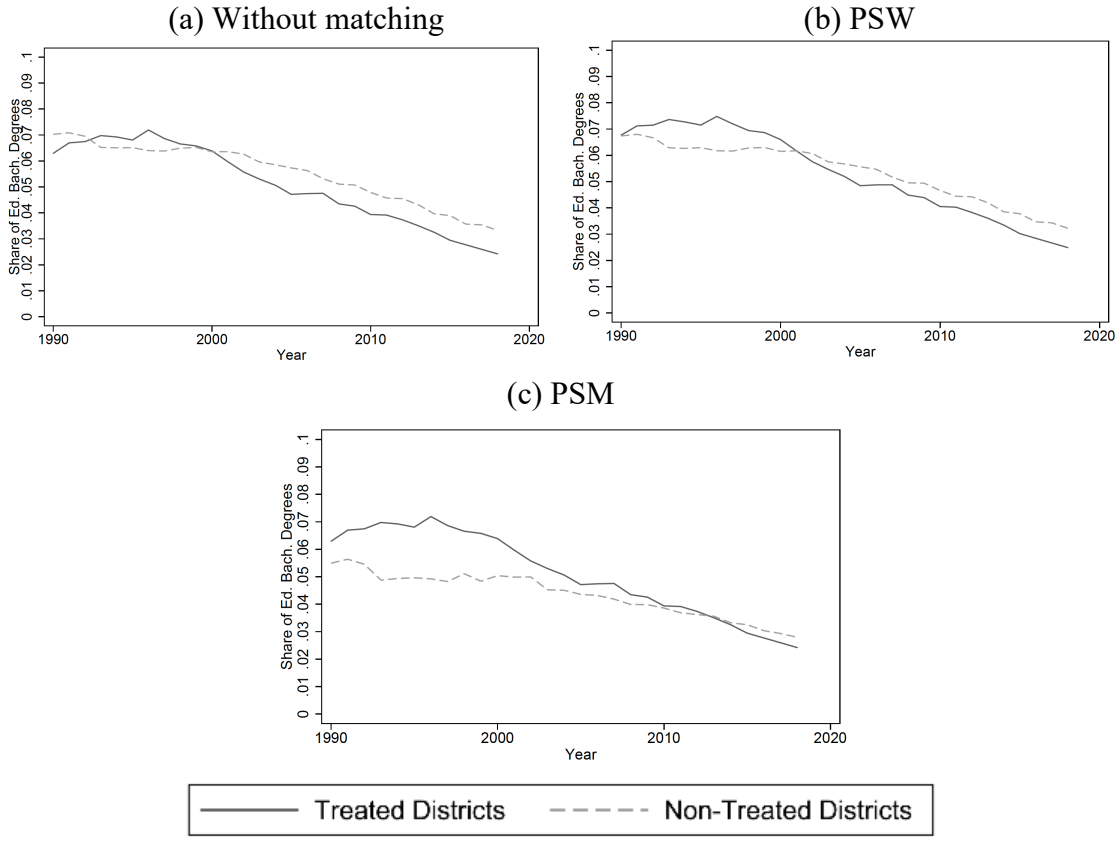
Table 6 Effect heterogeneity: education bachelor's degree completions by race

	(1)	(2)	(3)	(4)	(5)	(6)
	DD		DD-PSW		DD-PSM	
Panel A: White completions						
Impact of Charters	-0.00815*** [0.00311]	-0.00796*** [0.00282]	-0.00731** [0.00320]	-0.00749*** [0.00286]	-0.00818** [0.00326]	-0.00782*** [0.00281]
Pre-trend test	-0.000947 [0.000829]	-0.000360 [0.00100]	-0.00113 [0.000847]	-0.000573 [0.00106]	-0.000798 [0.000882]	-0.000126 [0.00115]
Observations	7,219	7,219	7,219	7,219	4,546	4,546
Panel B: Black completions						
Impact of Charters	-0.00813** [0.00394]	-0.00777** [0.00333]	-0.00768* [0.00402]	-0.00770** [0.00348]	-0.00844** [0.00403]	-0.00787** [0.00338]
Pre-trend test	-0.0000171 [0.000985]	0.000547 [0.00104]	-0.000214 [0.00103]	0.000377 [0.00110]	-0.000276 [0.00116]	0.000285 [0.00125]
Observations	7,190	7,190	7,190	7,190	4,543	4,543
Panel C: Hispanic completions						
Impact of Charters	-0.00720* [0.00391]	-0.00538 [0.00329]	-0.00850** [0.00412]	-0.00671* [0.00357]	-0.00783* [0.00398]	-0.00582* [0.00329]
Pre-trend test	0.000 [0.000]	-0.000200 [0.000661]	0.000 [0.000]	-0.0000952 [0.000683]	-0.000 [0.00000646]	0.0000399 [0.000604]
Observations	5,916	5,916	5,916	5,916	3,766	3,766
Panel D: Asian completions						
Impact of Charters	-0.00375 [0.00321]	-0.00258 [0.00318]	-0.00342 [0.00300]	-0.00227 [0.00296]	-0.00241 [0.00260]	-0.00143 [0.00260]
Pre-trend test	-0.000 [0.000]	0.0000663 [0.000250]	-0.000 [0.00000534]	0.0000815 [0.000290]	-0.000 [0.00000767]	0.000161 [0.000395]
Observations	5,634	5,634	5,634	5,634	3,648	3,648
N (district)	289	289	289	289	182	182
District, year FE	Yes	Yes	Yes	Yes	Yes	Yes
District controls	No	Yes	No	Yes	No	Yes

Notes: See notes to Table 3. Significantly different from zero at 1-percent level (***), 5 percent level (**), or 10 percent level (*). The table shows DD estimates of effect heterogeneity of charter entry on teacher supply by race of education preparation bachelor's degree completers for the commuter sample. Treatment is defined as any charter enrollment market share. The mean share of White education degrees is 0.0670, the mean for Black is 0.0456, the mean for Asian is 0.0236, and the mean for Hispanic is 0.0445. Information on the race of completers is available starting in 1995. Racial information on Hispanic and Asian completers

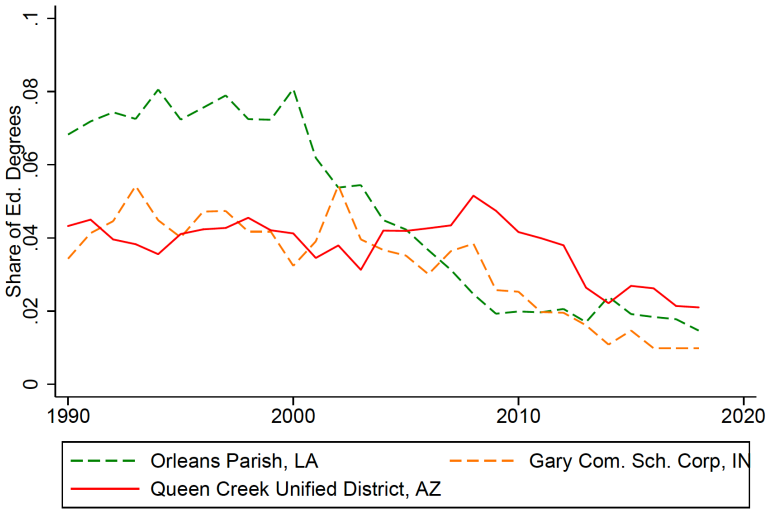
starts in 1999. The pre-trend test compares the outcome variable time trends between treated and control districts using the years 1990 to 1997.
Data source: See data source notes to Tables 2A and 2B.

Figure 1 Trends in share of education bachelor's degrees



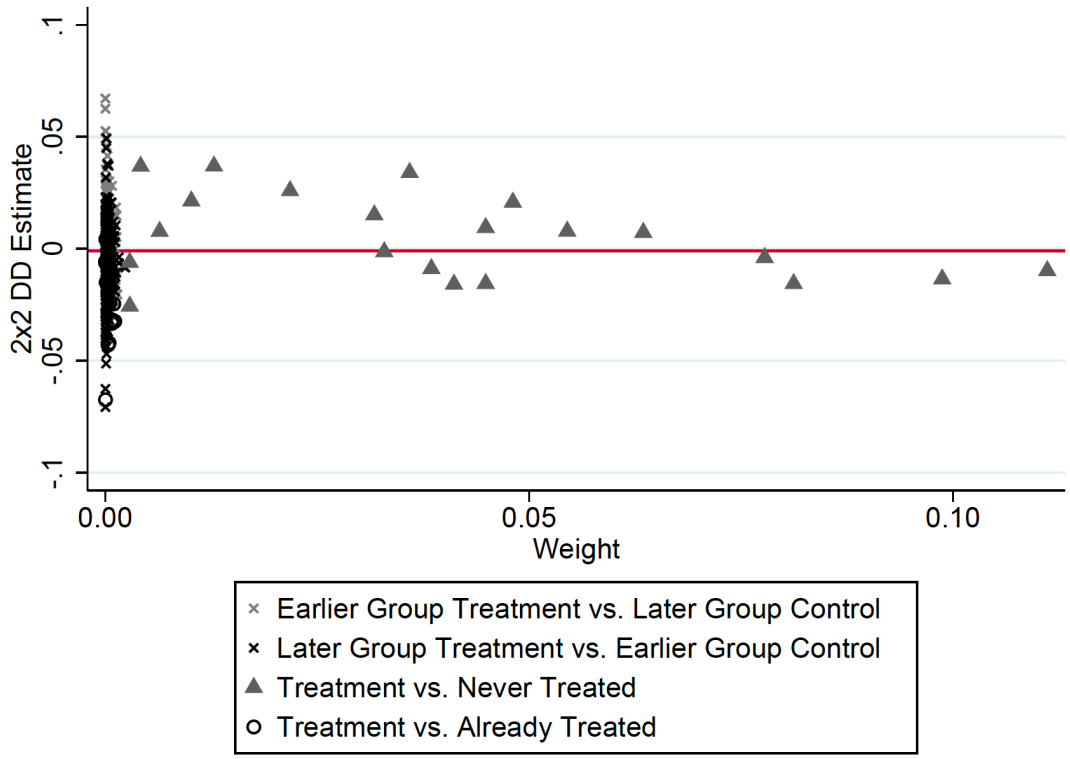
Notes: This figure plots the trends in share of education bachelor's degrees of treated districts (solid) and comparison (dashed) districts using the commuter sample. DD and DD-PSM are weighted by population (ages 5-17); DD-PSW is weighted by the DD weight times the inverse probability of the propensity score. Data source: See data source notes to Tables 2A and 2B.

Figure 2 Trends in education bachelor's degrees by districts with highest charter school market share



Notes: This figure plots the trends in share of education bachelor's degrees for districts with the highest charter school market share using the commuter sample. DD and DD-PSM are weighted by population (ages 5-17); DD-PSW is weighted by the DD weight times the inverse probability of the propensity score.
Data source: See data source notes to Tables 2A and 2B.

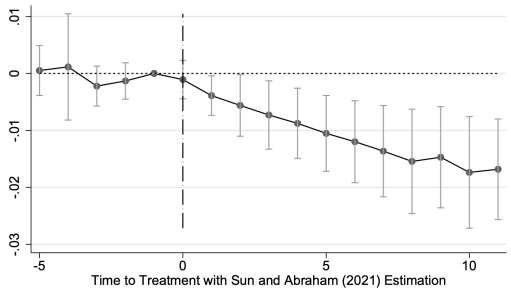
Figure 3: Goodman-Bacon decomposition



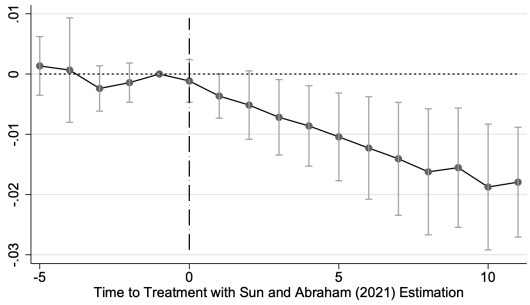
Notes: This figure plots the Goodman-Bacon decomposition (2021), which plots 2x2 DDs against their weight to show where identification comes from and heterogeneity of the 2x2 DDs.
Data source: See data source notes to Tables 2A and 2B.

Figure 4 Sun and Abraham (2021) event study of teacher supply

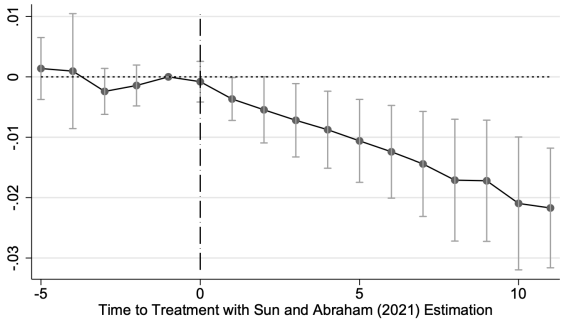
(a) DD



(b) DD-PSW



(c) DD-PSM

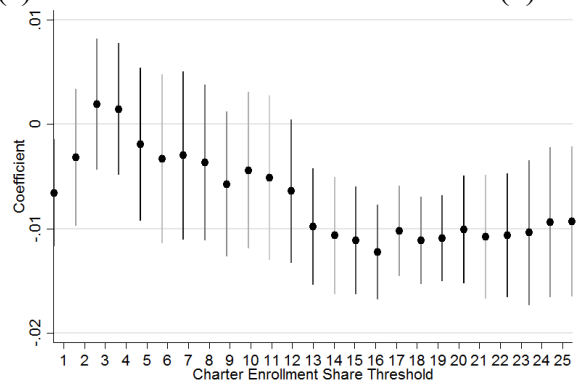


Notes: See notes to Table 3. This figure presents the Sun and Abraham (2021) event study results using the commuter sample. Treatment is defined as any charter school enrollment market share. Estimates are relative to the year prior to first charter entry, which is set to zero. 95% confidence intervals are reported based on robust standard errors clustered at the district level.

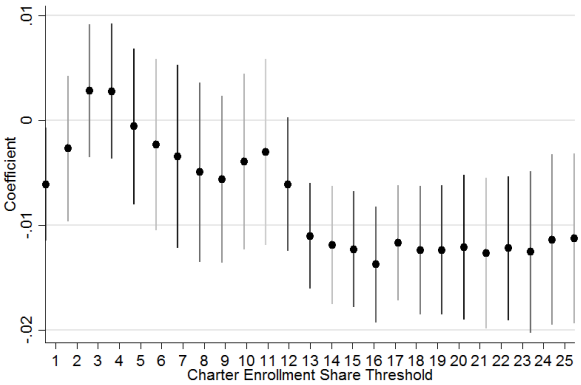
Data source: See data source notes to Tables 2A and 2B.

Figure 5 Plots of estimates using 1% to 25% as the threshold of treated districts

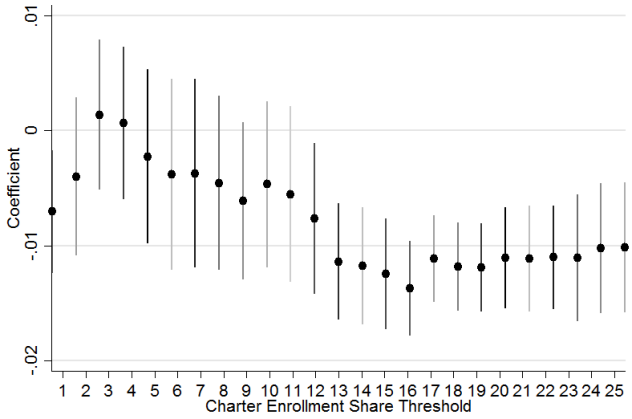
(a) DD



(b) DD-PSW



(c) DD-PSM



Notes: This figure plots the estimates in share of education bachelor's degrees using the charter enrollment share of 1- 25% as the threshold of treated districts and the commuter sample.
Data source: See data source notes to Tables 2A and 2B.

Appendix A: Tables and Figures

Table A1 Top 20 districts with largest charter enrollment share

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

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.

Table A2 Summary statistics, IPEDS data (unweighted)

	ALL	DD & DD-PSW		DD-PSM	
		Treated	Control	Treated	Control
Total bachelor's degrees	4389.0	5275.2	3981.8	5275.2	4721.5
Share of ed. prep. degrees	0.0741	0.0551	0.0828	0.0551	0.0702
Share of social studies ed. degrees	0.00165	0.00168	0.00164	0.00168	0.00134
Share of elementary ed. degrees	0.0364	0.0224	0.0428	0.0224	0.0328
Share of special ed. degrees	0.00472	0.00437	0.00488	0.00437	0.00458
Share of English and L.A. ed. degrees	0.00173	0.00156	0.00180	0.00156	0.00165
Share of science ed. degrees	0.000855	0.000765	0.000896	0.000765	0.000727
Share of math ed. degrees	0.000976	0.00102	0.000956	0.00102	0.000745
Black share of ed. degrees	0.0463	0.0340	0.0519	0.0340	0.0497
White share of ed. degrees	0.0689	0.0568	0.0745	0.0568	0.0623
Asian share of ed. degrees	0.0358	0.0251	0.0413	0.0251	0.0346
Hispanic share of ed. degrees	0.0495	0.0375	0.0552	0.0375	0.0490
Ed. awards (2≤years<4)	0.0125	0.000112	0.0203	0.000112	0.0117
Ed. awards (1≤years<2)	0.0128	0.0179	0.0107	0.0179	0.0101
Ed. awards (years≤1)	0.0168	0.0195	0.0155	0.0195	0.0205
Junior high, middle school, intermediate education	0.000940	0.00198	0.000461	0.00198	0.000398
Secondary school education	0.00455	0.00198	0.00573	0.00198	0.00540
Institutions per district	1	1	1	2	1
Observations	8,381	2,639	5,742	2,639	2,639
N (districts)	290	91	199	91	91

Notes: See notes to Table 2A. The table shows unweighted commuter sample summary statistics for the outcome variables and control variables.
Data source: See data source notes to Table 2A.

Table A3 Summary statistics, NLSD data (unweighted)

	ALL	DD & DD-PSW		DD-PSM	
		Treated	Control	Treated	Control
Enrollment	11,929	29,432	3,603	29,432	4,258
White	0.622	0.477	0.688	0.477	0.695
Black	0.163	0.318	0.0919	0.318	0.0812
Hispanic	0.137	0.155	0.128	0.155	0.165
Free or reduced-price lunch	0.518	0.587	0.486	0.587	0.436
Population 5-17	0.168	0.174	0.165	0.174	0.160
Population 5-17 in poverty	0.169	0.193	0.159	0.193	0.144
Special education	0.139	0.135	0.141	0.135	0.133
Urban	0.624	0.820	0.534	0.820	0.615
Suburb	0.329	0.387	0.300	0.387	0.391
Town	0.135	0.0884	0.160	0.0884	0.176
Rural	0.342	0.109	0.467	0.109	0.352
Revenue per student	9,894	8,752	10,419	8,752	10,830
Expenditure per student	10,009	8,925	10,507	8,925	10,888
Student-teacher ratio	16.07	17.26	15.52	17.26	15.82
Average teacher salary	76,181	73,121	77,587	73,121	82,867
Number magnet schools	11.25	13.17	3.914	13.17	4.853
Number schools	20.74	50.35	7.134	50.35	7.959
Observations	8,256	2,639	5,617	2,639	2,639
N (districts)	290	91	199	91	91

Notes: See notes to Table 2B. The table shows unweighted commuter sample summary statistics for the outcome variables and control variables.
Data source: See data source notes to Table 2B.

Table A4 Summary statistics full sample, IPEDS data

	ALL	DD & DD-PSW		DD-PSM	
		Treated	Control	Treated	Control
Total bachelor's degrees	5,777	6,749	3,224	6,749	3,248
Share of ed. prep. degrees	0.0513	0.0467	0.0633	0.0467	0.0512
Share of social studies ed. degrees	0.00134	0.00122	0.00165	0.00122	0.00136
Share of elementary ed. degrees	0.0252	0.0233	0.0301	0.0233	0.0242
Share of special ed. degrees	0.00386	0.00370	0.00425	0.00370	0.00395
Share of English and L.A. ed. degrees	0.00113	0.00113	0.00115	0.00113	0.00102
Share of science ed. degrees	0.000508	0.000471	0.000606	0.000471	0.000536
Share of math ed. degrees	0.000797	0.000729	0.000976	0.000729	0.000889
Black share of ed. degrees	0.0341	0.0335	0.0357	0.0335	0.0311
White share of ed. degrees	0.0540	0.0497	0.0655	0.0497	0.0568
Asian share of ed. degrees	0.0251	0.0229	0.0314	0.0229	0.0266
Hispanic share of ed. degrees	0.0374	0.0359	0.0415	0.0359	0.0344
Ed. awards (2≤years<4)	0.00311	0.00214	0.00864	0.00214	0.00801
Ed. awards (1≤years<2)	0.0134	0.00835	0.0327	0.00835	0.0216
Ed. awards (years≤1)	0.0364	0.0405	0.0210	0.0405	0.0109
Junior high, middle school, intermediate education	0.00135	0.00131	0.00144	0.00131	0.00127
Secondary school education	0.00200	0.00162	0.00300	0.00162	0.00168
Institutions per district	3	4	2	3	2
Observations	32,374	6,952	25,422	6,952	6,841
N (districts)	1,143	240	903	240	240

Notes: See notes to Table 2A. The table shows weighted full sample summary statistics for the outcome variables and control variables.
Data source: See data source notes to Table 2A.

Table A5 Summary statistics full sample, NLSD data

	ALL	DD & DD-PSW		DD-PSM	
		Treated	Control	Treated	Control
Enrollment	63,890	84,489	9,560	84,489	8,546
White	0.486	0.396	0.721	0.396	0.718
Black	0.284	0.340	0.136	0.340	0.137
Hispanic	0.182	0.213	0.103	0.213	0.104
Free or reduced-price lunch	0.565	0.606	0.458	0.606	0.446
Population 5-17	0.171	0.171	0.170	0.171	0.171
Population 5-17 in poverty	0.177	0.187	0.151	0.187	0.148
Special education	0.132	0.130	0.138	0.130	0.139
Urban	0.834	0.885	0.702	0.885	0.695
Suburb	0.447	0.474	0.373	0.474	0.402
Town	0.0730	0.0285	0.195	0.0285	0.168
Rural	0.116	0.0572	0.279	0.0572	0.289
Revenue per student	7,122	6,119	9,754	6,119	10,019
Expenditure per student	7,186	6,186	9,811	6,186	10,062
Student-teacher ratio	17	18	16	18	16
Average teacher salary	61,234	54,766	78,218	54,766	80,386
Number magnet schools	19	20	4	20	8
Number schools	96	126	17	126	16
Observations	32,374	6,952	25,422	6,952	6,841
N (districts)	1,143	240	903	240	240

Notes: See notes to Table 2B. The table shows weighted full sample summary statistics for the outcome variables and control variables.
Data source: See data source notes to Table 2B.

Table A6 Summary statistics full sample, IPEDS data (unweighted)

	ALL	DD & DD-PSW		DD-PSM	
		Treated	Control	Treated	Control
Total bachelor's degrees	2,333	3,780	1,948	3,780	1,834
Share of ed. prep. degrees	0.0763	0.0607	0.0804	0.0607	0.0644
Share of social studies ed. degrees	0.00237	0.00202	0.00246	0.00202	0.00200
Share of elementary ed. degrees	0.0380	0.0274	0.0408	0.0274	0.0318
Share of special ed. degrees	0.00544	0.00418	0.00577	0.00418	0.00468
Share of English and L.A. ed. degrees	0.00181	0.00169	0.00184	0.00169	0.00147
Share of science ed. degrees	0.000938	0.000749	0.000989	0.000749	0.000757
Share of math ed. degrees	0.00139	0.00100	0.00150	0.00100	0.00123
Black share of ed. degrees	0.0418	0.0353	0.0435	0.0353	0.0350
White share of ed. degrees	0.0767	0.0638	0.0802	0.0638	0.0682
Asian share of ed. degrees	0.0443	0.0387	0.0458	0.0387	0.0430
Hispanic share of ed. degrees	0.0536	0.0438	0.0563	0.0438	0.0444
Ed. awards (2≤years<4)	0.0152	0.0116	0.0172	0.0116	0.00689
Ed. awards (1≤years<2)	0.0183	0.0170	0.0187	0.0170	0.0276
Ed. awards (years≤1)	0.0273	0.0543	0.0188	0.0543	0.0131
Junior high, middle school, intermediate education	0.00116	0.00186	0.000972	0.00186	0.000964
Secondary school education	0.00269	0.00140	0.00304	0.00140	0.00173
Institutions per district	1	2	1	2	1
Observations	33,263	69,89	26,274	6,989	6,989
N (districts)	1,143	240	903	240	240

Notes: See notes to Table 2A. The table shows unweighted full sample summary statistics for the outcome variables and control variables.
Data source: See data source notes to Table 2A.

Table A7 Summary statistics full sample, NLSD data (unweighted)

	ALL	DD & DD-PSW		DD-PSM	
		Treated	Control	Treated	Control
Enrollment	6,618	21,720	2,453	21,720	2,828
White	0.760	0.577	0.808	0.577	0.786
Black	0.104	0.231	0.0703	0.231	0.0866
Hispanic	0.0936	0.146	0.0796	0.146	0.0972
Free or reduced-price lunch	0.468	0.558	0.444	0.558	0.439
Population 5-17	0.168	0.173	0.167	0.173	0.170
Population 5-17 in poverty	0.156	0.179	0.150	0.179	0.150
Special education	0.144	0.141	0.145	0.141	0.144
Urban	0.514	0.752	0.451	0.752	0.501
Suburb	0.217	0.300	0.192	0.300	0.235
Town	0.220	0.184	0.230	0.184	0.221
Rural	0.455	0.195	0.533	0.195	0.503
Revenue per student	11,496	8,898	12,187	8,898	11,480
Expenditure per student	11,520	8,995	12,192	8,995	11,442
Student-teacher ratio	15	17	15	17	15
Average teacher salary	77,212	72,697	78,413	72,697	79,165
Number magnet schools	9	11	3	11	3
Number schools	12	37	5	37	6
Observations	33,263	6,989	26,274	6,989	6,989
N (districts)	1,143	240	903	240	240

Notes: See notes to Table 2B. The table shows unweighted full sample summary statistics for the outcome variables and control variables.
Data source: See data source notes to Table 2B.

Table A8 Average charter enrollment share across models

	ALL	DD-Treated
Panel A: Commuter sample		
Average charter share	0.0148	0.0473
Average max charter share	0.0845	0.269
Panel B: Full sample		
Average charter share	0.00838	0.0400
Average max charter share	0.0412	0.196

Notes: This table shows the average charter enrollment share and average maximum charter share across DD and FE models.
Data source: See data source notes to Tables 2A and 2B.

Table A9 Effects of charter entry on teacher supply – share of education preparation bachelor's and master's degrees

	(1)	(2)	(3)	(4)	(5)	(6)
	DD		DD-PSW		DD-PSM	
Panel A: Commuter Sample						
Impact of Charters	-0.00712**	-0.00715***	-0.00706**	-0.00732***	-0.0085**	-0.00842***
	[0.00317]	[0.00271]	[0.00315]	[0.00272]	[0.00333]	[0.00284]
Observations	7,466	7,466	7,466	7,466	4,703	4,703
N (district)	177	177	177	177	132	132
Panel B: Full Sample						
Impact of Charters	-0.00298	-0.00380	-0.00449	-0.00565	-0.00478	-0.00548*
	[0.00240]	[0.00297]	[0.00345]	[0.00392]	[0.00290]	[0.00322]
Observations	28,835	28,835	28,835	28,835	12,281	12,281
N (districts)	1,134	1,134	1,134	1,134	480	480
District, year FE	Yes	Yes	Yes	Yes	Yes	Yes
District controls	No	Yes	No	Yes	No	Yes

Notes: See notes to Table 3. Significantly different from zero at 1-percent level (***), 5 percent level (**), or 10 percent level (*). The table shows DD estimates using any charter school enrollment share to define treated. Regressions are weighted by population (ages 5-17). The outcome variable is the share of education preparation bachelor's and master's degrees.
Data source: See data source notes to Tables 2A and 2B.

Table A10 Effects of charter entry on teacher supply – without weights

	(1)	(2)	(3)	(4)	(5)	(6)
	DD		DD-PSW		DD-PSM	
Panel A: Commuter Sample						
Impact of Charters	0.0108**	0.0105***	0.00553	0.00502	0.000653	0.000624
	[0.00373]	[0.00394]	[0.00385]	[0.00402]	[0.00394]	[0.00404]
Observations	7,507	7,507	7,507	7,507	4,726	4,726
N (district)	289	289	289	289	182	182
Panel B: Full Sample						
Impact of Charters	0.00505**	0.00522**	0.00247	0.00185	-0.000497	-0.000873
	[0.00200]	[0.00214]	[0.00235]	[0.00236]	[0.00210]	[0.00226]
Observations	29,667	29,666	29,667	29,666	12,463	12,463
N (districts)	1,143	1,143	1,143	1,143	480	480
District, year FE	Yes	Yes	Yes	Yes	Yes	Yes
District controls	No	Yes	No	Yes	No	Yes

Notes: See notes to Table 3. Significantly different from zero at 1-percent level (***), 5 percent level (**), or 10 percent level (*). The table shows DD estimates using any charter school enrollment share to define treated. Estimates are not weighted.
Data source: See data source notes to Tables 2A and 2B.

Table A11 Effects of charter entry on teacher labor supply using charter school market share

	(1)	(2)	(3)	(4)	(5)	(6)
	DD		DD-PSW		DD-PSM	
Panel A: Commuter Sample						
Impact of Charters	-0.00746**	-0.00822***	-0.00711**	-0.0080**	-0.00791**	-0.00830***
	[0.00304]	[0.00284]	[0.00315]	[0.00293]	[0.00322]	[0.00293]
Observations	7,396	7,396	7,396	7,396	4,679	4,679
N (district)	288	288	288	288	181	181
Panel B: Full Sample						
Impact of Charters	-0.00690*	-0.00768**	-0.00798*	-0.00935**	-0.00743**	-0.00841**
	[0.00388]	[0.00321]	[0.00412]	[0.00372]	[0.00358]	[0.00329]
Observations	28,890	28,890	28,890	28,890	12,300	12,300
N (districts)	1,130	1,130	1,130	1,130	478	478
District, year FE	Yes	Yes	Yes	Yes	Yes	Yes
District controls	No	Yes	No	Yes	No	Yes

Notes: See notes to Table 3. Significantly different from zero at 1-percent level (***), 5 percent level (**), or 10 percent level (*). The table shows DD estimates by school subject with treatment defined as any charter school share using the commuter sample. Regressions are weighted by population (ages 5-17). Data source: See data source notes to Tables 2A and 2B.

Table A12 Robustness checks using alternative matching

	(1)	(2)	(3)	(4)	(5)	(6)
	DD		DD-PSW		DD-PSM	
Impact of Charters	-0.00763**	-0.00840***	-0.0134***	-0.0134***	-0.0149***	-0.0145**
	[0.00301]	[0.00290]	[0.00348]	[0.00335]	[0.00506]	[0.00578]
Observations	7,507	7,507	7,482	7,482	3,507	3,507
N (district)	289	289	289	289	135	135
District, year FE	Yes	Yes	Yes	Yes	Yes	Yes
District controls	No	Yes	No	Yes	No	Yes

Notes: See notes to Table 3. Significantly different from zero at 1-percent level (***), 5 percent level (**), or 10 percent level (*). The table shows DD estimates using the commuter sample and an alternative matching specification. The districts are matched using the 1990 levels of: the share of education-to-total degrees, enrollment, percent Hispanic, percent FRL, percent school-aged children, percent school-aged children in poverty, percent urban, student-teacher-ratio, and number of institutions. Regressions are weighted by population (ages 5-17).
Data source: See data source notes to Tables 2A and 2B.

Table A13 Effects of charter entry on teacher labor supply – comparison with states without charter laws

	(1)	(2)	(3)	(4)	(5)	(6)
	DD		DD-PSW		DD-PSM	
Panel A: Commuter sample						
Impact of Charters	-0.00598*	-0.00717**	-0.00902**	-0.0101**	-0.00669*	-0.00804**
	[0.00329]	[0.00302]	[0.00362]	[0.00348]	[0.00376]	[0.00331]
Pre-trend test	0.00142*	0.000311	0.00175**	0.000714	0.00223***	0.000347
	[0.000773]	[0.000915]	[0.000806]	[0.000900]	[0.000818]	[0.000902]
Observations	735	735	735	735	504	504
N (district)	105	105	105	105	53	53
Panel B: Full sample						
Impact of Charters	-0.00692**	-0.00809**	-0.00755*	-0.00898*	-0.00898**	-0.00712**
	[0.00348]	[0.00346]	[0.00385]	[0.00390]	[0.00348]	[0.00345]
Pre-trend test	0.00113*	0.000541	0.00103	0.000433	0.00147	0.000860
	[0.000667]	[0.000761]	[0.000695]	[0.000763]	[0.000897]	[0.00101]
Observations	9,595	9,595	9,595	9,595	7,350	7,350
N (districts)	370	370	370	370	283	283
District, year FE	Yes	Yes	Yes	Yes	Yes	Yes
District controls	No	Yes	No	Yes	No	Yes

Notes: See notes to Table 3. Significantly different from zero at 1-percent level (***), 5 percent level (**), or 10 percent level (*). The table shows the DD estimates using an alternative comparison group. The comparison group includes districts in states without charter law as of the last year of the sample (2018). The pre-charter entry mean share of education bachelor's degrees in the commuter sample is 0.0607. The pre-charter entry mean share is 0.0575 for the full sample. The pre-trend test compares the outcome variable time trends between treated and control districts using the years 1990 to 1997.
Data source: See data source notes to Tables 2A and 2B.

Table A14 Robustness checks using alternative definitions of commuter school sample

	(1)	(2)	(3)	(4)	(5)	(6)
	DD		DD-PSW		DD-PSM	
Panel A: Commuter Sample						
Impact of Charters	-0.00752**	-0.00807***	-0.00716**	-0.00778**	-0.00791**	-0.00837***
	[0.00303]	[0.00296]	[0.00314]	[0.00302]	[0.00321]	[0.00308]
Observations	7,396	7,396	7,396	7,396	4,679	4,679
N (district)	289	289	289	289	182	182
Panel B: Alternative Commuter Sample 1						
Impact of Charters	-0.00808***	-0.00860***	-0.00748**	-0.00808***	-0.00862***	-0.00900***
	[0.00307]	[0.00299]	[0.00316]	[0.00302]	[0.00326]	[0.00312]
Observations	7,137	7,137	7,137	7,137	4,553	4,553
N (district)	278	278	278	278	177	177
Panel C: Alternative Commuter Sample 2						
Impact of Charters	-0.00969**	-0.00907**	-0.00911**	-0.00889**	-0.00947**	-0.00869**
	[0.00418]	[0.00403]	[0.00439]	[0.00416]	[0.00444]	[0.00418]
Observations	4,826	4,826	4,826	4,826	3,492	3,492
N (district)	188	188	188	188	136	136
Panel D: Alternative Commuter Sample 3						
Impact of Charters	-0.00947**	-0.00877**	-0.00933**	-0.00901**	-0.00947**	-0.00882**
	[0.00428]	[0.00409]	[0.00448]	[0.00423]	[0.00454]	[0.00428]
Observations	4,541	4,541	4,541	4,541	3,388	3,388
N (district)	177	177	177	177	132	132
Panel E: Full Sample						
Impact of Charters	-0.00698*	-0.00860**	-0.00806*	-0.00987**	-0.00760**	-0.00907**
	[0.00388]	[0.00434]	[0.00412]	[0.00468]	[0.00358]	[0.00385]
Observations	28,890	28,890	28,890	28,890	12,300	12,300
N (districts)	1,130	1,130	1,130	1,130	478	478

District, year FE	Yes	Yes	Yes	Yes	Yes	Yes
District controls	No	Yes	No	Yes	No	Yes

Notes: See notes to Table 3. Significantly different from zero at 1-percent level (***), 5 percent level (**), or 10 percent level (*). The table shows DD estimates using the commuter sample and an alternative matching specification. The baseline commuter sample using 30% living with family and less than 35% dormitory capacity, alternative sample 1 using 35% living with family and 30% dormitory capacity, alternative commuter sample 2 using 40% living with family and less than 30% dormitory capacity, and alternative commuter sample 3 uses 50% living with family and 30% dormitory capacity. Regressions are weighted by population (ages 5-17).
Data source: See data source notes to Tables 2A and 2B.

Table A15 Effects of charter entry on teacher supply – number of education bachelor's degrees

	(1)	(2)	(3)	(4)	(5)	(6)
	DD		DD-PSW		DD-PSM	
Panel A: Commuter Sample						
Impact of Charters	-0.193	-2.186	3.264	-1.387	-2.812	-5.793
	[14.40]	[12.69]	[14.63]	[13.06]	[15.50]	[13.49]
Observations	7,507	7,507	7,507	7,507	4,727	4,727
N (district)	289	289	289	289	182	182
Panel B: Full Sample						
Impact of Charters	-14.51	-11.24	-12.17	-9.814	-11.59	-9.099
	[11.32]	[11.74]	[11.00]	[11.22]	[10.96]	[10.76]
Observations	29,465	29,465	29,465	29,465	12,489	12,489
N (districts)	1,135	1,135	1,135	1,135	481	481
District, year FE	Yes	Yes	Yes	Yes	Yes	Yes
District controls	No	Yes	No	Yes	No	Yes

Notes: See notes to Table 3. Significantly different from zero at 1-percent level (***), 5 percent level (**), or 10 percent level (*). This table shows DD estimates of the effect of charter entry on the number of teacher preparation education bachelor's degrees.
Data source: See data source notes to Tables 2A and 2B.

Table A16 Robustness check using alternative comparison group: early vs. late adopter states

	(1)	(2)	(3)	(4)	(5)	(6)
	DD		DD-PSW		DD-PSM	
Impact of Charters	-0.00108	-0.00181	-0.00245	-0.00325	-0.00188	-0.00256
	[0.00349]	[0.00354]	[0.00389]	[0.00396]	[0.00346]	[0.00368]
Observations	3,348	3,348	3,348	3,348	2,256	2,256
N (district)	130	130	130	130	88	88
District, year FE	Yes	Yes	Yes	Yes	Yes	Yes
District controls	No	Yes	No	Yes	No	Yes

Notes: See notes to Table 3. Significantly different from zero at 1-percent level (***), 5 percent level (**), or 10 percent level (*). The table shows DD estimates using the commuter sample and an alternative comparison group. The comparison group includes districts in states with charter law during 2010-2016. Regressions are weighted by population (ages 5-17).
Data source: See data source notes to Tables 2A and 2B.

Table A17 Effects of charter entry on teacher supply (FE)

	(1)	(2)	(3)	(4)
Panel A: Commuter Sample				
	Same year share		Average last four years	
Impact of Charters	-0.0204*	-0.0230	-0.0158	-0.0133
	[0.0117]	[0.0146]	[0.0149]	[0.0197]
Observations	2,363	2,363	2,090	2,090
N (district)	91	91	91	91
Panel B: Full Sample				
	Same year share		Average last four years	
Impact of Charters	-0.0136	-0.00824	-0.00746	0.000142
	[0.0178]	[0.0205]	[0.0151]	[0.0171]
Observations	4,779	4,779	4,063	4,063
N (districts)	239	239	239	239
District, year FE	Yes	Yes	Yes	Yes
District controls	No	Yes	No	Yes

Notes: See notes to Table 3. Significantly different from zero at 1-percent level (***), 5 percent level (**), or 10 percent level (*). The table shows Fixed Effects estimates of effects of the charter enrollment share on teacher supply for districts with any charter schools during the sample period. In columns (1) and (2), we use the average last four-year charter enrollment share. Regressions are weighted by population age 5-17.

Data source: See data source notes to Tables 2A and 2B.

Table A18 Effects of charter enrollment share on teacher supply without weights (FE)

	(1)	(2)	(3)	(4)
Panel A: Commuter Sample				
	Same year share		Average last four years	
Impact of Charters	0.0108	0.0128	-0.0112	-0.0139
	[0.00975]	[0.00883]	[0.0146]	[0.0143]
Observations	2,363	2,363	2,090	2,090
N (district)	91	91	91	91
Panel B: Full Sample				
	Same year share		Average last four years	
Impact of Charters	0.0133	0.0127	-0.00912	-0.00244
	[0.00960]	[0.00969]	[0.0198]	[0.0197]
Observations	6,261	6,261	5,513	5,513
N (districts)	241	241	240	240
District, year FE	Yes	Yes	Yes	Yes
District controls	No	Yes	No	Yes

Notes: See notes to Table 3. Significantly different from zero at 1-percent level (***), 5 percent level (**), or 10 percent level (*). This table shows the fixed effects estimates of charter school enrollment on the teacher supply without using weights.
Data source: See data source notes to Tables 2A and 2B.

Table A19 Placebo effects using different grade levels of charter share (DD)

	(1)	(2)	(3)	(4)	(5)	(6)
	DD		DD-PSW		DD-PSM	
Panel A: Effect of charter elementary schools on elementary education bachelor's degrees						
Impact of charter elementary schools	-0.00248 [0.00182]	-0.00257 [0.00189]	-0.00159 [0.00189]	-0.00171 [0.00199]	-0.00313* [0.00184]	-0.00309 [0.00191]
Observations	6,610	6,610	6,589	6,589	3,826	3,826
N (district)	270	270	267	267	154	154
Panel B: Effect of charter middle and high schools on elementary education bachelor's degrees						
Impact of charter middle and high schools	-0.00207 [0.00194]	-0.00111 [0.00160]	-0.00103 [0.00191]	-0.000682 [0.00155]	-0.00234 [0.00180]	-0.00142 [0.00148]
Observations	6,040	6,040	5,988	5,988	2,964	2,964
N (districts)	251	251	243	243	120	120
District, year FE	Yes	Yes	Yes	Yes	Yes	Yes
District controls	No	Yes	No	Yes	No	Yes

Notes: See notes to Table 3. Significantly different from zero at 1-percent level (***), 5 percent level (**), or 10 percent level (*). The table shows DD estimates of placebo tests using different grade levels of charter share using the commuter sample. Regressions are weighted by population (ages 5-17).
Source: See source notes to Tables 2A and 2B

Table A20 Placebo effects using different grade levels of charter share (FE)

	(1)	(2)	(3)	(4)
Panel A: Effect of charter elementary schools on elementary education bachelor's degrees				
	Same year share		Average last four years	
Impact of charter elementary schools	-0.00344	-0.0208**	0.000287	-0.00605
	[0.00758]	[0.00908]	[0.00834]	[0.00916]
Observations	1,922	1,922	1,691	1,691
N (district)	77	77	77	77
Panel B: Effect of charter middle and high schools on elementary education bachelor's degrees				
	Same year share		Average last four years	
Impact of charter elementary schools	-0.00175	-0.0172	0.00196	-0.00328
	[0.00974]	[0.0113]	[0.0113]	[0.0109]
Observations	1,485	1,485	1,301	1,301
N (districts)	61	61	60	60
District, year FE	Yes	Yes	Yes	Yes
District controls	No	Yes	No	Yes

Notes: See notes to Table 3. Significantly different from zero at 1-percent level (***), 5 percent level (**), or 10 percent level (*). The table shows FE estimates of placebo tests using different grade levels of charter share using the commuter sample. Regressions are weighted by population (ages 5-17).
Data source: See data source notes to Tables 2A and 2B.

Table A21 Placebo test defining treatment as five years prior to actual treatment

	(1)	(2)	(3)	(4)	(5)	(6)
	DD		DD-PSW		DD-PSM	
Impact of Charters	-0.00309	-0.00303	-0.00211	-0.00205	-0.00261	-0.00256
	[0.00323]	[0.00318]	[0.00338]	[0.00329]	[0.00347]	[0.00339]
Observations	7,396	7,396	7,396	7,396	4,679	4,679
N (district)	288	288	288	288	181	181
District, year FE	Yes	Yes	Yes	Yes	Yes	Yes
District controls	No	Yes	No	Yes	No	Yes

Notes: See notes to Table 3. Significantly different from zero at 1-percent level (***), 5 percent level (**), or 10 percent level (*). The table shows DD estimates using the commuter sample and a hypothetical treatment year of five years prior to actual treatment for the treated districts. Regressions are weighted by population (ages 5-17).
Data source: See data source notes to Tables 2A and 2B.

Table A22 Effect of charter entry on district enrollment

	(1)	(2)	(3)	(4)	(5)	(6)
	DD		DD-PSW		DD-PSM	
Impact of Charters	0.0478	0.0591*	0.0430	0.0512*	0.0518	0.0597*
	[0.0395]	[0.0331]	[0.0355]	[0.0298]	[0.0372]	[0.0313]
Observations	7,118	7,118	7,118	7,118	4,503	4,503
N (district)	288	288	288	288	181	181
District, year FE	Yes	Yes	Yes	Yes	Yes	Yes
District controls	No	Yes	No	Yes	No	Yes

Notes: See notes to Table 3. Significantly different from zero at 1-percent level (***), 5 percent level (**), or 10 percent level (*). The table shows DD estimates using the natural log of district enrollment as the outcome variable. Regressions are weighted by population (ages 5-17).
Data source: See data source notes to Tables 2A and 2B.

Table A23 Effect of charter entry on free or reduced-price lunch recipients

	(1)	(2)	(3)	(4)	(5)	(6)
	DD		DD-PSW		DD-PSM	
Impact of Charters	-0.0291***	-0.0190***	-0.0234***	-0.0148**	-0.0260***	-0.0173**
	[0.00694]	[0.00626]	[0.00656]	[0.00636]	[0.00713]	[0.00668]
Observations	7,343	7,343	7,343	7,343	4,647	4,647
N (district)	285	285	285	285	180	180
District, year FE	Yes	Yes	Yes	Yes	Yes	Yes
District controls	No	Yes	No	Yes	No	Yes

Notes: See notes to Table 3. Significantly different from zero at 1-percent level (***), 5 percent level (**), or 10 percent level (*). The table shows DD estimates using the natural log of free or reduced-price lunch recipients in a district as the outcome variable. Regressions are weighted by population (ages 5-17).
Data source: See data source notes to Tables 2A and 2B.

Table A24 Effect of charter entry on student racial composition

	(1)	(2)	(3)	(4)	(5)	(6)
	DD		DD-PSW		DD-PSM	
Panel A: Change in share of Black students						
Impact of charters	-0.0119 [0.0103]	-0.0112 [0.00799]	-0.0109 [0.00989]	-0.00859 [0.00775]	-0.0131 [0.0108]	-0.0110 [0.00818]
Panel B: Change in share of Hispanic students						
Impact of charters	-0.0279** [0.0139]	-0.0239* [0.0144]	-0.0283** [0.0124]	-0.0248* [0.0131]	-0.0264* [0.0135]	-0.0233 [0.0143]
Observations	7,343	7,343	7,343	7,343	4,647	4,647
N (districts)	285	285	285	285	180	180
District, year FE	Yes	Yes	Yes	Yes	Yes	Yes
District controls	No	Yes	No	Yes	No	Yes

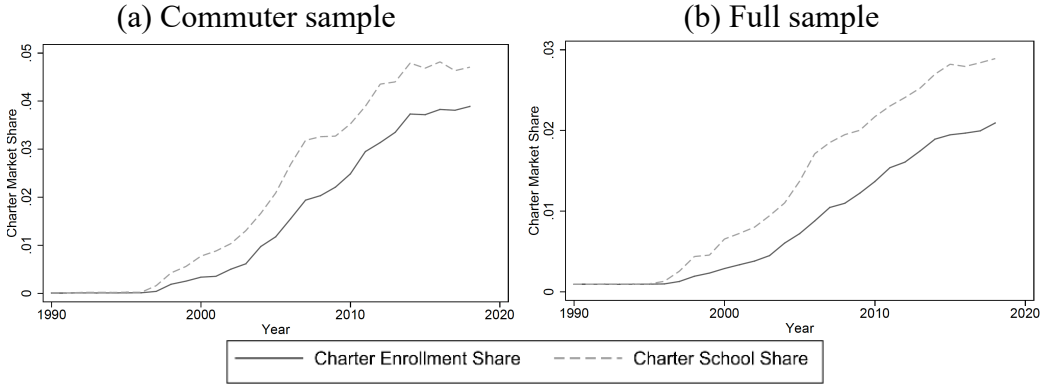
Notes: See notes to Table 3. Significantly different from zero at 1-percent level (***), 5 percent level (**), or 10 percent level (*). The table shows DD estimates using the natural log of Black and Hispanic students in a school district as the outcome variable. Regressions are weighted by population (ages 5-17).
Data source: See data source notes to Tables 2A and 2B.

Table A25 Effects of charter entry on teacher supply – share of education preparation awards

	(1)	(2)	(3)	(4)	(5)	(6)
	DD		DD-PSW		DD-PSM	
Panel A: Education awards 2-4 years						
Impact of Charters	-0.00158	-0.00201*	-0.00168	-0.00207*	-0.00175	-0.00215*
	[0.00112]	[0.00111]	[0.00118]	[0.00115]	[0.00121]	[0.00117]
Observations	1,143	1,143	1,143	1,143	893	893
N (district)	57	57	57	57	44	44
Panel B: Education awards 1-2 years						
Impact of Charters	0.0145	0.0174	0.0134	0.0161	0.0133	0.0162
	[0.0139]	[0.0150]	[0.0120]	[0.0130]	[0.0136]	[0.0146]
Observations	2,833	2,833	2,833	2,833	1,758	1,758
N (district)	135	135	135	135	84	84
Panel C: Education awards less than 1 year						
Impact of Charters	0.00340	-0.00575	0.00585	-0.00201	0.00353	-0.00458
	[0.00814]	[0.0155]	[0.00836]	[0.0140]	[0.00877]	[0.0153]
Observations	2,144	2,144	2,144	2,144	1,436	1,436
N (district)	168	168	168	168	103	103
District, year FE	Yes	Yes	Yes	Yes	Yes	Yes
District controls	No	Yes	No	Yes	No	Yes

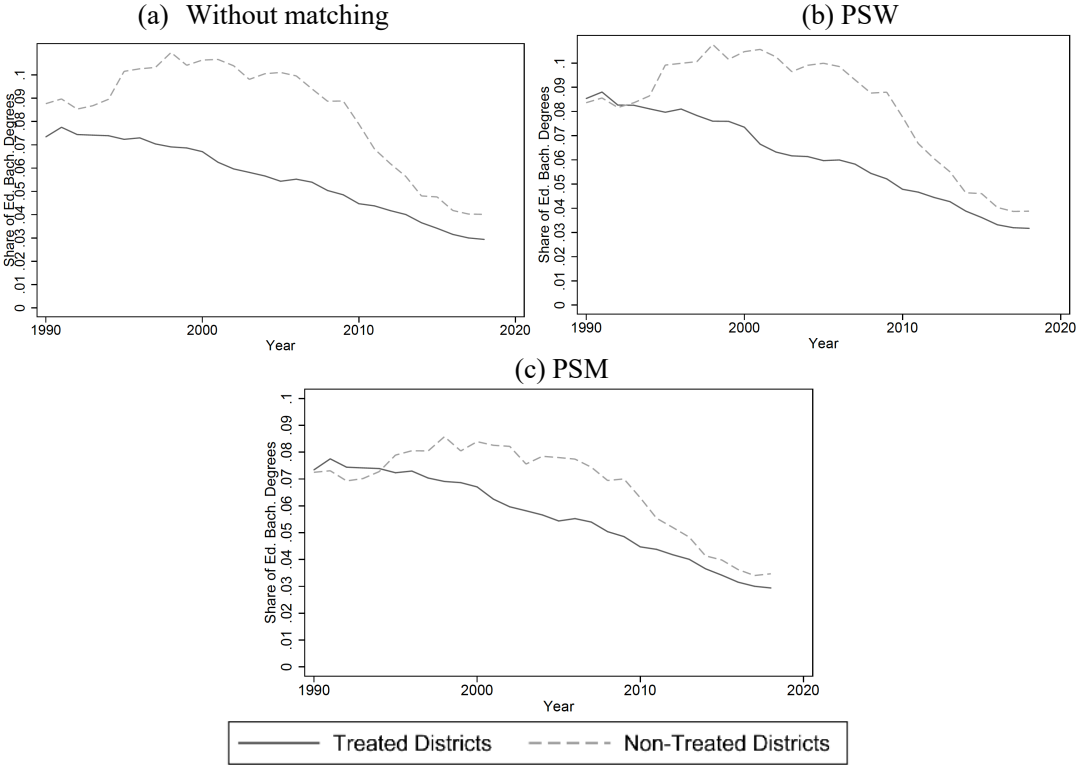
Notes: See notes to Table 3. Significantly different from zero at 1-percent level (***), 5 percent level (**), or 10 percent level (*). The table shows DD estimates using any charter school enrollment share to define treated. Regressions are weighted by population (ages 5-17). The outcome variable is the share of education preparation bachelor's and master's degrees.
Data source: See data source notes to Tables 2A and 2B.

Figure A1 Trends in charter school share and enrollment share



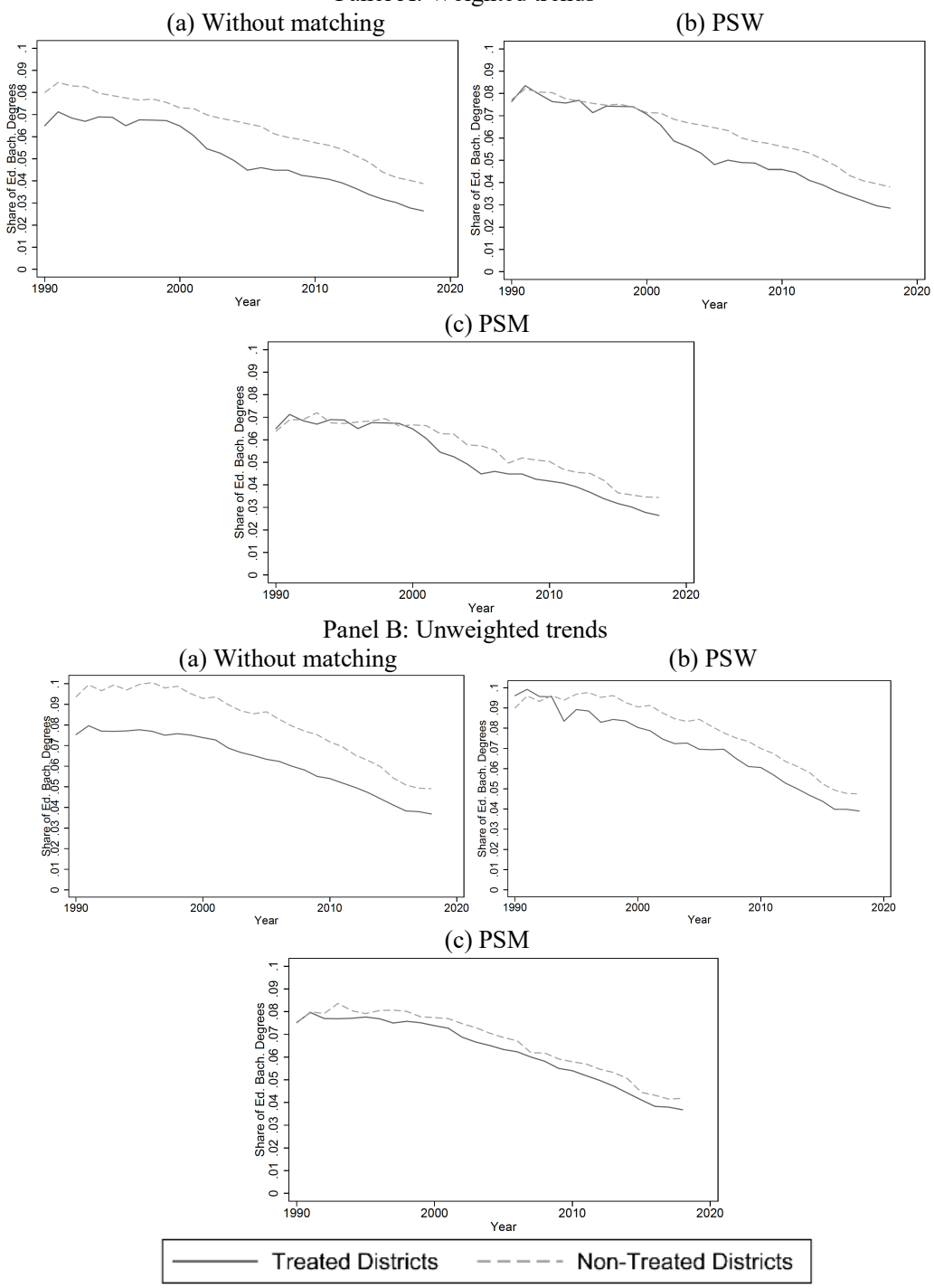
Notes: This figure plots the trends in charter school share (dashed) and charter enrollment share (solid).
Data source: See data source notes to Tables 2A and 2B.

Figure A2 Trends in share of education degrees (unweighted)



Notes: This figure plots the trends in share of education bachelor's degrees of treated districts (solid) and comparison districts (dashed) using the commuter sample. DD and DD-PSM are not weighted; DD-PSW is weighted by the inverse probability of the propensity score.
Data source: See data source notes to Tables 2A and 2B.

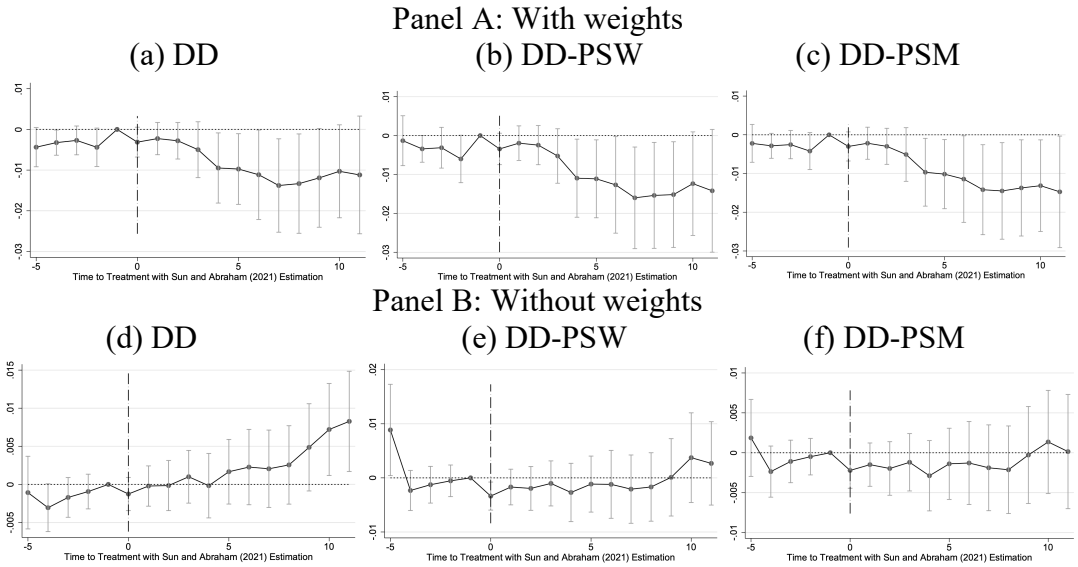
Figure A3 Trends in share of education degrees, full sample



Notes: This figure plots the trends in share of education bachelor's degrees of treated districts (solid) and comparison districts (dashed) using the full sample. Panel A uses weights and Panel B excludes weights. See Table 4 for notes on weights.

Data source: See data source notes to Tables 2A and 2B.

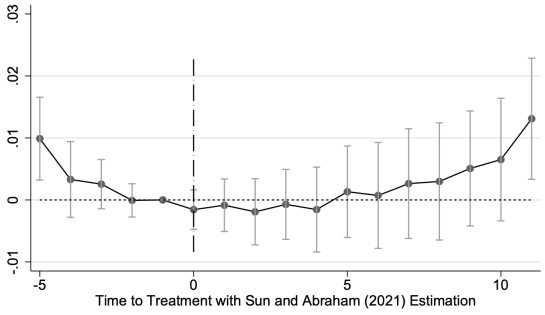
Figure A4 Sun and Abraham (2021) event study of teacher supply, full sample



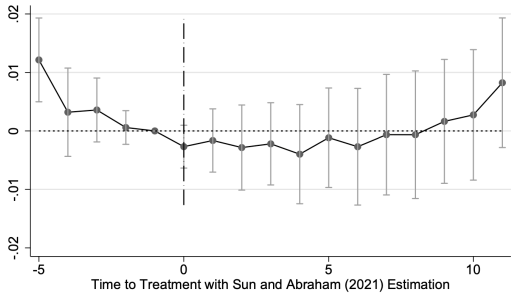
Notes: See notes to Table 3. This figure presents the Sun and Abraham (2021) event study results using the full sample. Treatment is defined as any charter school enrollment market share. Estimates are relative to the year prior to first charter entry, which is set to zero. 95% confidence intervals are reported based on robust standard errors clustered at the district level. No weights are included in the regressions. The regressions in Panel A include population (age 5-17) weights, and those in Panel B do not include weights.
Data source: See data source notes to Tables 2A and 2B.

Figure A5 Sun and Abraham (2021) event study (without weights)

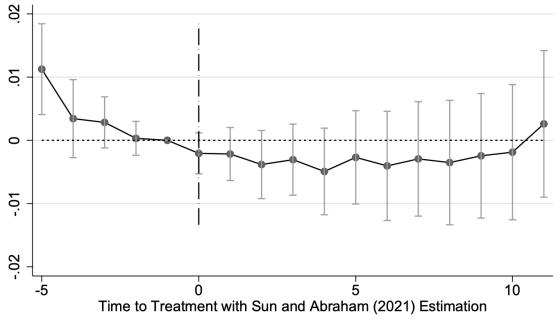
(a) DD



(b) DD-PSW



(c) DD-PSM

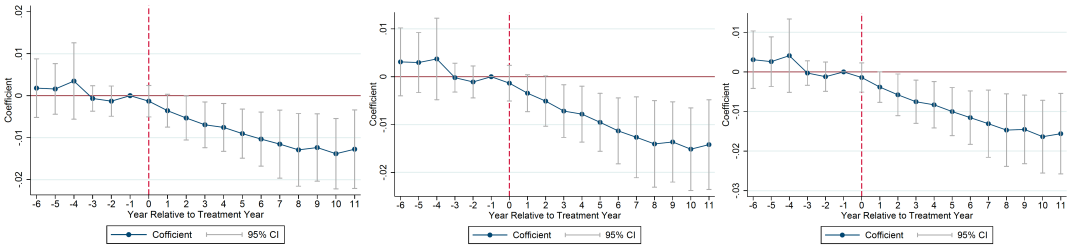


Notes: See notes to Table 3. This figure presents the Sun and Abraham (2021) event study results using the commuter sample. Treatment is defined as any charter school enrollment market share. Estimates are relative to the year prior to first charter entry, which is set to zero. 95% confidence intervals are reported based on robust standard errors clustered at the district level. No weights are included in the regressions.
Data source: See data source notes to Tables 2A and 2B.

Figure A6 Event study of teacher supply

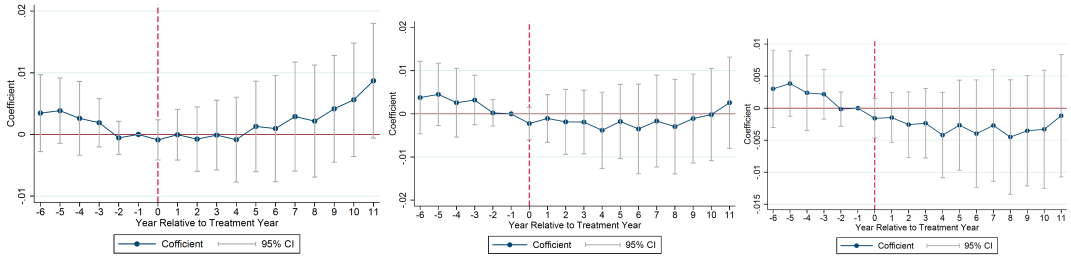
Panel A: With weights

(a) DD (b) DD-PSW (c) DD-PSM



Panel A: Without weights

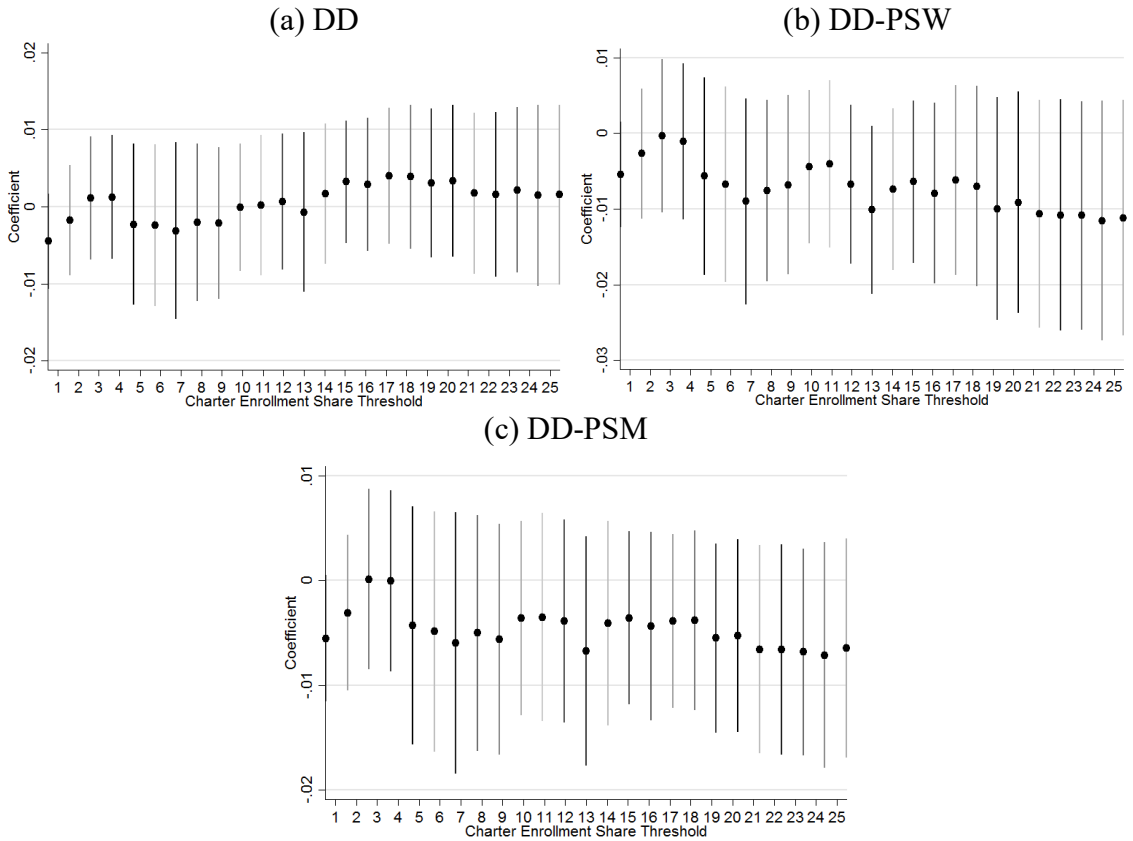
(d) DD (e) DD-PSW (f) DD-PSM



Notes: This figure presents the traditional event study results of the share of education bachelor's degrees using the commuter sample. Treatment is defined as any charter school enrollment market share. Zero is the first year of treatment. The regressions in Panel A include population (age 5-17) weights, and those in Panel B do not include weights.

Data source: See data source notes to Tables 2A and 2B.

Figure A7 Plots of estimates using 1% to 25% as the threshold of treated districts, full sample



Notes: This figure plots the estimates in share of education bachelor's degrees using the charter enrollment share of 1- 25% as the threshold of treated districts and the commuter sample.
Data source: See data source notes to Tables 2A and 2B.

Appendix B: Fixed effects model

A limitation to using a DD for this study is there may be a non-linear relationship between charter school market share and the outcome variables. It is possible that the effect of charter schools is greater when the market share is larger. Therefore, we then turn to a Fixed Effects (FE) model where we then treat charter school enrollment share as a continuous variable and only include districts with at least one charter school throughout the analysis time frame. Again, our main specification uses charter school enrollment share as the continuous treatment variable. The FE model is:

$$y_{it} = \alpha + \beta Charter_{it} + X_{it}\gamma + \mu_i + \lambda_t + \varepsilon_{it} \quad (3)$$

Where y_{it} is the outcome variable of interest for district *i* in year *t*. $Charter_{it}$ is the continuous charter enrollment share in district *i* in year *t*. The other variables are defined in previous sections.