Economics of Education Review 99 (2024) 102520

Contents lists available at ScienceDirect

Economics of Education Review

journal homepage: www.elsevier.com/locate/econedurev

The effects of charter school entry on the supply of teachers from university-based education programs

Feng Chen [a], Douglas N. Harris [b,*], Mary Penn [c]

[a] Li Anmin Institute of Economic Research, Liaoning University, China
[b] Tulane University, 6823 St. Charles Ave., Tilton Hall 208, New Orleans, LA 70118, United States
[c] Analysis Group, Boston, MA

ARTICLE INFO

Keywords:
Charter schools
Teacher labor market

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 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 teacher education degrees, exploiting within- and between-district variation in the timing of charter school entry in districts containing college teacher preparation programs. Applying a generalized difference-in-difference model, we find that a 10 percent increase in charter market share decreases the supply of traditionally prepared teachers by one percent per year on average. This effect is concentrated in elementary education and special education degrees, which, anecdotally, are less valued in charter schools.

1. Introduction

Charter schools have been growing in the United States since the 1990s (Hohnstein, 2017) 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 competition, which, in turn might increase student outcomes (e.g., Cohodes & Parham, 2021; Zimmer et al., 2015; Chen & Harris, 2023).

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 number of new teachers has decreased by about 20 percent (Kraft et al., 2020).[1] Fig. 1 confirms the drop in new teachers over these years, but also shows a longer time trend. 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; 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 younger teachers compared to traditional public schools (TPS).[3] Thus, as more charter schools enter, undergraduates may

* Corresponding author.
E-mail address: dharri5@tulane.edu (D.N. Harris).

[1] Fig.1 suggests a negative correlation between charter entry and teacher supply up until 2009, then the pattern reverses, so that there is no net difference between ever-charter and never charter districts over the entire panel. However, this figure is merely descriptive and the results we show later suggest that the limited difference between the two trends in this figure reflects state-specific differences that are correlated with both charter entry and teacher supply and which are accounted for in our various GDD/DD analyses.

[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).

https://doi.org/10.1016/j.econedurev.2024.102520
Received 21 August 2022; Received in revised form 31 January 2024; Accepted 13 February 2024
Available online 23 February 2024

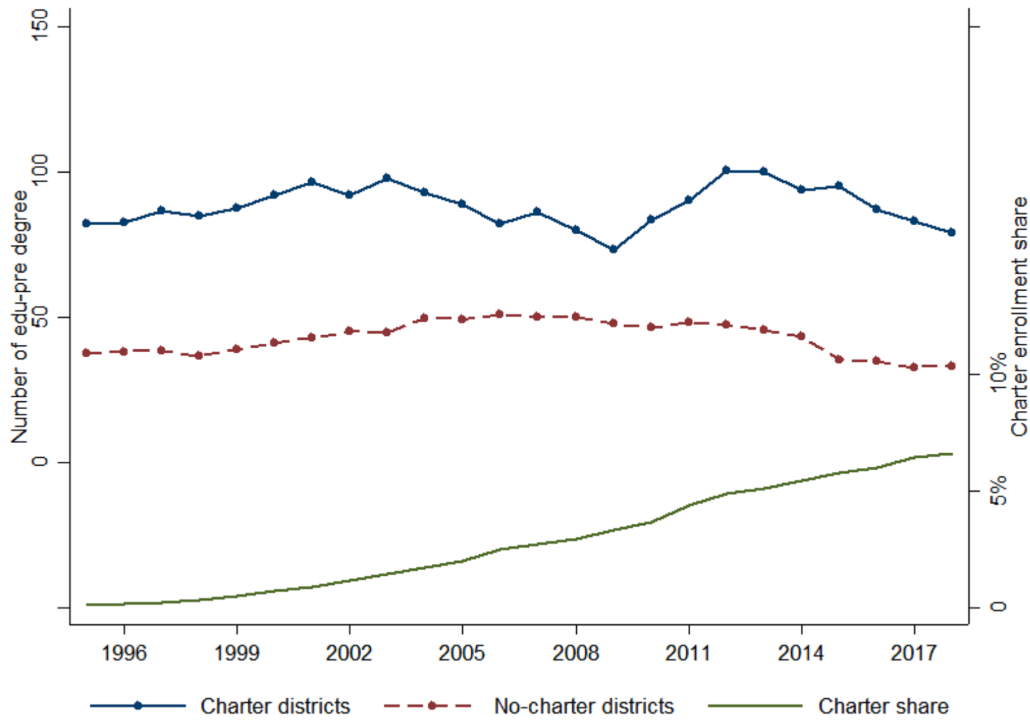

Fig. 1. Trends in teacher supply.
Notes: This figure plots the number of undergraduate education degrees of charter districts (solid) and no-charter districts (dashed) using the commuter sample. The trend in charter growth is measured as the charter enrollment share (with never-charter districts entered as zero).

see potential job opportunities with charter schools and change their college majors to education, ultimately supplanting experienced TPS teachers. This rise in demand would tend to increase the equilibrium quantity supply of college education-major graduates.

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).[4] 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 fringe benefits (Hoxby, 2000). Charter teachers are often not protected by tenure and union contracts (Carruthers, 2012; Jackson, 2012; Cowen & 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., 2018). 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.[5]

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 et al., 2022). 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. Fig. 2 shows these results for both New Orleans and similar declines in two other districts with large charter shares.

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 mainly on degrees from commuter institutions. With this sample, we use a generalized difference-in-differences model (GDD), and various robustness checks, for the years 1995 to 2018 to determine the impact of charter school teacher hiring on teacher education degrees. This GDD method leverages both within-district variation over time and differences between treated and untreated districts, helping to 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) and

4 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).

5 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.

broader changes in college enrollment patterns.

Our results suggest that district charter entry decreases the equilibrium quantity of IHE-based teacher education graduates (what we call, for short, the "new teacher supply"), especially for education majors in elementary education and special education. 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 et al., 2017).

Conversely, we see some evidence of a small increase in math education majors. This, and the decline in elementary education, are consistent with the theory that charter schools prefer teachers with more background in basic academic disciplines rather than general pedagogical knowledge (Podgursky, 2008). Non-education degrees, such as math, also have higher returns outside of teaching. In this sense, college students benefit by switching from education to disciplinary degrees because the latter has a higher option value, should students ultimately decide to exit teaching.

We take a variety of steps to address endogeneity. First, we implement a test for parallel trends that aligns with the GDD method. Second, we use two types of placebo analyses to test for endogeneity: (a) we 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 when middle and high school charters open; and (b) we estimate effects on teacher enrollments in competitive colleges where we expect small or no effects because these graduates rarely stay in the college's local community upon graduation. Third, we estimate effects comparing early- and late-charter-adopting states, leveraging possible exogenous timing in state charter school adoption. Fourth, we estimate effects based on the eventual charter market share and test whether the effects are smaller in low-charter-share districts, as we would expect. All of these tests reinforce that the main results reflect a causal effect of charter school entry on the number of university-based teacher education graduates.

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.[6] 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—supply—accordingly.

These results have several important implications. First, while there appears to be an ample supply of new teacher graduates in the aggregate, a reduction in the total supply makes it more difficult to address narrower shortages in particular subjects and locations (Dee & Goldhaber, 2017), especially in special education where we see some of the most noticeable drops in the new teacher supply. Second, while we are not able to directly study the effect of charter entry on teacher *quality*, teachers who major in education are more likely to stay in teaching than alternatively prepared teachers (Redding & Smith, 2016), thus reducing turnover and allowing them to leverage their return to experience; all else equal, high turnover and low experience reduce the average quality of teaching (Ronfeldt et al., 2013; Harris & Sass, 2011). Third, given the centrality of teachers in schooling, the fact that charter entry has such a noticeable effect on the teacher labor market is noteworthy. Fourth, the ongoing and aggressive expansion of school vouchers is likely to have similar effects on the teacher pipeline.

Section 2 provides a simple theoretical model of teacher supply and charter entry and sets up our analysis. This is followed by a discussion of our data (Section 3) and empirical framework (Section 4). Our results follow in Section 5. Section 6 concludes.

[6] Bruhn et al. (2020) also find that high-performing teachers entering because of charter schools end up teaching in traditional public schools.

2. Theory

Consider a situation where there are two types of schools: charter schools and traditional public schools (TPS). Also, the teacher labor market is segmented into two comprehensive and mutually exclusive teacher groups: teachers with university education degrees (*IHEs*) and alternatively prepared (*Alt*) teachers. We denote the total demand for teachers across both schooling sectors and teacher types as: $d_{it}^{All,\ Total} = d_{it}^{TPS,\ Total} + d_{it}^{Charter,\ Total} = (d_{it}^{TPS,\ IHE} + d_{it}^{TPS,\ Alt}) + (d_{it}^{Charter,\ IHE} + d_{it}^{Charter,\ Alt})$. We make no claims about the underlying school objective functions or constraints leading to these demand curves, but only assert that they exist. The subscript t implies that demand changes over time.

In the spirit of the Roy model and human capital investment theory, the new teacher supply is composed of students starting college and choosing from a vector of majors, each of which affects the probability of obtaining work in various occupations that have expected salaries and amenities. College students select majors to maximize the present discount value of future utility, which yields the supply of each type of teacher, denoted by s_{it}^{IHE} and s_{it}^{Alt}. Factors affecting supply include teacher compensation, the reservation wage, job security, and working conditons, all of which seem to differ in charter schools versus traditional public schools. Further, we assume that the labor market is localized so that each supply and demand function is specific to geographic unit i. Later, we discuss what level of unit i is necessary, in practice, to make this model realistic and to account for immigration across units.

We are mainly interested in decisions of recent college graduates and therefore specifically focus on the quantity of supply and demand for *new hires*. This is consistent with the idea that current college students are mainly focused on short-term job prospects. We could just as easily focus on the total quantity of labor and add assumptions about the stability of supply among prior college graduates, but we focus directly on new hires as a reminder that, in the analysis that follows, we are only capturing the behavior of new college graduates, all of whom would be new hires.

For now, we assume that an equilibrium is reached in each time period t such that the quantity demanded equals the quantity supplied, or $\theta^{\cdot} \cdot (Qd_{it}^{*TPS,\ \cdot} + Qd_{it}^{*Charter,\ \cdot}) = \gamma^{\cdot} \cdot Qs_{it}^{*\cdot}$ for each of the *IHE* and *Alt* teacher segments.[7] The parameter $\theta^{\cdot} \in [0, 1]$ is the share of total new hires in each period who graduated in the current period, in contrast to those who were trained previously and are not currently teachers in geographic unit i (e.g., trained teachers reentering the teacher labor force) who "take positions away" from new graduates. The parameter $\gamma^{\cdot} \in [0, 1]$ is the share of college students who prepare to be teachers and decide to apply for a teaching job (Cowan et al., 2016).[8] On average, 45 to 65 percent of education graduates become professional teachers (Kraft et al., 2020). In other words, for new graduates to be new hires, they have to apply for a teaching job in unit i and schools have to be hiring new graduates, not prior ones.

We are interested in modeling the supply of new teacher education graduates from IHEs as a function of charter school demand. So, we focus on the IHE version of the above equation and re-arrange terms to obtain: $Qs_{it}^{*IHE} = \theta^{IHE} / \gamma^{IHE} \cdot (Qd_{it}^{*TPS,\ IHE} + Qd_{it}^{*Charter,\ IHE})$. In practice, we cannot separate the number of *IHE* and *Alt* hires at the unit level. So, we also define the share of total demand in each schooling sector that comes

[7] Qd_{it} and Qs_{it} are the quantity demanded and supplied in the given school sector. We sum them here to obtain the total quantities across sectors. Also, note that the subsequent analysis does not depend on this market clearing assumption, but only helps establish the correct model for estimation.

[8] This might occur, for example, because college students are poorly informed early in college and learn more about their options over time. Also, as noted above, college majors affect prospects in specific occupations with some probability. So, it is also possible that some people will seek employment in teaching, but not obtain it. This is not reflected in θ.

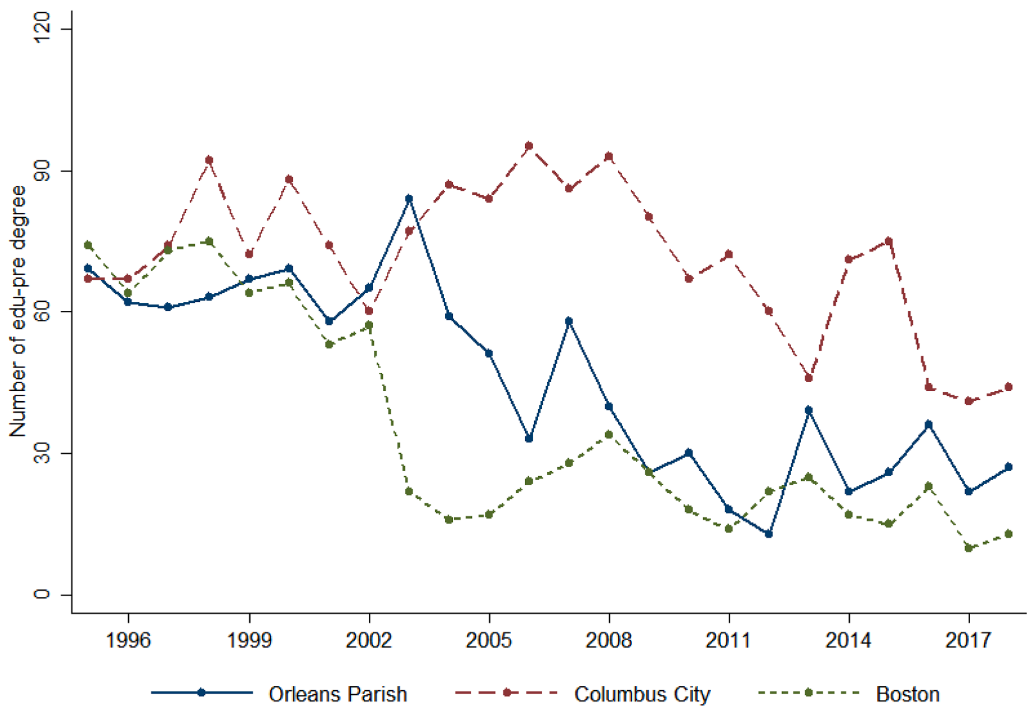

Fig. 2. Trends in teacher supply by districts.
Notes: This figure plots the trends in the number of undergraduate teacher education degrees for New Orleans and two other selected districts with larger charter shares that are also in the commuter sample. The charter shares in these three cities increased between 2000 and 2018 as follows: Boston (21 %−2 %=19 percentage points), Columbus (38 %−3 %=35 percentage points), and New Orleans (95 %−1 %=94 percentage points).

Table 1
Summary statistics (Means).

Districts	All	No-charters	With charters	Difference
No. education degrees	35	22	45	−23***
No. bachelor degrees	1472	740	2067	−1327***
No. teachers in charter	104	0	188	−188***
Enrollment	36,225	10,049	57,514	−47,465***
White	0.56	0.70	0.44	0.26***
Black	0.20	0.11	0.27	−0.15***
Hispanic	0.15	0.09	0.19	−0.11***
FRL	0.42	0.32	0.51	−0.18***
Special education	0.13	0.13	0.13	−0.00
Urban	0.44	0.20	0.63	−0.44***
Suburb	0.42	0.57	0.30	0.26***
Town	0.07	0.11	0.04	0.07***
Rural	0.07	0.13	0.03	0.10***
Revenue per student	11,221	11,825	10,730	1095***
Expenditure per student	11,307	11,897	10,827	1069***
Student-teacher ratio	16.45	15.73	17.04	−1.30***
Teacher salary	92,455	95,404	90,056	5348***
No. magnet school	3.09	0.56	5.15	−4.59***
Observation	6809	3054	3755	6809
N (district)	397	187	210	397

Notes: This table presents summary statistics of the outcome variables and control variables across the whole sample period. We include the control variables in some of the specifications that follow. The differences in baseline levels are not necessarily an issue for identification given that the analysis accounts for time-invariant differences between the comparison and treatment groups (see text for additional discussion).

from each source: $\phi^{\cdot} = \frac{Qd_{it}^{*\cdot,\ IHE}}{Qd_{it}^{*\cdot,\ Total}}$. Plugging this into the above equation, we obtain: $Qs_{it}^{*IHE} = \theta^{IHE}/\gamma^{IHE}\cdot(\phi^{TPS}Qd_{it}^{*TPS,Total} + \phi^{Charter}Qd_{it}^{*Charter,\ Total})$. Finally, since TPS demand is a function of charter demand (all teachers teach in one of the two sectors[9]), we substitute in for $Qd_{it}^{*TPS,\ Total} = Qd_{it}^{*All,\ Total} - Qd_{it}^{*Charter,Total}$ and obtain: $Qs_{it}^{*IHE} = \theta^{IHE}/\gamma^{IHE}\cdot(\phi^{\cdot TPS}(Qd_{it}^{*All,Total} - Qd_{it}^{*Charter,\ Total}) + \phi^{Charter}Qd_{it}^{*Charter,\ Total}) = \theta^{IHE}/\gamma^{IHE}\cdot((\phi^{\cdot TPS}Qd_{it}^{*All,\ Total} + (\phi^{Charter} - \phi^{TPS})Qd_{it}^{*Charter,\ Total})$.

We are specifically interested in the responsiveness of the new teacher supply to changes in charter entry and hiring, or $\frac{\partial Qs_{it}^{*IHE}}{\partial Qd_{it}^{*Charter,\ Total}} = \theta^{IHE}/\gamma^{IHE}(\phi^{Charter} - \phi^{TPS})$. The second term highlights that the direction of the charter entry effect on *IHE* graduate demand depends on the relative hiring preferences of charter and TPS for *IHE* and *Alt* teachers. If $\phi^{TPS} > \phi^{Charter}$, as prior evidence suggests, then $\frac{\partial Qs_{it}^{*IHE}}{\partial Qd_{it}^{*Charter,\ Total}} < 0$. Further the absolute value of that effect is increasing in the share of total new hires who were trained in IHEs in the current period (θ^{IHE}), but decreasing in the share of IHE graduates who have prepared to be teachers and apply for a teaching job (γ^{IHE}). In other words, the charter entry effect on teacher education graduates is larger when those students are more likely to obtain teaching jobs and smaller when more IHE teacher education graduates actually pursue teaching.

Some realistic numeric examples may help to illustrate. Suppose that $\theta^{IHE} = 0.25$ and $\gamma^{IHE} = 0.5$. In that case the first term is $0.25/0.5 = 0.125$. For the second term, if we take the national figures reported earlier, then $(\phi^{Charter} - \phi^{TPS}) = 0.75 - 0.83 = -0.08$, so that the total effect is $(-0.125)(-0.08) = -0.01$. In other words, if the typical charter school hires 100 additional teachers, then the supply of IHE graduates drops by about one per year. This might seem small, but, over a decade, every 100 charter teachers hired would reduce the number of teachers supplied by 10 in each charter district on average.

Implicit in the above model is the idea that charter schools and TPS might have different demand functions for *IHE* and *Alt* teachers. Such differences could arise because they have different objective functions (e.g., charter schools are under stricter test-based accountability). Even with the same objective function, they could also choose different production technology. For example, Harris (2020) notes that charter schools in New Orleans operate under a more business-like model with intense monitoring of teacher performance, coupled with weaker job protections.

We note that the above model makes no assumption about what charter entry might do to total teacher demand. Even if total enrollment in district *i* stays constant, charter schools might have different class sizes (USDOE, 2016), so that total demand would vary with the charter enrollment share.[10] In the empirical analysis that follows, we sometimes control for district enrollment to account for some of the factors affecting total demand.

We use this framework to set up the analysis in the next section and to interpret the results.

3. Data

3.1. 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 1994–95 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 40 percent average of students living off-campus with family over the time period or with dormitory capacity below 40 percent of total enrollment.[11] 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

[9] This is not quite true as we omit private schools from the model, but there is little student substitution between charter/TPS and private schools, so we can treat that as exogenous.

[10] The USDOE (2016) show very small differences in class size between the two sectors, though this varies somewhat by grade level and classroom type. Total demand might also increase charter schools tends to hire younger teachers (including recent college graduates). Though there is evidence of this, if charter entry led to a decline the number of TPS teachers, this would likely lead to layoffs of younger TPS teachers who would then seek employment elsewhere, including in local charter schools.

[11] 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 alternative thresholds to define commuter schools and the results are robust (see Fig. 4).

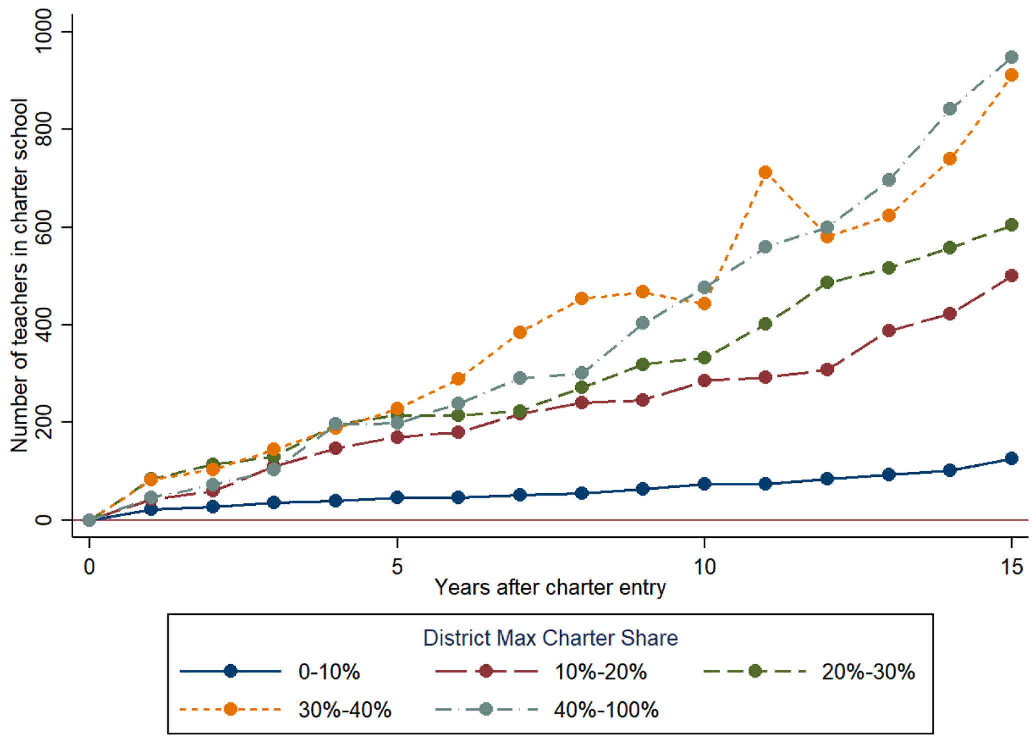

Fig. 3. Trends in Charter Teachers by District Max Charter Share.
Notes: This figure shows the trends in the number of teachers employed in the charter sector for different groups of districts based on their maximum charter shares.

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[12] and we sum the institutional level completions by year. We also conduct some analysis for master's degree completions.[13] 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 6000 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.

Our analysis focuses on the sample of districts where there is an IPEDS institution that generated at least one education major in 1995.[14] While the IPEDS includes 3000 to 7000 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 397 school districts that had at least one commuter college within its boundaries that granted education degrees in 1995–2018. 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.

Table 2
Effects of Charter share on teacher supply.

	(1)	(2)	(3)	(4)
Panel A unweighted (mean of outcome = 35)				
Charter teacher	−0.022*	−0.021*	−0.024**	−0.024**
	[0.011]	[0.011]	[0.010]	[0.010]
R-squared	0.703	0.714	0.715	0.705
Observations	6809	6809	6809	6809
N(district)	397	397	397	397
District FE	Yes	Yes	Yes	Yes
State-by-year FE	Yes	Yes	Yes	No
Student control	No	Yes	Yes	Yes
District control	No	No	Yes	Yes
State time trend	No	No	No	Yes

Notes: This table shows GDD estimates of the effects of the number of teachers in charter schools on teacher supply measured as the number of education bachelor degrees using the commuter sample. The outcome variable is forwarded by three years in the analysis, based on the assumption (reinforced by evidence in Fig. 4) that charter entry influences the teacher supply with some delay. (For example, we use the number of education bachelor degrees in 2013 to regress the number of teachers in charter schools in 2010.) Student control includes log of the number of bachelor degrees; log of district enrollment; and the shares of students who are Black, Hispanic, white, FRL, special education. District control includes revenue per student, expenditure per student, student-teacher ratio, teacher salary, number of magnet schools, and an indicator of charter law effectiveness. Standard errors are clustered at the district level.

From these data, we obtain the dependent variable suggested in the model in the prior section: the number of education bachelor's degrees from IHEs within the same geographic school district (summed across IHEs where appropriate).

3.2. Charter school entry and other data

We use data from the National Longitudinal School Database (NLSD) to obtain charter entry by district. The NLSD combines data from the

[12] 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.

[13] Table A6 shows the results for master's degrees shows similar results signs but less precise.

[14] Some institutions have missing information on bachelor's degrees in some years. In one robustness check, we recode these missing values as zero and create missing indicators for all specifications. These results (see Table A2) are nearly identical to our main results in Table 2.

Table 3
Effect heterogeneity by subject.

Subject	(1) Social studies education degrees	(2)	(3)	(4)	(1) Elementary education degrees	(2)	(3)	(4)
Charter teacher	0.000	0.000	−0.001	−0.001	−0.021***	−0.020***	−0.021***	−0.021***
	[0.001]	[0.001]	[0.001]	[0.001]	[0.007]	[0.007]	[0.007]	[0.007]
R-squared	0.790	0.791	0.797	0.769	0.807	0.814	0.815	0.804
Subject	Special education degrees				ELA education degrees			
Charter teacher	−0.003	−0.003	−0.003	−0.003	0.000	0.000	0.000	0.000
	[0.002]	[0.002]	[0.002]	[0.002]	[0.000]	[0.000]	[0.000]	[0.000]
R-squared	0.512	0.534	0.535	0.507	0.804	0.805	0.809	0.794
Subject	Science education degrees				Math education degrees			
Charter teacher	0.000	0.000	0.000	0.000	0.001*	0.001*	0.001*	0.001**
	[0.000]	[0.000]	[0.000]	[0.000]	[0.001]	[0.001]	[0.000]	[0.000]
R-squared	0.504	0.505	0.506	0.464	0.755	0.769	0.771	0.739
Observations	6809	6809	6809	6809	6809	6809	6809	6809
N(district)	397	397	397	397	397	397	397	397
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-by-year FE	Yes	Yes	No	Yes	Yes	Yes	Yes	No
Student control	No	Yes	Yes	Yes	No	Yes	Yes	Yes
District control	No	No	Yes	Yes	No	No	Yes	Yes
State time trend	No	No	No	Yes	No	No	No	Yes

Notes: This table shows GDD estimates of the heterogeneous effects of by subject using the commuting sample. See notes in Table 2 for details on outcome variables, control variables, and standard errors.

Common Core of Data (CCD), the 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. The longitude and latitude of colleges link them with geographic school districts' boundaries, and our analyses are conducted at the district level. As with the dependent variable, we sum the number of charter school teachers (the treatment variable) within districts (most of the covariates are already aggregated 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 A1 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.[15] In one robustness check, discussed later, we exploit the exogenous variation of years that charter law adopted to assign treated (earlier adopters) and control groups (later adopters) address the concern of time-varying unobservables.

The analysis is limited to districts that have IHE teacher education programs located within their geographic boundaries and we separate these into two groups. Table 1 compares the average characteristics of districts with any charter schools during the panel with other "untreated" districts across the school years 1995–2018. The treated and untreated districts are clearly different from one another. For example, they are larger in size and have more students who are racial/ethnic minorities. This motivates our method, described later, which leverages mostly within-district variation over time in charter enrollment and teacher education graduates.

We focus our analysis on the commuter sample; 397 districts had at least one education-degree-granting, commuter IHE, 210 of which have had at least one charter school during the panel period. Table 1 provides descriptive statistics for this sample. We also carry out some analysis on the larger sample of 1277 districts with any education degree granting IHE (commuter or otherwise), 520 of which are ever-charter.

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

Our GDD method identifies effects from both within- and between-district variation. More specifically, some of that variation is coming from the large share of ever-charter districts that have just one charter school and never go beyond 15 percent market share. However, roughly half the districts end up with market share in the 15–50 percent range (see kernel density in appendix Fig. A1). A much smaller share exceeds 50 percent market share.

The GDD method identifies effects mostly using within-district variation over time. In some cases, this variation comes from the entry of a single charter school in a district and, in other cases, it comes from continued growth in the number and size of charter schools. Fig. 3 provides descriptive evidence especially of the latter form of variation. Specifically, we plot the changes in the number of charter teachers, re-centering all treated districts to the year in which the first charter school enters. This shows, as expected, that the number of charter teachers hired grows gradually over time; and grows faster in districts that rise to higher charter market shares.

4. Empirical framework

4.1. Generalized difference-in-differences (GDD)

We use the above model to design and interpret an empirical analysis to examine the supply side response of charter entry. Specifically, we use a generalized difference-in-differences (GDD) approach where the unit of analysis is the school district, indexed by i, and the main dependent variable is the number teacher education degrees produced by IHEs located within the district ($\widetilde{Qs}_{it}^{*IHE}$). The treatment variable is the equilibrium quantity of teachers employed in charter schools ($\widetilde{Qd}_{it}^{*Charter,\cdot}$). (We use the tildes to distinguish the variables in the theoretical model (Section 2) from our measures of those variables in the empirical analysis.)

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

We specifically estimate the following model:

Table 4
Robustness: effects of charter share on teacher supply.

	(1)	(2)	(3)	(4)
Panel A Double-Log Form (elasticity)				
Log(Charter teacher)	−0.054***	−0.051***	−0.041**	−0.039**
	[0.019]	[0.017]	[0.018]	[0.015]
R-squared	0.927	0.931	0.931	0.924
Observations	6809	6809	6809	6809
N(district)	397	397	397	397
Panel B Double-Share Form				
Charter share	−2.268	−3.047*	−3.320**	−2.009*
	[1.523]	[1.565]	[1.502]	[1.120]
R-squared	0.785	0.790	0.792	0.767
Observations	6809	6809	6809	6809
N(district)	397	397	397	397
Panel C Outcome forwarded four years				
Charter teacher	−0.022*	−0.021	−0.024*	−0.023*
	[0.013]	[0.013]	[0.012]	[0.012]
R-squared	0.704	0.715	0.716	0.708
Observations	6414	6414	6414	6417
N(district)	386	386	386	386
Panel D Full Sample				
Charter teacher	−0.028**	−0.028**	−0.031***	−0.029**
	[0.011]	[0.011]	[0.011]	[0.011]
R-squared	0.890	0.894	0.894	0.890
Observations	25,774	25,774	25,774	25,774
N(district)	1277	1277	1277	1277
District FE	Yes	Yes	Yes	Yes
State-by-year FE	Yes	Yes	Yes	No
Student control	No	Yes	Yes	Yes
District control	No	No	Yes	Yes
State time trend	No	No	No	Yes

Notes: This table shows GDD estimates of the robustness checks of the effects of charter entry on teacher supply. In Panel A, the outcome is measured as the log of the number of education bachelor degrees and log of the number of charter teachers (elasticity). In Panel B, the outcome is measured as the percentage of education degrees to all bachelor's degrees, and the charter share is measured as the charter enrollment share (this necessitates weighting by district size). In Panel C, the outcome is the number of education bachelor's degrees, and it is forwarded by four years instead of three years in the main results. In Panel D, the analysis is conducted using the full sample, while analyses in other panels are using the commuting sample. See notes in Table 2 for additional details.

$$\widetilde{Qs}_{it}^{*IHE} = \alpha + \beta_1 \widetilde{Qd}_{it}^{*Charter,Total} + X_{it}\gamma + \mu_i + \lambda_{st} + \varepsilon_{it} \tag{1}$$

where μ_i reflects unit fixed effects (in this case, for geographic school districts) and λ_{st} reflects state-by-year fixed effects. The coefficient of interest is represented by β_1, which reflects the roles played by θ^{IHE}, γ^{IHE}, $\phi^{Charter}$, and ϕ^{TPS} in the earlier model. X_{it} is a vector of district characteristics. As these covariates are potentially endogenous, they are only included in certain specifications and as partial tests for charter effects on the student population. In our baseline analyses, we estimate the GDD model assuming a three-year lag in the effect of charter entry, which is reinforced by the later event studies, but we consider other lags as well. The error term ε_{it} is clustered at the district level in the main specification.[16]

[16] We estimate the model without weights. In theory, we could weight either by college size or district size, but this would be inconsistent with the theoretical model, which accounts for the size of each, modelling the *number* of new IHE graduates as a function of the *number* of charter school teachers hired. Nevertheless, we do note that the estimates are even larger and more precise when weighting on either dimension.

4.2. Threats to identification

Our method accounts for time-invariant unobserved differences across districts and our main identifying assumption is that there are no unobserved shocks that are correlated with both changes charter enrollment/hiring *and* changes in the new IHE teacher supply. We note that, even if charter teacher hiring changed based on time-invariant unobserved district characteristics that affected the new teacher supply, this would not introduce bias if the timing of charter entry were conditionally random, which is possible given all the steps involved in opening a charter school (e.g., putting in a charter application, getting it approved, gaining access to a building, recruiting students, and hiring staff).

Still, it is worth considering specific threats to identification. First, we might worry that there are unobserved shocks that are correlated with the timing of charter entry and with changes in the number of *total* college graduates generally (e.g., changes in state finances that affect both higher education and school funding), which indirectly alters the number of teacher graduates. To account for this, we sometimes include the number of college graduates (regardless of major) as a control variable.

Second, local governments might change non-charter education policies at the same time they introduce charter schools, as part of a package of reforms. Since school districts are often charter authorizers, it may be that a change in school board politics leads both to an increase in charter schools and, for example, new TPS teacher hiring and development policies. In this case, we might falsely attribute outcome changes to charter schools that were actually caused by other, simultaneous policy changes. We discuss various tests for these and related types of violations in the next section, after describing our main results.

4.3. Endogeneity tests

We implement several tests of endogeneity, including a variation of the usual parallel trends test adapted for the GDD setting, two placebo tests, and an additional test leveraging variation in the timing of state laws. We discuss each of these below.

The parallel trends assumption is still necessary for the GDD setup because some of the identification still comes from comparisons between treated and never-treated districts. While there is no generally accepted method for testing parallel trends in the GDD, we follow Chen and Harris (2023) and estimate event studies where the control group is never treated (during the panel period) and the treatment group involves only those districts that experience positive shocks in charter enrollment/teacher hiring of greater than 10 percentage points in five years. This test therefore leverages both between-group (treatment-comparison) variation in outcomes and within-treatment-group variation over time, which aligns with the GDD identification strategy.

We specifically use both the Sun and Abraham (2021) and traditional event study versions of (1) to estimate the dynamic treatment effects and investigate the parallel trends assumption. The traditional event study model is:

$$Qs_{it}^{*IHE} = \alpha + \sum_{r=-m}^{q} \beta_r \left(T_i \cdot d_{i,r}\right) + X_{it}\gamma + \mu_i + \lambda_t + \varepsilon_{it} \tag{2}$$

where $d_{i,r}$ is a dummy of the r years of leads or lags since district I initiated first charter school. 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 report the Sun and Abraham (2021) version of (2). While no parallel trends test can definitely establish parallel trends, this method provides important suggestive evidence.

The first placebo test is based on the idea that we should see more limited changes in the supply of elementary education degrees when

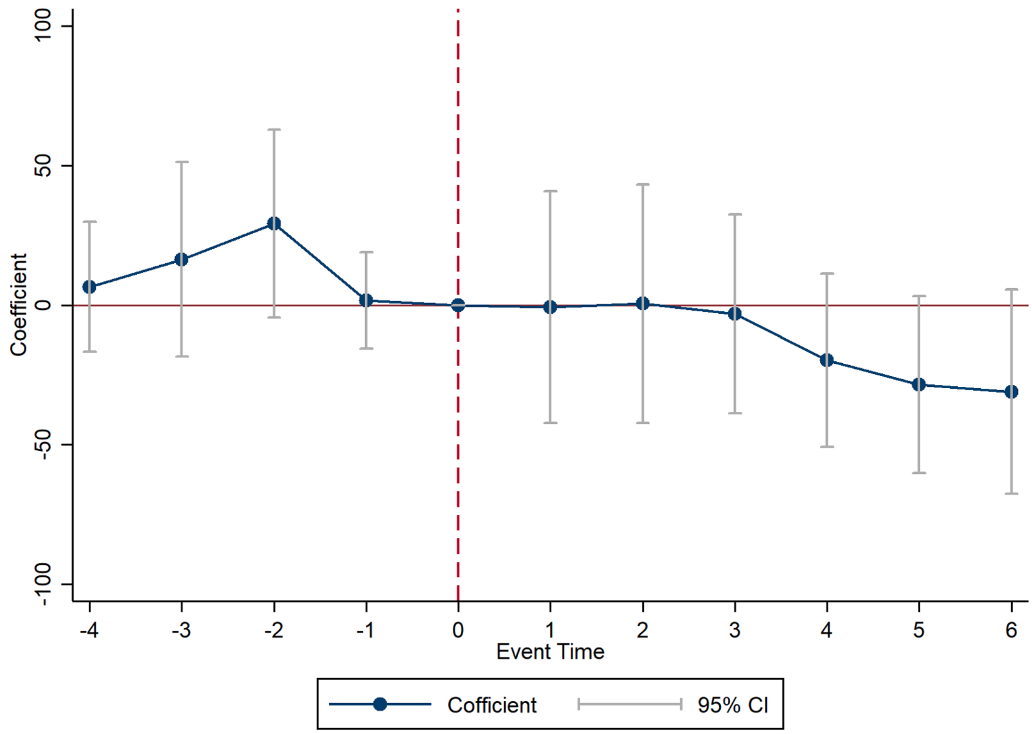

Fig. 4. Event Study Results.
(Treated Sample with Shocks to Charter Enrollment)
Notes: This figure shows the event study results using the method of Sun and Abraham (2021). Zero is the year that treated districts experienced increases in charter enrollment share of ten percentage points within five years. The outcome is not shifted forward as in the earlier tables; instead, we see how the effect is delayed by comparing the initial post-treatment years with the later ones. See Table 2 for additional details.

Table 5
Placebo effects of charter teacher on teacher supply.

	(1)	(2)	(3)	(4)
Panel A the number of secondary education degrees				
Elementary charter teacher	−0.004	−0.004	−0.004	−0.004
	[0.004]	[0.004]	[0.004]	[0.004]
R-squared	0.508	0.511	0.512	0.456
Observations	6809	6809	6809	6809
N(district)	397	397	397	397
Panel B the number of secondary elementary degrees				
Secondary charter teacher	0.005	0.004	0.001	0.001
	[0.021]	[0.020]	[0.022]	[0.020]
R-squared	0.808	0.815	0.816	0.804
Observations	6809	6809	6809	6809
N(district)	397	397	397	397
Panel C Competitive college sample				
Charter teacher	−0.003	−0.003	−0.004	−0.002
	[0.006]	[0.006]	[0.005]	[0.005]
R-squared	0.927	0.929	0.929	0.923
Observations	5231	5231	5231	5231
N(district)	240	240	240	240
District FE	Yes	Yes	Yes	Yes
State-by-year FE	Yes	Yes	Yes	No
Student control	No	Yes	Yes	Yes
District control	No	No	Yes	Yes
State time trend	No	No	No	Yes

Notes: This table presents GDD estimates of the placebo tests. Panels A and B use misaligned grade levels of charter teachers with outcomes, using the commuting sample. Panel C uses the competitive colleges' sample, defined as ACT Composite 75th percentile score is greater than 24 and the admission rate is below 60 %. The specifications are otherwise equivalent to Table 2, e.g., the outcome variable is forwarded by three years. See additional notes in Table 2.

middle and high school charters enter. Therefore, we modify the effect estimation and test for these effects on "misaligned" teacher supply grade levels. Similarly, we expect limited effects on teacher education degrees from competitive colleges whose students rarely originate from, or intend to stay in, the local community. Therefore, we test for effects on just this subset of competitive colleges.

By including all districts (with commuter IHEs), we open ourselves up to the possibility that charter schools locate in some areas because of unobserved shocks in those areas that do not occur in comparison districts. We can reduce this possibility by restricting the comparison group to districts that had no legal path to opening charter schools because of their state laws. They may have wanted to open charter schools but could not. We therefore compare charter districts in states that were early adopters of charter school laws (1991–2003) to non-charter districts in late-adopting states (2010–2019) .[17] Here, the assumption is that any unobserved shocks affecting charter entry occur in all states at the same time, but the peculiarities of state political situations lead to exogenous timing of charter school laws. A disadvantage of this approach is that there are few late-adopter states and many of them are in rural areas, which reduces precision and may reflect effect heterogeneity.

If our estimates are robust to all these tests—the adapted parallel

[17] 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, and Maryland. States that adopted charter laws between 2010-2019 (late adopter states) are: Mississippi, Maine, Alabama, Washington, Kentucky, and West Virginia. No states passed laws between 2004-2009.

trends test, two placebo tests, and one alternative test leveraging variation in state laws—then we argue that this will provide a strong claim to causal inference.

5. Results

5.1. Generalized difference-in-differences results

Table 2 reports results from the GDD using a three-year lag. All of the estimates are negative and precisely estimated and similar in magnitude across the columns that add various combinations of fixed effects and covariates. For example, the first estimate in the first column suggests that increasing charter demand by 100 teachers reduces the number of new teacher education graduates from local commuter colleges by about two. These figures are also of the right order of magnitude given the discussion in the theory section where we hypothesized that the effect size would be roughly -0.01.

Next, we consider specific types of education majors. Table 3 shows that the reductions are concentrated in elementary education. The coefficients are also negative, though not quite significant by the usual standards, for special education. (The estimates are negative and precise in other specifications, available upon request.) The number of math education majors actually increases, which may reflect the preferences of charter schools for teachers who have more content knowledge in core academic subjects, as well as student interest in having degrees with higher option values (outside of teaching). The estimates for master's degree are also negative, but too imprecise to draw any conclusions (Table A6).

In Table 4, we switch back to the combined measure that includes all education degrees and show that the results are robust to various specification changes: double-log (elasticity) and four-year lag in the effect, as well as using the full sample of colleges instead of just commuter colleges. We also estimate a version where the treatment and dependent variables are specified as shares; that is, we use the share of college graduates who are teacher education majors as the dependent variable and the share of public school enrollment that is in the charter sector as the treatment variable. The main specification, in contrast, places these denominators of the shares as control variables. While the magnitudes in the first two cases cannot be directed compared to Table 2, they are similar when we place them on a common scale. This reinforces that our results are not driven by the assumptions of the theoretical model that underlies this analysis (Section 2).

The results are also robust to various additional alterations in the estimation. Our results are nearly identical when we alter the definition of the commuter sample (Fig. A2). The point estimates are also negative, though smaller in absolute value, when we aggregate up from the district level to the commuting zone level (Table A5); these estimates are also less precise given the much smaller samples.

We also estimate a simple DD model using different specifications, including with various types of propensity score matching. As a starting point, we identify as treated those districts that ever have charter market share greater than 10 percent. As shown in Table A3, the estimates remain negative and precisely estimated; the scales are naturally different, but are similar in magnitude to the GDD when we place then on a common scale. These DD estimates are also robust to using various cut-offs for the charter share (appendix Fig. A3). For example, the first estimate to the left side shows the effect when we compare never-charter districts to those that have at least one percent charter market share at some point, then we raise this threshold to two percent, and so on up to 20 percent. As expected, the estimates get more negative as the charter dosage increases.

The GDD and DD have a strong claim to identification because endogeneity would have to be caused by a factor whose changes over time are correlated with changes over time in both teacher education majors and charter entry. Fig. 4 tests the version of the parallel trends test designed for the GDD (see Section 4 for a description). In addition to showing evidence of flat pre-trends, the results also show that the effects do indeed come with a 3–4 year delay.

The placebo tests, too, reinforce that we are identifying causal effects. We see no evidence of effects of charter elementary entry on high school education majors in Table 5 - Panel A (and vice versa in Table 5 – Panel B), nor do we see any evidence of effects on teacher supply in competitive colleges (Table 5–Panel C).[18] As a final test for endogeneity, we generally see evidence of effects when comparing early- and state-adopting states, which leverages variation in charter entry that is more plausibly exogenous (Table A4). All the estimates are negative and precisely estimated.

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 & 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. The direction and magnitude of the coefficients are robust to alternative identification strategies, specifications, comparison groups, and samples. In some of these robustness checks, the results become less precise, but the general similarity of the estimates across many different specifications, as well as the various tests for endogeneity, reinforce the validity of this conclusion. Our findings also support previous studies that find students are knowledgeable and responsive to labor market conditions (Acton, 2021; Baker et al., 2018; Blom et al., 2021).

The magnitude of these effects is not trivial. Roughly 7 percent of all U.S. students and teachers are now in charter schools. Our estimate that a 10 percent increase in charter share reduces the new teacher supply by 1 percent *per year* nationally means that, over the course of decade, charter schools reduce the total new teacher supply by about 7 percent. This may also be an under-estimate if charter entry in districts without IHE teacher education programs also reduces the new teacher supply.

Our analysis involves numerous caveats. While our results imply a decline in the supply of new traditionally prepared teachers, the number of education degrees is an imperfect proxy for the teacher supply. Also, while prior research suggests that teacher education graduates work close to home, our data do not allow us to locate where teacher education graduates work. Our findings also do not address effects on new alternatively prepared teachers. Finally, we cannot generalize our results to districts that might have their first charter schools in the future.

We also cannot directly address matters of teacher quality. Some evidence on non-traditional routes, such as *Teach for America*, suggest that alternately prepared teachers are sometimes more effective (Glazerman et al., 2006; Clark et al., 2013). Bruhn et al. (2020) 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 analysis of quality is more difficult.

But analyses of the quantity of teachers, such as this one, are also important. First, a reduction in the total supply makes it more difficult to address narrower shortages in particular subjects and locations (Dee & Goldhaber, 2017), especially in special education where we see some of the most noticeable drops in the new teacher supply. Second, while we are not able to directly study the effect of charter entry on teacher

[18] Competitive colleges are defined as ACT Composite 75th percentile score is greater than 24 and the admission rate is below 60%. We use ACT score and admission information from the 2010 IPDES data.

quality, teachers who major in education are more likely to stay in teaching than alternatively prepared teachers (Redding & Smith, 2016), thus reducing turnover and allowing them to leverage their return to experience; all else equal, high turnover and low experience reduce the average quality of teaching (Ronfeldt et al., 2013; Harris & Sass, 2011). Third, given the centrality of teachers in schooling, the fact that charter entry has such a noticeable effect on the teacher labor market is noteworthy. Fourth, the ongoing and aggressive expansion of school vouchers is likely to have similar effects on the teacher pipeline.

In short, it appears that charter school entry has noticeably altered the teacher pipeline and that has larger implications for the schooling market.

Data availability

Data will be made available on request.

Acknowledgments

This research was carried out under the auspices of the National Center for Research on Education Access and Choice (REACH) based at Tulane University, which is supported by the Institute of Education Sciences, U.S. Department of Education, through Grant R305C100025 to The Administrators of the Tulane Educational Fund. The opinions expressed are those of the authors and do not represent views of the Institute or the U.S. Department of Education. We thank Dan Oliver for his comments and for data assistance.

Appendix

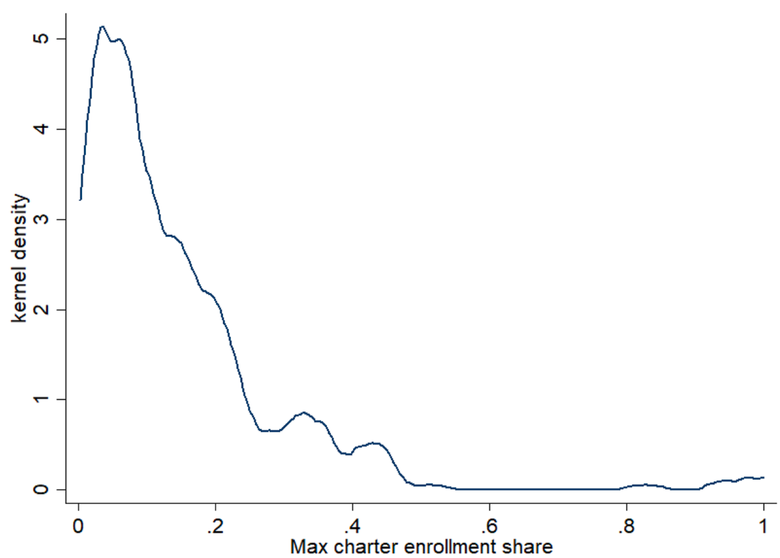

Fig. A1. Kernel density plot of charter enrollment share.
Notes: This figure presents the kernel density plot of the maximum of charter enrollment share for district with charter schools.

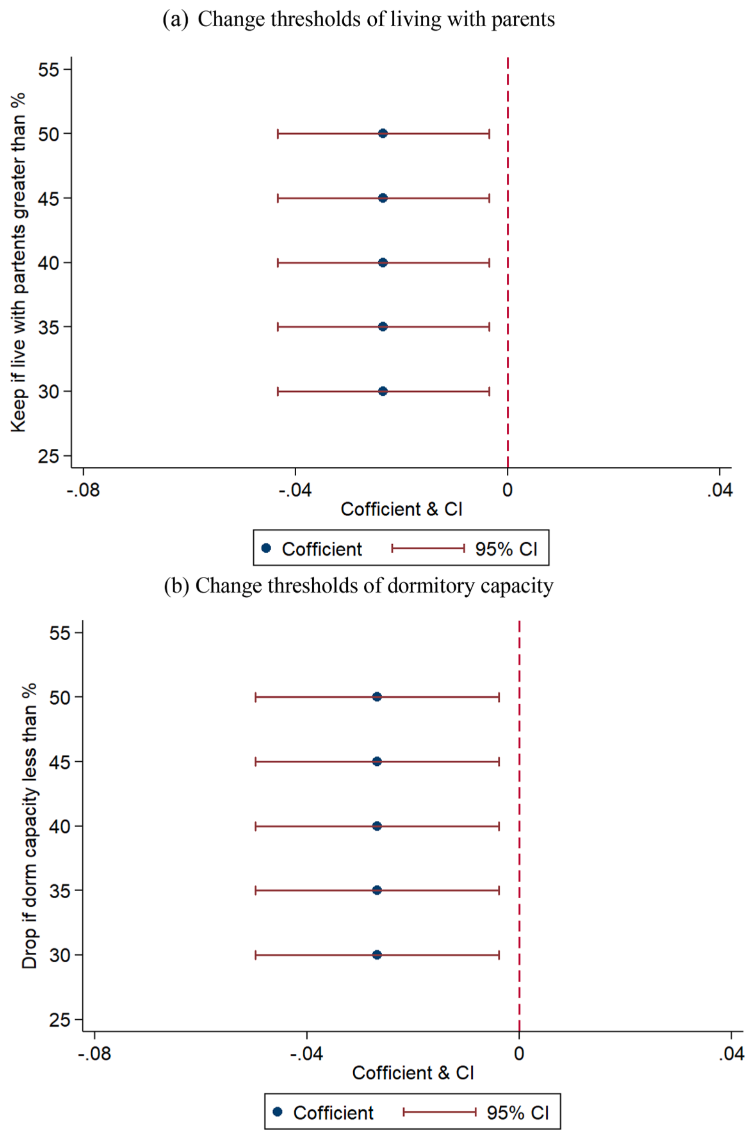

Fig. A2. Alternative Thresholds for Commuter Sample.
Notes: This figure shows how the GDD effects from Table 2 change when using alternative thresholds for commuter samples. Our main result is based on the commuter sample with at least a 40 percent average of students living off-campus with family over the time period or with dormitory capacity below 40 percent of total enrollment, i.e., the middle options in each panel.

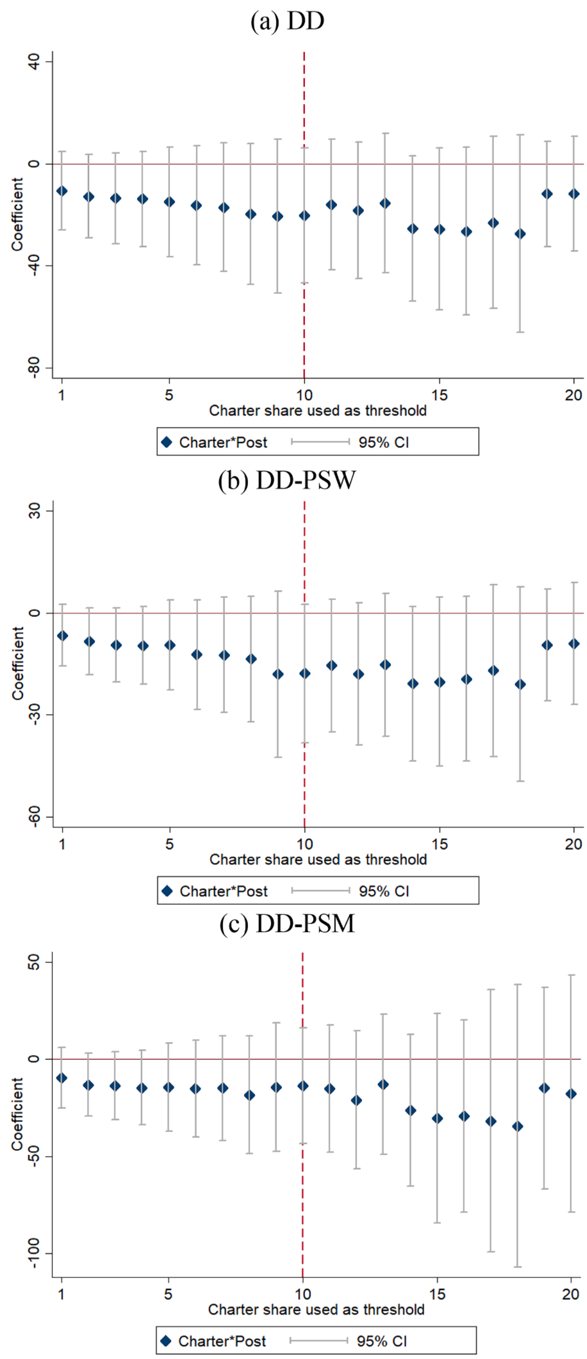

Fig. A3. Simple DD Estimates Varying the Definition of Treatment from 1 % to 20 % Charter Market Share.
Notes: This figure plots the DD estimates in the number of education bachelor's degrees using the charter enrollment share of 1- 20 % as the threshold of treated districts (Column 3 specification of Table A3). Table A3 for additional details.

Table A1
Year charter law passed by state.

Year	State
1991	Minnesota
1992	California
1993	Colorado, Massachusetts, Michigan, New Mexico, Wisconsin
1994	Arizona, Georgia, Hawaii, Kansas
1995	Alaska, Arkansas, Delaware, Louisiana, New Jersey, Rhode Island, Texas, Wyoming
1996	Connecticut, District of Columbia, Florida, Idaho, Illinois, New Hampshire, North Carolina, South Carolina
1997	Nevada, Ohio, Pennsylvania
1998	Missouri, New York, Utah, Virginia

(*continued on next page*)

Table A1 (*continued*)

Year	State
1999	Oklahoma, Oregon
2001	Indiana
2002	Iowa, Tennessee
2003	Maryland
2010	Mississippi
2011	Maine
2015	Alabama
2016	Washington
2017	Kentucky
2019	West Virginia
NA	Montana, Nebraska, North Dakota, South Dakota, Vermont

Data source: National Longitudinal School Database.

Table A2
Robustness with missing data flags.

	(1)	(2)	(3)	(4)
Charter teacher	−0.022*	−0.021*	−0.024**	−0.024**
	[0.011]	[0.011]	[0.010]	[0.010]
R-squared	0.703	0.714	0.715	0.706
Observations	6809	6809	6809	6809
N(district)	397	397	397	397
Missing data flag FE	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes
State-by-year FE	Yes	Yes	Yes	No
Student control	No	Yes	Yes	Yes
District control	No	No	Yes	Yes
State time trend	No	No	No	Yes

Notes: This table reports GDD estimates that are the same as Table 2, except that we add missing data flags and recode the missing outcome variable as zero. See additional notes in Table 2.

Table A3
Effects of charter entry on teacher supply (simple DD).

	(1)	(2)	(3)	(4)
Panel A DD				
Charter*Post	−19.193*	−19.203	−20.267	−18.899**
	[10.730]	[11.717]	[13.455]	[8.846]
R-squared	0.701	0.715	0.716	0.706
Observations	4806	4806	4806	4854
N(district)	286	286	286	286
Panel B DD-PSW				
Charter*Post	−16.286*	−17.479*	−17.862*	−10.946*
	[8.661]	[9.279]	[10.318]	[5.963]
R-squared	0.714	0.723	0.724	0.713
Observations	4806	4806	4806	4854
N(district)	286	286	286	286
Panel C DD-PSM				
Charter*Post	−15.593	−12.525	−13.638	−14.190
	[13.982]	[14.155]	[15.102]	[9.171]
R-squared	0.699	0.721	0.722	0.708
Observations	3231	3231	3231	3367
N(district)	198	198	198	198
District FE	Yes	Yes	Yes	Yes
State-by-year FE	Yes	Yes	Yes	No
Student control	No	Yes	Yes	Yes
District control	No	No	Yes	Yes
State time trend	No	No	No	Yes

Notes: This table shows various forms of DD estimates of the effect of charter entry on the number of undergraduate teacher education degrees using the commuting sample. The treated group includes districts that ever have charter enrollment share greater than 10 percent, and the comparison group includes districts without charter schools in all states. Districts with charter share greater than zero but less than 10 percent are removed from the analysis. Post is the period after the first charter entry. We use PSM to match districts using the baseline means of student controls and district controls. The DD-PSM uses nearest neighbor matching. The DD-PSW weights based on the inverse probability of treatment. See additional notes in Table 2.

Table A4
Early adopter states vs. late adopter states (GDD).

	(1)	(2)	(3)	(4)
Charter teacher	−0.025**	−0.025**	−0.028**	−0.025**
	[0.012]	[0.011]	[0.012]	[0.011]
R-squared	0.890	0.895	0.895	0.889
Observations	13,971	13,971	13,971	13,971
N(district)	697	697	697	697
District FE	Yes	Yes	Yes	Yes
State-by-year FE	Yes	Yes	Yes	No
Student control	No	Yes	Yes	Yes
District control	No	No	Yes	Yes
State time trend	No	No	No	Yes

Notes: These are estimates are identical to Table 2 except that we restrict to the sample of early and late adopting states.

Table A5
Effects of charter share on teacher supply (Commuting zone).

	(1)	(2)	(3)	(4)
Charter teacher	−0.016	−0.012	−0.008	−0.006
	[0.012]	[0.010]	[0.011]	[0.010]
R-squared	0.832	0.837	0.848	0.830
Observations	3107	3107	3107	3107
N(CZ)	167	167	167	167
District FE	Yes	Yes	Yes	Yes
State-by-year FE	Yes	Yes	Yes	No
Student control	No	Yes	Yes	Yes
District control	No	No	Yes	Yes
State time trend	No	No	No	Yes

Notes: The table shows GDD effect estimates with the data aggregated up to the commuting zone level using the commuting sample. Standard errors are clustered at the commuting zone level. See notes in Table 2 for additional details.

Table A6
Effects of charter share on teacher supply (Master's degrees).

	(1)	(2)	(3)	(4)
Charter teacher	−0.013	−0.008	−0.023	−0.026
	[0.038]	[0.036]	[0.046]	[0.042]
R-squared	0.803	0.806	0.812	0.791
Observations	6809	6809	6809	6809
N(district)	397	397	397	397
District FE	Yes	Yes	Yes	Yes
State-by-year FE	Yes	Yes	Yes	No
Student control	No	Yes	Yes	Yes
District control	No	No	Yes	Yes
State time trend	No	No	No	Yes

Notes: This table shows GDD estimates of charter effects on the supply of education *master's* degrees using the commuting sample. See notes in Table 2 for additional details.

References

Acton, R. (2021). Community college program choices in the wake of local job losses. *Journal of Labor Economics, 39*(4), 1129–1154. https://doi.org/10.1086/712555

Baker, B., Jacob, B., & Marinescu, I. (2018). The effect of labor market information on community college students' major choice. *Economics of Education Review, 65* (August), 18–30. https://doi.org/10.1016/j.econedurev.2018.05.005

Barrett, N., Carlson, C., Harris, D., & Lincove, J. (2022). When the walls come down: Evidence from New Orleans on teacher labor markets in the absence of unions, tenure, and certification rules. *Educational Evaluation and Policy Analysis, 44*(2), 283–312.

Blom, E., Cadena, B. C., & Keys, B. J. (2021). Investment over the business cycle: Insights from college major choice. *Journal of Labor Economics, 39*(4).

Boyd, D., Lankford, H., Loeb, S., & Wyckoff, J. (2005). The draw of home: How 'teachers' preferences for proximity disadvantage urban schools. *Journal of Policy Analysis and Management, 24*(1), 1134–1132.

Bruhn, J.M., Imberman, S.A., & Winters, M.A. (2020). Regulatory arbitrage in teacher hiring and retention: Evidence from Massachusetts Charter Schools. NBER Working Paper No. 27607. http://www.nber.org/papers/w27607.

Bulkley, K., Marsh, J., Strunk, K., Harris, D., & Hashim, A. (2020). *Challenging the One Best System: The Portfolio Management Model and Urban School Governance*. Harvard Education Press.

Burian-Fitzgerald, M., Luekens, M. T., & Strizek, G. A. (2004). Less red tape or more green teachers: Charter school autonomy and teacher qualifications. *The impact of Charter schools on teachers (pp. 11-31)*. New York, NY: Teachers College Press.

Cannata, M. A., & Penaloza, R. (2012). Who are charter school teachers? Comparing teacher characteristics, job choices, and job preferences. *Education Policy Analysis Archives, 20*. Education policy analysis archives, 01 September 2012, Vol.20.

Carruthers, C. K. (2012). The qualifications and classroom performance of teachers moving to charter schools. *Education Finance and Policy, 7*(3), 233–268.

Chen, F., & Harris, D. N. (2023). The market-level effects of charter schools on student outcomes: A national analysis of school districts. *Journal of Public Economics, 228*, Article 105015.

Chubb, J., & Moe, T. (1990). *Politics, markets, and America's schools*. Washington, DC: Brookings. Institution Press.

Clark, M.A., Chiang, H.S., Silva, T., McConnell, S., Sonnenfeld, K., Erbe, A., & Puma, M. (2013). The effectiveness of secondary math teachers from teach for America and the teaching fellows programs. NCEE 2013-4015. In *National center for education*

evaluation and regional assistance. National Center for Education Evaluation and Regional Assistance.
Cohodes, S.R. & Parham, K.S. (2021). Charter schools' effectiveness, mechanisms, and competitive influence. NBER Working Paper 28477. Cambridge, MA; National Bureau of Economic Research.
Cowan, J., Goldhaber, D., Hayes, K., & Theobald, R. (2016). Missing elements in the discussion of teacher shortages. *Educational Researcher, 45*(8), 460–462.
Cowen, J. M., & Winters, M. A. (2013). Do charters retain teachers differently? Evidence from elementary schools in Florida. *Education Finance and Policy, 8*(1), 14–42.
Dee, T. S., & Goldhaber, D. (2017). Understanding and addressing teacher shortages in the United States. *The Hamilton Project Policy Proposal*, (April), 1–28.
Education Commission of the States. (2020). Charter schools: Do teachers in a charter school have to be certified? Retrieved from http://ecs.force.com/mbdata/MBQuestNB2C?rep=CS2021.
Falch, J., & Strøm. (2009). Teacher shortages and the business cycle. *Labour Economics, 16*(6), 648–658.
Glazerman, S., Mayer, D., & Decker, P. (2006). Alternative routes to teaching: The impacts of teach for america on student achievement and other outcomes. *Journal of Policy Analysis and Management, 25*(1), 75–96.
Glomm, G., Harris, D., & Lo, T. F. (2005). Charter school location. *Economics of Education Review, 24*(4), 451–457.
Goodman-Bacon, A. (2021). Difference-in-differences with variation in treatment timing. *Journal of Econometrics, 225*(2), 254–277. https://doi.org/10.1016/j.jeconom.2021.03.014
Grossman, P., & Loeb, S. (2008). *Alternative routes to teaching: Mapping the new landscape of teacher education*. Cambridge, MA: Harvard Education Press.
Harris, D., & Sass, K. (2011). Teacher training, teacher quality and student achievement. *Journal of Public Economics, 95*(7), 798–812.
Harris, D. N. (2020). *Charter School City: What the End of Traditional Public Schools in New Orleans Means for American Education*. University of Chicago Press.
Hohnstein, S. (2017). The rise of the urban alternative teacher certification. *Teacher Education and Practice, 30*(1), 194(13). Retrieved from http://citeseerx.ist.psu.edu/viewdoc/download?doi=10.1.1.627.455&rep=rep1&type=pdf.
Hoxby, C. (2000). Would school choice change the teaching profession? *NBER Working Paper Series,* 7866.
Hoxby, C. (2003). School choice and school productivity. Could school choice be a tide that lifts all boats?. *The economics of school choice* (pp. 287–342). University of Chicago Press.
Ingersoll, R. (2001). Teacher turnover and teacher shortages: An organizational analysis. *American Educational Research Journal, 38*(3), 499–534. Web.
Jabbar, H. (2018). Recruiting "Talent": School choice and teacher hiring in New Orleans. *Education Administration Quarterly, 54*(1), 115–151.
Jackson, C. K. (2012). School competition and teacher labor markets: Evidence from charter school entry in North Carolina. *Journal of Public Economics, 96*(5–6), 431–448.
Kraft, M. A., Brunner, E. J., Dougherty, S. M., & Schwegman, D. J. (2020). Teacher accountability reforms and the supply and quality of new teachers. *Journal of Public Economics, 188*. https://doi.org/10.1016/j.jpubeco.2020.104212, 104212–.
Malloy, C., & Wohlstetter, P. (2003). Working conditions in Charter schools: What's the appeal for teachers? *Education and Urban Society, 35*(2), 219–241.
Miron, G., & Nelson, C. (2002). *What's public about charter schools? lessons learned about choice and accountability*. Corwin Press.
Nagler, M., Piopiunik, M., & West, M. R. (2020). Weak markets, strong teachers: Recession at career start and teacher effectiveness. *Journal of Labor Economics, 38*(2), 453–500.
Partelow, L. (2019). *What to make of declining enrollment in teacher preparation programs*. American Progress. Retrieved September 16, 2021 from https://www.americanprogress.org/article/make-declining-enrollment-teacher-preparation-programs/.
Podgursky, M. (2008). Teams versus bureaucracies personnel policy, wage setting, and teacher quality in traditional public, charter, and private schools. *Charter school outcomes*. New York: Routledge. Mark Berends, Matthew Springer, and Herbert Walberg (Eds.).
Redding, C., & Smith, T. M. (2016). Easy in, Easy out: Are alternatively certified teachers turning over at increased rates? *American Education Research Journal, 53*(4), 1086–1125.
Ronfeldt, M., Loeb, S., & Wyckoff, J. (2013). How teacher turnover harms student achievement. *American Educational Research Journal, 50*(1), 4–36.
Stuit, D., & Smith, T. (2012). Explaining the gap in charter and traditional public school teacher turnover rates. *Economics of Education Review, 31*(2), 268–279.
Sun, & Abraham, S. (2021). Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. *Journal of Econometrics, 225*(2), 175–199. https://doi.org/10.1016/j.jeconom.2020.09.006
U.S. Department of Education, Institute of Education Sciences, National Center for Education Statistics. 2016. Prevalence of teachers without full state certification and variation across schools and states. Retrieved July 12, 2022 from: https://www2.ed.gov/rschstat/eval/teaching/teachers-without-certification/report.pdf.
U.S. Department of Education. Institute of Education Sciences, National Center for Education Statistics. (2018). Characteristics of public school teachers who completed alternative route to certification programs. *The Condition of Education*. Retrieved from https://nces.ed.gov/programs/coe/indicator_tlc.asp.
Weixler, L. B., Harris, D. N., & Barrett, N. (2018). Teachers' Perspectives on the Learning and Work Environments Under the New Orleans School Reforms. *Educational Researcher, 47*(8), 502–515.
Will, M. (2022). *Fewer people are getting teacher degrees. Prep programs sound the alarm*. May 22. Education Week. Retrieved April 5, 2022 from https://www.edweek.org/teaching-learning/fewer-people-are-getting-teacher-degrees-prep-programs-sound-the-alarm/2022/03.
Winters, M. A., Carpenter, D. M., II, & Clayton, G. (2017). Does attending a charter school reduce the likelihood of being placed into special education? Evidence from Denver, Colorado. *Educational Evaluation and Policy Analysis, 39*(3), 448–463.
Zimmer, R., Romano, R., & Epple, D. (2015). Charter schools: A survey of research on their characteristics and effectiveness. 10.3386/w21256.