Federal Reserve Bank of Dallas

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Working Paper 2618 **July 2026**

Research Department

https://doi.org/10.24149/wp2618

Should States Reduce Teacher Licensing Requirements? Evidence from the Rise of For-Profit Training Programs in Texas*

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June 23, 2026

Abstract

We provide a comprehensive analysis of a Texas policy that relaxed teacher licensing requirements and created a large for-profit training industry. Using detailed administrative data, we show that for-profit-trained teachers have higher turnover and lower value-added than standard-trained teachers. But the policy significantly increased the supply of certified teachers, reducing schools' reliance on uncertified teachers with even worse outcomes. Exploiting variation in policy exposure across schools, we find a zero net impact on student achievement due to these offsetting forces. Thus lower licensing requirements improved access to teaching and reduced training costs without harming students.

JEL: J44, J24, J48

Keywords: teacher certification, teacher preparation, occupational licensing

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* The views expressed in this article are those of the authors and do not necessarily reflect the opinions or official position of the Federal Reserve Bank of Dallas, Federal Reserve System, Texas Education Agency, the Texas Higher Education Coordinating Board, Texas Workforce Commission, or the state of Texas. This research was supported by a Cornell Center for Social Sciences Grant. For helpful comments we thank Ryan Brown, Serena Canaan, Samuel Dodini, Benjamin Goldman, Douglas Harris, Michael Lovenheim, Pierre Mouganie, Germán Reyes, Seth Sanders, Tucker Smith, Christopher Walters, Melanie Wasserman, Alexander Willén, Jian Zou, and participants at various seminars and conferences. We are grateful to Holly Kosiewicz, Zhixiang Lu, and members of the Texas Education Research Center Advisory Board for help in accessing and working with the data. All errors are our own.

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1 Introduction

Although economists have long recognized that occupational licensing involves a tradeoff between worker supply and quality (Smith, 1776; Friedman and Kuznets, 1945; Kleiner, 2000), it has proven difficult to quantify the overall costs and benefits of licensing laws. Many papers ask how occupational licensing affects employment and wages but do not have the data to examine impacts on product quality (e.g., Kleiner and Krueger, 2010, 2013). Other work shows how licensing affects product quality but has limited evidence on supply side effects (e.g., Anderson et al., 2020). In a recent exception, Kleiner and Soltas (2023) estimate a model of occupational licensing and find that it reduces average welfare for U.S. workers and consumers. Yet their analysis relies on the strong assumption that the variation in licensing laws across states and occupations is as good as random.

This paper exploits a major policy change and detailed administrative data to estimate the supply, quality, and downstream impacts of occupational licensing for public school teachers. All U.S. states require individuals to undergo pedagogical training before becoming a certified teacher, which makes teaching the largest licensed occupation in the country. Analyzing the desirability of licensing is more complex in teaching than it is in other occupations because the government's role in the education market goes beyond regulation. Since education is mandatory and provided by the government for free, teacher wages may not be fully responsive to supply pressures and demand remains high regardless of quality. These factors lead to teacher shortages and the existence of a "black market" in which some schools are forced to hire unlicensed teachers to fill vacancies (García and Weiss, 2019). Training requirements may reduce the supply of certified teachers and increase schools' reliance on teachers who have little or no training, which can undermine the licensing laws even if training is effective.

Our analysis focuses on a reform in the state of Texas that significantly reduced the amount of pedagogical training required to become a certified teacher. In the face of teacher shortages, Texas enacted policies in 1999 and 2001 that encouraged flexibility in the approval of educator preparation programs (EPPs) and eliminated the requirement that programs include student teaching. Under these new guidelines, for-profit companies entered the EPP market in 2001 and grew to be the dominant provider of teacher training; as of 2020, more than half of all newly-certified teachers in Texas fulfill their training requirements at for-profit EPPs (see Figure 1). Compared to standard and other alternative EPPs, which are typically affiliated with universities, for-profit programs tend to be cheaper and much shorter in duration. For-profit EPPs are frequently criticized by education researchers and the media for lax admission standards and low-quality training, which has led the Texas State Board for Educator Certification to place some for-profit programs on probation.[1] Despite these concerns and a lack of rigorous evidence on their overall effectiveness, ten states have followed Texas' lead in approving for-profit teacher training programs as of 2019 (King and Yin, 2022).

We use detailed Texas administrative data that include measures of worker supply and quality that are hard to measure in many occupational licensing papers. Our data include the universe of teacher certifications, public school teachers, and public school students in Texas from 1996–2019, with individual links across datasets. This allows us to examine how training requirements affect the pool of certified teachers, the number of employed teachers, and the number of teachers without proper certification. We compare differences in teaching quality across EPPs by measuring teacher turnover and value-added (Rockoff, 2004; Rivkin et al., 2005; Kane and Staiger, 2008; Chetty et al., 2014a). Importantly, we also examine the *net* impacts of these supply and quality effects on student performance on standardized state exams—a key measure of product quality in this setting.

We begin by showing that the reduction in teacher training requirements significantly increased the number of certified teachers and reduced schools' reliance on uncertified teachers. The annual number of newly-certified teachers roughly doubled in the seven years following the initial entry of for-profit EPPs into the market in 2001. Estimates from a cross-state difference-in-differences (DiD) model show that the Texas policy increased the number of certified teachers per capita by roughly 40 percent relative to other states—an effect that persisted up through 2019. We do not find significant impacts of the policy on the number of *employed* teachers or on average wages—consistent with evidence that teaching vacancies rarely go unfilled—but the policy sharply reduced the fraction of teachers who were uncertified. Further, teachers who went through a for-profit EPP are more diverse than standard-trained teachers as measured by gender, race, and college major, suggesting that the lower-cost training routes brought new types of certified teachers into the profession.

We next show that teachers from for-profit EPPs are lower quality than standard-trained teachers as measured by both turnover rates and value-added, but they are better on both of these metrics than uncertified teachers. For-profit-trained teachers are 10 percentage points more likely to leave the profession within five years than standard-trained teachers, and their value-added on math and English language arts (ELA) tests is 0.01–0.03 standard deviations (SDs) lower than teachers from other certification routes. However, teachers who begin their careers without any certification have much higher turnover rates than for-profit-trained teachers, and their value-added is up to 0.1 SDs lower in math.

Our final analysis uses two sources of variation to examine the net implications of these

[1] See, for example, "Too big to fail? Texas' largest teacher prep program riddled with problems, state finds," *The Dallas Morning News*, April 20, 2022.

supply and quality effects for student achievement. First, we define schools with more and less exposure to the EPP policy changes based on grade level and geographic proximity to for-profit EPPs. Second, we identify a set of open teaching vacancies by using the departure of an experienced teacher as a shock to a school's labor supply.[2] Our main specification is a regression discontinuity differences-in-differences (RD-DiD) design that combines DiD variation in policy exposure across schools and years with RD regressions that identify sharp changes in outcomes following teacher departures. Intuitively, our RD-DiD strategy asks how the EPP policy changed the types of teachers who were hired to fill open vacancies in schools with differential policy exposure, and how the resulting hires affected student outcomes.

We find that, on net, the reduction in teacher training requirements in Texas neither raised nor lowered student achievement. Our RD-DiD analysis confirms our above finding that schools with more policy exposure became more likely to hire for-profit trained teachers to fill teaching vacancies and less likely to rely on uncertified teachers. Yet the effects of teacher departures on student math and ELA scores did not change differentially across schools with more and less policy exposure; we find positive but statistically insignificant test score effects across a range of RD-DiD models. Our average point estimate suggests that a 10pp increase in the share of teachers from for-profit EPPs led to a 0.047 SD increase in test scores, although we cannot rule out moderately-sized negative effects. Yet, as back-of-the-envelope calculations confirm, a null effect on student achievement is exactly what one would expect because the influx of lower value-added teachers from for-profit EPPs was offset by a reduction in uncertified teachers with even lower value-added.

Taken together, our findings show that the Texas EPP reform benefited teachers by reducing the burden of training without reducing the quality of education for students. Unlike many papers that examine occupational licensing in more privatized markets, we find no effects of reduced training requirements on total employment or average wages. Thus the main effect of the Texas policy for teachers was a reduction in the time and monetary costs of training — a fact that we confirm with data on EPP programming and pricing. Licensing has minimal effects on demand or prices in our setting because education is free and mandatory, so the null results on student achievement are a compelling summary of the impacts of the Texas policy for consumers. Thus while a formal welfare analysis is beyond the scope of our paper, our broad set of findings suggests that the reduction in teacher training requirements was a net positive for workers and consumers as a whole.

Our paper contributes to the literature on occupational licensing by providing compelling

[2]Our teacher departure strategy is analogous to research that uses worker deaths as a shock to firm labor supply to ask how firms find substitutes for their workers (e.g., Jäger and Heining, 2022), and it follows Chetty et al. (2014a) in using teacher departures to identify impacts of teachers on students.

and comprehensive evidence on the effects of teacher licensing requirements. Due to data limitations, many papers can only examine the partial impacts of licensing policies on specific outcomes such as wages and employment (e.g., Kleiner and Krueger, 2010, 2013; Thornton and Timmons, 2013; Blair and Chung, 2019; Dodini, 2023) and product quality (e.g., Kleiner and Kudrle, 2000; Kleiner, 2006; Anderson et al., 2020). Kleiner and Soltas (2023) provide a comprehensive analysis of the impacts of occupational licensing on worker and consumer welfare, although they do not observe product quality and prices and instead infer them from wages and employment combined with model assumptions.[3] In many of these papers, identification comes from cross-sectional variation in licensing policies—either across states or between licensed and unlicensed workers. We use reduced-form policy variation and administrative data to show how licensing impacts outcomes that are often hard to measure simultaneously, including product quality (student test scores), the potential supply of licensed workers (teacher certifications), and the existence of a "black market" (uncertified teachers). We find that it is important to examine both licensed workers and other workers who may be potential substitutes, as the effects of licensing on the two groups may offset. Consumers often have multiple options for the same service in markets with licensed occupations (e.g., licensed vs. unlicensed cosmetologists), which may help explain why studies often find minimal effects of licensing on both worker supply (DePasquale et al., 2016; Kleiner et al., 2016; Law and Marks, 2017; Zapletal, 2019) and product quality (Kleiner et al., 2016; Markowitz et al., 2017; Barrios, 2022; Farronato et al., 2024).

While there is a large body of research on teacher licensing policies (Jackson et al., 2014), our paper is unique in providing clear evidence on *both* their supply and quality effects. Previous work has found that teachers' certification pathways and license exam scores are at most modestly related to their value-added (Aaronson et al., 2007; Clotfelter et al., 2007b, 2010; Goldhaber, 2007; Kane et al., 2008; Chingos and Peterson, 2011; Shuls and Trivitt, 2015; Hendricks, 2016; Goldhaber et al., 2017; von Hippel and Bellows, 2018). There is little research on for-profit-trained teachers specifically, although some recent studies in education have found that they have higher turnover and lower value-added than teachers from standard programs (Marder et al., 2022; Templeton et al., 2022a). This work provides only a limited picture of the desirability of licensing policies because it treats the licensing landscape as fixed, effectively ignoring any supply-side effects. As we show, understanding supply effects is crucial because the creation of *new* certification pathways can mitigate teacher shortages and improve certification alignment even if the new programs produce lower-quality teachers.

[3] Hotz and Xiao (2011) examine the supply and quality effects of state regulation of child care services—including worker educational and training requirements—but they measure quality using business accreditation rather than child outcomes.

Our paper is closer in spirit to a smaller literature that examines the supply side effects of teacher licensing policies, but our administrative data on student test scores allows us to provide more convincing evidence on quality effects.[4] Angrist and Guryan (2008), Larsen et al. (2020), and Chung and Zou (2025) find mixed evidence on how the introduction of teacher testing requirements and other licensing policies affects the supply and quality of teachers. In each of these papers, the primary measure of teacher quality is the selectivity of the college that the teacher attended. Many observable teacher characteristics—including college selectivity—are at best weakly predictive of teachers' effects on student achievement (Clotfelter et al., 2006, 2007a; Kane et al., 2008; Rockoff et al., 2011; Jacob et al., 2018). Thus papers that cannot directly measure teacher value-added give an incomplete view of the quality impacts of licensing policies.[5] We find clear evidence that teachers who attend programs with laxer standards are lower quality as measured by value-added, but the supply side benefits of these programs offset the quality differences.

2 Texas Teacher Certification and Policy Background

2.1 Certification routes. To become a public school teacher in Texas, individuals must hold a bachelor's degree and be certified.[6] Like in many other states, the certification process in Texas requires that individuals complete an educator preparation program (EPP), which offers training on effective teaching practices, and pass both pedagogy and content-specific licensing exams.

In Texas, as in many states, there are two possible routes to earn a teaching certificate. In what is often called the *standard* certification route, prospective teachers fulfill the EPP training requirements at a university while completing a bachelor's degree (Agency, 2022a). The second route, known as *alternative* teacher certification, is designed for individuals who want a career change after they have already graduated from college. Alternative EPPs have existed in Texas since 1984, and until 2001, they were run by a variety of public and non-profit institutions, including universities, independent school districts, and state-legislated

[4] More broadly, our paper contributes to work that shows how various policies and incentives affect teacher supply (e.g., Hoxby and Leigh, 2004; Bacolod, 2007; Rothstein, 2015; Nagler et al., 2020; Deneault, 2024; Law et al., 2023; Orellana and Winters, 2023; Johnston, 2025)

[5] Chung and Zou (2025) also find no overall impact of stricter teacher entry requirements on student achievement using test scores from the National Assessment of Educational Progress (NAEP), but they cannot directly observe how teachers from different licensing pathways contribute to this overall effect. Kleiner and Petree (1988) find no clear evidence that teacher licensing policy impacts student achievement, but their analysis relies on cross-sectional variation in the prevalence of licensing across states.

[6] See sections TEC§21.003 and TEC§21.044. Since 2008, Texas also requires fingerprinting and background checks for the full certification (Agency, 2022c).

service organizations called Education Service Centers (Region 13 ESC, 2023). The content of standard and alternative programs is regulated by Texas state law, and the State Board for Educator Certification (SBEC) is in charge of EPP accreditation.

The timeline for coursework and requirements for completing the teacher training portion of certification differ across standard and alternative routes. In the standard route, students take a large number of courses on pedagogy while they are in college as part of their major requirements, and they usually complete unpaid student teaching positions. Individuals in the standard route typically take the content and pedagogy licensing exams before they begin teaching. If they pass these exams, they become eligible to teach under Texas' standard certificate. In alternative programs, individuals typically begin with a more abbreviated set of coursework and then take the content licensing exam. If they pass the content exam, individuals receive a 1-year probationary license and are eligible to become a full-time teacher of record with pay. During their first year as a teacher—which is often called the "internship" period—individuals take additional courses in their spare time to complete the EPP training requirements, and they must also pass the pedagogy licensing exam. If they fulfill all these requirements, individuals receive a standard certificate, typically in time for their second year as a teacher of record (Agency, 2022b).

Despite the regulatory certification requirements, there are instances when uncertified teachers fill classrooms. School districts that have difficulty hiring can gain approval to issue emergency teaching permits from the Commissioner of Education (Templeton et al., 2022b).[7] With state approval via this permit, school districts are legally allowed to hire a teacher whom is not certified but whom the district feels is nevertheless qualified to teach. There are also instances where schools may hire uncertified teachers and not get the appropriate approvals (Templeton et al., 2022b).

2.2 The growth of for-profit EPPs. Motivated by growing teacher shortages, Texas enacted several unique policies around the turn of the millennium to expand pathways to certification. In 1999, the 76th state legislature passed House Bill 714, which modified the sections of the Texas Administrative Code that deal with educator preparation and certification (Templeton and Horn, 2020). The new law was "designed to promote flexibility and creativity in the design of programs, including ... alternative routes to certification."[8] Most significantly, the legislature gave SBEC the authority to approve new EPP programs, and it prescribed that educator preparation "shall be delivered by institutions of higher education, regional education service centers, public school districts, or *other entities*" (emphasis

[7] See TEC§21.055 for legal code. It also stipulates that permit holders need to have a bachelor's degree.
[8] Texas Administrative Code Title 19, Part 7 §228.1(b) adopted to be effective July 11, 1999.

added).[9] Another influential change occurred in 2001 when the SBEC eliminated a requirement that preparation programs include "student contact hours" (May et al., 2003; Guthery and Bailes, 2023). This amendment meant that EPPs could offer programs that did not require any student teaching or other field experience prior to earning a certificate.

These policy changes opened the door for for-profit EPPs to enter the teacher certification market with a new business model.[10] Appendix Table A1 shows that 33 for-profit EPPs began operating between 2001 and 2011. Many of these programs are headquartered in major metropolitan areas, including Houston, Dallas, San Antonio, and Austin, but there is also a concentration of for-profit EPPs in the Rio Grande Valley.

For-profit EPPs have grown to dominate Texas' teacher certification market. Panel A of Figure 1 shows that the for-profit share of initial teacher certifications—i.e., individuals earning their first teaching certificate—grew from zero percent in 2000 to 30 percent by 2010. As of 2019, for-profit EPPs are the most popular certification route, accounting for roughly 50 percent of all newly-certified Texas teachers.

What made for-profit EPPs successful? For-profits often market themselves as offering a fast and easy route to a teaching career. Historically, alternative EPPs required in-person coursework, which limited flexibility in when and where training occurred. In 2003, iteachTexas became the first EPP to offer fully-online coursework, and A+ Texas Teachers had a similar business model when it opened two years later (Appendix Table A2). Data from the Department of Education's Title II reports (Appendix Table A3) show that for-profit EPPs tend to require fewer hours of training than other EPPs, often requiring the bare minimum that is allowed by regulation. For-profit EPPs also tend to have lower college GPA requirements for the students they admit.

For-profit EPPs also beat many of their competitors on price. Figure 2 shows average fees charged by for-profit and other alternative EPPs using information that we collected from historical websites (Appendix Table A2). During the 1999–2007 period for which we could find data, for-profit EPPs charged roughly \$6,400 on average (in 2024 dollars), whereas the average cost at other alternative EPPs was roughly \$7,000. The share of the total fee that is due up front (e.g., application and initial training fees) is also lower at for-profit EPPs; this may reduce credit constraints by allowing individuals to pay most of the program costs after they start earning a paycheck from the teaching internship. Notably, the average real fees at both for-profit and other alternative EPPs were roughly \$1,500 *lower* in 2024 than they were in the 2000s, and for-profits still offered lower average prices in 2024. This decline

[9] Texas Administrative Code Title 19, Part 7 §228.20(b) adopted to be effective July 11, 1999.

[10] We note that for-profit EPPs are different from for-profit colleges (Deming et al., 2012), both in ownership and in kind. For-profit EPPs help individuals fulfill state teacher certification requirements; they do not offer associate's degrees, bachelor's degrees, or other occupational certificates.

in market prices likely reflects the competitive pressure from for-profit entry.

While for-profit EPPs dominate the market, there are significant concerns about the quality of their training. For-profit EPPs have significantly higher faculty to student ratios than other EPPs (Appendix Table A3). Further, for-profit EPPs receive frequent scrutiny from regulators for failing to comply with accreditation standards. In 2022, for example, the Texas Education Agency recommended that the largest for-profit, Teachers for Tomorrow, be put on probation due to misleading marketing, insufficient mentorship, and unproven coursework.[11] These potential concerns motivate our analysis of the overall effects of the reduction in teacher training requirements.

3 Data

We briefly describe the datasets we compiled below. Appendix B provides further details on sample construction and variable creation for all sources.

3.1 Texas administrative data. We use administrative data from four Texas agencies:

- **Texas Education Agency (TEA)**. TEA provides data on all public school teachers in Texas from 1996–2019. These data include each teacher's demographics, salary, years of teaching experience, school of employment, and full-time equivalent (FTE) years associated with their teaching grade(s) and subject(s). We also use TEA data on grade 3–8 state standardized tests from 1994–2019, which include student test scores and demographics.[12] We can connect student test scores to small groups of teachers at the school/grade/subject level for all years from 1996–2019. From 2012–2019, we also observe classroom identifiers that allow us to connect students to individual teachers and compute teacher value-added estimates.

- **Texas State Board for Educator Certification (SBEC)**. SBEC provides data on all teacher certificates earned in Texas, with records that date back to the 1950s. This data includes the issuance date, effective date, expiration date, subjects, and grade levels for which each certificate is valid. Importantly, the SBEC data also includes the name of the EPP associated with each certificate, as well as information on EPP

[11] See: "Texas' largest teacher prep program faces probation after state finds continued problems" *The Dallas Morning News*, April 25, 2022. Teachers for Tomorrow is currently operating under a monitoring agreement with the SBEC: "Texas' largest educator preparation program will be monitored" *The Dallas Morning News*, September 19, 2024.

[12] Texas had three testing regimes during this time period: Texas Assessment of Academic Skills (TAAS, 1994–2002), Texas Assessment of Academic Skills (TAKS, 2003–2011), and State of Texas Assessments of Academic Readiness (STAAR, 2012–2019).

locations and types. We classify teachers into certification pathways using the EPP associated with their first teaching certificate. We also use the data on all certificates to measure whether teachers are appropriately certified.[13]

- **Texas Higher Education Coordinating Board (THECB)**. We use THECB data on all bachelor's degree graduates from public (1992–2019) and private colleges (2003–2019) in Texas. We observe each graduate's college, major, and graduation year.
- **Texas Workforce Commission (TWC).** TWC provides administrative earnings records for all individuals working at registered firms in Texas from 1992–2021. We use this data to measure individuals' annual earnings in the years before and after they transition into teaching careers.

We accessed the data via the University of Texas at Dallas Education Research Center (ERC), which contains unique personal identifiers that allow us to connect all datasets at the individual level. With these links we can follow teachers from their teacher certification programs into teaching jobs. We also observe college majors and the teacher's own eighth grade test scores for younger teachers who went to school in Texas. Throughout the paper, we restrict our analysis to teachers and students at Independent School Districts (ISDs) to focus on a stable set of schools during the long time period of our data.[14]

3.2 National Data. We also compare Texas to other U.S. states using a variety of state-level public data sources. Data from the Department of Education's Title II reports provide counts of the number of teacher preparation program completers and the number of initial certifications by state and year. Information on the number of employed teachers comes from the Department of Education's Common Core data, which we accessed through the National Center for Education Statistics (NCES). We also use state-level estimates from the NCES' Schools and Staffing Survey (SASS) and National Teacher and Principal Survey (NTPS) to measure teacher demographic characteristics, subjective measures of teacher preparation, and schools' assessment of the difficulty in filling teacher vacancies.

[13] For some appendix tables, we also use SBEC data on teacher certification exam performance (1986–2019) and EPP admissions/completions (2012–2019).

[14] The restriction to ISDs excludes charter districts, which comprise roughly six percent of total public school enrollment in the 2018–2019 academic year. Many Texas charter schools began operating after for-profit EPPs had already entered the market, and charters also have more flexibility in hiring teachers that do not meet the state certification requirements.

4 Teacher supply

4.1 Effects on the quantity of teachers. We begin our empirical analysis by examining how the EPP policy changes impacted the supply of teachers in Texas. A first indication of such supply effects is that the total number of newly-certified teachers increased dramatically following the 2001 policy change. Panel B of Figure 1 shows that the total number of initial teacher certifications—i.e., individuals receiving their first teaching certificate—increased from roughly 14,000 in 2000 to about 28,000 by 2007. In the first few years after the policy change, the growth in initial certifications was driven primarily by other alternative programs, but for-profit EPPs grew rapidly over the 2000s and comprised a majority of the alternative certification market by the end of the decade.[15] Around 2015, the annual counts of newly-certified teachers from standard and other alternative programs were similar or slightly below their 2000 levels, but the *overall* number of certifications was significantly higher due to for-profit EPPs producing roughly 10,000 newly-certified teachers per year.

To confirm that this growth in teacher certifications is not due to aggregate trends, we compare Texas to other states using the national datasets described in Section 3.2. This analysis is motivated by the fact that Texas is an extreme outlier in its use of for-profit EPPs.[16] We exploit this variation in a simple difference-in-differences (DiD) model,

$$Y_{st} = \gamma_s + \gamma_t + \beta[\text{Texas}_s \times \text{Post}_t] + \mathbf{X}'_{st}\theta + \epsilon_{st}, \tag{1}$$

where Y_{st} is a teacher supply outcome measured in state s and year t, γ_s and γ_t are state and year fixed effects, and the variable of interest is the interaction between an indicator for Texas (Texas_s) and an indicator for years after the EPP policy change (Post_t).[17] Our outcomes are measured from the 1990s up through 2019, with the range depending on the years of available data.[18] We report standard errors clustered at the state level. Statistical significance levels (denoted by stars) are computed following the method in Conley and Taber

[15]Other alternative programs are mostly run by colleges and independent schools districts (see Section 2). These institutions likely had an initial advantage in supplying the market because they could build on existing certification programs.

[16]In the 2018–2019 academic year, the for-profit share of total EPP enrollment was 63.9 percent in Texas; all other states had for-profit shares below 9.9 percent, and 39 states had no for-profit programs (King and Yin, 2022).

[17]We define Post_t as indicator for 2001 or later when we examine teacher preparation program outcomes and an indicator for 2002 or later when we examine outcomes for schools or employed teachers. We weight observations by population size using the U.S. Census' intercensal estimates of the number of 18–65 year olds in each state and year. For each outcome, we restrict to states for which the outcome is measured in all years of the data.

[18]Because we have only one year of pre-treatment data for some outcomes, we report raw data instead of event studies.

(2011) for settings with only one treated cluster.

Our setting features a long post-treatment period during which many other state and national education policies were passed, so we supplement our DiD specification with controls for other education policies, $\mathbf{X}_{st}$. We follow Dee and Jacob (2011) in including controls for years in and after each state had a consequential accountability policy. Texas enacted accountability prior to the national No Child Left Behind (NCLB) Act of 2002, so these controls implicitly compare Texas to other states with pre-NCLB accountability policies. We also follow Kraft et al. (2020) in including eleven binary variables to control for other teacher accountability policies (see their Appendix Table A1). These include policy changes related to teacher evaluation, teacher testing, tenure, collective bargaining, Race to the Top grants, and the implementation of Common Core standards.

We find that the number of newly-certified teachers increased significantly in Texas relative to other states over this two decade period. Panel A of Table 1 shows that the number of EPP completers in Texas increased by 3.8 people per 10,000 residents relative to other states (column D). These coefficients represent a roughly 40 percent increase relative to the value of 9.5 EPP completers per 10,000 Texas residents in 2000. The increase in EPP completers was driven entirely by alternative programs, and we find a similar impact on the number of initial certifications per capita. These point estimates do not change significantly when we layer in controls for accountability (column E) and other teacher policies (column F). The estimates for EPP completers are statistically significant at $p < 0.1$ using the Conley and Taber (2011) method, although we lose precision in the specification with all policy controls. Figure 3 shows these results graphically by plotting trends in total EPP completers (Panel A) and alternative EPP completers (Panel B) for Texas and other states. The number of EPP completers per capita in Texas was similar to that in other states in 2000, but it grew rapidly during the 2000s and remained roughly 40 percent higher throughout the 2010s.

Although these results suggest that for-profit EPPs helped to boost the supply of *certified* teachers in Texas, this did not increase the number of *employed* teachers or reduce their average wages. Our DiD estimates in Table 1 and Figure 3 show little change to employment resulting from increased certification. We also find a small and statistically insignificant one percent increase in log annual teaching earnings in Texas relative to other states, suggesting little change in wages resulting from increased employment.[19]

While the growth of for-profits did not impact the quantity of employed teachers, survey data from the SASS/NTPS suggest that for-profit EPPs made it easier for Texas schools to fill teaching vacancies. In Panel C of Table 1, our outcome variables are the proportion

[19]This increase may be consistent with certified teachers being paid on average slightly more than uncertified teachers they may replace as uncertified teachers typically work less than full-time.

of schools reporting that it was very difficult or that they were unable to fill vacancies in five teaching areas: elementary, math, English, English as a Second Language (ESL), and special education. In 2000, a large fraction of Texas schools reported difficulty hiring in these areas, especially in math (41 percent), ESL (50 percent), and special education (35 percent). Our DiD estimates suggest that the growth of for-profits made it easier for schools to fill vacancies, with estimates in all five areas ranging from -3pp to -18pp. These coefficients are larger relative to the pre-policy means in Texas, but they are statistically insignificant because these variables come from only four waves of SASS/NTPS surveys. We take this to be suggestive evidence of a reduction in difficulty hiring.

Our finding that for-profits increased the supply of certified teachers but not the number of employed teachers is consistent with anecdotal evidence on how school districts address teaching shortages. A 2004 report by the Texas SBEC states that "very few teaching positions have ever been found to be left unfilled" because "[d]istricts simply cannot and do not leave classes of students without teachers" (Herbert and Ramsay, 2004). Instead, the report notes that schools use a variety of approaches to fill hard-to-staff positions, including reassigning teachers to subjects in which they have not been trained.

Consistent with this anecdotal evidence, we find that the growth of alternatively-certified teachers in Texas reduced schools' reliance on uncertified teachers. Figure 4 displays estimates of the share of all teachers and first-year teachers who were uncertified in each year. We find that the uncertified share declined significantly during the 2000s, with a particularly striking drop among first-year teachers (from above 30 percent in 2000 to around four percent by 2010). Using SASS/NTPS data, we find that the share of teachers with alternative certification increased by 17.3pp in Texas relative to other states, while the share of teachers who report that they are not currently certified fell by 2.2pp in Texas relative to other states (Table 1, Panel D). These estimates are robust to other policy controls, although the uncertified coefficient is not statistically significant due to a small sample size. These long-term trends suggest that the primary benefit of for-profit EPPs may have been to reduce districts' reliance on uncertified teachers, as we investigate further in Section 6.

4.2 Characteristics of new teachers. What types of people become teachers through for-profit EPPs? To answer this, Table 2 shows summary statistics for 2012–2019 first-year teachers in Texas public schools by certification route.[20] For reference, column (A) shows averages for all individuals who graduated with a bachelor's degree from a Texas university in these years. Notably, our sample also includes nearly 10,000 individuals who did not have

[20]Appendix Table A5 presents summary statistics by certification route for first-year teachers in the years prior to the EPP policy changes (1996–2001). Although we do not observe test scores for these teachers, we find similar differences in demographic traits and college majors across certification route, suggesting that the findings in Table 2 may also characterize the pre-policy period.

any teaching certificate by November 1st of their first teaching year (column F).

Teachers who attended a for-profit EPP are more diverse than standard-trained teachers in terms of demographics, college majors, and teaching grades. For-profit-trained teachers are 16pp more likely to be male and 8pp less likely to be white than teachers from standard EPPs. Eighty-five percent of teachers from standard EPPs majored in two fields commonly associated with teacher training—Interdisciplinary and Humanities—as compared with only 34 percent of for-profit-trained teachers. Teachers from for-profit EPPs are much more likely to have degrees in Business, Communication, Social Sciences, and STEM and are thus more representative of college graduates as a whole. More than half of standard-trained teachers teach in elementary school, while teachers from for-profit EPPs are more likely to teach middle and high school grades. These grades tend to be more difficult for schools to staff, which may explain why for-profit-trained teachers earn roughly \$2,000 more in their first year of teaching than standard-trained teachers. Notably, teaching leads to a significant increase in pay for individuals from all certification routes, with annual earnings rising from below \$20,000 in the year prior to teaching to above \$45,000 in their first year teaching.

Despite these differences, individuals from standard and for-profit EPPs have similar levels of academic achievement. On average, teachers from both certification routes scored between 0.55–0.61 SDs on eighth grade math and English language arts (ELA) exams. Both for-profit- and standard-trained teachers have slightly lower average eighth grade math scores than the typical college graduate in Texas (0.69 SDs), and they have lower scores in both subjects than first-year teachers from other alternative programs (0.70–0.73 SDs). Notably, teachers without any certification scored approximately 0.3 SDs lower than for-profit trained teachers on both math and ELA exams.

Although for-profit and standard teachers have similar average achievement, Texas policymakers have raised significant concerns about the quality of training in for-profit programs. We explore the relationship between certification route and teaching quality more comprehensively in the next section.

5 Teacher quality

5.1 Turnover. To explore how teaching quality varies across certification pathways, we begin by examining teacher turnover—a key outcome of interest for school administrators given its negative relationship with student learning (Hanushek et al., 2016; Carver-Thomas and Darling-Hammond, 2017). Alternative certification routes target individuals who are transitioning to teaching from other occupations, so they may attract people who are less committed to the profession than those who prepared for teaching careers during college. To explore this, Figure 5 shows the average full-time equivalent (FTE) teaching years in Texas public schools for teachers who began their careers in 2012–2019 (Panel A) and 1996–2001 (Panel B). We show the average FTE in the sample over the next five years, where teachers who are no longer working in any Texas public school are counted as having zero FTE. We plot outcomes separately by certification route, defined by the EPP of individuals' first teaching certificate. The uncertified category includes all teachers who were not certified in their first year (regardless of whether or not they subsequently became certified).

Teachers from for-profit EPPs have higher turnover than standard-trained teachers but lower turnover than uncertified teachers. In the 2012–2019 period, only 60 percent of for-profit-trained teachers were still working in Texas public schools five years after beginning their careers as compared with 70 percent of standard-trained teachers. For-profit and other alternative EPPs produce teachers with similar turnover rates, but less than 40 percent of teachers who were initially uncertified remained in teaching after five years. Uncertified teachers were less likely to work full time in their first year and have particularly high departure rates in the second year. This suggests that schools use uncertified teachers to fill short-term teaching needs. Uncertified teachers also had much higher turnover relative to certified teachers in the pre-policy years (1996–2001).

Differences in turnover rates by certification route are not explained by teacher demographics or characteristics of their first teaching job. In Table 3, we regress their FTE five years after beginning teaching (including zeroes) on certification pathway dummies with standard EPPs as the omitted group. We layer in controls for the teacher's district, school, grade-level, and subject in their first year teaching, as well as other demographic variables including race/ethnicity, gender, and the teacher's own eighth grade test scores when available. School, grade-level, and subject are endogenous choices for teachers that could be related to turnover, so these regressions are descriptive and not causal. The coefficient on for-profit-trained teachers is nearly identical across all empirical models, reflecting 10pp higher turnover than standard-trained teachers. While the demographic and initial teaching job controls attenuate the coefficient for uncertified teachers, even with these controls they are still 20pp more likely to depart within five years relative to standard-trained teachers. Notably, higher 8th grade math and ELA scores are associated with an increased likelihood of leaving the profession, suggesting that higher-ability individuals might find outside opportunities more attractive.

5.2 Value-added. Our second measure of teaching quality is value-added on standardized exams. Value-added is the standard measure of teacher quality in the economics of education because it is highly predictive of student learning and long-run outcomes (Kane and Staiger, 2008; Chetty et al., 2014b; Koedel and Rockoff, 2015). We follow the methodology in Chetty et al. (2014a) by regressing student grade 4–8 math and ELA test scores on a large vector of student-, classroom-, and school-level controls, including lagged test scores. Figure 6 plots the average of these residuals by year of teaching experience and certification route (defined as in Figure 5).[21] Table 4 follows Kane et al. (2008) in regressing student test scores on the same vector of control variables plus dummies for the teacher's certification route, with standard EPPs as the omitted group and "no certification" denoting teachers who were not yet certified by the current teaching year. We can only compute value-added for teachers whose students take standardized exams and for the years in which our data includes classroom identifiers, so this analysis is limited to teachers who teach math or ELA in grades 4–8 during 2012–2019. Appendix B.4 provides details on our methods for calculating value-added.

Math value-added for teachers from for-profit EPPs is lower than that for teachers from standard or other alternative EPPs, but uncertified teachers have the lowest value-added. Figure 6 demonstrates that all teachers generally improve in value-added early in their careers, consistent with on-the-job learning and previous findings (Wiswall, 2013; Papay and Kraft, 2015). But for-profit-trained teachers have persistently lower math value-added than standard-trained teachers across the first six years of their careers, and value-added for uncertified teachers is even lower. Estimates from the benchmark value-added specification in Table 4 (column C) show that math value-added is 0.02 SDs lower for for-profit-trained teachers and 0.10 SDs lower for uncertified teachers relative to standard-trained teachers. We find similar patterns but smaller magnitudes for ELA value-added, with estimates of -0.006 SDs for for-profit-trained teachers and -0.03 SDs for uncertified teachers. We find negative and similarly-sized point estimates for the quality of uncertified teachers in 1996–2001 using specifications that measure teacher characteristics at the school $\times$ grade $\times$ year level rather than at the classroom level, suggesting that uncertified teachers also had much lower-value added in the pre-policy years (see Appendix Table A8).

Our preferred estimates in Table 4 do not control for experience level, though we present the same regressions controlling additionally for fully saturated experience levels in Appendix Table A6. Regressions without experience controls are relevant for policy evaluation because they allow differences in turnover rates across EPPs to matter for student achievement, and experience is highly correlated with value-added. While estimates in Appendix Table A6

[21] In Figure 6, we use test score residuals rather than the final Chetty et al. (2014a) value-added estimates with Bayesian shrinkage because we are particularly interested in teacher quality in the first year. Since many teachers improve significantly in their first few years, the shrinkage estimator tends to overestimate value-added in the first year because it uses test score residuals from other years to predict value-added in a given year. Further, the shrinkage estimator is not necessary in our case because we are interested in average value-added differences by certification route rather than value-added estimates for individual teachers. Using test score residuals also allows us to include teachers who only teach for a single year.

are predictably smaller than our benchmark estimates, we still find statistically significant differences in teacher quality among for-profit and uncertified teachers relative to standard-trained teachers.

Two pieces of evidence suggest that the variation in value-added across certification routes could be driven more by selection than by differences in the quality of training. First, if for-profit-trained and uncertified teachers have lower value-added because they receive low-quality or no training, one would expect the gap between their value-added to decrease with time. Yet the differences in value-added across certification routes are roughly parallel over the first six years of the teaching career, even among those who have worked the same number of years (Figure 6 and Appendix Figure A2).

Second, the variation in value-added across certification routes decreases significantly when we add controls for each teacher's *total* length of time in the teaching profession. This specification assumes that length of time in the profession is a proxy for a teacher's desire to teach and, consequently, their effort devoted to the job. If we infer that length of time in the profession is at least related to commitment and effort, then comparing teachers across certification pathways with the same length of time in the profession is akin to comparing them across similar levels of effort. Then we view any remaining differences in value-added as more likely to result from non-selection differences, such as training. With these additional controls, the math coefficient for teachers from for-profit EPPs falls to -0.006 SDs (Appendix Table A7), which is 70 percent smaller than benchmark estimate from Table 4 (-0.02 SDs). The differences in ELA value-added across certification routes disappear with controls for total time in the profession. Taken together, these results provide suggestive evidence that selection is more important than training in explaining the quality differences across certification routes.

Although the differences in value-added across certification routes are relatively modest, they are meaningful in dollar terms given the large number of Texas students who have for-profit-trained teachers. Using the estimate from Chetty et al. (2014b) on how value-added affects students' long-term earnings, our point estimate for the math value-added of for-profit-trained teachers (-0.02 SDs) suggests that the average for-profit-trained teacher reduces the net present value of student earnings by approximately \$16,000 per class relative to standard-trained teachers.[22] This number is sizable both as a proportion of average teacher pay (Table 2) and when considering that for-profit EPPs produce more than half of all newly-certified teachers for Texas' huge public school system. The difference in value-added between uncertified and standard teachers is five times larger in magnitude and is

[22]Chetty et al. (2014b) find that a two standard deviation increase in teacher value-added (roughly 0.30 SDs of student scores) increases the net present value of classroom earnings by \$250,000.

much larger than that found by Kane et al. (2008) for uncertified teachers in New York City, which may reflect the greater diversity of schools across Texas.

6 Overall impacts on teacher composition and student achievement

Our results so far show that Texas' relaxation of teacher training requirements had both upsides and downsides for student achievement. On the one hand, the policy changes increased the supply of certified teachers, which reduced schools' reliance on uncertified teachers. On the other hand, the new policy led to the growth of a large for-profit sector, which produced teachers with lower value-added than those from standard and other alternative programs. In this section, we develop an identification strategy that allows us to examine the net impact of these quality and supply mechanisms on student achievement.

A natural starting point for evaluating the student impacts of the EPP reform is a difference-in-differences (DiD) strategy that compares schools with greater and lesser exposure to the growth of for-profit EPPs. Because the policy was implemented statewide, identification must rely on cross-school variation in exposure. As such, we define schools as more affected by the EPP policy if they are located in a county where a for-profit EPP opened its headquarters. The geographic proximity to for-profit EPPs was important due to advertising, in person training requirements, and teachers' desire to stay local.[23] This geographic concentration meant that school districts that were close to a for-profit EPP's headquarters were likely to have a larger pool of for-profit program completers to consider for teaching positions.

Appendix Figures C1–C2 present standard event study graphs that show how outcomes evolved at schools in counties with a for-profit EPP opening to those in counties without for-profit EPPs. (See Appendix Section C for details on this analysis.) Our treated group includes schools in 12 counties that had an initial for-profit EPP opening between 2001 and 2009 (Appendix Table A9). Our control group includes 191 "never treated" counties that do not border a county with a for-profit EPP. These event studies show that the opening of for-profit EPPs led to an increase in the share of teachers with a for-profit certification and, if anything, *increases* in student achievement on standardized math and ELA exams.

We are reluctant to interpret these findings strongly, however, because the timing and location of for-profit EPP openings is unlikely to be as good as random. Although we do not find strong evidence that for-profit EPP openings are correlated with changes in observable

[23] For example, many for-profit EPPs send employees to observe and provide feedback to the candidate during their first year of teaching. See Reininger (2012) for preferences of teachers to remain local.

student characteristics, for-profit EPPs opened in areas with higher population growth rates, which may be indicative of trends in unobserved student ability (Appendix Figures C1–C2).

A more compelling empirical strategy requires another source of variation that nets out changes in population characteristics. Rather than relying on long-run comparisons in outcome levels, below we develop a new RD-DiD strategy that focuses on sharp staffing adjustments induced by teacher departures.[24] Specifically, we use RD regressions to estimate discontinuous changes in teacher composition and student achievement at the moment schools are forced replace a departing teacher, and then use DiD variation to examine how the impacts of teacher departures vary across more- and less-exposed schools. Our RD-DiD design allows us to difference out gradual demographic trends and isolate how the reform altered the marginal teacher hired. This approach substantially weakens the parallel trends requirement relative to a standard DiD design and provides a more credible framework for assessing the net effects of the policy on student achievement.

6.1 Teacher departures. We use the departure of experienced teachers as a shock to schools' labor supply. Intuitively, departures create open teaching vacancies, which allows us to ask how demand for new teachers affects student achievement under stringent or flexible training requirements. We define a *teacher departure* as an instance in which a math or ELA teacher with ten or more years of experience leaves a given school. To isolate large changes in teacher composition, we restrict our analysis to cases in which the departing teacher taught one-third or more of the students in a given school, grade, and subject in the year before their departure.[25]

To use all possible variation from teacher departures, we create a "stacked" dataset for each teacher departure from a given school, grade, and subject. We first collapse our individual-level data to the school/grade/subject/year level. We let s denote school/grade/-subject triplets and use t to denote years, so the variables in our collapsed dataset are mean teacher characteristics or mean student outcomes at the st level. A given school/grade/subject may experience multiple teacher departures during the period of our data. Thus we let y denote the year of a teacher departure, and we "stack" our collapsed dataset so that st observations occur multiple times for each departure. (We drop st observations for which there is no teacher departure.) Lastly, we let $\tau_{ty} = t - y$ denote years *relative* to the teacher departure, where $\tau_{ty} = 0$ is the first year in which the teacher is no longer in the school.

[24] Our strategy is inspired by related work that uses teacher arrivals and departures to examine the impact of teaching quality on student outcomes (e.g., Jackson, 2013; Chetty et al., 2014a).

[25] We focus on two subjects for middle schools (grades 6–8): math and ELA. For elementary school (grades 3–4), we use a single subject (the combination of math, ELA, science, social studies, and generic) since most elementary teachers teach all core subjects. We focus on departures of teachers with 10+ years of experience so that the departing teacher was trained in a very different EPP landscape than new teachers who might replace them. See Appendix B.5 for details on our definition of teacher departures.

Since teacher departures may be caused in part by school-specific trends in student achievement, we use an RD model to isolate sharp changes in teacher and student outcomes due to departures.[26] Specifically, we use our collapsed and stacked dataset to estimate a local linear RD regression:

$$Y_{st} = \beta \mathbf{1}\{\tau_{ty} \geq 0\} + \alpha \tau_{ty} + \psi \mathbf{1}\{\tau_{ty} \geq 0\}\tau_{ty} + \gamma_{sy} + \varepsilon_{sty} \quad \text{if } |\tau_{ty}| \leq h^Y. \tag{2}$$

The dependent variable, Y_{st} is an average teacher characteristic or student outcome at the school/grade/subject (s) and year (t) level. Our variable of interest is an indicator for years after the teacher departure, $\mathbf{1}\{\tau_{ty} \geq 0\}$. The running variable is years relative to the teacher departure, τ_{ty}, and we include an interaction between τ_{ty} and $\mathbf{1}\{\tau_{ty} \geq 0\}$. We include fixed effects for school/grade/subject/departure-year quadruplets, γ_{sy}, so that identification comes only from before and after variation within the same departure event. The regression includes years relative to departure, τ_{ty}, that are within the Calonico et al. (2019) RD bandwidth, h^Y, computed separately for each outcome Y, although our main results are robust to other bandwidths (Appendix Figure A8).[27] Standard errors are clustered at the school level to allow for correlation in outcomes within the same school (e.g., if schools ask teachers to switch grades following a departure). The coefficient of interest, β, estimates the projected change in average outcomes in the first year after the teacher departure ($\tau_{ty} = 0$). For example, if departing teachers have higher value-added on average than the teachers that replace them, we would find $\beta < 0$ for the outcome of student test scores.

6.2 Policy exposure. We next combine our departures with a DiD framework comparing teachers departing in schools more- or less-exposed to the for-profit training programs. To define treatment for our DiD, we estimate five binary measures of more-exposed (treated group) and less-exposed (control group) schools:

1. **Middle schools vs. elementary schools.** We compare middle schools to elementary schools because for-profit-trained teachers were disproportionately likely to earn certificates at higher grade levels (Table 2 and Appendix Table C1).

2. **Counties with for-profit EPPs.** Although most for-profit EPPs offered online training, the physical location of their headquarters still mattered for their demand

[26] Our teacher departure strategy is analogous to Jäger and Heining (2022)'s strategy of using worker deaths as a shock to firm labor supply. Jäger and Heining (2022) use a DiD specification because worker deaths are plausibly exogenous. As we show below, a DiD model does not work as well in our case because teachers may choose to leave a school if student outcomes are on a negative trend. The RD specification helps to address this issue by separating sharp changes in teacher composition from longer-term trends that may cause teacher turnover.

[27] We weight observations in equation (2) by the product of a triangular kernel (based on the RD bandwidth) and the number of individual teachers or students used to compute the outcome variable, Y_{st}.

due to advertising and some in-person training requirements.[28] Thus schools that were located close to a for-profit EPP headquarters were exposed to a larger pool of for-profit-trained teachers. We compare the 12 counties that had an initial for-profit opening between 2001 and 2009 (Appendix Tables A1 and A9) to the 191 non-contiguous Texas counties that never had a for-profit EPP.

3. **Predicted proportion of teachers from for-profit EPPs.** We use a random forest model to predict the proportion of each school's 2011–2016 teachers who were trained at a for-profit EPP. The model predictors are school mean characteristics measured from 1996–2000, including student demographics/test scores, teacher demographics/certification routes, and location. This approach lets the data define more- and less-exposed schools based on pre-policy characteristics. We compare schools in the top and bottom quartiles of predicted for-profit share, omitting the two middle quartiles.

4. **Predicted growth in the proportion of teachers from alternative EPPs.** This approach is the same as the previous one, except the outcome variable in our random forest model is the *change* in the share of a school's teachers who were trained at *any* alternative EPP between 1996–2000 and 2011–2016. This definition is motivated by the fact that the EPP policies impacted the design of programs at other alternative EPPs in addition to the creation of new for-profit programs.

5. **Proportion of uncertified teachers.** We compare schools in the top and bottom quartiles of the share of teachers who were uncertified in 1996–2000, omitting the two middle quartiles. The idea here is that schools with more uncertified teachers may have benefited more from the relaxation of EPP training requirements.

Appendix B.5 provides details on our definitions of policy exposure.

6.3 RD-DiD specification. Our main specification combines our teacher departure RD model with DiD variation in exposure to the EPP policy changes. Specifically, we estimate our RD regression (2) separately for schools that were more and less exposed to the EPP policy changes (treated/control groups) as defined above and for teacher departures that occurred before and after 2002 (pre/post variation). Our pre-period includes teacher departures that occurred in $y \in$ 1997–2001, when more stringent EPP requirements were in place. Our post-period includes departures in $y \in$ 2002–2016, after the implementation of

[28] For example, many for-profit EPPs send employees to observe and provide feedback to the candidate during their first year of teaching.

more flexible training requirements.[29] We estimate equation (2) separately for each pairwise combination of school exposure group $g \in \{\text{treated, control}\}$ and departure period $p \in \{1997\text{–}2001,\ 2002\text{–}2016\}$, which gives four RD coefficients β_{gp}.[30]

Lastly, we use these RD coefficients as the dependent variable in a simple DiD regression:

$$\beta_{gp} = \kappa + \phi \text{Treated}_g + \delta \text{Post}_p + \theta \text{Treated}_g \text{Post}_p + \varepsilon_{gp}, \tag{3}$$

where Treated_g is a dummy for more-exposed schools and Post_p is a dummy for teacher departures in $y \in 2002\text{–}2016$. Equation (3) gives our RD-DiD specification.[31] The coefficient of interest, θ, shows how the effects of teacher departures (as estimated by the RD coefficients) changed with the adoption of flexible EPP requirements in more-exposed schools relative to less-exposed schools. For example, if the lower quality of for-profit-trained teachers outweighs the benefits of having more certified teachers, then we would find $\theta < 0$ for the outcome of student achievement. In other words, the quality of newly-hired teachers at more-exposed would decline relative to that at less-exposed schools as the for-profit training sector grew between 1997 and 2016.

The key identification assumption in our RD-DiD strategy is parallel trends *in the RD coefficients.* Specifically, we assume that the effects of teacher departures on student outcomes (as estimated by the RD coefficients) would have trended in the same manner for more- and less-exposed schools in the absence of the EPP policy changes. This is a weaker assumption than the standard DiD assumption of parallel trends in outcome *levels.* In particular, the RD specification helps to net out demographic trends by restricting identification to changes

[29] Our test score data begin in 1994, so we use 1997 as the first teacher departure year so that we have a minimum of three years of pre-departure data for our RD regressions. We use 2016 as the last departure year so that we have three years of post-departure data before the Covid-19 pandemic. We define 2002 as our first post-policy year because this is when the first teachers from for-profit EPPs appeared in schools.

[30] For our measure of policy exposure based on counties with for-profit EPPs (approach #2), we define pre/post periods based on the year of that the first for-profit EPP appeared in each treated county. This specification has staggered treatment adoption across counties, so we follow Callaway and Sant'Anna (2021) in using a "stacked" model with clean controls. See Appendix B.5 for details.

[31] Although equations (2)–(3) present our RD-DiD specification in two steps to build intuition, in practice we estimate a single-step specification by plugging equation (3) into equation (2). In other words, our single-step RD-DiD specification is:

$$\begin{aligned} Y_{st} = \big(\kappa + \phi \text{Treated}_g + \delta \text{Post}_p + \theta \text{Treated}_g \text{Post}_p\big) \mathbf{1}\{\tau_{ty} \geq 0\} + \\ \alpha_{gp} \tau_{ty} + \psi_{gp} \mathbf{1}\{\tau_{ty} \geq 0\} \tau_{ty} + \gamma_{sy} + \varepsilon_{sty} \quad \text{if } |\tau_{ty}| \leq h^Y. \end{aligned} \tag{4}$$

Note that we estimate separate running variable coefficients α_{gp} and ψ_{gp} for each grade group g and departure period p pair. Grade groups g are a function of the school/grade/subject triplet s, i.e., $g = g(s)$. Departure periods p are a function of teacher departure years y, i.e., $p = p(y)$. Thus the fixed effects γ_{sy} subsume dummies for gp pairs. Equation (4) is equivalent to estimating the RD equation (2) separately for each gp pair and then constructing the DiD estimator: $\theta = \big(\beta_{\text{Treated,Post}} - \beta_{\text{Treated,Pre}}\big) - \big(\beta_{\text{Control,Post}} - \beta_{\text{Control,Pre}}\big)$.

in outcomes from just before to just after the teacher departure. One might be concerned that the RD assumption of "no threshold manipulation" may be violated in our case if, for example, teacher departures cause some students to change schools. But our RD-DiD specification allows for this type of behavioral response provided that it does not change differentially in response to the EPP policies.[32] We find no evidence that our main results are driven by differential pre-trends using event study versions of our RD-DiD specification (Appendix Figure A9), which provides support for our key identification assumption. Below we also test the validity of parallel trends by using a variety of student characteristics as outcome variables, Y_{st}.

6.4 Effects on teacher composition. We begin by showing how flexible EPP requirements affected the composition of newly-hired teachers using middle schools as our treated group and elementary schools as our control group (first approach in Section 6.2). Panels A–B of Table 5 show our RD-DiD estimates for teacher composition using this specification, and Figure 7 shows corresponding RD graphs. Column (A) shows the mean of each outcome in the year prior to the teacher departure ($\tau_{ty} = -1$) in school/grades that experienced a departure in the post-policy period (2002–2016). Columns (B)–(C) show RD coefficients β from equation (2) estimated separately for middle school teacher departures in 2002–2016 (post-policy) and 1997–2001 (pre-policy). Similarly, columns (D)–(E) show β coefficients for elementary teacher departures in the post- and pre-policy periods. Column (F) shows our main object of interest, the RD-DiD coefficient θ from equation (3), which is equal to column (B) − column (C) − (column D − column E). In Figure 7, the left graphs in each panel (red circles) depict middle school teacher departures, and the right graphs (black triangles) depict elementary teacher departures. Hollow symbols represent pre-policy departures ($y \in$ 1997–2001), and solid symbols represent post-policy departures ($y \in$ 2002–2016). The x-axis denotes years relative to the teacher departure, τ_{ty}.[33]

Our RD-DiD specification identifies open teaching vacancies with differential exposure to for-profit EPPs, as intended. Table 5 shows that there is no discontinuity in the share of teachers in a school/grade with for-profit certification for departures in the pre-policy years (columns C and E). But in the post-policy years, newly-hired teachers were more likely to have for-profit certification than the departing teachers they replaced, with RD estimates of $\beta = 3.0$pp for elementary schools and $\beta = 5.6$pp for middle schools (columns B and D). Since the post-policy estimate is larger for middle schools, our RD-DiD coefficient in column (F) shows that the EPP policy increased the share of teachers with for-profit training by

[32] This is analogous to the "difference-in-IV" estimator in Alsan et al. (2025), in which violations of the standard IV assumptions are permissible provided that they are the same in the pre- and post-periods.

[33] Appendix Figures A5–A7 show RD graphs for other teacher and student outcomes.

$\theta = 2.6$pp more in middle schools than in elementary schools (see also Panel A of Figure 7).

Consistent with our above findings, our RD-DiD results show that the EPP policy reduced schools' reliance on uncertified teachers, but it also reduced the share of teachers with standard certification. The RD coefficients are negative for the share of teachers with standard certificates and positive for the share of teachers with no certification (see also Figure 7). Thus departing teachers were more likely to have standard certificates than the teachers that replaced them, and some schools had to rely to uncertified teachers as replacements. Yet the RD coefficients are lower in the post-policy years for both outcomes, especially for middle school departures. Thus our RD-DiD estimates show that the EPP policy reduced the share of newly-hired teachers with standard certificates ($\theta = -3.3$pp) and no certificate ($\theta = -1.3$pp) in middle schools relative to elementary schools. Similarly, we find a relative increase in the share of teachers who have a certificate that is valid for their grade level and subject ($\theta = 1.4$pp), suggesting that the flexible EPP requirements helped schools find appropriately certified teachers.

Our findings for the effects the EPP policy on teacher composition are similar across our different measures of policy exposure. Table 6 shows RD-DiD coefficients θ from equation (3) for each of our five definitions of treated/control schools from Section 6.2 (columns B–F). In all cases we find that the share of for-profit teachers in more-exposed schools increased relative to that in less-exposed schools, with estimates ranging from 2.2pp to 5.4pp. Similarly, we always find reductions in the share of uncertified teachers (-1.3pp to -5.1pp) and increases in the share of appropriately certified teachers (1.4pp to 4.8pp), with the largest estimates coming from our specification that defines treatment based on the school's pre-policy share of uncertified teachers. Our specifications differ with respect to whether the shares of newly-hired teachers from standard and other alternative programs increased or decreased, which reflects heterogeneous effects of the EPP policy across geographic areas.

We do not find significant effects of the flexible EPP requirements on wages or teacher composition as measured by other demographic characteristics.[34] Table 5 shows that schools often fill open vacancies with teachers who have no prior teaching experience, with RD coefficients β ranging from 9.3pp–12.0pp. But our RD-DiD estimate shows that effect did not change differentially with the reform ($\theta = 1.1$pp). We also do not find a significant effect on the *number* of teachers in a school grade ($\theta = -0.02$ teachers). This is consistent with our finding in from Section 4 that the EPP policy increased the supply of certified teachers but did not affect the number of employed teachers. Across all of our definitions of treated/control schools, we see limited evidence of significant changes in the number of

[34] Our RD-DiD is underpowered to detect effects on the racial distribution of teachers given the modest differences in race across certification routes (see Table 2).

teachers or their demographics (Panel B of Table 6). Similarly, we also find no systematic evidence that the policy impacted the salaries of newly-hired teachers, with small and mostly insignificant point estimates across specifications.

6.5 Effects on student achievement. We find no systematic evidence that the EPP policy affected the number of students in school/grades with departing teachers or their demographic characteristics. In Tables 5 and 6, the outcome variables in Panel C are the number of students in the school/grade who took student achievement exams and their demographics as measured by an index of predicted math scores.[35] We find small and statistically insignificant RD-DiD estimates for student demographics across all five of our definitions of policy exposure. We also find insignificant effects on the number of exam takers in four out of five specifications, with the exception of a negative estimate of −3.3 students in our specification based on the pre-policy share of uncertified teachers. These results support our key assumption of parallel trends in the RD coefficients in that there is little evidence that enrollment responses to teacher departures diverged between more- and less-exposed schools over this time period.

Turning to our main results, we find no evidence that the flexible EPP requirements reduced student achievement. Panel D of Tables 5 and 6 show effects on student math and ELA test scores and corresponding test score residuals, which are the residuals from a value-added-like regression that includes individual, school/grade mean, and school mean student characteristics and lagged test scores (see also Figure 8). In our middle vs. elementary school comparison, we find positive RD-DiD coefficients for math and ELA scores (0.015–0.034 SDs), with statistically significant estimates for three out of four outcomes. These positive estimates are driven by a decrease in the effect of elementary teacher departures on student test scores from the pre- to post-policy periods (right graphs in Figure 8) and an increase in the effect of middle school departures on test scores (left graphs in Figure 8). Using our other measures of policy exposure, we find mostly positive but statistically insignificant test score effects, with RD-DiD coefficients ranging from −0.001 SDs to +0.027 SDs. Notably, we find no evidence that the EPP policy lowered student achievement.

We do not find systematic patterns of heterogeneity in the RD-DiD estimates for student test scores by race, gender, and socioeconomic status, but, again, there is little evidence of negative effects in any of these subgroups (Appendix Table A12). Since teachers also vary in quality beyond test score value added (Jackson, 2018), we also use our RD-DiD specification to estimate effects on a variety of academic and behavioral outcomes, including grade retention, attendance, and disciplinary incidents (Appendix Table A13). We find null

[35] Appendix Table A11 shows RD-DiD estimates for the student characteristics that underlie our demographic index. We see little evidence of differential changes in these characteristics across our specifications.

effects in most specifications, and some evidence that the more flexible EPP requirements led to a *reduction* in the number of suspensions.

6.6 Robustness. The consistency of our estimates across our measures of treatment exposure in Table 6 demonstrates the robustness of our main findings. In particular, each specification shows an increase in for-profit teachers, a reduction of uncertified teachers, and little impact on student achievement. Each version of treatment exposure has its limitations, but we view the consistency across these approaches as evidence of causal impacts. While we do have a few outcomes that are inconsistent across specifications (for example, the extent to which for-profits replace standard- or other alternative-trained teachers more), we believe this is due primarily to the heterogeneous effects of the EPP policy across geographic areas.

A limitation with our RD-DiD approach is that it only estimates effects in the first year of a teacher departure ($\tau_{ty} = 0$). In Appendix Table A10, we modify the RD regression (2) so that our RD-DiD specification estimates changes in outcomes from three years before to three years after the teacher departure ($\tau_{ty} = -3$ to $+3$). We continue to find null to positive effects on student test scores in this specification, although there is imbalance on student demographics in some specifications (which is why we prefer the RD model that estimates effects at $\tau_{ty} = 0$).

We also find similar results using a triple differences specification that compares middle school and elementary grade levels *within* counties. This specification uses the DiD variation in for-profit EPP openings across counties discussed at the beginning of Section 6, but we bring in another source of variation by comparing middle school and elementary grade levels given the propensity for for-profit trained teacher to certify more frequently at higher grade levels (Table 2). Our triple difference estimates largely confirm our primary results. In particular, we find an increases in for-profit certification, increased certification alignment, no change in log number of teachers, no change in demographic indices for students, and insignificant and small impacts on student achievement. Appendix Section C provides details on our triple differences specification and results.

A caveat is that our RD-DiD results are underpowered to detect meaningful changes in test scores since the differences in exposure to for-profit EPPs between treated and control schools are modest. In Panel D of Table 6, the average RD-DiD coefficient across our five measure of policy exposure and four test score outcomes is 0.015 SDs, with an average standard error of 0.014 SDs. Thus the average 95% confidence interval across our specifications includes test score effects of -0.012 SDs, which is sizable given that the average RD-DiD coefficient for the share of all teachers with for-profit certifications is 3.2pp (Panel A). Thus our average RD-DiD specification cannot rule out that a 20pp increase in the for-profit share of teachers reduced student achievement by 0.075 SDs. But we believe this average lower bound is overly conservative given that we find only one negative RD-DiD test score coefficient estimate (-0.001 SDs) across our twenty specifications.

Further, a simple back-of-the-envelope calculation shows that null impacts on student achievement are exactly what one would expect given the supply and quality effects documented in Sections 4 and 5. The share of all Texas teachers with for-profit certifications grew from zero percent to roughly 20 percent between 2001 and 2019. Our preferred math value-added estimate for for-profit-trained teachers from Table 4 (-0.02 SDs) suggests that this change would reduce average math scores by 0.004 SDs. But the share of teachers with no certification also fell from roughly five percent to one percent over these years, which would predict a 0.004 SD *increase* in math scores given our value-added estimate for uncertified teachers (-0.10 SDs).[36] This simple calculation suggests that the effects of lower-quality teachers from for-profit EPPs were almost exactly offset by the reduced share of even lower quality teachers with no certification. Calculations using ELA value-added yield a similar conclusion. Thus our supply and quality estimates throughout the paper corroborate our finding of no significant change in student achievement.

7 Discussion and conclusion

In a comprehensive analysis of occupational licensing in the United States, Kleiner and Soltas (2023) find that licensing laws reduce average welfare for consumers and workers. Although consumers benefit from increases in worker quality—consistent with the stated intent of licensing laws—these quality increases are not large enough to compensate consumers for higher prices. Similarly, licensing laws benefit some workers through higher wages, but reduced employment in the licensed occupation and increases in the cost of required human capital investments make the average worker worse off.

This paper showed that the typical training requirements for public school teachers in most U.S. states also have a net negative impact on workers and consumers, but the mechanisms for these effects are quite different. Using detailed administrative data, we analyzed the supply and quality impacts of a unique policy in Texas that expanded flexibility in the design of teacher training programs, which led to growth of a large for-profit sector that offered a lower-cost path to a teaching career. Unlike in Kleiner and Soltas (2023), the welfare impacts of occupational licensing for teachers are not revealed through changes in wages, employment, or prices given the government's major role in setting teacher salaries and providing free education. Indeed, we found no significant effects of the Texas policy

[36]Changes in the share of teachers from out of state and other alternative EPPs are effectively ignorable in this back-of-the-envelope calculation given their small value-added estimates in Table 4.

on the number of employed teachers, average teacher wages, or the number of students in impacted public schools.

Rather, the primary effect of the Texas policy was to reduce training costs for prospective teachers, especially for those switching from other careers. At for-profit and other alternative teacher training programs, the length of time between enrolling in the program and becoming an instructor of record is nearly one third of the time it takes to become a teacher through the standard training route (Appendix Table A4). This lowers the opportunity cost to become a teacher in terms of time and forgone earnings. For-profit programs are less expensive than other certification routes and have lower upfront costs (Figure 2), which can ease credit constraints for low-income individuals. These opportunity costs and upfront fees are significant for the typical prospective teacher; in our data, individuals who entered teaching careers through alternative EPPs earned roughly $20,000 on average in the years prior to teaching and over $40,000 in their first years in the classroom (Appendix Figure A10).[37] The entry of for-profit programs also forced other university-affiliated and non-profit alternative programs to reduce their training requirements and costs to remain competitive (Appendix Table A2).[38] The importance of this reduced training burden is evident in the fact that the number of newly-certified teachers in Texas doubled in the years after the policy, and it remained more than 40% higher than in other states up through 2019.

Importantly, we found that the reduction in teacher training requirements in Texas did not come at the cost of reduced product quality for the consumers of education—students. Using identifying variation from teacher departures and heterogeneous exposure to the policy across grade levels and geographic areas, we found mostly insignificant effects of the policy on student achievement and no evidence of negative impacts. While teachers who went through the lower-cost training routes were lower quality as measured by turnover and value-added, the Texas policy alleviated supply constraints for school districts, which reduced their reliance on even lower quality uncertified teachers. Additionally, for-profit programs attracted a more diverse group of candidates, which could also have broader positive impacts on students' life outcomes that we are unable to study in this paper. While there are some potential drawbacks of the Texas policy which are beyond the scope of our analysis, including a flow of tuition dollars from public to private entities, overall the policy reduced the costs of training for prospective teachers without lowering average student achievement.

[37]Depressed wages in the pre-teaching period may partly reflect part-time employment while individuals work on completing certification requirements, but Appendix Figure A10 shows that mean earnings in this population are around $20,000 in each of the three years prior to teaching.

[38]The tuition costs for career switchers are potentially cheaper among alternative programs than taking several credit hours at a college, and alternative EPPs reduce the need for credit (student loans). Completing just the education coursework portion of a bachelor's at Texas State University would be 78 credit hours for a total cost of $30,550 (Texas State University, 2024a,b).

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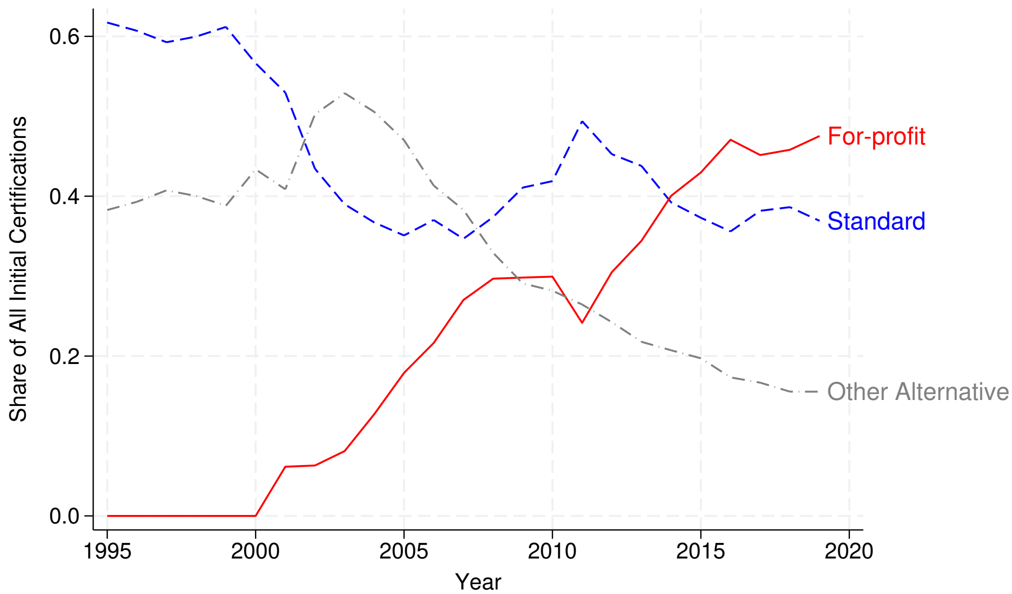
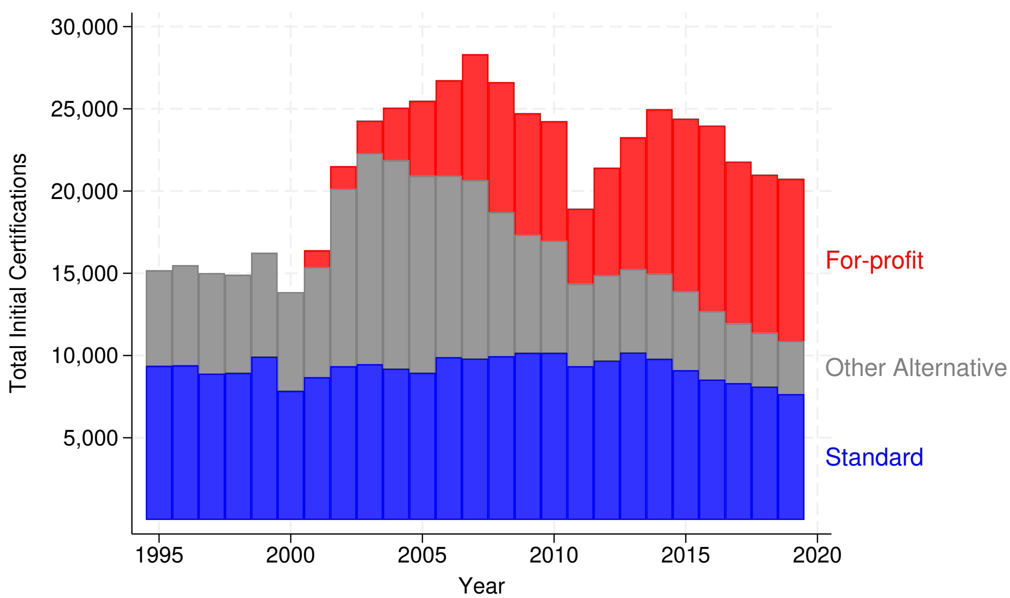
Figures



Panel A. Share of all initial teacher certifications



Panel B. Total initial teacher certifications

Figure 1: Growth of for-profit Educator Preparation Programs (EPPs)

Notes: Panel A shows the share of all initial teacher certifications by year and certification route. Panel B shows the total number of initial certifications by year and certification route. Data: SBEC.

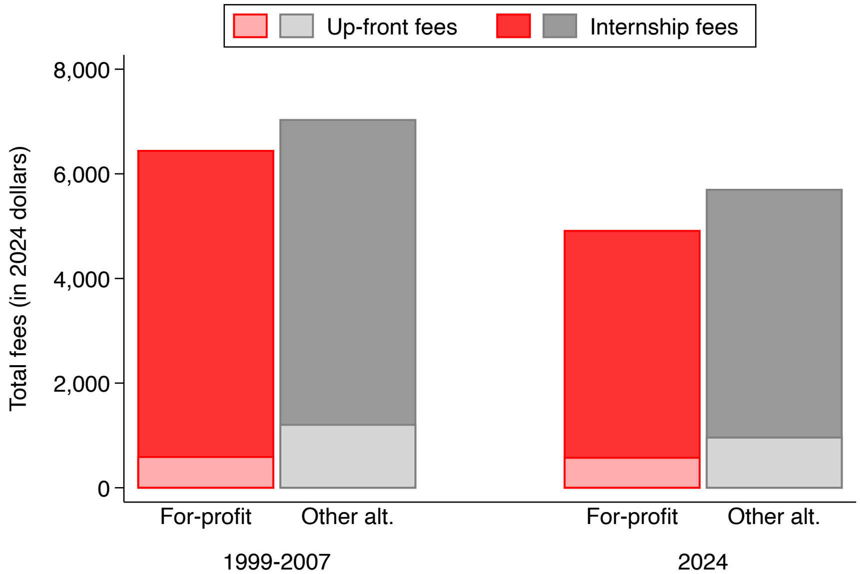


Figure 2: Pricing of for-profit and other alternative EPPs — Early 2000s vs. 2024

Notes: This figure displays average prices charged by for-profit and other alternative EPPs. The sample of for-profit companies includes the two largest EPPs (iteachTexas and A+ Texas Teachers) and the two earliest EPPs (ACT-Rio Grande Valley and Education Career Alternatives Porgram). The sample of other alternative programs includes the two largest EPPs operated by independent school districts (Dallas ISD and Houston ISD) and all EPPs run by Education Service Centers for which we could find historical information. These EPPs collectively represent the large majority of the alternative teacher certification market. The leftmost bars display pricing data obtained from historical versions of each EPP's website using archive.org; we use data from the earliest version of each website that we could find, which range from 1999–2007. The rightmost bars display information from each EPP's website obtained in October 2024. Light-shaded bars indicate up-front program fees, which typically include application fees and training fees. Dark-shaded bars include fees due during the internship period, which are typically paid out of the candidate's teaching paycheck. We convert 1999-2007 prices to 2024 dollars. See Appendix Table A2 for details on the data and sources.

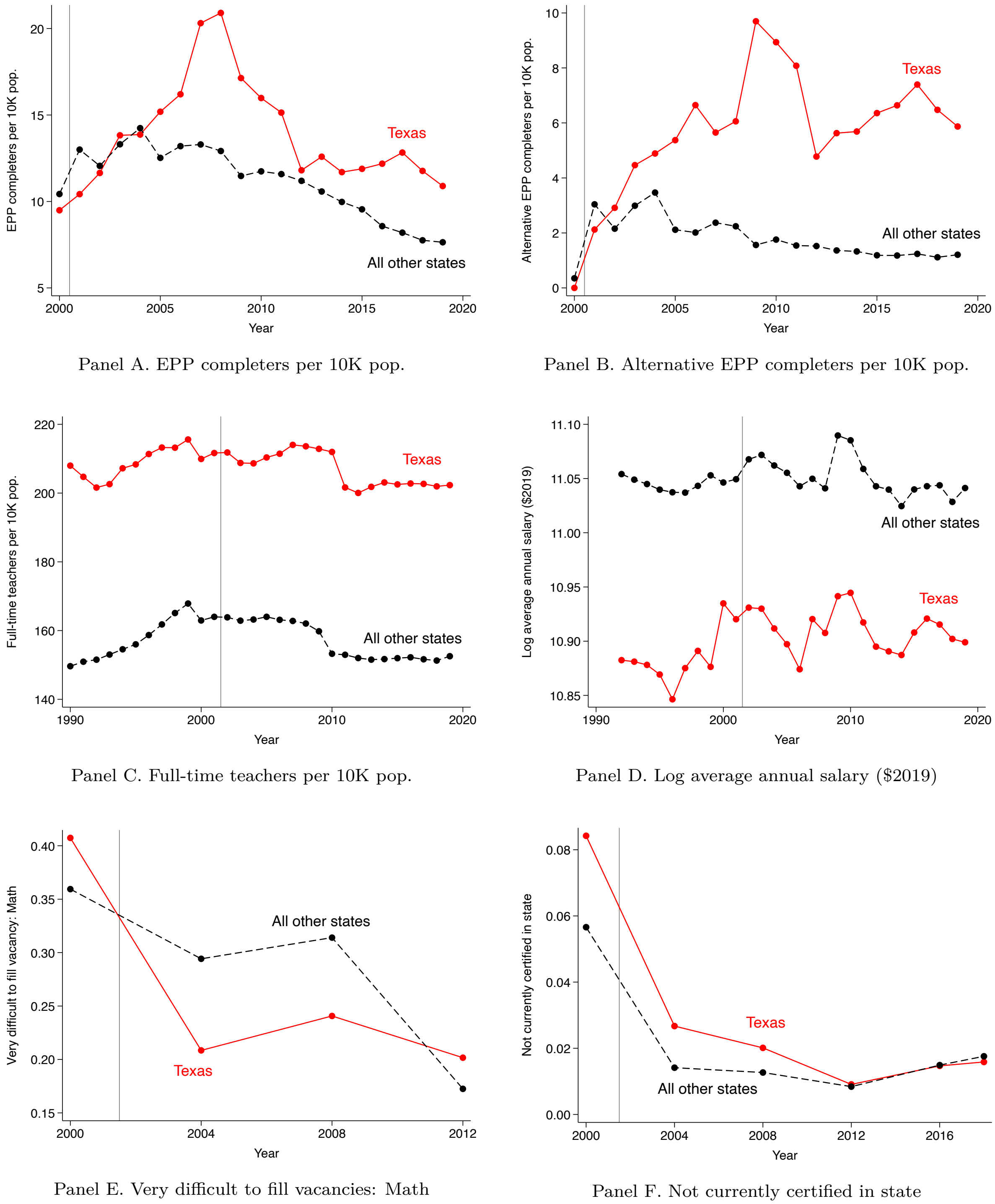


Figure 3: Changes in teacher supply — Texas vs. other states

Notes: This figure plots raw data for each outcome in Texas (red solid lines) and all other states (black dashed lines). In Panels A–B, the horizontal line represents the year in which changes in EPP policy in Texas allowed for-profits to operate (2001). In Panels C–F, the horizontal line represents the first academic year after the law (2002), which was when the law began to impact teacher composition. See Appendix B.3 for details on data sources and variable definitions. Data: Title II, Common Core, and SASS/NTPS.

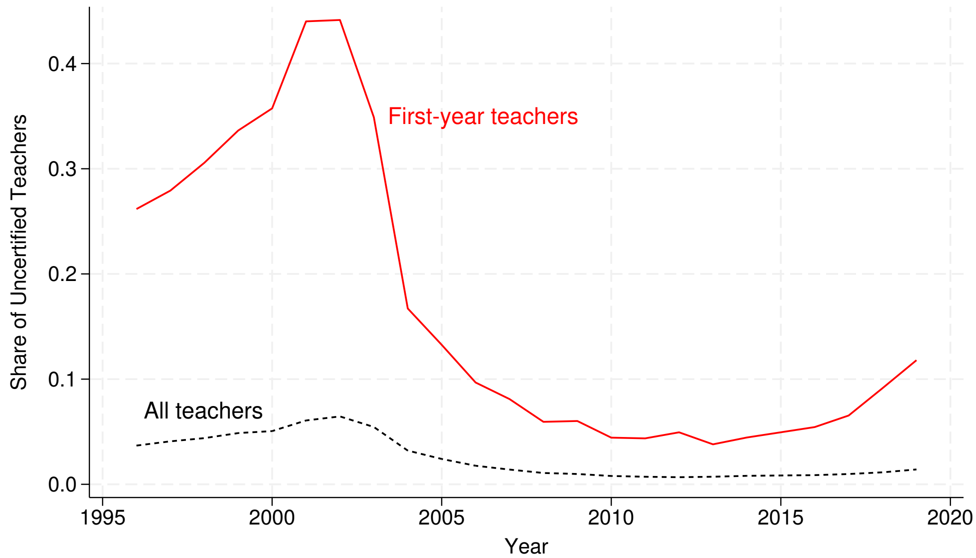
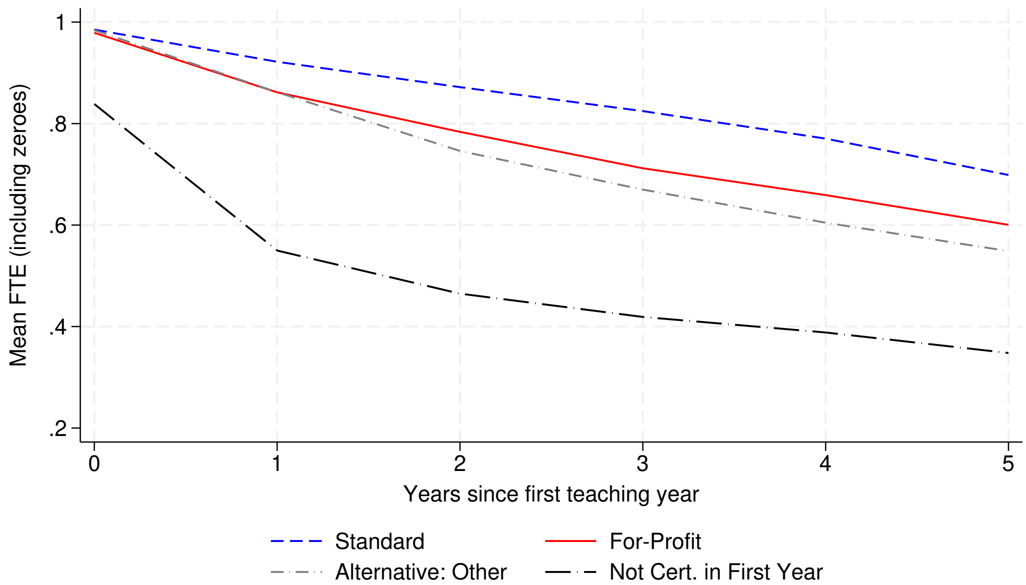
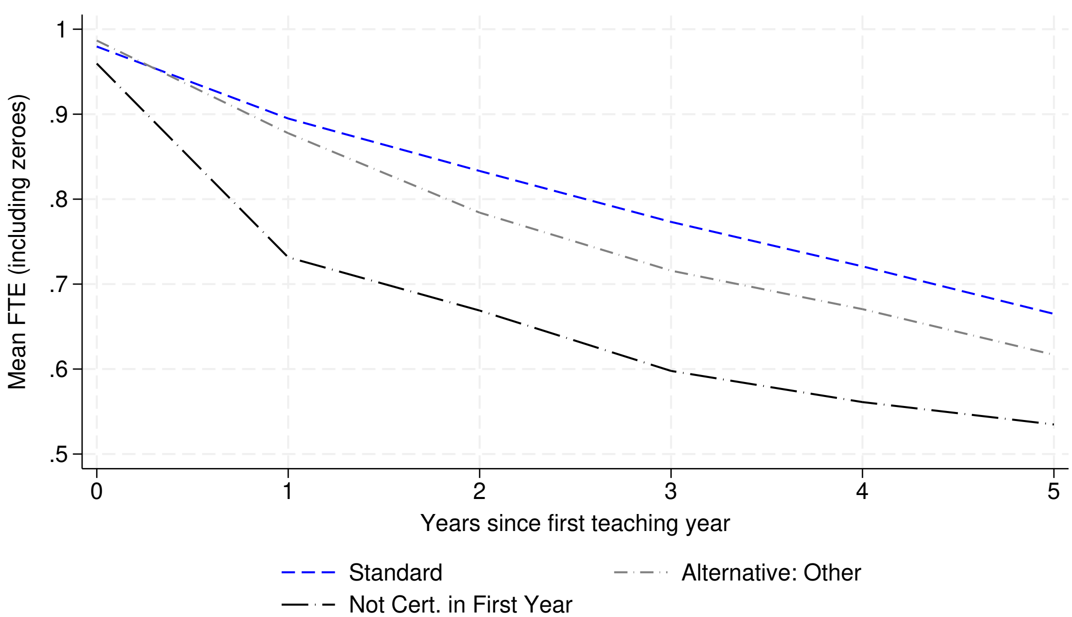


Figure 4: Share of teachers with no certification by year

Notes: This figure plots estimates of the share of all teachers and first-year teachers with no certification in each year. Data: TEA and SBEC.



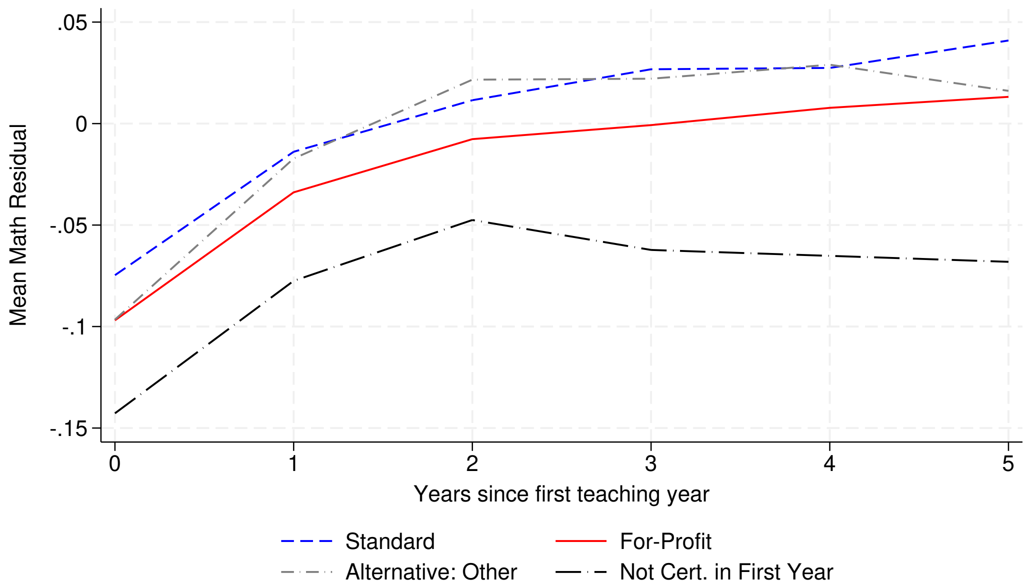
Panel A. 2012–2019 first–year teachers



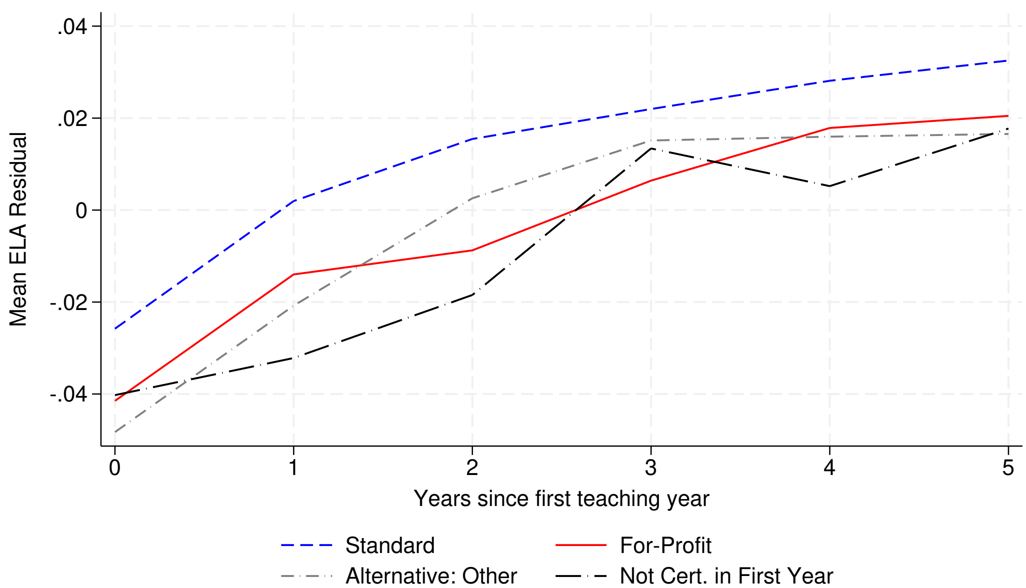
Panel B. 1996–2001 first–year teachers

Figure 5: Teacher turnover by experience and certification route

Notes: The y-axis is the mean FTE for teachers in each category. Panel A includes teachers who started in the 2012–2019 academic years. Panel B includes teachers who started in 1996–2001. When a teacher leaves the dataset, we assign them a FTE equal to zero for that and all following experience years. See Appendix Figure A1 for the person-level (not FTE) version of Panel A. Data: TEA and SBEC.



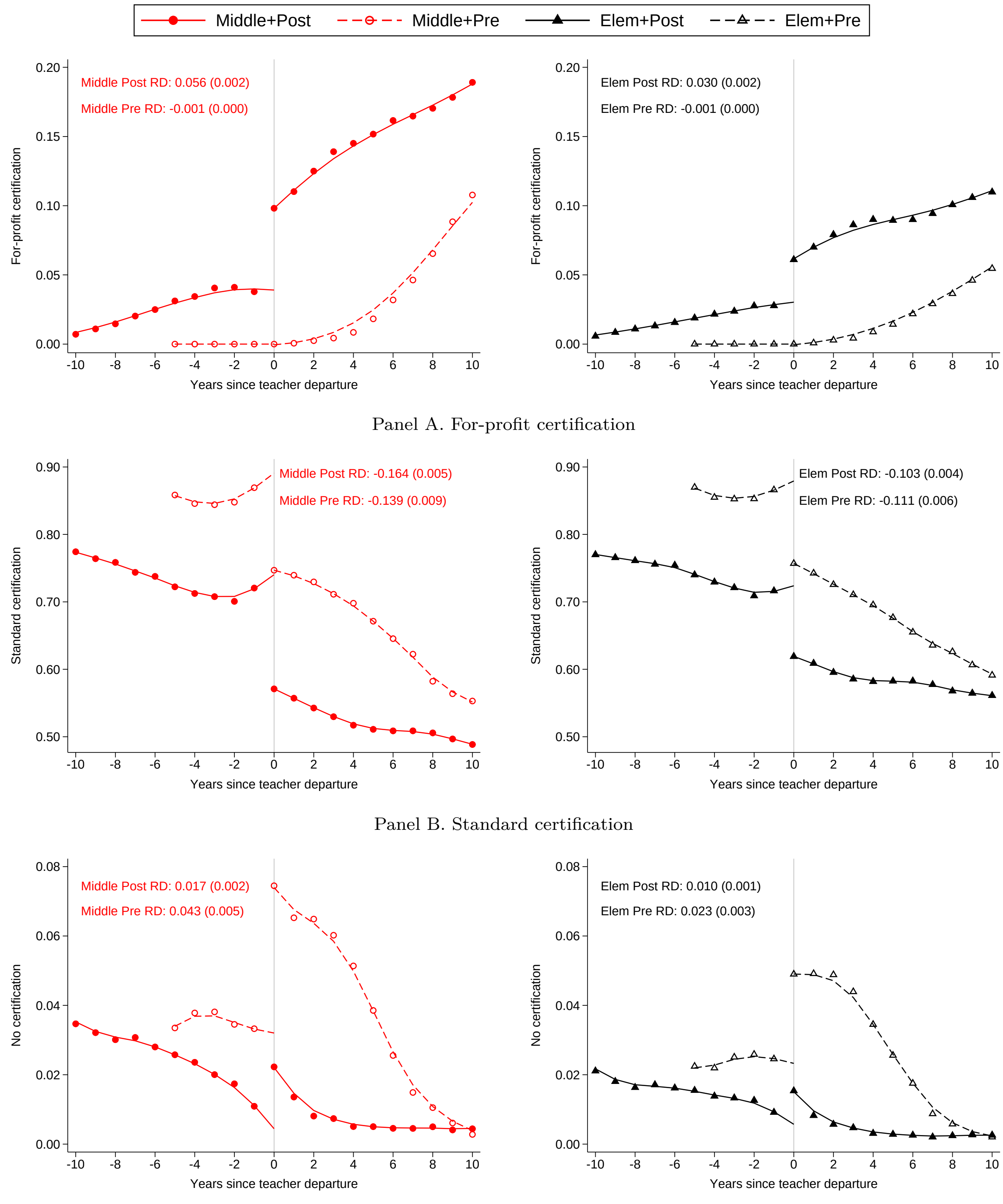
Panel A. Math value-added



Panel B. ELA value-added

Figure 6: Value–added by experience and certification route

Notes: The y-axis is the mean teacher-year value-added, calculated as described in Section B.4, for teachers with a given experience-level. We include only teachers who started in the 2012–2019 academic years. See Figure A4 for the histogram of all value-added estimates. See Appendix Figure A2 for a consistent sample of those who work 6 years. See Appendix Figure A3 for value-added calculated as in Chetty et al. (2014a). Data: TEA and SBEC.



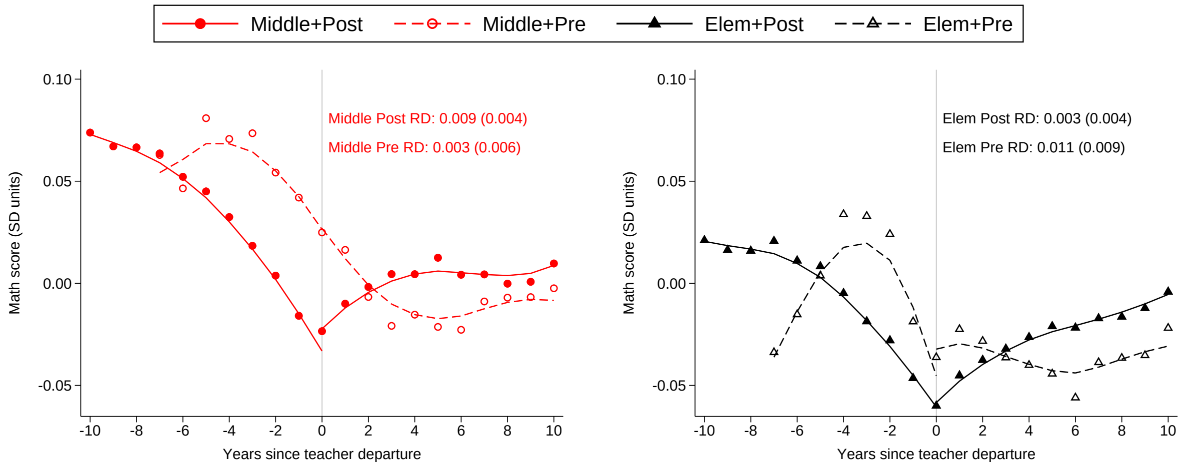
Panel A. For-profit certification

Panel B. Standard certification

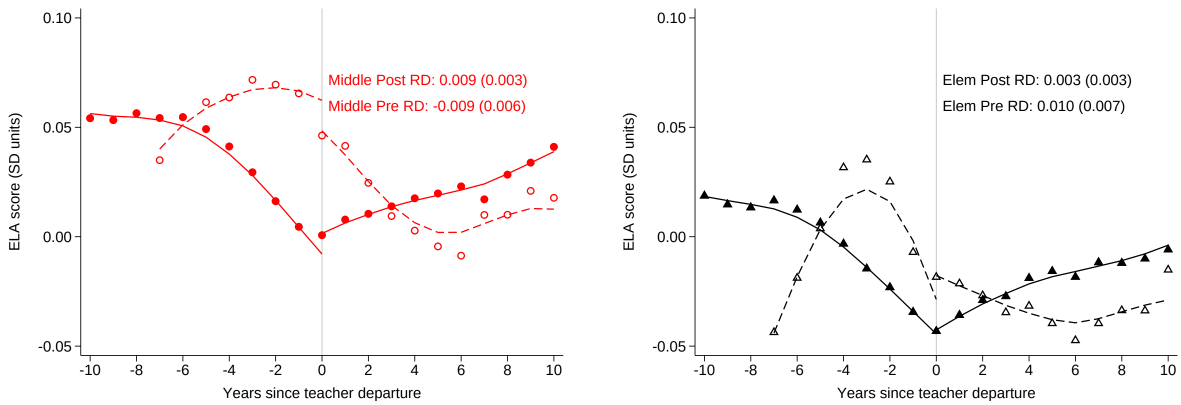
Panel C. No certification

Figure 7: Effects of teacher departures on teacher composition — Middle and elementary schools

Notes: This figure contains RD graphs that show how teacher departures affect the composition of teachers in a school/grade as defined by their certification route. Red circles represent middle school teachers (grades 6–8). Black triangles represent elementary teachers (grades 3–5). Hollow symbols depict teacher departures that occurred in $y \in$ 1997–2001 (pre-policy). Solid symbols depict teacher departures that occurred in $y \in$ 2002–2016 (post-policy). The x-axis is years relative to the teacher departure, τ_{ty}. The y-axis depicts the average outcome at the school/grade/year level. Each graph displays RD coefficients β from equation (2) with standard errors clustered at the school level in parentheses. Data: TEA and SBEC.



Panel A. Math score (SD units)



Panel B. ELA score (SD units)

Figure 8: Effects of teacher departures on student achievement — Middle and elementary schools

Notes: This figure contains RD graphs that show how teacher departures affect the math/ELA test scores of students in a school/grade. Red circles represent middle school teachers (grades 6–8). Black triangles represent elementary teachers (grades 3–5). Hollow symbols depict teacher departures that occurred in $y \in$ 1997–2001 (pre-policy). Solid symbols depict teacher departures that occurred in $y \in$ 2002–2016 (post-policy). The x-axis is years relative to the teacher departure, τ_{ty}. The y-axis depicts the average outcome at the school/grade/year level. Each graph displays RD coefficients β from equation (2) with standard errors clustered at the school level in parentheses. Data: TEA.

Tables

Table 1: Changes in teacher supply and preparation — Texas vs. other states

(A)	(B)	(C)	(D)	(E)	(F)	(G)
			DiD estimates			
Dependent variable	Data source (year range)	Texas in 2000	No controls	Account-ability	Other policies	N
Panel A. Number of potential teachers						
EPP completers per 10K pop.	Title II (2000–2019)	9.491	3.799* (0.739)	3.869* (0.741)	3.301 (1.906)	480
Alternative EPP completers per 10K pop.	Title II (2000–2019)	0.000	4.575* (0.594)	4.725* (0.658)	3.908 (1.536)	480
Initial certifications per 10K pop.	Title II (2000–2019)	11.350	8.434 (1.817)	9.442 (1.381)	9.517 (1.545)	420
Panel B. Teacher employment and wages						
Full-time teachers per 10K pop.	Common Core (1990–2019)	209.927	−1.310 (1.967)	−1.113 (2.109)	−0.073 (1.819)	1,530
Log average annual salary ($2019)	Common Core (1992–2019)	10.935	0.013 (0.015)	0.013 (0.015)	0.004 (0.010)	1,428
Panel C. Difficulty filling teacher vacancies						
Very difficult to fill vacancy: Elementary	SASS/NTPS (2000–2012)	0.087	−0.033 (0.013)	−0.028 (0.016)	−0.031 (0.014)	132
Very difficult to fill vacancy: Math	SASS/NTPS (2000–2012)	0.407	−0.086 (0.025)	−0.095 (0.033)	−0.075 (0.039)	196
Very difficult to fill vacancy: English	SASS/NTPS (2000–2012)	0.137	−0.083 (0.016)	−0.082 (0.021)	−0.085 (0.021)	180
Very difficult to fill vacancy: ESL	SASS/NTPS (2000–2012)	0.496	−0.177 (0.031)	−0.181 (0.032)	−0.140 (0.035)	84
Very difficult to fill vacancy: Special ed.	SASS/NTPS (2000–2012)	0.348	−0.065 (0.026)	−0.055 (0.030)	−0.039 (0.031)	136
Panel D. Teacher preparation						
Entered teaching through alternative EPP	SASS/NTPS (2000–2018)	0.095	0.173** (0.007)	0.169** (0.009)	0.152** (0.017)	294
Had any student teaching	SASS/NTPS (2000–2018)	0.857	−0.104* (0.017)	−0.111* (0.023)	−0.097* (0.024)	264
Not currently certified in state	SASS/NTPS (2000–2018)	0.084	−0.022 (0.006)	−0.017 (0.006)	−0.017 (0.007)	174

Notes: This table shows how outcomes related to teacher supply changed in Texas relative to other states from before to after the 2001 EPP policy change. Column (A) lists the dependent variable for each regression, and column (B) lists the data source and years with available data. Column (C) shows the value for Texas in the year 2000. Columns (D)–(F) report DiD coefficients β from equation (1) with various controls, $\mathbf{X}_{st}$. Column (D) includes no policy controls (i.e., no $\mathbf{X}_{st}$ vector). Column (E) includes a binary control for states/years with consequential accountability policies following Dee and Jacob (2011). Column (F) includes the Dee and Jacob (2011) control variable plus binary controls for eleven other teacher policies following Kraft et al. (2020) (see their Appendix Table A1). Parentheses contain standard errors clustered at the state level. Statistical significance levels (denoted by stars) are computed following the method in Conley and Taber (2011) for settings with only one treated cluster, with * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. This method computes an empirical p value based on the position of the DiD coefficient β is the distribution of post $-$ pre mean residuals for all control states. Column (G) shows the sample size for each regression, which is the number of years with available data times the number of states that have data in every year. See Section 4.1 for details on the empirical specification. See Appendix B.3 for details on data sources and variable definitions. Data: Title II, Common Core, and SASS/NTPS.

Table 2: Characteristics of first-year teachers, 2012–2019

	(A)	(B)	(C)	(D)	(E)	(F)
	Texas BAs	First-year teachers by certification route				
	All	All	Standard	For-profit	Other alt.	No cert
Panel A. Demographics						
Male	0.42	0.25	0.17	0.33	0.28	0.33
White	0.48	0.58	0.61	0.53	0.58	0.51
Asian	0.07	0.02	0.02	0.02	0.03	0.01
Black	0.10	0.11	0.05	0.17	0.10	0.18
Hispanic	0.29	0.27	0.31	0.26	0.26	0.29
In grade 8 testing data	0.62	0.56	0.73	0.54	0.44	0.45
Grade 8 math score (SD units)	0.69	0.56	0.55	0.57	0.70	0.27
Grade 8 ELA score (SD units)	0.61	0.59	0.57	0.61	0.73	0.32
In college data	1.00	0.72	0.97	0.68	0.62	0.51
Age at certification		26.65	24.92	28.22	28.54	
N	946,378	136,363	49,663	44,182	22,462	9,982
Panel B. Distribution of college majors						
Business	0.19	0.05	0.00	0.11	0.10	0.07
Communication/Family Studies	0.08	0.06	0.02	0.10	0.10	0.07
Health	0.11	0.02	0.00	0.04	0.03	0.07
Humanities	0.11	0.21	0.16	0.26	0.27	0.21
Interdisciplinary	0.10	0.39	0.69	0.08	0.11	0.22
Parks/Leisure/Fitness	0.04	0.06	0.05	0.08	0.05	0.11
Social Sciences	0.11	0.07	0.01	0.14	0.15	0.08
STEM	0.18	0.07	0.05	0.10	0.11	0.07
Other	0.07	0.04	0.02	0.06	0.06	0.08
N	946,378	97,575	48,378	29,976	13,940	5,079
Panel C. Distribution of teaching grades						
Early childhood/Pre-kindergarten		0.03	0.03	0.02	0.03	0.04
Elementary school (grades K–5)		0.39	0.53	0.28	0.36	0.21
Middle school (grades 6-8)		0.22	0.19	0.27	0.23	0.17
High school (grades 9-12)		0.30	0.20	0.37	0.33	0.51
All grade levels		0.05	0.04	0.06	0.05	0.06
N		136,363	49,663	44,182	22,462	9,982
Panel D. Distribution of teaching fields						
Mathematics		0.16	0.18	0.15	0.17	0.09
English Language Arts (ELA)		0.22	0.23	0.22	0.23	0.14
Science		0.12	0.11	0.14	0.16	0.08
Social studies		0.11	0.11	0.11	0.12	0.07
Fine arts		0.07	0.09	0.06	0.04	0.04
Career & technical education		0.05	0.02	0.07	0.06	0.13
Special education		0.08	0.06	0.10	0.08	0.07
Bilingual students		0.04	0.05	0.03	0.04	0.03
English as a Second Language (ESL)		0.02	0.01	0.02	0.02	0.01
N		136,363	49,663	44,182	22,462	9,982
Panel E. Earnings						
Annual earnings one year prior to teaching ($2019)		14,616	9,249	19,449	14,990	18,096
Annual earnings in first year of teaching ($2019)		44,037	43,719	45,669	44,617	36,973
N (first year)		116,642	45,220	41,875	20,253	9,294

Notes: This table shows summary statistics for 2012–2019 Texas college graduates (column A) and 2012–2019 first-year teachers by certification route (columns B–F). Column (B) includes first-year teachers with out-of-state certification routes, which are not included in the remaining columns. Numbers are rounded to two decimals places, and thus values of 0.00 do not represent true zeroes. Data: TEA, THECB, and TWC.

Table 3: Turnover five years after first teaching year by certification route

	(A)	(B)	(C)	(D)	(E)	(F)
For-profit certification	-0.108***	-0.094***	-0.110***	-0.113***	-0.108***	-0.127***
	(0.006)	(0.008)	(0.012)	(0.012)	(0.023)	(0.016)
Other alternative certification	-0.168***	-0.124***	-0.105***	-0.099***	-0.074***	-0.083***
	(0.006)	(0.009)	(0.014)	(0.014)	(0.025)	(0.019)
No certification	-0.389***	-0.337***	-0.237***	-0.261***	-0.210***	-0.237***
	(0.011)	(0.016)	(0.031)	(0.031)	(0.062)	(0.041)
Teacher's grade 8 math score (SD units)				-0.026***	-0.017	-0.026***
				(0.007)	(0.012)	(0.010)
Teacher's grade 8 ELA score (SD units)				-0.031***	-0.031**	-0.038***
				(0.007)	(0.013)	(0.010)
N (# of teachers)	38,232	32,358	15,612	15,612	5,508	8,082
Start year FE	x					
Start year-start campus FE		x	x	x		
Non-missing covariates			x	x	x	x
Teacher demographics				x	x	x
Start year-start campus-start subject FE					x	
Start year-start campus-start grade FE						x

Notes: This table reports the regression output for turnover in experience year 5 (first year denoted 0). Regression coefficients for training type are interpreted relative to standard-trained teachers. Outcome is the FTE in experience year 5. Teachers not teaching in experience year 5 have 0 FTE. Regressions estimated on teachers who first started in years 2012-2014. Fixed effects for start year, start, campus, and start grade are defined by the first year/campus/grade we observe for each teacher in the data. If the teacher appears at multiple campuses or in multiple grades in their first year, we use the modal campus/grade as defined by their FTE across courses. Teachers have no non-missing covariates if they have a non-missing value for ethnicity, sex, and both grade 8 math and ELA test scores. Standard errors in parentheses are robust with * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Data: TEA and SBEC.

Table 4: Math and ELA value-added by certification route

	(A)	(B)	(C)	(D)	(E)
Panel A. Math scores					
For-profit certification	-0.143***	-0.029***	-0.021***	-0.021***	-0.017***
	(0.008)	(0.003)	(0.003)	(0.002)	(0.002)
Other alternative certification	-0.057***	-0.001	0.002	0.000	0.002
	(0.007)	(0.003)	(0.002)	(0.002)	(0.002)
Out of state certification	0.076***	0.009***	0.000	-0.002	0.001
	(0.010)	(0.004)	(0.003)	(0.003)	(0.003)
No certification	-0.459***	-0.142***	-0.100***	-0.084***	-0.063***
	(0.027)	(0.013)	(0.013)	(0.011)	(0.014)
N (# of students)	9,029,412	9,029,412	9,029,412	9,029,412	9,029,412
Panel B. ELA scores					
For-profit certification	-0.151***	-0.014***	-0.006***	-0.005***	-0.005***
	(0.007)	(0.001)	(0.001)	(0.001)	(0.001)
Other alternative certification	-0.069***	-0.009***	-0.005***	-0.004***	-0.004***
	(0.006)	(0.001)	(0.001)	(0.001)	(0.001)
Out of state certification	0.094***	0.009***	0.000	-0.002	-0.000
	(0.010)	(0.002)	(0.001)	(0.001)	(0.001)
No certification	-0.412***	-0.060***	-0.030***	-0.018**	-0.019*
	(0.024)	(0.010)	(0.009)	(0.009)	(0.010)
N (# of students)	9,449,937	9,449,937	9,449,937	9,449,937	9,449,937
Grade-year FE	x	x	x	x	
Student Covariates		x	x	x	x
Class Covariates			x	x	x
School Covariates			x		
School FE				x	
School-grade-year FE					x

Notes: This table reports the regression output described in Section B.4 for grade 4–8 teachers in 2012–2019. Column C is our preferred model. The top panel presents value-added differences across teacher training type for math standardized exam scores, while the bottom panel reports them for ELA. Coefficients are interpreted in standardized test units relative to standard-trained teachers. Standard errors in parentheses are clustered at the school-level with * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Data: TEA and SBEC.

Table 5: Effects of flexible EPP requirements: Middle vs. elementary schools

	(A)	(B)	(C)	(D)	(E)	(F)
		Middle school		Elementary school		
	Post-policy mean at τ_{ty} = -1	Post-policy RD	Pre-policy RD	Post-policy RD	Pre-policy RD	RD-DiD
Panel A. Certification route						
For-profit certification	0.032	0.056***	-0.001***	0.030***	-0.001***	0.026***
		(0.002)	(0.000)	(0.002)	(0.000)	(0.003)
Standard certification	0.718	-0.164***	-0.139***	-0.103***	-0.111***	-0.033***
		(0.005)	(0.009)	(0.004)	(0.006)	(0.012)
Other alternative certification	0.163	0.086***	0.072***	0.053***	0.056***	0.018*
		(0.004)	(0.006)	(0.003)	(0.005)	(0.009)
No certification	0.010	0.017***	0.043***	0.010***	0.023***	-0.013**
		(0.002)	(0.005)	(0.001)	(0.003)	(0.006)
Appropriate certification (if certified)	0.973	-0.021***	-0.047***	-0.013***	-0.025***	0.014**
		(0.002)	(0.005)	(0.002)	(0.004)	(0.007)
Panel B. Teacher characteristics						
Number of teachers	2.254	0.022***	0.022	0.081***	0.056**	-0.024
		(0.008)	(0.016)	(0.015)	(0.027)	(0.036)
Male	0.114	0.012***	0.006	0.005**	0.006*	0.007
		(0.003)	(0.005)	(0.002)	(0.004)	(0.007)
White	0.690	-0.025***	-0.019***	-0.018***	-0.010**	0.003
		(0.003)	(0.006)	(0.002)	(0.004)	(0.008)
First-year teacher	0.040	0.120***	0.118***	0.093***	0.101***	0.011
		(0.004)	(0.008)	(0.003)	(0.006)	(0.011)
Total annual salary	46,679	-4,466***	-5,223***	-3,662***	-4,396***	23
		(96)	(170)	(64)	(135)	(242)
Panel C. Student characteristics						
Number of exam takers	98.693	-0.068	-2.129***	0.351**	-0.287	1.423
		(0.345)	(0.814)	(0.176)	(0.337)	(1.007)
Demographic index (math score)	0.024	0.008***	-0.008**	0.003**	-0.011***	0.001
		(0.002)	(0.003)	(0.002)	(0.003)	(0.005)
Panel D. Student achievement						
Math score (SD units)	-0.028	0.009**	0.003	0.003	0.011	0.015
		(0.004)	(0.006)	(0.004)	(0.009)	(0.012)
Math score residual (SD units)	-0.022	0.009**	-0.001	0.002	0.025***	0.034***
		(0.004)	(0.007)	(0.004)	(0.010)	(0.013)
ELA score (SD units)	-0.012	0.009***	-0.009	0.003	0.010	0.026**
		(0.003)	(0.006)	(0.003)	(0.007)	(0.011)
ELA score residual (SD units)	-0.008	0.003	-0.003	-0.000	0.023***	0.029***
		(0.003)	(0.006)	(0.003)	(0.008)	(0.011)
N (# *sty* observations)	18,227	90,283	22,582	70,228	21,338	204,431

Notes: This table displays RD and RD-DiD estimates of the effects of flexible EPP requirements on teacher composition (Panels A–B), student characteristics (Panel C), and student achievement (Panel D). Column (A) shows the mean of each outcome in the year prior to the teacher departure ($\tau_{ty} = -1$) in school/grades that experienced a departure in the post-policy period (2002–2016). Columns (B)–(E) show RD coefficients β from equation (2) estimated separately for middle and elementary schools, and for departures in 2002–2016 (post-policy) and 1997–2001 (pre-policy). Column (F) shows the RD-DiD coefficient θ from equation (3) where Treated$_g$ is defined as an indicator for middle schools (grades 6–8). Standard errors in parentheses are clustered at the school level with * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Data: SBEC and TEA.

Table 6: Effects of flexible EPP requirements using different treated/control groups

	(A)	(B)	(C)	(D)	(E)	(F)
		RD-DiD coefficients with different treated/control groups				
	Post-policy mean at τ_{ty} = -1	Middle vs elem. school	Counties w/ for-profit EPPs	Predicted FP teacher share	Predicted Alt teacher growth	Share of teachers w/ no cert
Panel A. Certification route						
For-profit certification	0.032	0.026*** (0.003)	0.025*** (0.003)	0.054*** (0.004)	0.032*** (0.004)	0.022*** (0.004)
Standard certification	0.718	-0.033*** (0.012)	0.063*** (0.011)	0.038** (0.016)	-0.029* (0.017)	0.053*** (0.015)
Other alternative certification	0.163	0.018* (0.009)	-0.050*** (0.008)	-0.038*** (0.012)	0.046*** (0.013)	-0.024** (0.012)
No certification	0.010	-0.013** (0.006)	-0.017*** (0.004)	-0.030*** (0.009)	-0.027*** (0.009)	-0.051*** (0.008)
Appropriate certification (if certified)	0.973	0.014** (0.007)	0.018*** (0.005)	0.028*** (0.009)	0.024** (0.010)	0.048*** (0.010)
Panel B. Teacher characteristics						
Number of teachers	2.254	-0.024 (0.036)	-0.035 (0.028)	-0.012 (0.050)	0.032 (0.051)	-0.010 (0.043)
Male	0.114	0.007 (0.007)	-0.018*** (0.006)	-0.009 (0.009)	0.007 (0.010)	-0.014 (0.009)
White	0.690	0.003 (0.008)	0.010 (0.007)	0.019* (0.011)	-0.009 (0.011)	-0.005 (0.010)
First-year teacher	0.040	0.011 (0.011)	-0.034*** (0.009)	-0.020 (0.015)	-0.011 (0.015)	-0.034** (0.014)
Total annual salary	46,679	23 (242)	913*** (192)	470 (317)	-551* (329)	322 (301)
Panel C. Student characteristics						
Number of exam takers	98.693	1.423 (1.007)	0.129 (0.859)	1.537 (1.894)	2.213 (1.717)	-3.267*** (1.240)
Demographic index (math score)	0.024	0.001 (0.005)	0.000 (0.005)	0.006 (0.007)	-0.003 (0.007)	-0.009 (0.008)
Panel D. Student achievement						
Math score (SD units)	-0.028	0.015 (0.012)	0.015 (0.011)	-0.001 (0.017)	0.004 (0.017)	0.019 (0.015)
Math score residual (SD units)	-0.022	0.034*** (0.013)	0.006 (0.011)	0.021 (0.018)	0.013 (0.018)	0.019 (0.016)
ELA score (SD units)	-0.012	0.026** (0.011)	0.000 (0.009)	0.018 (0.015)	0.023 (0.015)	-0.000 (0.014)
ELA score residual (SD units)	-0.008	0.029*** (0.011)	0.009 (0.009)	0.027* (0.016)	0.018 (0.016)	0.007 (0.014)
N (# *sty* observations)	18,227	204,431	592,589	99,879	99,573	140,813

Notes: This table displays RD-DiD estimates of the effects of flexible EPP requirements on teacher composition (Panels A–B), student characteristics (Panel C), and student achievement (Panel D). Column (A) shows the mean of each outcome in the year prior to the teacher departure ($\tau_{ty} = -1$) in school/grades that experienced a departure in the post-policy period (2002–2016). Columns (B) and (D)–(F) show RD-DiD coefficients θ from equation (3) with Treated$_g$ defined as listed in the column header (see Section 6.2). Column (C) shows θ coefficients from the stacked RD-DiD specification (B2) described in Appendix B.5. Standard errors in parentheses are clustered at the school level with * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Data: SBEC and TEA.

Appendix — For Online Publication Only

A Appendix Figures and Tables

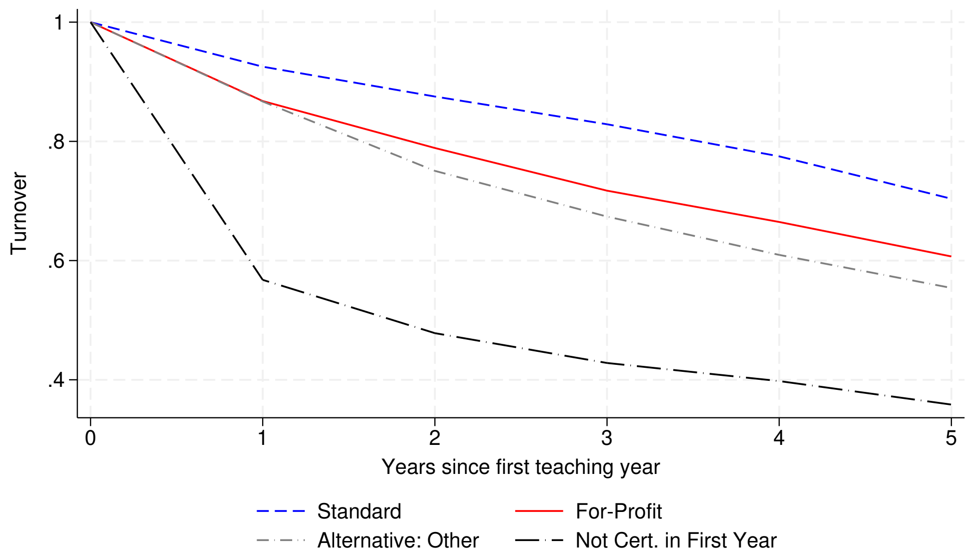
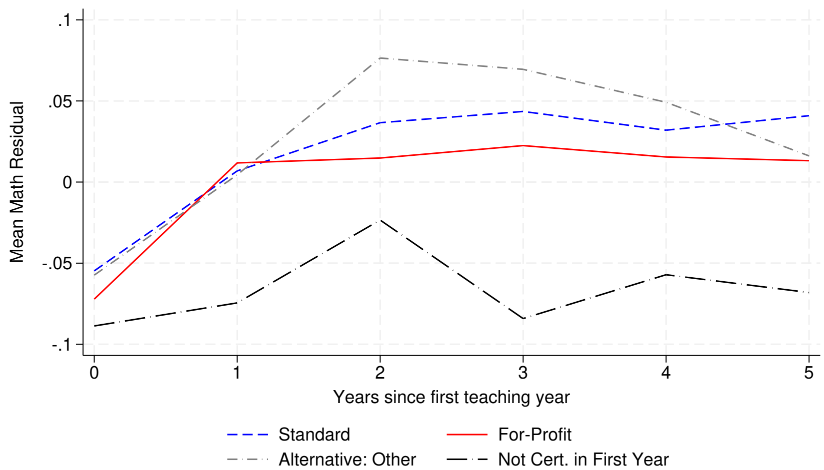
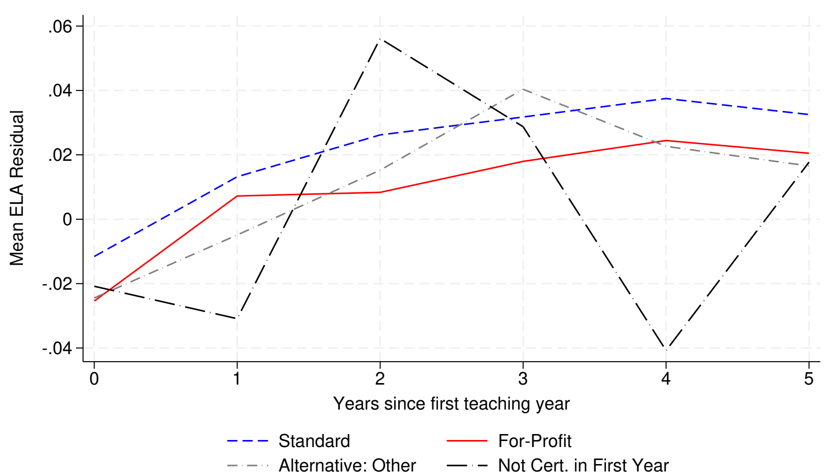


Figure A1: Teacher turnover by experience and certification route – Person-level

Notes: The y-axis is the average number of teachers who are still teaching in a given experience year. This is turnover based on total number of teachers and not FTE. We include only teachers who started in the 2012–2019 academic years. Data: TEA and SBEC.



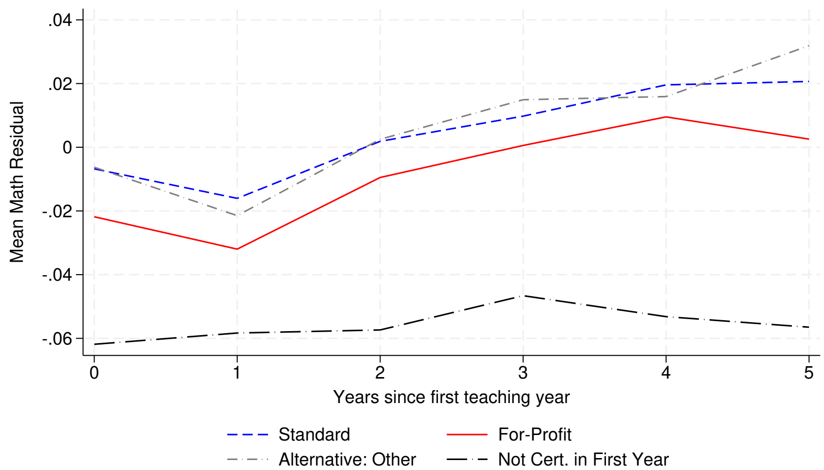
Panel A. Math VA



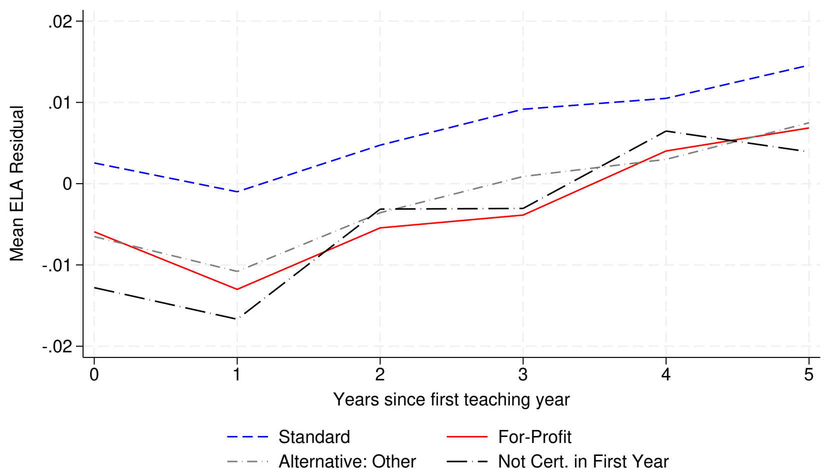
Panel B. ELA VA

Figure A2: Value-added by experience and certification route among teachers with 6 years of experience

Notes: The y-axis is the mean teacher-year value-added, calculated as described in the main text, by experience-level among those who worked 6 years as a teacher in our sample. Data: TEA and SBEC.



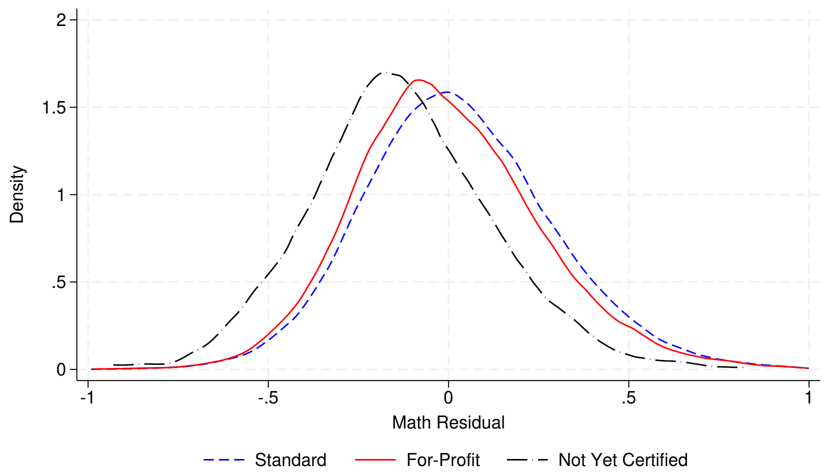
Panel A. Math VA



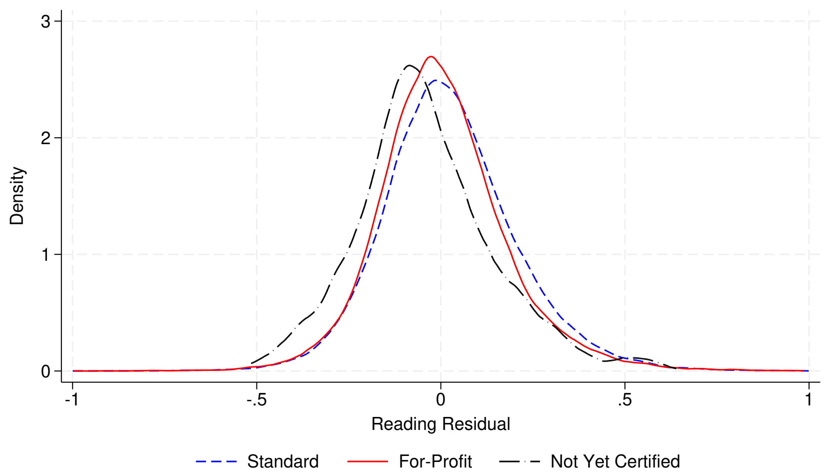
Panel B. ELA VA

Figure A3: Alternative value-added by experience and certification route

Notes: The y-axis is the mean teacher-year value-added, calculated as described in Chetty et al. (2014a), by experience-level. Data: TEA and SBEC.



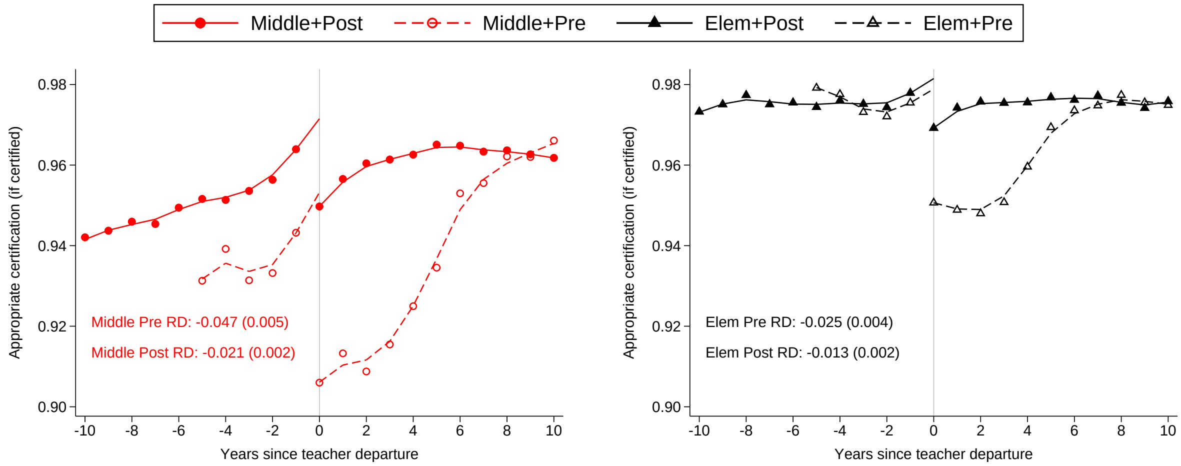
Panel A. Math VA



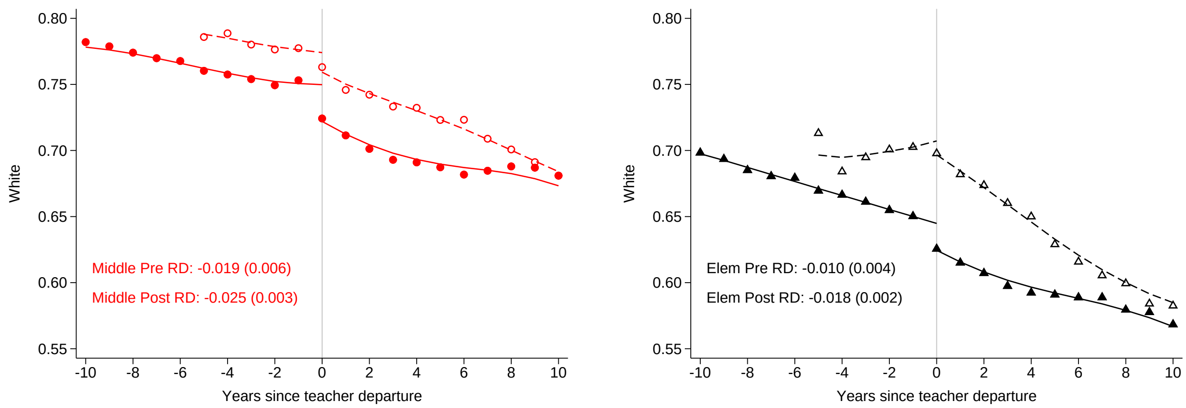
Panel B. ELA VA

Figure A4: Teacher-year value-added histogram by certification route

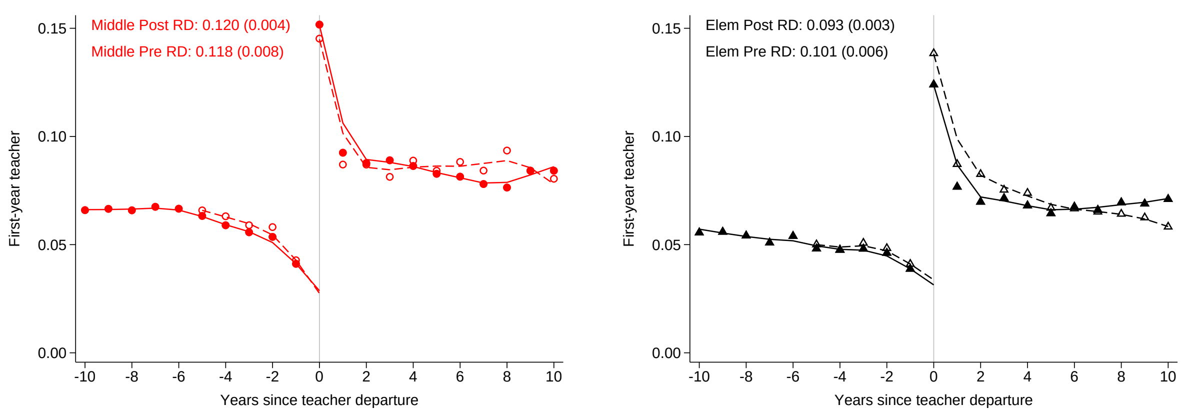
Notes: These are the histograms by training pathway for all teacher-year value-added inclusive of all teachers and all years (2012-2019) available. Data: TEA and SBEC.



Panel A. Appropriate certification (if certified)



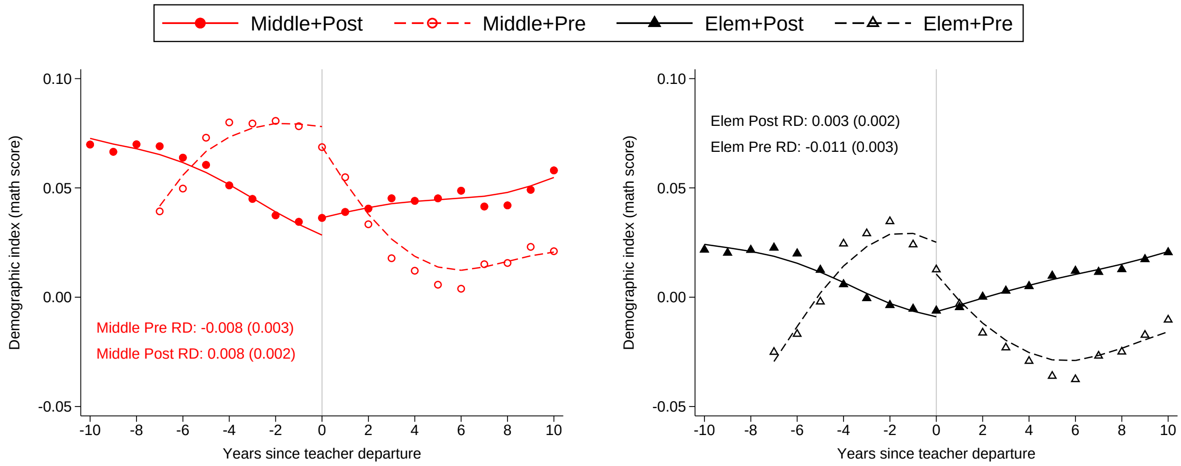
Panel B. White



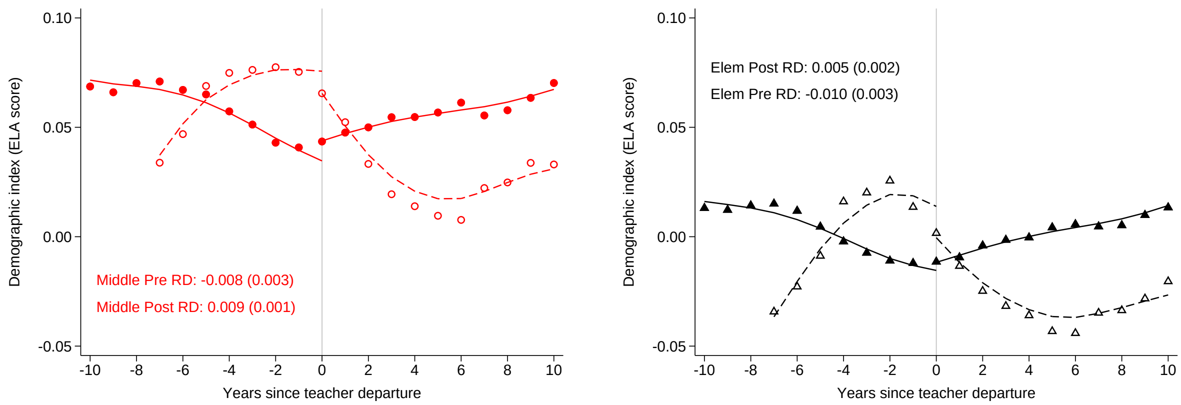
Panel C. First-year teacher

Figure A5: Effects of teacher departures on teacher certification status and characteristics — Middle and elementary schools

Notes: This figure contains RD graphs that show how teacher departures affect the composition of teachers in a school/grade as defined by their certification status, race, and years of experience. Red circles represent middle school teachers (grades 6–8). Black triangles represent elementary teachers (grades 3–5). Hollow symbols depict teacher departures that occurred in $y \in$ 1997–2001 (pre-policy). Solid symbols depict teacher departures that occurred in $y \in$ 2002–2016 (post-policy). The x-axis is years relative to the teacher departure, τ_{ty}. The y-axis depicts the average outcome at the school/grade/year level. Each graph displays RD coefficients β from equation (2) with standard errors clustered at the school level in parentheses. Data: TEA and SBEC.



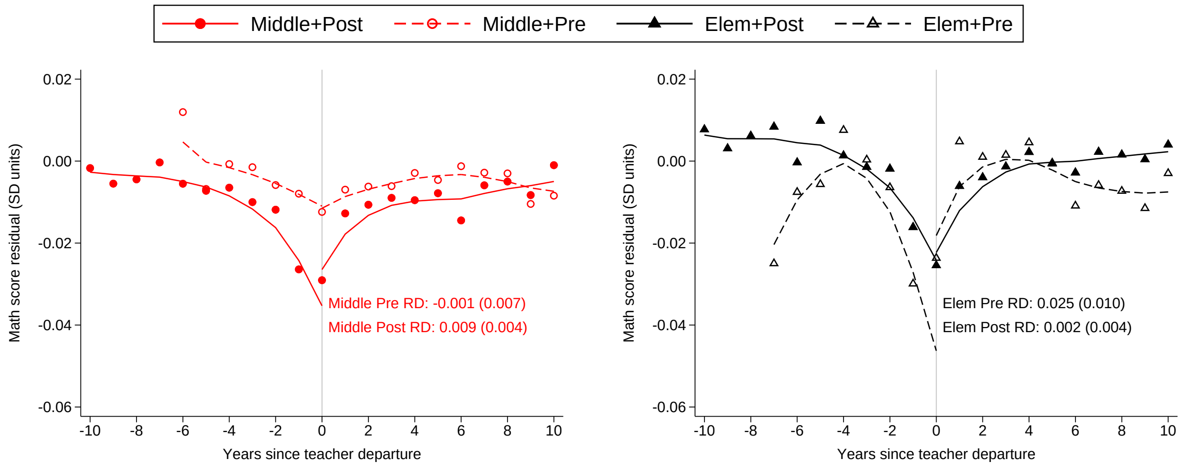
Panel A. Demographic index (math score)



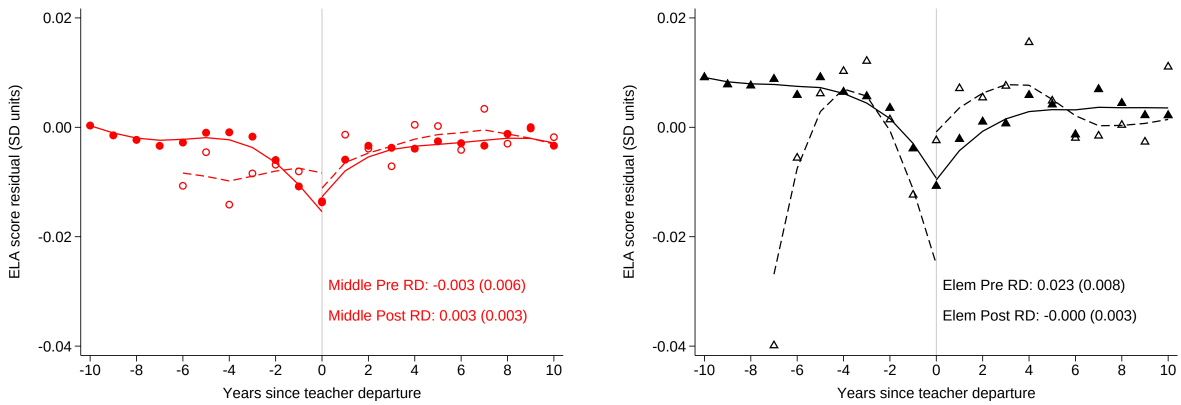
Panel B. Demographic index (ELA score)

Figure A6: Effects of teacher departures on student characteristics — Middle and elementary schools

Notes: This figure contains RD graphs that show how teacher departures affect the demographic characteristics of students in a school/grade as defined by demographic indices of predicted math/ELA test scores. Red circles represent middle school teachers (grades 6–8). Black triangles represent elementary teachers (grades 3–5). Hollow symbols depict teacher departures that occurred in $y \in$ 1997–2001 (pre-policy). Solid symbols depict teacher departures that occurred in $y \in$ 2002–2016 (post-policy). The x-axis is years relative to the teacher departure, τ_{ty}. The y-axis depicts the average outcome at the school/grade/year level. Each graph displays RD coefficients β from equation (2) with standard errors clustered at the school level in parentheses. Data: TEA.



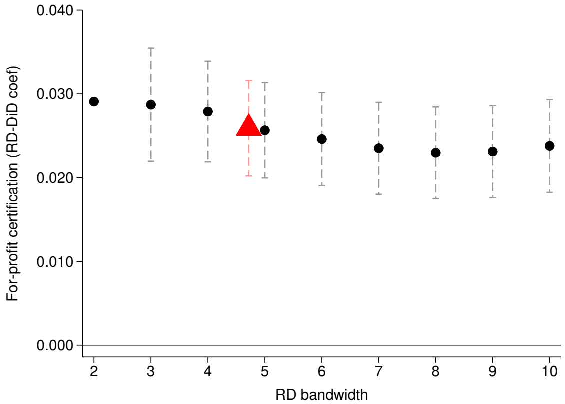
Panel A. Math score residual (SD units)



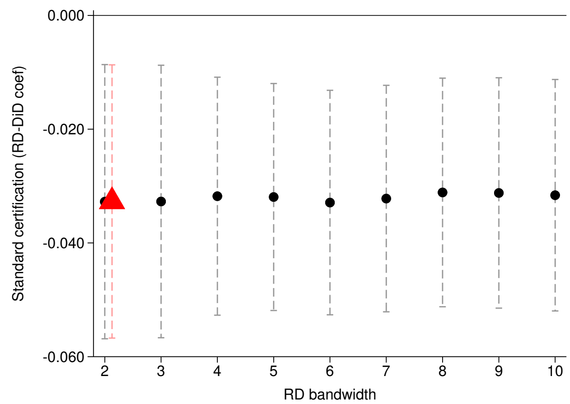
Panel B. ELA score residual (SD units)

Figure A7: Effects of teacher departures on student test score residuals — Middle and elementary schools

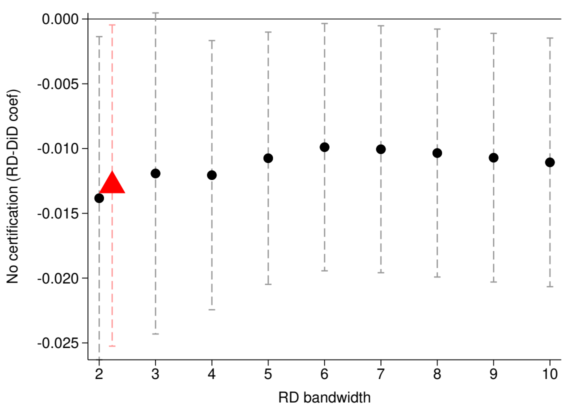
Notes: This figure contains RD graphs that show how teacher departures affect the math/ELA achievement of students in a school/grade as defined by math/ELA test score residuals (see Appendix B.2 for details). Red circles represent middle school teachers (grades 6–8). Black triangles represent elementary teachers (grades 3–5). Hollow symbols depict teacher departures that occurred in $y \in$ 1997–2001 (pre-policy). Solid symbols depict teacher departures that occurred in $y \in$ 2002–2016 (post-policy). The x-axis is years relative to the teacher departure, τ_{ty}. The y-axis depicts the average outcome at the school/grade/year level. Each graph displays RD coefficients β from equation (2) with standard errors clustered at the school level in parentheses. Data: TEA.



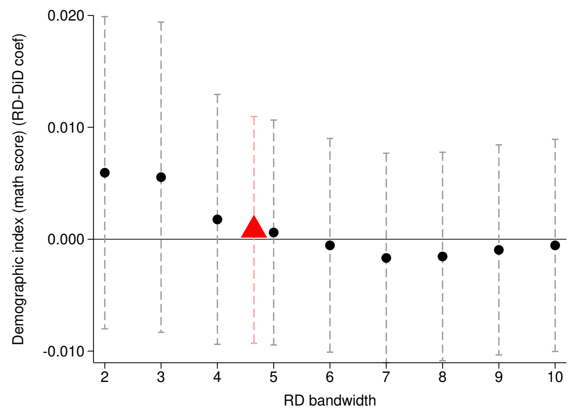
Panel A. For-profit certification



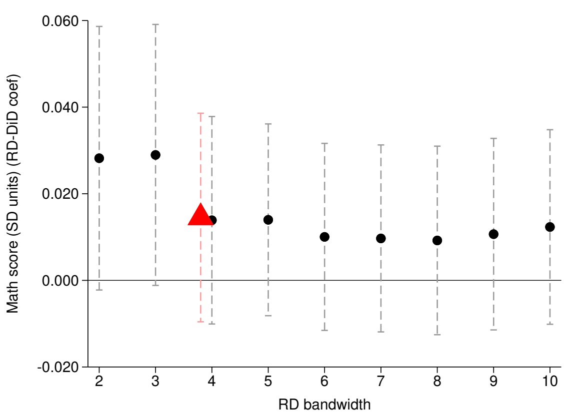
Panel B. Standard certification



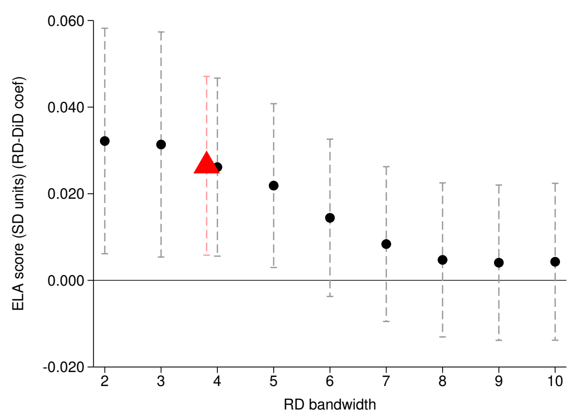
Panel C. No certification



Panel D. Demographic index (Math score)



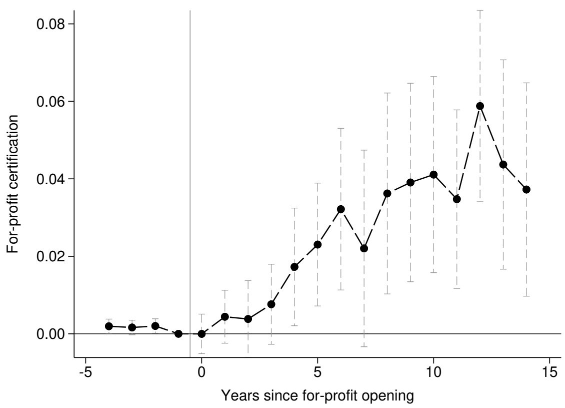
Panel E. Math score (SD units)



Panel F. ELA score (SD units)

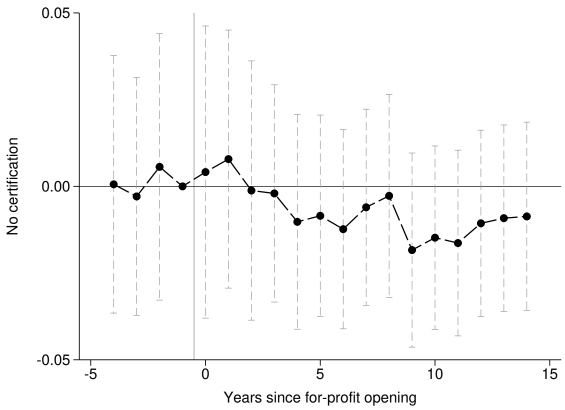
Figure A8: RD-DiD effects of flexible EPP requirements — Robustness to RD bandwidth

Notes: This figure displays the robustness of our RD-DiD estimates to the choice of RD bandwidth. The y-axis in each graph displays the θ from equation (3). The x-axis displays the bandwidth used for the RD regression (2). Circular markers depict RD coefficients computed using integer bandwidths from $h^Y \in$ 2–10 years relative to the year of the teacher departure. The large triangular markers show our benchmark estimates from column (F) of Table 5 using the Calonico et al. (2019) bandwidth. Note that the Calonico et al. (2019) RD bandwidths (triangular markers) vary across treated/control groups and pre/post periods since we compute them separately for each RD regression, while our robustness bandwidths (circular markers) are constant across these groups. Dashed lines represent 95 percent confidence intervals using standard errors clustered at the school level. Data: SBEC and TEA.

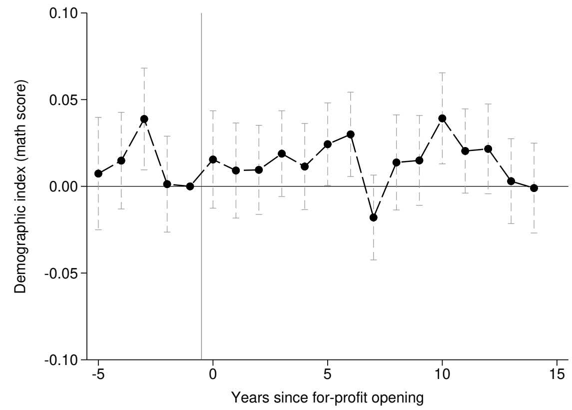


Panel A. For-profit certification

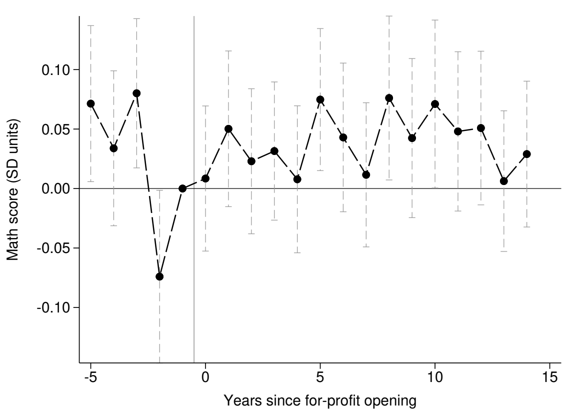
Panel B. Standard certification



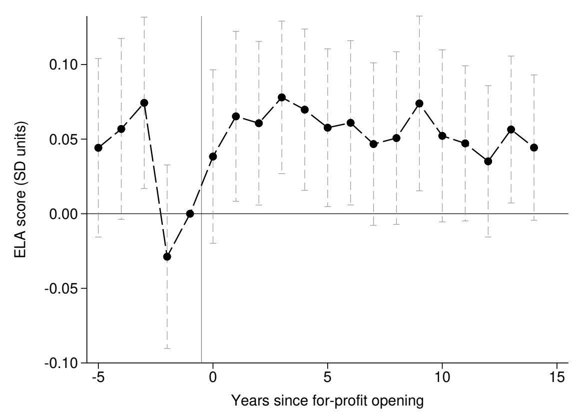
Panel C. No certification



Panel D. Demographic index (Math score)



Panel E. Math score (SD units)



Panel F. ELA score (SD units)

Figure A9: RD-DiD effects of flexible EPP requirements — Event studies

Notes: This figure displays event study versions of our RD-DiD estimates. For this specification, we replace the Post_p indicators in equation (3) with dummies for teacher departure years y *relative* to the adoption of the flexible EPP requirements (2002), omitting the dummy for $y = -1$. This yields θ_y coefficients on the interactions between Treated_g and the relative departure year dummies. These coefficients represent the differential change in the effects of teacher departures in middle and elementary school grade levels between 2002 and year y. Circular markers represent the θ_y coefficients from this event study specification. Dashed lines represent 95 percent confidence intervals using standard errors clustered at the school level. Data: SBEC and TEA.

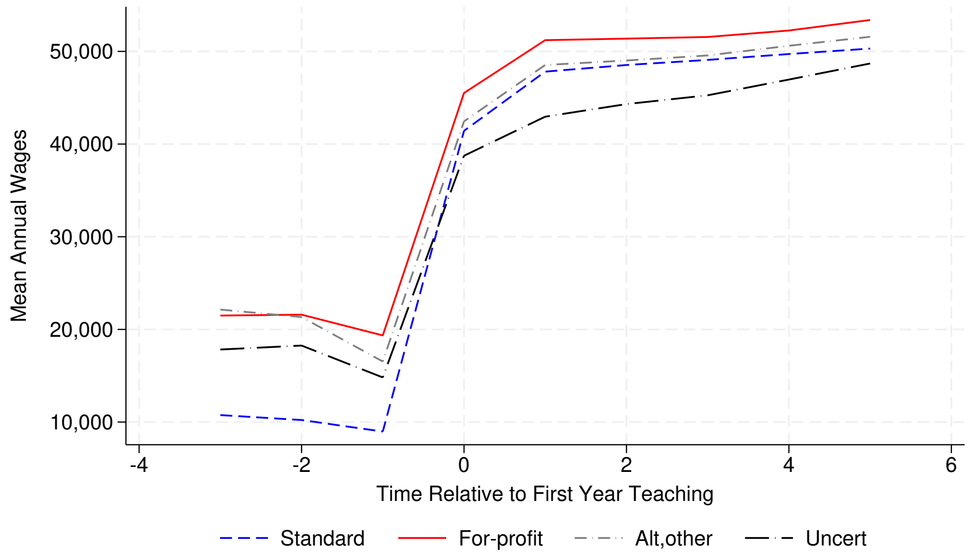


Figure A10: Annual wages pre- and post-entering the teaching profession

Notes: This figure plots the mean annual wage across all UI covered employment by certification pathway pre- and post-entering the teaching profession (time = 0). The sample includes teachers who were in their first year teaching from 1995–2019 and corresponding wages from years 1992–2021. For annual wages, we sum Q3 and Q4 total wages of the fall academic year and Q1 and Q2 total wages of the current year. For example, for the academic year 2010–2011, wages for the current year would be (Q1 + Q2 wages in 2011) + (Q3 + Q4 wages in 2010). The wages for the year prior to that would be (Q1 + Q2 wages in 2010) + (Q3 + Q4 wages in 2009). Wages come from the Texas Workforce Commission (TWC) and represent all UI covered employment (teaching or otherwise). Missing data are treated as zero earnings. There may be inherent data measurement issues in the cases where teachers start at times besides in the fall of an academic year, though these are less common. Wages are normalized to 2019$ using the CPI. Uncertified teachers are those that are uncertified in their first year. Data: TEA, SBEC, and TWC.

Table A1: List of for-profit EPPs by year of opening

	(A)	(B)	(C)	(D)	(E)
#	EPP name	City	County	Year opened	Total initial certs by 2019
1	Education Career Alternatives Program	N Richland Hills	**Tarrant County**	2001	10,603
2	ACT-Rio Grande Valley	Pharr	**Hidalgo County**	2001	4,874
3	Alternative-South Texas Educator Program	Brownsville	**Cameron County**	2002	2,740
4	iteachTexas	Denton	**Denton County**	2003	20,378
5	ACT-Houston	Houston	**Harris County**	2004	8,588
6	Steps to Teaching - ACP	Pharr	Hidalgo County	2004	438
7	Teachers for the 21st Century	El Paso	**El Paso County**	2004	293
8	A+ Texas Teachers	Houston	Harris County	2005	57,654
9	Web-Centric Alternative Cert Program	Cypress	Harris County	2005	4,689
10	Teachworthy	San Antonio	**Bexar County**	2005	3,459
11	Teacherbuilder.com	Edinburg	Hidalgo County	2005	2,653
12	Texas Alternative Certification Program	El Paso	El Paso County	2005	1,640
13	South Texas Transition to Teaching ACP	Edinburg	Hidalgo County	2005	1,434
14	A Career in Teaching-Epp (Corpus Christi)	Corpus Christi	**Nueces County**	2005	1,172
15	Quality ACT: Alternative Certified Tchrs	Irving	**Dallas County**	2005	1,006
16	Training Via E-Learning: An Alt Crt Hybr	Austin	**Travis County**	2005	485
17	ATC-East Houston	Houston	Harris County	2006	67
18	A Career in Education-ACP	Universal City	Bexar County	2008	148
19	ACT-Houston at Dallas	Dallas	Dallas County	2009	2,312
20	A+ Texas Teachers (Dallas)	Dallas	Dallas County	2009	547
21	A Career in Teaching-Epp (Mcallen)	Mcallen	Hidalgo County	2009	543
22	A+ Texas Teachers (San Antonio)	San Antonio	Bexar County	2009	435
23	ACT-Central Texas - Temple	Temple	**Bell County**	2009	400
24	A+ Texas Teachers (Austin)	Austin	Travis County	2009	364
25	Alternative-So Tx Ed Pgm-Laredo (A-Step)	Laredo	**Webb County**	2009	307
26	A+ Texas Teachers (Bedford/Fort Worth)	Bedford	Tarrant County	2009	265
27	EIT: Excellence in Teaching	Weslaco	Hidalgo County	2009	125
28	Texas Alternative Cert Pgm @ Austin	Leander	Travis County	2009	69
29	A Career in Teaching-Epp (Humble)	Humble	Harris County	2009	59
30	Texas Alternative Cert Pgm @ Brownsville	Brownsville	Cameron County	2010	477
31	Texas Alternative Cert Pgm @ Houston	Katy	Harris County	2010	26
32	ACT-Houston at Austin	Austin	Travis County	2010	5
33	Texas Alternative Cert Pgm @ San Antonio	San Antonio	Bexar County	2011	11

Notes: This table lists all the EPPs that we classify as for-profits with their year of opening (column D) and their total number of initial certifications through 2019 (column E). Bold text in column (C) indicates the first opening of a for-profit EPP in that county. Data: SBEC.

Table A2: Historical and modern EPP pricing and course requirements

	(A)	(B)	(C)	(D)	(E)	(F)	(G)	(H)	(I)	(J)	(K)
	Historical data (1999–2007)						Modern data (2024)				
EPP name	Year of data	In-person courses	Up-front fees	Intern fee	Total cost	Total cost ($2024)	Year of data	In-person courses	Up-front fees	Intern fees	Total cost
Panel A. For-profit programs											
ACT-Rio Grande Valley	2002	Yes	450	3,550	4,000	6,968	2024	Yes	900	4,700	5,600
A+ Texas Teachers							2024	No	299	4,700	4,999
Education Career Alternatives Program	2002	Yes	275	3,000	3,275	5,705	2024	Yes	990	3,500	4,490
iteachTexas	2004	No	300	3,700	4,000	6,641	2024	No	99	4,449	4,548
Average			342	3,417	3,758	6,438			572	4,337	4,909
Panel B. Other alternative programs											
Region 1 Education Service Center	2002	Yes	440	3,375	3,815	6,646	2024	Yes	350	6,245	6,595
Region 2 Education Service Center	1999	Yes	200	3,300	3,500	6,573	2024	Yes	1,150	4,700	5,850
Region 4 Education Service Center	2002	Yes	1,085	3,300	4,385	7,639	2024	No	100	5,484	5,584
Region 10 Education Service Center	2007	Yes	650	3,200	3,850	5,822	2024	No	699	4,550	5,249
Region 11 Education Service Center	2002	Yes	340	3,000	3,340	5,818	2024	No	2,975	2,975	5,950
Region 12 Education Service Center	2000	Yes	1,540	3,500	5,040	9,149					
Region 13 Education Service Center	2001	Yes	795	3,600	4,395	7,772	2024	Yes	1,900	4,475	6,375
Region 20 Education Service Center	2002	Yes	385	3,300	3,685	6,419	2024	Yes	311	5,284	5,595
Dallas ISD	2003	Yes	380	3,000	3,380	5,754	2024	Yes	890	4,165	5,055
Houston ISD	2000	Yes	1,040	3,750	4,790	8,695	2024	Yes	250	4,750	5,000
Average			686	3,333	4,018	7,029			958	4,736	5,695

Notes: This table displays information on the pricing and course requirements of for-profit (Panel A) and other alternative (Panel B) EPPs. The sample of EPPs includes the two largest for-profit EPPs (iteachTexas and A+ Texas Teachers), the two earliest for-profit EPPs (ACT-Rio Grande Valley and Education Career Alternatives Porgram), the two largest alternative EPPs operated by independent school districts (Dallas ISD and Houston ISD), and all EPPs run by Education Service Centers (ESCs) for which we could find historical information. These EPPs collectively represent the large majority of the alternative teacher certification market.

Columns (A)–(F) display information collected from historical versions of each EPP's website using archive.org; we use data from the earliest version of each website that we could find. Columns (G)–(K) display information from each EPP's website obtained in October 2024. We could not find a historical version of A+ Texas Teachers' website, and Region 12 ESC no longer offered an alternative certification program as of October 2024. Sources for all of this information are available from the authors upon request.

Columns (A) and (G) indicate the year for which we obtained data. Columns (B) and (H) indicate whether the EPP required some in-person courses prior to the teaching internship period; "No" indicates that all pre-internship training courses were online. Columns (C) and (I) indicate up-front program fees in nominal dollars, which typically include application fees and training fees. Columns (D) and (J) include fees due during the internship period in nominal dollars, which are typically paid out of the candidate's teaching paycheck. Some EPPs offer monthly payment plans; in this case we count the first month's payment as the up-front fee and all other monthly payments as the internship fee. If the EPP charges different prices for different teaching certificates, we report the cheapest option. We exclude other costs such as certification and testing fees. Columns (E) and (K) report the sum of the up-front and internship fees in nominal dollars. Column (F) converts the total cost in column (E) to 2024 dollars.

Table A3: Teacher training program requirements and characteristics (2013–2019)

	(A)	(B)	(C)	(D)	(E)
	Texas			Other states	
	Standard	For-profit	Other alternative	Standard	Alternative
Panel A. Enrollment and completion					
Total EPP enrollment	147,749	186,904	48,786	2,636,808	313,556
Total EPP completers	70,640	49,959	26,537	927,354	146,627
Share of state's EPP enrollment	0.38	0.49	0.13	0.89	0.11
Share of state's EPP completers	0.48	0.34	0.18	0.86	0.14
Program size (EPP completers per program)	139.3	320.2	46.4	95.7	42.7
Panel B. Characteristics of enrollees					
Female	0.81	0.66	0.71	0.77	0.68
Male	0.19	0.34	0.29	0.23	0.32
White	0.52	0.46	0.51	0.74	0.63
Asian	0.02	0.03	0.04	0.03	0.03
Black	0.07	0.23	0.16	0.08	0.17
Hispanic	0.36	0.26	0.27	0.11	0.12
Panel C. Undergraduate grade point average (GPA)					
Minimum GPA required for admission	2.70	2.50	2.61	2.81	2.72
Median GPA of individuals accepted	3.23	3.03	3.14	3.38	3.23
Panel D. Number of faculty					
# full-time faculty supervising clinical experience	16.1	76.8	7.0	20.8	5.9
# adjunct faculty supervising clinical experience	93.3	83.2	19.8	168.1	34.8
EPP completer/faculty ratio	9.2	23.5	12.2	5.7	11.0
Panel E. Training requirements					
# students in supervised clinical experience (SCE)	433.1	165.5	137.6	489.1	126.5
# students in SCE / # completers	1.30	0.22	0.99	1.84	1.25
Hours of SCE required prior to student teaching	243.0	124.7	140.9	166.8	73.4
Hours required for student teaching	566.9	505.7	799.9	538.5	366.4
Hours required for mentoring/induction support	32.6	34.9	59.7	19.6	123.5
Hours of content training (TEA data)	44.1	25.7	36.9		

Notes: This table shows summary statistics for teacher preparation programs in Texas (columns A–C) and all other U.S. states (columns D–E). We use teacher preparation program level data for the report years 2013–2019 from the U.S. Department of Education's Higher Education Act Title II State Report Card System. Columns (A) and (D) show statistics for standard programs. Columns (C) and (E) show statistics for alternative programs. We identify for-profit EPPs in Texas by their names and show results for these programs separately in column (B). Statistics in Panels C–E are completer-weighted averages across programs and years. See Appendix B.3 for details on data sources and variable definitions. Data: Title II and TEA.

Table A4: Median length of time (in days) from admissions to classroom by certification type

	For-Profit	Standard	Other Alternative
Admission to Content Exam	64	326	33
Content Exam to Certification	151	163	135
Certification to Classroom	22	146	29
Total	237	635	197

Notes: Median number of days for individuals from being admitted to an EPP, to taking their first content exam, to their certification effective date, to instructor of record by EPP type. We use Texas administrative data on EPP admissions between 2012–2019. We only have data available from 2012 to 2022 for the admissions and completions. For a teacher to eligible to contribute to these statistics, they had be observed teaching in a classroom up through 2019. The variables are defined as follows:

- Admission to content exam. Using the very first content (excludes PPR exams) exam ever attempted (not necessarily passed), we take the median difference in days among the administrative date of a teacher's first content exam and the date that the were admitted into an EPP program.
- Content exam to certification. Median difference in days between an individual's first certification effective date (using SBEC's minimum date) and the administrative date of their first attempted content exam.
- Certification to classroom. Median difference in days of between August 30th of the year in which a teacher is first observed in the teaching staff files and the minimum certification date.
- Total. Each median added together (not the median of the difference between admission and classroom).

Data: SBEC.

Table A5: Characteristics of first-year teachers, 1996–2001

	(A)	(B)	(C)	(D)	(E)
	Texas BAs	First-year teachers by certification route			
	All	All	Standard	Other alt.	No cert
Panel A. Demographics					
Male	0.44	0.25	0.16	0.28	0.33
White	0.69	0.69	0.82	0.68	0.52
Asian	0.05	0.01	0.01	0.02	0.01
Black	0.07	0.10	0.03	0.10	0.18
Hispanic	0.16	0.19	0.14	0.20	0.28
In grade 8 testing data	0.00	0.00	0.00	0.00	0.00
Grade 8 math score (SD units)					
Grade 8 ELA score (SD units)					
In college data	1.00	0.54	0.75	0.44	0.54
Age at certification		27.20	25.89	27.41	
N	548,863	96,024	33,866	19,571	32,497
Panel B. Distribution of college majors					
Business	0.22	0.04	0.01	0.11	0.05
Communication/Family Studies	0.07	0.04	0.02	0.08	0.04
Health	0.07	0.02	0.01	0.04	0.03
Humanities	0.13	0.21	0.17	0.25	0.25
Interdisciplinary	0.12	0.43	0.64	0.10	0.29
Parks/Leisure/Fitness	0.03	0.10	0.08	0.07	0.14
Social Sciences	0.12	0.06	0.02	0.18	0.07
STEM	0.16	0.06	0.03	0.10	0.09
Other	0.08	0.03	0.02	0.07	0.04
N	491,064	51,498	25,343	8,594	17,495
Panel C. Distribution of teaching grades					
Early childhood/Pre-kindergarten		0.02	0.02	0.02	0.02
Elementary school (grades K–5)		0.40	0.51	0.34	0.30
Middle school (grades 6-8)		0.21	0.21	0.19	0.23
High school (grades 9-12)		0.23	0.17	0.22	0.31
All grade levels		0.14	0.10	0.23	0.14
N		96,024	33,866	19,571	32,497
Panel D. Distribution of teaching fields					
Mathematics		0.08	0.07	0.08	0.09
English Language Arts (ELA)		0.13	0.13	0.14	0.12
Science		0.07	0.05	0.09	0.08
Social studies		0.07	0.06	0.07	0.07
Fine arts		0.05	0.06	0.03	0.06
Career & technical education		0.03	0.02	0.02	0.05
Special education		0.11	0.07	0.24	0.09
Bilingual students		0.06	0.04	0.09	0.08
English as a Second Language (ESL)		0.02	0.01	0.03	0.02
N		96,024	33,866	19,571	32,497
Panel E. Earnings					
Annual earnings one year prior to teaching ($2019)		12,310	8,758	16,349	13,583
Total annual salary in first teaching year		26,408	26,052	26,290	26,561
N (annual salary)		96,024	33,866	19,571	32,497

Notes: This table shows summary statistics for 1992–2001 Texas college graduates (column A) and 1996–2001 first-year teachers by certification route (columns B–E). Column (B) includes first-year teachers with out-of-state certification routes, which are not included in the remaining columns. Numbers are rounded to two decimals places, and thus values of 0.00 do not represent true zeroes. Data: TEA, THECB, and TWC.

Table A6: Math and ELA value-added by certification route controlling for experience

	(A)	(B)	(C)	(D)	(E)
Panel A. Math scores					
For-profit certification	-0.089***	-0.014***	-0.010***	-0.012***	-0.008***
	(0.008)	(0.003)	(0.003)	(0.002)	(0.002)
Other alternative certification	-0.062***	-0.003	0.000	-0.002	-0.000
	(0.007)	(0.003)	(0.002)	(0.002)	(0.002)
Out of state certification	0.066***	0.006*	-0.002	-0.005*	-0.001
	(0.010)	(0.003)	(0.003)	(0.003)	(0.003)
No certification	-0.287***	-0.078***	-0.047***	-0.034***	-0.015
	(0.026)	(0.013)	(0.012)	(0.011)	(0.013)
N (# of students)	9,029,412	9,029,412	9,029,412	9,029,412	9,029,412
Panel B. ELA scores					
For-profit certification	-0.090***	-0.002	0.002*	0.002	0.001
	(0.007)	(0.002)	(0.001)	(0.001)	(0.001)
Other alternative certification	-0.061***	-0.007***	-0.004***	-0.003**	-0.003***
	(0.006)	(0.001)	(0.001)	(0.001)	(0.001)
Out of state certification	0.093***	0.009***	0.001	-0.002	-0.000
	(0.009)	(0.002)	(0.001)	(0.001)	(0.001)
No certification	-0.251***	-0.024**	-0.003	0.005	0.003
	(0.024)	(0.010)	(0.009)	(0.009)	(0.010)
N(# of students)	9,449,937	9,449,937	9,449,937	9,449,937	9,449,937
Grade-year FE	x	x	x	x	
Student Covariates		x	x	x	x
Class Covariates			x	x	x
School Covariates			x		
School FE				x	
School-grade-year FE					x
Experience FE	x	x	x	x	x

Notes: This table reports the regression output described in Section B.4 for grade 4–8 teachers in 2012–2019. Column C is our preferred model. The top panel presents value-added differences across teacher training type for math standardized test scores, while the bottom panel reports them for ELA. Coefficients are interpreted in standardized test units relative to standard-trained teachers. All regressions control for experience level of the teacher in a given calendar year. Standard errors in parentheses are clustered at the school-level with * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Data: TEA and SBEC.

Table A7: Math and ELA value-added by certification route controlling for total length of time in the teaching profession

	(A)	(B)	(C)	(D)	(E)
Panel A. Math scores					
For-profit certification	-0.089***	-0.009***	-0.006**	-0.007***	-0.005**
	(0.008)	(0.003)	(0.003)	(0.002)	(0.002)
Other alternative certification	-0.062***	-0.001	0.002	-0.000	0.001
	(0.007)	(0.003)	(0.002)	(0.002)	(0.002)
Out of state certification	0.066***	0.007**	-0.001	-0.004	-0.001
	(0.010)	(0.003)	(0.003)	(0.003)	(0.003)
No certification	-0.287***	-0.069***	-0.039***	-0.029**	-0.011
	(0.026)	(0.013)	(0.013)	(0.011)	(0.013)
N (# of students)	9,029,412	9,029,412	9,029,412	9,029,412	9,029,412
Panel B. ELA scores					
For-profit certification	-0.082***	0.000	0.004***	0.004***	0.002**
	(0.007)	(0.002)	(0.001)	(0.001)	(0.001)
Other alternative certification	-0.056***	-0.006***	-0.003**	-0.002*	-0.003***
	(0.006)	(0.001)	(0.001)	(0.001)	(0.001)
Out of state certification	0.096***	0.010***	0.001	-0.001	0.000
	(0.009)	(0.002)	(0.001)	(0.001)	(0.001)
No certification	-0.229***	-0.020**	-0.001	0.008	0.003
	(0.024)	(0.009)	(0.009)	(0.009)	(0.010)
N (# of students)	9,449,937	9,449,937	9,449,937	9,449,937	9,449,937
Grade-year FE	x	x	x	x	
Student Covariates		x	x	x	x
Class Covariates			x	x	x
School Covariates			x		
School FE				x	
School-grade-year FE					x
Experience FE	x	x	x	x	x

Notes: This table reports the regression output described in Section B.4 for grade 4–8 teachers in 2012–2019. Column C is our preferred model. The top panel presents value-added differences across teacher training type for math standardized test scores, while the bottom panel reports them for ELA. Coefficients are interpreted in standardized test units relative to standard-trained teachers. All regressions control for the total length of time we observe the teacher in our sample (maximum experience level). Standard errors in parentheses are clustered at the school-level with * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Data: TEA and SBEC.

Table A8: Math and ELA value-added approximations by certification route, 1996–2001 teachers

	(A)	(B)	(C)	(D)	(E)
Panel A. Math scores					
Other alternative certification	-0.565***	-0.037***	-0.003	-0.011	0.016
	(0.027)	(0.011)	(0.010)	(0.010)	(0.010)
Out of state certification	0.073**	-0.002	-0.003	-0.048***	-0.022*
	(0.034)	(0.014)	(0.012)	(0.011)	(0.011)
No certification	-1.074***	-0.191***	-0.117***	-0.111***	-0.086***
	(0.047)	(0.019)	(0.016)	(0.014)	(0.014)
Tot Obs	6,566,309	6,566,309	6,566,309	6,566,309	6,566,309
Panel B. ELA scores					
Other alternative certification	-0.643***	-0.044***	-0.018**	-0.021***	-0.001
	(0.026)	(0.009)	(0.007)	(0.007)	(0.008)
Out of state certification	0.175***	0.034***	-0.007	-0.035***	-0.020**
	(0.034)	(0.011)	(0.009)	(0.008)	(0.009)
No certification	-1.070***	-0.157***	-0.097***	-0.085***	-0.069***
	(0.044)	(0.017)	(0.014)	(0.011)	(0.011)
Tot Obs	6,566,309	6,566,309	6,566,309	6,566,309	6,566,309
Grade-year FE	x	x	x	x	
Student Covariates		x	x	x	x
School-grade-year Covariates			x	x	x
School Covariates			x		
School FE				x	
School-grade FE					x

Notes: This table approximates differences in value-added by certification route for a sample of grade 4–8 teachers in 1996–2001. We modify the value-added regression specification described in Section B.4 to be feasible for 1996–2001, when we can only link teachers to students at the school-grade-year level rather than at the classroom level. Specifically, we replace all classroom level covariates (e.g., averages of student demographics and lagged test scores) with school-grade-year level covariates. The covariates reported in the table—Other alternative certification, Out of state certification, and No certification—are the proportion of teachers in a school-grade-year cell from each certification route rather than indicators for individual teachers. We also use school-grade fixed effects in column (E) rather than school-grade-year fixed effects (as in column E of Table 4). All other covariates are the same as described as in Section B.4. The top panel presents value-added differences across teacher training type for math standardized exam scores, while the bottom panel reports them for ELA. Coefficients are interpreted in standardized test units relative to a school-grade-year cell with only standard-trained teachers. Standard errors in parentheses are clustered at the school-level with * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Data: TEA and SBEC.

Table A9: Opening of for-profit EPPs by county

(A)	(B)	(C)	(D)	(E)	(F)	(G)	(H)	(I)	(J)	(K)	(L)	(M)
		Total initial certifications		For-profit (FP) share of initial certifications								
County where EPP is located	First FP year	2000	2019	2000	2001	2002	2003	2004	2005	2009	2014	2019
Tarrant County	2001	861	834		0.41	0.47	0.46	0.48	0.48	0.37	0.42	0.43
Hidalgo County	2001	581	791		0.31	0.37	0.42	0.36	0.36	0.46	0.58	0.50
Cameron County	2002	195	95			0.24	0.47	0.64	0.64	0.38	0.32	>0.94
Denton County	2003	706	2,417				0.11	0.44	0.59	0.55	0.57	0.69
Harris County	2004	2,053	8,535					0.16	0.25	0.62	0.73	0.79
El Paso County	2004	491	355					0.01	0.29	0.19	0.15	0.05
Bexar County	2005	800	1,006						0.09	0.20	0.17	0.34
Nueces County	2005	314	196						0.09	0.33	0.19	0.22
Dallas County	2005	884	732						0.01	0.09	0.27	0.17
Travis County	2005	5,100	4,592						<0.01	0.01	0.01	<0.01
Bell County	2009	113	129							0.30	0.41	<0.04
Webb County	2009	178	95							<0.02	0.44	<0.06
All other counties		5,810	4,977									

Notes: This table shows the 12 Texas counties that experienced a for-profit EPP opening (column A). Column (B) shows the first year that a for-profit EPP opened in that county. Columns (C)–(D) report the total number of initial certifications by all (for-profit and not for-profit) EPPs located in that county in 2000 and 2019. Columns (E)–(M) report the share of all initial certifications that were produced by for-profit EPPs for each year listed in the column header. Data: SBEC.

Table A10: Effects of flexible EPP requirements with τ_{ty} = -3 to +3 RD window

	(A)	(B)	(C)	(D)	(E)	(F)
		RD-DiD coefficients with different treated/control groups				
	Post-policy mean at τ_{ty} = -1	Middle vs elem. school	Counties w/ for-profit EPPs	Predicted FP teacher share	Predicted Alt teacher growth	Share of teachers w/ no cert
Panel A. Certification route						
For-profit certification	0.032	0.029***	0.028***	0.063***	0.043***	0.026***
		(0.003)	(0.003)	(0.003)	(0.004)	(0.003)
Standard certification	0.718	-0.041***	0.058***	0.027**	-0.009	0.049***
		(0.009)	(0.007)	(0.012)	(0.012)	(0.011)
Other alternative certification	0.163	0.028***	-0.056***	-0.022***	0.027***	-0.026***
		(0.006)	(0.006)	(0.009)	(0.009)	(0.008)
No certification	0.010	-0.009**	-0.015***	-0.042***	-0.043***	-0.051***
		(0.004)	(0.003)	(0.006)	(0.006)	(0.006)
Appropriate certification (if certified)	0.973	0.004	0.016***	0.029***	0.032***	0.047***
		(0.005)	(0.003)	(0.006)	(0.006)	(0.006)
Panel B. Teacher characteristics						
Number of teachers	2.254	-0.086***	-0.038*	0.035	0.000	-0.077**
		(0.027)	(0.023)	(0.039)	(0.037)	(0.032)
Male	0.114	0.011*	-0.022***	-0.004	0.008	-0.009
		(0.006)	(0.005)	(0.008)	(0.009)	(0.008)
White	0.690	0.004	0.020***	0.026***	0.008	0.011
		(0.007)	(0.006)	(0.010)	(0.010)	(0.009)
First-year teacher	0.040	0.017***	-0.027***	-0.010	0.000	-0.024***
		(0.006)	(0.005)	(0.008)	(0.009)	(0.008)
Total annual salary	46,679	-189	471***	100	-460**	441**
		(154)	(126)	(198)	(205)	(185)
Panel C. Student characteristics						
Number of exam takers	98.693	-0.857	-4.673***	-3.051	-1.516	-7.067***
		(1.125)	(0.951)	(2.196)	(1.946)	(1.427)
Demographic index (math score)	0.024	-0.001	0.011***	0.010*	0.009	-0.014**
		(0.004)	(0.004)	(0.006)	(0.006)	(0.006)
Panel D. Student achievement						
Math score (SD units)	-0.028	0.008	0.012	0.013	0.044***	0.038***
		(0.009)	(0.009)	(0.013)	(0.014)	(0.012)
Math score residual (SD units)	-0.022	0.018***	0.002	0.011	0.003	0.025***
		(0.007)	(0.006)	(0.010)	(0.010)	(0.009)
ELA score (SD units)	-0.012	-0.002	0.020***	0.028***	0.032***	0.010
		(0.008)	(0.007)	(0.011)	(0.011)	(0.010)
ELA score residual (SD units)	-0.008	0.012**	0.002	0.016**	0.010	0.007
		(0.006)	(0.005)	(0.008)	(0.008)	(0.007)
N (# *sty* observations)	18,227	162,453	470,708	79,296	79,142	111,897

Notes: This table displays estimates of the effects of flexible EPP requirements on teacher composition (Panels A–B), student characteristics (Panel C), and student achievement (Panel D) from an RD-DiD specification with a larger RD window. Column (A) shows the mean of each outcome in the year prior to the teacher departure ($\tau_{ty} = -1$) in school/grades that experienced a departure in the post-policy period (2002–2016). Columns (B) and (D)–(F) show θ coefficients from a modified version of our RD-DiD specification (4) in which we omit the running variables terms (τ_{ty} and $\mathbf{1}\{\tau_{ty} \geq 0\}\tau_{ty}$) and include only observations from $\tau_{ty} = -3$ to $+3$ in the regression sample. Column headers describe how we define the treatment indicator Treated$_g$ for each regression (see Section 6.2). Column (C) shows θ coefficients from the stacked RD-DiD specification (B2) described in Appendix B.5 with analogous modifications. Standard errors in parentheses are clustered at the school level with * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Data: SBEC and TEA.

Table A11: Effects of flexible EPP requirements on teacher and student characteristics

	(A)	(B)	(C)	(D)	(E)	(F)
		RD-DiD coefficients with different treated/control groups				
	Post-policy mean at τ_{ty} = -1	Middle vs elem. school	Counties w/ for-profit EPPs	Predicted FP teacher share	Predicted Alt teacher growth	Share of teachers w/ no cert
Panel A. Teacher characteristics						
Number of teachers	2.254	-0.024	-0.035	-0.012	0.032	-0.010
		(0.036)	(0.028)	(0.050)	(0.051)	(0.043)
Class size	18.887	-0.515	0.588**	-0.834*	0.975**	0.475
		(0.421)	(0.274)	(0.467)	(0.480)	(0.510)
Male	0.114	0.007	-0.018***	-0.009	0.007	-0.014
		(0.007)	(0.006)	(0.009)	(0.010)	(0.009)
White	0.690	0.003	0.010	0.019*	-0.009	-0.005
		(0.008)	(0.007)	(0.011)	(0.011)	(0.010)
Hispanic	0.193	-0.004	-0.004	-0.011	0.005	0.017**
		(0.006)	(0.006)	(0.008)	(0.009)	(0.008)
Black	0.105	0.002	-0.005	-0.010	-0.002	-0.011
		(0.006)	(0.005)	(0.008)	(0.009)	(0.008)
First-year teacher	0.040	0.011	-0.034***	-0.020	-0.011	-0.034**
		(0.011)	(0.009)	(0.015)	(0.015)	(0.014)
Years of teaching experience	15.230	-0.472*	0.270	-0.016	-0.971***	0.564*
		(0.264)	(0.225)	(0.360)	(0.365)	(0.332)
Total annual salary	46,679	23	913***	470	-551*	322
		(242)	(192)	(317)	(329)	(301)
Panel B. Student characteristics						
Number of exam takers	98.693	1.423	0.129	1.537	2.213	-3.267***
		(1.007)	(0.859)	(1.894)	(1.717)	(1.240)
Male	0.493	0.001	0.004*	0.005	-0.000	0.002
		(0.002)	(0.002)	(0.004)	(0.004)	(0.003)
White	0.382	-0.001	0.007***	0.003	0.005	0.001
		(0.003)	(0.003)	(0.005)	(0.004)	(0.003)
Hispanic	0.445	0.002	-0.003	-0.005	-0.002	-0.000
		(0.003)	(0.002)	(0.004)	(0.003)	(0.003)
Black	0.136	0.000	0.001	0.001	0.001	-0.000
		(0.002)	(0.001)	(0.002)	(0.002)	(0.002)
Economically disadvantaged	0.557	0.006	-0.004	0.004	0.002	0.016***
		(0.004)	(0.003)	(0.006)	(0.006)	(0.006)
At risk of dropping out	0.386	-0.016*	0.013	0.001	-0.001	0.020
		(0.009)	(0.008)	(0.012)	(0.013)	(0.013)
In gifted/talented program	0.108	-0.000	-0.003	0.004	0.001	-0.005
		(0.003)	(0.003)	(0.005)	(0.004)	(0.005)
Demographic index (math score)	0.024	0.001	0.000	0.006	-0.003	-0.009
		(0.005)	(0.005)	(0.007)	(0.007)	(0.008)
Demographic index (ELA score)	0.027	0.002	-0.000	0.006	0.004	-0.008
		(0.005)	(0.005)	(0.007)	(0.007)	(0.008)
N (# *sty* observations)	18,227	204,431	592,589	99,879	99,573	140,813

Notes: This table displays RD-DiD estimates of the effects of flexible EPP requirements on teacher composition (Panel A) and student characteristics (Panel B). In Panel B, we use student demographics associated with math exam takers. Column (A) shows the mean of each outcome in the year prior to the teacher departure ($\tau_{ty} = -1$) in school/grades that experienced a departure in the post-policy period (2002–2016). Columns (B) and (D)–(F) show RD-DiD coefficients θ from equation (3) with Treated$_g$ defined as listed in the column header (see Section 6.2). Column (C) shows θ coefficients from the stacked RD-DiD specification (B2) described in Appendix B.5. Standard errors in parentheses are clustered at the school level with * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Data: TEA.

Table A12: Heterogeneity in the effects of flexible EPP requirements on student achievement

	(A)	(B)	(C)	(D)	(E)	(F)
		RD-DiD coefficients with different treated/control groups				
	Post-policy mean at τ_{ty} = -1	Middle vs elem. school	Counties w/ for-profit EPPs	Predicted FP teacher share	Predicted Alt teacher growth	Share of teachers w/ no cert
Panel A. Math scores						
White	0.296	0.006	0.013	0.002	0.006	0.011
		(0.014)	(0.014)	(0.019)	(0.020)	(0.018)
Black and Hispanic	-0.269	0.028*	0.017	-0.005	0.005	0.034*
		(0.016)	(0.013)	(0.025)	(0.024)	(0.020)
Economically disadvantaged	-0.284	0.029*	0.019	-0.013	0.008	0.028
		(0.015)	(0.012)	(0.023)	(0.023)	(0.019)
Not economically disadvantaged	0.308	0.008	0.011	0.004	0.008	0.016
		(0.014)	(0.013)	(0.019)	(0.019)	(0.017)
Male	-0.009	0.008	0.008	-0.011	0.003	0.025
		(0.014)	(0.012)	(0.019)	(0.019)	(0.017)
Female	-0.046	0.020	0.023*	0.008	0.007	0.012
		(0.014)	(0.012)	(0.019)	(0.019)	(0.017)
Panel B. ELA scores						
White	0.344	0.016	-0.005	0.032**	0.031*	0.000
		(0.011)	(0.010)	(0.015)	(0.017)	(0.013)
Black and Hispanic	-0.259	0.030**	0.000	0.015	0.034*	0.008
		(0.014)	(0.011)	(0.022)	(0.021)	(0.018)
Economically disadvantaged	-0.284	0.029**	-0.010	-0.001	0.018	0.004
		(0.014)	(0.011)	(0.020)	(0.020)	(0.018)
Not economically disadvantaged	0.359	0.020*	-0.002	0.034**	0.030*	-0.007
		(0.011)	(0.010)	(0.015)	(0.016)	(0.015)
Male	-0.071	0.036***	0.004	0.031*	0.040**	0.014
		(0.012)	(0.010)	(0.017)	(0.017)	(0.016)
Female	0.046	0.011	-0.004	0.011	0.011	-0.013
		(0.012)	(0.010)	(0.017)	(0.018)	(0.016)
N (# *sty* observations)	18,227	204,431	592,589	99,879	99,573	140,813

Notes: This table displays RD-DiD estimates of the effects of flexible EPP requirements on average math (Panel A) and ELA (Panel B) test scores by race/ethnicity, socioeconomic status, and gender. Column (A) shows the mean of each outcome in the year prior to the teacher departure ($\tau_{ty} = -1$) in school/grades that experienced a departure in the post-policy period (2002–2016). Columns (B) and (D)–(F) show RD-DiD coefficients θ from equation (3) with Treated$_g$ defined as listed in the column header (see Section 6.2). Column (C) shows θ coefficients from the stacked RD-DiD specification (B2) described in Appendix B.5. Standard errors in parentheses are clustered at the school level with * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Data: TEA.

Table A13: Effects of flexible EPP requirements on non-academic outcomes

	(A)	(B)	(C)	(D)	(E)	(F)
		RD-DiD coefficients with different treated/control groups				
	Post-policy mean at τ_{ty} = -1	Middle vs elem. school	Counties w/ for-profit EPPs	Predicted FP teacher share	Predicted Alt teacher growth	Share of teachers w/ no cert
Panel A. Grade retention						
Retained in grade	0.0135	-0.0003	-0.0003	0.0002	0.0010	0.0009
		(0.0010)	(0.0008)	(0.0013)	(0.0014)	(0.0013)
Panel B. Attendance						
Number of days absent	6.318	0.066	0.023	0.113	0.013	-0.080
		(0.060)	(0.054)	(0.084)	(0.088)	(0.097)
Number of days present	165.669	0.238	-0.052	-0.212	0.374	0.044
		(0.235)	(0.196)	(0.332)	(0.358)	(0.298)
Percent of days attended	0.9621	-0.0003	0.0000	-0.0006	0.0001	0.0004
		(0.0004)	(0.0004)	(0.0006)	(0.0006)	(0.0007)
Panel C. Disciplinary incidents						
Number of disciplinary incidents	0.602	-0.043	-0.042*	-0.016	-0.039	0.025
		(0.031)	(0.023)	(0.048)	(0.048)	(0.052)
Any suspension	0.193	-0.011	-0.007*	-0.009	-0.003	0.009
		(0.008)	(0.004)	(0.012)	(0.012)	(0.012)
Number of in-school suspensions	0.742	-0.091	0.030	-0.059	-0.064	0.050
		(0.057)	(0.032)	(0.080)	(0.088)	(0.100)
Number of out-of-school suspensions	0.261	-0.049**	-0.012	-0.062**	-0.055*	0.018
		(0.020)	(0.012)	(0.031)	(0.031)	(0.031)
N (# *sty* observations)	18,227	204,431	592,589	99,879	99,573	140,813

Notes: This table displays RD-DiD estimates of the effects of flexible EPP requirements on student grade retention (Panel A), attendance (Panel B), and disciplinary incidents (Panel C). Column (A) shows the mean of each outcome in the year prior to the teacher departure ($\tau_{ty} = -1$) in school/grades that experienced a departure in the post-policy period (2002–2016). Columns (B) and (D)–(F) show RD-DiD coefficients θ from equation (3) with Treated$_g$ defined as listed in the column header (see Section 6.2). Column (C) shows θ coefficients from the stacked RD-DiD specification (B2) described in Appendix B.5. Standard errors in parentheses are clustered at the school level with * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Data: TEA.

B Empirical Appendix

B.1 Data processing and samples. This subsection describes our processing of the Texas administrative data and the samples we use in the paper.

B.1.1 Teacher panel. We create a teacher-level panel dataset for use throughout our analyses. Specifically, we start with course schedule data from the TEA (the `peims/staff/class` files) for the years 1996–2019.[39] Teachers are identified by the `role` codes 25, 29, and 87. For teachers in this dataset, we observe the full-time-equivalent (FTE) years for each course taught and the corresponding subject, grade-level, and student population of the class (regular, special education, etc.).[40] We drop teaching observations with zero or missing FTE. We restrict our sample of teachers to those who work at instructional campuses (`camptype` = 1) in Independent School Districts (`disttype` = 4).

Using a unique personal identifier, we connect teachers' course schedules to their initial teaching certificate in the SBEC data. We use the 2022 certification file, which includes all certificates that SBEC has digitized, including some that go back to the 1950s. This data contains the EPP route associated with each certificate. We identify each individual's initial teaching certificate based on the earliest certificate observed in the data. A person is certified, with an associated EPP route, if their first certification was effective by November 1 of the school year in which they are teaching and any year thereafter. Most teachers are certified in their first year and consequently are permanently classified into that certification pathway. But given that school districts can acquire a permit or fill open teaching vacancies even without a permit (see Section 2), some teachers may not be certified by November 1 of the academic year. We refer to this as being uncertified in a given year.[41] We are also able to ascertain whether educators are teaching subjects and grades for which they are certified, which we refer to as appropriate certification. See the variable definitions in Appendix B.2 for details.

We connect these teachers to other employment data containing information on their experience and demographics (the `peims/staff/employ` files).[42] For a subset of teachers who also attended Texas public schools prior to becoming educators, we also observe their eighth grade math and ELA standardized exam scores in the TAAS, TAKS, and STAAR files. We can also identify the college majors of teachers who graduated with bachelor's degrees from 4-year public in Texas in the years 1992–2019, and for those who graduated from 4-year private colleges in Texas in the years 2003–2019 (the `thecb/report9` files).

B.1.2 Samples. We use two main samples that are subsets of our teacher panel. In Sections 4 and 5, we focus on teachers who were employed in 2012–2019, which are the years for which

[39] Although the course schedule data is also available in 1995, we begin in 1996 because this is the first year in which teaching grades and subjects are available.

[40] FTE is the proportion of full-time work for a given class for a given teacher. For example, a full-time person who spends half of their time teaching a class would have FTE = 0.5 for that class. All their classes would sum to one because they are full-time.

[41] About half of these initially uncertified teachers ultimately obtain certification at some point in their teaching career.

[42] We use experience-level defined by TEA. This variable has some measurement error. In our analyses of first-year teachers, we restrict to teachers who both have zero years of experience as reported by this variable and who also do not appear in the data in priors years to reduce measurement error.

we can compute teacher value-added estimates. Table 2 shows summary statistics for teachers in our panel who were in their first year of teaching in 2012–2019. Table 3 shows turnover for teachers in our panel who were in their first year of teaching in 2012–2019. Table 4 shows value-added estimates for all math and ELA teachers in our panel dataset from 2012–2019 for whom we can compute value-added (see Appendix B.4).

In Section 6, our sample includes teachers and students at school/grade/subjects for which one of the teachers in our panel departed between 1997–2016. See Appendix B.5.2 for details on how we define these teacher departures.

B.2 Variable definitions: Texas administrative data. This subsection provides details on how we define variables that are derived from Texas administrative data.

- **Age at certification.** Available for teachers who have a bachelor's degree. Calculated from the difference in number of years between bachelor's degree and certification and age at bachelor's degree conferral. Note this will inherently have some noise because we observe age at a point in time and not date of birth.

- **Annual earnings one year prior to teaching ($2019).** Total annual earnings reported in the year prior to the individual's first teaching year using 1992–2021 administrative data from the Texas Workforce Commission (TWC), converted to 2019 dollars.

- **Annual earnings in first year of teaching ($2019).** Total annual earnings reported in the individual's first teaching year using 1992–2021 administrative data from the Texas Workforce Commission (TWC), converted to 2019 dollars.

- **Appropriate certification (if certified).** We define teachers as having an appropriate certification if they have an active teaching certificate that matches the grade and subject they are teaching. A match on grade means that the teaching grade is contained within the allowable grade ranges for the teaching certificate. To define matches on teaching subject, we restrict to math, ELA, and generic teachers and require that the certificate is appropriate for these three subjects. Specifically, we define math/ELA teachers as appropriately certified if they have a Generalist, Self-Contained, Core Subjects, or Elementary certificate, or if they have a math/ELA-specific certificate. We define generic subject teachers as appropriately certified if they have a Generalist, Self-Contained, Core Subjects, or Elementary certificate, or if they have a math, ELA, science, social studies, or history specific certificate. We define teachers as appropriately certified only if their certificate is "active," meaning that the academic year in which they are teaching (November 1 through the following October 31) overlaps with the date range of the certificate (defined by the issuance/effective date and the expiration date). When we examine the outcome of appropriate certification, we exclude teachers with no certification.

- **College major.** THECB reports CIP (NCES maintained) codes for bachelor's completers. We take the 2 digit CIP code of a person's first conferred bachelor's degree and categorize it into similar major fields. When a person has multiple majors per initial bachelor's completion, we select one randomly to be representative. See Table B1 below for the mapping of CIP codes to broad major category.

Table B1: Broad major categories and 2-digit CIP codes

Major Category	CIP Code	Description
Business	52	Business, management, marketing, and related support services
Communication	9	Communication, journalism and related programs
	10	Communications technologies/technicians and support services
	19	Family and consumer sciences/ human sciences
	35	Interpersonal and social skills
	44	Public administration and social services professions
Health	34	Health-related knowledge and skills
	51	Health professions and related programs
	60	Health professions residency/fellowship programs
	61	Medical residency/fellowship programs
Humanities	16	Foreign languages, literatures, and linguistics
	23	English language literature/letters
	24	Liberal arts and sciences, general studies and humanities
	38	Philosophy and religious studies
	39	Theology and religious vocations
	50	Visual and performing arts
	54	History
Interdisciplinary	13	Education
	30	Interdisciplinary
Parks and Leisure	31	Parks, recreation, leisure, fitness, and kinesiology
Social Studies	5	Area, Ethnic, Cultural, Gender and Group Studies
	42	Psychology
	45	Social Sciences
STEM		
Computer Sci	11	Computer and information science and support services
Engineering	14	Engineering
	15	Engineering/engineering-related technologies/technicians
Math	27	Mathematics and statistics
Science	26	Biological and biomedical sciences
	40	Physical sciences
Other	1	Agriculture/Animal/Plant/Veterinary Science and related fields
	3	Natural resources and conservation
	4	Architecture and related services
	12	Culinary, entertainment, and personal services
	22	Legal professions and studies
	25	Library science
	28	Military science, leadership and operational art
	29	Military technologies and applied sciences
	32	Basic skills and developmental/remedial education
	34	Health-related knowledge and skills
	36	Leisure and recreational activities
	37	Personal awareness and self-improvement
	41	Science technologies/technicians
	43	Homeland security, law enforcement, firefighting and related protective services
	46	Construction trades
	47	Mechanic and repair technologies/technicians
	48	Precision production
	49	Transportation and materials moving

Notes: This table represents the aggregation of 2-digit CIP codes, based on 2020 specification, to broader major degree categories.

- **Demographic index (math/ELA score).** An index of predicted math/ELA scores (in SD units) based on a large vector of student covariates. The covariates are dummies for the full interaction of sex, race/ethnicity, economic disadvantage, at-risk of dropping out, special education, gifted education, grade, and year. The demographic indices are predicted values from regressions of math/ELA scores (standardized to the exam/year level) on these dummies, estimated separately for each test regime (TAAS, TAKS, and STAAR).

- **First-year teacher.** A teacher that meets two criteria: 1) They are in the first academic year in which we observe the teacher in the TEA staff files; and 2) the TEA employment files report that the teacher has 0 years of experience.

- **For-profit certification.** We define individuals as having for-profit certification if their initial teaching certificate is listed as an alternative EPP in the SBEC data (`org_type` = 14) and if the EPP name appears in the list of for-profit programs in King and Yin (2022). When in doubt, we verified the EPP's for-profit status with Google searches and archived websites. Appendix Table A1 lists the for-profit EPPs that we identified in the SBEC data.

- **Grade 8 math/ELA score (SD units).** The math/ELA score we have for a teachers 8th grade exams, standardized at the grade-year level. In the instance that a given individual had multiple, we averaged their test scores. See the definition of math/ELA scores below for details on the test scores.

- **Hours of content training (TEA data).** We average all non-missing values for the content hours variable provided in the SBEC admissions file. We restrict to admissions years being between 2018-2019 and require that their admission year is the same year or before the academic year of their initial certification by the EPP type associated with their initial certification.

- **In college data.** This variable is equal to one if we observe a bachelor's degree (in any conferral year between 1992–2019; 2003–2019 for private universities) for a given teacher.

- **In grade 8 testing data.** This takes a value of one if we have a (non-missing) value for either math or ELA 8th grade test score for either our set of BA earners or teachers.

- **Initial teacher certification.** We define an individual's initial teacher certification as the first teaching certificate that appears in the SBEC data based on the earlier of the issuance data and the effective date. "Teaching certificates" include one-year, intern, probationary, provisional, standard, and visiting teacher certificates; we do not count emergency permits, educational aide certificates, or other temporary permits as teaching certificates. We then classify initial certificates into one of four certification routes based on the EPP that is associated with their initial certification: Standard, for-profit, other alternative, and out of state. The associated EPP is identified using the organization name and certification program type provided by the SBEC. Teachers are defined as having a certificate through that route if they obtained their initial certification before November 1 of the year in which they are teaching; otherwise they are classified as having no certification. See elsewhere in this list for details on the definitions of each of the four certification routes and teachers with no certification.

- **Math/ELA score (SD units).** Scale scores on standardized grade 3–8 math and English Language Arts (ELA) achievement tests, standardized to mean zero and SD one within the population of individuals who took the same exam in the same year. For each student × year × subject exam, we use the first non-missing score. Scores are from the three testing regimes that existed in Texas during our time period: TAAS (1994–2002), TAKS (2003–2011), and STAAR (2012–2022). We use Texas Learning Index (TLI) and Normal Curve Equivalent (NCE) scores for the TAAS regime, and scale scores for the TAKS and STAAR regimes. Scores for some exam takers in grades 3–5 are from Spanish language versions of these tests.

- **Math/ELA score residuals (SD units).** Residuals from a regression of math/ELA scores in SD units (defined above) on a large vector of student, school × grade, and school level controls. The controls variables mirror those in Chetty et al. (2014a)'s teacher value-added specification, except we use school/grade level controls rather than classroom controls because we only observe students' classrooms from 2012 onward. Student-level controls include cubics in lagged math and ELA scores, sex, economic disadvantage, race/ethnicity dummies, at-risk of dropping out, gifted education, and indicators for missing values of the demographics variables. We also include school × grade and school averages of each of these variables. We interact all covariates with grade dummies, and include grade × year dummies in the regression. For this residual variable we include only students who have lagged test scores in both subjects, with the exception of grade 3 students since there are no grade 2 tests. Thus controls for grade 3 students include only those based on demographic variables, not lagged test scores.

- **No certification/Uncertified.** We define individuals as having no certification if we do not observe any initial teaching certification in the SBEC data by November 1 of the year they are teaching. Uncertified may change over time for a given teacher.

- **Number of exam takers.** The total number of students in the school/grade with non-missing math test scores.

- **Number of teachers.** The sum of full-time equivalent (FTE) years for all teaching assignments in a given school/grade.

- **Other alternative certification.** We define individuals as having other alternative certification if they have an initial teaching certification but it is not classified as standard, for-profit, or out of state (i.e., a residual category). This category includes certificates from alternative programs run by universities, independent school districts, Education Service Centers, and other non-profit organizations. This category also includes certificates from university-affiliated Post-Baccalaureate programs.

- **Out of state certification.** We define individuals as having out of state certification if their initial teaching certificate meets either of the following two criteria:

– Their certification is classified as a standard program (ce_pgm = 8) or an out of state program (ce_pgm = 4) and their the organization name is "State Board for Educator Certification" or "Texas Education Agency";[43]

– The certification program options variable indicates that the certificate is from out of state (ce_pgm_opt contains "Out of State");

- **Standard certification.** We define individuals as having standard certification if their initial teaching certificate is classified as a standard program in the SBEC data (ce_pgm = 8) and their certificate is not from out of state (defined elsewhere in this list).

- **Teacher turnover.** We calculate this only for teachers who were in their first year (see definition above) in 2012–2014 for Table 3 (the only group of teachers we could observe for 5 experience years with our dataset ending in 2019) or 2012–2019 for the Figure 5. Between years 2012–2019, if we observe our subset of teachers in the staff files, we sum their full-time equivalent across classes taught for that calendar year. If we do not see them the following year teaching any courses, we assign them a 0 full-time equivalent value in that year and for the remainder of the calendar years. For the table, we restrict to experience year 5, so that it takes a value of their full-time equivalent in year 5 or 0 if we do not observe them in year 5. For the figure, it is the average of the full-time equivalent and zeros across teachers in each certification group. For the corresponding appendix figure, we use 1 instead of FTE for anyone observed in the dataset and 0 once they leave.

- **Teaching grades.** Using the variable corresponding to grade-level or category for each class, we categorize classes according to the following distribution: "pre-kindergarten" is pre-kindergarten or early education; "elementary" is kindergarten through 5th grade when we observe specific grade-level or grade categories pre-k/kindergarten, G1-6, or Gk-6; "middle school" is grade 6-8 when we can define a grade or the grade category G6-8; "high school" is grade categories G7-12 or G9-12; all grade categories includes the other grade categories such as Gk-12. These are weighted at the class-level by the amount of full-time-equivalent a teacher spends on the class.

- **Teaching fields.** We use the TEA defined subject category for the class from the service ID variables provided. We assign them based on name ("math" variants are our math category, etc.). For Table 2 and Appendix Table A5, this excludes some categories including "self-contained" or technology. For student populations, we base special education (8), bilingual (2), and English as a secondary language (ESL) (7) on TEA's defined population served values (in parenthesis). To get the averages, we weight at the class-level by the amount of full-time-equivalent a teacher spends on the class.

- **Texas BA.** We take an individual's first observation of bachelor's degree, based on earliest conferral year. We observe bachelor's degrees comprehensively for public uni-

[43] ce_pgm = 4 is a rarely used code. More commonly, the certificate is classified as ce_pgm = 8 but the organization is listed as SBEC or TEA.

versities and colleges, health institutions and independent (private) colleges and universities operating in Texas. We use THECB's "level" variable (2 for public, health, and independent; 7 for public colleges) to define bachelor's versus other completed degrees (master's, associates, etc.). For this table, we restrict the initial bachelor's degrees to be conferred between 2012-2019.

- **Total annual salary.** This is the total base pay and other supplemental pay for a teacher in a year. The average salary in district-year-grade-level is a weighted average of salary using the full-time-equivalent of courses taught among teachers in each bin. Elementary comprises of courses that are grades 1-5 or in grade groupings: "prek/k", "G1-6", or "Gk-6". Middle school comprises of courses that are grades 6-8 or grouping: "G6-8".
- **Years of teaching experience.** TEA defined years of experience for a teacher in a given year.

B.3 Data sources and variable definitions: Public use data. This subsection provides details on data sources and variable definitions for variables that are derived from public use datasets. Our public use data are from the following sources.

- **Census.** Intercensal population estimates by state, age, and race. Downloaded in May 2024 from:
  - https://www2.census.gov/programs-surveys/popest/tables/1990-2000/state/asrh/
  - https://www.census.gov/data/datasets/time-series/demo/popest/intercensal-2000-2010-state.html
  - https://www2.census.gov/programs-surveys/popest/datasets/2010-2020/state/asrh/
  - https://www2.census.gov/programs-surveys/popest/datasets/2020-2022/state/asrh/
- **Common Core.** State-level estimates of the number of full-time equivalent public school teachers (1990–2019) and average teacher salaries in public schools (1992–2019) from the National Center for Education Statistics (NCES) Digest of Education Statistics. Downloaded in May 2024 from:
  - https://nces.ed.gov/programs/digest/
- **SASS/NTPS.** National Center for Education Statistics' Schools and Staffing Survey (SASS) and National Teacher and Principal Survey (NTPS). Data are from the public schools and public school teachers surveys for the 1999–2000, 2003–2004, 2007–2008, 2011–2012, 2015–2016, and 2017–2018 survey waves. Downloaded in August 2023 from:
  - https://nces.ed.gov/datalab/sass
- **Title II.** State and EPP level data for 2000–2019 from the U.S. Department of Education's Higher Education Act Title II State Report Card System. Downloaded in August 2023 (first two links) and October 2024 (third link) from:

– https://title2.ed.gov/Public/DataTools/Tables.aspx
– https://title2.ed.gov/Public/SecReport.aspx
– https://title2.ed.gov/Public/Report/DataFiles/DataFiles.aspx?p=5_01

The variables derived from these public use datasets are:

- **# adjunct faculty supervising clinical experience (Title II).** Number of full-time equivalent faculty supervising clinical experience (institution of higher education and PreK–12 staff) during this academic year.
- **# full-time faculty supervising clinical experience (Title II).** Number of full-time equivalent faculty supervising clinical experience during this academic year.
- **# students in SCE / # completers (Title II).** Number of students in supervised clinical experience during this academic year divided by the total number of teacher preparation program completers.
- **# students in supervised clinical experience (SCE) (Title II).** Number of students in supervised clinical experience during this academic year.
- **Age when first started teaching (SASS/NTPS).** Age when first started teaching.
- **Alternative EPP completers per 10K pop. (Title II).** Total number of alternative teacher training program completers by state and academic year, divided by the number of state residents between the ages of 18–65 (in 10,000s) from intercensal U.S. Census estimates.
- **Entered teaching through alternative EPP (SASS/NTPS).** Proportion of teachers who entered teaching through an alternative certification program.
- **EPP completer/faculty ratio (Title II).** Total number of teacher preparation program completers divided by the number of full-time + adjunct faculty supervising clinical experience.
- **EPP completers per 10K pop. (Title II).** Total number of traditional + alternative teacher training program completers by state and academic year, divided by the number of state residents between the ages of 18–65 (in 10,000s) from intercensal U.S. Census estimates.
- **Full-time teachers per 10K pop. (Common Core).** Number of full-time equivalent teachers by state and academic year, divided by the number of state residents between the ages of 18–65 (in 10,000s) from intercensal U.S. Census estimates.
- **Felt prepared to assess students (SASS/NTPS).** Percentage of teachers who felt well prepared or very well prepared for the following situations in their first year of teaching: 1) to assess students; 2) to differentiate instruction in classroom; 3) to handle a range of classroom management; 5) to use a variety of instructional methods; 4) to teach their subject matter.

- **Full-time teachers per school (SASS/NTPS).** Estimated number of full-time equivalent teachers in the school.

- **Had any student teaching (SASS/NTPS).** Percentage of teachers who had any student teaching prior to beginning teaching.

- **Has taught 3 or fewer years (SASS/NTPS).** Proportion of teachers who have taught three or fewer years.

- **Hours of SCE required prior to student teaching (Title II).** Average number of clock hours of supervised clinical experience required prior to student teaching.

- **Hours required for mentoring/induction support (Title II).** Average number of clock hours required for mentoring/induction support

- **Hours required for student teaching (Title II).** Average number of clock hours required for student teaching.

- **Initial certifications per 10K pop. (Title II).** Total number of teachers receiving an initial teaching credential in the state, divided by the number of state residents between the ages of 18–65 (in 10,000s) from intercensal U.S. Census estimates.

- **Log average annual salary ($2019) (Common Core).** Natural log of the estimated average annual salary of teachers in public elementary and secondary schools by state and year. Converted to 2019 dollars using the May Consumer Price Index (CPI) values from the Bureau of Labor Statistics (BLS).[44]

- **Median GPA of individuals accepted (Title II).** Median GPA of individuals accepted into the teacher preparation program.

- **Minimum GPA required for admission (Title II).** Minimum GPA required for admission into the teacher preparation program).

- **Not currently certified in state (SASS/NTPS).** Proportion of teachers who don't currently hold a teaching certification in the state.

- **Program size (EPP completers per program) (Title II).** Average annual number of teacher preparation program completers per program.

- **Racial/ethnic minority (students) (SASS/NTPS).** Percentage of students in the school who are of a racial/ethnic minority.

- **Racial/ethnic minority (teachers) (SASS/NTPS).** Percentage of teachers who are of a racial/ethnic minority.

- **Share of state's EPP completers (Title II).** Share of teacher preparation program completers in standard, alternative, and for-profit programs.

[44] BLS CPI data were downloaded in August 2025 from: https://data.bls.gov/timeseries/CUUR0000SA0.

- **Share of state's EPP enrollment (Title II).** Share of teacher preparation program enrollees in standard, alternative, and for-profit programs.
- **Student/teacher ratio (SASS/NTPS).** Estimated number of students per full-time teacher in the school.
- **Total EPP completers (Title II).** Total number of teacher preparation program completers from 2013–2019.
- **Total EPP enrollment (Title II).** Total number of students enrolled in teacher preparation programs from 2013–2019. We also use enrollment counts by gender and race/ethnicity.
- **Very difficult to fill vacancy (SASS/NTPS).** Proportion of schools that found it very difficult or could not fill vacancies in the following areas: Elementary, Math, English, English as a Second Language (ESL), and Special Education.
- **Years of teaching experience (SASS/NTPS).** Teacher's total teaching experience in years.

B.4 Calculating value-added. Using data on more than four million students in grades 3-8 in math and ELA subjects, we link students and teachers via a classroom ID available for academic years 2012-2019. To obtain an estimate of the differences between certification pathways on student achievement, we estimate the following equation separately for each subject *sub* (math or ELA):

$$A^{sub}_{ijkgst} = \alpha_1 A^{sub}_{it-1} + \alpha_2 A^{-sub}_{it-1} + \gamma X_{it} + \lambda C_{kgst} + \nu_{gt} + \zeta S_{st} + \text{CertType}_{jt} + \epsilon_{ikgst} \quad \text{(B1)}$$

where A^{sub}_{ijkgst} is student i's standardized math or ELA score in year t, grade g, classroom k, and taught by teacher j in school s. Student i's A^{sub}_{it-1} and A^{-sub}_{it-1} represent lagged standardized math and ELA scores and their squares and cubes, and X_{it} are student characteristics (economic disadvantage, ethnicity/race, sex, whether they are in special education, whether they are at risk, and whether they are gifted).[45] Classroom characteristics, C_{kgst}, and school characteristics, S_{st}, include the mean individual characteristics, mean lagged standardized test scores in math and ELA and their squares and cubes for all students in classroom k and school s, respectively. We interact all student, class, and school-level controls with grade-level to allow for differences in effect across grades (Chetty et al., 2014a). To control for grade-year specific factors affecting all students, we include fixed-effects ν_{gt}. The dummies for certification type, CertType_{jt}, estimate student achievement gains relative to standard-trained teachers. Specifically we include dummies for whether a person is first certified through a for-profit, other alternative, out-of-state, or was not certified in the current year t. Certification status can change over time for some teachers. In some models we explore alternative controls such as grade-school-year FEs or additionally control for experience-level of the teacher in a given year (Kane et al., 2008). Finally, we cluster standard errors at the school-level. This estimation is represented in Tables 4, A6, and A7.

[45] We also include fixed-effects for student population type defined at the classroom-level interacted with grade.

Table B2: Teacher-year value-added summary statistics

	mean/(sd)	N (# of teacher-years)
Math VA	0.03	215,402
	(0.27)	
ELA VA	0.02	219,671
	(0.18)	

Notes: Mean and standard deviation of teacher-year value-added as calculated in part two of Section B.4. N represents the total number of observations. Data: TEA and SBEC.

To get teacher-year estimates of value-added, we make slight modifications. First we estimate equation B1 but replace CertType_{jt} dummies with a fixed effect at the teacher-level, μ_j. Then we perform the following separately for each subject:

1. Obtain residuals by taking the difference between the student test score and the estimates from the previous regression with all controls except the teacher dummy: $a_{it} = A_{it} - [\hat{\alpha}_1 A^{sub}_{it-1} + \hat{\alpha}_2 A^{-sub}_{it-1} + \hat{\gamma} X_{it} + \hat{\lambda} C_{kgst} + \nu_{gt} + \hat{\zeta} S_{st}]$

2. Then average the residuals among teacher j's students at the teacher-year: $\bar{A}_{jt} = \text{Mean}[a_{it} | i \in \{i : j(i,t) = j\}]$

$\bar{A}_{jt}$ is our estimate of teacher-year value-added. It is essentially the classroom-year residual where $\hat{\beta}$ is estimated using within teacher variation. See Table B2 for summary statistics. This follows Chetty et al. (2014a) by accounting for non-random sorting of students across teachers and its effect on the estimation of student, class, and school characteristics. We differ from Chetty et al. (2014a) by not completing their final step which calculates each $\bar{A}_{jt}$ as a function of other years' estimates (drift). We deviate because we prefer to have an estimate for teachers with only one year of teaching, which is not possible in their method. In practice, the estimates on value-added between the two approaches are highly correlated. Using the teacher-year value-added we collapse the mean value-added by experience-level and EPP type for teachers who were teaching between 2012-2019 for Figure 6 and A3 (the latter only includes teachers who have taught at least two years). The histograms in Figure A4 present the teacher-year value-added by EPP type.

B.5 RD-DiD specification. This section provides details on our regression discontinuity difference-in-differences (RD-DiD) specification that we use in Section 6

B.5.1 Definition of treated/control schools. In Table 6 (and Appendix Tables A10, A11, A12, and A13), we use five different definitions of treatment/control groups as defined by schools' exposure to the EPP policy changes.

- **Column (B): Middle vs. elementary schools.**
  - Treated group: Middle schools, defined as grades 6–8.
  - Control group: Elementary schools, defined as grades 3–5.
- **Column (C): Counties with for-profit EPPs.**

- Treated group: The 12 Texas counties that had an initial for-profit opening between 2001 and 2009 (see Appendix Table A9). We use the corresponding school districts' county variable assigned by the TEA to connect students/teachers to counties. Districts may over lap counties.
- Control group: The 191 Texas counties that never had a for-profit EPP and that do not border any of the 12 treated counties.

- **Column (D): Predicted for-profit teacher share.**
  - Treated group: Schools in the top quartile of the predicted share of 2011–2016 teachers with for-profit certifications.
  - Control group: Schools in the bottom quartile of this predicted share.

  To compute the predicted shares, we use a random forest model in which the outcome variable is the proportion of each school's teachers in the years 2011–2016 with for-profit certifications. We include 283 predictor variables, each of which is defined based on average characteristics of the school measured over the years 1996–2000. The model predictors are:

  - Teacher certification routes (5 variables): The proportion of teachers with standard, other alternative, out-of-state, or no certification. The proportion of teachers with appropriate certification (conditional on having any certification).
  - Teacher demographics (6 variables): The proportion of teachers who are White, Black, Hispanic, and male. The proportion of teacher's with bachelor's degrees and master's degrees.
  - Class size (1 variable): Average number of students per teaching assignment.
  - Teacher experience/pay (4 variables): The proportion of teachers in their first year of teaching, average years of teaching experience, average base pay, and average total pay.
  - Student demographics (8 variables): Proportion of students who took math achievement tests who are White, Black, Hispanic, male, economically-disadvantaged, at-risk of dropping out, and gifted.
  - Student achievement (3 variables): Number of students taking achievement tests, average math and ELA scores.
  - Grade (1 variable): (Student-weighted) average grade taught at the school.
  - School latitude and longitude (2 variables).
  - County dummies (253 variables): Fixed effects for the county where the school is located (omitting one county).

- **Column (E): Predicted alternative teacher growth.**
  - Treated group: Schools in the top quartile of the predicted *change* in the proportion of teachers with any alternative certification between 1996–2000 and 2011–2016.

- Control group: Schools in the bottom quartile of this predicted change.

We use the same random forest model and predictor variables as for column (D), but the outcome variable is the *change* in the proportion of each school's teachers with any alternative certification between 1996–2000 and 2011–2016.

- **Column (F): Share of teachers with no certification.**
  - Treated group: Schools in the top quartile of the proportion of teachers with no certification measured over the years 1996–2000.
  - Control group: Schools in the bottom quartile of this proportion.

Appendix Table B3 shows the predictor variables and variance importance for the random forest models for columns (D) and (E).

B.5.2 Definition of teacher departures. We define a *teacher departure* as an instance in which a teacher with ten or more years of experience leaves a given school. In other words, a departure occurs when a teacher with 10+ years of experience appears in a school one year but does not appear at that same school in the next year. Teachers that change subjects or grades within the same school are not counted as departures. We let t denote calendar years, y denote the year of the teacher departure, and τ_{ty} denote years relative to the departure. We define the departure year y as the first year that the teacher is no longer at the school, or, equivalently, $\tau_{ty} = 0$.

The sample of teacher departures that we include in our RD-DiD analysis includes two main restrictions. First, we consider only the departure of grade 3–5 generic subject teachers and grade 6–8 math or ELA teachers. We define a grade 3–5 teacher as a generic subject teacher if they taught any of the following subjects (with associated `subject` codes in the ERC data): General Science (8), Mathematics (10), English (22), Reading (27), Social Studies (38), and Generic (98). We combine all of these subjects into a single generic subject because many elementary teachers teach all of these core subjects, and in many cases the TEA data codes the teaching subject as Generic (98). For grade 6–8 teachers, we require that the departing teacher taught either Mathematics (`subject` = 10) or English/Reading (`subject` = 22 or 27), and we treat the two subjects separately for our analysis.

Second, we include only teachers who taught one third or more of the students in a given school, grade, and subject in the year prior to their departure year. We sum the teacher's full-time equivalent (FTE) years in the school/grade/subject, divide it by the total FTE in that school/grade/subject, and keep only departures in which the teacher's FTE is one-third or more of the total FTE. This restriction allows us to focus on instances in which the departing teacher causes a significant change in teacher composition at the school/grade/subject level, which is the level at which we can connect teachers to student test scores across all years of our data.

Teacher departures that meet both the subject and the FTE requirements are included in our RD-DiD analysis. We let s denote the school/grade/subject triplets that are associated with each teacher departure, and stack our dataset to include observations associated with each departure event as described in Section 6.1.[46]

[46] Note that a teacher can depart from multiple grades or subjects in the same year if they teach more

Table B3: Predictors for random forest models

	(A)	(B)	(C)	(D)
	Variable importance		OLS coefficients	
Predictor	FP share	Alt growth	FP share	Alt growth
No certification	0.894	2.579	0.025	0.121*
Standard certification	1.723	3.480	0.025	0.037
Other alternative certification	1.050	9.755	-0.013	-0.750***
Out of state certification	0.916	2.269	(omit)	(omit)
Appropriate certification (if certified)	1.613	3.151	-0.075*	-0.094
White	0.790	1.823	0.073	0.088
Hispanic	0.740	1.585	0.028	0.059
Black	0.634	4.567	-0.048	0.015
Male	0.935	2.212	0.014	0.011
Has bachelor's degree	0.205	0.425	0.087	0.072
Has master's degree	0.967	2.363	-0.015	-0.020
Class size (10s)	1.175	2.356	-0.007**	0.014***
Years of teaching experience	0.909	2.253	0.001	0.007***
First-year teacher	2.054	2.689	0.124***	0.110**
Years employed in position	0.907	2.245	0.000	-0.002
Base salary ($1000s)	0.960	2.867	0.008**	0.009
Total annual salary ($1000s)	0.922	3.004	-0.010**	-0.012**
White	2.086	3.689	0.000	-0.058
Hispanic	1.478	2.737	0.026	0.049
Black	1.844	9.017	0.157***	0.161***
Economically disadvantaged	1.068	2.541	0.016	0.020
Male	0.886	2.398	-0.062	-0.001
At risk of dropping out	1.170	2.888	0.056***	-0.002
In gifted/talented program	1.112	2.559	-0.041**	0.072***
Math score (SD units)	0.939	3.922	-0.035**	-0.085***
ELA score (SD units)	1.070	2.948	0.029*	-0.009
Number of exam takers (100s)	2.607	5.551	0.000	-0.001
Grade	3.997	6.675	0.022***	0.030***
School longitude	3.023	3.994	-0.044***	-0.043***
School latitude	2.394	2.531	-0.067***	-0.069***
County fixed effects (mean)	0.021	0.026	0.217	0.168
County fixed effects (max)	1.265	1.241	0.531	0.690
Name of county with max coef.	Tarrant	Harris	Bowie	Roberts
N (# of schools)	4,656	4,656	4,656	4,656

Notes: Columns (A)–(B) show variable importance statistics for our random forest models with outcomes: (A) the proportion of each school's teachers in the years 2011–2016 with for-profit certifications; and (B) the change in the proportion of each school's teachers with any alternative certification between 1996–2000 and 2011–2016. Columns (C)–(D) show coefficients from OLS regressions for these two outcomes. Covariates are average characteristics of the school measured over the years 1996–2000. Regressions are at the school level with observations weighted by the number of underlying individuals used to compute the outcome variable.

B.5.3 Regression specifications. This subsection provides details on our RD-DiD regression specifications.

In Table 6 (and Appendix Tables A10, A11, A12, and A13), column (B) and columns (D)–(F) display RD-DiD coefficients θ from equation (4) with Treated_g defined by the treated/control schools listed in the previous subsection and Post_p defined as an indicator for teacher departures in $y \in$ 2002–2016. As described in Sections 6.1–6.3, the intuition for equation (4) comes from a two-step specification. First, estimate the RD regression (2) separately for each pairwise combination of school exposure group $g \in \{\text{treated, control}\}$ and

than one grade or subject.

departure period $p \in \{1997\text{–}2001, 2002\text{–}2016\}$, which gives four RD coefficients β_{gp}. Second, use the resulting RD coefficients β_{gp} as dependent variables in the simple DiD regression (3).

For column (C) of Table 6 (and corresponding Appendix Tables), we use a different specification because this specification has staggered treatment adoption across counties as defined by the year in which the first for-profit EPP opened in the county (see Appendix Table A9). Thus we follow the intuition of Callaway and Sant'Anna (2021)'s approach to DiD with staggered adoption in using a "stacked" model with clean controls for each treated county. Specifically, we form pairwise groups G that include counties with initial for-profit openings in the same year (treated counties) and the 191 non-contiguous counties that never had a for-profit opening (clean controls).[47] We stack our dataset so that it includes treated counties and clean controls for each pairwise group G, and we define pre- and post-policy departure periods p based on the year of the initial for-profit opening for the treated counties in each pairwise group G. Finally, we include interaction terms for pairwise groups G so that the identification in our RD regressions is restricted to treated/control schools in the same pairwise group. Thus our full regression specification for column (C) is:

$$\begin{aligned} Y_{st} = &\Big(\phi \text{Treated}_g + \delta \text{Post}_p + \theta \text{Treated}_g \text{Post}_p\Big)\mathbf{1}\{\tau_{ty} \geq 0\} + \\ &\alpha_{gpG}\tau_{ty} + \psi_{gpG}\mathbf{1}\{\tau_{ty} \geq 0\}\tau_{ty} + \gamma_{syG} + \varepsilon_{styG} \quad \text{if } |\tau_{ty}| \leq h^Y. \end{aligned} \tag{B2}$$

This stacked specification has two differences from our benchmark RD-DiD specification (4). First, we allow the coefficients on the running variable, α_{gpG} and ψ_{gpG}, to vary with pairwise groups G (in addition to varying with treated/control groups g and pre/post-policy departure periods p, as in our benchmark specification). Second, we include school/grade/subject s × departure year y × pairwise group G fixed effects, γ_{syG} (as opposed school/grade/subject s × departure year y fixed effects, γ_{sy}, as in our benchmark specification). Intuitively, this stacked RD-DiD specification still estimates four RD coefficients β_{gp} defined by treated/control groups g and pre/post departures periods p (as in our benchmark specification), but in estimating these RD coefficients we restrict identification to comparisons within the same pairwise group G.

[47]For example, one pairwise group includes the two counties with initial for-profit openings in 2001 (Tarrant and Hidalgo) as well as all 191 clean control counties. Another pairwise group include the one county with an initial for-profit opening in 2002 (Cameron) plus all 191 clean control counties.

C Alternative Empirical Strategy for Student Impacts

C.1 DiD and triple-differences specification. This section presents an alternate empirical strategy for studying the impacts of exposure to for-profit EPPs on student achievement. This strategy uses two of the sources of variation in exposure to the EPP policy discussed in Section 6.2. First, we exploit geographic variation in EPP concentration by comparing the 12 counties in which a for-profit EPP opened to all 242 other counties with no for-profit EPPs (Appendix Table A9). Second, we exploit the greater concentration of for-profit certifications at higher grade levels (Appendix Table C1). As we show below, for-profits tended to open in areas with high population growth rates and growing Hispanic populations, and so variation in for-profit exposure within districts and across grades helps to address differential county trends.

We use these two sources of variation in DiD and triple-differences (DDD) specifications:

$$Y_{dtgp} = \gamma_{dgp} + \gamma_{tgp} + \beta \mathrm{FP}_{c(d)t} + \epsilon_{dtgp} \tag{C1}$$

$$Y_{dtgp} = \gamma_{dgp} + \gamma_{tgp} + \beta^{E} \mathrm{FP}_{c(d)t} + \theta[\mathrm{FP}_{c(d)t} \times \mathrm{Middle}_g] + \epsilon_{dtgp}. \tag{C2}$$

In these regressions, Y_{dtgp} is an average or total outcome for school district d, year t, grade level g, and pairwise group p (discussed below). Our sample includes outcomes measured from 1996–2019 for districts that operated continuously over this entire period. We focus on elementary and middle school grades because our main outcome—student test scores—is measured consistently over this period only for grades 3–8.[48]

Equation (C1) is our DiD specification that exploits only variation in the timing and geographic concentration of for-profit openings. This specification includes fixed effects for district × grade level × pairwise group triplets (γ_{dgp}) and year × grade level × pairwise group triplets (γ_{tgp}). The variable of interest, $\mathrm{FP}_{c(d)t}$, is a binary indicator for years t in or after any for-profit EPP opened in a school district's county $c(d)$. The DiD coefficient, β, indicates how educational outcomes changed in counties where a for-profit EPP opened relative to other counties. We separately estimate this equation for elementary, middle school, and both combined.

Our DDD specification, equation (C2), additionally uses variation in for-profit exposure across grade levels. This specification is similar to equation (C1), except we also include the interaction between $\mathrm{FP}_{c(d)t}$ and an indicator for middle school grades (6–8), Middle_g. In equation (C2), the β^E coefficient is identical to the DiD coefficient β that we get when we estimate equation (C1) in a sample that includes only elementary school grades (K–5). The DDD coefficient from equation (C2), θ, is equal to the *difference* between the DiD coefficients for middle and elementary school. We cluster standard errors at the county-level in both specifications.

The pairwise groups p in equations (C1) and (C2) address potential concerns about treat-

[48]Specifically, our sample includes teachers instructing grades K–8 (Table C2), but it only includes grade 3–8 for student achievement outcomes (Table C3). We include grades K–2 in our teacher regressions because we cannot always identify a teacher's exact grade in the early years of TEA data (although we can distinguish between elementary or middle school teachers). Texas also administered high school math and English exams during 1995–2019, but these were end-of-course exams in some years and end-of-grade exams in other years. Thus the test-taking populations and exam content are not consistent over our sample period.

ment effect heterogeneity in two-way fixed effects models (De Chaisemartin and d'Haultfoeuille, 2020). Each of our pairwise groups p contains a set of "treated" counties that experienced an initial for-profit opening in the same year and a set of "never treated" counties that did not have any for-profit opening.[49] We stack our dataset so that it contains all pairwise combinations of treated and never treated counties, and then interact our district $\times$ grade level (dg) and year $\times$ grade level (tg) fixed effects with dummies for these pairwise groups p. The resulting β and θ coefficients are regression-weighted averages of the pairwise treatment effects.[50] Our stacked estimates from equations (C1) and (C2) are similar to those using a simple two-way fixed effects model, primarily because we have a large number of never treated counties.

Our identification strategy relies on the usual DiD assumption of parallel trends. For our DiD coefficients, β, the parallel trends assumption requires that outcomes would have trended similarly in counties with and without for-profits in the absence of for-profit openings. For our DDD coefficients, θ, the key assumption is that the *difference* between middle and elementary school outcomes would have trended similarly across counties in the absence of for-profit openings. We present event study estimates for our β and θ coefficients to shed light on the plausibility of these assumptions.[51]

C.2 Results. Tables C2 and C3 show our DiD and DDD results for the effects of exposure to for-profit EPPs on teacher composition and student achievement. Column (A) in each table shows the mean of the dependent variable in 1996–2000. Columns (B)–(D) show DiD coefficients β from equation (C1) estimated separately for all grades (K–8), elementary school (grades K–5) and middle school (grades 6–8). Column (E) shows the DDD coefficient θ from equation (C2), which is equal to the difference between the coefficients in columns (D) and (C). Figures C1–C2 present corresponding event study estimates for our DiD specification. Figures C3–C4 show event studies for our DDD specification.

Consistent with our results in Section 6, we find no significant effects of exposure to for-profit EPPs on student achievement. Table C2 shows that greater exposure to for-profits

[49]For example, one of our pairwise groups contains Tarrant and Hidalgo Counties, which experienced a for-profit opening in 2001 (Appendix Table A9), and the full set of 242 never treated counties.

[50]Equations (C1) and (C2) are consistent with Callaway and Sant'Anna (2021) in that they implicitly estimate treatment effects separately for each set of treated counties (using never treated counties as the control group) and then average these treatment effects to recover a single point estimate. The only difference between our approach and that in Callaway and Sant'Anna (2021) is that our pairwise treatment effects are averaged using regression weights because we estimate everything in a single regression.

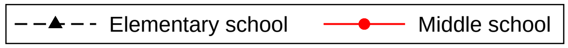
[51]Our event study specifications are:

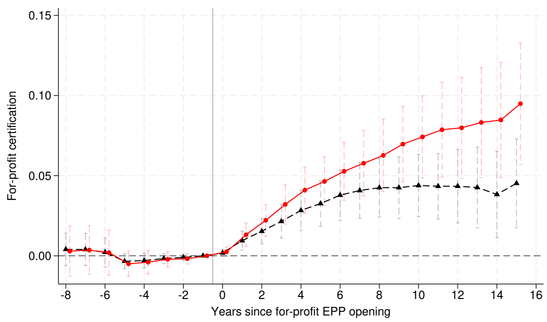
$$Y_{dtgp} = \gamma_{dgp} + \gamma_{tgp} + \sum_{\tau=-13}^{18} \beta_\tau \mathbb{1}\{t - t^*_{c(d)} = \tau\} + \epsilon_{dtgp} \tag{C3}$$

$$Y_{dtgp} = \gamma_{dgp} + \gamma_{tgp} + \sum_{\tau=-13}^{18} \beta^E_\tau \mathbb{1}\{t - t^*_{c(d)} = \tau\} + \sum_{\tau=-13}^{18} \theta_\tau [\mathbb{1}\{t - t^*_{c(d)} = \tau\} \times \text{Middle}_g] + \epsilon_{dtgp}, \tag{C4}$$

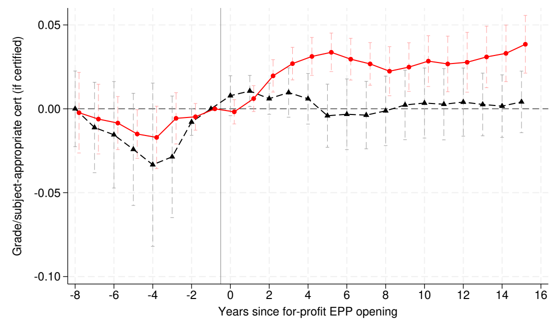
where $t^*_{c(d)}$ denotes the first year that a for-profit EPP opened in county $c(d)$, and τ indicates years relative to the initial for-profit opening. We include dummies for all possible years τ except $\tau = -1$, but we restrict our graphs to $-8 \le \tau \le 15$ because estimates the composition of treatment counties changes significantly outside this range. Figures C1 and C2 display estimates of β_τ from equation (C3) estimated separately for elementary and middle school. Figures C3 and C4 display estimates of θ_τ from equation (C4).

increased the proportion of teachers with for-profit certification and reduced the share of teacher with no certification or with inappropriate certification (though the effects on no certification do not vary between elementary and middle schools). In Table C3, we find positive and frequently statistically significant impacts on math and ELA scores using our DiD specifications. We are hesitant to place too much weight on these positive results given that these specifications are also associated with large increases in the number of students (although we do not see much evidence of imbalance in observable student characteristics). Our DDD specification in column (E) shows small and insignificant effects on student test scores. This specification also shows some evidence of imbalance on student characteristics, which is why we prefer the RD-DiD specification presented in Section 6.

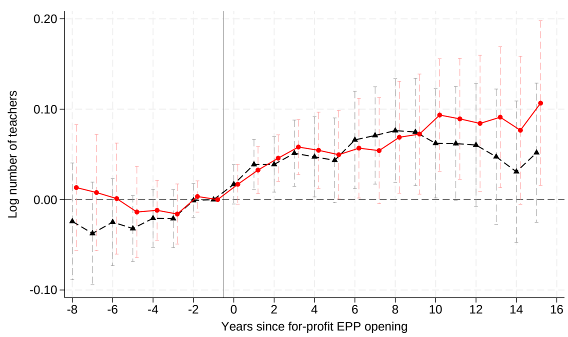




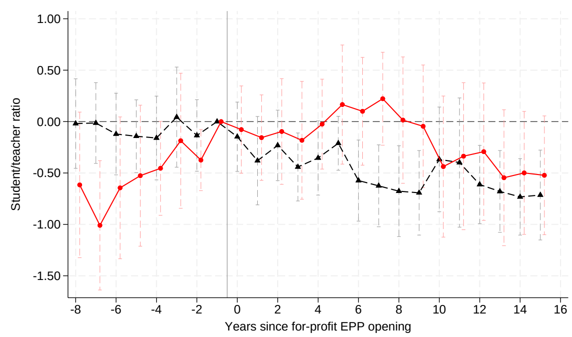
Panel A. Initial cert from for-profit EPP



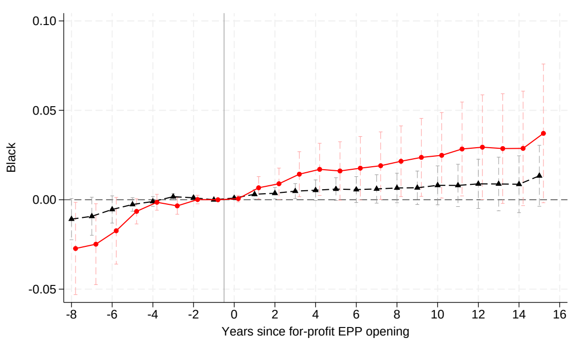
Panel B. Active grade- and subject-appropriate cert



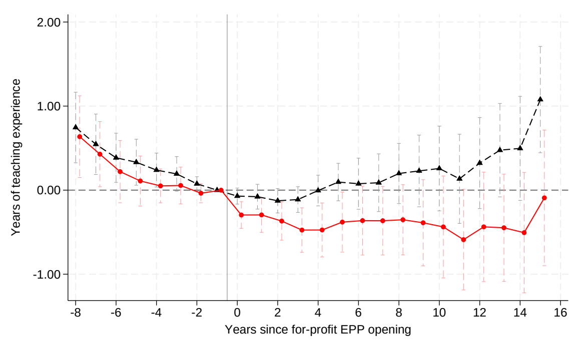
Panel C. Log number of teachers



Panel D. Student/teacher ratio



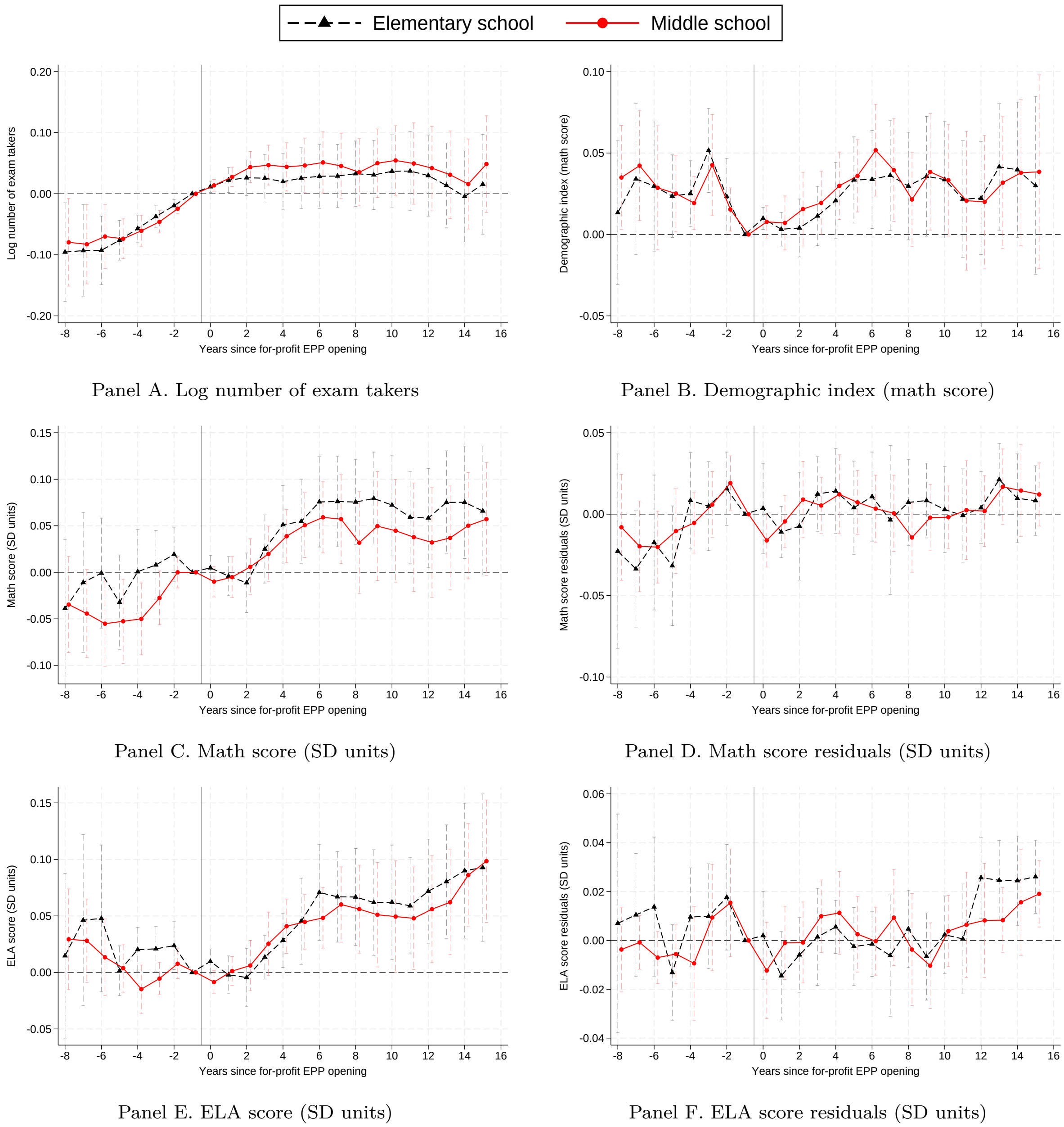
Panel E. Black



Panel F. Years of teaching experience

Figure C1: DiD event studies — Teacher composition

Notes: This figure plots DiD event study coefficients β_τ from equation (C3) estimated separately for elementary school (grades K–5, black dashed line) and middle school (grades 6–8, red solid line). Horizontal dashed lines are 95 percent confidence intervals using standard errors clustered at the county-level. Data: TEA and SBEC.



Panel A. Log number of exam takers

Panel B. Demographic index (math score)

Panel C. Math score (SD units)

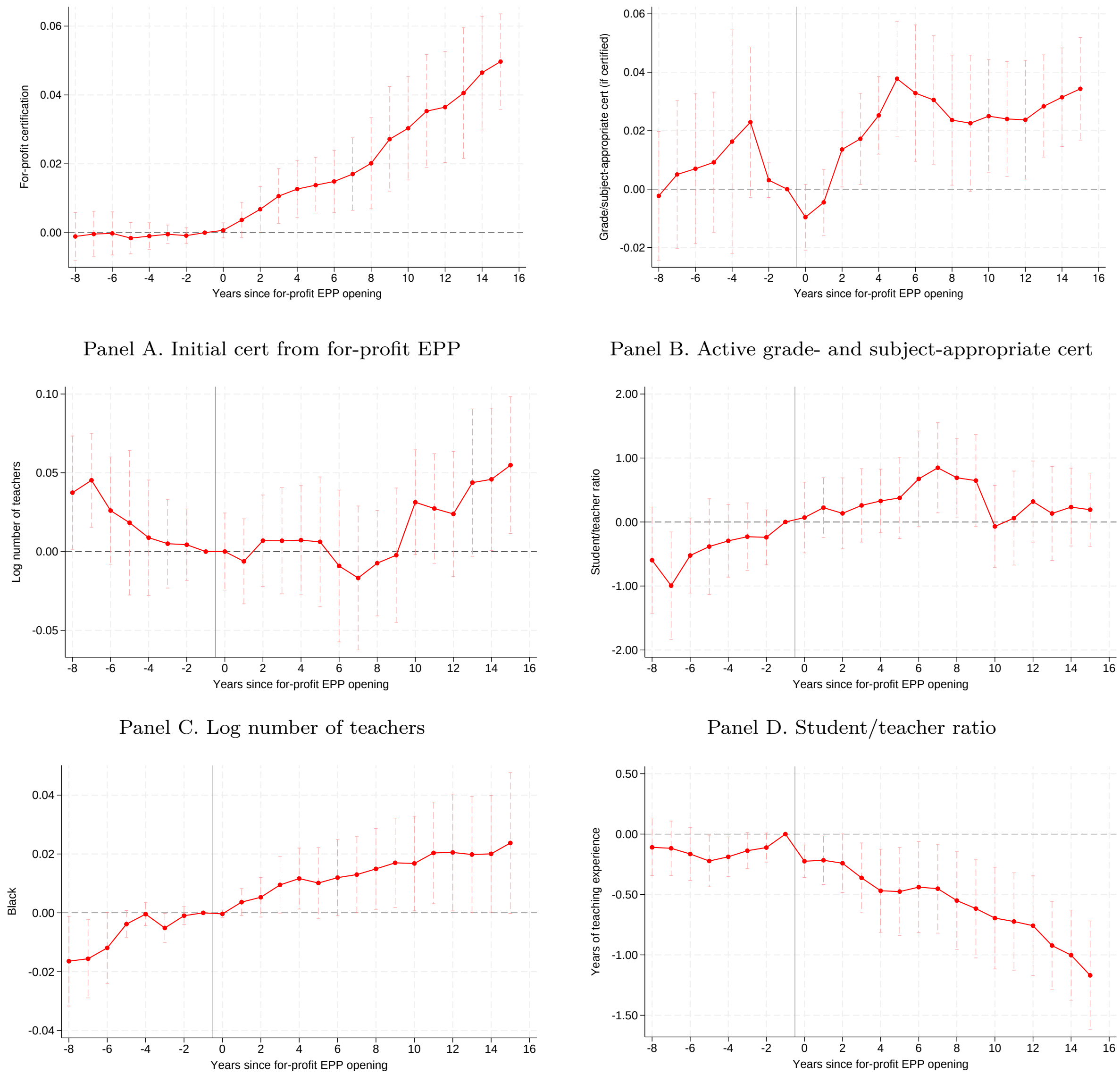
Panel D. Math score residuals (SD units)

Panel E. ELA score (SD units)

Panel F. ELA score residuals (SD units)

Figure C2: DiD event studies — Student achievement

Notes: This figure plots DiD event study coefficients β_τ from equation (C3) estimated separately for elementary school (grades 3–5, black dashed line) and middle school (grades 6–8, red solid line). Horizontal dashed lines are 95 percent confidence intervals using standard errors clustered at the county-level. Data: TEA and SBEC.



Panel A. Initial cert from for-profit EPP

Panel B. Active grade- and subject-appropriate cert

Panel C. Log number of teachers

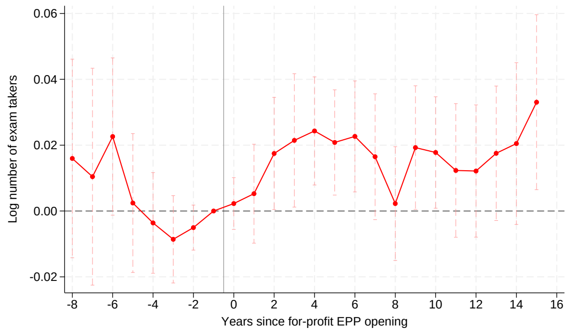
Panel D. Student/teacher ratio

Panel E. Black

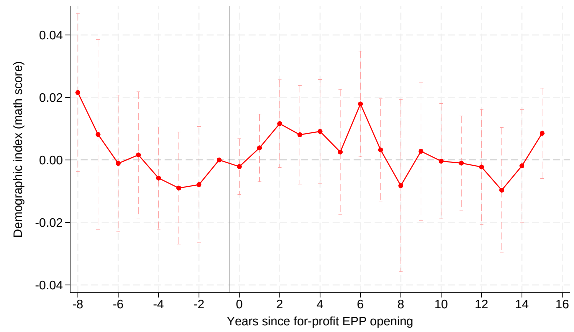
Panel F. Years of teaching experience

Figure C3: DDD event studies — Teacher composition

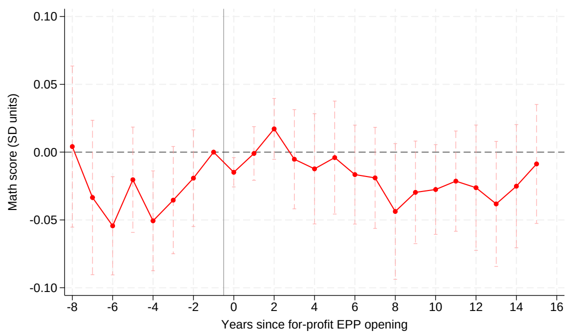
Notes: This figure plots DDD event study coefficients θ_τ from equation (C4). Horizontal dashed lines are 95 percent confidence intervals using standard errors clustered at the county-level. Data: TEA and SBEC.



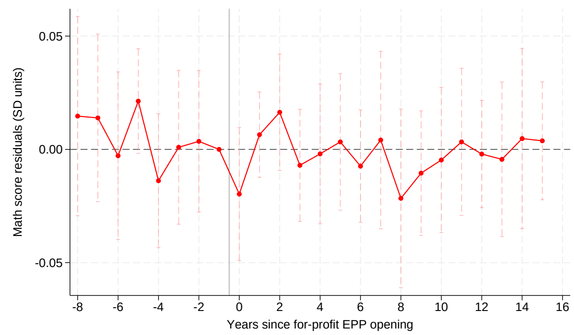
Panel A. Log number of exam takers



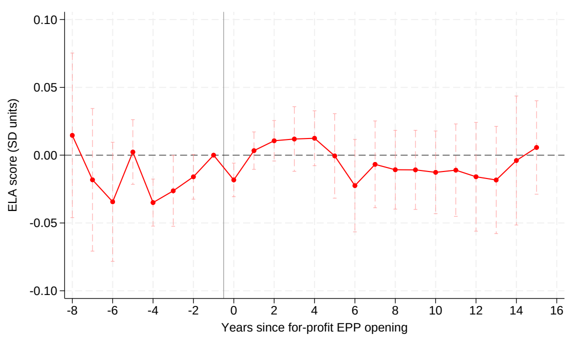
Panel B. Demographic index (math score)



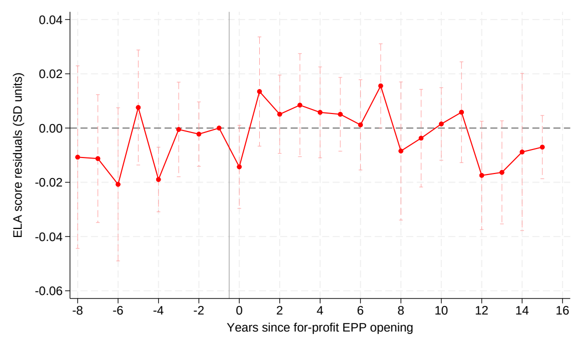
Panel C. Math score (SD units)



Panel D. Math score residuals (SD units)



Panel E. ELA score (SD units)



Panel F. ELA score residuals (SD units)

Figure C4: DDD event studies — Student achievement

Notes: This figure plots DDD event study coefficients θ_τ from equation (C4). Horizontal dashed lines are 95 percent confidence intervals using standard errors clustered at the county-level. Data: TEA and SBEC.

Table C1: For-profit shares of initial certifications by grade level

(A)	(B)	(C)	(D)	(E)	(F)
		Total initial certifications		For-profit share of initial certifications	
Certificate grade range	Years offered	2000–2009	2010–2019	2000–2009	2010–2019
PK–KG	2000–2004	514		<0.01	
EC–4	2000–2016	70,820	8,985	0.13	0.16
EC–6	2009–2019	258	94,629	<0.02	0.23
PK–6	2000–2005	7,713		0.08	
1–6	2000–2007	4,872		0.07	
1–8	2000–2008	27,594		<0.01	
4–8	2000–2019	44,943	43,688	0.20	0.36
PK–12	2000–2017	18,879	9	0.05	*
EC–12	2000–2019	30,192	54,352	0.29	0.42
6–12	2000–2019	37,756	9,199	0.10	0.50
7–12	2010–2019		23,186		0.47
8–12	2000–2019	31,112	21,241	0.25	0.34
Elementary school (K–5)		140,098	132,660	0.13	0.28
Middle school (6–8)		72,992	68,195	0.15	0.36
High school (9–12)		61,563	54,433	0.19	0.42

Notes: This table shows the for-profit share of initial certifications by grade level of the certificate. Column (A) lists the certificate grade ranges, and column (B) lists the years in which at least one certificate with that grade range was offered. Columns (C)–(D) report the total number of initial certifications by all (for-profit and not for-profit) EPPs for each grade range in 2000–2009 and 2010–2019. Columns (E)–(F) report the share of all initial certifications that were produced by for-profit EPPs for each grade range in 2000–2009 and 2010–2019. The last three rows report weighted totals/averages based on the proportion of each certificate grade range (column A) that overlaps with elementary (K–5), middle (6–8), and high (9–12) school grades. Values of for-profit shares (columns E–F) that correspond to fewer than five observations are censored. The asterisk (*) denotes that no value can be reported due to the small sample size. Data: SBEC.

Table C2: Effects of exposure to for-profit EPPs on teacher composition

	(A)	(B)	(C)	(D)	(E)
	Pre-2001 mean	DiD coefficients			DDD coef.
	All grades	All grades	Elem. school	Middle school	Middle – Elem.
Panel A. Certification route and status					
For-profit certification	0.000	0.041***	0.033***	0.058***	0.025***
		(0.009)	(0.008)	(0.011)	(0.006)
Standard certification	0.854	-0.021	-0.016	-0.034**	-0.019***
		(0.014)	(0.015)	(0.015)	(0.005)
Other alternative certification	0.068	0.001	0.004	-0.005	-0.009
		(0.012)	(0.013)	(0.012)	(0.006)
Out of state certification	0.049	-0.005	-0.006	-0.002	0.004
		(0.004)	(0.004)	(0.006)	(0.004)
No certification	0.030	-0.016***	-0.016***	-0.017**	-0.001
		(0.006)	(0.005)	(0.008)	(0.004)
Appropriate certification (if certified)	0.930	0.028***	0.017***	0.034***	0.017***
		(0.005)	(0.006)	(0.007)	(0.005)
Panel B. Number of teachers					
Log number of teachers	4.876	0.071**	0.073**	0.068*	-0.005
		(0.036)	(0.036)	(0.035)	(0.012)
Student/teacher ratio	17.564	-0.206	-0.430**	0.335	0.765***
		(0.171)	(0.180)	(0.247)	(0.235)
Panel C. Teacher characteristics					
Male	0.128	0.013***	0.013**	0.015**	0.002
		(0.005)	(0.005)	(0.006)	(0.004)
White	0.827	-0.078***	-0.078***	-0.080***	-0.002
		(0.012)	(0.013)	(0.011)	(0.008)
Hispanic	0.112	0.059***	0.065***	0.044***	-0.021
		(0.009)	(0.009)	(0.015)	(0.016)
Black	0.056	0.015*	0.009	0.029**	0.019**
		(0.008)	(0.006)	(0.014)	(0.009)
Panel D. Teacher experience					
First-year teacher	0.063	-0.010***	-0.012***	-0.006	0.006**
		(0.003)	(0.003)	(0.004)	(0.002)
Years of teaching experience	11.690	-0.215	-0.088	-0.516*	-0.427***
		(0.211)	(0.198)	(0.269)	(0.151)
Total annual salary	33,134	2,286***	2,371***	2,084***	-287*
		(423)	(443)	(393)	(161)
N (# districts/grade levels/years)	53,885	258,648	130,032	128,616	258,648

Notes: Column (A) shows the mean of each dependent variable in 1996–2000. Columns (B)–(D) present estimates of β from equation (C1) estimated separately for all grades (K–8), elementary school (grades K–5), and middle school (grades 6–8). Column (E) presents estimates of θ from equation (C2) estimated separately for all grades (K–8), elementary school (grades K–5), and middle school (grades 6–8). Standard errors in parentheses are clustered at the county level with * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Data: TEA and SBEC.

Table C3: Effects of exposure to for-profit EPPs on student achievement

	(A)	(B)	(C)	(D)	(E)
	Pre-2001 mean	DiD coefficients			DDD coef.
	All grades	All grades	Elem. school	Middle school	Middle – Elem.
Panel A. Student characteristics					
Log number of exam takers	7.222	0.089***	0.082**	0.096***	0.014**
		(0.033)	(0.033)	(0.033)	(0.007)
Male	0.502	0.003**	0.002*	0.003**	0.001
		(0.001)	(0.001)	(0.001)	(0.001)
White	0.584	0.004	0.006	0.002	-0.004*
		(0.018)	(0.018)	(0.018)	(0.002)
Hispanic	0.277	0.019	0.019	0.020	0.001
		(0.018)	(0.018)	(0.017)	(0.002)
Black	0.117	-0.011*	-0.013*	-0.009	0.004
		(0.006)	(0.007)	(0.006)	(0.003)
Economically disadvantaged	0.422	0.017	0.012	0.023	0.010***
		(0.021)	(0.020)	(0.023)	(0.004)
At risk of dropping out	0.319	0.020	0.027*	0.013	-0.014**
		(0.015)	(0.016)	(0.015)	(0.007)
In gifted/talented program	0.109	0.000	0.006	-0.006	-0.012
		(0.006)	(0.005)	(0.009)	(0.009)
Demographic index (math score)	0.041	0.004	0.004	0.004	-0.000
		(0.015)	(0.014)	(0.018)	(0.008)
Demographic index (ELA score)	0.048	-0.004	-0.009	0.002	0.012
		(0.018)	(0.017)	(0.020)	(0.008)
Panel B. Student achievement					
Math score (SD units)	0.051	0.065***	0.062***	0.068***	0.006
		(0.021)	(0.022)	(0.024)	(0.020)
Math score residuals (SD units)	0.002	0.010*	0.015*	0.007	-0.007
		(0.005)	(0.008)	(0.006)	(0.008)
ELA score (SD units)	0.045	0.038**	0.034	0.041**	0.006
		(0.019)	(0.022)	(0.019)	(0.017)
ELA score residuals (SD units)	-0.003	0.002	-0.000	0.004	0.004
		(0.003)	(0.007)	(0.004)	(0.008)
N (# districts/grade levels/years)	53,885	258,648	130,032	128,616	258,648

Notes: Column (A) shows the mean of each dependent variable in 1996–2000. Columns (B)–(D) present estimates of β from equation (C1) estimated separately for all grades (3–8), elementary school (grades 3–5), and middle school (grades 6–8). Column (E) presents estimates of θ from equation (C2) estimated separately for all grades (K–8), elementary school (grades K–5), and middle school (grades 6–8). Standard errors in parentheses are clustered at the county level with * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Data: TEA and SBEC.