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Teacher Licensing, Teacher Supply, and Student Achievement: Nationwide Implementation of edTPA

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Teacher Licensing, Teacher Supply, and Student Achievement: Nationwide Implementation of edTPA*

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April 28, 2023

Abstract

The debate on the stringency of licensure exams for prospective public school teachers is on-going, including the recent controversial roll-out of the educative Teacher Performance Assessment (edTPA). We leverage the quasi-experimental setting of different adoption timing by states and analyze multiple data sources containing a national sample of prospective teachers and students of new teachers in the US. With extensive controls of concurrent policies, we find that the edTPA reduced prospective teachers in undergraduate programs, less-selective and minority-concentrated universities. Contrary to the policy intention, we do not find evidence that edTPA increased student test scores.

Keywords: teacher licensing, edTPA, occupational licensing, teacher supply
JEL Classification: I28, J2, J44, K31, L51

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1 Introduction

The earliest call for teacher entry requirements in the US dates back to the 1960s following a concerning trend in student test scores (Rudner and Adelman, 1987). The underlying belief is that a minimum standard for public school teachers can enhance student learning. After decades of development, teacher licensure became the primary guarantee of teacher quality in U.S. public schools. Public school teachers also become the largest licensed profession in the US (Gittleman et al., 2018). Although teacher licensing is universal in the U.S. public sector, the requirements have been determined by the state legislature and they have varied substantially across jurisdictions (Kleiner, 2010). The complex historical development and a lack of concurrent national data create challenges to evaluating the impacts of licensure exams on teachers and their students on a nationwide scale.

The net effect of license exams is unclear: license requirements increase entry costs that reduce teacher availability and may distort investments; but a minimum standard of teachers may improve student learning by eliminating incompetent teachers or training teacher skills. Since 2014, the educative Teacher Performance Assessment (edTPA) – a performance-based examination to evaluate the teaching readiness of prospective teachers – has gained popularity across the nation. By 2018, edTPA had become a mandatory testing component for both program completion and initial teacher licensure in eight states. The rollout of edTPA provides a contemporaneous quasi-experimental setting to evaluate the effectiveness of teacher license exams.[1]

Unlike the traditional one-time written examinations, edTPA is a semester-long project involving lesson plans, classroom videos, and follow-up reports. The required money and time investment create an additional barrier to entry, potentially exacerbating the existing teacher shortages (Bergstrand Othman et al., 2017; Goldhaber et al., 2017; Petchauer et al., 2018; Gilbert and Kuo, 2019).[2] It is also an open question as to whether the assessment

[1]In total, 18 states recognized edTPA as a test option for initial teacher licensure in 2018. We define treatment states as those with edTPA being the only option. See Section 2 about the policy timing.

[2]A common alternative for pedagogy testing is Praxis PLT test, which costs around $150. By comparison,

benefits students. A higher requirement filters pre-service teachers at the lower tail of quality distribution, but may lead to negative sorting where higher ability candidates opt for better outside options (Goldhaber, 2007; Larsen et al., 2020).[3] The complementarity between the test content and quality of teaching is also a key for the new standard to benefit student learning. The overall impacts of edTPA then connect broadly to classical debates in economics about whether occupational licensing is welfare-improving (Friedman, 1962; Leland, 1979; Shapiro, 1986; Kleiner and Soltas, 2019).

This paper provides the first causal evidence about the effects of edTPA on teacher supply and student outcomes. We build on extant qualitative or case-specific analyses in education literature, providing a quantitative evaluation of edTPA using a national sample of new teachers and their students.[4] Controlling for an extensive set of concurrent policies, our identification strategy leverages different policy timing of edTPA, that compares the outcomes of interest in treatment states with other states before and after the implementation of edTPA. Our analysis not only applies to the ongoing debate about the implementation/revocation of edTPA but also speaks to the efficacy of teacher licensure and occupational licensing in general.

We first examine the number of graduates from teacher preparation programs – an important source of new teachers in the US public schools – documented in the Integrated Postsecondary Education Data (IPEDS).[5] Analyzing graduation years from 2011 to 2019, we find that edTPA reduced teacher graduates by a magnitude between 6.3% to 8.7%, depending on the empirical specifications. We also find that the negative effect primarily occurs in undergraduate programs, in less selective universities, and in minority-concentrated universities, suggesting issues associated with equity concerns and entry barriers created by

the administration cost of edTPA is $300, with additional $100-$300 if a retake is needed.

[3]Kugler and Sauer (2005) also documented that licensing induced negative selection in the physician profession.

[4]Related edTPA studies from education scholars include Greenblatt (2016), Goldhaber et al. (2017), Hébert (2019), and Gitomer et al. (2019).

[5]We find a similar result using the initial licensure data in the Title II. In our context, IPEDS has fewer measurement errors to identify the relevant teacher population. More discussion in the Data section.

edTPA. We are one of the first to document the employment/labor supply effect of teacher license exams (Kleiner and Petree, 1988; Larsen et al., 2020).[6]

We then assess the impact on students whose teachers likely went through edTPA. We analyze the restricted student data from 2009 to 2019 in the National Assessment of Educational Progress (NAEP) that contains the test scores of a national sample of students in the US. The NAEP is the largest nationally representative assessment in core subjects that provides a common yardstick to compare student progress in different states. Importantly for our analysis, the dataset links students to the years of experience of their corresponding subject teachers. This unique feature allows us to minimize measurement errors in identifying students of new teachers. We explore various specifications, sample criterion, and heterogeneity by school and student type. In all attempts, we do not find edTPA increased student test scores. Our confidence interval also dismisses a positive effect as small as 0.038 of a standard deviation for the overall test score. The current finding about students supplements the discussion about the potential merit/defect of this on-going debated policy (Gitomer et al., 2019; Goldhaber et al., 2017). In general, the null impact also echoes the study by Buddin and Zamarro (2009), who find that teacher licensure test performance is not related to student performance.[7]

Our results offer important empirical updates about occupational licensing. License regulations have become a major labor institutions in the US that affects one-third of the workers (Kleiner, 2010). Researchers generally found that licensing reduces employment (Blair and Chung, 2019; Chung, 2020), increases price/wage (Kleiner, 2000; Kleiner and Krueger, 2013; Thornton and Timmons, 2013), and has minimal improvement on quality (Carpenter and Dick, 2012; Kleiner et al., 2016; Farronato et al., 2020).[8] Most empirical work on licensing uses cross-sectional variation or historical data. As a socially-influential

[6]In the Schools and Staffing Survey (SASS) and the National Teacher and Principal Survey (NTPS), we also find that edTPA reduced the number of teachers who hold a regular license.

[7]As a supplementary analysis, in SASS/NTPS, we also observe that edTPA reduced subjective readiness of new teachers.

[8]The study by Anderson et al. (2020) is among the few to find a positive quality effect.

workforce and the largest licensed profession in the US, economists have endeavored to quantify the effects of teacher license exams. Results are mixed, which reflects the differences in research design and policy context.[9] For example, Goldhaber and Brewer (2000) analyze a national sample of 12th-grade teachers with their individual certificate status and find that students perform better under teachers who hold a standard license (compared to alternative types of certification). Kleiner and Petree (1988) exploit cross-sectional variation in 70s of state license requirements and find mixed effects of licensing on teachers' ability. Larsen et al. (2020) find that the license policies across the US during the 90s filtered lower-quality teachers.[10] We offer complementary evidence that can be generalized to the current teacher licensure reform, with a sharper identification, by looking at a controversial licensure initiative in recent years. The heterogeneity by the race of teacher candidates and program type also speaks to the distributional effect of licensing by population characteristics (Law and Marks, 2009; Blair and Chung, 2018, 2020; Xia, 2021).

We also document the extent to which the license policy is related to teacher shortages. The supply of new teachers has been declining in the recent 10 years (King and James, 2022). The significance of evolving license requirements on new teacher supply complements commonly-discussed factors, including monetary incentives (Goldhaber et al., 2015; Feng and Sass, 2018), work environment (Carter and Carter, 2000; Carroll et al., 2000), support from teacher programs (Liu et al., 2004), and other education reforms (Guarino et al., 2006; Kraft et al., 2020). We find a significant decline in teacher candidates who studied the traditional-route programs, which is the major source of new teachers in the US public schools (National Center for Education Statistics, 2022).

Lastly, our results speak to the unintended consequences of high-stake teacher assessments. The goal of performance-based evaluations in public schools is to improve teacher performance by providing incentives. Unfortunately, studies have found that

[9] State-specific studies include Clotfelter et al. (2007, 2010), Kane et al. (2008), and Sass (2015).

[10] Teacher quality is measured by the college background of the teachers. Angrist and Guryan (2008) also adopt a similar approach and find the state-mandated testing has no effects on teacher quality.

high-stakes on-the-job evaluations exerted pressure on teachers, hampering teacher recruitment and retention (Reback et al., 2014; Dee and Wyckoff, 2015; Sartain and Steinberg, 2016; Kraft et al., 2020; Cullen et al., 2021). We evaluate a new performance-based assessment for pre-service teachers and offer complementary findings that high-stake assessments dampen new teacher supply.

2 Background of edTPA

Licensure exams for prospective teachers in the US mostly cover three areas: basic skills (such as reading, writing, grammar, mathematics), subject matter, and pedagogical knowledge (Larsen et al., 2020). For pedagogical knowledge, the education community in the 1990s started to recognize the need for performance-based evaluation rather than written examinations to guarantee the teaching readiness of prospective teachers (Sato, 2014).

The earliest attempt to incorporate a performance evaluation process into the teacher licensure system was in 1998 in California.[11] Borrowing from the experience and models in California, the American Association of Colleges of Teacher Education (AACTE), which is the leading organization representing educator preparation programs in the US, cooperated with the Stanford Center for Assessment to develop a standardised assessment called the educative Teacher Performance Assessment (edTPA) for nation-wide adoptions. EdTPA is now administered by Pearson Education.

Unlike the usual form of written examinations, edTPA requires candidates to show competency in preparing classes by submitting detailed lesson plans, delivering instruction effectively by recording the lesson during the internship, and properly assessing student performance to guide future instruction via a thorough analysis of student learning outcomes. The experts at Pearson then score a candidate's materials in three areas: 'Planning for Instruction and Assessment', 'Instructing and Engaging Students in Learning', and

[11] The legislation is 'CA Senate Bill 2042'. Among a variety of models, popular options include the California Teaching Performance Assessment (CalTPA) and the Performance Assessment for California Teachers (PACT)

'Assessing Student Learning'.[12] Preparation for edTPA takes place alongside the teaching internship. The entire process can take months.

Some education scholars contend that this performance-based format better reflects the complexity of teaching better than written examinations and prepare teachers to focus on student learning (Darling-Hammond and Hyler, 2013). However, ample qualitative evidence suggests that edTPA discourages new teachers from entering the teaching profession. Gilbert and Kuo (2019) find that the test fee together with miscellaneous expenses add a significant burden to students who have already struggled financially. Bergstrand Othman et al. (2017) find that time commitment and the uncertainty about passing the exam created mental stress to the teacher candidates. Besides, Greenblatt (2016) and Shin (2019) suggest that teacher candidates often found themselves focusing too much on catching up the scoring rubrics and deadline at the expense of teaching opportunities. Worse still, the negative impacts fall disproportionately on minority and lower-income candidates (Greenblatt and O'Hara, 2015; Goldhaber et al., 2017; Petchauer et al., 2018).[13]

By 2018, eight states had implemented edTPA to evaluate teaching effectiveness for prospective public school teachers (see Figure 1).[14] Washington and New York were among the earliest states mandated edTPA as a necessary component for program completion and initial teacher licensure in January and May 2014, respectively. Prospective teachers have to satisfy a cutoff score to graduate from the teacher preparation program and qualify for a teacher license.[15] Later, the mandatory nature of edTPA expanded to Georgia (September 2015), Illinois (September 2015), Wisconsin (September 2016), New Jersey (September 2017), Alabama (September 2018) and Oregon (September 2018).[16]

[12] Interested readers can refer to the official edTPA document (`http://www.edtpa.com/Content/Docs/edTPAMGC.pdf`) for a more detailed description on the assessment scheme.

[13] From the 2019 official statistics of edTPA (`https://edtpa.org/resource_item/2019AR`), the average pass rate is between 72% and 93%. The pass rate for ethnic minorities is significantly lower than for their white counterparts.

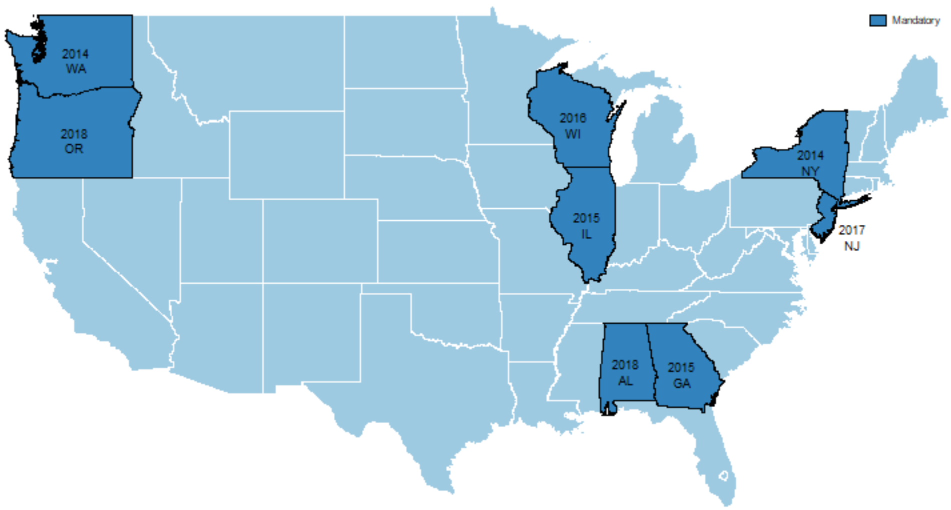
[14] Official document can be found here: `https://edtpa.org/resource_item/StatePolicyOverview`. We cross-check the mandatory nature in the official websites of state education departments.

[15] The cutoff scores vary by states and subjects. For a typical 15-Rubric criteria with a full score of 75, passing scores range from 35 to 42.

[16] New Jersey did not require a cutoff score until September 2019, and our results are robust to dropping

Not all states consider edTPA as the sole assessment choice. By 2018, ten other states had added edTPA as an assessment option. Since teacher candidates in these states may opt for existing options other than edTPA, we do not include the optional states in our analysis.[17]

Figure 1: States mandated edTPA as an program completion and initial licensure requirement, Snapshot in 2018



Notes: In 2018, eight states have already introduced edTPA as the only assessment option for program completion and initial teacher licensure.

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[17]The optional states include Arkansas, California, Delaware, Hawaii, Iowa, Maryland, Minnesota, North Carolina, South Carolina, West Virginia. Ohio, and Texas. We also do not observe the timing of edTPA in the optional states.

3 Data

3.1 IPEDS

We measure the teacher supply response to the implementation of edTPA by the number of graduates from teacher preparation programs in post-secondary institutions. The data is obtained from the Integrated Postsecondary Education Data (IPEDS), which contains rich information about the characteristics of post-secondary institutions in the entire US.[18] We exploit the detailed statistics of program completion by majors and identify graduates in teacher preparation programs (both bachelor's and master's degrees) from school year 2010/2011 to 2018/2019.[19] The majors include 'Education, General', 'Bilingual, Multilingual, and Multicultural Education','Curriculum and Instruction', 'Special Education and Teaching', 'Teacher Education & Professional Development, Specific Levels and Methods', 'Teaching English or French as a Second or Foreign Language", and 'Education, Other'.[20] We then aggregate the number of teacher graduates at the institution level. In the sample (excluding optional states), we have a panel of 858 post-secondary institutions that offer teacher preparation programs (either the traditional or alternative route).[21]

In Panel A of Table 1, in addition to the outcomes of interest — the number of teacher graduates and the breakdown by white and non-white candidates — we report time-varying

[18]An alternative data to measure the change in new teacher supply is the state-level initial licensure issuance documented in the Title II. It is less suitable than IPEDS in our context because Title II does not differentiate whether a license type requires edTPA. For example, in Washington, the aggregate count in Title II collapses 'Conditional certificate' and 'Residency certificate', where only the latter requires the edTPA score. The Washington districts can issue the conditional certificate for an individual who has not completed all the requirements for the regular certificate. In general, the state-level statistics in the Title II mix together temporary licenses (which do not require edTPA) with full licenses (which require edTPA). The measurement error in the outcome variable potentially attenuates the edTPA estimate. Nonetheless, in Table B1 of Appendix, we provide a supplementary result (excluding WA due to the aforementioned issue) using the Title II data and find a marginally significant effect.

[19]To become a licensed public school teacher in the US, a prospective teacher from the traditional route goes through training in a teacher preparation program. Alternatively, a person with a degree from non-education major can opt for the alternative route to complete an approved postgraduate program.

[20]IPEDS defines the major of a program using CIP codes. We follow the definition of teacher preparation programs recommended by Kraft et al. (2020) in their Appendix C.

[21]Including optional states, we have a total of 1,243 post-secondary institutions. The summary statistics are presented in Table A1 of appendix.

institution characteristics to account for concurrent changes in student demographics and the quality of institutions. The variables include the number and percent of minority of graduates in non-education majors, the submission rates and percentile scores of SAT/ACT, first-year full-time enrollment, par-time to full-time faculty ratio, and the amount of and the percent of students receiving federal grants/loans.

Table 1: Summary statistics (IPEDS) - Estimation sample

	Mean	SD	Min	Max
A. Outcomes:				
Education graduates	142.31	188.28	0.00	3041.00
Education graduates (white)	107.42	139.71	0.00	1763.00
Education graduates (non-white)	34.89	66.76	0.00	1968.00
B. Time-varying controls:				
Graduates (non-education majors)	1594.51	2160.49	1.00	16364.00
Minority graduates (% of non-education majors)	16.57	17.68	0.00	100.00
SAT submission rate	51.22	34.20	0.00	100.00
ACT submission rate	52.81	32.12	0.00	100.00
SAT 25 percentile score	476.10	65.18	215.00	740.00
SAT 75 percentile score	583.41	63.97	349.00	800.00
ACT 25 percentile score (cumulative)	20.38	3.31	3.00	33.00
ACT 75 percentile score (cumulative)	25.58	3.18	8.00	35.00
First-year FT enrollment	1056.10	1315.98	9.00	9082.00
Part-time/full-time faculty ratio	0.03	0.11	0.00	2.32
Grant (% student)	76.19	16.74	16.00	100.00
Grant (dollar amount, thousands)	44407.21	48785.28	326.33	397711.80
Loan (% student)	58.80	16.66	0.00	99.00
Loan (dollar amount, thousands)	21099.54	24175.53	0.00	256364.16

Sources: IPEDS 2011-2019.
Notes: This table shows summary statistics of estimation sample for teacher supply using IPEDS. Optional states are excluded. Summary statistics for all states are presented in Table A1 of appendix.

3.2 NAEP

To assess the effect of edTPA on student achievement, we analyze the biennial restricted data of the National Assessment of Educational Progress (NAEP) administered by the U.S Department of Education and the Institute of Education Sciences from 2009 to 2019. The assessment is a nationwide test in the US that measures the knowledge of a representative sample of students in various core subjects.[22] The standardized nature of the test enables us to compare student achievement across the country using a common measurement. We standardize the assessment scores by first averaging the composite values of five (or twenty) assessment items within each year-grade-subject and then standardize the averaged assessment scores over the estimation sample to have a zero mean and one standard deviation within the same year-grade-subject level.[23]

NAEP also provides important characteristics of students and schools, which enable more precise estimations by including them as controls. They allow us to conduct balance tests by regressing these predetermined variables on edTPA policy variances in our later analyses. The student controls include student's race and gender, if the student needs an Individualized Education Program (IEP), and if the student is an English-language learner. The school controls include share of black students, indicators for charter school, urban area, eligibility of lunch programs, and whether school enrollment is larger than 500 students.[24]

In addition to rich student and school characteristics, the NAEP data links students to characteristics of the corresponding subject teacher. This enables us to narrow down the sample to students whose teachers have less than two years since edTPA only applies to new teachers.[25] To ensure that the teachers have gone through the standard license procedure, we drop students whose subject teachers do not have a teacher license. We also restrict our attention to students with traditional route teachers because the parallel trend assumption does not hold for the alternative route sample. We defer the discussion to the result section.

[22]The subjects include reading, mathematics, science, writing, arts, civics, geography, economics, U.S. history, and technology & engineering literacy.

[23]In survey year 2009 and 2011, NAEP uses a five-item scale to measure the composite values of students' math and reading assessment at grade 4 and 8. In survey year 2013, 2015, 2017, and 2019, NAEP uses a twenty-item scale for math and reading assessment at grade 4 and 8.

[24]While most control variables employed in this study share consistent measures across the two subjects and grades, one exception is the school enrollment. For students at grade 4, we use enrollment larger than 500 to indicate magnitude of schools. However, for students at grade 8, we use enrollment larger than 600 in year 2009, 2011, 2013, 2017, and 2019, as data in these years use a different category for student enrollment.

[25]The question on years of experience contains continuous measures in survey year 2009 and 2011 and categorical responses in year 2013, 2015, 2017, and 2019. The categorical responses are listed as the following: Less than 1 year, 1-2 years, 3-5 years, 6-10 years, 11-20 years, 21 or more years, omitted, and multiple responses.

As far as the data provides, we assess student performances in the mathematics score at grade 4, and the reading scores at grades 4 and 8.[26] Combining the NAEP from different cohorts yields a repeated cross-sectional sample of students. To address the concern that changes in student and school characteristics may affect teacher assignments and contaminate the causal estimates, we control for student and school characteristics presented in Table 2.[27]

Table 2: Summary statistics (NAEP) - Estimation sample

	Grade 4 Math	Grade 4 Reading	Grade 8 Reading
A. Outcomes:			
Assessment score (raw)	235.76 (28.20)	216.75 (34.49)	252.97 (37.37)
B. Student controls:			
White	0.46 (0.50)	0.46 (0.50)	0.50 (0.50)
Black	0.14 (0.35)	0.13 (0.34)	0.13 (0.34)
Hispanic	0.28 (0.45)	0.29 (0.45)	0.24 (0.43)
Female	0.49 (0.50)	0.50 (0.50)	0.49 (0.50)
Individualized Education Program (IEP)	0.13 (0.33)	0.12 (0.33)	0.11 (0.32)
English learner	0.10 (0.29)	0.09 (0.29)	0.06 (0.23)
C. School controls:			
Charter school	0.05 (0.22)	0.05 (0.22)	0.04 (0.20)
Urban area	0.77 (0.42)	0.77 (0.42)	0.74 (0.44)
Share of black student	16.68 (25.64)	16.50 (25.32)	15.73 (25.35)
Lunch program	0.56 (0.50)	0.56 (0.50)	0.50 (0.50)
Student enrollment ($\geq$ 500)	0.45 (0.50)	0.45 (0.50)	0.50 (0.50)
Number of Student	51,460	53,530	41,260

Sources: NAEP 2009, 2011, 2013, 2015, 2017, and 2019.
Notes: This table shows summary statistics of estimation sample (students with new traditional route teachers) for student achievement using NAEP. The mean is shown in the cell and the standard deviation is shown in the parentheses. Each column presents one of the three student assessment samples: Math at Grade 4 , Reading at Grade 4, and Reading at Grade 8. Raw assessment scores are reported in the summary statistics. The number of observations is rounded to the nearest 10 per IES disclosure guidelines. Summary statistics for all states are presented in Table A2 of appendix.

[26] The restricted data also tracks the mathematics scores at grade 8. However, it does not contain teacher experience in 2017 survey year and cannot identify new teachers.

[27] Table A2 of appendix presents the summary statistics by including also the optional states. Table A3 contrasts the summary statistics between the traditional and alternative route sample.

4 Identification Strategy

4.1 Teacher Supply

We estimate the effects of the mandatory edTPA requirement on teacher and student outcomes using a difference-in-differences framework with the leads and lags of treatment. Formally, for teacher supply analysis, we employ the following specification:

$$Y_{u,s,t} = \sum_{k \neq -1} \beta_k edTPA_{s,t} \mathbb{1}(t = t^* + k) + \mathbf{X}_{u,s,t}\Gamma + \mathbf{Z}_{s,t}\theta + \alpha_u + \alpha_t + \epsilon_{u,s,t} \tag{1}$$

where $Y_{u,s,t}$ refers to the log of the number of teacher graduates from institution u in state s in year t. To differentiate the edTPA effects on teacher supply by race, we run separate regressions on the number of white and non-white candidates. $edTPA$ is a dummy indicator equals 1 after a state mandated edTPA as the initial licensure requirement in the graduation year t^*. In the above non-parametric model, the omitted period is the graduation year right before the policy took effect. For example, the effective date in Illinois is September 2015. Its omitted year is the 2014/2015 school year. Then, $\beta_{(k>-1)}$ measures the edTPA effect on teacher supply in a given post-policy year, whereas $\beta_{(k<-1)}$ detects any deviation in trends in the pre-policy period between the edTPA and non-edTPA states. $X_{u,s,t}$ refers to a vector of time-varying controls at the institution level presented in Table 1. $\mathbf{Z}_{s,t}$ refers to a series of education policy indicators studied by Kraft et al. (2020) to control for potential confounds on the teacher supply response. The policies include the accountability reforms, the elimination of teacher tenure, the increase in probationary period, the elimination of mandatory union dues, the adoption of Common Core Standards, and changes in the licensure contents.[28] α_u and α_t are institution and year fixed effects, respectively. To account for serial correlation within a state, we cluster the standard errors at the state level.

[28] We code the policy year based on Table A1 of Kraft et al. (2020).

4.2 Student Outcomes

To estimate the impacts of edTPA on student achievement, we exploit the same policy variation in which edTPA becomes consequential in the educator licensing process shown in Figure 1 using the NAEP data. We employ the same differences-in-differences framework with a repeated cross-sectional sample of students. Formally, we estimate the following model:

$$Y_{i,j,s,t} = \sum_{k=-5,k\neq-1}^{k=2} \beta_k edTPA_{s,t} \mathbb{1}\left(t = t^* + k\right) + \mathbf{X}_{i,j,s,t}\Gamma + \mathbf{Z}_{s,t}\theta + \alpha_s + \alpha_t + \epsilon_{i,j,s,t} \qquad (2)$$

where Y_{ist} is the reading/mathematics score of student i's in school j in state s sampled in period t. We again included the leads and lags of treatment indicators ($edTPA$) to check the parallel-trend assumption and also capture the dynamic effects. t_s^* is the policy implementation year for the eight treated states.

Continuing with the control variables, $\mathrm{X}_{i,j,s,t}$ is a vector of student and school characteristics listed in Table 2. $\mathbf{Z}_{s,t}$ refers to the same set of policy controls as in equation 1 that is studied in Kraft et al. (2020). α_s and α_t are the state fixed effects and year fixed effects, respectively. Standard errors are clustered at state level, which is the level the edTPA was implemented.

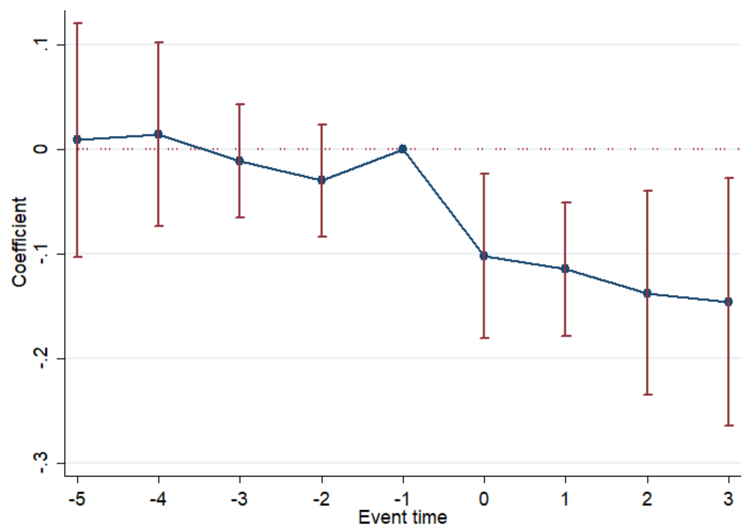
While this specification is almost identical to the one for teacher supply above, there is a difference on the time period because of the data structure. NAEP is a biennial assessment and the NAEP data we obtained is from 2009 to 2019, the time period of this specification ranges from -5 to 2 with each period t represents two academic years.

5 Results - Teacher Supply

5.1 Main Pattern

In Figure 2, we plot the event study dummies with the corresponding 95% confidence interval, conditional on institution and year fixed effects. the pre-treatment effects show that there is no systematic deviation in pre-trends. This further validates the difference-in-differences model in producing a reliable post-edTPA counterfactual. The raw post-treatment pattern indicates a sudden decrease in teacher graduates that captures the immediate impact of candidates failing the test. The effect size also grows over time, suggesting that the test both screens out students who failed the test and dissuades students from enrolling into teacher programs.[29]

Figure 2: No significant deviation of the pre-trend



Notes: This figure plots the estimates of the event study dummies and the corresponding 95% confidence interval. The regression in this figure includes year and institution fixed effects. No control variables are added. The endpoints are binned up to show a balanced window.

In Table 3, we present the estimates from the diff-in-diff strategy in various specifications. With the basic time-varying controls, fixed effects, and regional fixed effects, Column 1 shows

[29] In the result not presented, we also find that edTPA reduced teacher-degree fall enrollment using IPEDS data from 2010 to 2018. Because the data is only biennial and the pre-trend has significant deviation, we do not over-interpret the enrollment finding.

that edTPA reduced the number of teacher graduates by 8.7%. In Column 2, we discern the edTPA from concurrent policies in the preK-12 public schools. We include the set of policy controls suggested by Kraft et al. (2020) that may influence new teacher supply. More importantly, the policy controls include changes in state licensure standards (basic skills, subject knowledge, and pedagogy). This help distinguish the effect of edTPA and other licensing changes that may affect the passing rate and degree choices. The negative effect drops by about a standard deviation, but the coefficient remains statistically significant at the 1% level.

In Column 3, we further control for the implementation of public accountability reforms. Kraft et al. (2020) find that the on-the-job high-stake evaluations created pressure on teaching and impeded new teacher supply. This reform had an overlapping implementation schedule with the roll-out of the edTPA in the eight treatment states. Washington and Illinois implemented the reform one year after the roll-out of edTPA, while six of them implemented the reform prior to edTPA.

Despite the potential competing effects, we believe our estimate of edTPA does not pick up the influence of the accountability reform. First, the high-stake evaluation reform and the edTPA affect new teachers at different margins. Kraft et al. (2020) finds a reduction in initial issuance of licensure, while our focus is the number of new teacher graduates which captures the potential supply. New teachers are possible to pursue other occupations and to not obtain a teacher license upon graduation.[30] The two policies affect different margins because edTPA applies directly to the graduation requirement, while the high-stake evaluation reform can be seen as a cost at the occupation choice margin. Therefore, the accountability reform likely affected the occupational choice more than the degree completion rate. Consistent with the fact that the two polices affect new teacher at different margins, when we control for the accountability reform in Column 3, our estimate only changes slightly. The negative effect of the edTPA on degree completion rate remains strong at 1% level.

[30]While both margins are important, as we discussed in the data section, our focus is the effect on potential supply because the IPEDS provides a cleaner measurement than the licensure data.

To further show that the edTPA estimate does not pick up the influence of the teacher accountability reforms, we explore two additional exercises. As shown by Kraft et al. (2020) (in their Section 8.1), the degree completion of one-year postgraduate programs is more appropriate to capture the effects by high-stake evaluation reforms. The reason is that the occupational choice of postgraduates is more responsive than those in a four-year program. By contrast, the effect of edTPA on the degree completion margin is likely to be smaller for one-year postgraduate programs than that for four-year undergraduate programs. While the test agent does not publish the score report by degree type, we can learn the differential ability in the education literature. For example, Sass (2015) finds prospective teachers pursuing a master degree are generally more committed than their undergraduate-counterparts. Some of the master students also might have already had a license. These make edTPA less relevant to their degree completion.

Our first check is to leverage the aforementioned possible heterogeneity by program type. We separate the analysis into four-year undergraduate programs and one-year postgraduate programs using the full specification in Column 3 of Table 3. Column 4 of Table 3 shows that edTPA significantly reduced the number of teacher graduates in the undergraduate programs and the magnitude is bigger than the average effect in Column 3. By contrast, we do not observe significant changes in the number of teacher graduates in post-graduate programs as shown in Column 5 of Table 3.

The null effect on postgraduates is a useful placebo test. When edTPA may not bind for some candidates in postgraduate programs, we observe insignificant impacts, both economically and statistically, on degree completion. Overall, the null impact on graduate degrees again shows our estimates of edTPA is less likely to be affected by the high-stake evaluations studied by Kraft et al. (2020), where postgraduate programs are more likely than undergraduate programs to be affected in their case.

To further show that the edTPA estimate does not pick up the influence of the teacher accountability reforms, in Figure B1 of appendix, we perform a permutation test as a second

Table 3: Diff-in-diff estimates with various specifications

	All programs			Undegraduate	Postgraduate
	(1)	(2)	(3)	(4)	(5)
edTPA	-0.0870***	-0.0651***	-0.0630***	-0.0830**	-0.0137
	(0.0285)	(0.0229)	(0.0221)	(0.0397)	(0.0279)
R-squared	0.394	0.398	0.398	0.445	0.504
Observations	7,168	7,168	7,168	6,453	5,605
Time-varying controls	X	X	X	X	X
Regional trend	X	X	X	X	X
Confounding policies#		X	X	X	X
Accountability Reform			X	X	X

Sources: IPEDS, 2011-2019.
Notes: Each column in each panel represents one regression. Dependent variable in each regression is the log of the number of teacher graduates. All regressions include time-varying controls in Panel A of Table 1, year and institution fixed effects. #Confounding policies are based on Table A1 of Kraft et al. (2020). All regressions are weighted by the pre-2014 average program size. Standard errors in the parenthesis are clustered at the state level. ***, **, and * represent 1%, 5%, and 10% significant level, respectively.

check. The test runs 10,000 permutations with placebo treatments on the non-edTPA states in the conditional sample. In each round, we randomly assign placebo treatments to eight of the non-edTPA states that mimic the implementation timing of edTPA relative to the teacher accountability reform: two of them implemented edTPA 1 year prior; two implemented edTPA 1 year after; two implemented edTPA 2 years after; and the remaining two implemented edTPA 4 and 5 years after. If our edTPA estimate does pick up the effect of accountability reforms, the distribution of the placebo estimates should overlap with our DID estimates in Table 3. As shown in the first figure, our DID estimate (-0.063) in Column 3 does not overlap with the placebo distribution (p-value=0.087). In the second figure, we compares the effect on undergraduate programs in Column 4 of Table 3, which is the main driver of the average effect in Column 3 of Table 3, with the empirical placebo effects. The estimate is distinctively different than the placebo distribution (p-value=0.02). implying that the identified treatment effects less likely to pick up residual influences of the competing policy.

5.2 Robustness

We perform several tests to show that our identified effect on teacher supply does not capture other confounding factors. In Table 4, we find that the placebo treatment has essentially zero effects on the number of non-education graduates. This alleviates the concern that the drop of teacher graduates simply reflects state-specific shocks in tertiary education.

Table 4: Placebo test on non-education majors

	(1)	(2)	(3)
Placebo treatment	0.0218	0.0185	0.0184
	(0.0186)	(0.0129)	(0.0124)
R-squared	0.130	0.131	0.131
Observations	7,200	7,200	7,200
Regional trend	X	X	X
Confounding policies#		X	X
Accountability Reform			X

Sources: IPEDS, 2011-2019.
Notes: Dependent variable in each regression is the log of the number of non-education graduates (by race). All regressions include time-varying controls in Panel A of Table 1, year and institution fixed effects. All regressions are weighted by the pre-2014 average program size. Standard errors in the parenthesis are clustered at the state level. ***, **, and * represent 1%, 5%, and 10% significant level, respectively.

Because we are leveraging different policy timing across states, one concern is that the propensity to adopt edTPA is correlated with the regional teacher market conditions. Although previous licensing studies have pointed out that state variation in licensing policy is largely determined randomly by political forces, we perform a balancing test to show there are no systematic differences in observed characteristics between edTPA and non-edTPA states.[31] In all columns of Table 5, we regress an indicator equals 1 if a state adopted edTPA during the sample period on its pre-2014 attributes, including the level/growth of the number of teacher graduates, and average institution characteristics. Across columns, we use different measures of institution quality available in IPEDS to probe the sensitivity of the estimates. In all specifications, the only significant co-variate is the state number of education

[31] Relevant studies include Kleiner and Soltas (2019) and Larsen et al. (2020).

graduates that implies larger states tend to adopt edTPA. It is less of a concern because the state fixed effect takes into account time-invariant state differences and the growth in education graduates does not correlate significantly with the edTPA implementation. Overall, we do not find strong evidence that edTPA adoption was correlated with pre-policy characteristics of post-secondary institution or teacher graduates. This gives us credence about the quasi-random nature of edTPA implementations.

Table 5: Balancing test - Pre-2014 characteristics do not predict edTPA implementation

	(1)	(2)	(3)	(4)
Education graduates (level)	0.258*	0.258*	0.266*	0.261*
	(0.138)	(0.138)	(0.138)	(0.138)
Education graduates (growth)	0.0393	0.0479	0.0398	0.0406
	(0.161)	(0.161)	(0.164)	(0.161)
First-year FT enrollment (thousands)	-0.122	-0.120	-0.149	-0.141
	(0.261)	(0.260)	(0.255)	(0.255)
Part-time/full-time faculty ratio	-0.268	-0.278	-0.254	-0.261
	(0.370)	(0.372)	(0.374)	(0.371)
Grant (% student)	-0.00334	-0.00421	-0.00469	-0.00471
	(0.00957)	(0.00905)	(0.00903)	(0.00899)
Grant (dollar amount)	-6.43e-05	0.000448	0.00182	0.00139
	(0.00801)	(0.00697)	(0.00756)	(0.00658)
Loan (% student)	0.0113	0.0122	0.0114	0.0118
	(0.00864)	(0.00883)	(0.00866)	(0.00880)
Loan (dollar amount)	-0.00191	-0.00269	-0.00215	-0.00219
	(0.0141)	(0.0140)	(0.0147)	(0.0141)
SAT 25 percentile score	0.00155			
	(0.00355)			
SAT 75 percentile score		0.00145		
		(0.00298)		
ACT 25 percentile score			0.00790	
			(0.0694)	
ACT 75 percentile score				0.0192
				(0.0625)
Observations	51	51	51	51
R-squared	0.157	0.158	0.154	0.155

Sources: IPEDS, 2011-2014.
Notes: Dependent variable in all regressions is an indicator equals 1 if a state mandated edTPA after 2014. All regressors are pre-2014 averages.

Next, in Table 6, we run a series of auxiliary fixed-effect models excluding the time-varying controls. As shown in Column 1 to 6, the edTPA treatment does not change institution characteristics, including first-year enrollment (all majors), faculty resource, and the financial background of students. In Column 7 of Table 6, we also find that there is no significant changes in teacher demand measured by public school enrollments.[32]

Table 6: Changes in institution characteristics and teacher demand are not significant confounders

	(1) First-year enrollment	(2) PT/FT faculty ratio	(3) Grant (% students)	(4) Grant (amount)	(5) Loan (% students)	(6) Loan (amount)	(7) Teacher demand
Placebo treatment	0.00730	0.00219	0.670	1.475	-0.348	-0.0247	-0.00972
	(0.0237)	(0.00382)	(0.562)	(1.382)	(0.551)	(0.533)	(0.0116)
Observations	7,281	7,281	7,281	7,281	7,281	7,281	351
R-squared	0.025	0.002	0.023	0.219	0.118	0.093	0.172
Number of unit id	858	858	858	858	858	858	-
Number of state	-	-	-	-	-	-	39

Sources: IPEDS, 2011-2019 (Column 1 to 6); NCES (Column7).
Notes: All regressions include year and institution fixed effects. Standard errors in the parenthesis are clustered at the state level. ***, **, and * represent 1%, 5%, and 10% significant level, respectively.

Lastly, the DID strategy with staggered timing may be susceptible to 'bad comparison' that the earlier treated units are used as the comparison groups for later-treated units. The 'bad comparison' concern manifests as a problem when treatment effects evolve over time. Depending on the direction of the dynamic effects, the DID estimates with staggering timing may over- or under-state the average treatment effect. To check the robustness, we first assess if our estimation involves negative weights, which is the source of the bias (De Chaisemartin and d'Haultfoeuille, 2020). In the total of 1225 ATTs we have, only 1.5% of the weights are negative. The sum of negative weights equals -0.000904, which is negligible. Nonetheless, to check the sensitivity, we adopt the stacked regression estimator summarised by Baker et al. (2021).[33] We create event-specific data sets that pair the corresponding treatment states with only never-treated states. According to Figure 1, we have five treatment cohorts in total. Stacking all five into one single dataset, we run the full specification (including policy controls and regional trends) with set-specific institution- and year-fixed effects. This way we ensure the comparison group in each event cohort consists of 'clean control' states. With

[32] We pool the state level statistics (2011-2019) from the National Center for Education Statistics (NCES).
[33] A recent application is by Cengiz et al. (2019), who analyze the effect of minimum wage laws.

various specifications, Table 7 shows a similar reduction in new teachers with slightly bigger magnitudes compared to the main analysis in Table 3. The decrease again primarily occurs to four-year undergraduate programs.

Table 7: Robust to Addressing Staggered DID Issue - Stacked DID approach

	All programs			Undegraduate	Postgraduate
	(1)	(2)	(3)	(4)	(5)
edTPA	-0.0948**	-0.0950**	-0.0915**	-0.117**	-0.0384
	(0.0385)	(0.0370)	(0.0373)	(0.0469)	(0.0436)
R-squared	0.353	0.355	0.355	0.363	0.503
Observations	26,692	26,692	26,692	24,289	20,385
Regional trend	X	X	X	X	X
Confounding policies#		X	X	X	X
Accountability Reform			X	X	X

Sources: IPEDS, 2011-2019.
Notes: We adopted the stacked regression estimator summarised by Baker et al. (2021). We create event-specific data sets (a total of 5 in our case) that pair the corresponding treatment states with only never-treated states. In the final step, we stack the data sets and run the standard DID models with set-specific institution- and year-fixed effects. All regressions include time-varying controls in Panel A of Table 1, year and institution fixed effects. All regressions are weighted by the pre-2014 average program size. Standard errors in the parenthesis are clustered at the state level. ***, **, and * represent 1%, 5%, and 10% significant level, respectively.

5.3 Heterogeneity

The negative impacts on teacher supply fall disproportionately on candidates with a disadvantaged background, as the education literature suggests. To explore possible heterogeneity, we define the type of university in two aspects. First, we utilize the university-wide the 25^{th} SAT admission scores contained in IPEDS before 2014 to categorize institutions into two groups: more (top 50%) and less (less 50%) selective. In the regression, we interact the bottom 50% indicator with the treatment dummy to see if there exists heterogeneity by institution selectivity.[34] We present the results using the undergraduate sample in which the effect is concentrated.

In Panel A of Table 8, we present the heterogeneity result by ranking institutions based on their 25^{th} SAT percentile scores. In Column 1, while the total edTPA effect on teacher supply becomes less precise, there exists a marginally significant differential impact on less-selective programs. When we break down the analysis by the race of candidates, the differential effect by selectivity is mainly driven by Hispanics and candidates of 'other races', as shown in Column 4 and 5 respectively.

We then look at the heterogeneity by the racial composition of a university. In Panel B of Table 8, we categorize universities based on the percent of non-white graduates in non-education majors. The estimate of the interaction term in Column 1 is not precise, but the magnitude indicates a stronger negative impact on minority-concentrated universities. The combined effect (-14.6%) is also statistically significant at the 5% level (F-stat: 5.18). When we split the analysis by subgroup from Column 3 to 5, we also observe a stronger magnitude for non-white candidates than white candidates in Column 2.

Our heterogeneity results overall echo previous findings that minority teachers tend to have lower performances in teacher tests. For example, Angrist and Guryan (2008) also find that Hispanics have lower licensure scores than non-Hispanics candidates in state-mandated exams (basic-skills or subject-matter) in the 90s. More recently, Goldhaber et al. (2017)

[34]The base indicators for 'bottom 50%' is time-invariant and is absorbed by institution-fixed effects.

also find that Hispanic candidates are more likely than non-Hispanics to fail edTPA in Washington.

Table 8: Heterogeneity by the type of university - Undergraduate programs

	(1) Total	(2) White	(3) Black	(4) Hispanic	(5) Other race
Panel A: X = University ranks at bottom 50% (SAT score 25^{th} percentile)					
edTPA	-0.0546	-0.0839*	-0.0266	0.0427	-0.0295
	(0.0445)	(0.0459)	(0.0631)	(0.0667)	(0.0561)
edTPA*X	-0.0654*	-0.0279	-0.0821	-0.180***	-0.167**
	(0.0328)	(0.0318)	(0.0905)	(0.0574)	(0.0786)
R-squared	0.446	0.465	0.092	0.074	0.168
Panel B: X = Minority students in non-education majors (above mean)					
edTPA	-0.0587*	-0.0662*	0.0213	0.0280	-0.0876*
	(0.0337)	(0.0360)	(0.0740)	(0.0510)	(0.0489)
edTPA*X	-0.0874	-0.0851**	-0.166**	-0.119*	-0.229**
	(0.0558)	(0.0390)	(0.0711)	(0.0629)	(0.104)
R-squared	0.445	0.494	0.088	0.067	0.179
Observations	6,453	6,453	6,453	6,453	6,453

Sources: IPEDS, 2011-2019.
Notes: Sample is restricted to undergraduate programs. Dependent variable in each regression is the log of the number of teacher graduates (by race). All regressions include time-varying controls in Panel A of Table 1, policy controls, year and institution fixed effects. We categorise institutions as bottom 50% according to their pre-2014 25^{th} percentile SAT in Panel A. In Panel B, we categorize institutions using the minority (non-white students) concentration in non-education majors. All regressions are weighted by the pre-2014 average program size. Standard errors in the parenthesis are clustered at the state level. ***, **, and * represent 1%, 5%, and 10% significant level, respectively.

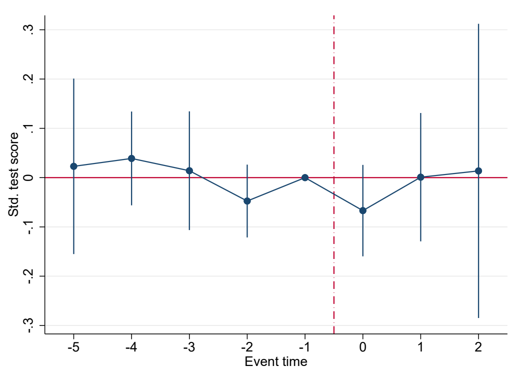
6 Results - Student Achievement

6.1 Main Pattern

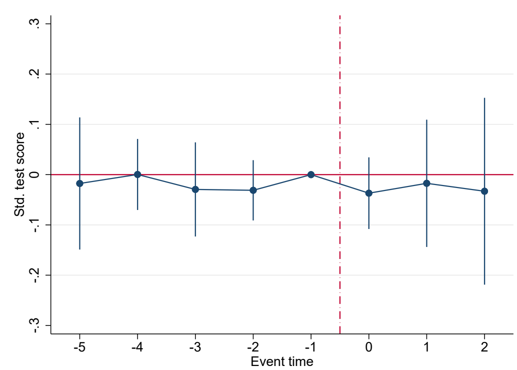
In this section, we test if edTPA benefits student learning using the student sample of new instructors who obtained a standard license through a traditional-route program.

Figure 3 plots the estimates of event study dummies for standardized test scores to visualize the trend. We combine the three subjects - namely mathematics in grade 4, reading in grades 4 and 8 - to show an aggregate picture. On the left, we do not include any control variables to demonstrate the pattern in the raw data, conditional on subject, year, and state fixed effects. On the right, we present the trend using the full specification. In both figures, there is no significant deviation in pre-treatment trend that validates the assumption of the difference-in-differences approach. Under the full specification, the pre-treatment trend is more stable and flat, demonstrating the importance of controlling time-varying factors and concurrent education policies. In both specifications, we do not see edTPA imposed any significant effect on student test scores.

Figure 3: Event study figure for student test score



(a) No controls



(b) With controls

Sources: NAEP 2009, 2011, 2013, 2015, 2017, and 2019.
Notes: The dependent variable is the standardized test score. Event period -1 is normalized to 0. The underlying regression of subfigure (a) contains no controls to show raw data patterns, while subfigure (b) contains student, school and policy controls, conditional on year fixed effects, and state-by-assessment fixed effects. The figures show the 95% confidence interval with robust standard errors clustered at the state level.

We then track the change in the DID estimate when we gradually add various control variables from Column 1 to Column 3 in in Table 9. In Column 1, only conditional on fixed effects, we do not see a significant impact of edTPA on student test scores. When we control for student and school characteristics in Column 2 and confounding policies in Column 3, the effect size gets closer to 0.

Based on the findings on student standardized scores in other policy evaluation studies, we can compare our upper bound (adding 1.96 of S.E. to the estimate) and assess to what extent our confidence intervals is informative to dismiss small effects. Kraft (2020) surveyed 747 education policy research and recommended the effect size categories: 'small' if less than 0.05; 'large' if bigger than 0.2; and 'medium' if in between. According to our estimate and its confidence interval in our full model in Column 3, we can rule out an effect size as small as 0.038 of a standard deviation for overall test scores, that belongs to the 'small' effect-size category.

Table 9: Impacts of edTPA on students' achievement

	Std. test score					
	Total			G4 Math	G4 Reading	G8 Reading
	(1)	(2)	(3)	(4)	(5)	(6)
edTPA	-0.044	-0.026	-0.018	-0.005	-0.054	0.010
	(0.034)	(0.025)	(0.028)	(0.033)	(0.034)	(0.044)
R-squared	0.030	0.350	0.351	0.332	0.370	0.372
Observations	146,250	146,250	146,250	51,460	53,530	41,260
State-Subject FE	X	X	X			
State FE				X	X	X
Year FE	X	X	X	X	X	X
Student and school controls		X	X	X	X	X
Policy controls #			X	X	X	X

Sources: NAEP 2009, 2011, 2013, 2015, 2017, and 2019.
Notes: The table shows estimates using student samples with teachers obtained the license through a traditional teacher preparation program. Columns (1)-(3) pool student samples from the three assessments and estiamte the effects with the state-by-subject fixed effects. The dependent variables in following columns (4)-(6) are Grade 4 Math, Grade 4 Reading, and Grade 8 Reading, respectively. The test scores are standardized to a zero mean and one standard deviation within each assessment sample. 'edTPA' refers to the treatment indicator. All regressions include state and year fixed effects. Student and school controls are listed in Panel B of Table 1. #The policy controls are based on Table A1 of Kraft et al. (2020). Standard errors in brackets are clustered at the state level. Sample sizes are rounded to the nearest 10 per IES disclosure guidelines. ***, **, and * represent 1%, 5%, and 10% significant level, respectively.

We then run the full specification by subject - namely Grade 4 reading, Grade 4 mathematics, and Grade 8 reading – from Column 4 to Column 6 in Table 9. We again do not observe significant impacts of edTPA on various subject scores. Referencing the benchmarks, we can rule out a positive effect of 0.06 of a standard deviation for Grade 4 Math score, which is the lower end of the 'medium' effect-size category. We can also rule out a 'small' effect as 0.014 of a standard deviation for Grade 4 Reading score. The upper bound of the estimate for Grade 8 Reading is slightly larger at about 0.1 of a standard deviation increase in test score, which remains below the median of the empirical distribution surveyed by Kraft (2020).[35]

Similar to the teacher supply analysis, one may concern about the overlapping timing of edTPA and the public accountability reform. Based on the recent findings by Bleiberg et al. (2023), the accountability reform did not yield significant impacts on students. Therefore, we believe our edTPA estimate on students is not confounded by the residual influences of high-stake evaluation reforms.

We also check the need to use alternative DID methods by assessing the 'bad comparison' problem. Consistent with the null effect findings, the De Chaisemartin and d'Haultfoeuille (2020) decomposition shows that none of the weights in our estimation samples are negative, and the sum of the negative weights is also zero. The analysis indicates the 'bad comparison' problem is not a concern when interpreting estimates in Table 9.

As stated in the data section, the reason we focus on students with traditional route teachers is that the parallel trend assumption does not hold in the alternative route sample. As we demonstrate in Figure C1 and C2, the pre-treatment trends are largely bumpy and have significant deviation in most of the cases. Nonetheless, for completeness, we also present the main pattern using the alternative-route sample in the appendix. We include the DID estimates in the Panel C of Table C1 of Appendix. Because of the pre-trend deviation, we do not over-interpret the negative effects in the alternative-route sample.

[35]The detail of the distribution can be found in Table 1 of Kraft (2020).

6.2 Further Analysis on Test Scores

6.2.1 Balancing Test

Our student sample is based on the years of experience of teachers. One concern of this sample selection criteria is that the estimation sample changes systematically with the edTPA timing. For example, if new teachers are more/less likely to be assigned to disadvantaged students after edTPA, our negative estimates would falsely be attributed to the causal impact of edTPA.

We test if the edTPA treatment is correlated with student characteristics by performing a number of auxiliary models. We regress student characteristics on the edTPA indicator conditional on state and year fixed effects. As shown in the Table 10, edTPA in general is not related to changes in most of the predetermined student characteristics (except for Hispanic in Grade 8 reading). Overall, we do not find evidence that there is a systematic sample selection issue in our estimation.[36]

6.2.2 Heterogeneity

The richness of NAEP allows us to look beyond the average effects on test scores. In this extension, we perfom heterogeneity analysis using the traditional-route sample.[37] In all the following analyses, we perform the full specification with student, school, and policy controls.

We utilize the rich detail about school information to explore if the effects of edTPA differ by schools. We run the full specification and interact the edTPA indicator with several hard-to-staff characteristics, namely the percent of black students, the percent of Hispanic students, whether the school is in an urban area, and whether the school participates in the free lunch program.

Results are presented in Table 11. For Panel A and B, we do not find heterogeneity by

[36] Table C2 of appendix contains the corresponding robustness result using the full sample (both traditional and alternative-route).

[37] In our earlier version, we also find no significant heterogeneity by student ability using quantile regressions. Results are available upon request.

Table 10: edTPA is not correlated with changes in student characteristics

	White	Black	Hispanic	Female	IEP	Eng learner
Panel A. Grade4 Math	(1)	(2)	(3)	(4)	(5)	(6)
edTPA	0.006	-0.010	0.007	-0.013	-0.005	0.021
	(0.017)	(0.014)	(0.015)	(0.011)	(0.009)	(0.014)
R-squared	0.088	0.112	0.067	0.001	0.007	0.047
Observations	51,460	51,460	51,460	51,460	51,460	51,460
Panel B. Grade4 Reading	(7)	(8)	(9)	(10)	(11)	(12)
edTPA	-0.019	-0.009	0.022	0.005	-0.003	0.002
	(0.018)	(0.019)	(0.017)	(0.013)	(0.013)	(0.018)
R-squared	0.083	0.098	0.068	0.001	0.009	0.048
Observations	53,530	53,530	53,530	53,530	53,530	53,530
Panel C. Grade8 Reading	(13)	(14)	(15)	(16)	(17)	(18)
edTPA	-0.044	-0.001	0.036***	-0.012	0.014	0.027
	(0.027)	(0.019)	(0.013)	(0.015)	(0.015)	(0.017)
R-squared	0.111	0.132	0.103	0.001	0.011	0.037
Observations	41,260	41,260	41,260	41,260	41,260	41,260
State FE	X	X	X	X	X	X
Year FE	X	X	X	X	X	X

Sources: NAEP 2009, 2011, 2013, 2015, 2017, and 2019.
Notes: The traditional route samples in panel A, B, and C are from three NAEP assessments: Grade 4 Math, Grade 4 Reading, and Grade 8 Reading, respectively. The dependent variables are students' predetermined characteristics, while the independent variable 'edTPA' is an indicator where its value equals 1 if state s passes compulsory edTPA policy and 0 otherwise. All regressions include state and year fixed effects. Robust standard errors clustered at state level are in brackets. Sample sizes are rounded to the nearest 10 per IES disclosure guidelines. $***p < 0.01, **p < 0.05, *p < 0.1$.

the four school characteristics in Grade 4 reading and Grade 4 mathematics. In Panel C, we find a strong differential effect if the school is majority-black at the 1% significance level. While the coefficient is positive, the magnitude is economically small such that combining the interaction with the base term still cannot reject the null effect hypothesis.

Table 11: Heterogeneous Impacts of edTPA reforms: by school characteristics

	Student outcome: Std. test score			
X≡	% Black	% Hispanic	Urban	Free lunch
Panel A. Grade 4 Math	(1)	(2)	(3)	(4)
edTPA*X	0.001	0.001	0.068	-0.023
	(0.001)	(0.001)	(0.067)	(0.050)
edTPA	-0.023	-0.019	-0.030	0.009
	(0.044)	(0.028)	(0.041)	(0.040)
R-squared	0.332	0.334	0.332	0.332
Observations	51,460	51,460	51,460	51,460
Panel B. Grade 4 Reading	(5)	(6)	(7)	(8)
edTPA*X	0.000	0.000	0.011	0.003
	(0.000)	(0.001)	(0.044)	(0.039)
edTPA	-0.055*	-0.057*	-0.058*	-0.056
	(0.030)	(0.032)	(0.033)	(0.044)
R-squared	0.372	0.373	0.372	0.372
Observations	53,530	53,530	53,530	53,530
Panel C. Grade 8 Reading	(9)	(10)	(11)	(12)
edTPA*X	0.002***	-0.003	0.092	0.042
	(0.001)	(0.043)	(0.055)	(0.060)
edTPA	-0.038	0.025	-0.033	-0.016
	(0.050)	(0.175)	(0.058)	(0.067)
R-squared	0.372	0.373	0.372	0.372
Observations	41,260	41,260	41,260	41,260
State FE	X	X	X	X
Year FE	X	X	X	X
Student controls	X	X	X	X
School controls	X	X	X	X
Policy controls #	X	X	X	X

Sources: NAEP 2009, 2011, 2013, 2015, 2017, and 2019.
Notes: The table uses student samples with new teachers obtained the license through a traditional teacher preparation program. The sample in Panel A, B, and C is from Grade 4 Math, Grade 4 Reading, and Grade 8 Reading assessment, respectively. The dependent variable is the standardized test score. 'edTPA' refers to the treatment indicator. All regressions include state and year fixed effects. Student and school controls are listed in Table 2. #The policy controls are based on Table A1 of Kraft et al. (2020). Standard errors in brackets are clustered at the state level. Sample sizes are rounded to the nearest 10 per IES disclosure guidelines. ***, **, and * represent 1%, 5%, and 10% significant level, respectively.

6.2.3 Indirect Spillover

In the main analysis, we explore the direct effect of edTPA on the students when we focus the sample of new teachers who likely went through edTPA. From a broader perspective, the general equilibrium effect also concerns policymakers matter, say, if the schools need to hire other teachers (e.g. substitute teachers) to fill the vacancies. Through the change in teacher composition, edTPA could potentially elicit indirect spillover on students even though their teachers may not have taken edTPA.

In the appendix, we utilize two datasets to explore the indirect effect. In Panel A of Table C3, we first use the full sample - teachers of all type - in NAEP that contains a national representative sample of about 1.4 million students. We again find edTPA has a null impact on test scores in both Math (Columns 1 and 2) and Reading (Columns 3 and 4), and the result is invariant of including control variables. In the full specification for the overall test score in Column 6, the upper bound of our estimate can reject an effect size of 0.054 of a standard deviation.

To verify the null impact, we explore the Stanford Education Data Archive (SEDA) as an additional check. Although SEDA does not differentiate teacher types, its merit over NAEP is the annual data structure that expands our post-treatment period. The SEDA provides district-level average Math and Reading test scores for students from Grade 3 to 8 between 2009 and 2018, which were comparable to the NAEP sample (Fahle et al., 2021).[38] The SEDA contains 374,951 and 383,192 district-level observations for Math and Reading over the 3-8 grades between our sample period. To address district confounding factors, we further merge the SEDA with a set of district-level characteristics in the American Community Survey – Education Tabulation (ACS-ED).[39] The district-level time varying controls include enrollment number (in log value), population percentage with a college degree or above,

[38]We employ the SEDA version 4.1 that is available at https://edopportunity.org/get-the-data/seda-archive-downloads/.

[39]The ACS-ED provides school district characteristics via their five-year estimates, which is available at https://nces.ed.gov/programs/edge/demographic/acs. We use the median year of the five-year period as the survey year. For example, the ACS-ED 2006-10 is used for year of 2008.

percentage of black population, and household median income (in log value). Consistently, the results in Panel B of Table C3 show no evidence of indirect effects on student achievement. In Column (12), the upper bound of our estimate can dismiss a medium effect size of 0.085 of a standard deviation for overall test scores.

7 Alternative Outcomes

In addition to student test scores, we explore alternative measurements about the potential impacts on schools and students as supplementary exercises. In this final section, we analyze the teacher surveys in the Schools and Staffing Survey (SASS) in 2011-2012 and the National Teacher and Principal Survey (NTPS) in 2015-16 and 2017-2018. SASS is the former version of NTPS conducted by the U.S. Department of Education to survey U.S. elementary and secondary schools about staffing issues. Although there are only three waves of surveys, we construct an annually repeated cross-section of data of new public school teachers using their years of graduation (Larsen et al., 2020). In the 2011-2012 SASS, we pool teachers who graduated in 2009, 2010, or 2011; in the 2015-2016 NTPS, we pool teachers who graduated in 2012, 2013, 2014, or 2015; and in the 2017-2018 NTPS, we pool teachers who graduated after 2015. Combining the teacher sample in three waves, we resemble a yearly repeated cross-section panel for newly-minted public school teachers from 2009 to 2017.

Based on the resembled panel, we first investigate the license type of a teacher. As suggested by the immediate drop in new teacher graduates identified in the main analysis, the teacher composition in the licensure background of schools should change because fewer teachers are qualified for a standard license. Some school districts are allowed to fill vacancies with teachers with temporary licenses which typically do not require edTPA. Applying the generalized DID model to teacher-level data, we analyze an indicator that equals 1 if the new teacher holds a standard full license as the outcome. In Column 1 of Table C4 in the appendix, we find that edTPA reduced the likelihood of having a standard license by

13.8 percentage points. To the extent that job security is crucial for teacher retention, the decrease in teachers who hold a standard license is concerning to addressing teacher shortage.

We next investigate a set of unique subjective measures of teaching readiness. Teachers are asked to rate their first-year readiness, from 1 (not at all) to 4 (very well prepared), in six aspects class management: discipline, methods, subject matter, computer, assess students, differentiate instruction. Based on the six aspects, we construct a single measurement of 'subjective readiness' using factor analysis to avoid multiple hypotheses. In this exercise, we limit our sample to teachers who hold a regular license that requires passing edTPA. Table C5 of the appendix first provides summary statistics of the six raw measures and the factor variable.[40] In Column 2 of Table C4 in the appendix, we find that new teachers in the edTPA states have lower subjective readiness. Referencing the statistics in Table C5 of the appendix, the effect size is about 40% of a standard deviation of the factor variable. The lower subjective readiness coincides with the qualitative evidence that the edTPA exam distracted learning during internship (Greenblatt, 2016; Shin, 2019).

[40]Since the questions are asked only in 2011-2012 SASS and 2015-2016 NTPS, the resembled panel has a shorter sample period from 2009 to 2015 and the treatment effect consists of the first four states.

8 Conclusion

This paper makes the first attempt to provide causal evidence about the effect of edTPA on teacher supply and student performance, leveraging the quasi-random setting where states integrated edTPA into their licensure systems in different years.

For teacher supply, we analyze university-level graduation data from IPEDS which captures the major source of new teachers in the US. We find that edTPA reduced the number of teacher graduates and disproportionately hurt minority candidates in less-selective or minority-concentrated universities. Our results speak to the potential consequence on the existing shortage and diversity issue in the US public schools. The loss of minority teachers is also worrying given many researchers have found that teachers of the same race bring about a role-modeling effect for minority students (Dee, 2004; Gershenson et al., 2018).

Whereas licensure in general is a regulation on inputs, its quality influence on outputs is often obscure. While the student test score is not the only quality aspect, it concerns consumers (i.e. parents and education stakeholders) the most. NAEP provides us a unique test score data to identify students with new teachers among a nationally representative sample of students in the US. Testing different specifications, sample criterion, and heterogeneity, we do not find evidence that edTPA improved student test scores of the new teachers. Cross-checking different data sources (SEDA and SASS/NTPS), we do not see indirect spillover on other teachers while we find fewer teachers holding a regular license. At the same time, we observe edTPA reduced subjective readiness of new teachers.

A limitation we face is the lack of post-treatment years for the student outcomes. New teachers usually struggle in the first few years, and the skills they learn in the exams may take time to fully realize. While our discussion is constrained by the adoption timing and sample, this research serves as a starting point. The explanations behind the statistical patterns we document will be an important future agenda for both qualitative and quantitative research. An important note is that there may be positive changes in teaching methods that secondary data could not reflect. Our aim is not to provide an exhaustive list of explanations, but to document important cause-and-effect patterns.

A final note is that our results do not cast a veto against the entire teacher licensure system. Rather, we focus on a particular component of the licensure system that is frequently debated in the current education community. Our discussion is widely applicable to the educational policymakers nationwide, especially in the states which had integrated or are planning to integrate edTPA as a necessary component for initial teacher licensure. As of the time we prepare the manuscript, Georgia, Washington, and Wisconsin had removed the edTPA requirements, while Texas is trying a pilot program. The heterogeneity patterns we identify provide policymakers the areas to improve the assessment, if they add or retain the mandatory nature of edTPA. For example, a middle-ground solution is to provide more supports (financially and mentally) and guidelines to help prospective teachers get through the hurdle, which is found to have improved the experience of teacher candidates (Lachuk and Koellner, 2015; Muth et al., 2018).

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Appendix to: "Teacher Licensing, Teacher Supply, and Student Achievement: Nationwide Implementation of edTPA"

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April, 2023

A Additional Summary Statistics

Table A1: Summary statistics (IPEDS) - Include optional states

	Mean	SD	Min	Max
A. Outcomes:				
Education graduates	138.25	184.57	0.00	3496.00
Education graduates (white)	100.85	135.32	0.00	1763.00
Education graduates (non-white)	37.40	71.67	0.00	1968.00
B. Time-varying controls:				
Graduates (non-education majors)	1623.25	2190.37	1.00	16364.00
Minority graduates (% of non-education majors)	18.17	18.72	0.00	100.00
SAT submission rate	51.70	33.25	0.00	100.00
ACT submission rate	54.24	30.44	0.00	100.00
SAT 25 percentile score	474.28	65.21	215.00	745.00
SAT 75 percentile score	581.62	64.95	349.00	800.00
ACT 25 percentile score	20.24	3.33	3.00	33.00
ACT 75 percentile score	25.44	3.27	8.00	35.00
First-year FT enrollment	1101.15	1370.80	6.00	10099.00
Part-time/full-time faculty ratio	0.03	0.11	0.00	2.60
Grant (% student)	76.63	16.46	16.00	100.00
Grant (dollar amount, thousands)	46209.60	51275.09	198.32	488027.59
Loan (% student)	58.71	16.50	0.00	100.00
Loan (dollar amount, thousands)	21562.41	24967.36	0.00	406393.00

Sources: IPEDS 2011-2019.
Notes: This table shows summary statistics of sample for teacher supply using IPEDS, including optional states.

Table A2: Summary statistics (NAEP) - Include optional states

	Grade 4 Math	Grade 4 Reading	Grade 8 Reading
A. Outcomes:			
Assessment score	235.44 (28.22)	216.08 (34.62)	251.99 (37.37)
B. Student controls:			
White	0.43 (0.49)	0.43 (0.50)	0.48 (0.50)
Black	0.15 (0.36)	0.15 (0.36)	0.14 (0.35)
Hispanic	0.29 (0.45)	0.29 (0.45)	0.25 (0.43)
Female	0.49 (0.50)	0.50 (0.50)	0.49 (0.50)
Individualized Education Program (IEP)	0.13 (0.33)	0.12 (0.32)	0.11 (0.31)
English learner	0.10 (0.31)	0.10 (0.30)	0.06 (0.24)
C. School controls:			
Charter school	0.05 (0.22)	0.06 (0.23)	0.05 (0.22)
Urban area	0.77 (0.42)	0.77 (0.42)	0.74 (0.44)
Share of black student	18.38 (26.09)	18.21 (25.75)	16.76 (25.07)
Lunch program	0.58 (0.49)	0.57 (0.49)	0.52 (0.50)
Student enrollment ($\geq$ 500)	0.49 (0.50)	0.49 (0.50)	0.52 (0.50)
Number of Student	70,390	72,970	56,940

Sources: NAEP 2009, 2011, 2013, 2015, 2017, and 2019.
Notes: This table shows summary statistics of the sample (students with new teachers) for student achievement using NAEP, including optional states. The mean is shown in the cell while the standard deviation is shown in the parentheses. Each column presents one of the three student assessment samples: Math at Grade 4, Reading at Grade 4, and Reading at Grade 8. Raw assessment scores are reported in the summary statistics. The number of observations is rounded to the nearest 10 per IES disclosure guidelines.

Table A3: Summary statistics (NAEP) - Estimation sample, by traditional/alternative route

	Grade4 Math		Grade4 Reading		Grade8 Reading	
	Traditional	Alternative	Traditional	Alternative	Traditional	Alternative
A. Outcomes:						
Assessment score	235.76	227.99	216.75	208.02	252.97	247.66
	(28.20)	(29.49)	(34.39)	(36.09)	(37.37)	(36.14)
B. Student controls:						
White	0.46	0.28	0.46	0.29	0.50	0.33
	(0.50)	(0.45)	(0.50)	(0.45)	(0.50)	(0.47)
Black	0.14	0.27	0.13	0.26	0.13	0.27
	(0.35)	(0.44)	(0.34)	(0.44)	(0.34)	(0.44)
Hispanic	0.28	0.34	0.29	0.33	0.24	0.28
	(0.45)	(0.48)	(0.45)	(0.47)	(0.43)	(0.45)
Female	0.49	0.48	0.50	0.49	0.49	0.49
	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)
IEP	0.13	0.13	0.12	0.13	0.11	0.13
	(0.33)	(0.34)	(0.33)	(0.34)	(0.32)	(0.34)
English learner	0.10	0.13	0.09	0.12	0.06	0.07
	(0.29)	(0.33)	(0.29)	(0.32)	(0.23)	(0.26)
C. School controls:						
Charter school	0.05	0.10	0.05	0.11	0.04	0.09
	(0.22)	(0.30)	(0.22)	(0.31)	(0.20)	(0.29)
Urban area	0.77	0.84	0.77	0.83	0.74	0.80
	(0.42)	(0.37)	(0.42)	(0.38)	(0.44)	(0.40)
Share of black student	16.68	34.06	16.50	33.34	15.73	31.54
	(25.64)	(36.32)	(25.32)	(35.93)	(25.35)	(34.33)
Lunch program	0.56	0.70	0.56	0.69	0.50	0.63
	(0.50)	(0.46)	(0.50)	(0.46)	(0.50)	(0.48)
Student enrollment ($\geq$ 500)	0.45	0.48	0.45	0.49	0.50	0.51
	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)
Observations	51,460	9,040	53,530	9,470	41,260	12,680

Sources: NAEP 2009, 2011, 2013, 2015, 2017, and 2019.
Notes: This table shows summary statistics of estimation sample (students with new teachers) for student achievement using NAEP, by traditional and alternative route of teachers. The mean is shown in the cell while the standard deviation is shown in the parentheses. Each column presents one of the three student assessment samples: Math at Grade 4 , Reading at Grade 4, and Reading at Grade 8. Raw assessment scores are reported in the summary statistics. The number of observations is rounded to the nearest 10 per IES disclosure guidelines.

B Additional Results on Teacher Supply

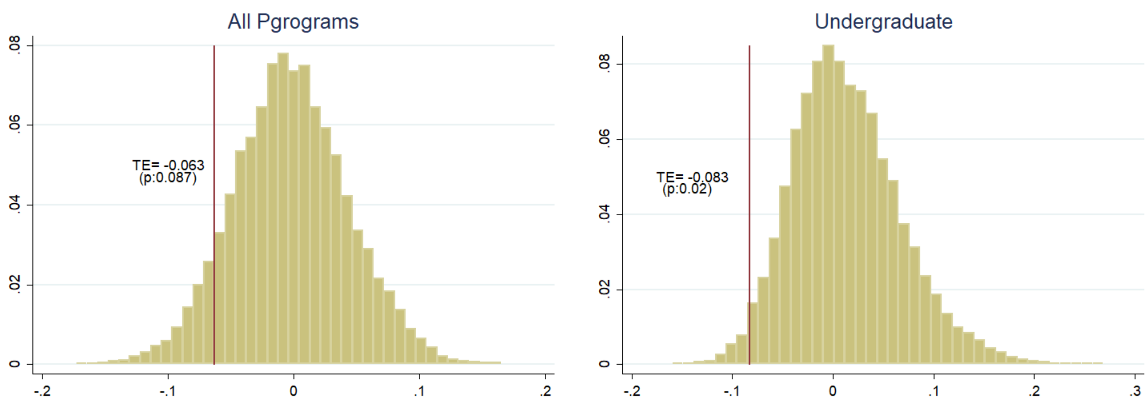
Table B1: Alternative data - State-level initial licensure in Title II

	(1)	(2)	(3)	(4)
edTPA	-0.295*	-0.125*	-0.122*	-0.0990*
	(0.148)	(0.0686)	(0.0685)	(0.0556)
Observations	449	449	449	449
R-squared	0.192	0.335	0.335	0.382
State control		X	X	X
Confounding policies#		X	X	X
Accountability Reform			X	X
Regional trends				X

Source: Title II, 2011-2019

Notes: Washington is dropped due to the measurement errors in the data. Dependent variable in all regressions is the log of the number of initial teacher licensure issued in a state. All regressions include year and state fixed effects, and state-level time-varying controls (unemployment rate, percent of college-educated population, black population). #Confounding policies are based on Table A1 of Kraft et al. (2020). All regressions are weighted by the 18-65 state population (per 10,000). Standard errors in the parenthesis are clustered at the state level. ***, **, and * represent 1%, 5%, and 10% significant level, respectively.

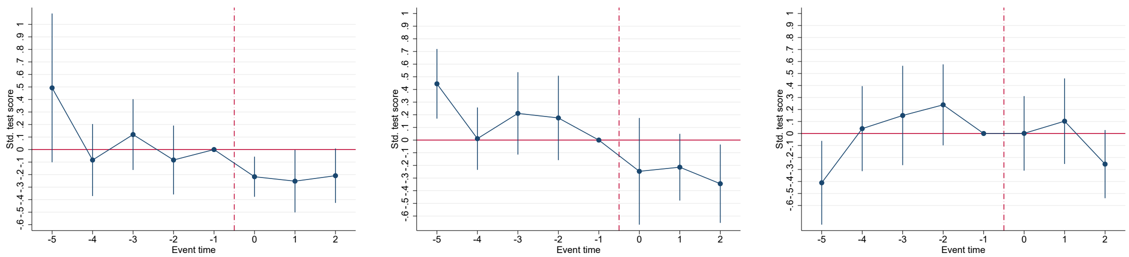
Figure B1: Permutation tests: Placebo treatments in non-edTPA states



Note: The permutation test in this figure constructs the distribution of placebo effects (10,000 rounds of permutation) using the non-edTPA states that implemented accountability reforms (Kraft et al., 2020). The first figure compares our treatment effect on all programs in Column 3 of Table 3 with the empirical placebo effects. The placebo treatments mimic the implementation timing of edTPA relative to the teacher accountability reform in the eight edTPA states: two of them implemented edTPA 1 year prior; two implemented edTPA 1 year after; two implemented edTPA 2 years after; and the remaining two implemented edTPA 4 and 5 years after. The second figure compares our treatment effect on undergraduate programs in Column 4 of Table 3 with the empirical placebo effects. Our estimates do not overlap with the placebo distribution, implying that the identified treatment effects less likely to pick up residual influences of the competing policy.

C Additional Results on Student Outcomes

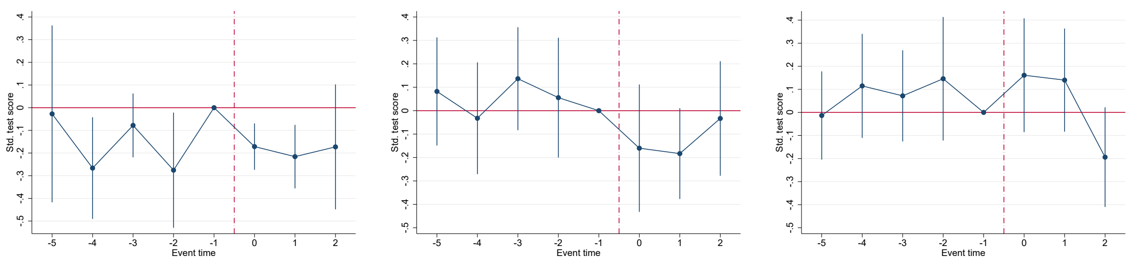
Figure C1: Event study figures for the alternative route sample: No controls



(a) Grade4 Math **(b)** Grade4 Reading **(c)** Grade8 Reading

Sources: NAEP 2009, 2011, 2013, 2015, 2017, and 2019.
Notes: The figure shows estimates using student samples with teachers obtained the license through alternative routes. The dependent variable is the standardized test score for Grade 4 Math (subfigure a), Grade 4 Reading (subfigure b), and Grade 8 Reading (subfigure c). Event period -1 is normalized to 0. The underlying regressions contain no controls to show raw data patterns, conditional on state, and year fixed effects. The figures show the 95% confidence interval with robust standard errors clustered at the state level.

Figure C2: Event study figures for the alternative route sample: With controls



(a) Grade4 Math **(b)** Grade4 Reading **(c)** Grade8 Reading

Sources: NAEP 2009, 2011, 2013, 2015, 2017, and 2019.
Notes: The figure shows estimates using student samples with teachers obtained the license through alternative routes. The dependent variable is the standardized test score for Grade 4 Math (subfigure a), Grade 4 Reading (subfigure b), and Grade 8 Reading (subfigure c). Event period -1 is normalized to 0. The underlying regressions contain student and school controls listed in Table 2, conditional on state, and year fixed effects. The figures show the 95% confidence interval with robust standard errors clustered at the state level.

Table C1: Impacts of edTPA reforms on students' achievement (By subsample)

	Std. test score								
Panel A	Grade 4 Math			Grade 4 Reading			Grade 8 Reading		
Full sample	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
edTPA	-0.038	-0.039	-0.005	-0.080*	-0.080***	-0.074**	-0.058	0.010	0.019
	(0.050)	(0.030)	(0.027)	(0.042)	(0.029)	(0.029)	(0.040)	(0.039)	(0.044)
R-squared	0.033	0.339	0.341	0.027	0.379	0.380	0.040	0.379	0.379
Observations	60,500	60,500	60,500	63,000	63,000	63,000	53,940	53,940	53,940
Panel B									
Traditional route	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
edTPA	-0.030	-0.023	-0.005	-0.048	-0.044	-0.054	-0.059	0.004	0.010
	(0.051)	(0.032)	(0.033)	(0.038)	(0.026)	(0.034)	(0.051)	(0.039)	(0.044)
R-squared	0.029	0.331	0.332	0.025	0.309	0.370	0.036	0.371	0.372
Observations	51,460	51,460	51,460	53,530	53,530	53,530	41,260	41,260	41,260
Panel C									
Alternative routes	(19)	(20)	(21)	(22)	(23)	(24)	(25)	(26)	(27)
edTPA	-0.254**	-0.156*	-0.053	-0.380***	-0.303***	-0.218***	-0.070	0.037	0.031
	(0.104)	(0.083)	(0.067)	(0.124)	(0.089)	(0.061)	(0.081)	(0.054)	(0.060)
R-squared	0.066	0.357	0.363	0.061	0.337	0.405	0.055	0.381	0.382
Observations	9,040	9,040	9,040	9,470	9,470	9,470	12,680	12,680	12,680
State FE	X	X	X	X	X	X	X	X	X
Year FE	X	X	X	X	X	X	X	X	X
Student controls		X	X		X	X		X	X
School controls		X	X		X	X		X	X
Policy controls #			X			X			X

Sources: NAEP 2009, 2011, 2013, 2015, 2017, and 2019.
Notes: Panel A uses full student sample, Panel B uses student samples with teachers obtained the license through a traditional teacher preparation program, and Panel C uses students with teachers obtained the license through alternative routes. Samples in all panels exclude students in optional states. The dependent variables in column (1) to (3), (4) to (6), and (7) to (9) are Grade 4 Math, Grade 4 Reading, and Grade 8 Reading, respectively. The test scores are standardized to a zero mean and one standard deviation in the sample. 'edTPA' refers to the treatment indicator. All regressions include state and year fixed effects. Student and school controls are listed in Table 2. #The policy controls are based on Table A1 of Kraft et al. (2020). Standard errors in brackets are clustered at the state level. Sample sizes are rounded to the nearest 10 per IES disclosure guidelines. ***, **, and * represent 1%, 5%, and 10% significant level, respectively.

Table C2: Balancing test - Correlation between edTPA and student characteristics (Full sample)

	White	Black	Hispanic	Female	IEP	Eng learner
Panel A. Grade4 Math	(1)	(2)	(3)	(4)	(5)	(6)
edTPA	-0.008	-0.012	0.011	-0.012	-0.007	0.014
	(0.018)	(0.015)	(0.016)	(0.010)	(0.010)	(0.014)
R-squared	0.097	0.135	0.071	0.001	0.007	0.049
Observations	60,500	60,500	60,500	60,500	60,500	60,500
Panel B. Grade4 Reading	(7)	(8)	(9)	(10)	(11)	(12)
edTPA	-0.012	-0.013	0.020	0.011	0.002	0.005
	(0.021)	(0.021)	(0.018)	(0.013)	(0.014)	(0.018)
R-squared	0.092	0.125	0.072	0.001	0.008	0.052
Observations	63,000	63,000	63,000	63,000	63,000	63,000
Panel C. Grade8 Reading	(13)	(14)	(15)	(16)	(17)	(18)
edTPA	-0.045*	0.006	0.036**	-0.014	0.011	0.023
	(0.026)	(0.017)	(0.014)	(0.014)	(0.016)	(0.014)
R-squared	0.126	0.162	0.114	0.001	0.011	0.039
Observations	53,940	53,940	53,940	53,940	53,940	53,940
State FE	X	X	X	X	X	X
Year FE	X	X	X	X	X	X

Sources: NAEP 2009, 2011, 2013, 2015, 2017, and 2019.
Notes: The samples in panel A, B, and C are students with new teachers from both traditional and alternative routes, excluding those in optional states, from three NAEP assessments: Grade 4 Math, Grade 4 Reading, and Grade 8 Reading, respectively. The dependent variables are students' predetermined characteristics, while the independent variable 'edTPA' is an indicator where its value equals 1 if state s passes compulsory edTPA policy and 0 otherwise. All regressions include state fixed effects and year fixed effects. Robust standard errors clustered at state level are in brackets. Sample sizes are rounded to the nearest 10 per IES disclosure guidelines. $***p < 0.01, **p < 0.05, *p < 0.1$.

Table C3: Impacts of edTPA on students' achievement: NAEP and SEDA

	Std. test score					
	Math		Reading		Total	
Panel A. NAEP	(1)	(2)	(3)	(4)	(5)	(6)
edTPA	-0.033	0.003	-0.001	0.001	-0.017	0.006
	(0.036)	(0.029)	(0.020)	(0.022)	(-0.026)	(0.024)
R-squared	0.034	0.355	0.003	0.384	0.003	0.362
Observations	1,403,080	1,403,080	1,391,580	1,391,580	2,794,660	2,794,660
Panel B. SEDA	(7)	(8)	(9)	(10)	(11)	(12)
edTPA	-0.029	-0.004	0.011	0.033	-0.009	0.015
	(0.038)	(0.039)	(0.028)	(0.036)	(0.030)	(0.035)
R-squared	0.226	0.528	0.216	0.559	0.222	0.543
Observations	374,951	374,951	383,192	383,192	758,143	758,143
State-grade FE	X	X	X	X		
State-grade-subject FE					X	X
Year FE	X	X	X	X	X	X
Controls		X		X		X
Policy controls #		X		X		X

Sources: NAEP 2009, 2011, 2013, 2015, 2017, and 2019; and SEDA 2009-2018.
Notes: The table shows estimates using the student level data from grades 4 and 8 in NAEP (Panel A) and the district-by-year panel from grades 3 to 8 in SEDA (Panel B) containing eight states that adopted edTPA as the compulsory option and the control states that do not introduce edTPA. The test scores are standardized to a zero mean and one standard deviation. 'edTPA' refers to the treatment indicator. All regressions include state and year fixed effects. Controls in Panel A include student and school controls as listed in equation 2, while controls in Panel B include time-varying district characteristics from NCES's ACS Education Tabulations (i.e., log value of enrollment number, population percentage with a college degree or above, percentage of black population, and log value of household median income). #The policy controls are based on Table A1 of Kraft et al. (2020). Standard errors in brackets are clustered at the state level. Sample sizes in Panel A are rounded to the nearest 10 per IES disclosure guidelines.

Table C4: Other measures using NTPS/SASS

	Regular license	Subjective readiness
	(1)	(2)
edTPA	-0.138**	-0.328**
	(0.0536)	(0.125)
Observations	4,050	2,300
R-squared	0.113	0.091

Sources: Schools and Staffing Survey (SASS), 2011-2012; National Teacher and Principal Survey (NTPS), 2015-16 and 2017-2018.

Notes: We resemble an annual repeated cross-section panel from 2009 to 2017 (Column 1) and from 2009 to 2015 (Column 2) using the graduation year of a teacher (Larsen et al., 2020). All regressions include the policy controls in the main analysis, a regional time trend, and state and year-fixed effects. In Column 2, the model also controls for teacher (female, black, Hispanic, other races, union, age, and the 2001 standardized average SAT of the graduating college pooled from College Scorecard) and school (elementary/secondary school, city, teacher-student ratio, percent of LEP, percent of IEP) characteristics. Sample weight applies in all regressions. Standard errors are clustered at the state level. Sample sizes are rounded to the nearest 10 per IES disclosure guidelines.

Table C5: Summary Statistics of Teacher's Subjective Readiness

	Mean	SD	Min	Max
First year preparation - discipline	2.70	0.79	1.00	4.00
First year preparation - methods	2.98	0.75	1.00	4.00
First year preparation - subject matter	3.26	0.73	1.00	4.00
First year preparation - computers	2.96	0.85	1.00	4.00
First year preparation - Assess students	2.92	0.74	1.00	4.00
First year preparation - differentiate instruction	2.80	0.81	1.00	4.00
Factor variable	0.01	0.88	-2.96	1.66
Observations	2300			

Sources: Schools and Staffing Survey (SASS), 2011-2012; National Teacher and Principal Survey (NTPS), 2015-16.
Notes: The table shows the summary statistics of teacher's subject readiness across six categories, as well as the factor variable. Sample sizes are rounded to the nearest 10 per IES disclosure guidelines.