Teacher Accountability Reforms and the Supply and Quality of New Teachers

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Abstract

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Keywords: Teacher Supply, Teacher Evaluation, Teacher Quality, Education, Teacher Hiring

JEL No. I21, I28, J22, J24

I.   Introduction

In response to mounting public pressure and strong incentives from the federal government, state legislatures across the country have enacted laws aimed at increasing accountability for public school teachers. By 2016, 44 states had implemented major reforms to their teacher evaluation systems intended to increase the rigor of performance reviews and streamline the dismissal process. A handful of states also enacted laws that restricted tenure protections, lengthened the probationary period for teachers, limited the scope of collective bargaining with teacher unions, and eliminated mandatory union dues. Together, these accountability reforms weakened the employment protections unions could provide to teachers.

Proponents of accountability reforms argued that high-stakes evaluation systems combined with merit pay could transform the teacher labor force by removing low-performing teachers and attracting more highly-qualified candidates into the profession (Hanushek, 2009; Klein, 2010). Opponents argued that high-stakes teacher evaluation systems were unreliable and would only serve to make teaching a less attractive profession (Fullan, 2011). However, limited empirical evidence exists on whether and how these reforms actually have affected the teacher labor market.

What evidence we have comes from studies that examine the effect of accountability reforms on the effort and career decisions of *current* teachers. Prior research has shown that school-level accountability reforms decrease teachers' perceptions of job security (Reback, Rockoff, and Schwartz, 2014) and increase their effort via reduced absences (Jacob, 2013; Gershenson, 2016). Several studies have also shown that high-stakes evaluation systems have increased voluntary attrition among lower-performing teachers in large urban school districts

(Dee and Wyckoff, 2015; Loeb, Miller, and Wyckoff 2015; Sartain and Steinberg, 2016; Cullen, Koedel, and Parsons, 2019).

This paper addresses the largely unexamined question of how accountability reforms affect *new* teachers. Using both event study and difference-in-differences methods, we exploit arguably exogenous variation in the timing of teacher evaluation and other accountability reforms across states to provide the first empirical evidence on how these reforms affected the supply of prospective public school teachers and the ability of schools to fill vacant teaching positions. We then examine the distributional effects of accountability reforms on the quality of newly hired teachers as measured by the selectivity of their undergraduate institutions and whether they held an emergency license.

Several studies have examined the potential learning gains from dismissing low-performing teachers through simulation analyses. These studies implicitly assume that dismissed teachers can always be replaced with average-quality novice teachers (Gordon, Kane, and Staiger, 2006; Hanushek, 2009; Staiger and Rockoff, 2010; Winters and Cowen, 2013a, 2013b; Goldhaber and Hansen, 2010). However, both qualitative case studies (Johnson, 2019) and simulation analyses suggest this assumption may not be realistic, particularly for low-performing and hard-to-staff schools. Rothstein's (2015) simulation of a teacher dismissal policy that allows for potential effects on current and future labor supply suggests that it would require "substantial increases in teacher salaries" to continue to draw equivalent numbers of new entrants into the teaching profession (p.126). Our analyses provide new empirical evidence to evaluate the assumption that accountability reforms do not affect the ability of schools to fill open teaching positions or the quality of the new teachers who fill these vacancies.

We examine the effect of evaluation reforms on the supply of new teachers using state-by-year panel data from 2002 to 2016 on the number of initial teaching licenses states granted. We also examine the effect of evaluation reforms on teaching position vacancies, proxy measures of new teacher quality, and a range of potential mechanisms using four waves of nationally representative data on public schools and teachers.

The broad nature of education reforms enacted by some state legislatures during this period makes definitively isolating the effect of evaluation reforms from other contemporaneous accountability reforms and education initiatives challenging. We attempt to address this challenge in several ways, while recognizing the limitations created by the bundled nature of these reforms. We analyze the effects of implementing high-stakes teacher evaluation systems both independently as well as part of a joint treatment of accountability reforms to examine how the intensity of these reforms affected new teacher labor supply. We define our primary treatment indicator as the year evaluation reforms were *implemented* statewide, which serves to break much of the collinearity with other education initiatives passed in the same legislative sessions. Finally, we control for a range of concurrent education reforms including winning a Race to the Top (RTT) grant, expanding alternative certification, implementing Common Core State Standards (CCSS), requiring new licensure exams, pass rates for these licensure exams, and increases in the proportion of teachers' salaries that are withheld to fund pension obligations.

We find that implementing high-stakes evaluation reforms reduced the supply of newly licensed teachers by 16 to 18%. Using an alternative measure of supply, we also find that evaluation reforms decreased the number of degree completers from graduate teacher preparation programs by 8 to 10%. The more accountability reforms enacted by states, the greater the

intensity of these effects. Flexible models suggest that evaluation reforms resulted in a steady decline in new labor supply over time.

Given that the number of graduates from teacher preparation programs each year has historically been more than double the number of vacant teaching positions in the U.S. (Cowan et al., 2016), this reduction in teacher supply could have little effect on the ability of schools to fill vacant teaching positions. However, we find that reductions in new teacher supply caused by evaluation reforms appear to bind for schools. Evaluation reforms increased the probability a school had at least one unfilled teaching vacancy by 2.6 percentage points relative to a pre-reform mean of 4%. As prior evidence would suggest, these effects are concentrated in traditionally hard-to-staff schools that serve larger proportions of disadvantaged students (Steele, Murnane, and Willett, 2010; Clotfelter et al., 2008).

At the same time, we also find suggestive evidence that evaluation reforms increased the quality of new teachers. These analyses examine the best available state-level measure for teacher quality: the selectivity of teachers' undergraduate institutions. Although this input measure of quality is only a weak proxy for teacher performance on the job, it allows us to explore the nature of supply-side responses to accountability reforms. We find that the increase in teacher quality is primarily driven by a decrease in the supply of teachers coming from less competitive undergraduate institutions.

We explore a range of alternative explanations for the effects we find on teacher supply and quality. These robustness tests suggest that our results are not driven by declines in demand for public school teachers, changes in macroeconomic conditions that affected the broader labor market, or shifts in the demographic composition of school-age children. We also show that the effects of evaluation persist even when we control for other related accountability reforms or

restrict the sample of treated states to states where the only accountability reform was the adoption of a new teacher evaluation system. These findings suggest that new evaluation systems were likely the leading factor behind accountability reforms, but that contemporaneous accountability reforms intensified effects by further limiting job protections provided by unions.

Finally, we explore possible mechanisms for our findings by analyzing new teachers' perceptions of their working conditions. These analyses show that among new teachers, evaluation reforms substantially decreased perceptions about job security, job satisfaction, cooperative effort, and control over their teaching. We conclude with a discussion of the implications for policy, practice, and future research.

II.   Teacher Accountability Reforms

A. Teacher Evaluation

Efforts to introduce greater accountability in schools and classrooms have ebbed and flowed throughout the history of U.S. public education. The No Child Left Behind (NCLB) Act, passed in 2002, expanded test-based school accountability policies nationally and established more rigorous teacher licensure standards with the aim of improving teacher quality. In the following years, a growing body of research on teacher effectiveness exploited new administrative datasets linking students to teachers that NCLB helped to create. Three seminal findings from this research served as signposts for the Obama administration's efforts to promote teacher accountability reform: 1) the effects teachers have on student learning are large and vary considerably across teachers, 2) teacher qualifications are only weakly related to student learning, and 3) teacher evaluation systems were failing to differentiate among teachers despite the large differences in teacher effectiveness (Kraft, 2018).

Starting in 2009, the Obama administration leveraged $4.35 billion from the American Reinvestment and Recovery Act to fund the RTT grant competition. The RTT application rubric detailed specific evaluation system reforms required for a competitive proposal such as evaluating teachers using multiple measures including student achievement growth, rating teachers on a scale with multiple categories, conducting annual evaluations, and using evaluation ratings to inform high-stakes personnel decisions. In 2012, the Obama administration then made adopting evaluation reforms one of several conditions for states to receive a waiver from the increasingly stringent accountability consequences of failing to meet Annual Yearly Progress targets set by NCLB. Between February 2012 and April 2014, forty-three states and DC were granted a waiver from NCLB's provisions.

By 2016, a total of 44 states had passed legislation that mandated major teacher evaluation reforms (National Counsel on Teacher Quality [NCTQ], 2016). The design of these new evaluation systems differed meaningfully across and within states, but all shared several common features. The vast majority of states adopted state-wide systems but allowed districts a degree of discretion in adapting these systems to local contexts, while a handful mandated the use of state-designed systems or only provided broad guidelines (Steinberg and Donaldson, 2016). Almost every system incorporated multiple measures of teacher performance and differentiated teachers across multiple performance rating categories. The two most common performance measures in terms of their use were classroom observation ratings and test-based scores such as value-added measures or student growth percentiles. At the same time, the weights assigned to test-based measures of student performance ranged considerably across states from 0 to 50 percent. Approximately half of the states also phased in requirements to use

test-score based measures across several years following statewide implementation of the evaluation system.

On paper, most states and districts emphasized that the primary goal of evaluation reforms was to support teachers to improve their instruction. Consistent with this espoused goal, Steinberg and Donaldson (2016) find that the vast majority (83%) of states explicitly linked teacher evaluation ratings to professional development requirements. About 60% of states established systems where low ratings could lead to teachers being dismissed, and almost 50% permitted teachers to be denied tenure based on their performance. Some states also delayed the use of evaluation scores to inform high-stakes personnel decisions until after initial statewide implementation.

On December 10th of 2015, Present Obama signed the Every Student Succeeds Act (ESSA) into law, which greatly curtailed the degree to which the federal government could mandate or make federal funding conditional on a range of education policy prescriptions including teacher evaluation reforms. Relevant to our study, many states took advantage of this increased autonomy to revise their teacher evaluation systems. States first submitted proposed ESSA plans to the Federal Department of Education (ED) in the spring and fall of 2017. Thus, our panel, which runs through 2016, largely captures the effects of high-stakes teacher evaluation reforms implemented before states made their proposed revisions public. By 2019, most states had retained the core features of high-stakes teacher evaluation systems with the most prominent change being the 11 states that replaced objective measures of student growth with teacher-developed Student Learning Objectives (SLOs) or abandoning test-based performance measures altogether (Ross and Walsh, 2019).

B. Contemporaneous Accountability Reforms

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Several states also adopted a range of related laws that weakened teacher job protections and served to enhance how performance ratings from teacher evaluation systems could be used for high-stakes decisions. In five states, new laws effectively eliminated the ability of new teachers to earn tenure, which provides considerable job security to teachers after just several years of employment (all between 2011 and 2014).[1] Among the states that maintained tenure protections, twelve increased the number of probationary years during which new teachers can be dismissed without cause (all between 2010 and 2015). Six states restricted or eliminated mandatory collective bargaining for teachers, which often limited a union's ability to negotiate over evaluation systems (three between 2003 and 2005 and three in 2011). Finally, four states passed "right to work" laws that eliminated mandatory union dues, creating the possibility that unions would have less funding and lower membership to advocate for things like strong job protections (all between 2012 and 2016).

C. Other Concurrent Education Policy Reforms

Accountability reforms were not the only education policy reform states implemented during this time period. Several of these reforms, such as new licensure tests and alternative pathways into teaching, largely occurred before the push for teacher accountability reforms. The NCLB Act's high-quality teacher provisions prompted many states to adopt new licensure tests, e.g., basic skills tests (seven states between 2003 and 2008 and nine states between 2012 and 2016), pedagogical knowledge tests (nine states between 2003 and 2005 and four states in 2015), and subject content knowledge tests (14 states between 2003 and 2008 and six states between 2015 and 2016). Between 2002 and 2016, the total number of alternative teacher certification

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[1] In Georgia, HB1187 eliminated the ability for newly hired teachers after 2000 to earn tenure. This bill was later rescinded by SB193 in 2003.

programs doubled from 66 to 132. However, most of these alternative programs were established prior to 2011 (e.g., there were 116 programs in 2010).

Eighteen states won RTT grants. These grants provided meaningful financial resources, but the four-year total of these awards amounted to less than 1% of most state education budgets. The influence of the RTT grant competition was far broader as 46 states applied across three RTT funding rounds[2] For instance, RTT grant scoring criteria incentivized states to implement new college and career ready standards. Between 2011 and 2014, 43 states adopted the Common Core State Standards (CCSS). Additional reforms also included thirty-two states that increased the proportion of teachers' salaries that were withheld to fund pension obligations at least once between 2002 and 2016.

We describe our data sources and coding procedures for all the accountability other education policy reforms described above and in Appendix A and provide a complete list of education reform dates for each state in Appendix Table A1.

III.   Conceptual Framework

How might teacher accountability reforms affect the supply and quality of prospective teachers? Evidence suggests that individuals who select into teacher preparation programs place a higher premium on job security than other college graduates (Bowen et al., 2015; Lang and Palacios, 2018). Accountability reforms that weaken job security could decrease new labor supply in the absence of offsetting increases in teacher salaries. Reforms could also decrease new labor supply if they make the profession less enjoyable by, for example, decreasing teacher autonomy through a prescribed curriculum aligned with high-stakes tests and an increased focus

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[2] Private foundations and philanthropic organizations such as the Gates and Broad Foundations and the New Schools Venture Fund also invested millions of dollars to support evaluation reforms across the country.

on test preparation (Reback et al., 2014). Importantly, even if accountability reforms have no direct effect on job protections or satisfaction, they may still affect new labor supply if they create the perception among potential entrants that teaching is a less secure or enjoyable career (Kraft and Gilmour, 2016).

Here we provide an intuitive discussion of the potential effects of teacher accountability reforms on the supply and quality of prospective teachers based on a simple Roy (1951) model.[3] Consider a labor market where individuals choose between a career teaching in public schools or an alternative occupation that represents all outside options, by choosing the occupation that maximizes their expected earnings. As noted by Nagler et al. (forthcoming) among others, such a model predicts negative selection on ability into teaching if two conditions are met: 1) ability is valued in both sectors[4] and 2) teaching has lower returns to ability. Evidence from several studies suggest these two conditions are likely to hold in the U.S. context.[5]

The effect of accountability reforms on the supply of new teachers is unambiguously negative in the model. Similar to Angrist and Guryan (2004, 2008), we conceptualize accountability reforms as increasing the relative costs associated with teaching through the monetized costs of declines in job security and job satisfaction in teaching relative to the alternative occupation. This reduces expected earnings in the teaching profession and leads to an intuitive decline in the share of individuals that choose teaching as a career.

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[3] See Appendix B for a more formal analysis.
[4] More specifically the type of ability that is valued in both sectors is positively correlated and the correlation is sufficiently strong to induce migration across sectors.
[5] In terms of the first condition, Chingos and West (2012) find that higher value-added teachers in Florida tend to earn more outside of teaching than do teachers who are less effective in promoting student achievement. Feng and Sass (2017) find that high-quality teachers (as measured by value-added) are significantly more likely to exit teaching to pursue more attractive outside options than average quality teachers. Similarly, Britton and Propper (2016) find that teacher quality is lower in local labor markets where the gap between outside of teaching wages and teaching wages are higher. In terms of the second condition, see Lang and Palacios (2018), Hoxby and Leigh (2004), Goldhaber et al. (2007), and Chingos and West (2012) for evidence that wages are more compressed for individuals employed in public-sector teaching than for individuals employed in the private sector.

The effect of accountability reforms on the quality of prospective teachers is more ambiguous. The effect of these reforms depends on whether the relative costs associated with teaching are the same for everyone or vary with an individual's ability. When the relative costs are the same for everyone, individuals on the margin between teaching and the alternative occupation are the highest ability prospective teachers. Consequently, when costs increase and supply declines, the average ability of individuals choosing a career in teaching also declines. On the other hand, there is reason to believe *relative* costs decline with ability. For example, passing licensure tests is likely costlier for low-ability individuals. Similarly, reductions in perceived job security or stress associated with high-stakes teacher evaluations may be lower for higher-ability individuals. If relative costs decline with ability, there is both a high- and low-ability individual on the margin between teaching and the alternative occupation. As a result, when relative costs increase, both the share of high- and low-ability individuals that choose teaching as a career decline leading to an ambiguous change in new teacher quality (See Appendix Figure B1).

While teacher accountability reforms may increase the perceived costs associated with teaching, merit pay programs based on evaluation ratings could attract more high-ability teachers into the profession. In theory, merit pay programs could increase the relative return to ability in the teaching sector enough to offset the cost increases associated with accountability reforms, leading to an increase in the quality of prospective teachers. However, only 20% of states designed some type of bonus or merit pay system (Steinberg and Donaldson, 2016). Even among districts that won federal grants to design and fund merit pay systems, these programs were often poorly understood by teachers with relatively small bonuses that were awarded to a majority of

teachers (Chiang et al., 2017).[6] Very few of these programs were sustained over time. Thus, even with merit pay reforms it remains theoretically unclear how accountability might affect the quality of prospective teachers. Our empirical results help to shed light on this question.

IV.   Data

We conduct our analyses using an original state-by-year panel from 2002 through 2016 that combines measures from a range of datasets maintained by the U.S. Department of Education, the Bureau of Labor Statistics (BLS), the U.S. Census Bureau, and the National Center for Education Statistics (NCES). We complement these data with repeated waves of the Schools and Staffing Survey (SASS) and the National Teacher and Principal Survey (NTPS). The SASS and NTPS are nationally representative surveys of U.S. schools and teachers conducted by NCES every four years. Critical for our analyses, NCES has maintained a large set of consistent items across administrations on both the Teacher Questionnaire and the School Questionnaire. We use data from the 2003-04, 2007-08, and 2011-12 SASS and 2015-16 NTPS to construct a four-period panel dataset covering the relevant period for evaluation reforms.

Evaluation Reforms: We draw upon two systematic reviews of teacher evaluation reforms to create two binary measures that reflect the timing of evaluation reforms across states (Steinberg and Donaldson, 2016; NCTQ, 2016). We code our preferred measure, *Implement Evaluation,* as one in the fall of the academic year in which the new evaluation systems was implemented statewide for the first time. As shown in Figure 1, states rolled out their new evaluation systems across several years allowing us to jointly model evaluation effects and

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[6] While Race to the Top incentivized states to adopt merit pay, Buck and Greene (2011), argue "merit pay plans are more likely to be symbolic than substantive and more likely to be promised than delivered." See Dee and Wyckoff (2015) for a further discussion of the limited and short-lived nature of most merit pay plans.

control for underlying trends in outcomes over time. We test the robustness of our results using an alternative measure, *Passed Evaluation*, which we code as one in the calendar year in which evaluation reforms were passed by state legislatures.

Accountability Reform Intensity: Given the related nature of the different accountability reforms passed between 2011 and 2016, we construct a composite measure of these reforms to capture the intensity of reform efforts in a state. Our measure is a simple count measure ranging between 0 and 5 that takes on an incremental value of 1 for each of the five teacher accountability reform measures of the period: adopting high-stakes teacher evaluation systems, eliminating or weakening tenure, increasing the length of the probationary period, eliminating or restricting the scope of mandatory collective bargaining, and eliminating mandatory union dues.[7]

Teacher Supply: Ideally, we would measure the supply of new teachers as the total number of candidates that applied for K-12 public school teaching positions for the first time. Although such a measure is unavailable at the national level, data collected by the U.S. ED on the number of initial teacher licenses granted by states each year serves as an advantageous proxy. These federal data collected under Title II requirements capture all new teachers eligible to work in publicly-funded schools (traditional or charter), regardless of certification pathway or licensure type, and links each teacher to their state of intended employment.

The range of licenses include professional certifications granted to graduates of traditional preparation programs, initial certifications granted to graduates of alternative pathway programs, and temporary teaching licenses such as emergency, probationary, or intern teaching credentials. Teachers who enter the profession via alternative pathways such as Teach for America are required to have some type of temporary license to teach while they complete the

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[7] Results based on a non-parametric specification that includes indicators for whether a state adopted 1, 2, 3, 4 or 5 reforms provide support for this simple linear parameterization.

requirements necessary to obtain a provisional teaching license. This measure is also largely

robust to the rapid expansion of the charter school sector during our panel period. The vast

majority of states require teachers who work in charter schools to obtain a state license.[8]

Although licensure reciprocity agreements exist between some states, individuals seeking to

teach in a new state must still acquire a state-specific initial teaching license and will be included

in our data.[9] In Figure 2 we plot national trends in the supply of new teachers as measured by the

number of licenses issued. The relative supply increased from 2002 to 2007 and then declined

sharply during the Great Recession. Relative to pre-recession levels in 2007, the number of new

teaching licenses issued nationally has declined by 23.4%.

 School Hiring: We leverage data from the SASS/NTPS School Questionnaire to construct

two measures related to schools' experiences filling vacant teaching positions. Schools report on

the difficulty of filling vacancies across a range of teaching fields on a four-point Likert scale

(*Easy*, *Somewhat Difficult*, *Very Difficult*, *Could not Fill the Vacancy*). We pool responses across

12 subject areas and construct a binary measure of whether there were *any* unfilled vacancies at

the time the survey was completed (typically in October). This measure of schools' ability to find

qualified candidates complements our analyses of teacher supply and quality. It provides a direct

measure of supply, in terms of applicant volume, and potentially captures a gradient of teacher

quality to the degree that schools screen candidates based on some minimum threshold.

 Figure 3 plots national trends in the proportion of schools with a least one unfilled

teaching position. Nationally, prior to the Great Recession, approximately 4% of schools had at

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[8] Of the 42 states with charter school laws in 2016, 28 states required that teachers obtain state licenses, while 11 states required the majority the teachers to be licensed (around 50 to 75% of teachers). Only three states (Arizona, Alabama [which only allowed charters in 2015], and Louisiana) had no licensure requirement for charter school teachers (Education Commission on the States, 2016).

[9] Evidence suggests that despite reciprocity agreements, costs associated with transferring licensure are quite high (Barnum, 2017; Sindelar et al., 2007).

least one unfilled teaching vacancy. In 2011, the proportion of unfilled teaching vacancies dropped by half to 2% but then rose swiftly to 7.1% by 2015, easily surpassing pre-recession averages.

Teacher Quality: The SASS/NTPS Teacher Questionnaire captures information on the institutions where teachers attended college as well as the type of licensure they hold. These data allow us to merge Barron's rankings of the selectivity of each institutions' admissions process in 2014 as a proxy measure for teacher quality.[10] Barron's rank is measured on a six-point scale ranging from *Non-Competitive* to *Most Competitive*.[11] Research documents a positive, albeit weak, relationship between Barron's rankings and teacher outcomes including pedagogical content knowledge tests, hiring interviews, and lesson demonstrations (Jacob et al., 2016), performance evaluations (Rockoff et al., 2011; Jacob et al., 2016), and value-added to student achievement (Clotfelter, Ladd, and Vigdor, 2006; Boyd et al., 2008). At the same time, other studies find no significant relationship between undergraduate selectivity and teachers' contributions to student achievement, conditional on a large set of time-varying teacher characteristics (Clotfelter, Ladd, and Vigdor, 2007; Chingos and Peterson, 2011).

To allow the effects of evaluation reforms on new teacher quality to be heterogeneous and non-monotonic, we apply a flexible, non-parametric approach (i.e., a "distribution regression") for examining effects on Barron's rank. Specifically, following Chernozhukov, Fernández-Val, and Melly (2013), we construct a set of Barron's ranking indicators where each indicator captures students who attended an undergraduate institution at a given level of competitiveness or higher. We then use those indicators to estimate the entire conditional

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[10] We merge on Barron's Rankings from earlier years (2004 and 2008) to improve our match rate from 90.3 to 90.8 percent. We find no evidence that the probability a new teacher attended an unranked undergraduate institution is affected by teacher evaluation reforms.

[11] We include institutions in the Special category as Non-Competitive as they are largely trade schools.

distribution of teacher quality.[12] We also present results based on a simple binary model where the outcome takes a value of one for teachers that attended undergraduate institutions ranked in the top four categories of *Competitive or higher*.

We construct a second measure of teacher quality based on teachers' certification type. Previous research has found that compared to traditionally certified teachers, teachers with emergency or temporary licenses are less effective at raising student achievement, improve less as they gain experience on the job, and are more likely to leave the profession (Boyd et al., 2006; Clotfelter et al., 2007; Clotfelter, Ladd, and Vigdor, 2010). We code this indicator as taking a value of one if a teacher reported holding a "certificate issued to persons who must complete a certification program in order to continue teaching (often called a waiver or emergency certificate)" or if they do not hold a teaching certificate of any type.

State Controls: In our preferred models, we include a parsimonious set of plausibly exogenous control variables to capture state-specific economic conditions and population characteristics. Controls for economic conditions include annual unemployment rates, real average hourly wages in the private sector, and real state tax revenue per capita. Controls for population characteristics include the percentage of 5-to-17-year olds that are Black, Hispanic, or White as well as living in families at or below the federal poverty line.[13]

Policy Controls: We also include a vector of indicators for the concurrent education policy reforms described above. These include indicators for having won a RTT grant;

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[12] In supplemental analyses, we find that a likelihood ratio test confirms that the proportional odds assumption from an ordered logistic regression is violated. Predicted probability estimates from a partial proportional odds model are quite similar to those reported below.
[13] We constructed data on average hourly wages in the private sector using annual data from the Current Population Survey. Data on state unemployment rates are from the BLS. Poverty and racial/ethnic data for the percentage of 5-to-17 year olds and state tax revenue data come from the U.S. Census Bureau. We link school data to our panel using the spring year of the academic year (e.g., 2015 for AY 2014-15). Hourly wages are deflated to real 2014 dollars using the consumer price index.

implementing Common Core state standards; requiring teacher candidates to take a basic skills licensure test, a content area licensure test, or a pedagogical content knowledge test; the overall passing rates for licensure tests in a state; the number of alternative certification programs; and the percent of teachers' salary withheld for pension funds.

School Controls: Across all analyses using data from the SASS/NTPS, we pair our set of time-varying state-level controls with controls for school characteristics to account for any random variation in the composition of sampled schools in each wave of the data. These controls include the percent of students that are Black or Hispanic, eligible for free- or reduced-price lunch (FRPL), and receive services as part of an Individualized Education Plan (IEP), as well as school size, urbanicity, average daily attendance, and type. In Table 1, we provide descriptive statistics for the primary variables used in our analysis to anchor the magnitude of our estimates.

V.   Empirical Framework

We estimate the effects of teacher evaluation reforms on the decision of individuals to enter the teacher labor market using a difference-in-differences framework. Our identification strategy compares changes within treated states over time to other non-treated states in the same geographic regions. Importantly, the differential timing of accountability reforms across treated states allows us to remove any regional trends in teacher labor supply and demand that might confound our estimates.

We begin with a non-parametric event-study specification. This approach allows us to model any anticipatory effects or time-varying treatment effects in a fully flexible way:[14]

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[14] For example, the full effect of evaluation reforms on teacher labor may not be immediate given prospective teachers make decisions about enrolling in educator preparation programs at least one year before entering the profession. Similarly, a number of states phased in system design features and requirements to use evaluation scores to inform high-stakes personnel decisions.

$$Y_{st} = \sum_{r=-7}^{3} 1(t = t_s^* + r)\beta_r + X_{st}\theta + \pi_s + \gamma_{g(s)t} + \varepsilon_{st}, \tag{1}$$

where Y_{st} is an outcome of interest for state s in year t, X_{st} is a vector of time-varying state covariates, π_s and $\gamma_{g(s)t}$ are state and region-by-year fixed effects, respectively, that account for fixed differences in teacher labor markets across states and regional labor market shocks across time, and ε_{st} is a random disturbance term.[15] The term $1(t = t_s^* + r)$ represents a set of indicators for the years pre- and post-policy reform, with t_s^* denoting the year in which state s implemented an evaluation reform and $r \in [-7, 3]$.

The coefficients of primary interest in (1) are the β_r's, which represent the effect evaluation reforms on our outcomes of interest r years before or after a reform. We measure these effects relative to the year just prior to a reform ($r = -1$), which is the omitted category. Furthermore, we censor r at -7 and 3 so that β_{-7} and β_3 represent the average effect of reforms on our outcomes of interest 7 or more years prior to a reform and 3 or more year after a reform, respectively.

We complement the event-study specification with a standard difference-in-differences (DD) model to increase our precision by pooling estimates across post-reform years:

$$Y_{st} = 1(t \geq t_s^*)\beta_1 + X_{st}\theta + \pi_s + \gamma_{g(s)t} + \mu_{st}, \tag{2}$$

where, $1(t \geq t_s^*)$ represents an indicator variable that equals unity in all years post-policy adoption, μ_{st} is a random disturbance term and all other variables are as defined in (1). The coefficient of primary interest in (2) is β_1, which is the difference-in-differences estimate of the effect of a given policy reform averaged across the post-period years in our panel.[16]

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[15] We specify γ_{gt} using the eight U.S. regions identified by the Bureau of Economic Analysis.
[16] Goodman-Bacon (2018) demonstrates that β_1 is a weighted average of treatment effects across all treated states that is biased in the presence of time-varying treatment effects. He notes that this does not invalidate the DD research design, but that it argues for the importance of presenting both standard DD estimates as well as estimates

Finally, we relax the assumption of time-invariant treatment effects imposed by the standard DD model in (2) to more formally test for any incremental effects and differential pre-trends in outcomes (Goodman-Bacon, 2018). Specifically, we follow LaFortune, Rothstein, and Schanzenbach, (2018) and add two linear time trends as follows:

$$Y_{st} = 1(t \geq t_s^*)\beta_1 + 1(t \geq t_s^*)(t - t_s^{*-1})\beta_2 + (t - t_s^{*-1})\beta_3 + X_{st}\theta + \pi_s + \gamma_{g(s)t} + \upsilon_{st}, \quad (3)$$

where, $(t - t_s^{*-1})$ is a linear trend centered at zero on the year before a state implemented an evaluation reform, υ_{st} is a random disturbance term and all other variables are as defined in (2).[17] The interaction term in (3), $1(t \geq t_s^*)(t - t_s^{*-1})$ allows for the relative time trends among treated states to differ pre- and post-reform. The coefficient on the main effect of treatment, β_1, captures the immediate response of the policy change on our outcome of interest, while the coefficient on the interaction term, β_2, captures any deviation from the linear trend in labor supply in the post-reform period among treated states. The coefficient associated with the relative year term, β_3, tests for any differential linear trends in the pre-reform period among states that adopted teacher accountability reforms relative to those that did not. We report standard errors clustered at the state level in all tables.

We account for differences in the size of state labor markets, and the fact that supply is relative to the size of the working-age pool of potential new entrants in the teaching profession, by scaling our measure of new teacher labor supply per 10,000 individuals aged 18-to-65 in a given state and year.[18] When the outcome of interest is this scaled measure of new teacher labor

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from models that allow for time-varying treatment effects such as our event study (equation 1) and linear DD model (equation 3).

[17] We center our relative time trend on the year before evaluation reforms were adopted so that in the first year of the reform (r=0) the linear time trends takes on a value of 1 making the treatment effect a linear combination of the intercept shift β_1 and the change in slope β_2. Centering on the year of evaluation reforms produces identical results but causes the joint effect of β_1 and β_2 to load entirely on β_1 because $t - t_s^*$=0 in the first year of the reform.

[18] Nationally representative data from the NTPS show the while the age of novice teachers is concentrated among individuals in their 20s and early 30s, novice teachers are spread across this full range of working-age individuals.

supply, we also weight our models by the number of individuals aged 18-to-65 per 10,000.[19] We apply the appropriate SASS/NTPS probability sampling weights for our analyses of school hiring and teacher quality outcomes. These approaches allow us to recover nationally representative estimates of the effect of teacher evaluation reforms, improve the precision of our estimates, and account for the endogenous sampling framework used in the SASS/NTPS (Solon, Haider, and Wooldridge, 2015).

Our overall DD analytic framework relies on two key assumptions: 1) that comparison states provide a valid counterfactual for the trends in treated states, and 2) that there are no unobserved factors that are correlated with both our outcomes of interest and the timing of teacher evaluation reforms across states. We test the first assumption by examining pre-trends using our non-parametric event study specifications and then formally testing for differential linear pre-trends in our DD model. To examine the validity of the second assumption, we run a series of auxiliary regressions where we regress our exogenous demographic and economic state controls as well as endogenous measures for new teacher labor demand on our treatment indicator with and without region-by-year and state fixed effects. It is reassuring that these auxiliary regressions present little evidence that accountability reforms affected the demand for new teachers or coincided with changing student demographics or broader negative economic shocks that decreased labor demand across entire state labor markets.

VI.   Results

A. *Effects on New Teacher Supply*

Nevertheless, in Appendix Table A1 we show that our results are robust when we instead scale by the number of 22-25 year olds per 1,000.

[19] Weighting serves to increase the precision of our estimates because the number of licensures granted varies considerably within states over time (ICC = 0.35).

We begin by presenting estimates from a simple event-study model of the effect of evaluation reforms on the number of new teaching licenses. As shown in Figure 4, the parameter estimates with associated 95% confidence intervals for the years pre-reform (hollow dot) and post-reform (solid dot) are strongly suggestive of a negative effect of evaluation reforms on the supply of new teachers.[20] Specifically, the number of teacher licenses granted declines steadily starting the year in which high-stakes evaluation reforms are implemented statewide. These individual point estimates become statistically significant in the second year and suggest that the effects of the reform increase over time. Importantly, there is little evidence that the supply of new teachers was trending downward prior to the implementation of evaluation reforms: the estimated coefficients on the pre-treatment indicators tend to be small in magnitude and statistically insignificant. The one exception is the positive coefficient on the pre-treatment indicator for two years prior to the implementation of reforms, which is the opposite sign as the post-treatment indicators.

Results from our standard DD model confirm the effect of high-stakes evaluation reforms. In Table 2, we estimate that high-stakes evaluation reforms reduced licenses granted in a state by 2.69 per 10,000 18-to-65-year olds, on average, in our baseline specification without controls. This represents an 17% reduction in the average number of licenses granted in the post-policy reform years among treated states, relative to the pre-reform state mean. As shown in column 2, this estimate is quite robust to the inclusion of controls for state-specific economic conditions, student population characteristics, and other education policy reforms. Adding these controls slightly increases our estimate to an 18% reduction in new teacher supply.

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[20] Point estimates and standard errors from the event study specification are reported in column 1 of Appendix Table A2.

Estimates based on our parametric DD specification that includes linear trends also confirm the patterns suggested by our event-study analyses. Focusing on results from our model that includes controls in column 4 of Table 2, our estimates reveal a significant downward linear trend in supply among treated states in the post-policy reform years of -1.47 licenses per 10,000 18-to-65-year olds. Consistent with the visual evidence in the event study, we also fail to reject the null hypothesis of no differential pre-reform trends for states that adopted evaluation reforms: the coefficient on the pre-period trend variable (*Trend*) is both small in magnitude (0.19) and statistically insignificant.

In columns 5 and 6, we add contemporaneous accountability reform measures to the model. In our model with controls, the estimates for these complementary reforms are all negatively signed as we hypothesized, but the limited variation in these measures produces large corresponding confidence intervals. When we model these related reforms using our single continuous count measure we find strong evidence that the aggregate intensity of accountability reforms affected the overall magnitude of decline in new teacher labor supply. As shown in columns 7 and 8, our linear term capturing the number of accountability reforms adopted is negatively signed and significant suggesting an approximate 10% decline for each additional accountability reforms passed in a state.

B. Effects on Teacher Hiring

We next examine whether, in addition to reducing new teacher labor supply, evaluation reforms also made the process of filling teaching vacancies more challenging for schools. We present results from a simple event-study model in Figure 5 (see Appendix Table A2 column 3). The pattern of results clearly suggest that evaluation reforms increased the difficulty of filling vacant positions with a growing effect over time. We find an isolated negative point estimate in

the pre-period at $t=-3$, possibly the result of the unbalanced sample across point estimates given the four-year gaps between each SASS/NPTS.

In column 2 of Table 3, we estimate that evaluation reforms increased the probability a school had at least one unfilled vacancy by 2.6 percentage points in our model that includes both state, policy and school controls. Our linear DD estimates in column 3 and 4 suggest these effects were likely both immediate and increasing over time. While there is some evidence of a very small positive and statistically significant pre-trend in the probability of unfilled vacancies (0.002 SD), it is reassuring that even when allowing for this pre-trend we continue to find effects of similar magnitude in our linear DD model as in our standard DD model.

As shown in columns 5 and 6, adding our full set of concurrent accountability measures again has little effect on our estimate of the effect of evaluation reforms. Here the accountability measures are all positively signed, as we would expect, and relatively small in magnitude with the exception of weakening collectively bargaining which we estimate decreased the likelihood of a vacant position by 1.4 percentage points. We caution against placing too much weight on this estimate given the difficulty of interpreting it conditional on all the other related accountability reforms. Instead, we prefer evidence from the linear accountability intensity measure which suggests that the probability of a school having at least one unfilled vacancy went up by approximately 1 percentage point for each accountability reform a state adopted (columns 7 and 8).

In Table 4, we test for heterogeneous effects of evaluation reforms by several proxy measures for hard-to-staff schools. Specifically, we present results from a set of standard DD models where we interact the evaluation reform indicator (Implement Evaluation) with: 1) an indicator for whether the school is located in an urban area; 2) the percent of FRPL students; 3)

the percent of students who are Black and Hispanic; and 4) the percent of students with an IEP plan all measured at the school level. We find consistent and statistically significant evidence across all four measures that evaluation reforms had a differentially large impact on the ability of hard-to-staff schools to fill vacant teaching positions. We estimate, for instance, that non-urban schools experienced a 2.0 percentage point increase in the probability of having at least one unfilled teaching position, while the effect of evaluation reforms on urban schools was more than double this at 4.4 percentage points. As shown in Figure 6, similar patterns exist for schools with higher percentages of students that are eligible for FRPL, that are Black and Hispanic, and that have IEPs, all of which are scaled so that a one-unit change is a ten percentage point change.

C. Effects on New Teacher Quality

How did teacher evaluation reforms affect the quality of newly hired novice teachers in public schools? As discussed in the conceptual framework and detailed in Appendix B, evaluation reforms may reduce both the number of low- and high-ability individuals that choose to teach, making the effect of evaluation reforms on teacher quality ambiguous. Given this potential heterogeneous and non-monotonic effect, we begin by estimating the effect of evaluation reforms across the range of teacher quality. In Figure 7 and Appendix Table A3, we present results from our standard DD model with state, policy, and school controls where our outcomes are a set of indicators capturing whether a teacher attended an undergraduate institution of a given rank or higher. Although most of the individual point estimates lack precision, the pattern of results across the rankings is strongly suggestive of a rightward shift in the quality of the marginal low-quality teacher, with little change in the quality of the marginal high-quality teacher. These findings are consistent with a simple Roy model of occupational choice where costs vary by ability. They suggest that evaluation reforms shifted the supply of

new teachers upward along the quality distribution by primarily reducing the probability that teachers graduated from non-competitive institutions and increasing the probability they graduated from competitive institutions.

In our preferred model in Table 5 Panel A, we find that evaluation reforms increased the probability a teacher graduated from an undergraduate institution ranked competitive or higher by 8.1 percentage points. Estimates from linear DD models suggest these impacts were largely immediate and continued to increase in the post-reform years. In Panel B of Table 5 we present estimates of the effect of evaluation reforms on the probability that a newly hired novice teacher held an emergency certification. Here we find small and statistically insignificant estimates of the effect of evaluation reforms across both standard and linear DD models suggesting that the reforms had little effect on the number of new teachers who lacked more formal state licenses.[21]

D. Mechanisms

In our conceptual framework, we posit that increases in the relative costs of teaching due to the implementation of evaluation reforms are a likely mechanism behind our core findings regarding the supply and quality of new teachers. To examine this possibility, we leverage a rich set of self-reported survey questions across waves of the SASS/NTPS to explore the ways in which evaluation reforms might have increased the relative cost of entering the teaching profession. These measures include new teachers' responses on a 4-point Likert scale from *Strongly Disagree* to *Strongly Agree* to statements about job security, job satisfaction, and cooperative effort among teachers. We complement these three measures with new teachers'

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[21] We further test for heterogeneous effects of evaluation reforms on new teacher quality by school characteristics and find no evidence that effects differed across urban and non-urban schools or by the percentage of students in a school that are eligible for FRPL, that are Black and Hispanic, and that have IEPs. Results are available upon request.

responses on a 4-point Likert scale from *No Control* to *A Great Deal of Control* about how much control they have in their classrooms over: selecting textbook and other instructional materials; selecting content, topics, and skills to be taught; and selecting teaching techniques.

We find consistent evidence that evaluation reforms increased the perceived costs of teaching among new entrants into the profession. Figure 8 displays predicted average marginal effects from ordered logistic regressions based on our standard DD specification with controls (see Appendix Table A3 for results from underlying ordered logistic regression models). We find that evaluation reforms increased the number of teachers who *Agree* and *Strongly Agree* that they worry about job security by 8.8 and 7.7 percentage points, respectively. We depict an event study version of these results in Figure 9 using a binary outcome of whether teachers *Agree* or *Strongly Agree* that they worry about job security. Here we see no pretend but evidence that novice teachers began to worry about job security a year before evaluation reforms were fully implemented. This is suggestive of an anticipatory effect on how much teachers worried about their job security as districts were preparing to fully implement new evaluation systems and teachers were aware of these impending changes.

Evaluation reforms also appear to have reduced teacher satisfaction and autonomy. We find that evaluation resulted in a 14.6 percentage point drop in the likelihood teachers *Strongly Agree* that they are satisfied with being a teacher. Similarly, we find a 12.7 percentage point drop in the proportion of teachers who *Strongly Agree* that there exists a great deal of cooperative effort among teachers. We find a 5.7 percentage point decrease in the probability that new teachers *Strongly Agree* that they have control over the content and skills they teach and a 8.9 percentage point drop in the probability that new teachers *Strongly Agree* that they have control over their teaching techniques. We find no effects on teachers' perceptions about their control

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over selecting instructional materials. Together, these analyses suggest that evaluation reforms substantially decreased new teachers' perceived job security, job satisfaction, cooperative effort, and control over content and teaching methods.

VII.   Robustness Tests

A. Alternative Modeling and Weighting Approaches

We begin by testing the sensitivity of our main results on teacher supply to a range of modeling and weighting choices. In Appendix Table A5, we show that our results are remarkably robust to alternative modeling approaches including defining treatment as the year teacher evaluation reform laws were passed instead of implemented, replacing region-year fixed effects with year fixed effects, restricting the sample to the 29 states that implemented evaluation reforms but no other accountability reforms, including endogenous controls for teacher demand, and allowing for pre- and post-trends for accountability and other education policy reforms. Our main treatment estimate remains statistically and economically significant across all models. As expected, defining treatment as the year evaluation reforms were passed results in somewhat attenuated effects of a 12% decline in new teacher labor supply given the additional years included as treated before evaluation was actually implemented. Allowing for pre- and post-trends for other accountability reforms also attenuates the effect of evaluation (11%). In Table A6, we confirm that our results are robust to applying different scaling and weighting factors, dropping weights, and logging rather than scaling new teacher labor supply. All other modeling and weighting approaches produce estimated effects of a 15% decline in new teacher labor supply or larger. Finally, in Figure A1 we show that our event study results are consistent when we extend the range of bins to $r \in [-10,4]$.

B. Covariate Balance Tests

Another potential concern is that evaluation reforms coincided with changing demographics of the student-age population or negative economic shocks that decreased labor demand across entire state labor markets. We further explore the degree to which our core DD model sufficiently accounts for any correlation between these time-varying measures and states' implementation of evaluation reforms. As shown in Table 6, in specifications where we exclude our identifying controls, namely state and region-by-year fixed effects, we do see a relationship between the timing of evaluation implementation and several of our covariates (unemployment, private hourly wages, percent of the student-age population that is African American and from low-income households) in simple bivariate models. However, simply adding region-by-year and state fixed effects substantially reduces these relationships such that none of them represent more than a 3% change relative to the pre-reform state mean and only one is still marginally significant (percent African-American). One exception is that with the addition of basic DD fixed effects, evaluation implementation predicts a drop in state tax revenue per capita of approximately 6%. Nevertheless, it is reassuring that our results are quite robust to including these controls in our models.

C. Falsification Tests and Shocks to Teacher Demand

We next test for an effect of evaluation reforms on a range of additional measures that are plausibly related to teacher labor markets. Given the relationship between our treatment indicator and one of our three economic controls (state tax revenue per capita), we conduct a falsification test to explore whether evaluation appears to affect prospective entrants into a private sector industry that also requires a bachelors' degree and state certification: accounting. Specifically, we estimate models where the outcome is a measure of the supply of accountants, first time

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Certified Public Accountant (CPA) exam takers in a given state and year.[22] As shown in Panel B columns 7 and 8 of Table 6, we find no evidence of a broader effect on labor supply outside of the teacher sector, thus reducing concerns about evaluation reform effects being related to broader trends in the labor market.

Contemporaneous shocks to teacher demand could also cause prospective teachers to update their expectations about job prospects and lifetime earnings in the teaching sector, causing some to choose not to teach. Furthermore, as noted by Nagler et al. (forthcoming), changes in the demand for teachers that coincide with accountability reforms could also affect the quality of new teachers under two conditions: 1) school administrators can effectively screen potential applicants and only hire the highest quality ones and 2) the number of vacant positions following the adoption of accountability reforms is smaller than before the reforms. If both of these conditions were to hold the quality of new teachers would increase, providing an alternative explanation for the increase in teacher quality that we observe.

Testing for exogenous shocks to teacher labor demand is challenging for two reasons: 1) there is no measure of demand that is independent of supply, such as the number of open positions posted, available at the national level, and 2) it is possible that evaluation reforms may have affected demand through effects on teacher turnover and retirement rates. Our approach is to test for evidence of large demand shocks caused by, or concurrent with, teacher evaluation reforms that might account for the effects we find on teacher supply and quality. We begin with an intuitive but endogenous measure of demand – the number of new public school teachers

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[22] These state-by-year cohorts counts are collected by the National Association of State Boards of Accountancy (NASBA) in partnership with The American Institute of Certified Public Accountants (AICPA). All candidates who begin the CPA examination process (begin any one of the four sub-exams) in the same calendar year are members of a state-year cohort. Requirements for CPA candidates are similar to those for teaching candidates – while all CPA candidates are required to have *at least* a bachelor's degree and complete a set number of accounting courses, the CPA examination/licensure requirements vary from state-to-state.

hired. We then test for effects on proxies of teacher demand that suffer from endogeneity to a lesser degree: public and private school enrollment, and pupil-teacher ratios.[23] As shown in Panel C of Table 6, these falsification tests reveal no evidence to suggest that changes in teacher demand are driving the effects we see on new teacher labor supply.

VIII.   Extensions

A. Effects on Teacher Preparation Program Completers

We further explore the effect of accountability reforms on the number of graduates from university-based teacher preparation programs (TPP) using data from the Integrated Postsecondary Education Data System (IPEDS).[24] We consider these results as exploratory given several limitations of the IPEDS data for capturing state-specific new teacher labor supply. First, IPEDS does not include graduates of alternative non-degree granting TPPs that are not affiliated with a college or university such as Teach for America and TNTP Teaching Fellows programs in some states. Second, program completers may intend to teach in private schools or work in a non-teaching position in education. Third, program completers may intend to teach in a state other than the one in which they completed their degree.

A fourth challenge is the likely delayed effects of evaluation reforms on the number of students who enroll in and complete TPP programs. Students apply to one-year graduate TPPs roughly two years prior to entering the teacher labor market and up to four years prior for bachelor's programs. Figure 10 displays national trends in number of TPP degree completers disaggregated by graduate and bachelor's programs. The figure is suggestive of a more

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[23] These measures still suffer from potential endogeneity if parents' decisions to enroll their students in public schools, or districts' class-size policies, were influenced by evaluation reforms.

[24] See Appendix C for a detailed description of the classification of instructional programming (CIP) codes we used to identify graduates of teacher preparation programs.

immediate and steep decline of graduate TPP degree completers relative to bachelor's degree completers over the period when evaluation reforms were implemented across states. Relative to 2011, the number of graduate TPP degree completers declined nationally by 26% compare to 16% for bachelor's TPP degree completers. We focus our primary analyses on degree completers from graduate programs because we expect enrollment and persistence in these predominantly one-year programs to be more immediately responsive to evaluation reforms and to be a more direct measure of supply. Slightly more than half of all TPP completers are from graduate degree programs.

Our results from a simple event study shown in Figure 11 (and Appendix Table A2) closely mirror the pattern of effects we find on licensure and vacancies. We see a relatively flat pretend and a steady decline in completers from graduate TPPs post reform although the individual point estimates lack precision. In our standard DD model with covariates, we estimate that evaluation reforms reduced the number of completers by 10%. These results are largely unchanged when we include other contemporaneous accountability reform measures. Our estimates of the effect of accountability reform intensity is negative and marginally significant, suggesting that for every additional accountability reform a state adopted, the number of graduates from Masters' granting TPPs declined by 5%.[25]

B. Effects on Teacher Compensation

The sizable negative consequences of evaluation reforms on the supply of new teachers are consistent with the evidence of increased occupational costs for new public-school teachers. They also suggest that districts did little to offset these increased costs with higher wages. We explore how wages changed for teachers affected by evaluation reforms by estimating the effect

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[25] In results not shown, we find little evidence that teacher accountability reforms affected the number of TPP graduates from undergraduate programs.

of evaluation reforms on two different measures of teacher wages. The first is real average public school teacher wages calculated using district reported total FTE instructional staff salaries collected by NCES. The second is average total real wages for public school teachers estimated using the 2005-2016 Public Use Microdata Sample (PUMS) of the American Community Survey (ACS), adjusted for age and educational attainment.[26] This adjustment serves to account for differences in average wages caused by changes in the distribution of experience and educational attainment among public school teachers rather than changes in the underlying salary schedules.

We find no evidence of any compensating differentials that might explain or offset the increased occupational costs in the teaching profession caused by teacher evaluation reforms. As shown in Appendix Table A7, estimates from our standard DD model with controls, weighted by the total number of FTE public school teachers in a state, are both small in magnitude (less than $510 or 1% of average pre-reform wages), negatively signed, and only marginally significant for adjusted wages.

C. The Effects of Other Education Reform Policy Reforms

In Table 8, we present results from standard DD models with controls where we estimate the effect of accountability reforms as well as these other education policy reforms both separately and simultaneously. When entered alone, our point estimate of the effect of eliminating tenure suggests an equally large negative impact on new teacher supply as the effect of evaluation reforms, but the estimate is very noisy. In joint models, we find direct and intuitive evidence that the number of new teacher licensures increases as the passing rates for licensure tests increase and the number of alternative certification programs increase. These results suggest

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[26] We do this by predicting state-by-year mean wages conditional on a quadratic function of age and fixed effects for highest degree earned.

states directly influence new teacher supply based on the minimum score they set for passing licensure tests and the number of alternative certification programs they approve.

IX. Conclusion

Education policy over the past decade has focused, in large part, on improving human capital in schools through teacher labor market reforms. Many of these policies have identified teacher accountability as a promising mechanism for raising teacher quality. These reform efforts, and the research base upon which they draw, have made strong assumptions about the potential consequences of accountability reforms on the supply and quality of new teachers. Counter to most assumptions, our findings document how a package of teacher accountability reforms, centered on high-stakes evaluation systems, reduced the supply of new teacher candidates available to public schools. We find further evidence that suggests this decline in new teacher labor supply was caused by a perhaps predictable consequence of the reforms, a decrease in perceived job security and autonomy.

Reductions in the supply of new teachers had direct consequences for schools and students. We show that teacher evaluation reforms increased the likelihood that schools had vacant teaching positions well after the start of the school year. This is particularly concerning given that unfilled vacancies were concentrated in hard-to-staff schools that often serve larger populations of low-income students and students of color. Prior research has found that teachers hired after the start of the school year are substantially less effective at supporting student achievement growth than their counterparts who were hired on time, even in the same school, grade, and year (Papay and Kraft, 2016). Evaluation reforms have allowed district administrators

to better understand how teacher quality is distributed across schools, but may have also exacerbated the challenge of providing all students with equal access to high-quality teachers.

At the same time, we find that evaluation reforms increased the quality of newly hired novice teachers as judged by the selectivity of their undergraduate institutions. These effects are driven by a reduction in the number of teachers that graduated from the least selective institutions. However, we find no evidence that evaluation reforms served to attract teachers who attended the most selective undergraduate institutions.

Together, these results also provide new evidence of the competitiveness of the teacher labor market. The loss of non-pecuniary benefits due to accountability reforms was not offset by any compensatory changes in teacher salaries at a national scale. Very few districts implemented merit pay programs of any significance despite efforts by the federal government and private philanthropists to more directly link teacher evaluation and compensation (Buck and Green, 2011; Chiang et al., 2017). Consistent with Rothstein's (2015) model-based predictions, the lack of meaningful offsetting increases in teacher base salaries or merit-based compensation caused a substantial fraction of would-be teachers to choose other professions or remain outside the labor market.

Enrollment in K-12 public schools in the United States is projected to increase by over a million students in the next decade, an increase of 2% from current levels. Even with new personalized learning technologies, it is hard to imagine a future where demand for classroom teachers is not increasing given expanding enrollments and a labor force where 29% of teachers are over the age of 50 (NCES, 2017). Understanding the consequences of education policy reforms on the supply and quality of new teachers will remain a key element of efforts to improve human capital in the education sector. This is particularly important for subject areas

with teacher shortages, as well as for the pipeline of new teachers needed to work in hard-to-staff

schools in urban, rural, and low-income settings.

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Tables

Table 1: Descriptive Statistics

	Obs	Mean	Std. Dev.	Date Range	Data Source
State Measures					
Teacher Licensures^	750	14.97	5.80	2002-2016	Title II
Program Completers from Graduate TPPs	750	6.31	3.59	2002-2016	IPEDS
Unemployment Rate	750	5.99	1.99	2002-2016	BLS
Average Hourly Wages in the Private Sector	750	27.57	3.50	2002-2016	CPS
Tax Revenue per Capita	750	2.82	1.01	2002-2016	Census
% of 5-17 year olds that are African-American	750	13.07	11.15	2002-2016	Census
% of 5-17 year olds that are Hispanic	750	14.02	12.67	2002-2016	Census
% of 5-17 year olds that are White	750	65.97	17.49	2002-2016	Census
% of 5-17 year olds Below Poverty Line	750	16.89	4.98	2002-2016	Census
Full-time Novice New Teacher Hires^	200	6.94	3.05	2003, '07, '11, '15	SASS/NTPS
Log Public School Total Enrollment	750	13.30	1.02	2002-2016	NCES
Log Private School Total Enrollment	400	10.87	1.20	2002-2016, biannually	NCES
Pupil Teacher Ratio	750	15.38	2.64	2002-2016	NCES
Certified Public Account Exam Takers^	550	2.61	3.41	2006-2016	AICPA
Average Public Teacher Salary (NCES)	750	54,999	8,054	2002-2016	NCES
Adjusted Average Public Teacher Salary (ACS)	600	46,625	6,577	2005-2016	ACS
School Measures					
At least one unfilled vacancy in a school	28,610	0.04	0.20	2003, '07, '11, '15	SASS/NTPS
Teacher Measures					
Worry About Job Security	6,460	2.30	0.99	2003, '07, '11, '15	SASS/NTPS
Job Satisfaction	6,460	3.45	0.75	2003, '07, '11, '15	SASS/NTPS
Teacher Cooperation	6,460	3.26	0.78	2003, '07, '11, '15	SASS/NTPS
Control over Selecting Instructional Materials	6,460	2.44	1.05	2003, '07, '11, '15	SASS/NTPS
Control over Content, Topics, Skills Taught	6,460	2.75	1.03	2003, '07, '11, '15	SASS/NTPS
Control over Teaching Techniques	6,460	3.58	0.64	2003, '07, '11, '15	SASS/NTPS
Competitive Undergraduate Institution or Higher	5,800	0.85	0.36	2003, '07, '11, '15	SASS/NTPS
Emergency or Temporary Certification	5,800	0.16	0.36	2003, '07, '11, '15	SASS/NTPS

Notes: Teacher characteristics are weighted using appropriate probability weights from the SASS dataset. Per pupil expenditures, average hour wages in the private sector and state tax revenue per capita are reported in 2014 real dollars. Missing values are imputed for Full-time Novice New Teacher Hires using linear interpolation. BLS = Bureau of Labor Statistics, CPS = Current Population Survey, NCES = National Center for Education Statistics, SASS = Schools and Staffing Survey, NTPS = National Teacher and Principal Survey, AICPA - American Institute of Certified Public Accountants, ACS = American Community Survey.

Table 2: The Effect of Teacher Accountability Reforms on the Number of New Teaching Licenses

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Implement Evaluation	-2.686**	-2.996***	-1.061	-1.256	-2.369**	-2.618***		
	(0.836)	(0.722)	(0.982)	(0.907)	(0.831)	(0.719)		
Implement Evaluation * Trend			-1.308*	-1.468**				
			(0.527)	(0.456)				
Trend			0.231	0.188				
			(0.212)	(0.177)				
Eliminate Tenure					-1.483	-0.453		
					(1.798)	(1.713)		
Increase Probationary Period					-0.986	-2.003		
					(1.247)	(1.348)		
Weaken Collective Bargaining					0.735	-0.251		
					(1.730)	(1.482)		
Eliminate Mandatory Union Dues					-1.584	-0.280		
					(2.263)	(3.318)		
Total Accountability Reforms							-1.242**	-1.560***
							(0.454)	(0.434)
% change relative to state mean	-17%	-18%			-15%	-16%	-8%	-10%
State Controls	No	Yes	No	Yes	No	Yes	No	Yes
Policy Controls		Yes		Yes		Yes		Yes
n	750	750	750	750	750	750	750	750

Notes: +p<.10, *p<.05, **p<.01, ***P<.001. Cells report estimates and associated standard errors clustered at the state level in parentheses. Time-varying state controls include real average hourly wages in the private sector, unemployment rates, real state tax revenue per capita, the fraction of the population ages 5 to 17 that are White, Black, Hispanic, and living below the federal poverty line. Policy controls include indicators for having won a Race to the Top grant; implementing Common Core state standards; requiring teacher candidates to take a basic skills licensure test, a content area licensure test, and a pedagogical content knowledge test; the number of alternative certification programs; and the percent of teachers' salary withheld for pension funds. All models include state and region-by-year fixed effects. All models are weighted by the number of 18-65 year olds per 10,000 in a state.

Table 3: The Effect of Teacher Accountability Reforms on the Probability Schools Have at least One Unfilled Teaching Position

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Implement Evaluation	0.032**	0.026***	0.013	0.010	0.029*	0.026**		
	(0.009)	(0.007)	(0.009)	(0.008)	(0.011)	(0.008)		
Implement Evaluation * Trend			0.008**	0.006*				
			(0.003)	(0.003)				
Trend			0.002	0.002*				
			(0.001)	(0.001)				
Eliminate Tenure					0.009	0.013		
					(0.008)	(0.013)		
Increase Probationary Period					0.007	0.007		
					(0.009)	(0.006)		
Weaken Collective Bargaining					-0.006	-0.014*		
					(0.006)	(0.006)		
Eliminate Mandatory Union Dues					0.023	0.002		
					(0.018)	(0.017)		
Total Accountability Reforms							0.014***	0.010***
							(0.003)	(0.003)
School Controls	No	Yes	No	Yes	No	Yes	No	Yes
State Controls	No	Yes	No	Yes	No	Yes	No	Yes
Policy Controls	No	Yes	No	Yes	No	Yes	No	Yes
n	750	750	750	750	750	750	750	750

Notes: +p<.10, *p<.05, **p<.01, ***P<.001. Cells report estimates and associated standard errors clustered at the state level in parentheses. Time-varying state controls and policy controls are the same as those listed in Table 2. School controls include the percent of students that are Black and Hispanic, eligible for free- or reduced-price lunch, and receive services as part of an Individualized Education Plan (IEP) as well as school size, urbanity, and grade level. All models include state and region-by-year fixed effects and use appropriate sampling weights provided by the SASS/NTPS.

Table 4: Differential Effects of Teacher Evaluation Reforms on the Probability Schools Have at least One Unfilled Teaching Position

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Implement Evaluation (Eval)	0.026**	0.020*	0.018*	0.010	0.019+	0.010	0.022*	0.016*
	(0.008)	(0.008)	(0.009)	(0.008)	(0.009)	(0.007)	(0.010)	(0.007)
Urban	0.019**	0.005						
	(0.006)	(0.006)						
Eval * Urban	0.023+	0.024+						
	(0.013)	(0.013)						
Percent FRPL			0.003***	0.001+				
			(0.001)	(0.001)				
Eval * Percent FRPL			0.002+	0.003*				
			(0.001)	(0.001)				
Percent Black and Hispanic					0.005***	0.003***		
					(0.001)	(0.001)		
Eval * Percent Black and Hispanic					0.004*	0.005**		
					(0.002)	(0.002)		
Percent IEP							0.001	-0.002
							(0.001)	(0.001)
Eval * Percent IEP							0.007**	0.008**
							(0.003)	(0.003)
School Controls	No	Yes	No	Yes	No	Yes	No	Yes
State Controls	No	Yes	No	Yes	No	Yes	No	Yes
Policy Controls	No	Yes	No	Yes	No	Yes	No	Yes
n	28,610	28,610	28,610	28,610	28,610	28,610	28,610	28,610

Notes: +p<.10, *p<.05, **p<.01, ***P<.001. Cells report estimates and associated standard errors clustered at the state level in parentheses. Percent FRPL, Black and Hispanic, and IEP measure are scaled so that a one-unit change is equivalent to a ten percentage point change. See Table 2 notes for a complete list of time-varying state and policy controls and Table 3 notes for a complete list of school controls. All models include state and region-by-year fixed effects and use appropriate sampling weights provided by the SASS/NTPS. FRPL = Free or Reduced Price Lunch, IEP = Individualized Education Plan.

Table 5: The Effect of Teacher Evaluation Reforms on New Teacher Qualifications

	(1)	(2)	(3)	(4)
	Panel A. Competitive Undergraduate Institution or Higher			
Implement Evaluation	0.046	0.081**	0.010	0.073*
	(0.029)	(0.028)	(0.025)	(0.031)
Implement Evaluation * Trend			0.022+	0.017
			(0.011)	(0.012)
Trend			-0.002	-0.008*
			(0.003)	(0.004)
n	5,800	5,800	5,800	5,800
	Panel B. Emergency or No Certification			
Implement Evaluation	-0.008	0.006	-0.009	-0.026
	(0.030)	(0.033)	(0.029)	(0.037)
Implement Evaluation * Trend			-0.000	0.017
			(0.009)	(0.011)
Trend			0.000	0.002
			(0.003)	(0.005)
n	6,460	6,460	6,460	6,460
School Controls	No	Yes	No	Yes
State Controls	No	Yes	No	Yes
Policy Controls	No	Yes	No	Yes

Notes: +p<.10, *p<.05, **p<.01, ***P<.001. Cells report estimates and associated standard errors clustered at the state level in parentheses. See Table 2 notes for a complete list of time-varying state and policy controls and Table 3 notes for a complete list of school controls. All models include state and region-by-year fixed effects and use appropriate sampling weights provided by the SASS/NTPS.

Table 6: Auxiliary Regressions Examining the Effect of Teacher Evaluation Reforms on Teacher Labor Demand and Economic Conditions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Panel A: Student Demographics							
	Percent of 5 to 17 year-olds that are African American		Percent of 5 to 17 year-olds that are Hispanic		Percent of 5 to 17 year-olds that are white		Percent of 5 to 17 year-olds that are from Low-Income Households	
Implement Evaluation	2.841*	0.375+	-3.957	0.085	1.476	-0.331	2.030**	0.408
	(1.397)	(0.197)	(4.808)	(0.278)	(4.539)	(0.332)	(0.680)	(0.274)
n	750	750	750	750	750	750	750	750
% change relative to state mean	22%	3%	-31%	1%	2%	0%	13%	3%
	Panel B: Economic Conditions							
	Unemployment Rate		Private Hourly Wages		State Tax Revenue Per Capita (real dollars)		Certified Public Accountant Exam Takers (per 10,000 18-65 year olds)	
Implement Evaluation	-0.498+	0.066	2.832***	0.050	-0.035	-0.168**	-0.176	0.011
	(0.269)	(0.172)	(0.650)	(0.188)	(0.178)	(0.061)	(0.192)	(0.108)
n	750	750	750	750	750	750	550	550
% change relative to state mean	-9%	1%	11%	0%	-1%	-6%	-7%	0%
	Panel C: Teacher Labor Demand							
	Novice Public School Teachers (per 10,000 18-65 year olds)		Public School Enrollment (in logs)		Private School Enrollment (in logs)		Public School Pupil Teacher Ratio	
Implement Evaluation	-0.806	0.862	-0.191	-0.004	-0.308	0.003	-0.907	-0.243
	(0.720)	(0.579)	(0.243)	(0.009)	(0.236)	(0.020)	(0.915)	(0.260)
n	200	200	750	750	400	400	750	750
% change relative to state mean	-10%	11%	-1%	0%	-3%	0%	-6%	-2%
State & Region-by-Year FE	No	Yes	No	Yes	No	Yes	No	Yes

Notes: +p<.10, *p<.05, **p<.01, ***P<.001. State and policy controls are omitted from all models Cells report estimates and associated standard errors clustered at the state level in parentheses. All models are weighted by the number of 18-65 year olds per 10,000 in a state.

Table 7: The Effect of Teacher Accountability Reforms on the Number of Graduates from M.A. Granting Teacher Preparation Programs

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Implement Evaluation	-0.555	-0.674*	-0.337	-0.324	-0.423	-0.538+		
	(0.472)	(0.322)	(0.234)	(0.215)	(0.489)	(0.316)		
Implement Evaluation * Trend			-0.263	-0.287				
			(0.204)	(0.183)				
Trend			0.114	0.031				
			(0.106)	(0.083)				
Eliminate Tenure					-0.451	-1.009		
					(0.535)	(0.707)		
Increase Probationary Period					0.151	0.353		
					(0.400)	(0.380)		
Eliminate Mandatory Union Dues					-0.973	-0.745		
					(0.593)	(0.550)		
Weaken Collective Bargaining					0.610	0.282		
					(0.672)	(0.501)		
Total Accountability Reforms							-0.270	-0.303+
							(0.184)	(0.167)
% change relative to state mean	-9%	-10%			-7%	-4%	-4%	-5%
State Controls	No	Yes	No	Yes	No	Yes	No	Yes
Policy Controls		Yes		Yes		Yes		Yes
n	750	750	750	750	750	750	750	750

Notes: +p<.10, *p<.05, **p<.01, ***P<.001. Cells report estimates and associated standard errors clustered at the state level in parentheses. Time-varying state controls include real average hourly wages in the private sector, unemployment rates, real state tax revenue per capita, the fraction of the population ages 5 to 17 that are White, Black, Hispanic, and living below the federal poverty line. Policy controls include indicators for having won a Race to the Top grant; implementing Common Core state standards; requiring teacher candidates to take a basic skills licensure test, a content area licensure test, and a pedagogical content knowledge test; the number of alternative certification programs; and the percent of teachers' salary withheld for pension funds. All models include state and region-by-year fixed effects. All models are weighted by the number of 18-65 year olds per 10,000 in a state.

Table 8: The Effect of Education Policy Reforms on the Number of New Teaching Licenses

	Separate Models	Joint Model
	(1)	(2)
Implement Evaluation	-2.400**	-2.618***
	(0.845)	(0.719)
Eliminate Tenure	-2.409	-0.453
	(1.800)	(1.713)
Increase Probationary Period	-1.452	-2.003
	(1.294)	(1.348)
Weaken Collective Bargaining	-0.560	-0.251
	(1.619)	(1.482)
Eliminate Mandatory Union Dues	-1.453	-0.280
	(2.452)	(3.318)
Won Race to the Top Grant	0.075	0.335
	(1.268)	(1.173)
Number of Alt. Cert. Program Types	0.673	0.837*
	(0.468)	(0.368)
Implement Common Core Standards	-0.522	-0.168
	(1.585)	(1.205)
Basic Skills Licensure Tests	0.647	1.398
	(1.115)	(1.034)
Content Area Licensure Tests	-0.336	0.052
	(1.280)	(1.116)
Pedagogical Knowledge Licensure Tests	-1.048	-1.695
	(1.609)	(1.297)
Licensure Test Pass Rate	0.312**	0.381***
	(0.101)	(0.096)
Percent of Salary Withheld for Pension Fund	0.068	-0.142
	(0.428)	(0.413)
% change relative to state mean (Eval)	-15%	-16%
School Controls	No	No
State Controls	Yes	Yes
n	750	750

Notes: +p<.10, *p<.05, **p<.01, ***P<.001. Cells report estimates and associated standard errors clustered at the state level in parentheses. See Table 2 notes for a complete list of time-varying state-level controls and Table 3 notes for a complete list of school-level controls. All models include state and region-by-year fixed effects. All models are weighted by the number 18-65 year old per 10,000 in a state.
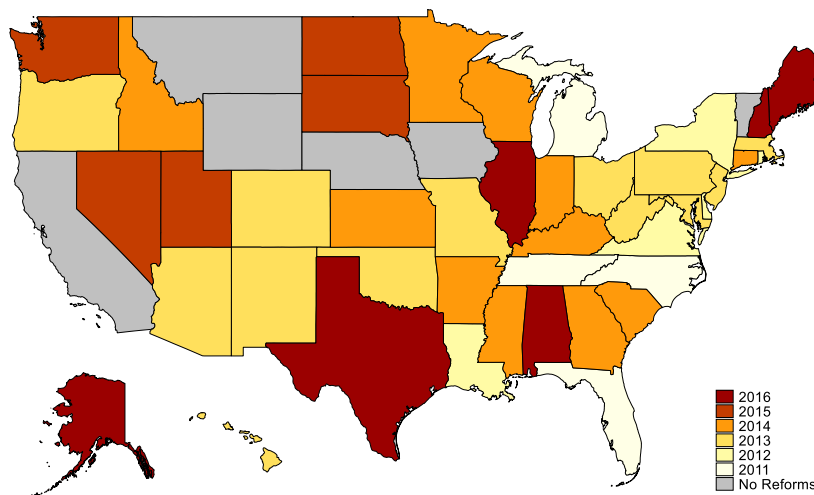
Figure 1: The Timing of Statewide Implementation of Teacher Evaluation Reforms

Notes: Years represent the fall of the academic year in which the new systems were first fully implemented statewide.
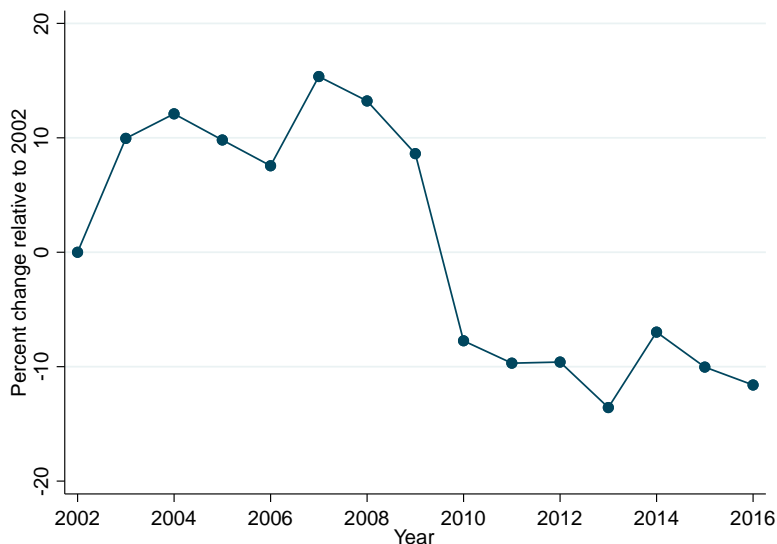

Figure 2: National Trends in the Number of Teaching Licenses Issued by States, 2002-2016

Notes: The baseline 2002 value for licenses is 277,696 for licenses. Data on the number of teaching licenses issued come from Title II data provided by states to the U.S. DOE. A reporting year for Title II is from September 1st to August 31st for the latter year.
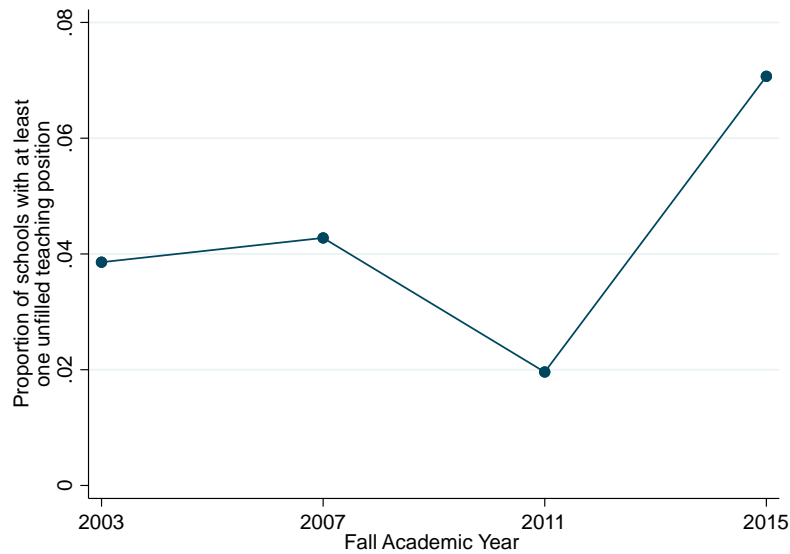
Figure 3: National Trends in School Hiring: Schools with at least One Unfilled Teaching Position

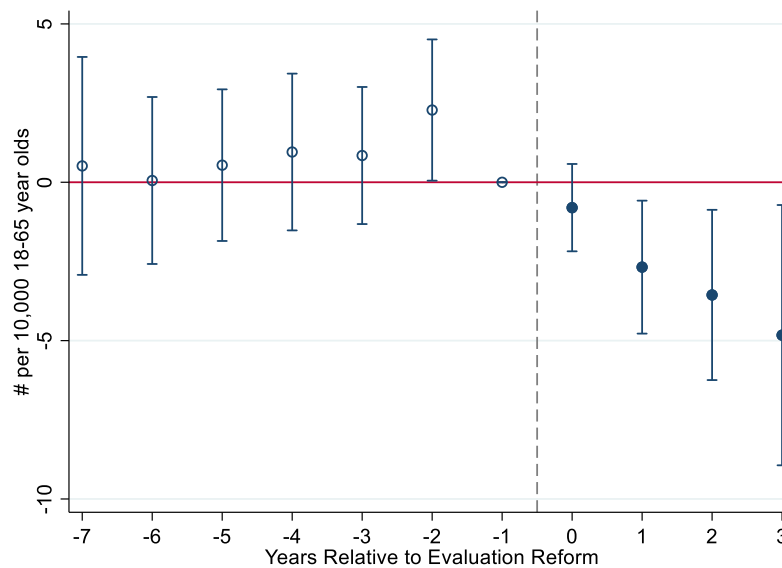Notes: Data come from the SASS and NTPS survey School Questionnaires.

Figure 4: Event Study Depicting Effect of Evaluation Reforms on the Number of New Teaching Licenses

Notes: Point estimates for years pre- (hollow dot) and post-reform (solid dot) and corresponding 95 percent confidence intervals are derived from a simple event study model. Estimates depicted in this figure are reported in Appendix Table A2.
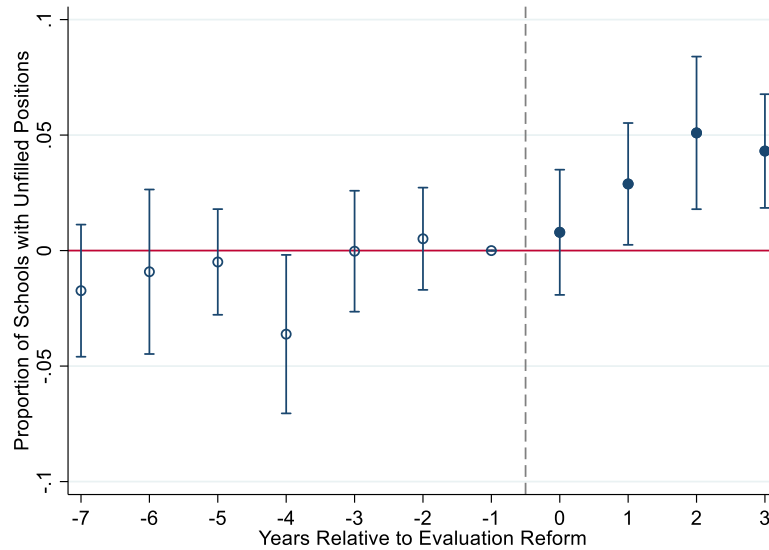
Figure 5: Event Study Depicting Effect of Evaluation Reforms on School Hiring: Schools with at Least One Unfilled Teaching Position

Notes: Point estimates for years pre- (hollow dot) and post-reform (solid dot) and corresponding 95 percent confidence intervals are derived from a simple event study model. Estimates depicted in this figure are reported in Appendix Table A2.
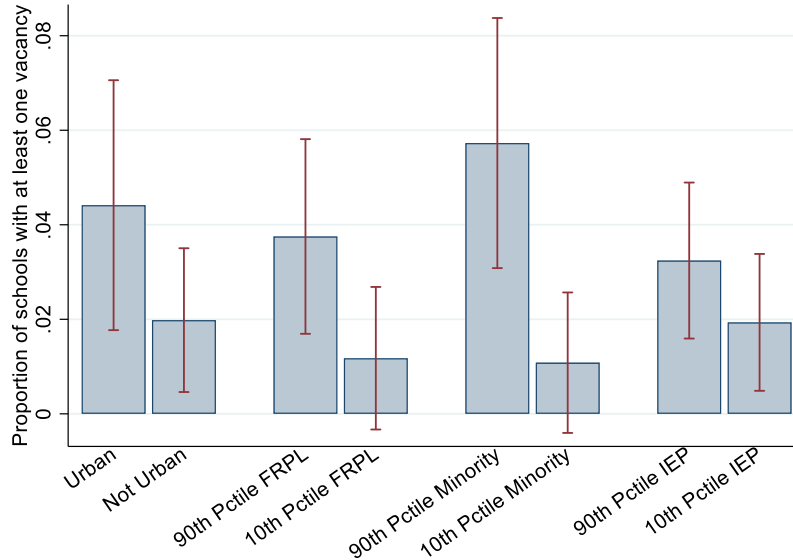
Figure 6. The Heterogeneous Effects of Evaluation Reforms on School Hiring: The Probability Schools Had At least One Unfilled Teaching Position by School Characteristics

Notes: Bars represent 95% confidence intervals. Estimates based on models that include state, policy, and school controls reported in Table 4.
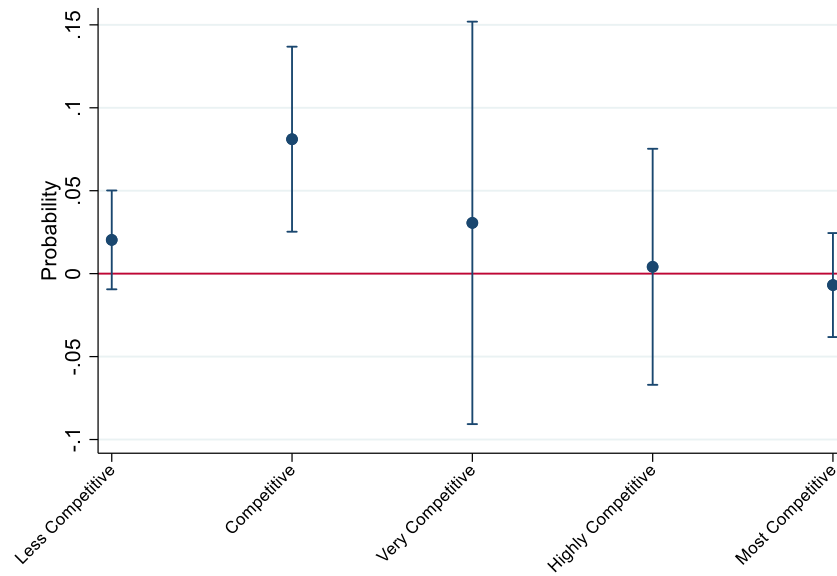
Figure 7: Point Estimates of the Effect of Evaluation Reforms at Different Points in the Distribution of the Selectivity of Newly Hired Novice Teachers' Undergraduate Institutions.

Notes: Each estimate is associated with a binary outcome measure that divides the Barron's ranking scale into two groups, the ranking level labeled in the figure or higher rankings (which take on a value of one) versus all lower ranking levels (which take on a value of zero).
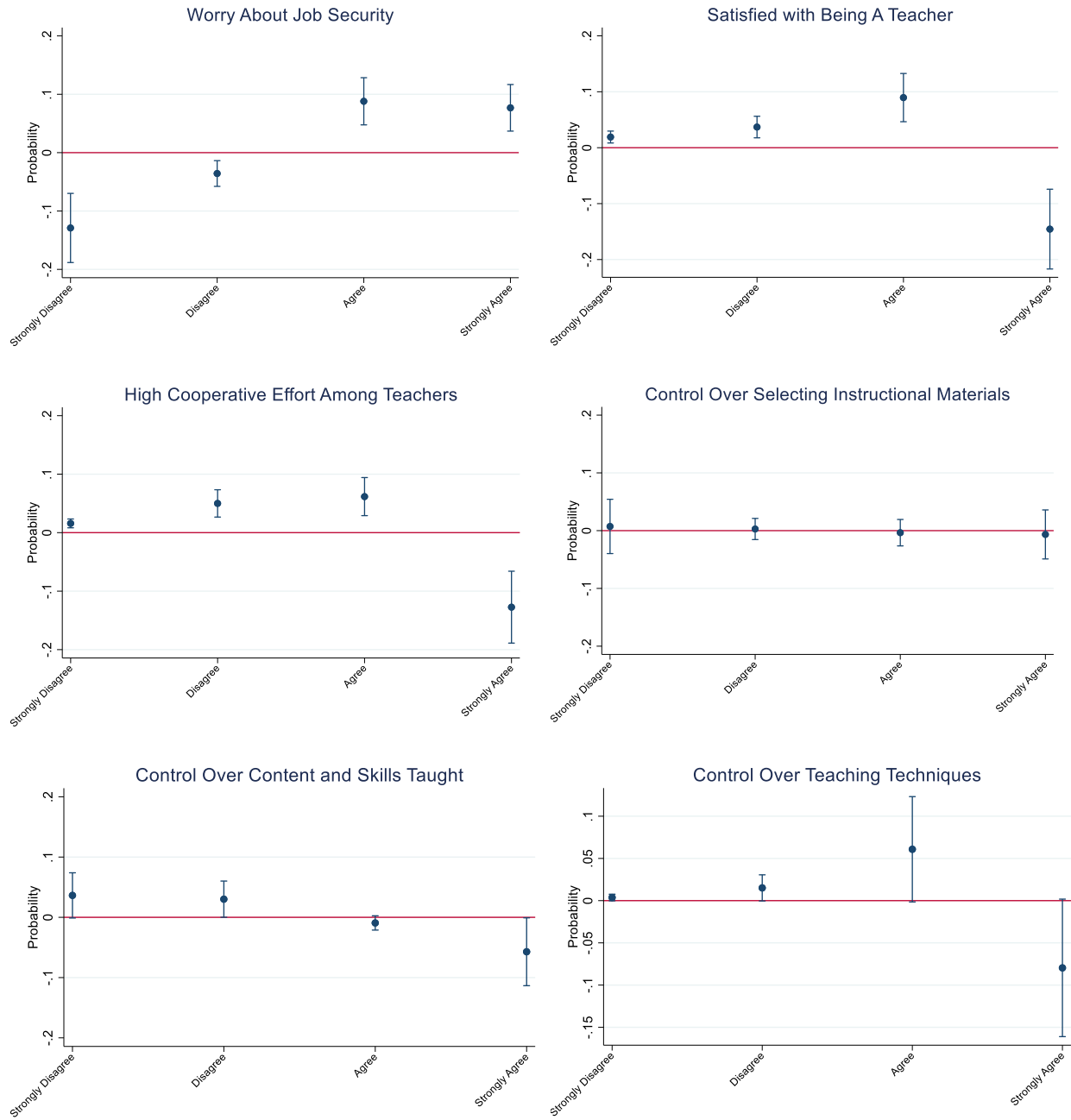
Figure 8. Predicted Marginal Effects of Evaluation Reforms on New Teachers' Perceived Working Conditions.
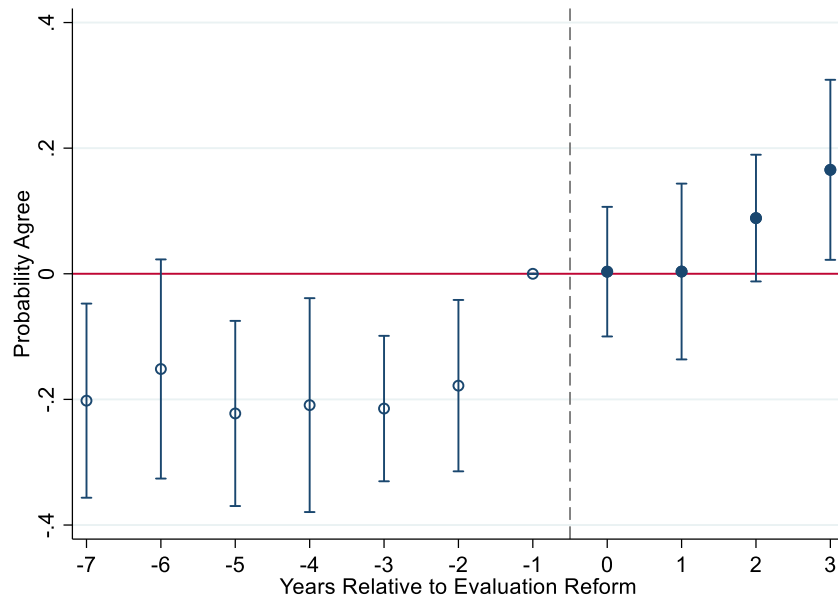
Figure 9. Event Study Depicting Effect of Evaluation Reforms on Probability Teachers Agree that They Worry About Job Security.

Notes: Point estimates for years pre- (hollow dot) and post-reform (solid dot) and corresponding 95 percent confidence intervals are derived from a simple event study model. Estimates depicted in this figure are reported in Appendix Table A2.
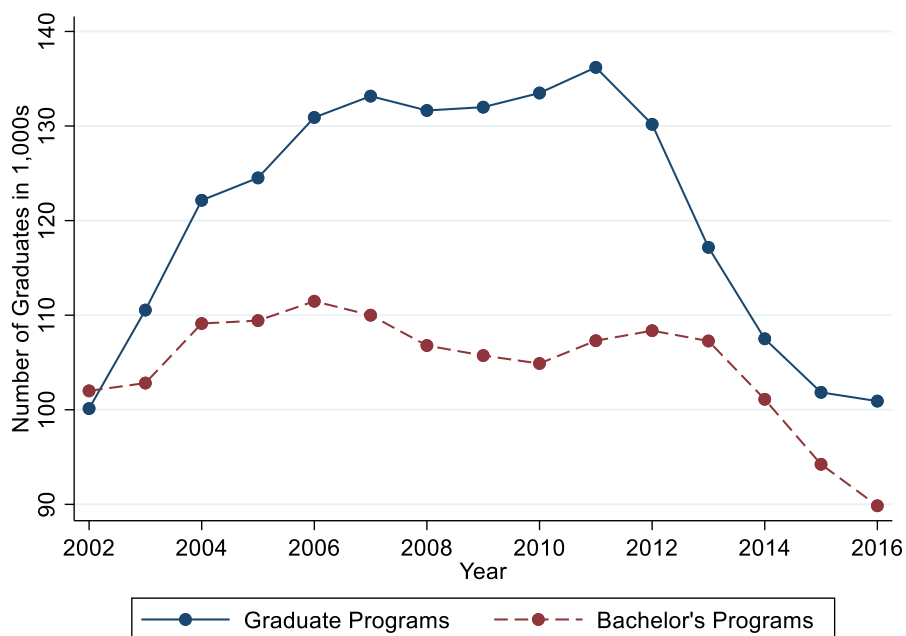

Figure 10: National Trends in the Number of Degree Completers from Graduate and Bachelor's Teacher Preparation Programs, 2002-2016

Notes: Data come from the Integrated Postsecondary Education Data System

Figure 11. Event Study Depicting Effect of Evaluation Reforms on The Number of Graduates from Graduate-level Teacher Preparation Programs

Notes: Point estimates for years pre- (hollow dot) and post-reform (solid dot) and corresponding 95 percent confidence intervals are derived from a simple event study model. Estimates depicted in this figure are reported in Appendix Table A2.

Table A1: Education Policy Reforms by State, 2002-2016

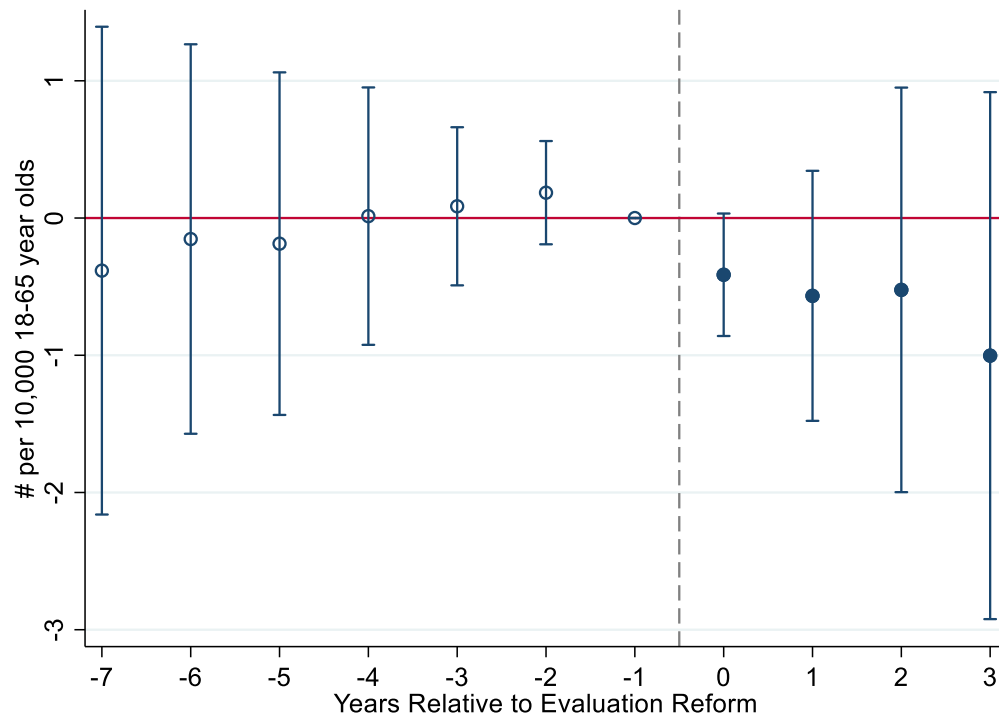|	Pass Evaluation	Implement Evaluation	Eliminate Tenure	Increase Probationary Period	Weaken Collective Bargaining	Eliminate Mandatory Union Dues	Won Race to the Top Grant	Implement Common Core Standards	Basic Skills Licensure Tests	Content Area Licensure Tests	Pedagogical Knowledge Licensure Tests
Alabama	2013	2016						2013	2003	2005	2005
Alaska	2012	2016							2006	2016	
Arizona	2010	2013					2011	2013			
Arkansas	2011	2014						2013	2002		
California								2014			
Colorado	2010	2013					2011	2013		2007	
Connecticut	2012	2014						2013			2005
Delaware	2010	2012					2010	2012		2005	
Florida	2011	2011	2011				2010				
Georgia	2012	2014					2010	2014			
Hawaii	2010	2013		2013			2010	2013			
Idaho	2011	2014	2011		2011			2013		2005	2005
Illinois	2010	2016					2011	2013	2005	2005	2003
Indiana	2011	2014			2005	2012					
Iowa								2012	2003	2015	2015
Kansas	2012	2014	2014					2013		2004	
Kentucky	2013	2014					2011	2011	2008	2003	2003
Louisiana	2010	2012	2012	2012			2011	2013			
Maine	2012	2016		2011				2012	2015	2015	2015
Maryland	2010	2013		2010			2010	2013			
Massachusetts	2011	2013					2010	2013	2005	2005	
Michigan	2010	2011		2011	2011	2013		2012			
Minnesota	2011	2014								2005	2002
Mississippi	2012	2014						2013	2015	2015	2015
Missouri	2011	2013			2005			2014	2005		2015
Montana								2013		2016	
Nebraska	2012								2006	2016	

State											
Nevada	2011	2015		2011				2013	2015	2006	
New Hampshire	2013	2016		2011				2014			
New Jersey	2012	2013		2012			2011	2013	2015	2015	
New Mexico	2011	2013						2013		2003	
New York	2010	2012		2015			2010	2013	2015	2005	
North Carolina	2010	2011	2013				2010	2012	2015	2015	2015
North Dakota	2011	2015						2013	2015	2008	2015
Ohio	2011	2013		2009			2010	2013		2006	2006
Oklahoma	2010	2013									2004
Oregon	2011	2013						2014	2006	2006	
Pennsylvania	2012	2013					2011	2013			
Rhode Island	2010	2012					2010	2013	2015	2015	2005
South Carolina	2013	2014									
South Dakota	2010	2015						2014	2007		2007
Tennessee	2010	2011		2011	2011		2010	2013	2005		
Texas	2013	2016						2012	2012		2012
Utah	2012	2015						2013	2016	2005	
Vermont								2013	2002	2016	
Virginia	2011	2012									2005
Washington	2012	2015		2010				2014	2005	2005	
West Virginia	2011	2013				2016		2014			
Wisconsin	2012	2014			2011	2015		2014		2005	
Wyoming	2012							2014			

Notes: See Appendix C for details about each measure. Our time-varying measures account for reforms that were later reversed. Georgia eliminated tenure in 2000 and reinstituted tenure in 2003. Reform years in bold are reforms that occurred during the same year as evaluation reforms. This table does not contain data on two other non-binary education reforms for which we control, the number of alternative certification program types and the percent of salary withheld for pension funds.

Table A2: The Effect of Teacher Evaluation Reforms from Event Study Models

	Number of Licenses		At least one unfilled vacancy in a school		Undergraduate Institution with Competitive Admissions		The Number of Program Completers from Graduate Teacher Preparation Programs		Agree Worry about Job Security	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
r=-7 or <	0.516	1.736	-0.017	-0.022+	-0.049	0.039	-0.383	0.268	-0.202*	-0.179*
	(1.755)	(1.670)	(0.015)	(0.012)	(0.042)	(0.044)	(0.907)	(0.755)	(0.079)	(0.086)
r=-6	0.056	0.768	-0.009	-0.013	-0.135**	-0.073	-0.153	0.326	-0.152+	-0.091
	(1.345)	(1.369)	(0.018)	(0.015)	(0.042)	(0.047)	(0.724)	(0.592)	(0.089)	(0.098)
r=-5	0.540	1.347	-0.005	-0.002	0.036	0.082*	-0.187	0.233	-0.222**	-0.195*
	(1.221)	(1.180)	(0.012)	(0.008)	(0.033)	(0.039)	(0.637)	(0.576)	(0.075)	(0.076)
r=-4	0.955	1.696	-0.036*	-0.044**	-0.093+	-0.003	0.014	0.233	-0.209*	-0.197*
	(1.263)	(1.186)	(0.018)	(0.015)	(0.050)	(0.055)	(0.478)	(0.486)	(0.087)	(0.096)
r=-3	0.845	1.481	-0.000	-0.013	-0.051	0.056	0.086	0.184	-0.215***	-0.213*
	(1.104)	(1.060)	(0.013)	(0.012)	(0.046)	(0.046)	(0.294)	(0.275)	(0.059)	(0.080)
r=-2	2.281+	2.725*	0.005	0.009	-0.097*	-0.075+	0.185	0.241	-0.178*	-0.133+
	(1.137)	(1.135)	(0.011)	(0.011)	(0.042)	(0.042)	(0.192)	(0.163)	(0.070)	(0.073)
r=0	-0.802	-1.040	0.008	-0.001	-0.018	0.065	-0.413+	-0.511*	0.003	0.023
	(0.704)	(0.728)	(0.014)	(0.012)	(0.037)	(0.041)	(0.228)	(0.191)	(0.053)	(0.063)
r=1	-2.679*	-3.189**	0.029*	0.013	0.086	0.184**	-0.567	-0.736+	0.004	-0.025
	(1.071)	(1.055)	(0.013)	(0.013)	(0.057)	(0.056)	(0.465)	(0.389)	(0.071)	(0.091)
r=2	-3.558*	-4.193**	0.051**	0.041**	-0.051	-0.007	-0.524	-0.766	0.089+	0.097
	(1.371)	(1.292)	(0.017)	(0.015)	(0.044)	(0.048)	(0.752)	(0.604)	(0.051)	(0.073)
r=3 or >	-4.828*	-6.043**	0.043**	0.029*	0.142*	0.163**	-1.003	-1.370+	0.166*	0.103
	(2.096)	(1.872)	(0.013)	(0.013)	(0.063)	(0.055)	(0.980)	(0.804)	(0.073)	(0.089)
School Controls	No	No	No	Yes	No	Yes	No	No	No	Yes
State Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Policy Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
n	750	750	28,610	28,610	5,800	5,800	750	750	5,800	5,800

Notes: +p<.10, *p<.05, **p<.01, ***P<.001. Cells report estimates and associated standard errors clustered at the state level in parentheses. See Table 2 notes for a complete list of time-varying state and policy controls and Table 3 notes for a complete list of school controls. All models include state and region-by-year fixed effects.

Table A3: The Effect of Teacher Evaluation Reforms on the Competitiveness of New Teachers' Undergraduate Institutions

	Less Competitive or higher	Competitive or higher	Very Competitive or higher	Highly Competitive or higher	Most Competitive or higher
	(2)	(3)	(4)	(5)	(6)
Implement Evaluation	0.020	0.081**	0.031	0.004	-0.007
	(0.015)	(0.028)	(0.062)	(0.036)	(0.016)
n	5,800	5,800	5,800	5,800	5,800
School Controls	Yes	Yes	Yes	Yes	Yes
State Controls	Yes	Yes	Yes	Yes	Yes
Policy Controls	Yes	Yes	Yes	Yes	Yes

Notes: +p<.10, *p<.05, **p<.01, ***P<.001. Cells report estimates and associated standard errors clustered at the state level in parentheses. See Table 2 notes for a complete list of time-varying state and policy controls and Table 3 notes for a complete list of school controls. All models include state and region-by-year fixed effects.

Table A4: The Effect of Teacher Evaluation Reforms on New Teacher Working Conditions

	(1)	(2)	(3)	(4)
	Panel A. Worry About Job Security			
Implement Evaluation	1.892***	2.038***	1.572*	1.791**
	(0.320)	(0.354)	(0.337)	(0.405)
Implement Evaluation * Trend			1.066	1.034
			(0.073)	(0.070)
Trend			1.033	1.035
			(0.022)	(0.028)
	Panel B. Job Satisfaction			
Implement Evaluation	0.579***	0.523***	0.636*	0.685+
	(0.073)	(0.083)	(0.114)	(0.134)
Implement Evaluation * Trend			0.968	0.866*
			(0.055)	(0.061)
Trend			0.983	0.983
			(0.021)	(0.023)
	Panel C. Teacher Cooperation			
Implement Evaluation	0.673**	0.582***	0.475***	0.455***
	(0.088)	(0.077)	(0.090)	(0.077)
Implement Evaluation * Trend			1.181*	1.107
			(0.078)	(0.075)
Trend			1.022	1.041+
			(0.020)	(0.022)
	Panel D. Control over Selecting Textbooks and Instructional Materials			
Implement Evaluation	0.997	0.958	1.141	1.001
	(0.165)	(0.137)	(0.250)	(0.216)
Implement Evaluation * Trend			0.960	0.991
			(0.060)	(0.061)
Trend			0.971	0.987
			(0.032)	(0.027)
	Panel E. Control over Content, Topics, Skills Taught			
Implement Evaluation	0.765+	0.736+	0.694*	0.590**
	(0.118)	(0.115)	(0.111)	(0.099)
Implement Evaluation * Trend			1.036	1.072
			(0.051)	(0.056)
Trend			1.016	1.050
			(0.043)	(0.035)
	Panel F. Control over Teaching Techniques			
Implement Evaluation	0.772	0.656*	0.669**	0.569**
	(0.124)	(0.110)	(0.096)	(0.106)
Implement Evaluation * Trend			1.048	0.998
			(0.061)	(0.051)
Trend			1.031	1.071*
			(0.029)	(0.033)
n	6,460	6,460	6,460	6,460

School Controls	No	Yes	No	Yes
State Controls	No	Yes	No	Yes
Policy Controls	No	Yes	No	Yes

Notes: +p<.10, *p<.05, **p<.01, ***P<.001. Cells report estimates and associated standard errors clustered at the state level in parentheses. See Table 2 notes for a complete list of time-varying state and policy controls and Table 3 notes for a complete list of school controls. All models include state and region-by-year fixed effects.

Table A5: Alternative Model Specifications for the Effect of Teacher Evaluations on the Number of New Teaching Licenses

Alternative Specification	Year state legislatures passes evaluation reforms		Replacing region-by-year with year fixed effects		Restrict sample to only 29 state that only passed teacher evaluation reforms		Include endogenous controls for teacher demand		Pre a Post Trends for Concurrent Accountability Reforms and Policy Controls	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Evaluation	-1.898**	-3.021***	-2.421**	-1.725+	-4.169***	-2.504*	-2.733***	-1.276	-1.779*	-0.115
	(0.652)	(0.711)	(0.820)	(0.977)	(1.111)	(1.074)	(0.692)	(0.888)	(0.758)	(0.770)
Evaluation * Trend		-2.239***		-0.943+		-1.383*		-1.135*		-2.054***
		(0.422)		(0.523)		(0.623)		(0.453)		(0.396)
Trend		1.023***		0.368**		0.075		0.001		0.517*
		(0.205)		(0.136)		(0.298)		(0.212)		(0.236)
% change relative to state mean	-12%		-15%		-26%		-17%		-11%	
State Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Policy Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
n	750	750	750	750	405	405	750	750	750	750

Notes: +p<.10, *p<.05, **p<.01, ***P<.001. Cells report estimates and associated standard errors clustered at the state level in parentheses. See Table 2 notes for a complete list of time-varying state and policy controls. Endogenous controls for teacher labor demand include the number of novice public school teachers, logged public and private school enrollment, and public school pupil-teacher ratio. We impute data for years in which the number of novice public school teachers and private school enrollment are not available using linear interpolation.

Table A6: Alternative Weighting Approaches for Modeling the Effect of Teacher Evaluations on the Number of New Teaching Licenses

Outcome specification	Scaled per number of 18-65 year olds per 10,000		Scaled per number of 22-25 year olds per 1,000		Scaled per number of 18-65 year olds per 10,000		Logged, controlling for logged number of 18-65 year olds per 10,000	
Weights	Number of 18-65 year olds per 10,000		Number of 22-25 year olds per 1,000		No weights		No weights	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Implement Evaluation	-2.996***	-1.256	-3.654***	-1.576	-2.880**	-1.729+	-0.202**	-0.097
	(0.722)	(0.907)	(0.861)	(1.044)	(0.927)	(0.898)	(0.061)	(0.065)
Implement Evaluation * Trend		-1.468**		-1.789**		-1.225**		-0.097**
		(0.456)		(0.527)		(0.404)		(0.034)
Trend		0.188		0.249		0.214		0.010
		(0.177)		(0.207)		(0.218)		(0.014)
% change relative to state mean	-18%		-22%		-18%			
State Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Policy Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
n	750	750	750	750	750	750	750	750

Notes: +p<.10, *p<.05, **p<.01, ***P<.001. Cells report estimates and associated standard errors clustered at the state level in parentheses. See Table 2 notes for a complete list of time-varying state and policy controls.

Table A7: The Effect of High-Stakes Teacher Evaluation and Tenure Reforms on Public Teacher Salaries

	NCES Average Salary		ACS Adjusted Average Salary	
	(1)	(2)	(3)	(4)
Implement Evaluation	-330.652	-173.864	-509.883+	-152.010
	(499.984)	(388.157)	(275.208)	(261.144)
Implement Evaluation * Trend		237.096		-195.319
		(245.300)		(191.060)
Trend		-334.939*		-127.370
		(126.930)		(107.655)
% change relative to state mean	-1%		-1%	
State Controls	Yes	Yes	Yes	Yes
Policy Controls	Yes	Yes	Yes	Yes
n	750	750	600	600

Notes: +p<.10, *p<.05, **p<.01, ***P<.001. Cells report estimates and associated standard errors clustered at the state level in parentheses. See Table 2 notes for a complete list of time-varying state and policy controls. All models include state and region-by-year fixed effects and are weighted by the total number of Full Time Equivalent (FTE) teachers in each state and year.
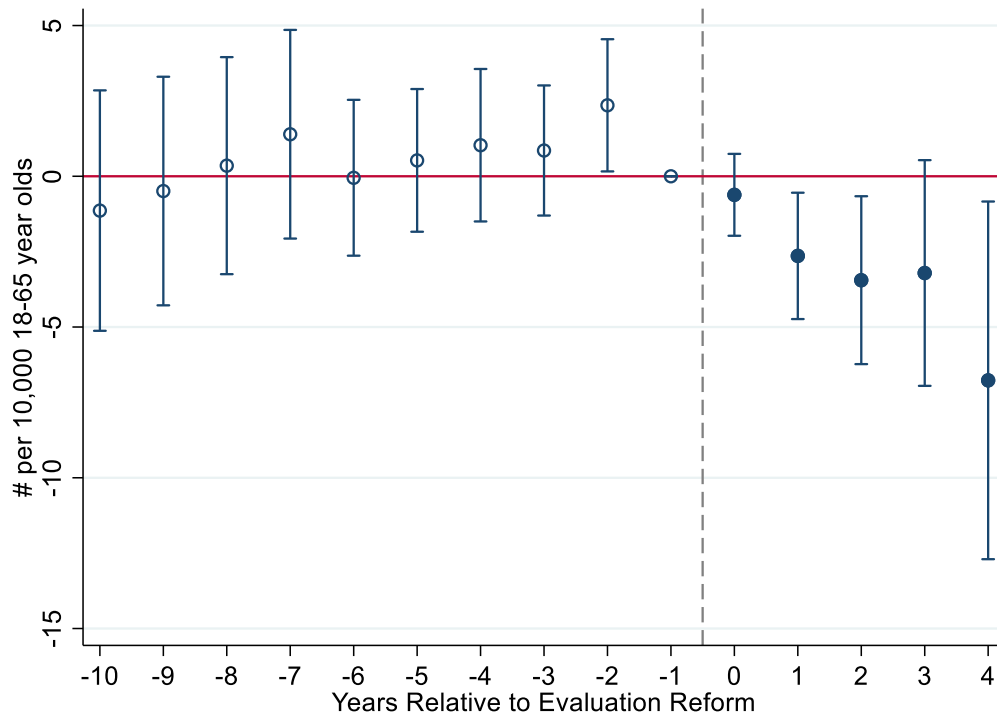
Appendix Figures

Figure A1: Event Study Depicting Effect of Evaluation Reforms on the Number of New Teaching Licenses with Alternative Minimum and Maximum Years.

Notes: Point estimates for years pre- (hollow dot) and post-reform (solid dot) and corresponding 95 percent confidence intervals are derived from a simple event study model. Estimates depicted in this figure are reported in Appendix Table A2.

Appendix A

Data Sources for Education Policy Reforms

Teacher Evaluation Reforms

We draw upon two systematic reviews of teacher evaluation reforms to create two binary measures that reflect the timing of evaluation reforms across states. We define "pass evaluation" as the year in which state legislatures pass a law reforming teacher evaluation systems and all following years. We define "implement evaluation" as the school year in which the state first implemented evaluation reforms state-wide and all following years. We code states as implementing evaluation even if some component of the new evaluation system were not yet in place or did not yet count towards teachers' formal evaluation scores.

> **Pass Year**: We code passing evaluation reforms based on the calendar year the law was passed.

> **Implementation Year**: We code implementing evaluation reforms based on the fall of the first academic year with state-wide implementation.

> **Sources**: National Council on Teacher Quality (2016); Steinberg and Donaldson (2016). We prioritize the NCTQ report and use Steinberg and Donaldson 2016 to resolve ambiguities and address states with missing information.

Teacher Tenure Reforms

We compiled data on teacher tenure reforms from reviews of state statutes, case law, new articles and prior literature. To capture the effect of the elimination of tenure on teacher labor supply, we created an indicator variable, *Eliminate Tenure,* which is equal to one when state eliminated tenure and all following years, and zero otherwise.

Note: Ohio and South Dakota passed laws eliminating tenure protections that were overturned the same year by state ballot referendums. Pennsylvania's tenure bill was vetoed by Governor. We do not code these three states as having eliminated tenure given the legislation was overturned in the same calendar year. Georgia eliminated tenure in 2000 and later reinstated it in 2003. We do not code Georgia as eliminating tenure as their reform occurred prior to the start of our data panel. Advocates have filed lawsuits in California, Minnesota, and New York attempting to challenge the legality of teacher tenure although none of these suits were ultimately successful.

> **Year**: We code tenure reforms based on the calendar year in which the state legislature passed the reform.

> **Sources**:
> * The National Council on Teacher Quality's (NCTQ) State Teacher Policy Yearbooks

- The Education Commission of the States' State Policy Database
- Students First: State Policy Report Cards

We resolved discrepancies between these sources using information from states' department of education websites, RTT federal grant applications, Lexis-Nexis searches of state and local newspapers, and conversations with academics and state Department of Education officials.

Probationary Period Increases

We collected data on probationary periods through a comprehensive review of state laws during our panel period. A total of 12 states extend their probationary period for tenures by at least one year during our panel period. We create a binary variable that takes on a value of one in the year a state increase the probationary period and all following years. This serves to eliminate the missing data problem caused by states that do not have probationary periods or teacher tenure.

Note: Hawaii decreased the probationary period in 2008 from two to one years and then raised to two again in 2009. It then raised the probationary period to three in 2013. We only code this later raise in our data.

> **Year**: We code these changes as occurring in the year in which the legislation was passed. It appears these laws all went into effect immediately upon being enacted.

> **Source**: Authors' own research based on news articles and state legislative documents.

Collective Bargaining Reforms

We compiled data on collective bargaining reforms through a comprehensive review of state laws during our panel period. A total of six states passed legislation that eliminated mandatory collective bargaining with public teacher unions or substantially restricted the scope of which aspects of teachers' contracts were subject to collective bargaining (ID [later reversed], IN, MO, NM, TN, and WI). Ohio also made collective bargaining illegal in 2011, but the law was overturned that same year in a statewide referendum. We control for a time-varying indicator of whether collective bargaining is not mandatory in a given state.

> **Year**: We code collective bargaining reforms based on the calendar year in which the state legislature passed the reform.

> **Source**: Authors' own research based on news articles and state legislative documents.

Right To Work Reforms (Eliminate Mandatory Union Dues)

We collected data on probationary periods through a comprehensive review of state laws during our panel period. A total of four states ended mandatory union dues during our panel period. These laws were challenged in court in three of these states (IN, WI, and WV), but were ultimately upheld.

Year: We code right to work laws as occurring in the year in which the legislation was passed. It appears these laws all went into effect immediately upon being enacted.

Source: Authors' own research based on news articles and state legislative documents.

Race to the Top Winners

A full list of the timing and dates of race to the top winners is available from the U.S. Department of Education. We code this time-varying variable as taking on a value of one in the year grant awards were announced and all following years.

Year: We code Race to the Top winners based on the calendar year in which the award was announced by the U.S. Department of Education.

Source: U.S. Department of Education
https://www2.ed.gov/programs/racetothetop/awards.html

Alternative Certification Programs

We measure the availability of alternative certification programs using data maintained by the U.S. Department of Education Office of Title II. These data track characteristics of alternative certification programs in operation across each state including the year in which each program was first implemented. We use these data to reconstruct a panel dataset from 2002 to 2016 with counts of number of alternative certification programs operating in each state in each year.

Year: We code alternative certification programs based on the implementation year (calendar vs. academic unspecified) provided by U.S. Department of Education, Higher Education Act Title II State Report Card System.

Source: U.S. Department of Education Office of Title II
https://title2.ed.gov/Public/DataTools/Tables.aspx

Common Core State Standards

We compiled data on the implementation of the Common Core State Standards (CCSS) from the Common Core State Website. We compiled data on the timing and duration of a state's adoption of the Common Core State Standards (CCSS) from several news sources listed below. We constructed a time-varying indicator for the school years in which states implemented the common core state standards statewide. Oklahoma and Indiana had initially adopted the standards, but dropped them before implementing them statewide. South Carolina implemented the CCSS for the 2014-15 year, but then abandoned the standards the following year. Some states such as Pennsylvania have renamed their standards and modified them. States that have maintain at least 85 percent of the CCSS maintain their CCSS status.

Year: We code CCSS implementation based on the fall school year.

Source: Common Core State Website
http://www.corestandards.org/standards-in-your-state/

We use a range of news article to update these data for states that dropped CCSS.

New Teacher Licensure Tests

We construct time-varying indicators for different types of licensure exams states can require based on tables from the annual Digest of Education Statistics compiled by the National Center for Education Statistics (NCES). Each year the digest contains a table titled "States requiring testing for initial certification of teachers, by skills or knowledge and state." We include indicators for three types of licensure exams coded: basic skills exams, subject exams, and professional knowledge exams. The basic skills exam is a test of fundamental reading, writing and mathematics skills. The subject exams test content knowledge in the area for which a teacher will receive licensure. The professional knowledge exam tests knowledge of pedagogy in areas such as educational psychology, classroom management, lesson planning and evaluation, and assessment. When data was listed as missing in the tables we coded it as zero for not having adopted a new licensure test.

Note: Some states, prior to instituting a uniform licensure test, used other exams such as an "Institution's Exit Exam" or "Subject matter exam or completion of an approved subject matter program" as proxies. We do not code these licensure tests.

Year: We code licensure data based on the fall school year a new law was adopted.

Source: Annual Digest of Education Statistics compiled by the National Center for Education Statistics (NCES).
https://nces.ed.gov/programs/digest/d11/tables/dt11_179.asp
The source data for these tables come from National Association of State Directors of Teacher Education and Certification (NASDTEC) Manuel on the Preparation and Certification of Educational Personnel in the United States and Canada States.

Teacher Licensure Exam Passing Rates

We measure teacher passing rates for each state for each year using the number of licensure exams taken and passers in a given state in a given year, and then calculating the share of tests that scored at or above the passing threshold. This measure is an average across all licensure tests required for an initial teaching credential in a state, which vary by state. Approximately 74% of these tests were administered by ETS, 20% were administered by Pearson, and the rest were administered by individual states. Because each state sets its own cut score for passing each exam, the cut scores vary across states and across exams. Note that some states, particularly in the earlier years, have missing data due to differences in assessment requirements and systems and processes for state reporting. We impute the state average pass rate across the panel for missing years and include an indicator for missing data.

Year: The report year provided in the Title II database reflects the year that follows the spring academic year. We recode this to the prior calendar year, the year in which most teachers were likely taking the test (January – July) to start teaching in the prior academic year.

Source: The data on state by-year licensure exam pass rates come from the Title II office, who obtained data from the states. States and teacher preparation providers work directly with testing companies to submit lists of enrolled students and program completers. The testing companies match those lists to produce the pass rates for each assessment and provider, and there are several rounds of data verification. Once the data are verified by all parties, the testing companies package the data and send them to the Department of Education to upload into the Title II reports.

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

Data starting in 2010-2011 are available online. We obtained earlier data in aggregate form from Westat which provides technical assistance and manages the Title II data.

Teacher Pensions

We measure teacher pension contributions as the required employee contribution rate, which captures the percent of total wages that teachers must contribute towards pension funds. We control for a continuous measure of the percentage of teachers' salary that they contribute towards pension funds in each state and year.

Year: We use the year (likely calendar) provided in the Public Plans Database.

Source: Annual data on employee contribution rates for state and locally sponsored teacher pension funds comes from the Public Plans Data maintained by the Center for Retirement Research at Boston College.
https://crr.bc.edu/data/public-plans-database/

Appendix B

Roy Model of Occupational Choice

To illustrate the potential effects of teacher evaluation reforms on the supply and quality of prospective teachers, we adapt a simple Roy (1951) model of occupational choice in which accountability reforms increase the relative costs associated with teaching. Individuals choose between a career teaching in public schools (hereafter teaching), T, or an alternative occupation, A, that represents all outside labor market options, by choosing the occupation that maximizes expected earnings, w. Earnings in teaching and the alternative occupation are given by:

$$w_T = \mu_T + \eta_T v - Cg(v)$$

$$w_A = \mu_A + v$$

where μ_T and μ_A denote average earnings in the teaching and alternative occupation respectively, v denotes the individual's ability, which is continuously distributed with mean zero and variance σ_v^2, and η_T is the return to ability in the teaching sector relative to the alternative occupation. Assuming $0 < \eta_T < 1$, earnings will be more compressed in the teaching sector.[27]

The term C denotes costs common to all teachers. These costs may include the opportunity costs of earning a teaching certificate from an educator preparation program and passing state teacher licensure exams. We conceptualize evaluation reforms as increasing C through the monetized costs of declines in job security and/or job satisfaction in teaching relative to the alternative occupation. The term $g(v)$ allows costs to depend on ability. We assume $g(v)$

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[27] For simplicity we assume that the type of ability that is valued in the teaching and non-teaching sectors is perfectly positively correlated. More realistically, the types of skills valued in the two sectors likely differs to some extent implying that v may differ across sectors. Nevertheless, if ability is valued in both sectors and the correlation between the type of ability that is valued in both sectors is sufficiently strong, then relaxing the assumption that v is the same across sectors leads to predictions that are qualitatively the same as those based on our simplifying assumption.

is from the family of exponential functions with: $g(v) > 0$, $g_v < 0$, $g_{vv} > 0$, $\lim_{v \to -\infty} g(v) = \infty$,

$\lim_{v \to \infty} g(v) = 0$.[28] The condition $g_v < 0$ implies the relative costs associated with teaching decline

with ability.[29]

An individual chooses teaching as a career if $w_T > w_A$, implying the individual

indifferent between a career in teaching and the alternative occupation is characterized by the

implicit function:

$$F(v, C, \eta_T, \mu_T, \mu_A) = \mu_T - \mu_A - Cg(v) - v(1 - \eta_T) = 0.[30] \qquad (1)$$

Figure B1 illustrates the effect of an increase in the relative costs of teaching. Note that

the concavity of $F(v, C, \eta_T, \mu_T, \mu_A)$ with respect to v implies there is both a high- *and* low-ability

individual on the margin between teaching and the alternative occupation. Denoting the marginal

low- and high-ability individuals as v_L *and* v_H respectively, the change in $v_j \in (v_L, v_H)$ due to a

change in the relative costs associated with teaching is:

$$\frac{\partial v_j}{\partial C} = \frac{g(v_j)}{-Cg_{v_j} - (1 - \eta_T)}. \qquad (2)$$

Because the numerator of (2) is strictly positive, the sign of (2) depends on the sign of the

denominator, which is the slope of the implicit function given by (1) evaluated at the roots, v_j.

Consequently, it follows that, $\frac{\partial v_L}{\partial C} > 0$ and $\frac{\partial v_H}{\partial C} < 0$, causing v_L to shift right to v_L' and v_H to shift

left to v_H'. As a result, both the share of high- and low-ability individuals that choose teaching as

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[28] Examples of functional forms that satisfy these assumptions are $g(v) = e^{-\rho v}$ and $g(\rho, v) = \rho^v$, where the parameter $0 < \rho < 1$ determines the degree to which costs differ by ability.

[29] A special case of our model occurs when the relative costs associated with teaching are the same for all individuals, implying $Cg(v) = C$. In that case it is trivial to show that an increase in C, leads to a reduction in the supply of prospective teachers and a decline in the average ability of teachers. Intuitively, when the relative costs are the same for everyone, individuals on the margin between teaching and the alternative occupation are the highest ability prospective teachers. Consequently, when costs increase, the supply and average ability of individuals choosing a career in teaching declines.

[30] Note that $\lim_{v \to -\infty} F(v, C, \eta_T, \mu_T, \mu_A) = -\infty$ and $\lim_{v \to \infty} F(v, C, \eta_T, \mu_T, \mu_A) = -\infty$.

a career declines, leading to an unambiguous reduction in the supply of prospective teachers but an ambiguous change in teacher quality.[31] Note that our model also predicts that v_L shifts to the right by more than v_H shift to the left. Intuitively, because costs decline with ability, costs rise more for the marginal low-ability individual than for the marginal high-ability individual. Nevertheless, the net effect of these changes on teacher quality remains ambiguous since it depends on the density of individuals close to the marginal low- and high-ability teacher.

Changes in the Return to Ability

While teacher evaluation reforms may increase the perceived costs associated with teaching, merit pay schemes based on evaluation ratings could attract more qualified teachers into the profession. In the context of our model, merit pay can be thought of as an increase η_T or an increase in the returns to ability in the teaching sector. Solving for the change in the marginal ability individual due to a change in η_T yields:

$$\frac{\partial v_j}{\partial \eta_t} = \frac{-v_j}{-Cg_{v_j}-(1-\eta_T)}. \tag{3}$$

To sign (3) note that the denominator is the same as the denominator of (2), implying it is positive for v_L and negative for v_H. The sign of (3) therefore depends on the numerator, which solely depends on ability evaluated at v_L and v_H. Empirical evidence on the ranking of teachers within the ability distribution of college graduates suggests that on average teachers rank around the 40th to 50th percentile relative to their peers, implying that v_L is likely negative and v_H positive when v is standardized with mean zero.[32] An increase in η_T therefore causes the share

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[31] More formally, the share of individuals that choose a career in teaching is given by $\Pr(T) = \int_{v_L}^{v_H} f(v)dv$ and the effect of an increase in the relative costs associated with teaching on the supply of prospective teachers is: $\frac{\partial \Pr(T)}{\partial C} = \frac{\partial v_H}{\partial C} f(v_H) - \frac{\partial v_L}{\partial C} f(v_L) < 0$.

[32] For example, Master et al. (2016) find that in 2000 and 2008, college graduates entering the teaching profession ranked around the 42nd and 48th percentiles, respectively, on SAT and ACT scores relative to their peers. For more evidence see Corcoran at al. (2004), Goldhaber and Walch (2014), and Lankford et al. (2014).

of low-ability individuals that choose to teach to decline, $\frac{\partial v_L}{\partial \eta_t} > 0$, and the share of high-ability

individuals that choose to teach to increase, $\frac{\partial v_H}{\partial \eta_t} > 0$. Consequently, the introduction of merit pay

causes average teacher quality to increase while leading to an ambiguous change in the supply of

prospective teachers.

In summary, when the relative costs associated with teaching increase due to the

implementation of teacher evaluation reforms, both the share of high- and low-ability individuals

that choose teaching as a career declines, leading to an overall reduction in the supply of

prospective teachers and an ambiguous change in teacher quality. However, if evaluation reforms

simultaneously increase both the relative cost of teaching and the degree to which earnings

depend on ability (i.e., increase η_T), the share of high-ability individuals that choose to teach

may increase. As a result, the quality of prospective teachers may increase but the effect on the

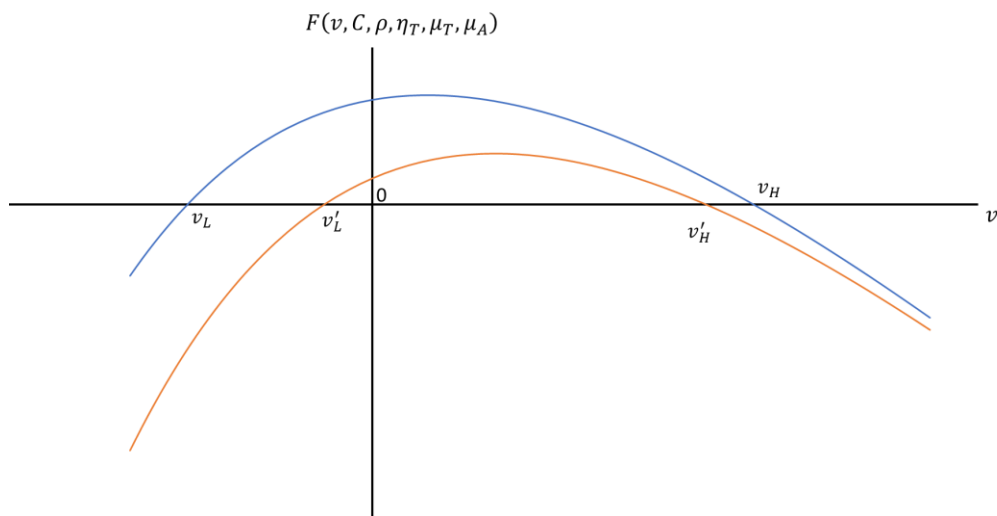share of individuals that choose to teach is ambiguous.

Figure B1: An Increase in the Relative Costs of Teaching when Costs Vary by Ability

Appendix C

Identifying Graduates of Teacher Preparation Programs and by Subject Areas using CIPS Codes

The Integrated Postsecondary Education Data System (IPEDS) includes the results of eleven surveys conducted annually by the U.S. Department of Education's National Center for Education Statistics (NCES). All institutions must submit race and ethnicity data if they receive, are applicants for, or expect to be applicants for federal financial assistance as defined under the DOE's regulations implementing Title VI of the Civil Rights Act of 1964, Title IX of the Education Amendments of 1972, and the Carl D. Perkins Education Act. These surveys thus capture the substantive universe of postsecondary schools, colleges, and universities.

In this paper, we use the entire "Completions" survey available for download on the IPEDS website. Each observation in the file corresponds to the completers of a particular academic program, identified by classification of instruction program (CIP) code, and the award level (e.g., bachelor's degree, master's degree, etc.) of that particular program for each reporting institution.

The NCES has devised a six-digit taxonomy for organizing academic programs called CIP codes. The first two digits of the code correspond to a broad area of study. For instance, all majors under the two-digit CIP category "13" are majors within the education field. These majors include not only education programs designed to prepare individuals to be teachers, but also teaching assistants preparation programs (13.1501) and programs for education program evaluators (e.g., Education Evaluation and Research, 13.0601), among others. The next two digits in the CIP code cluster majors with similar instructional content within the board area of study. All majors with a 13.13 CIP code sequence, for example, are "teacher education or professional development majors within specific subject areas" within the broad field of

education. The final two digits are unique to each specific major that falls within the specific subject area. For example, a "structural engineering" major has a 14.0803 CIP code ("14" corresponds to engineering, "08" correspond to civil engineering, and the final "03" is unique to structural engineering). NCES adds and removes CIP codes regularly. We created consistent categories for all six-digit codes across the panel using crosswalks supplied by NCES.[33]

Based on conversations with IPEDS-reporting institutions and the IPEDS Help Desk Staff, we identify teacher preparation programs based on the following CIP codes:[34]

- Education, General: 13.0101
- Bilingual, Multilingual, and Multicultural Education 13.0201-12.0299
- Curriculum and Instruction: 13.0301
- Special Education and Teaching: 13.1001-13.1099
- Teacher Education & Professional Development, Specific Levels and Methods: 13.1201-13.1299
- Teacher Education & Professional Development, Specific Levels and Methods: 13.1301-13.1399
- Teaching English or French as a Second or Foreign Language: 13.1401-13.1499
- Education, Other: 13.9999

We restrict the data to include only graduates that earned either a bachelor's or master's degree and then sum these university-level counts to the state-by-year level.

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[33] Crosswalks can be found here: https://nces.ed.gov/ipeds/cipcode/crosswalk.aspx?y=55
[34] Dan Goldhaber and Roddy Theobald provided valuable guidance here as well.