Union Reform, Performance Pay, and New Teacher Supply: Evidence from Wisconsin’s Act 10*

E. Jason Baron†

Abstract

This study examines the impact of performance pay on teacher selection. I exploit a shift toward performance pay in Wisconsin induced by the enactment of Act 10, which gave school districts autonomy to redesign their compensation schemes. Following the law, half of Wisconsin school districts eliminated salary schedules and started negotiating pay with individual teachers based on performance. Comparing the quantity of teaching degrees in Wisconsin institutions before and after Act 10, and relative to those in similar states, I find that Act 10 led to a 20% increase in teaching degrees. This effect was entirely driven by selective universities.

Keywords: Teachers’ Unions, New Teacher Supply, Performance Pay, Salary Schedule

JEL Classification: I20, I28, J24, J31, J45, J51

---

*I would like to thank Shawn Kantor, Carl Kitchens, Luke Rodgers, Anastasia Semykina, David Welsch, and numerous conference participants at Florida State University and other institutions for valuable comments and support. All mistakes and conclusions are my own.

†Department of Economics, Florida State University, 288 Bellamy Building, Tallahassee, FL 32306, United States. Tel.: (850) 644-5001; E-mail: ejb15c@my.fsu.edu.
1 Introduction

Teacher quality has been shown to be one of the most important school-related inputs in the education production function (Rivkin et al., 2005; Rockoff, 2004). Furthermore, teachers’ impacts on students are long-lasting. All else equal, students assigned to high-quality teachers are more likely than students assigned to low-quality teachers to go to college and attain higher earnings (Chetty et al., 2014b). The established importance of teachers to both the short- and long-run outcomes of students has led policymakers and scholars alike to explore policies that are most likely to attract qualified individuals to the teaching profession.

Despite the documented importance of teachers to student learning, little is known about policies that foster an environment whereby qualified individuals are lured into the profession. This issue has gained growing attention in recent years as school districts across the country continue to face challenges in attracting new individuals to teaching. For instance, data gathered by the ACT show that pursuing a career in education has dramatically lost popularity among ACT-tested high school students since 2008. Indeed, decreasing interest in the teaching profession is often cited as one of the main drivers of the current wave of teacher shortages impacting school districts throughout the country (Sutcher et al., 2016). Moreover, in the 2017-18 academic year, high school students who expressed interest in becoming teachers had significantly lower ACT test scores than students who expressed no interest in joining the profession (Croft et al., 2018).

While numerous policies to improve selection into teaching have been proposed, compensation reform is often at the forefront of these discussions. Teachers’ compensation in most unionized public school districts is rigid and solely based on seniority and educational attainment. Collective bargaining agreements between teachers’ unions and local school boards usually oppose performance pay and prohibit school districts from individually negotiating pay with teachers (Strunk et al., 2017; West and Mykerezi, 2011).

Critics of collectively-bargained compensation schemes, or salary schedules, argue that they fail to differentiate among teachers based on their quality or subject-area of expertise and thus may distort the ability of school districts to attract teachers in specialized fields with higher outside options (West, 2013). Additionally, salary schedules are structured as back-loaded compensation

---

1 Improving selection into teaching is clearly not the only way to improve overall teacher quality. Better professional development and termination of low-productivity teachers are among the many alternatives proposed. However, improving the quality of new teacher supply may be a more cost effective policy since it avoids the costs of exposing students to low-quality teachers and compensating incumbent teachers for the increased risk of termination (Jacob et al., 2018; Rothstein, 2015).

2 As of 2015, roughly half of elementary and secondary school teachers belonged to a union (Hirsch and Macpherson, 2015). Teachers’ unions influence public education in the United States mainly through the collective bargaining process. However, they can also influence public education through their involvement in the political arena (Cowen and Strunk, 2015). With the exception of Georgia, North Carolina, South Carolina, Texas, and Virginia, collective bargaining for teachers is allowed in every state. Generally, schools are unionized on a district-by-district basis.
systems: that is, educators are compensated for lower starting salaries with steady, certain salary raises and generous retirement benefits. Yet low starting salaries are the main reason most high school students are not interested in teaching (Croft et al., 2018).³

Alternatively, proponents of the salary schedule argue that adopting performance pay would discourage individuals from joining the profession. Previous studies have shown that individuals who select into the teaching profession are more risk averse and place a larger premium on job security than similar college graduates (Bowen et al., 2015; Rothstein, 2015). In fact, one of the biggest challenges in recruiting high-quality individuals to the teaching profession has been a perceived decline in job security (Croft et al., 2018). Given imperfect information regarding an individual’s own ability to teach, as well as the perceived inability of school districts to accurately measure teacher quality (Rothstein, 2015), uncertainty over the stream of future earnings may discourage high-quality individuals from pursuing a teaching career. Finally, proponents of the salary schedule argue that performance pay would discourage new individuals from joining the teaching profession by reducing teacher morale and collaboration as teachers would have to compete with each other for a fixed amount of resources.

Despite the pervasive debate surrounding compensation reform and its effects on new teacher recruitment, an empirical examination of this relationship has been difficult to conduct. Collective bargaining agreements between teachers’ unions and local school boards expanded dramatically during the 1960s when various states passed favorable collective bargaining legislation for government workers and remained stable until recently (Frandsen, 2016). Stable collective bargaining patterns have resulted in a lack of variation in pay practices among unionized school districts; this has impeded an empirical analysis of the effects of performance pay on new teacher supply.

This paper exploits a recent shift toward performance pay in Wisconsin to fill this gap in the literature. In 2011, in an effort to address a looming state budget deficit resulting from the Great Recession, Wisconsin enacted the Budget Repair Bill (or Act 10). This landmark law severely reduced the influence of public sector unions in the state by limiting the scope of union negotiations and restricting their fundraising abilities. As a result, school districts in Wisconsin were given full autonomy to redesign their compensation schemes. Through an examination of the impact of Wisconsin’s Act 10 on the quantity and the quality of individuals completing a teaching degree in the state, this paper seeks to shed light on the effect of compensation reform on new teacher supply.

Figure 1 shows the dramatic decline in public sector union membership in Wisconsin following the enactment of Act 10 in 2011. Roughly half of public sector employees in Wisconsin belonged to a union prior to 2011, well above the national average of 35%. Between 2011 and 2012, Wisconsin’s public sector union membership decreased from 50% to 37% and continued to plummet.

³The literature has shown that incumbent teachers also prefer an equivalent increase in current wages to the guaranteed stream of income that large deferred compensation packages offer (Fitzpatrick, 2015).
in the following years to approximately 22% in 2016. In contrast, the national average remained roughly constant at 35%. This effect was mainly driven by declines in the membership of teachers’ unions in the state. Reports from the local press indicate that the Wisconsin Education Association Council (WEAC), the state’s largest teachers’ union, experienced a decline in membership of over 50%, or 60,000 members, from 2011 to 2015.⁴

Wisconsin’s weakening of teachers’ unions brought notable changes to the teaching profession in the state. The most important provision of Act 10 for Wisconsin’s school districts was the reduction in the scope of collective bargaining. Given that the law prohibited union negotiations over compensation schemes, school districts were awarded the possibility to individually negotiate pay with teachers. Roughly half of school districts eliminated rigid salary schedule schemes and moved to negotiate salaries with individual teachers in the years following the law. Although a lot of variation arose in the way districts designed their new compensation schemes, most districts sought to tie pay to performance. Similarly, freed from union contracts that prohibited financial incentives for the recruitment of specific teachers, school districts began to offer signing bonuses and stipends to attract qualified job candidates in high-demand positions (Heneman et al., 2018).

Previous studies have shown that after Act 10, the compensation of teachers with high value-added prior to the reform rose more than did that of low-value-added teachers in school districts that switched to flexible compensation.⁵ The differences in salaries that arose among school districts after Act 10 changed teachers’ incentives to work in a given district. Following the law high-value-added incumbent teachers flowed into school districts that adopted flexible pay and tied compensation to performance, while low-value-added teachers moved to school districts that kept their rigid salary schedules (Biasi, 2018).

While responses of incumbent teachers to the law have been widely studied (Baron, 2018; Biasi, 2018; Roth, 2017; Litten, 2016), the effects of Act 10 on prospective teachers remain unexplored. However, just as they impacted the incentives of incumbent teachers, the changes triggered by Act 10 to the design of compensation schemes could fundamentally alter the decision of prospective workers to enter the teaching profession.

Examining this question presents identification challenges that I address using multiple datasets and research designs. The passage of Act 10 provides the policy-induced variation that I exploit throughout the paper. I begin by collecting state-level data on the number of enrollees in Teacher Preparation Programs (TPPs). A TPP is a state-approved course of study whose completion signifies that a student has met all educational requirements for initial certification to teach in the

---


⁵Teacher value-added has been shown to be a good measure of teacher quality (Chetty et al., 2014a). It is defined as a teacher’s contribution to the growth of students’ test scores once all other determinants of student learning have been considered.
state’s K-12 system. Using this dataset, I present descriptive evidence of changes in the number of individuals enrolled in a TPP in Wisconsin before and after Act 10 and relative to similar states.

I complement the state-level descriptive analysis with more detailed institution-level data from the Integrated Postsecondary Education Data System (IPEDS) on the number of individuals graduating with a postsecondary degree in teaching. While enrollment in TPPs is an attractive measure of immediate changes in potential new teacher supply, not all students who enroll in a TPP complete it. Therefore, complementing the enrollment measure with the number of awarded teaching degrees may help paint a broader picture regarding the effects of Act 10 on new teacher supply.

Using this dataset, I compare the number of awarded teaching degrees at Wisconsin institutions before and after Act 10 and relative to similar states in a difference-in-differences (DID) framework. I additionally estimate an event study specification to describe the dynamics of the impacts of Act 10 and to directly test whether or not Wisconsin institutions and those in control states followed similar trajectories in new teacher supply prior to the enactment of the law.

Finally, to shed light on the impacts of Act 10 on the quality of potential teachers, I examine heterogeneity in the effects of the law by institutional selectivity. I use an institution’s 25th and 75th percentiles of freshman ACT composite scores as proxies for institutional selectivity. While work attempting to link observable characteristics at the time of hire to future teacher effectiveness is still ongoing (Rothstein, 2015; Rockoff et al., 2011), proxies for the selectivity of an individual’s undergraduate institution are correlated with future teaching performance and have been widely used in the literature (Kraft et al., 2018; Jacob et al., 2018; Angrist and Guryan, 2008; Hoxby and Leigh, 2004; Figlio, 2002; Figlio and Rueben, 2001; Ehrenberg and Brewer, 1994).

In general, I find that the number of prospective teachers in Wisconsin increased as a result of Act 10. Estimates from the event study show that institutions in Wisconsin and those in control states had similar trajectories in the number of teaching degrees prior to the law. However, following the enactment of Act 10, the number of awarded teaching degrees at a given Wisconsin institution increased by roughly 20%. Exploring heterogeneity in the effects of the law by institutional quality, I find that this effect was entirely driven by the most selective universities. This finding suggests that the quality of the prospective teacher pool in Wisconsin increased as a result of the compensation reform. A number of robustness checks are conducted to ensure that the

6These findings are consistent with the literature examining how human capital investment decisions respond to shocks in the real economy. A number of studies have examined whether students are sensitive to the local labor market returns of particular college majors. Long et al. (2015) find that an increase in the wages offered by particular occupations is associated with subsequent increases in the completion of college majors associated with those occupations. Their paper provides evidence that students’ college major choices are indeed influenced by wage information that is received before or during their college career. Weinstein (2017) provides evidence that college major choice responds to local labor market conditions by showing that universities in areas more exposed to sectoral shocks experience greater changes in sector-relevant majors. For instance, he finds that after the dot-com crash, there was a sharp decline in computer science degrees at research universities in high computer employment areas, but there was no effect at research universities in areas with low computer employment shares. The main findings in this paper provide evidence
results are not sensitive to the choice of control group and specification.

This study makes three main contributions to the literature. First and most importantly, it adds to the literature examining the impact of performance pay on the teachers’ labor market. There is a large literature examining the impact of performance pay on incumbent teacher retention and on student outcomes, but not on new teacher recruitment. Research examining the impact of performance pay on student outcomes has yielded mixed results. For instance, research examining the Teacher Incentive Fund found small increases in test scores at schools that offered merit-based bonuses to teachers (Chiang et al., 2017). Studies in Minnesota (Sojourner et al., 2014) and D.C. (Adnot et al., 2017) found similar results. However, other state-specific studies in New York (Fryer, 2013) and Tennessee (Springer et al., 2011) found no evidence that performance pay improves student outcomes. While the literature examining student outcomes is mixed, studies have generally documented a positive impact of performance pay on teacher retention (Springer et al., 2016; Fulbeck, 2014). My study contributes to this literature by demonstrating that performance pay may improve the quality of the teacher workforce through a less explored channel: attracting qualified individuals to the teaching profession.

This study also contributes to the literature that examines policies affecting the quality of new teacher supply. For instance, Kraft et al. (2018) show that the repeal of teacher tenure protections across states in recent years have reduced the number of individuals completing a teaching degree at less selective universities. Figlio and Rueben (2001) show that a state-level reduction in school spending through tax limits significantly reduces the quality level of entrants into a state’s teaching force. My paper contributes to this literature by demonstrating that new high-quality individuals can be lured into the teaching profession by enacting compensation schemes that reward performance rather than seniority and years of educational attainment alone.

Finally, this paper adds to the small literature examining the impact of teachers’ unions on student outcomes. Exploiting plausibly exogenous variation in the timing of exposure to Act 10 across Wisconsin school districts, Baron (2018) shows that the law led to a large decline in student test scores in the short run. These effects were primarily driven by the sharp increase in teacher turnover that arose immediately after the reform. Even though Act 10 had negative, disruptive effects on student achievement in the short run, the present study suggests that the reform may that the education sector functions like any other labor market – an increase in the wages of high-quality teachers in Wisconsin is associated with a subsequent increase in the completion of teaching degrees at selective universities in the state.

Two notable exceptions include Rothstein (2015), who shows in a simulated labor market that performance-based compensation and retention policies can significantly improve selection into teaching when accompanied with substantial salary increases, and Jones and Hartney (2017), who find that school districts with performance-based compensation are able to recruit a larger number of teachers from more selective universities than districts with more rigid compensation schemes.

For influential papers in this literature see Brunner et al. (2018), Lovenheim and Willén (2018), Lovenheim (2009), and Hoxby (1996).
have led to an increase in the quality of prospective teachers in the state. An analysis of the effects of Act 10 on student success that incorporates these changes in new teacher supply represents a promising avenue for future research.

2 Background

2.1 Unions in Wisconsin Pre-Act 10

In 1959 Wisconsin became the first state to grant teachers’ unions the right to collectively bargain. At the time teachers’ unions served as professional and political advocates for teachers, but had no formal bargaining rights. From the time they were granted the right to collectively bargain, until 2011, teachers’ unions operated under a relatively favorable regulatory environment. Unions had the ability to collectively bargain with local school boards over issues such as working conditions and teachers’ compensation schemes.

For instance, prior to 2011 teachers’ unions opposed performance pay and bargained for seniority-based compensation. As a result, all unionized districts in Wisconsin paid their teachers according to a salary schedule, where years of experience and level of education were generally the two sole determinants of compensation. Individual-specific salary negotiations were prohibited. In other words, two teachers in a school district with the same level of education and experience would usually get paid the same amount, regardless of variation in teacher quality or subjects taught (Biasi, 2018).

2.2 Wisconsin’s Act 10

On June 29, 2011, Wisconsin enacted the Wisconsin Budget Repair Bill, or Act 10, which included a number of provisions that weakened the power of teachers’ unions in the state. First and most importantly, the law restricted the scope of collective bargaining to negotiations over inflation adjustments to total base wages for most state employees. As a result, teachers’ unions are prohibited from negotiating with local school boards over the design of teachers’ compensation schemes. Second, the law implemented a right-to-work measure that prohibited unions from deducting dues from paychecks and allowed employees to refuse to pay agency fees while remaining in the bargaining unit. Third, the Act restricted the length of collective bargaining agreements (CBAs) to one-year periods.

Fourth, the law created a new system of certification elections. Prior to Act 10, once a collective bargaining unit elected a labor organization as its representative, the organization remained the

---

9 Act 10 excluded public safety employees from its provisions.
unit’s representative unless a petition was supported for a new election to decertify. Act 10 required an annual certification election for a union to maintain legal status. If less than a majority of members in the bargaining unit vote to keep the union, then the union is decertified.

Finally, prior to the enactment of Act 10 state employees paid on average 6% of their annual health insurance premiums, and most paid little or nothing toward their pensions. Act 10 required state employees to contribute at least 12.6% of their average annual health premiums and 50% of their annual pension contributions. When considered simultaneously, it is clear that the law severely reduced the influence of teachers’ unions in Wisconsin by limiting the scope of union negotiations and altering the funding structure of union activity.

It is important to understand why a bill designed to address a budget deficit would target the influence of public sector unions. In addition to Act 10, Wisconsin imposed a significant reduction in state appropriations to school districts under the 2011-13 biennial budget known as Act 32. Act 10 and Act 32 were proposed concurrently to address the state’s looming budget deficit. The architects of Act 10 explained that by allowing school administrators to make decisions unconstrained by teachers’ unions (e.g., reduce the salary of a low-productivity tenured teacher), its provisions would allow school districts to generate enough savings to offset the decrease in state aid, which in turn would help the state reduce its deficit. In Section 4.2, I discuss in more detail whether Act 32 poses a threat to identification of the effects of Act 10 on new teacher supply.

### 2.3 Effects of Act 10 on Wisconsin’s Teacher Workforce

In order to understand how Act 10 might impact new teacher supply, it is useful to begin by describing the various changes that the reform brought to Wisconsin’s teacher workforce. The most important provision of Act 10 for Wisconsin’s school districts was the reduction in the scope of collective bargaining. Given that the law restricted the scope of collective bargaining to negotiations over inflation adjustments to base wages, school districts were awarded the possibility of individually negotiating pay with teachers.

As a result, immediately following the law roughly half of all school districts eliminated rigid salary schedule schemes and moved to negotiate salaries on an individual basis (Biasi, 2018). These changes immediately triggered significant differences in pay among teachers whose compensation would have been identical in the years prior to Act 10. While a lot of variation arose in the way districts designed their new compensation schemes, a majority of districts sought to

---

10School districts in Wisconsin derive a large part of their total revenue from two main sources: state aid and property taxes (88% in the 2014-15 academic year). Most state aid is delivered to school districts as equalization aid. Act 32 decreased funding for school districts in two ways. First, it reduced equalization aid by 8.4% in the 2011-12 academic year (relative to 2010-11). Second, it reduced each district’s revenue limits, which restrict the amount of revenue a district can raise from state aid and property taxes, by 5.5%. The reduction in revenue limits prevented school districts from simply raising school levies to make up for the loss in aid.
tie pay to teacher performance. Heneman et al. (2018) explore changes in compensation schemes in Wisconsin school districts after Act 10 in more detail and document a substantial shift toward performance-based salary raises, with performance being measured mainly by teacher performance evaluation ratings, rather than student test scores. In most school districts, performance was linked to pay in one of two ways: first, teachers were required to meet a rating of “effective” or “highly effective” in order to qualify for a pay raise. Second, the size of the pay raise was linked to the rating level, allowing for greater differentiation among teachers with different ratings. Competitive pay for teacher recruitment emerged as a widespread practice following the enactment of Act 10. Bonuses and other monetary incentives are now used to attract high-quality recruits to fill positions in high-skill and critical-shortage areas.

To demonstrate the effects of these changes on the education sector in Wisconsin, I use an individual-level dataset published annually by the Wisconsin Department of Public Instruction (WDPI). This dataset contains detailed information on the universe of Wisconsin public school teachers. It includes covariates such as each teacher’s first and last name, district and school of employment, birth year, total salary and fringe benefits, and years of teaching experience. This information allows me to construct variables such as the share of interdistrict teacher transfers and teacher compensation in Wisconsin’s public school districts from 1995-96 through 2015-16. Figure 2 presents the time series of these variables before and after 2011-12, the academic year in which Act 10 came into effect. Clear changes in the trends of these measures arise following the enactment of the law.

### 2.3.1 Teacher Salaries

Panel (a) shows that after slowly rising in the years prior to Act 10, real average teacher compensation (salary + fringe benefits) decreased following the law. The drop in teacher compensation appears to be a combination of a reduction in average teacher salaries and fringe benefits. The decline in average fringe benefits is predictable, as it was mandated by the law. Some of the decline in average salaries may simply reflect the fact that a large number of older, higher-paid teachers retired following Act 10.\(^\text{12}\)

---

11 The teacher turnover rate in year \( t \) is computed as the proportion of all teachers who in year \( t \) transferred to a new public school district. When computing aggregate variables from the individual-level dataset, I weight teachers by their corresponding full-time equivalent (FTE) units. Total salary and fringe benefits are converted to 2010 dollars using the CPI-U in the Midwest region.

12 Roth (2017) examines teacher retirements after Act 10 in more detail and argues that the law created short-run incentives for eligible teachers to retire prior to the expiration of their district’s pre-existing CBA. Act 10 required school districts to honor collectively bargained (district-provided) post-retirement benefits such as retiree health care and life insurance for teachers who retired prior to the end of the pre-existing CBA in place at their district. However, with post-retirement benefits now outside the scope of collective bargaining, continuation of these benefits was left at the discretion of the district once the pre-existing CBA expired. Given uncertainty about whether or not districts would discontinue these benefits, a large number of eligible-to-retire teachers chose to retire and secure benefits under
Indeed, while compensation may have declined on average, previous studies have documented substantial heterogeneity in compensation changes by teacher quality and experience levels. Biasi (2018) shows that after Act 10, the compensation of teachers with high-value-added prior to the reform rose more than did that of low-value-added teachers in school districts that abandoned the salary schedule and switched to more flexible, performance-based compensation. These findings demonstrate that school districts are in fact able to identify high-value-added teachers and when given the opportunity to do so choose to compensate them accordingly.

Consistent with Biasi’s findings, Litten (2016) documents a reduction in the return to tenure following Act 10. Given the ability of districts to link compensation to performance rather than to seniority and educational attainment alone, younger teachers with relatively higher value-added experienced wage increases after Act 10 while older teachers with relatively lower value-added suffered declines in compensation.

Panel (b) documents the heterogeneity in salary changes by teaching experience levels. While real average salaries have remained roughly constant for the cohorts of teachers with the most experience, real average salaries for the cohorts of teachers with the lowest levels of experience at the time the law was enacted have sharply increased. Novice teachers, those with 0-5 years of experience at the time Act 10 was enacted, have seen their salaries increase by roughly 20% from their 2010-11 levels.

2.3.2 Interdistrict Teacher Transfers

Panel (c) plots the time series of the share of interdistrict teacher transfers, a measure of teacher turnover, before and after 2011-12. The share was relatively low in the years prior to the enactment of Act 10 (around 1.5%). This was likely the result of union contracts that incentivized teachers to remain in the same school district throughout their careers with seniority-based compensation. However, the removal of incentives that tied educators to their school districts, as well as the differences in salaries that arose among school districts following Act 10, could have changed the incentives of teachers to work in a given school district.

In fact, the change in the trend in Panel (c) is clear immediately following Act 10’s implementation. The share of interdistrict transfers increased by roughly 100% in 2011-12 (relative to 2010-11) and continued to increase thereafter (to approximately 3% in 2014-15). Biasi (2018) examines post-Act 10 teachers’ movements across districts and shows that incumbent teachers

---

13Prior to Act 10, when making compensation decisions districts generally defined seniority as the number of years of teaching experience within a particular school district. Furthermore, union contracts often rejected awarding teachers full credit for years of experience gained at other school districts. These policies created strong incentives for teachers to remain in one school district throughout their careers as they risked earning lower salaries if they transferred school districts.
with high-value-added prior to Act 10 responded to the increase in salaries at school districts that adopted flexible pay and flowed into these districts.

2.3.3 Potential Effects on New Teacher Supply

Altogether, previous research has shown that following the enactment of Act 10, districts in Wisconsin were able to attract high-value-added incumbent teachers by tying compensation to performance and raising the salaries of high-value-added teachers. While responses of incumbent teachers to Wisconsin’s Act 10 have been widely studied, the effects of the law on new teacher supply remain unexplored. Yet the changes in compensation induced by Act 10 could fundamentally alter both the quantity and the quality of new individuals entering the teaching profession.

An individual will pursue a teaching major if teaching yields the highest expected utility among all potential professions. In this context, expected utility is a function of the profession’s relative expected wages, benefits, job security, and other non-pecuniary aspects of the profession such as personal satisfaction. Compensation reform can impact all inputs of the expected utility function.

A Roy model of occupational choice predicts that high-aptitude workers will be pushed out of a profession that compresses pay-for-aptitude (Roy, 1951). The intuition for this result is that salary schedules over-compensate low-quality teachers and under-compensate high-quality ones. Hoxby and Leigh (2004) test these predictions and show that the compression in wages resulting from teacher unionization during the second half of the twentieth century explains much of the decline in entry of high-aptitude women into the teaching profession. Therefore, as more districts in Wisconsin continue to enact merit-based compensation schemes new high-quality individuals may be lured into the profession.

There a number of studies showing that higher pay and performance-based compensation will improve selection in an environment that post-Act 10 Wisconsin resembles. For instance, Rothstein (2015) shows that performance pay can significantly improve selection into teaching when accompanied with substantial salary increases. Figlio (2002) finds that a school’s probability of hiring qualified teachers increases with higher teacher salaries at non-union schools but not at unionized ones, perhaps because non-unionized schools differentially reward high-quality teachers. Finally, the potential for higher starting wages stemming from the departure of the back-loaded salary schedule and the enactment of recruitment policies such as signing bonuses could incentivize qualified students to enter the teacher workforce (Heneman et al., 2018).

On the other hand, performance pay might discourage qualified individuals from entering the teaching profession. Previous studies have shown that individuals who select into the teaching profession are more risk averse and place a larger premium on job security than similar college

---

14While the cost of choosing a particular major is clearly an input in the expected utility function, it is unlikely to be affected by the union reform and is thus omitted from the discussion.
graduates (Bowen et al., 2015; Rothstein, 2015). In fact, one of the biggest challenges in recruiting high-quality individuals to the teaching profession has been a perceived decline in job security (Croft et al., 2018). Given imperfect information regarding an individual’s own ability to teach as well as the perceived inability of school administrators to accurately measure teacher quality, increased uncertainty over the stream of future earnings may discourage risk averse, highly-qualified individuals from joining the profession.

The net effect of Act 10 on both the quantity and the quality of prospective teachers will depend on whether or not the increase in compensation for high-quality teachers and the enactment of merit-based schemes outweigh the increase in risk associated with uncertainty over the stream of future income. The theoretical ambiguities described in this section highlight that the net effect of Act 10 is largely an empirical question.

3 State-Level Descriptive Evidence

Ideally, one would measure a state’s new teacher supply as the total number of fully licensed candidates that applied for a K-12 teaching position in the state for the first time. While such a measure is not publicly available, the number of individuals in a state who enroll in and complete a TPP can be used as a proxy for the number of new entrants. TPPs throughout the country generate the pool of new teachers that enter the profession each year. In most states, individuals pursuing an initial teacher certification must complete a bachelor’s degree and a state-approved TPP.

While enrollment in TPPs can be used as a proxy for new teacher supply, simply regressing the number of students in a state who are enrolled in a TPP on a measure of the state’s public schools’ reliance on performance pay schemes would likely yield a biased estimate due to omitted-variable bias. As an example, states in which individuals are particularly passionate about the teaching profession may have both a larger share of postsecondary enrollment in teaching and be more likely to have strong teachers’ unions that oppose performance pay. Ideally, to solve the endogeneity concern, one would randomly assign distinct compensation schemes to states and measure subsequent differences in TPP enrollment. Given the infeasibility of such an experiment, I instead exploit variation induced by the enactment of Wisconsin’s Act 10.

The empirical approach in this study compares the change in the number of TPP enrollees and completers in Wisconsin before and after Act 10 and relative to control states. Throughout the paper, I use three distinct sets of nearby states as control groups: states that border Wisconsin (IL, IA, MI, and MN), henceforth “border states”; states in the East North Central Division (IL, IN, MI, MN), and the remainder of the country.

15Estimates in the literature typically find that roughly 70%-90% of newly-minted teachers enter the classroom within a year of completing their teacher preparation (Sutcher et al., 2016).
and OH), henceforth “division states”; and states in the Midwest Region (IL, IN, MI, OH, MN, IA, MO, ND, SD, NE, and KS), henceforth “Midwestern states.” The map in Figure 3 depicts these control groups. Using local controls has been used in many other settings (Feigenbaum et al., 2018; Dube et al., 2010; Holmes, 1998). The intuition behind this approach is that nearby states ought to have a more similar higher education sector to Wisconsin than states in other regions of the country and thus may follow similar trends in prospective teacher supply prior to the enactment of Act 10.

I begin by presenting state-level descriptive evidence of changes in the share of postsecondary students who are enrolled in a TPP in Wisconsin before and after Act 10 and relative to the three control groups. While the quality of new teacher supply is of first-order importance, a decline in the number of students in TPPs is often cited as one of the main drivers of the current wave of teacher shortages impacting school districts throughout the country (Sutcher et al., 2016). Given the large negative impacts that teacher shortages can have on student outcomes, it is crucial to understand which types of policies either encourage or discourage students from enrolling in a TPP.

Trends in the share of postsecondary students in the state who are enrolled in a TPP are shown in Figure 4 separately for Wisconsin and the respective control groups. The figure shows that Wisconsin’s share was lower than either of the control groups and proceeded in a similar pattern in the years prior to the enactment of Act 10. The figure also shows a slower decline in enrollment in TPPs in Wisconsin relative to each control group starting in 2012, the first year under the new Act 10 regime. By the 2012-13 academic year TPP enrollment was higher in Wisconsin than in each of the control groups, and remained higher for the remainder of the sample period. This descriptive evidence begins to suggest that Wisconsin would have suffered a much sharper decline in TPP enrollment had Act 10 not been enacted.

---

16Census Regions are groupings of states that subdivide the U.S. for presentation of census data. There are four census regions: Northeast, Midwest, South, and West. Each of the four census regions is divided into two or more census divisions. Wisconsin belongs to the Midwest Region and to the East North Central Division.

17Information on the number of students who have been admitted to a TPP comes from the Department of Education (DOE) and is available from 2008-09 through 2015-16. State-level enrollment in postsecondary education is published annually by the National Center for Education Statistics (NCES).

18For instance, teacher shortages could lead to increases in class sizes which have been shown to have large, negative impacts on student achievement (Angrist and Lavy, 1999; Finn and Achilles, 1990).
4 Institution-Level Analysis

4.1 Data

To formally estimate the impact of Act 10 on the supply of prospective teachers, I complement the state-level descriptive analysis with more detailed institution-level information from IPEDS on the number of graduates from university-based TPPs.\(^\text{19}\) While enrollment in TPPs is an attractive measure of immediate changes in potential new teacher supply, not all students who enroll in a TPP complete it.\(^\text{20}\) Therefore, complementing the enrollment measure with the number of awarded postsecondary teaching degrees may help paint a broader picture regarding the effects of Act 10 on new teacher supply.

I restrict the sample to all degree-granting public and private universities located in Wisconsin and in the control states and calculate the number of awarded teaching degrees at each institution as the total number of students who graduated with a bachelor’s degree (first or second major) in teaching. To isolate teaching degrees, I use the Classification of Instructional Programs (CIP) codes used by Kraft et al. (2018).\(^\text{21}\) However, the results in the paper are robust to alternative definitions such as that used by Park et al. (2018),\(^\text{22}\) or simply using the total number of education degrees instead.\(^\text{23}\)

IPEDS additionally provides information on the total number of bachelor’s degrees awarded at each institution. This allows me to compute the share of graduates in a given institution who were awarded a teaching degree. Throughout the paper I use both the share of awarded teaching degrees and the total number of teaching degrees at each institution as the outcome variables of interest.

In order to examine heterogeneity in the impacts of Act 10 by institutional quality, I collect

---

\(^{19}\)IPEDS collects institution-level data from each postsecondary institution that participates in the federal student financial aid program. Most students pursuing a teaching degree are enrolled in university-based TPPs. For instance, in the 2012-13 academic year, only 6% of teaching candidates were enrolled in a TPP not based at a higher education institution. Source: Department of Education’s Enrollment in Teacher Preparation Programs, Informational Paper. Available at: https://title2.ed.gov/.

\(^{20}\)Estimating the number of students who complete a TPP once they have been admitted is difficult, as it requires a dataset that tracks individual cohorts over time. A survey of seven TPP providers in the Milwaukee Metropolitan Area suggests that roughly 80-90% of students who begin a TPP complete it (Yeado, 2016).

\(^{21}\)IPEDS uses six-digit CIP codes for organizing academic programs. The first two digits of a CIP code correspond to the broadest area of study (e.g., all majors under CIP 13 pertain to those within the education field. These majors include those designed to prepare individuals to become teachers, but also other majors such as “Educational Assessment, Evaluation, and Research” and “Educational Administration and Supervision”). The next two digits in the code group majors within similar instructional content and within the broader area of study. For instance, all majors under the 13.12 CIP code belong to “Teacher Education and Professional Development, Specific Levels and Methods.” The last two digits are unique to the specific major. As an example, CIP code 13.1202 corresponds to the major “Elementary Education and Teaching.” The CIP codes used by Kraft et al. (2018) are the following: 13.01, 13.02, 13.03, 13.10, 13.12, 13.13, 13.14, and 13.99.

\(^{22}\)CIP codes 13.12 and 13.13.

\(^{23}\)CIP code 13.
an institution’s 2010-11 25th and 75th percentiles of freshman ACT composite scores as proxies for institutional selectivity.\textsuperscript{24} IPEDS does not report an institution’s average freshman ACT score. Instead, it reports only the 25th and 75th percentiles of an institution’s ACT score distribution. While work attempting to link observable characteristics at the time of hire to future teacher performance is still ongoing (Jacob et al., 2018; Rothstein, 2015), proxies for the selectivity of an individual’s undergraduate institution have been widely used in the literature and have been shown to be correlated with future teaching performance (Kraft et al., 2018; Jacob et al., 2018; Angrist and Guryan, 2008; Hoxby and Leigh, 2004; Figlio, 2002; Figlio and Rueben, 2001; Ehrenberg and Brewer, 1994).

To capture potential differences in teacher demand across states, I match the institution-level dataset to time-varying, state-level covariates published by the NCES such as K-12 total state appropriations per pupil and student-teacher ratios. Furthermore, I estimate state-level teacher retirements from the Quarterly Workforce Indicators (QWI) as separations in elementary and secondary education of people older than 55 years old (the minimum early retirement age for Wisconsin public school teachers). Finally, since teacher supply may be related to overall labor market conditions through changes in relative expected wages (Nagler et al., 2015), I collect state-level economic indicators such as each state’s unemployment rate and per-capita GDP from the Bureau of Labor Statistics (BLS) and the Bureau of Economic Analysis (BEA), respectively.\textsuperscript{25}

The final sample contains a balanced panel of higher education institutions in Wisconsin and in each of the control states for the years 2004-05 through 2016-17. I exclude from the final sample states that experienced changes in the influence of their teachers’ unions at some point during the sample period.\textsuperscript{26} However, in Section 4.2 I show that the results are robust to this sample restriction.

Columns 1 and 2 of Table 1 report the pre-Act 10 means and standard deviations (in parentheses) of various observables separately for institutions in Wisconsin and those in the Midwest (excluding Wisconsin). The variables are averaged over the seven years preceding the enactment of Act 10 (2004-05 through 2010-11). Column 3 reports the point estimates and robust standard errors (in parentheses) of tests for equality of means.\textsuperscript{27}

\textsuperscript{24}The results in the paper are robust to using the institution’s acceptance rate rather than freshman ACT scores as a proxy for quality.

\textsuperscript{25}Per-capita GDP is reported in chained 2012 dollars.

\textsuperscript{26}While Wisconsin’s union reform was arguably the most dramatic both in terms of regulatory scope and political upheaval around it, since 2010 seven other states have passed legislation that weakens teachers’ unions. Idaho, Iowa, and Tennessee restricted collective bargaining. West Virginia and Missouri passed right-to-work laws in the public sector. Finally, Indiana and Michigan passed both a right-to-work law and restrictions on collective bargaining (Roth, 2017).

\textsuperscript{27}Institution-level rates are weighted by the institution’s total number of awarded degrees. State-level rates such as the state’s K-12 appropriation per pupil, student-teacher ratio, unemployment rate, and per-capita GDP are weighted by the state’s total population.
Overall, prior to the enactment of Act 10 the average Wisconsin institution had a similar FTE enrollment and number of awarded teaching degrees to that of the average Midwestern institution. Similarly, the institutions had no meaningful differences in institutional selectivity, as measured by freshman ACT test scores. The balance in these measures reinforces that Wisconsin institutions and those in the rest of the Midwest were similar prior to Act 10’s enactment. The differences in state-level variables between Wisconsin and the average Midwestern state highlight the need to control for these covariates in the empirical analysis if one wishes to isolate the supply responses of prospective teachers. Wisconsin and the average Midwestern state had large statistical differences in pre-Act 10 variables that attempt to capture overall teacher demand and economic conditions.

4.2 DID

In order to obtain an estimate of the causal impact of Act 10 on the supply of prospective teachers, I estimate Equation 1 separately for each of the three control groups. This specification compares the share of teaching degrees in Wisconsin institutions before and after Act 10 and relative to institutions in the control groups in a DID framework:

\[ Y_{ist} = \beta Act10_{ist} + X_{ist} \Theta + \mu_i + \tau_t + \epsilon_{ist} \]  

(1)

where \( Y_{ist} \) is either the share or the total number of bachelor’s degrees awarded in teaching at institution \( i \) in state \( s \) at time \( t \); \( Act10_{ist} \) is a dummy variable equal to 1 if the institution is in Wisconsin and the time period is after the enactment of Act 10 (2011-12 to 2016-17); \( X_{ist} \) is a vector of various state- and institution-level covariates that attempt to capture institutional selectivity, state-level teacher demand, and the state-level economic climate. The estimation also includes institution (\( \mu_i \)) and year (\( \tau_t \)) fixed effects. Here, institution fixed effects control flexibly for any time invariant institution-specific characteristics, while year fixed effects control for any time-varying shocks that affect all institutions in the same way. The parameter of interest is \( \beta \), which captures the change in the share of teaching degrees in Wisconsin’s higher education institutions and relative to institutions in the given control group.

The identifying assumption of the model is that were it not for Act 10, then the share of awarded teaching degrees in Wisconsin institutions and in those in the control group would have evolved in a similar way. While this assumption is inherently untestable, two necessary conditions must be met for the assumption to hold: (1) institutions in Wisconsin and in the control group are on parallel trends in the share of awarded teaching degrees prior to Act 10; and (2) there are no differential shocks (other than the enactment of Act 10) in the treatment period.

In the next section I show using an event study that the first condition is likely to hold. I focus on the second necessary condition here. While many policies around the country, particularly those
repealing tenure protections and implementing teacher evaluation reforms took place around the
time Act 10 was enacted, to the best of my knowledge Act 32 is the only policy passed concurrently
with Act 10 that could have differentially impacted Wisconsin and the control groups in the study.

For instance, Race to the Top, a competitive grant program launched by the U.S. Department
of Education in 2009 to incentivize states to adopt and experiment with performance-based evalua-
tions, did not directly impact Wisconsin during this time. The program awarded grants to winning
states in three phases, beginning with the 2009-10 academic year and through 2011-12. However,
Wisconsin was not awarded a Race to the Top Grant in any of the first three phases of the program.
Ohio is the only state in the control group who was awarded a Race to the Top Grant in August,
2010 (phase 2). Given that treatment timing in Equation 1 is defined as starting in the 2011-12
academic year, including Ohio in the control group could bias estimates of $\beta$. In Section 4.3, I
show that the estimates are robust to the exclusion of Ohio from the control group.

In terms of state-specific policies, Wisconsin is not one of the states in which state legisla-
tion/court cases restricted or repealed teacher tenure for new teachers during the sample period.
Likewise, no states in the control groups in this study experienced legislation of this kind around
the time Act 10 was enacted (Kraft et al., 2018). Thus, to the best of my knowledge, Act 32 is the
only policy that represents a threat to my identification strategy.

As mentioned in Section 2.2, Act 32 decreased school districts’ equalization aid and revenue
limits in the 2011-12 academic year. I argue that any effect of Act 32 on the share of awarded
teaching degrees would only attenuate positive estimates of $\beta$. A reduction in state aid to school
districts should, if anything, decrease both the quantity and the quality of individuals pursuing
and completing a teaching degree. For instance, Figlio and Rueben (2001) show that a state-level
reduction in school spending through tax limits significantly reduces the quality of entrants into a
state’s teaching workforce. Nevertheless, I next show that estimates of the effects of Act 10 are
robust to the inclusion of state aid to school districts as a control variable.28

The baseline results from the estimation of Equation 1 are shown in Table 2. The table presents
estimates of $\beta$ along with standard errors in parentheses and clustered at the institution level. The
table shows the robustness of the estimates across each control group and three different specifica-
tions. Each column presents estimates obtained when using a particular set of states as the control
group. In Columns 4 - 6, I add to the sample states with some kind of teachers’ union reform
during the sample period to show that the estimates are robust to their initial exclusion.

Panel (a) shows the estimates obtained when including only institution and year fixed effects
in the estimation. In Panel (b) I add time-varying institution-level covariates to the specification.
As a final robustness check, in Panel (c) I add state-level covariates that attempt to capture teacher

---

28While all results shown in the paper control for state aid linearly, the results are additionally robust to controlling
for it flexibly using cubic splines. These results are available upon request.
demand, state-level changes to public education funding, and the state’s economic conditions. In
the first row of each panel, I use as the dependent variable the share of all graduates in a given
institution who were awarded a teaching degree. In the second row I use the institution’s total
number of awarded teaching degrees instead and control for the total number of awarded degrees
on the right hand side of the equation. All regressions in which the dependent variable is the share
of awarded teaching degrees are weighted by the institution’s total number of awarded degrees to
account for the large heterogeneity in institution size.

Overall, the estimates are robust to the choice of specification and control group, and indicate
that Act 10 led to an increase in the share of awarded teaching degrees at Wisconsin institutions
of roughly 1.5 percentage points. This effect corresponds to an increase of approximately 20
additional teaching degrees, or roughly a 20% increase relative to the average number of annual
teaching degrees awarded at a given Wisconsin institution prior to Act 10.

4.3 Event Study

Using the number of graduates from university-based TPPs as an outcome variable presents diffi-
culties in defining treatment timing. Theoretically, one may expect responses in major choice to
be relatively inelastic for older cohorts (e.g., juniors and seniors). These cohorts have presumably
made costly investments toward degree completion and may not respond to the new incentives in
a meaningful way. It is likely that any effect of Act 10 on major choice would be primarily driven
by younger cohorts (e.g., freshmen and sophomores). Therefore, the effect of the law may not
manifest in the data until three or four years after its passage. The estimates of $\beta$ shown in Table 2
represent a weighted average of the effects of Act 10 by year. As a result, they provide no informa-
tion about the dynamics of the effects of the law. To uncover these effects, I estimate the following
event study specification:

$$Y_{ist} = \sum_{j \neq -1} \gamma_j D(t - 2012 = j) + \mu_i + \tau_t + X_{ist} \Theta + \epsilon_{ist}$$  \hspace{1cm} (2)$$

where $Y_{ist}$, $\mu_i$, $\tau_t$, and $X_{ist}$ are defined as in Equation 1; $D(t - 2012 = j)$ is a dummy variable that
equals one when the institution is in Wisconsin and $j$ years from 2011-12, the academic year
following the enactment of Act 10. I include six leads and five lags of the treatment effect so that
$\gamma_j$ represents the coefficient on the $j$th lead or lag, and I omit the year prior to treatment so that
all estimates are relative to this year. This specification is attractive for two main reasons. First,
a test for the parallel trends assumption is that $\gamma_j = 0$ for all $j < 0$. Rather than controlling for
differential pre-law trends across Wisconsin and the control group, this specification tests directly
for the existence of such differentials in a fully flexible way. Second, this model allows for $\gamma_j$,
\( j > 0 \) to differ by year. This allows me to describe the dynamics of the impacts of Act 10 on the share of individuals graduating with a teaching degree without assuming particular cohorts were more or less affected by the law.

Figure 5 displays the results of the estimation of Equation 2. The figure presents point estimates and 95% confidence intervals of the \( \gamma_j \)’s for each period leading to and immediately after treatment. Panel (a) presents the estimates obtained when using border states as a control group, while Panels (b) and (c) show the results obtained when using division and Midwestern states instead. In all specifications, the share of all graduates in a given institution who were awarded a teaching degree is the outcome variable.

Prior to the enactment of Act 10, all treatment estimates are statistically insignificant at the 5% level. This suggests that institutions in Wisconsin and those in the control groups had similar trajectories in the share of teaching degrees prior to the law. As expected, there appears to be a lag in the effect of Act 10 on the share of teaching degrees in Wisconsin institutions. The effect of the law is small and statistically insignificant for the first two years after its enactment, which reflects the short-run inelastic responses of older cohorts of students.

When individuals who were in earlier years of their college careers at the time of Act 10’s enactment and who had more time to respond to the increase in compensation and the enactment of performance-pay schemes begin to graduate, the effects of Act 10 become larger and statistically significant. On average, the magnitude of the estimates closely resembles that obtained from the estimation of Equation 1, and suggests that the Act led to an increase in the share of awarded teaching degrees at Wisconsin institutions of roughly 1.5 percentage points.

As a final robustness check, Figure 6 displays the estimates obtained when dropping the state of Ohio from the “division states” and the “Midwestern states” control groups. As mentioned above, Ohio was awarded a Race to the Top Grant in August, 2010. This could violate the identifying assumption of no differential shocks between the treatment and control groups during the treatment period. The estimates when excluding Ohio from the control groups are nearly identical to those in Figure 5, alleviating concerns that differential shocks during the treatment period, rather than the enactment of Act 10, may be driving the main results.

4.4 Heterogeneity by Institutional Quality

All else equal, students assigned to high-quality teachers are more likely than students assigned to low-quality teachers to go to college and attain higher earnings (Chetty et al., 2014b). Therefore, while an analysis of the impact of Act 10 on the number of prospective teachers is important due to the current wave of teacher shortages throughout the U.S., the question of first-order importance is how compensation reform impacts the quality of the teacher pipeline.
To shed light on this question, I examine heterogeneity in the effects of Act 10 by institutional selectivity. Specifically, I estimate Equation 3 separately for each of the three control groups:

\[ Y_{ist} = \beta_{Act10_{i}} + \delta (Act10_{i} \times ACT_{is}) + X_{ist} \Theta + \mu_i + \tau_t + \nu_{ist} \]  

(3)

where \( Y_{ist}, \mu_i, \tau_t, \) and \( X_{ist} \) are defined as in Equation 1; \( ACT_{is} \) is either the institution’s 2010-11 25th or 75th percentile of freshman ACT scores.\(^{29}\) IPEDS does not report an institution’s average freshman ACT score. Instead, it reports only the 25th and 75th percentiles of an institution’s ACT score distribution. While the results are very similar using either percentile, throughout this section I present estimates of Equation 3 separately for each measure. I standardize both the 25th and the 75th percentile scores to a mean of zero and a standard deviation of 1 using the distribution of scores of the relevant control group. For instance, when estimating Equation 3 using Midwestern states as the control group, I standardize the scores using the distribution of scores of institutions in the Midwest.

In this specification the effect of Act 10 on the institution’s share of teaching degrees depends on \( ACT_{is} \) linearly and is represented by the expression \( \beta + \delta ACT_{is} \). Therefore, \( \beta \) measures the impact of Act 10 at institutions with an ACT score in the middle of the distribution. \( \delta \) measures how the effect of Act 10 changes with institutional selectivity. A positive estimate of \( \delta \) indicates that the effect of Act 10 on new teacher supply is more positive for institutions with higher freshman ACT scores.

Table 3 presents estimates of \( \beta \) and \( \delta \) separately for each control group. The first three columns present the estimates obtained when using an institution’s 25th percentile of freshman ACT scores, while the last three columns show the results obtained when using the 75th percentile instead. All estimates of \( \delta \) are positive and statistically significant, indicating that institutions with higher freshman ACT scores experienced relatively larger increases in the share of teaching degrees as a result of Act 10.

Figures 7 and 8 plot estimates of \( \beta + \delta ACT_{is} \) for a range of values of \( ACT_{is} \) and separately for each control group. The figures trace out estimates of the effect of Act 10 on the share of awarded teaching degrees, \( \beta + \delta ACT_{is} \), for values of \( ACT_{is} \) between three standard deviations below and above the mean. For instance, the leftmost point of the line shows estimates of \( \beta - 3\delta \), the effect of Act 10 on the share of awarded teaching degrees at institutions with freshman ACT scores of three standard deviations below the mean. The solid line traces out the point estimates while the dashed line delineates the corresponding 95% confidence intervals.

The results in the figures show that the increase in the share of teaching degrees brought about

\(^{29}\)The results are robust to choosing other pre-Act 10 ACT reporting years. They are additionally robust to using the institution’s acceptance rate rather than freshman ACT scores as a proxy for quality.
by Act 10 was entirely driven by relatively more selective institutions. For instance, using any control group, the figures provide no evidence that institutions with a less-than-average pre-reform ACT score experienced a change in the share of teaching degrees as a result of the reform. While most of the coefficient estimates are negative, they are not statistically significant. However, the figures show that institutions with freshman ACT scores of one or more standard deviations above the mean experienced increases in the share of teaching degrees of roughly 2 percentage points.

A caveat of these results, however, is that the specification in Equation 3 assumes the effect of Act 10 is linear in \( ACT_{is} \). To relax this assumption, I complement the analysis by estimating the following equation:

\[
Y_{ist} = \beta_1 Act_{10st} + \beta_2 (Act_{10st} \times Q_{2is}) + \beta_3 (Act_{10st} \times Q_{3is}) + \beta_4 (Act_{10st} \times Q_{4is}) + X_{ist} \Theta + \mu_i + \tau_t + \nu_{ist}
\]

where \( Y_{ist}, \mu_i, \tau_t, X_{ist}, \) and \( ACT_{is} \) are defined as in Equation 3; \( Q_2, Q_3, Q_4 \) are dummy variables indicating which quartile of the distribution of 2010-11 ACT composite scores the institution is in. As with Equation 3, I estimate this specification separately for the 25th and 75th percentile scores and for each control group. Thus, when using the 25th percentile, \( Q_2 \) is equal to 1 if institution \( i \) is in the second quartile of the distribution of 25th percentile ACT scores. In this specification \( \beta_1 \) measures the impact of Act 10 on the share of teaching degrees at the least selective institutions (those with an ACT score in the first quartile of the distribution). \( \beta_i, i \in 2, 3, 4 \) captures the differential impact of Act 10 on the share of teaching degrees at institutions with ACT scores in the \( i \)th quartile of the distribution (relative to those in the first quartile).

Panel (a) of Table 4 presents estimates of \( \beta_1, \beta_2, \beta_3, \) and \( \beta_4 \) separately for each control group and using either the 25th or 75th percentile of ACT scores. In all specifications, \( \hat{\beta}_4 \) is positive and statistically significant. These results indicate that the impact of Act 10 on the share of teaching degrees is relatively more positive for the most selective institutions than for the least selective ones.

Panel (b) presents estimates of linear combinations of the coefficients. These linear combinations reveal the average effect of Act 10 at institutions in different parts of the ACT score distribution. For instance, the fourth row presents estimates of \( \beta_1 + \beta_4 \), the effect of Act 10 on the most selective institutions. The estimates in Panel (b) indicate that the average effect of Act 10 on the share of teaching degrees is entirely driven by institutions in the fourth quartile of the distribution. There is no evidence that Act 10 had an impact on the share of teaching degrees at less selective institutions. These results are consistent with those obtained from the estimation of Equation 3.

Altogether, the results in this section suggest that the individuals who selected into a teaching major under the new Act 10 incentives were those at relatively more selective institutions. These
results are suggestive of an increase in the quality of the prospective teacher pool in Wisconsin as a result of Act 10. The estimates also suggest that the increase in compensation for high-quality teachers, as well as the departure from salary schedules that over-compensate low-quality teachers and under-compensate high-quality ones, outweighed the increase in risk associated with performance-based pay and incentivized high-quality individuals to select into the teaching profession.

5 Conclusion

Despite the pervasive debate surrounding performance pay and its impacts on new teacher recruitment, a rigorous examination of this topic has been difficult to conduct due to a lack of variation in public sector unionism. Exploiting recent variation in union strength induced by the enactment of Wisconsin’s Act 10, a measure that severely reduced the influence of teachers’ unions in the state and gave school districts the freedom to redesign teacher compensation schemes, this study aims to fill this gap in the literature.

Comparing the quantity of individuals completing a teaching degree in Wisconsin institutions before and after Act 10 and relative to those in similar states, I find that Act 10 led to a 20% increase in the number of awarded teaching degrees. This effect was entirely driven by the most selective institutions, which suggests an increase in the quality of the prospective teacher pool in Wisconsin as a result of the compensation reform.

A few words of caution are in order. First, using the number of graduates from university-based TPPs is far from a perfect measure of new teacher supply. Not all graduates from TPPs seek to obtain a job in the public education system. Some may decide to teach in private schools or work outside of the education sector altogether. Furthermore, even if graduates do wish to enter the public education system, they may seek to teach in a different state than the one in which they completed their preparation. Therefore, while my results suggest that the quantity of individuals graduating from Wisconsin institutions with a teaching degree increased as a result of Act 10, they do not necessarily imply that the quantity of new teachers in the state increased as well.

Second, teacher quality is subjective and unobservable. Even though proxies for teacher quality such as the selectivity of an individual’s undergraduate institution have been widely used in the literature and have been shown to be correlated with future teacher effectiveness, they are imperfect measures. The literature has yet to reach a consensus regarding which traits of individuals at the time of hire fully capture future teacher quality. The findings in this study regarding the impacts of Act 10 on the quality of prospective new teachers should thus be interpreted with caution.

With these weaknesses in mind, the present study has important policy implications. The findings of this and other recent studies have begun to provide a clearer understanding of the effects of
teacher compensation reform on both the labor market for teachers and student outcomes. Similar to the findings of Biasi (2018) for incumbent teachers in Wisconsin, the results here indicate that new high-quality individuals can be lured into school districts with compensation schemes that reward performance rather than seniority and educational attainment alone.

Understanding whether the increase in the quality of prospective new teachers as a result of Act 10 translates into improvements in student outcomes in Wisconsin is an important question for future research. While Baron (2018) shows that Act 10 led to a decrease in student test scores in the short run, an analysis of the long-run effects that arise after Act 10’s initial shock remains to be done. The present study provides evidence that the general equilibrium effects of Act 10 on student outcomes, which will depend on its effect on the supply of new teachers, might differ substantially from its short-run disruptive effects.
References


Figure 1: Effects of Act 10 on Wisconsin’s Union Membership

Figure shows the dramatic decline in public sector union membership in Wisconsin following the enactment of Act 10 in 2011. Roughly half of public sector employees in Wisconsin belonged to a union prior to 2011, well above the national average of 35%. Between 2011 and 2012, Wisconsin’s public sector union membership decreased from 50% to 37% and continued to plummet in the following years to approximately 22% in 2016. In contrast, the national average remained roughly constant at 35% (Hirsch and Macpherson, 2017).
Figure 2: Changes in the Teaching Profession Around Act 10

(a) Teacher Compensation

(b) Teacher Salaries by Experience

(c) Interdistrict Teacher Transfers

Figure presents the time series of the share of interdistrict teacher transfers, average teacher compensation, and teacher salaries by experience level in Wisconsin’s public school districts before and after 2011-12, the academic year following the enactment of Act 10. Data come from the WDPI. The teacher turnover rate in year $t$ is computed as the proportion of all teachers who in year $t$ transferred to a new public school district. When computing aggregate variables from the individual-level dataset, I weight teachers by their corresponding FTE units. Total salary and fringe benefits are converted to 2010 dollars using the CPI-U in the Midwest region.
Figure shows the three distinct sets of nearby states that I use throughout the paper as control groups. The first control group is border states (IL, IA, MI, and MN). The second control group consists of states in the East North Central Division (IL, IN, MI, and OH). Finally, states in the Midwest Region (IL, IN, MI, OH, MN, IA, MO, ND, SD, NE, and KS) comprise the third control group.
The figure shows trends in the share of postsecondary students who are enrolled in a TPP in Wisconsin before and after Act 10 and relative to the three control groups. Information on the number of students who have been admitted to a TPP comes from the Department of Education (DOE) and is available from 2008-09 through 2015-16. State-level enrollment in postsecondary education is published annually by the National Center for Education Statistics (NCES).
Figures present point estimates and 95% confidence intervals of the $\gamma_j$'s for each period leading to and immediately after treatment. The year prior to treatment is omitted so that all estimates are relative to this year. The model is estimated separately for each of the three control groups. Standard errors used in the construction of the confidence intervals are clustered at the institution level. All specifications are weighted by the institution’s total number of awarded degrees.
Figures present point estimates and 95% confidence intervals of the $\gamma_j$'s for each period leading to and immediately after treatment. The year prior to treatment is omitted so that all estimates are relative to this year. The model is estimated separately for each of the three control groups. Standard errors used in the construction of the confidence intervals are clustered at the institution level. All specifications are weighted by the institution's total number of awarded degrees.
Figure 7: Heterogeneity by Quality (ACT 25th Percentile)

The figure plots estimates of $\beta + \delta ACT_{25}$ obtained from the estimation of Equation 3 for distinct values of $ACT_{25}$ and separately for each control group. I perform a z-score transformation of the scores to aid interpretation of the results. The figure traces out estimates of the effect of Act 10 on the share of awarded teaching degrees, $\beta + \delta ACT_{25}$, for values of $ACT_{25}$ between three standard deviations below and above the mean. As an example, the leftmost point of the line shows estimates of $\beta - 3\delta$, the effect of Act 10 on the share of awarded teaching degrees at institutions with 25th percentile ACT scores that are three standard deviations below the mean. The solid line traces out the point estimates while the dashed line delineates the corresponding 95% confidence intervals. All specifications are weighted by the institution’s total number of awarded degrees. Standard errors used in the construction of the confidence intervals are clustered at the institution level.
Figure 8: Heterogeneity by Quality (ACT 75th Percentile)

(a) Border States

(b) Division States

(c) Midwest States

The figure plots estimates of $\beta + \delta ACT_{is}$ obtained from the estimation of Equation 3 for distinct values of $ACT_{is}$ and separately for each control group. I perform a z-score transformation of the scores to aid interpretation of the results. The figure traces out estimates of the effect of Act 10 on the share of awarded teaching degrees, $\beta + \delta ACT_{is}$, for values of $ACT_{is}$ between three standard deviations below and above the mean. As an example, the leftmost point of the line shows estimates of $\beta - 3\delta$, the effect of Act 10 on the share of awarded teaching degrees at institutions with 75th percentile ACT scores that are three standard deviations below the mean. The solid line traces out the point estimates while the dashed line delineates the corresponding 95% confidence intervals. All specifications are weighted by the institution’s total number of awarded degrees. Standard errors used in the construction of the confidence intervals are clustered at the institution level.
Table 1: Pre-Act 10 Summary Statistics

<table>
<thead>
<tr>
<th>Institution-Level Variables</th>
<th>Midwest</th>
<th>Wisconsin</th>
<th>Differences</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Dependent Variables</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Share of Teaching Degrees</td>
<td>10.16</td>
<td>8.20</td>
<td>1.96***</td>
</tr>
<tr>
<td>(7.74)</td>
<td>(5.03)</td>
<td>(0.68)</td>
<td></td>
</tr>
<tr>
<td>Number of Teaching Degrees</td>
<td>104.66</td>
<td>97.87</td>
<td>6.79</td>
</tr>
<tr>
<td>(144.41)</td>
<td>(88.73)</td>
<td>(7.52)</td>
<td></td>
</tr>
<tr>
<td><strong>Institution Features</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Number of Total Degrees</td>
<td>1,030.07</td>
<td>1,193.62</td>
<td>-163.55</td>
</tr>
<tr>
<td>(1,436.64)</td>
<td>(1,521.35)</td>
<td>(113.63)</td>
<td></td>
</tr>
<tr>
<td>Total FTE Enrollment</td>
<td>5,777.48</td>
<td>6,179.91</td>
<td>-402.43</td>
</tr>
<tr>
<td>(8,466.68)</td>
<td>(7,322.24)</td>
<td>(566.53)</td>
<td></td>
</tr>
<tr>
<td>ACT 25th Percentile</td>
<td>21.75</td>
<td>21.73</td>
<td>0.02</td>
</tr>
<tr>
<td>(2.94)</td>
<td>(2.84)</td>
<td>(0.43)</td>
<td></td>
</tr>
<tr>
<td>ACT 75th Percentile</td>
<td>26.70</td>
<td>26.23</td>
<td>0.47</td>
</tr>
<tr>
<td>(2.63)</td>
<td>(2.55)</td>
<td>(0.38)</td>
<td></td>
</tr>
<tr>
<td>Acceptance Rate</td>
<td>72.20</td>
<td>77.35</td>
<td>-5.16***</td>
</tr>
<tr>
<td>(13.08)</td>
<td>(11.36)</td>
<td>(1.45)</td>
<td></td>
</tr>
<tr>
<td><strong>State-Level Variables</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Education System Features</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>State Aid per Pupil ($)</td>
<td>4,871.16</td>
<td>5,785.83</td>
<td>-914.66***</td>
</tr>
<tr>
<td>(1,441.10)</td>
<td>(195.42)</td>
<td>(46.78)</td>
<td></td>
</tr>
<tr>
<td>Student-Teacher Ratio</td>
<td>15.64</td>
<td>14.76</td>
<td>0.89***</td>
</tr>
<tr>
<td>(0.74)</td>
<td>(0.25)</td>
<td>(0.03)</td>
<td></td>
</tr>
<tr>
<td>Number of Teacher Retirements</td>
<td>24,730.54</td>
<td>18,002.86</td>
<td>6,727.68***</td>
</tr>
<tr>
<td>(13,159.21)</td>
<td>(1,472.92)</td>
<td>(411.06)</td>
<td></td>
</tr>
<tr>
<td><strong>Economic Conditions</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Unemployment Rate</td>
<td>7.07</td>
<td>6.34</td>
<td>0.74***</td>
</tr>
<tr>
<td>(2.27)</td>
<td>(1.76)</td>
<td>(0.15)</td>
<td></td>
</tr>
<tr>
<td>Per-Capita GDP ($)</td>
<td>51,224.98</td>
<td>46,994.83</td>
<td>4,230.16***</td>
</tr>
<tr>
<td>(4,151.62)</td>
<td>(787.58)</td>
<td>(158.56)</td>
<td></td>
</tr>
</tbody>
</table>

Columns 1 and 2 report the pre-Act 10 means and standard deviations (in parentheses) of various observables separately for institutions in Wisconsin and those in the average Midwestern state (excluding Wisconsin). Column 3 reports the point estimates and robust standard errors (in parentheses) of tests for equality of means. Institution-level rates are weighted by the institution’s total number of awarded degrees. State-level rates such as the state’s aid per pupil, student-teacher ratio, unemployment rate, and per-capita GDP are weighted by the state’s total population. Per-capita GDP is reported in chained 2012 dollars.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Table 2: Estimates of Equation 1

<table>
<thead>
<tr>
<th></th>
<th>Excluding Reform States</th>
<th>Including Reform States</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td><strong>Panel (a): Institution and Year Fixed Effects</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Share of Teaching Degrees</td>
<td>1.203***</td>
<td>1.179***</td>
</tr>
<tr>
<td></td>
<td>(0.603)</td>
<td>(0.539)</td>
</tr>
<tr>
<td>Number of Teaching Degrees</td>
<td>17.120**</td>
<td>20.199***</td>
</tr>
<tr>
<td><strong>Panel (b): Institutional Features</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Share of Teaching Degrees</td>
<td>1.362***</td>
<td>1.423***</td>
</tr>
<tr>
<td></td>
<td>(0.432)</td>
<td>(0.492)</td>
</tr>
<tr>
<td>Number of Teaching Degrees</td>
<td>17.638**</td>
<td>20.517***</td>
</tr>
<tr>
<td></td>
<td>(7.009)</td>
<td>(7.013)</td>
</tr>
<tr>
<td><strong>Panel (c): State-Level Covariates</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Share of Teaching Degrees</td>
<td>1.672***</td>
<td>1.719***</td>
</tr>
<tr>
<td></td>
<td>(0.435)</td>
<td>(0.454)</td>
</tr>
<tr>
<td>Number of Teaching Degrees</td>
<td>22.641***</td>
<td>24.946***</td>
</tr>
<tr>
<td></td>
<td>(6.850)</td>
<td>(7.120)</td>
</tr>
</tbody>
</table>

Pre-Reform Average Share of Teaching Degrees in WI 8.20 8.20 8.20 8.20 8.20 8.20
Pre-Reform Average Number of Teaching Degrees in WI 97.87 97.87 97.87 97.87 97.87 97.87
Control Group Border Division Midwest Border Division Midwest
Years 2005-17 2005-17 2005-17 2005-17 2005-17 2005-17
N 1,313 1,456 2,418 2,041 2,327 4,017

Table presents estimates of $\beta$ along with standard errors in parentheses and clustered at the institution level. The table shows the robustness of the estimates across each control group and three different specifications. In Columns 4 - 6, I add to the sample states with some kind of teachers’ union reform during the sample period. Each column presents the results obtained when using a particular set of states as the control group. Panel (a) shows the estimates obtained when including only institution and year fixed effects in the estimation. In Panel (b) I add time-varying institution-level covariates to the specification. As a final robustness check, in Panel (c) I add state-level covariates that attempt to capture teacher demand, state-level changes to public education funding, and the state’s economic conditions. In the first row of each panel, I use as the dependent variable the share of all graduates in a given institution who were awarded a teaching degree. In the second row I use the institution’s total number of awarded teaching degrees instead and control for the total number of awarded degrees on the right hand side of the equation. All regressions in which the dependent variable is the share of awarded teaching degrees are weighted by the institution’s total number of awarded degrees to account for the large heterogeneity in institution size. $^*$ $p < 0.10$, $^{**} p < 0.05$, $^{***} p < 0.01$. 
Table 3: Estimates of Equation 3

<table>
<thead>
<tr>
<th>Independent Variable (Coefficient)</th>
<th>25th Percentile</th>
<th>75th Percentile</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td>Act 10 (β)</td>
<td>0.902</td>
<td>0.884*</td>
</tr>
<tr>
<td></td>
<td>(0.578)</td>
<td>(0.511)</td>
</tr>
<tr>
<td>Act 10 × ACT (δ)</td>
<td>0.665***</td>
<td>0.701***</td>
</tr>
<tr>
<td></td>
<td>(0.165)</td>
<td>(0.174)</td>
</tr>
<tr>
<td>Control Group</td>
<td>Border</td>
<td>Division</td>
</tr>
<tr>
<td>Years</td>
<td>2005-17</td>
<td>2005-17</td>
</tr>
<tr>
<td>N</td>
<td>1,313</td>
<td>1,456</td>
</tr>
</tbody>
</table>

Table presents estimates of $\beta$ and $\delta$ along with standard errors in parentheses and clustered at the institution level. Each column presents the estimates obtained when using a particular control group. The first three columns present estimates obtained when using an institution’s 25th percentile of freshman ACT scores, while the last three columns show the results of the estimation when using the 75th percentile instead. All estimates of $\delta$ are positive and statistically significant, indicating that institutions with higher freshman ACT scores experienced relatively larger increases in the share of teaching degrees as a result of Act 10. All specifications are weighted by the institution’s total number of awarded degrees.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. 
**Table 4: Estimates of Equation 4**

<table>
<thead>
<tr>
<th></th>
<th>25th Percentile</th>
<th>75th Percentile</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td><strong>Panel (a): Individual Coefficients</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>(Act 10 (\beta_1))</td>
<td>0.503</td>
<td>0.480</td>
</tr>
<tr>
<td></td>
<td>(0.766)</td>
<td>(0.717)</td>
</tr>
<tr>
<td>(Act 10 \times Q2 (\beta_2))</td>
<td>0.224</td>
<td>0.222</td>
</tr>
<tr>
<td></td>
<td>(0.805)</td>
<td>(0.804)</td>
</tr>
<tr>
<td>(Act 10 \times Q3 (\beta_3))</td>
<td>0.127</td>
<td>0.125</td>
</tr>
<tr>
<td></td>
<td>(0.736)</td>
<td>(0.736)</td>
</tr>
<tr>
<td>(Act 10 \times Q4 (\beta_4))</td>
<td>1.568 ***</td>
<td>1.567 ***</td>
</tr>
<tr>
<td></td>
<td>(0.591)</td>
<td>(0.592)</td>
</tr>
<tr>
<td><strong>Panel (b): Effects of Act 10 by Quartile</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>First Quartile (\beta_1)</td>
<td>0.503</td>
<td>0.480</td>
</tr>
<tr>
<td></td>
<td>(0.766)</td>
<td>(0.717)</td>
</tr>
<tr>
<td>Second Quartile (\beta_1 + \beta_2)</td>
<td>0.727</td>
<td>0.702</td>
</tr>
<tr>
<td></td>
<td>(0.759)</td>
<td>(0.708)</td>
</tr>
<tr>
<td>Third Quartile (\beta_1 + \beta_3)</td>
<td>0.630</td>
<td>0.125</td>
</tr>
<tr>
<td></td>
<td>(0.687)</td>
<td>(0.631)</td>
</tr>
<tr>
<td>Fourth Quartile (\beta_1 + \beta_4)</td>
<td>2.071 ***</td>
<td>2.048 ***</td>
</tr>
<tr>
<td></td>
<td>(0.529)</td>
<td>(0.454)</td>
</tr>
<tr>
<td>Control Group</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Years</td>
<td>2005-17</td>
<td>2005-17</td>
</tr>
<tr>
<td>N</td>
<td>1,313</td>
<td>1,456</td>
</tr>
</tbody>
</table>

Table presents the results obtained from the estimation of Equation 4. Panel (a) presents estimates of \(\beta_1, \beta_2, \beta_3, \text{ and } \beta_4\). In this specification, \(\beta_1\) measures the impact of Act 10 on the share of teaching degrees at the least selective institutions (those with a pre-reform ACT score in the first quartile of the distribution). \(\beta_i, i \in 2, 3, 4\) captures the differential impact of Act 10 on the share of teaching degrees at institutions with ACT scores in the \(i\)th quartile of the distribution (relative to those in the first quartile). Panel (b) presents estimates of linear combinations of the coefficients. These linear combinations reveal the effect of Act 10 on institutions in different parts of the pre-reform ACT score distribution. For instance, the fourth row presents estimates of \(\beta_1 + \beta_4\), the effect of Act 10 on the most selective institutions - those in the fourth quartile of the ACT score distribution. All specifications are weighted by the institution’s total number of awarded degrees. Standard errors are clustered at the institution level.

\(^* p < 0.10, \,** p < 0.05, \,** * p < 0.01.\)