The Effect of Changes in Legal Institutions Weakening Teachers' Unions on Districts' Spending on Teacher Compensation

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Purpose: The unanticipated changes in state legislation in Idaho, Indiana, Michigan, Tennessee, and Wisconsin in 2011–12 significantly restricted or entirely prohibited the collective bargaining rights of teachers. Considering these institutional changes as a natural experiment, we examine the causal impact of weakening teacher unionization on districts' spending on teacher compensation. **Research Methods/Approach:** We merge two nationally representative data sets from the United States: the Local Education Agency (School Districts) Finance Survey (F-33) and the Stanford Education Data Archive (SEDA) in 2009–16. We identify the effect of the institutional changes regarding teachers' unions by employing a difference-in-difference estimation and synthetic control method, exploiting district-level national data on spending on teacher compensation. **Findings:** We find that the antiunion institutional changes substantially reduced districts' spending on both teacher salaries and benefits. The negative impact is larger for the districts located at the bottom of the distribution of spending than for districts at the top. **Implications:** Our study suggests that the antiunion legal changes will raise income inequality among teachers, and the increased inequality in compensation among teachers may translate into greater performance gaps between students, if teachers receiving lower compensation are more likely to quit teaching or to move to districts that pay more. Therefore, the negative effects of the antiunion legal changes be even greater in the long run if the current trends persist.

Legal environments and labor laws play important roles in the labor market for public sector workers, including public school teachers. For instance, whether collective bargaining (CB hereafter) is mandated, legal, or prohibited, whether

unions are permitted to collect "fair share" agency fees from non-union workers, and whether public sector employees are allowed to strike contribute to shaping the work environment and labor-market outcomes in the public sector (Hanushek 2020; Winkler et al. 2012). In particular, legal institutions for teachers' unions lay the framework for unions to influence teachers' well-being and the educational landscape by affecting the level of revenue available to each district and how districts allocate their educational spending (Cowen and Strunk 2015; Han 2019, 2020; Jones et al. 2019; Moe 2011; Moore-Johnson et al. 2007).

Historically, the legal institutions related to teachers' unions have been fairly stable since the 1960s, when state laws established governments' obligation to bargain with public sector employees, although a few additional changes to teachers' CB were made in the late 1980s (Paglayan 2019). In recent periods, however, several states have experienced significant alterations in their long-standing legislation regarding the environment in which public sector unions can operate. In 2011–12, state legislators in Idaho, Indiana, Michigan, Tennessee, and Wisconsin launched unprecedented initiatives drastically restricting or entirely prohibiting the CB rights of public school teachers. The legal changes in these five states were partly motivated as an attempt to fix budget problems in the aftermath of the Great Recession in 2008 and seem partly derived from major changes in the political dominance of the states' legislatures.[1] In spite of the commonalities of substantial changes in state legislation, it is still not well established how these union reforms that altered the legal and institutional landscapes in those five states impacted districts' spending on teacher compensation, which is the largest portion of school spending (National Center for Education Statistics [NCES] 2018). The lack of solid empirical evidence on this issue presents policy makers and educators with a challenge in determining which course of action to choose in improving the quality of public education and educational efficiency with limited resources.

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In this study, we examine the causal impact of changes in legal institutions that curb the bargaining power of teachers' unions on a district's spending for teacher compensation, using district-level national data from 2009 through 2016. We focus on the demand side of the teacher labor market by examining how districts (employers) respond to the changes in state legislation that hamper the bargaining power of their teachers (employees). The research questions we attempt to answer are the following: Do changes in legal institutions limiting the bargaining rights of teachers' unions influence districts' spending on teacher compensation? If so, what are the magnitudes of the impacts of such antiunion reforms? How do the impacts of changes in legal institutions differ by districts' characteristics?

According to Freeman and Han (2013), the changes in legal institutions in four states (Idaho, Indiana, Tennessee, and Wisconsin) significantly reduced the strength of unions for teachers and other public sector employees. Han (2020) finds that, on average, the CB coverage and union membership rate of public school teachers dropped by approximately 20 and 10 percentage points, respectively, after the changes in the state legislation, and the new antiunion laws shifted the teacher turnover pattern in those states. This substantial drop in union strength sets the institutional and environmental backdrop of our study. Based on similar natural experiments (the four states mentioned plus Michigan), we identify with two approaches the effect of the changes in legal institutions diminishing union strength on teacher compensation. We first employ a difference-in-difference (DID) estimation, using the treatment group composed of the five states and a comparison group including all other states. We then use a synthetic control method as a supplement to better address the heterogeneity among the comparison states and select a more comparable comparison group.

We benefit from the existence of the Local Education Agency (School Districts) Finance Survey, which we combine with data from the Stanford Education Data Archive (SEDA) for 2009 through 2016. The finance data contain key variables of districts' expenditure for teacher pay, and the SEDA provides information on students and their schools, along with descriptive information on the characteristics of families with school-age children residing in each district.

The Supreme Court's ruling in *Janus v. American Federation of State, County and Municipal Employees* (No. 16-1466, 2018; *Janus* hereafter), which eliminated public-sector unions' ability to collect agency fees from non-union workers (McNicholas 2018; Rosales 2018), has created a more hostile climate toward teachers' unions. In addition, as the US economy recovers from the COVID-19 recession, most funds from the federal relief and recovery acts are expiring. Thus, many districts face decreased education funding in the upcoming school years even as they have utilized more resources to support the learning recovery of students (Baker and Di Carlo 2020). Therefore, understanding how spending cuts in public education influence districts' decisions to allocate scarce resources and to budget for teacher compensation is critically important. Our study

conducts a rigorous causal analysis for the case of changes in legal institutions in the educational sector that may lead to an alteration in districts' spending patterns. Therefore, our study tackles a timely issue, and the findings in this study offer predictions for the repercussions of reduced educational spending.

Literature

Teachers' Unions, Educational Spending, and Teacher Compensation

Several strands of research contribute to framing our understanding of the role of legal and institutional environments in influencing educational spending directly and indirectly through teachers' unions. Most well known is the literature that has shown a positive relationship between unions and district spending and between unions and teacher outcomes. These studies tend to offer a somewhat narrow perspective by comparing unionized versus non-unionized settings without referring to the various legal environments related to public sector unions (Eberts and Stone 1984; Hoxby 1996; West 2015).

Studies that find a positive link between teachers' unions and district spending can be roughly categorized into two main branches. The first view claims that unions work as rent-seekers in the market for educational production by increasing school budgets, though they bring no gains to students and reduce input productivity (Cook et al. 2021; Cowen and Strunk 2015; Hoxby 1996; Lott and Kenny 2013; Lovenheim 2009; Moe 2011; Whitehurst et al. 2015). For example, Hoxby (1996) argues that teachers' unions extract rent from school districts by raising educational inputs through negotiating higher salaries and better working conditions, but she finds that teachers' unions have negative effects on student outcomes, measured by high school dropout rates. Lott and Kenny (2013) find that students in states with strong unions, identified by high union dues and spending, have lower test scores than students in states with low dues and spending. Brunner et al. (2020) show that in states with stronger unions, increases in state aid lead to significantly higher education expenditures in districts, which are primarily allocated to teacher compensation, whereas in states with weaker unions, in exchange for increases in state aid, districts reduce local taxing and spend the remaining funds on hiring new teachers.

The second view also sees that unions increase resources and inputs but claims that unions raise input productivity and teacher quality. In this case, unions, in collaboration with management, support strategies such as the promotion of teaching quality via improving the professionalism of teaching, the rigorous evaluation of teachers, and the encouragement of staffing in hard-to-attract schools that would result in improved teaching quality, more effective schools,

and higher student learning (Han 2020; Jones et al. 2019; Moore-Johnson 2009; Moore-Johnson et al. 2007; National Education Association Foundation 2012).

Researchers also look at the restrictiveness of CB and find that contract strength is associated with union strength and with higher district spending, but there is no consensus regarding how the increased spending is allocated to teachers. Eberts and Stone (1984) find that districts with higher contract strength spend more money on instruction, benefits, and average salaries. In contrast, Strunk (2011) shows that districts with more restrictive CB have greater overall expenditures, but that this higher spending is not derived from the increased allocation of resources to teachers' salaries or benefits.

Literature has long found that teachers' unions play an important role in raising teachers' compensation, not just overall school spending. Reviews of earlier literature show that teachers' unions are associated with higher salaries (Ehrenberg and Schwarz 1986; Freeman 1986; Lipsky 1982). The estimated union wage premium varies widely. A recent meta-analysis finds a modest, 2%–4.5% union premium (Merkle and Phillips 2018). Several studies using the Current Population Survey estimate a premium for unionized teachers of 10%–22% (Baugh and Stone 1982; Belman et al. 1997; Freeman and Valletta 1988). Researchers show that this premium is extensive to teachers in both public and private schools who are unionized (Allegretto and Tojerow 2014).

Several studies find that teachers' unions raise nonwage benefits. Eberts and Stone (1984) find that CB is associated with higher nonwage benefits for teachers in some schools in New York. Delaney (1985) finds that Illinois school districts with CB had a higher "fringe benefit index" compared with school districts without bargaining contracts. Similarly, Budd (2007) notes that the facilitation role of unions serves to enhance workers' awareness of, and access to, employee benefits. Podgursky (2003) argues that teachers' unions in Chicago effectively increase pension contributions for their members in public schools. Cowen and Strunk (2015)'s review finds that average payments for fringe benefits may be higher in unionized districts.

Another strand of literature finds no union effects on educational spending or on teacher pay. For example, focusing on teachers' union election certifications in three Midwestern states (Iowa, Indiana, and Minnesota), Lovenheim (2009) concludes that there is no union impact on per student district expenditures or on teacher pay. Frandsen (2016) also finds no union influence on the compensation of public school teachers.

Antiunion Legal Changes and Spending on Teacher Compensation

There is a growing body of work from economics, public sector law, and political science studying the relationship between legal frameworks and the outcomes of

the teachers' labor market as well as the underlying historical contexts of current educational policies. These studies can help us gain a deeper understanding of the role of legal framework in governing how unions operate and how they can affect outcomes in the educational sector (Cowen and Strunk 2015; Goldstein 2014; Han 2019; Lyon 2020; Moe 2011; Paglayan 2019; Winkler et al. 2012).

Legal frameworks governing public sector unions have been stable for several decades until the new state legislation in Idaho, Indiana, Tennessee, Michigan, and Wisconsin passed in 2011–12.[2] In 2011, Indiana enacted a law stating that CB is no longer mandatory and that only wage and wage-related items can be bargained. In 2011, Tennessee passed the Professional Educators' Collaborative Conferencing Act, making teachers' CB illegal. Wisconsin's new law, called Act 10 and enacted in 2011, eliminated the agency shop and restricted CB so that only wage and wage-related items can be bargained. It capped the annual growth in base pay to the rate of inflation and required teachers' unions to obtain annual recertification. In Idaho, in 2012, CB was no longer permitted unless the union could validate that at least half of a district's teachers were union members. The new law also limited CB to teacher salaries and benefits. With the new "Freedom to Work" law, Michigan became a right-to-work state in 2012, allowing non-union workers not to pay their "fair share" of union dues, significantly reducing the financial strength of unions. Michigan also restricted the scope of bargaining by removing matters of teacher evaluation, discharge, discipline, and layoffs from bargaining negotiations.

These natural experiments offer researchers a fresh tool to analyze the causal impact of changes in legal institutions in the public sector on various outcomes of teachers and students (Baron 2018; Biasi 2021; Brunner et al. 2019; Freeman and Han 2013; Han 2020; Litten 2016; Quinby 2017; Roth 2019). However, most of these studies focus on a single state (for instance, Baron [2018], Biasi [2021], and Litten [2016] for Wisconsin; Litten [2016] for Tennessee; Brunner et al. [2019] for Michigan), which is helpful in establishing internal validity but casts doubt in the external validity, where the results of their studies may be hard to generalize.

The new antiunion laws in the five states share stipulations and provisions that can affect teachers' unions and districts' decisions on finances in several ways, and there is a common knowledge regarding the role of the changes in the legal institutions regarding unions' bargaining capacity. For instance, the intent of these legal changes in all five treatment states was to curb unionization of public sector employees, including teachers. In particular, before such legal changes in 2011–12, all five states maintained duty-to-bargain laws (i.e., mandatory bargaining laws), which require employers to bargain in good faith with their employees' representatives and to sign any CB agreement that has been reached. This duty encompasses several legal obligations, including a duty not to make certain changes without bargaining with the union and not to bypass the union to deal directly with the employees it represents. The duty-to-bargain laws were the backbone of the

legal institutions for public sector unions in these treatment states for more than a half a century since they were established in the 1960 and 1970s (Hoxby 1996). Thus, the elimination of this law was considered a turning point for teachers' unions, and our study attempts to examine the overall impact of this turn of events.

The legal changes in the treatment states have the explicit goal of reducing spending on education and personnel expenses. Thus, directly, those five treatment states would observe a decrease in teacher compensation because the legal changes restricted the CB provision of teacher pay and benefits, such as capping a salary increase to the level of inflation and forbidding unions' negotiation over benefits (Freeman and Han 2013; Goldstein 2014; Han 2019; Lafer 2013; Workman 2011). Thus, by pooling similar policy changes across several states, we are able to address the statistical power issues arising from studying a single policy change in isolation and offer nuances that were missed in prior work. There are also indirect ways in which the legal changes would influence districts' spending on teacher compensation to the extent that the changes in legal institutions ultimately weakened the strength of public employee unions (Freeman and Han 2013; Han 2020). As the bargaining power of teachers' unions weakens, the demand for unions is more likely to fall because more teachers expect lower benefits from unionization (Han 2022). Reduced union membership will put pressure on the financial capability of unions, further decreasing their bargaining power.

In addition, some of the new legislation includes abolishing the collection of "fair share" or agency fees from non-union workers, which opens the door for free-riding problems (Freeman et al. 2015). Because teachers' unions must represent all teachers in the bargaining unit, regardless of membership, some teachers may want to enjoy the benefits of union contracts without paying for it, and these free riders are likely to further undermine unions' financial capacity (Marianno and Strunk 2018). To the extent that lower membership and dwindling financial capacity will negatively affect the bargaining power of unions, these changes in legislation may reduce districts' spending on teacher compensation.

Given these direct and indirect channels through which the institutional changes affect teacher compensation, therefore, we expect to observe a substantial drop in districts' spending on teacher compensation. One of the main goals of our study is to estimate how extensively these institutional changes in five states affected districts' spending on teacher compensation.

Building on these broad topics of unions and educational spending, our research aims to make several key contributions to literature. First, our research fills the knowledge gap in scholarly work regarding the role of legal institutions in districts' finances by investigating the causal impact of changes in state legislation related to public sector unions on districts' spending for teacher compensation. Second, we utilize national data, which can produce results with stronger external validity. Third, the source of exogenous variation in our study allows us to look at the decrease in spending, whereas most research on school funding

examines the effects of higher spending, as that has been the course of US educational policy. Fourth, we use various measures of teacher compensation, including both salaries and benefits expenditure, to offer a comprehensive analysis of teacher pay. Last, we conduct an analysis for districts with different socioeconomic status (SES) status and teacher compensation distribution to identify the key factors that link the change in teacher pay and legal environment.

Data

This study utilizes linked data between the Local Education Agency (School Districts) Finance Survey (SDFS) and the Stanford Education Data Archive (SEDA) in 2009–16. The SDFS details annual fiscal data for every US school district providing public education to students from prekindergarten to grade 12, and there are approximately 16,000 districts for each survey year. After the close of the fiscal year, revenues and expenditures are audited and then submitted to the NCES by each state education agency. The data based on the SDFS provide detailed revenue sources at the local, state, and federal levels, as well as specific expenditure categories (e.g., instructor, staff, administration, food service) and their subtotals.

We draw three main variables of districts' expenditure for teacher compensation from the SDFS: teacher salaries, benefits, and total compensation (salaries plus benefits) each district pays during a school year.[3] Using this information, we construct two additional variables for teacher compensation: the share of district expenditure devoted to teacher compensation and average value of teacher pay.[4] All these expenditure variables are adjusted to 2015 dollars.

The SEDA, housed in the Center for Education Policy Analysis at Stanford University, provides district-level information on schools and students from the Department of Education's Common Core Data (CCD) for all school districts in the country, along with descriptive information on the characteristics of families with school-age children residing in each district, derived from the American Community Survey via the NCES's School Districts Demographic System (SDDS).[5]

Based on the unique school district ID number, we merge these two sources to construct a data set containing a great deal of information on school districts and their surrounding neighborhoods. This data set is national and district-level panel data covering the 2008–09 through 2015–16 school years. Appendix I (Appendixes I–IV are available online) shows descriptive statistics from our linked data set.

In addition to instructor salary, benefits, total instructor compensation, total district revenue, and total district spending, our covariates for districts' characteristics include the share of students in each grade who are White, Black, Hispanic, Asian, or Native American; K–12 enrollment; the total number of

teachers; the total number of instructional aides; the total number of guidance counselors; the share of students in the district who are English-language learners; the share of students in the district who are receiving special education; and the share of all students on free or reduced-price lunch programs.

In addition, we also control for features of the community (defined to match the geographic scope of the district) such as median household income, the share of adults with a bachelor's degree or higher, the proportion of households that are Supplemental Nutrition Assistance Program (SNAP) recipients, the proportion of households headed by single mothers, the share of residents living in the same house as in the prior year, the unemployment rate, the Gini coefficient, and whether the district is in an urban, suburban, town, or rural location.

Table 1 compares the summary statistics between the treatment group composed of the five states and the comparison group including all other states for the 2008–09 school year. On average, the total expenditure of the districts from the treatment states is $12.3 million less than that of the districts from the comparison states. The treatment states spend less on teacher salaries than the comparison states by $4.5 million. The average district size, measured by student enrollment and number of staff hired, is smaller and the student body is less diverse for the treatment states. In terms of economic and social indicators of the community (such as median household income, percentage of adults with bachelor's degree and above, percentage of households receiving SNAP benefits, percentage of households with children and female head, and percentage of the unemployed), districts in the treatment states are located in less advantageous neighborhoods than those in the comparison states. Districts' overall financial status is associated with these district and community characteristics, so it is important to take these differences into consideration when comparing districts' spending patterns.

Empirical Strategy

The natural experiment in five states enables us to examine if the changes in legal institutions influence districts' spending on teacher compensation. To estimate the causal impact of changes in legal institutions restricting the bargaining power of teachers' unions on districts' spending on teacher salaries and benefits, we first employ a DID estimation of the following equation, using the treatment group composed of the five states and the comparison group including all other states:

$$Y_{kst} = \beta_0 + \beta_1 \text{Treat}_s + \beta_2 \text{After}_t + \beta_3 (\text{Treat}_s \times \text{After}_t) + \beta_4 X_{kst} + \lambda_t + \gamma_s + \varepsilon_{kst},$$
(1)

where k, s, and t indicate districts, states, and years, respectively. Treat equals 1 for the treatment group and 0 for the comparison group, and After equals 1 if

TABLE 1

Summary Statistics for District Finance on Teacher Compensation 2008–09 School Year

Variables	Treatment States	Control States	Difference = Treat—Control
District expenditure:			
Total district expenditure (in million $)	33.73	45.99	−12.26*
			(5.80)
Teacher salary expenditure (in million $)	11.26	15.74	−4.48*
			(2.20)
Teacher benefits expenditure (in million $)	4.74	5.15	−.41
			(1.09)
Total teacher compensation expenditure (in million $)	16.01	20.90	−4.89
			(3.45)
District characteristics:			
Grades 3–8 enrollment	1,354.6	1,696.9	−342.3[+]
			(182.6)
Number of teachers	165.5	222.4	−56.89*
			(22.80)
Number of instructional aides	37.97	51.03	−13.06**
			(4.04)
Number of instructional coordinators/supervisors	4.39	4.94	−.55
			(.89)
Number of elementary guidance counselors	1.62	2.17	−.55*
			(.25)
% Hispanic students	.061	.138	−.077**
			(.005)
% Black students	.056	.075	−.19**
			(.004)
% Asian students	.012	.021	−.009**
			(.001)
% Native American students	.016	.033	−.017**
			(.003)
% Free/reduced-price lunch	.479	.481	−.002
			(.006)
% ELL students	.023	.044	−.020**
			(.002)
% Special education students	.149	.138	.011**
			(.002)
% Public school students in charter schools	.017	.011	.006**
			(.002)

TABLE 1 (*Continued*)

Variables	Treatment States	Control States	Difference = Treat—Control
Community characteristics:			
Median household income (in $)	59,049	62,507	−3,457**
			(742)
% Adults with bachelor's degree and above	.208	.240	−.031**
			(.004)
% Households with children and female head	.208	.223	−.015**
			(.003)
% Households receiving SNAP benefits	.160	.152	.008*
			(.003)
% Living in the same house as last year	.875	.873	.002
			(.002)
% Unemployed	.056	.043	.013**
			(.0007)
Gini coefficient	.353	.355	−.002
			(.002)
City/urban locale	.061	.054	.006
			(.006)
Suburban locale	.188	.214	−.027*
			(.011)
Town locale	.211	.180	.031**
			(.010)

NOTE.—The treatment group includes ID, IN, MI, TN, and WI, and the comparison group includes all other states. Total teacher compensation expenditure is the sum of teacher salary expenditure and teacher benefits expenditure. The share of teacher salary expenditure is teacher salary expenditure divided by total district expenditure; the share of teacher benefits expenditure is teacher benefits expenditure divided by total district expenditure; the share of total teacher compensation expenditure is total teacher compensation expenditure divided by total district expenditure. Average teacher salary is computed by teacher salary expenditure divided by total number of teachers; average teacher benefit is computed by teacher benefits expenditure divided by total number of teachers; and average total teacher compensation expenditure is the sum of average teacher salary and average teacher benefit. ELL = English/Language Arts.

SOURCE.—Linked data between the Local Education Agency (School Districts) Finance Survey and the Stanford Education Data Archive for 2008–09 school year.

  [+] $p < .1$.
  * $p < .05$.
  ** $p < .01$.

the year is 2012 and after and 0 if the year is before 2012. X_{kst} is a vector of district and community characteristics. To allow for the effect of legal change on teacher pay to emerge over time and to control for treatment year heterogeneity that occurs in two different time periods (2011 and 2012), we include a full set of time effects, λ_t. To control for unobservable heterogeneity in each state, we add state dummies γ_s. ε_{kst} is the error term, reflecting variation not accounted for in the model. β_3 gives us the DID estimator, the treatment effect, for the effect of legal changes on teacher compensation variables. Because the treatment occurs at the state level, the standard errors are clustered within states to control for possible serial correlation.

Although there may be no best practice involving different numbers of states and time points in the DID estimation, formal statistical inference becomes questionable as the number of states gets smaller (Bertrand et al. 2004; Cameron and Miller 2015). Therefore, our approach utilizing a DID model based on all 50 US states is likely to produce more reliable estimates.

It is noteworthy that because the new statutes do not overwrite existing CB agreements, any district with a bargaining agreement that did not expire when the new laws became effective can still operate under its previous teacher compensation scheme. A typical CB contract is renewed every 2–3 years, although some contracts can last 5 years. Thus, most districts would have renewed their existing contracts by the end of the 2015–16 school year, and our data would have captured the full effect of the legal changes that occurred before 2012.

The DID results may not be driven by the legal changes (antiunion reforms) but by systematic differences in the treatment and comparison states. Thus, the key assumption for the DID estimator is that the treatment and control states would have parallel trends in outcome variables before the legal changes. Although we cannot observe the counterfactual—what would have happened to the outcome variables for the treatment states if the legal changes had not occurred—we can examine the trends in outcome variables for both groups before the legal changes and examine whether the two groups are indeed comparable. Based on the SDFS between the 2003–04 and 2015–16 school years, we test for this parallel-trend assumption using an event-study approach of the following equation:

$$Y_{kst} = \beta_0 + \sum_{j=-8}^{-1} \beta_j \text{Event}_{ksj} + \sum_{j=1}^{4} \beta_j \text{Event}_{ksj} + \varepsilon_{kst}, \tag{2}$$

where Event is the number of periods since the treatment (antiunion reforms). Event is normalized to −1 for control units. Each of the event-study coefficient β_j is a simple DID estimator using the period just before the onset of antiunion reform as the "before" period, and the period of the event-study coefficient as the "after" period. Thus, coefficient β_j measures the difference in the outcome

variables at a specified period relative to the difference between the two groups 1 year before the legal changes. If the pretreatment trends are parallel, β_j would be close to zero for any period before the legal changes. If the legal changes reduced district spending on teacher pay in treatment states relative to comparison states, β_j would be negative for any period after the legal changes.

In figure 1, we plot the estimated event-study coefficients (β_j in eq. [2]) and their 95% confidence intervals for districts' spending on teacher salary in figure 1A, teacher benefits in figure 1B, and total compensation in figure 1C, respectively. The vertical lines refer to the enactment of the legal changes. The event-study coefficients in all three figures before the legal changes are close to zero. This indicates that the pretreatment trends for district spending on teacher compensation are constant for the treatment and comparison states, providing evidence that the parallel-trend assumption is likely to be satisfied. The event-study coefficients in all three figures turn negative after the antiunion reforms. This implies that districts in the treatment states, compared with districts in the comparison states, spent less on teacher compensation.

It is important to examine if there were other concurrent changes to the educational policies affecting teacher compensation in the treatment states. In 2013, one state, Tennessee, revised its state minimum salary schedule for teachers, reducing pay for senior teachers and those with graduate degrees to give districts the flexibility to adopt performance-based pay plans, if desired. There are about 140 districts in Tennessee, and only four districts had adopted "alternative salary schedules" by the 2011–12 school year (Wesson 2013), but more than 40% of districts had incorporated a performance bonus into their salary schedules by 2015. Because this new law became effective in the 2014–15 school year, it may pose a confounding influence on our main mechanism if the effect of the new bargaining law sets in with some delay.

In figure 2, we plot teacher compensation in Tennessee over time to check if the change in minimum salary schedules causes a substantial alteration in the trend for teacher pay. The graph displays a considerable drop in teacher salary expenditure after 2012, immediately followed by the legal change prohibiting CB of teachers, but it does not show any significant change after 2013. This suggests that the state and local governments responded to the new state minimum salary schedule by allocating funds for teacher pay such that overall districts' spending on teacher salaries remained largely constant (i.e., low minimum salary schedule accompanied by higher expenditure on performance pay). Therefore, it is unlikely that the changes in the minimum salary schedule bias our estimates of the effect of change in legal institutions severely limiting or prohibiting the CB of teachers.

During our study period, several states experienced accountability reforms, such as the implementation of high-stakes teacher evaluation systems, elimination or weakening of tenure, and increase in the length of probationary periods.
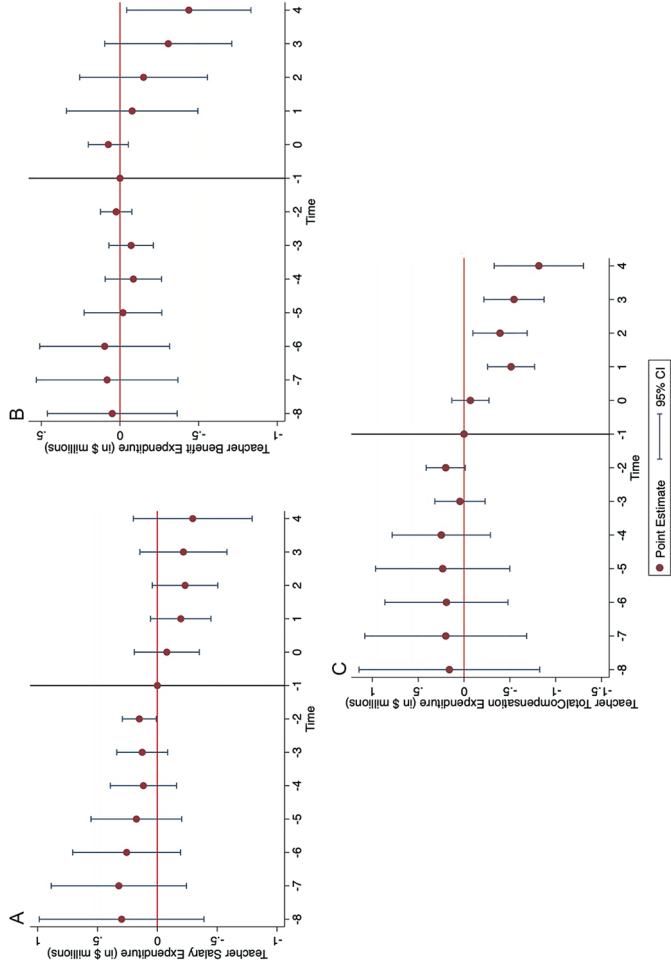
FIG. 1.—Testing of parallel trend condition. (A) Teacher salary expenditure. (B) Teacher benefit expenditure. (C) Teacher total compensation expenditure. Data are from Local Education Agency (School Districts) Finance Survey between 2003–04 and 2015–16 school years. Color version available as an online enhancement.
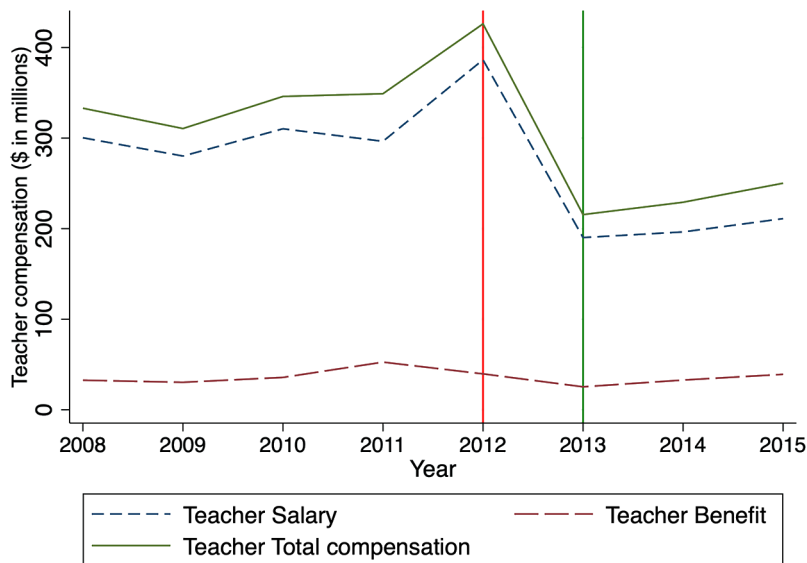
FIG. 2.—Teacher total compensation expenditure in Tennessee, 2008–15. Data are from Local Education Agency (School Districts) Finance Survey between 2008–09 and 2015–16 school years. Color version available as an online enhancement.

Kraft et al. (2020) find that those accountability reforms had negative effects on the supply of new teachers, possibly shifting the distribution of experience and educational attainment among public school teachers. They, however, detect no evidence that teacher evaluation reforms changed average teacher salaries, suggesting that such policy changes do not directly influence district spending on teacher compensation.

Because the DID estimation in model (1) uses all other US states as a comparison group, assuming that the variables have fixed relationships over time, the DID estimation may not produce accurate treatment effects. Thus, we also employ the synthetic control method (SCM), popularized by Abadie et al. (2010), to supplement our DID estimates.

The SCM synthesizes a control from a weighted sum of "donor states" chosen from a pool of potential candidates. That weighted sum is computed by matching explanatory variables and outcome variables in the pretreatment period of these donor states to the same variables in the pretreatment period of the treated state. If the control's outcome closely matches the treated state's outcome during pretreatment periods, and the outcome paths of the synthetic control and treated

state diverge, comparing the outcome paths after the treatment provides estimates for the treatment effects.[6]

To implement the SCM, we collapse the district-level information from our data set to create a state-level balanced panel data set. Instead of combining Idaho, Indiana, Michigan, Tennessee, and Wisconsin and treating them as a single treatment unit as we did in the DID estimation, the new SCM approach allows us to deal with multiple treatments at different time periods so that effects are centered on the treatment period so as to be comparable (the treatment effects from union reforms occurred after 2012 for Idaho, Indiana, Tennessee, and Wisconsin but after 2013 for Michigan).[7]

To identify predictors of the outcome variables, we use all the covariates used in the DID estimates as well as one lagged variable from 2009. After identifying the potential donor states that will synthesize the control group, the SCM creates state-level weights that will minimize the synthetic's mean squared prediction error (MSPE) to form a synthetic control state. We then evaluate how closely the outcome path of synthetic control during the pretreatment period follows that of the treated states. The SCM's primary output is a pretreatment and post-treatment path of districts' spending on teacher compensation for both the synthetic control state and the treated states.

In table 2, I list the nine selected donor states and their associated weights. Pennsylvania is the most heavily weighted (22%). Ohio and Iowa, respectively, receive weights of 19% and 14%. Colorado and Minnesota have a weight of 14% and 11%, respectively. Oregon, Kansas, Missouri, and Connecticut share the rest of the weights.

Although the SCM offers more accurate treatment effects than the DID approach, it has limitations when applied to our study. First, because the SCM relies on state-level data, it does not allow us to analyze the effect of legal changes by

TABLE 2

Synthetic Donor State Weights

State	Weight
Pennsylvania	.221
Ohio	.194
Iowa	.142
Colorado	.138
Minnesota	.109
Oregon	.069
Kansas	.063
Missouri	.050
Connecticut	.013
Sum	1

SES status or by quartile of distribution for teacher compensation of each district. In addition, unlike the DID estimation, the SCM offers a path, instead of a single measure, of treatment effects that are year specific for all posttreatment periods, requiring an "eyeball test" to compute the average treatment effects across all posttreatment periods. Thus, we use the SCM to provide additional evidence for overall treatment effects across all districts but still rely on the DID estimates for other district-level analysis.

Results

Table 3 presents the augmented DID estimates with a full set of control variables presented in table 1, as well as year and state dummies. We use three measures for teacher compensation along with total district spending, and the results for each dependent variable are presented in a separate panel: teacher salary expenditure in panel A, teacher benefits expenditure in panel B, teacher total compensation expenditure, which is the sum of teacher salary expenditure and teacher benefits expenditure, in panel C, and total district spending in panel D. For all measures of district expenditure, we use log-transformation to deal with skewedness in data and to make patterns we detect in the data more interpretable.

Panel A shows that districts in the treatment states significantly reduced teacher salary expenditure by 4%, whereas districts in the comparison states raised it by 1%. Thus, the DID estimation demonstrates that the antiunion legal changes reduced teacher salary expenditure by 5%. In panel B, district expenditure on teacher benefits in the treatment states did not change much, whereas the benefits expenditure in the comparison states significantly increased by about 10%. This results in the DID estimation of a 9.7% decrease in teacher benefits expenditure. Thus, the legal changes reduced both teacher salary and benefits expenditure of districts, but their negative impact was greater on teacher benefits. Panel C shows that districts in the treatment states reduced expenditure on total teacher compensation by 3%, whereas districts in the comparison states raised it by 3%, indicating that the antiunion laws reduced total teacher compensation expenditure by 6%.

Panel D shows that the legal changes reduced district's total spending by 3.8%, which is smaller in magnitude than the decrease in total teacher compensation expenditure of 6%. This suggests that districts reallocate their resources such that the reductions in teacher compensation increased district spending elsewhere.

As a sensitivity analysis, we also look at other measures of district spending on teacher compensation. Panel A of table 4 presents the results of the DID estimation using the ratio between teacher pay expenditure and total district expenditure (i.e., the share of total district expenditure devoted to teacher compensation) as an

TABLE 3

The Effect of Legal Changes Weakening the Strength of Teachers' Unions on the District Expenditure for Teacher Compensation

Augmented DID Estimators	Before Legal Changes	After Legal Changes	Treatment Effect = After—Before
Panel A: Log(teacher salary expenditure)			
Treatment group	13.903	13.863	−.040*
	(.017)	(.015)	(.020)
Control group	13.904	13.914	.010
	(.008)	(.007)	(.010)
Difference (treatment—control)	−.001	−.051**	**−.050****
	(.017)	(.015)	(.006)
Panel B: Log(teacher benefits expenditure)			
Treatment group	12.742	12.745	.003
	(.018)	(.016)	(.023)
Control group	13.464	13.564	.100**
	(.008)	(.007)	(.011)
Difference (treatment—control)	−.722**	−.819**	**−.097****
	(.177)	(.176)	(.007)
Panel C: Log(teacher total compensation expenditure)			
Treatment group	14.176	14.150	−.026*
	(.018)	(.015)	(.012)
Control group	14.420	14.455	.034*
	(.008)	(.007)	(.010)
Difference (treatment—control)	−.244	−.304+	**.060****
	(.175)	(.176)	(.006)
Panel D: Log(total district expenditure)			
Treatment group	14.900	14.888	−.012
	(.018)	(.015)	(.023)
Control group	15.384	15.412	.026*
	(.008)	(.007)	(.010)

TABLE 3 (*Continued*)

Augmented DID Estimators	Before Legal Changes	After Legal Changes	Treatment Effect = After—Before
Difference (treatment—control)	−.484** (.158)	−.522** (.168)	**−.038**** (.006)

NOTE.—Errors are clustered within states (presented in parentheses). *N* is rounded to the nearest 10. The treatment group includes ID, IN, MI, TN, and WI, and the control group includes all other states. Covariates for district characteristics include total grades 3–8 enrollment, number of teachers, number of instructional aides, number of instructional coordinators/supervisors, number of elementary guidance counselors, % Hispanic students, % Black students, % Asian students, % Native American students, % reduced-price lunch, % free lunch, % ELL students, % special education students, and % public school students in charter schools. Covariates for community characteristics include median household income (in $), % adults with bachelor's degree and above, % households with children and female head, % households receiving SNAP benefits, % living in the same house as last year, % unemployed, Gini coefficient, city/urban locale, suburban locale, town locale, and state and year dummies.

SOURCE.—Linked data between the Local Education Agency (School Districts) Finance Survey and the Stanford Education Data Archive for 2009–10 and 2015–16 school year.

$^+$ $p < .1$.
* $p < .05$.
** $p < .01$.

outcome measure for teacher salary, teacher benefits, and total teacher compensation. The changes in legal institutions weakening the strength of teachers' unions reduced the share of teacher salary expenditure in districts' total spending by 0.4%, the share of teacher benefits expenditure by 0.7%, and the share of total teacher compensation by 1.1%. This implies that the changes in legal institutions reduced not only the level of teacher compensation but also the fraction of teacher pay out of districts' total spending.

In panel B of table 4, we present the DID results for average teacher compensation for three pay variables (total expenditure divided by total number of teachers): average teacher salary, average teacher benefits, and average total teacher compensation. The legal changes reduced average teacher salary, teacher benefits, and total compensation expenditure by 5%, by 8.9%, and by 5.8%, respectively.[8]

To assess the validity and robustness of our DID model, we use a "placebo" test, in which we test whether the DID model detects an "effect" for an outcome that should logically be unrelated to the policy intervention (Lipsitch et al. 2010). We estimate the "effects" of limiting the CB rights of teachers on compensation for food service personnel. The new state legislations do not target these school employees, so we expect to see no significant effects on district expenditure on

TABLE 4

The Effect of Legal Changes Weakening the Strength of Teachers' Unions on the Other Measures of District Expenditure for Teacher Compensation

Augmented DID estimates	
Panel A: The share of district expenditure devoted to teacher compensation	
(Teacher salary expenditure/Total district expenditure) × 100	−.394**
	(.089)
(Teacher benefits expenditure/Total district expenditure) × 100	−.715**
	(.072)
(Teacher total compensation/Total district expenditure) × 100	−1.109**
	(.129)
Panel B: Average teacher compensation	
Log(Teacher salary expenditure/Total number of teachers)	−.050**
	(.004)
Log(Teacher benefits expenditure/Total number of teachers)	−.089**
	(.005)
Log(Teacher total compensation/Total number of teachers)	−.058**
	(.004)

NOTE.—Errors are clustered within states (presented in parentheses). N is rounded to the nearest 10. The treatment group includes ID, IN, MI, TN, and WI, and the control group includes all other states. Covariates for district characteristics include total grades 3–8 enrollment, number of teachers, number of instructional aides, number of instructional coordinators/supervisors, number of elementary guidance counselors, % Hispanic students, % Black students, % Asian students, % Native Americans students, % reduced-price lunch, % free lunch, % ELL students, % special education students, and % public school students in charter schools. Covariates for community characteristics include median household income (in $), % adults with bachelor's degree and above, % household with children and female head, % household receiving SNAP benefits, % living in same house as last year, % unemployed, Gini coefficient, city/urban locale, suburban locale, town locale, and state and year dummies. Estimates for panel B do not control for number of teachers to avoid collinearity problems.

SOURCE.—Linked data between the Local Education Agency (School Districts) Finance Survey and the Stanford Education Data Archive for 2009–10 and 2015–16 school year.

+ $p < .1$.
* $p < .05$.
** $p < .01$.

their compensation. Before 2012, the time trends for treatment and comparison groups are similar, and both salaries and benefits expenditure for food service personnel do not show any significant change after 2013. Table B1 reports the summarized results for the DID estimates. We find that the legal changes restricting the strength of teachers' unions make no significant impact on compensation of food service personnel.

Recently, several researchers have found that two-way fixed effects (TWFE) models can produce a biased estimation, especially when heterogeneous treatment effects are present (Borusyak et al. 2021; Callaway and Sant'Anna 2021; de Chaisemartin and D'Haultfœuille 2020; Goodman-Bacon 2021). For instance, if the treatment effects differ by intensity, time periods, or sets of covariates, the causal interpretation of the DID model combined with TWFE can lead to a bias in overall estimation of the treatment effect and result in nonparallel trends of counterfactual outcomes. These new econometrics approaches expand the previous TWFE DID model by producing estimates of variance-weighted averages of many different treatment effects. The treatment (antiunion reforms) in our study has heterogeneity in terms of legislative items and the timing of law effectiveness. Thus, to assess the robustness of our estimates to potential heterogeneous treatment effects, we employ several different estimators, along with the OLS, and present the results in Appendix III.[9] These approaches show similar results, implying that the potential bias from the heterogeneity is small. This confirms the negative impacts of the antiunion legal changes on teacher pay expenditure.

Several other states, such as Nebraska, New Hampshire, New Jersey, and Oklahoma, also passed laws weakening the unionism of public school teachers as of 2011–12.[10] The terms of the legal changes in these states, however, are more subtle compared with the changes in our treatment states. For instance, in 2012, Nebraska passed legislation that requires local unions to have periodic certification votes of their members. In 2012, New Jersey's new law limiting public employees' ability to bargain over health care became effective, and employee pension and health care contributions have increased. These four states still had duty-to-bargain laws and maintained their union membership rate, even after their legal changes (Han 2022). As a sensitivity test, however, we redo the analysis using these four alternative treatment states. The estimated DID estimates are statistically insignificant in all models, suggesting that the implications and the consequences of the antiunion legal changes depend on their extent and magnitude.

The DID estimation uses all other US states as a single comparison group, and it cannot account for the effects of confounders changing over time. To overcome this shortcoming of the DID approach and to establish a better comparison group that is more comparable with treatment group, we employ the SCM. The four states (Nebraska, New Hampshire, New Jersey, and Oklahoma) are excluded from the pool of donor states in our SCM.

Figure 3 displays the SCM results for teacher salary. Figure 3A shows that the average instructor salary of the synthetic state closely follows that of the treated state until 2011, meaning that the synthetic state appears to be a good control for the treated state. After 2011, the curves between the two groups diverge, providing insights into the treatment effects. Figure 3B depicts the treatment effects,
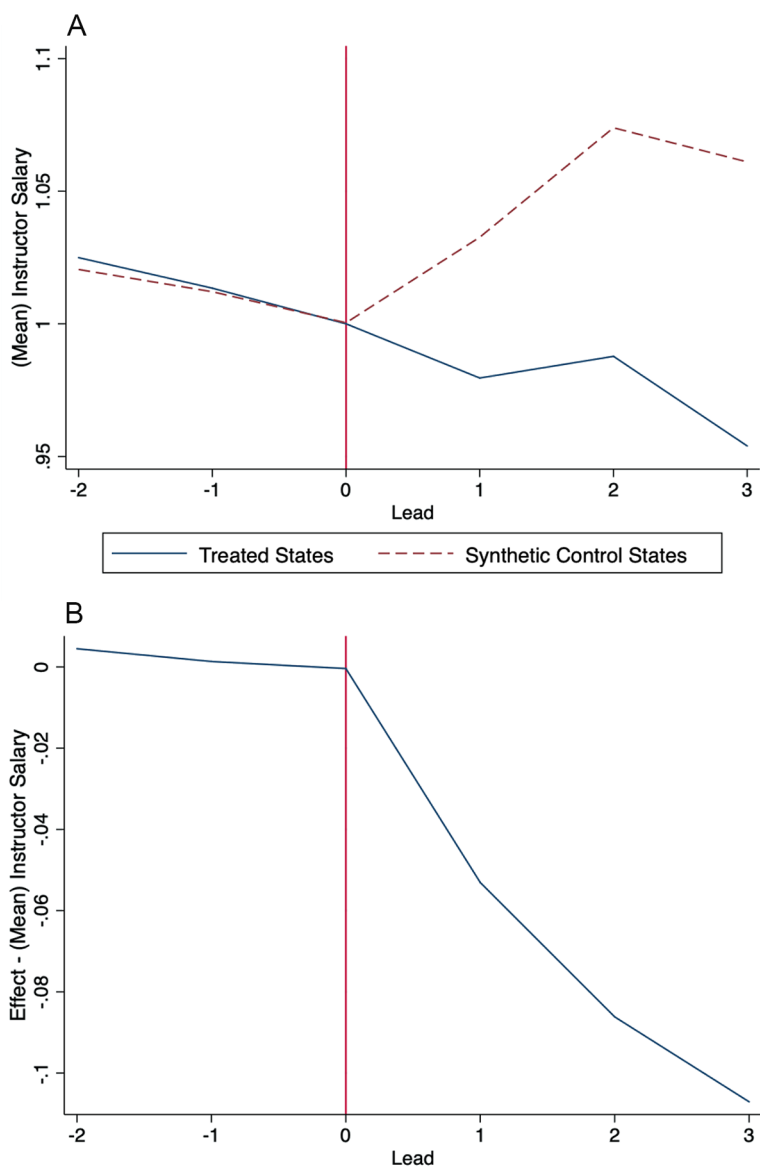
FIG. 3.—Synthetic control method for teacher salary expenditure. (A) Trend for treated and synthetic control states. (B) Treatment effect. The treatment effects are from the synthetic control method based on treatment group (ID, IN, MI, TN, and WI) and the synthetic control constructed from all other states. Color version available as an online enhancement.

the gap of the mean instructor salary between the treated state and synthetic control. The treatment effects display a continuous decrease in instructor salary, and, by 2016, the treatment effects exceed a 10% drop.

Figure 4 presents the SCM results for teacher benefits and figure 5 for teacher total compensation. Both figures 4A and 5A describe similar trends from the synthetic state and treated state before treatment, but the curves move in the opposite direction (an increasing pattern for the synthetic state but a decreasing pattern for the treated states). Accordingly, the treatment effects displayed in figures 4B and 5B show a negative effect of legal changes in the treated states on teacher compensation. By 2016, the treatment effects reached an approximately 15% drop for teacher benefits and 8% drop for total compensation.[11]

In sum, the results from the SCM are consistent with our DID estimation, and their average effect sizes across posttreatment periods are fairly similar. This result shows that the changes in legal institutions that significantly reduced union strength in Idaho, Indiana, Michigan, Tennessee, and Wisconsin substantially cut district spending on both teacher salary and benefits. In addition, the SCM results suggest that those legal changes may have long-run effects on teacher compensation, if the divergent pattern of each group continues.

Literature finds that that the impact of school spending differs by students' family background, and the distribution of educational funding is far from being considered equitable (Baker et al. 2019; Chingos and Blagg 2017; Jackson et al. 2016; Lafortune et al. 2018; Leachman and Figueroa 2019; Morgan and Amerikaner 2018). Following up on these studies, we examine whether the impacts of legal changes restricting the CB of teachers' unions vary by the SES of districts.

We measure districts' SES status with a composite index based on the share of children in poverty, median household income, the share of adults with a bachelor's degree and above, the share of households with children and a female head, the share of residents living in the same house as in the prior year, the share unemployed, and the Gini coefficient.[12] We expect that the legal changes may have greater effects on teacher pay in the high-SES districts than in the low-SES districts because the low-SES districts are more likely to operate with greater constraints, which leaves less room for a change in teacher compensation. For instance, it is difficult to attract high-quality young applicants to the teaching sector and retain them, especially in low-SES districts (Goldhaber et al. 2015; Ingersoll and Merrill 2017), and reducing teacher pay may not be well received by educators and policy makers.

Table 5 presents the DID results by districts' SES status. As expected, we find that teacher salaries, benefits, and total compensation fell more sharply in the high-SES districts than in the low-SES districts. The gap in the fall in teacher pay between the two groups of districts is more noticeable with teacher benefits than with teacher salaries.
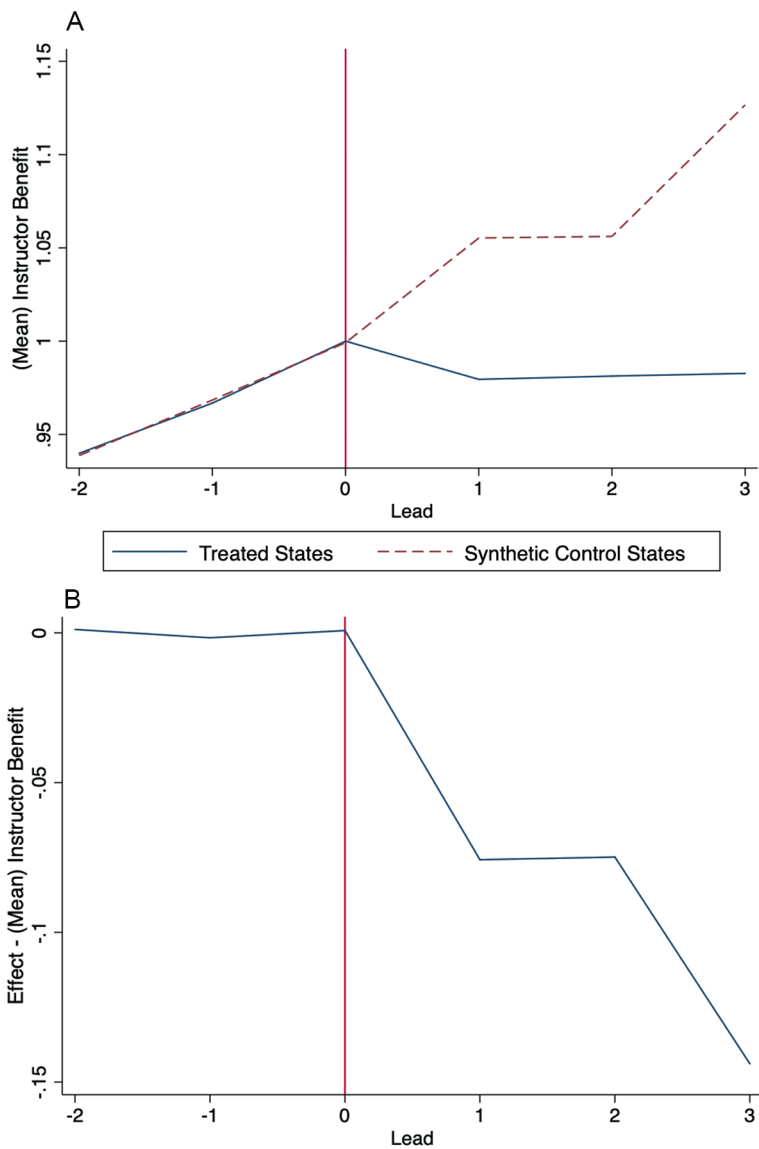
FIG. 4.—Synthetic control method for teacher benefits expenditure. (A) Trend for treated and synthetic control states. (B) Treatment effect. The treatment effects are from the synthetic control method based on treatment group (ID, IN, MI, TN, and WI) and the synthetic control constructed from all other states. Color version available as an online enhancement.
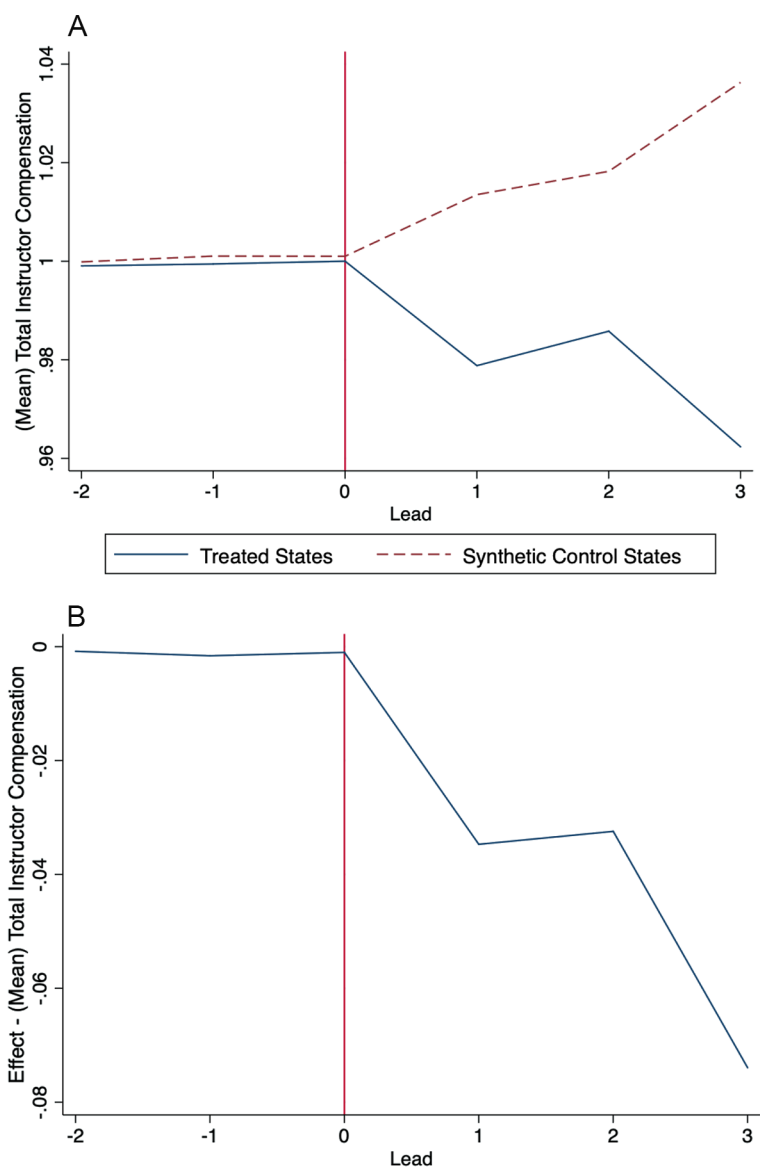
FIG. 5.—Synthetic control method for teacher total compensation expenditure. (A) Trend for treated and synthetic control states. (B) Treatment effect. The treatment effects are from the synthetic control method based on treatment group (ID, IN, MI, TN, and WI) and the synthetic control constructed from all other states. Color version available as an online enhancement.

TABLE 5

The Effect of Legal Changes Weakening the Strength of Teachers' Unions on Teacher Compensation, by SES Status

Augmented DID Estimates for	High-SES districts (1)	Mid-SES districts (2)	Low-SES districts (3)
Log(teacher salary expenditure)	−.0500**	−.0450**	−.0437**
	(.0065)	(.0042)	(.0098)
Log(teacher benefits expenditure)	−.154**	−.0910**	−.0225*
	(−.0135)	(.0067)	(.0109)
Log(teacher total compensation)	−.0867**	−.0529**	−.0439**
	(.0105)	(.0081)	(.0118)
Observations	18,750	37,620	19,090

NOTE.—Errors are clustered within states (presented in parentheses). N is rounded to the nearest 10. The treatment group includes ID, IN, MI, TN, and WI, and the control group includes all other states. Covariates for district characteristics include total grades 3–8 enrollment, number of teachers, number of instructional aides, number of instructional coordinators/supervisors, number of elementary guidance counselors, % Hispanic students, % Black students, % Asian students, % Native Americans students, % reduced-price lunch, % free lunch, % ELL students, % special education students, and % public school students in charter schools. Covariates for community characteristics include median household income (in $), % adults with bachelor's degree and above, % household with children and female head, % household receiving SNAP benefits, % living in same house as last year, % unemployed, Gini coefficient, city/urban locale, suburban locale, town locale, and state and year dummies.
Source.—Linked data between the Local Education Agency (School Districts) Finance Survey and the Stanford Education Data Archive for 2009–10 and 2015–16 school year.
  + $p < .1$.
  * $p < .05$.
  ** $p < .01$.

Will the effects of legal changes weakening the strength of teachers' unions differ by quartile of distribution for teacher compensation? Is the impact of the legal changes greater for the high-paying or low-paying districts? Table 6 summarizes the results from the quantile DID estimation at the specified quantile from the 10th to the 90th percentile based on our full models (quantile 50 performs the quantile DID at the median). The legal changes affected teacher salaries uniformly across the distribution, suggesting that the outcome was a district-wide salary freeze rather than an alteration in the shape of the salary distribution. The negative impact of the legal changes on teacher benefits, however, was much greater for districts located at the bottom of the distribution than for districts at the top. This indicates that the districts that had paid lower benefits to teachers before the legal changes are those that decreased teacher benefits even more after the new laws were established. Because the teacher pay schedule is mostly based on

TABLE 6

The Effect of Legal Changes Weakening the Strength of Teachers' Unions on Teacher Compensation, at Multiple Points in the District's Distribution

Quantile DID Estimates for	10th Percentile (1)	25th Percentile (2)	50th Percentile (3)	75th Percentile (4)	90th Percentile (5)
Log(teacher salary expenditure)	−.057*	−.052*	−.053**	−.052**	−.053**
	(.023)	(.021)	(.015)	(.011)	(.012)
Log(teacher benefits expenditure)	−.106**	−.103**	−.092**	−.085**	−.066**
	(.025)	(.019)	(.017)	(.013)	(.012)
Log(teacher total compensation)	−.072**	−.075**	−.060**	−.052**	−.047**
	(.024)	(.019)	(.015)	(.012)	(.011)
Observations	77,082	77,082	77,082	77,082	77,082

NOTE.—Errors are clustered within states (presented in parentheses). N is rounded to the nearest 10. The treatment group includes ID, IN, MI, TN, and WI, and the control group includes all other states. Covariates for district characteristics include total grades 3–8 enrollment, number of teachers, number of instructional aides, number of instructional coordinators/supervisors, number of elementary guidance counselors, % Hispanic students, % Black students, % Asian students, % Native Americans students, % reduced-price lunch, % free lunch, % ELL students, % special education students, and % public school students in charter schools. Covariates for community characteristics include median household income (in $), % adults with bachelor's degree and above, % household with children and female head, % household receiving SNAP benefits, % living in same house as last year, % unemployed, Gini coefficient, city/urban locale, suburban locale, town locale, and state and year dummies.

SOURCE.—Linked data between the Local Education Agency (School Districts) Finance Survey and the Stanford Education Data Archive for 2009–10 and 2015–16 school year.

 + $p < .1.$
* $p < .05.$
** $p < .01.$

education level and experience, districts at the bottom of the distribution within a state are likely to have lower base salary schedules and more novice teachers than districts at the top of the distribution. Thus, the findings of table 6 suggest that novice teachers, compared with more experienced teachers, pay a greater burden for districts' reduction of benefits expenditure.

As an additional sensitivity test for our DID estimation, we also use as an alternative comparison states that have duty-to-bargain laws and allow agency fees. All five states in our treatment group had a duty-to-bargain law before their legal changes, thus other states that have the same legal environment may be more comparable to the treatment group.[13] Thus, the posttrend of the alternative comparison states can serve as a counterfactual—what treatment states would have experienced if they did not have such legal changes. Tables C1 through C3 show the results based on this new comparison group. The alternative results are similar to those presented in tables 2 through 6 but with somewhat larger coefficients in magnitude (more negative), implying that the effects of restricting teachers' bargaining rights are greater when we focus on states that share a similar legal environment related to teachers' unions. This alternative comparison group generally provides more prounion environments, so antiunion reforms are likely to impose greater impact on teacher pay when compared with the union-friendly group that continues to pay teachers well.

Discussion and Conclusion

The changes in legal institutions to severely reduce union strength in Idaho, Indiana, Michigan, Tennessee, and Wisconsin in 2011–12 form a natural experiment, allowing us to examine the causal impact of weakening unionism on teacher compensation by employing the DID design and SCM.

What did the new laws restricting the bargaining rights of teachers' unions achieve? The data show that they reduced total teacher compensation, the share of districts' expenditures on teacher compensation, and the average spending on compensation per teacher. After the legal changes, teacher salary expenditure dropped by 6%, and teacher benefits expenditure fell by almost 10%, in the treatment states. The SCM shows similar results, with the magnitude of the treatment effects growing over time. Because teachers' unions negotiate for all teachers in public school districts regardless of union membership, impeding unions' negotiating power means lower pay and poorer working conditions for all teachers. When teachers are already underpaid relative to comparable peers, slashing teacher pay even more may cause more harm than benefit to our educational outcomes (Allegretto and Mishel 2020).[14]

The findings of this study also demonstrate that after the legal changes in limiting teachers' CB rights, districts substantially reduced teacher compensation

in all three measures, instead of reallocating salary expenditure among teachers with different pay schedules, while maintaining their spending on teacher pay. To the extent that districts manage their budget by evaluating the previous budget, monitoring current budgets, and planning for the next budget, such legal changes are likely to have long-term effects on the spending pattern of the districts. The estimates of this study, therefore, might be at the lower end of the total impact of legal changes on teacher compensation in the treatment states.

Moreover, according to our findings from the quantile DID estimates, the districts that had paid lower benefits to teachers reduced teacher benefits further after the legal changes. This suggests that the legal changes will raise income inequality among teachers, mostly resulting from lower benefits. The increased inequality in compensation among teachers may translate into greater performance gaps between students if teachers receiving lower benefits are more likely to quit teaching or move to districts that pay more. Therefore, our study predicts that the negative effects of the legal changes on teacher pay will be even greater in the long run if the current trend persists.

In recent periods, we have observed dramatic changes in the legal environment for public sector unions. In 2016, Kentucky and West Virginia became right-to-work states. Then, in 2018, the Supreme Court decision in *Janus* substantially diminished the financial capacity of public sector unions by banning their collection of agency fees from nonmembers who are covered by union contracts. These continued movements that restrict teacher unions' bargaining power and threaten their strength will result in the decrease of our teachers' well-being by reducing pay and deteriorating employment conditions, shifting the course of our educational system onto a different path. Union dues or agency fees for union members before *Janus* are about 1%–2% of teacher salary. Our study shows that changes in legal institutions weakening teachers' unions reduce the average teacher compensation by about 6%. Non-union teachers are able to save a union fee of 2% of their salary, but they lose 6% in salary gain, which results in a net loss of 4% of their salaries. Therefore, these new legal environments related to teachers' unions have led to reduced compensation for all teachers, regardless of their union status.

In response to the legal changes, some districts in Wisconsin adopted a flexible pay system in the form of merit pay, which may increase teacher quality (Biasi 2021). On the other hand, reduced teacher compensation contributes to the attrition of high-quality teachers in states that passed antiunion legislations (Han 2020). Recent studies show that the COVID-19 pandemic has significantly increased teachers' stress and raised teacher attrition (Carver-Thomas et al. 2021; Diliberti et al. 2021). Thus, as the pandemic persists, low teacher compensation may have exacerbated teacher shortage problems while reducing overall teacher quality in the treatment states. Due to the data limitations, we cannot examine which channel dominates. The topic of the impact of the antiunion legal changes on teacher quality and student outcomes is left for future study.

Notes

1. For instance, for four out of these five states (Indiana, Michigan, Tennessee, and Wisconsin), the legal changes limiting teachers' CB rights occurred immediately after a Republican governor took office following a Democratic governor. Idaho had elected Republican governors since 1994.

2. Three states have passed "right-to-work" laws since 2016. West Virginia became a right-to-work state in 2016, and the West Virginia Supreme Court upheld right-to-work in April 2020. Both Kentucky and Missouri passed a right-to-work bill in 2017, but the law in Missouri was defeated in a 2018 referendum before it could take effect. In 2017, Iowa significantly restricted the bargaining rights of teachers and other public employees. Our study period covers 2009–16, so legal changes in these states do not influence our study.

3. We use the broader instructional salary item (Z33) and benefit item (V10), which include expenditures both for teachers by program area and for instructional assistants and aides.

4. Most of the antiunion reforms in our treatment states limited CB to teacher compensation, potentially removing class size from the negotiation items. Thus, using spending per pupil as a dependent variable can lead to the underestimation of the impact of antiunion reform on district spending for teacher pay.

5. Note that because some districts are missing in some years, the panel data are unbalanced. The SEDA also contains outcome measures for student, district, and grade-level averages of achievement test scores for each district.

6. Abadie et al. (2010) describe three assumptions for effective use of the method: (i) only the treated state is affected by the legal change for all years in the pretreatment period, (ii) the legal change has no effect before it is enacted, and (iii) the treated state's counterfactual outcome can be approximated by a fixed combination of donor states.

7. For our SCM implementation, we use Stata's "synth_runner" package developed by Quistorff and Galiani (2017).

8. The results in table 4 should be taken with caution because the average values of teacher compensation depend on the tradeoff between pay level and employment. For instance, the average teacher salary will be higher if a district hires fewer teachers but pays the same salaries to incumbent teachers as in the previous year. We find that the effect of the legal changes on class size and full-time equivalent (FTE) teachers is not statistically significant: the augmented DID estimate is –0.214 (0.221) for pupil-teacher ratio and 0.665 (1.481) for total number of teachers.

9. For more details on implementation of these estimations, visit Borusyak's site at https://github.com/borusyak/did_imputation.

10. See Freeman et al. (2015) for further discussion on these states' legal changes.

11. We tried several different sets of lagged variables to identify predictors of the outcome variables, and our alternative results are very similar to figures 3, 4, and 5.

12. For details on how to construct the composite index for socioeconomic status of districts, see Fahle et al. (2017).

13. During our study period, there are 29 other states with duty-to-bargain laws. They are Alaska, California, Connecticut, Delaware, Florida, Hawaii, Illinois, Iowa, Kansas, Maine, Maryland, Massachusetts, Minnesota, Montana, Nebraska, Nevada, New Hampshire,

New Jersey, New Mexico, New York, North Dakota, Ohio, Oklahoma, Pennsylvania, Rhode Island, South Dakota, Vermont, and Washington (Moe 2011, table 2-2, pp. 54–55).

14. Allegretto and Mishel (2020) estimate the pay gap between teachers and their comparable college graduates was 19.2% in 2019, which has been constantly growing since 1996. They also estimate the teacher wage penalty by state ranges from 2.0% in Wyoming to 32.7% in Virginia for 2014–19. The teacher wage penalty was 20.9% in Idaho, 21.3% in Indiana, 15.9% in Michigan, 21.4% in Tennessee, and 19.9% in Wisconsin.

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